

AN ABSTRACT OF THE DISSERTATION OF

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Title: Geneticist L.C. Dunn: Politics, Activism, and Community

Abstract Approved:

Mary Jo Nye

L. C. Dunn (1893-1974) spent most of his scientific career conducting research in developmental genetics as a member of the Zoology Department at Columbia University in the City of New York. He had an accomplished scientific career researching mutations in mice, which earned him respect from other geneticists and scientists. Genetics research, however, was only one aspect of Dunn's activities. He also campaigned for political and social rights, usually focusing his attention on problems pertinent to scientists and science. Guiding Dunn were his beliefs in socialism, democracy, and intellectual freedom, as well as his humanitarianism. He effectively participated on campaigns in two ways, remaining out of the public eye by performing committee work and entering the public sphere by publishing and lecturing.

Dunn did not act alone, but rather was a member of various networks forming an active community of intellectuals. Together these scholars organized privately and acted publicly in an effort to combat what they viewed as society's injustices, and

through their intellectual and political participation they shaped American society.

This dissertation focuses on Dunn's professional years from the 1920s to 1950s and explores L.C. Dunn's role as a socio-political activist among a community of scholars.

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Geneticist L.C. Dunn: Politics, Activism, and Community

by
Melinda Gormley

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APPROVED:

Major Professor, representing History of Science

Chair of the Department of History of Science

Dean of the Graduate School

I understand that my dissertation will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my dissertation to any reader upon request.

Melinda Gormley, Author

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LIST OF ABBREVIATIONS

APS:	American Philosophical Society
HUA:	Harvard University Archives
MCZ:	Museum of Comparative Zoology
NYPL:	New York Public Library
NYU:	New York University
RAC:	Rockefeller Archive Center
UACL:	University Archives and Columbian Library at Columbia University

Geneticist L.C. Dunn: Politics, Activism, and Community

Introduction

L. C. Dunn (1893-1974) spent most of his scientific career conducting research in developmental genetics as a member of Columbia University's Zoology Department. He had an accomplished scientific career that earned him respect from other geneticists and scientists. His brief work as a poultry geneticist and more lengthy devotion to the T-locus in mice resulted in important contributions to genetics.¹ Professional research, however, was only one aspect of Dunn's activities. He also campaigned for political and social rights, usually focusing his attention on problems pertinent to scientists and addressing scientific issues relevant to citizens inside and outside of the United States. Guiding Dunn were his beliefs in socialism, democracy, and intellectual freedom, as well as his humanitarianism. He effectively participated on campaigns in two ways, remaining out of the public eye by performing committee work and entering the public sphere by publishing and lecturing.

¹ Dorothea Bennett, "L.C. Dunn and his Contribution to T-Locus Genetics," Annual Review of Genetics 11 (1977): 1-12; Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs 49 (Washington, D.C.: National Academy of Sciences, 1978): 79-104; Mary F. Lyon, Sohaila Rastan, and S.D.M. Brown, eds., Genetic Variants and Strains of the Laboratory Mouse 3rd edition, volume 1 (Oxford: Oxford University Press, 1996): see dedication.

Dunn chose his political and social activities by responding to situations developing in the world around him. He involved himself in a wide spectrum of campaigns ranging from regional and national issues to international affairs. In the early part of the twentieth century, genetics and eugenics were complementary disciplines in the United States; however, starting in the late 1920s more and more geneticists grew wary of eugenics. During the 1920s and 1930s, Dunn participated in activities that fortified genetics and undermined eugenics especially. When Nazi laws ousted Jews from academic positions beginning in 1933, he acted as an executive member of the Faculty Fellowship Fund at Columbia University and the Emergency Committee in Aid of Displaced Foreign (formerly, German) Scholars. Both organizations relocated scholarly refugees from Germany and other European countries to academic centers in the United States. With the progression of events in Europe, Dunn and other New York intellectuals reacted by founding anti-fascist organizations. One group morphed into another as events progressed and these organizations combined in 1939 to form a national association, the American Committee for Democracy and Intellectual Freedom.

During the war and particularly after it ended, Dunn and an increasing number of scientists grappled with science-related issues, some of which had political dimensions. He supported public funding of science and contributed ideas to the drafting of legislation that ultimately established the National Science Foundation. He helped with post-war reconstruction in European and Asian countries by sending scientific materials to colleagues overseas. As one of his primary efforts toward

rebuilding science and relations between scientists, Dunn presided over the American-Soviet Science Society, which aimed at facilitating the exchange of scientific supplies such as publications, research specimen, and scholars. In response to restrictions developing from Cold War fears in the late 1940s, Dunn redirected his attention to societal discrimination by undermining scientific racism. Addressing racism was not new to Dunn. He earlier had confronted the issue with reference to eugenics, and during the war he co-authored Heredity, Race and Society with his friend and colleague Theodosius Dobzhansky. Beginning in 1950, Dunn assisted the United Nations Educational, Scientific, and Cultural Organization (UNESCO) in its campaign to educate people about race and racism. He also helped found the Institute for the Study of Human Variation at Columbia University, and through it he studied small, isolated human groups in an effort to learn about evolution and the biological concept of race.

In summary, Dunn participated in many, diverse campaigns centering on issues facing science and scientists. In a memoir concentrating on Dunn's scientific work, one of his last students, Dorothea Bennett, remarked on the several facets of his life noting that "without a whole volume or maybe two to do it in" writing about all aspects of Dunn's life "is just too big to attempt." Bennett accurately assessed what a full biography of Dunn would require. I share Dorothea Bennett's opinion and for this reason only a portion of his life is discussed in-depth below. Dunn's political activities during his professional years, from roughly the 1920s to the 1950s are the core of my dissertation. Since he taught in the Zoology Department at Columbia

University for thirty-five of these years, most of my dissertation concentrates on his life in New York City. I focus on this period for several reasons. During it, Dunn established his scientific reputation, developed his political views, and participated in socio-political campaigns. Moreover, this time period coincides with the years leading up to and following World War II, which was an era of great change and activism in the United States.

Dunn's Political Views

At the height of his political activities Dunn held leftist, liberal views, and he can best be defined as a socialist. He developed his opinions slowly over time, and he was self-conscious about his ambiguous political stance in the late 1920s. In a letter to Walter Landauer, his close friend and scientific colleague at Storrs Agricultural Experiment Station in Connecticut, Dunn noted that he was naïve about politics and that his views typically fell outside of conventional political categories.

He [E. J. Gumbel] was obviously disappointed with my non-political & non-partisan individualism since he is such a certain and convinced communist. He is out of sympathy with all idealism – and is equally opposed to Liberalism & anarchy. I felt we should not be sympathetic politically, and that argument would not help matters since instruction is more necessary than anything else for me & I am too ignorant to hold any beliefs very seriously except in some vague & rather idealistic way.²

² APS, Dunn Papers, Series I, Box 18, Walter Landauer, Dunn to Landauer, 14 Oct 1927. Dunn went to a party while in Germany at which politics were debated from about 1 to 3 AM. Dunn referred to Gumbel, who was most likely Emil Julius Gumbel (1891-1966), a known leftist who overtly opposed German militarism and the Nazi Party during the 1920s. Dunn mentioned E.J. Gumbel of Heidelberg University in the travel report to C.B. Hutchison of the International Education Board, the organization

Dunn was in his mid-thirties in 1927 when he made this comment. Landauer was about three years younger than Dunn, and politically more astute. Landauer attended the University of Heidelberg from 1915 to 1921, his graduation delayed due to spending two years serving the International Red Cross during World War I. After his return to graduate studies in 1918, Landauer vociferously promoted socialism and democracy. Dunn and Landauer's friendship grew strong during their overlapping years at Storrs between 1924 and 1928, and they remained in constant contact for the rest of their lives.

As years passed, Dunn developed a strong political stance and found it impossible to stand aside. He actively combated ideas and actions that he viewed as unjust by undermining eugenic and racist ideas and speaking out against fascism. Dunn seldom defined his political views throughout his lifetime, except for describing himself as a Fabian Socialist when he was almost seventy years old.³ Fabian Socialists believe in gradual political and social change; therefore, their ideology is evolutionary as opposed to revolutionary. England had a strong tradition of Fabian

that funded his trip (APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928.) Dunn would help Gumbel to immigrate to the United States in 1940 by helping Gumbel get visas. Gumbel, a mathematical statistician, initially found employment through the New School for Social Research's University in Exile (Arthur D. Brenner, Emil J. Gumbel: Weimar German Pacifist and Professor, Boston: Humanities Press, 2001: 164, 181-84).

³ L.C. Dunn, "The Reminiscences of Leslie Clarence Dunn," Oral History Collection of Columbia University, Interviews by Saul Benison, 1958-1960, 1022.

Socialism dating from the late nineteenth century, but attempts to establish an organization in the United States did not succeed.⁴

Dunn explicitly shared his thoughts on socialism and its pertinence to scientists around the end of World War II. He spoke candidly and vigorously on political topics at this time, which was roughly during 1944 and 1945.

We all know that the test of a good scientific hypothesis is not how much it tells us but how fruitful it is for further studies; and old and respectable theories have to be discarded overnight when they fail to explain a crucial fact. All scientists ought thus by nature to be radicals but they are not.

Dunn followed this statement with a discussion of H.G. Wells' views on socialism and noted that "Scientists will certainly be increasingly aware and active socially and politically, and if Wells' diagnosis is correct the normal position of a scientist should be socialism."⁵ Two topics informed Dunn's views when he made this comment. He was fostering communication and improving relations with Soviet scientists through the American-Soviet Science Society, and he was also actively developing legislation

⁴ G.D.H. Cole, Fabian Socialism (London: George Allen and Unwin Ltd., 1943); Patricia Pugh, Educate, Agitate, Organize: 100 Years of Fabian Socialism (London: Methuen and Company, 1984): 1. Socialism had a strong following in the United States during the late 19th and early 20th centuries. Its popularity declined in the years surrounding World War I; however, socialists remained active in American society during the 1930s and 1940s. Bernard K. Johnpoll and Lillian Johnpoll, The Impossible Dream: The Rise and Demise of the American Left (Connecticut: Greenwood Press, 1981); Seymour Martin Lipset and Gary Marks, It Didn't Happen Here: Why Socialism Failed in the United States (New York: W.W. Norton & Company, 2000); Frank A. Warren, Noble Abstractions: American Liberal Intellectuals and World War II (Columbus: Ohio State University Press, 1999).

⁵ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-"Science and the Future of Society" (Lecture), 1945: 19 pages, 6. Dunn delivered this lecture at Columbia University's Teachers' College on 8 Nov 1945.

for what would become the National Science Foundation. Dunn sincerely believed that socialism might gain a foothold in American society by using science as its vehicle, a view he fully supported and shared during the mid-1940s.⁶

Dunn's actions also help to pinpoint where he stood on the political spectrum. He supported the Soviet experiment after visiting Russia in 1927, but he became disillusioned with the Soviet Union after Josef Stalin favored Lysenkoism and decreed an end to the study of Mendelian genetics in 1948. Dunn also helped Democratic Senator Harvey M. Kilgore to write legislative measures on governmental funding of science in 1943 and 1944 and campaigned for Progressive Party candidate Henry Wallace in the 1948 presidential election.

Looking back in 1970 on his earlier years, Dunn reiterated his apolitical beginnings, and he credited Walter Landauer for inspiring his political consciousness and activism. In this reminiscence to Landauer, Dunn described his political views as socialist and anarchist, as well as noted that he remained true to his personal convictions rather than following a specific political party line:

I had been accustomed to the rather restricted and parochial outlook formed in a conventional and comfortable childhood & youth, without strong political or social views... Without indoctrination or persuasion on your part I began to turn toward socialist or anarchist solutions as desirable without weighing their feasibility or applicability to particular circumstances, but as a matter of personal taste or

⁶ APS, Dunn Papers, Series I, Box 9, L.C. Dunn – “Science under Socialism” (Lecture), 1944: 13 pages and L.C. Dunn – “Science and the Future of Society” (Lecture), 1945: 19 pages. Dunn delivered these talks at Boston University on 9 Nov 1944 and at Columbia University's Teachers' College on 8 Nov 1945.

principle... I am quite sure that the good causes I pursued, especially during the thirties & forties, had their origins in discussions with you.⁷

Throughout his lifetime, Dunn interacted with people from a variety of political perspectives in scientific and political debates, while in political and social campaigns, Dunn typically engaged with other like-minded individuals who were fairly liberal too.

Throughout his politically and socially active years, Dunn held tight to a few basic tenets as guiding principles. As an American citizen, he fought for the preservation of civil liberties and democracy. As a scientist, he vehemently supported international cooperation. When denied a passport extension in 1953, Dunn summarized his views and actions in the following manner:

(1) a lifelong belief that civil liberties lie at the very foundation of democratic government; (2) an aversion and active opposition to Fascism which began when Hitler, Mussolini and later Franco destroyed civil liberties and attempted to destroy democratic government; (3) an active belief in and practice of international cooperation in science which extends over my whole scientific life.⁸

These doctrines underlined his work with scientific and other academic refugees and with his studies of populations that had been racially and eugenically targeted. In addition to preserving that which he believed in whole-heartedly, Dunn also strove to establish that which he felt was missing. He aimed to instate science into the infrastructure of the United States by drafting legislation on governmental funding of

⁷ APS, Dunn Papers, Series I, Box 20, Walter Landauer, Dunn to Landauer, Easter Sunday 1970.

⁸ APS, Dunn Papers, Series I, Box 8, L.C. Dunn-Passport Correspondence, 1953, Dunn to Secretary of State [John Foster Dulles], n.d. He wrote it in response to a letter dated 9 Apr 1953.

science and by establishing genetics as a credible discipline, divorced from eugenics. In reference to genetics, Dunn fought internationally to prove its integrity as a science and to safeguard its future. As mentioned above, he not only undermined eugenics and scientific racism, but also aided Jewish refugees to relocate from Europe to the United States and combated the assault on Mendelian genetics in the Soviet Union. Moreover Dunn helped to establish genetics as a discipline in other countries, and rejuvenated its practice by sending literature and specimens to war-torn countries.

Why L.C. Dunn?

Dunn's contemporaries, including academic administrators, politicians, and scientists, turned to him for information and help with various problems. Geneticists in particular knew that they could count on Dunn to help and advise them. Curt Stern asked Dunn in 1933 for advice on building a sterilizer used to disinfect *Drosophila* bottles in his zoology laboratory at University of Rochester. Dunn quickly responded by sending Stern the blueprints and costs for the one built in the laboratory at Columbia University.⁹ Professors searching for zoologists and geneticists contacted Dunn for suggestions about whom they should hire. Beverly W. Kunkel of Lafayette College in Easton, Pennsylvania requested suggestions for a professor in genetics and acquired two men that Dunn had mentioned. Kunkel hired Dunn's former student

⁹ APS, Curt Stern Papers, Correspondence with Dunn, Stern to Dunn, 3 Oct 1933 and APS, Dunn Papers, Series I, Box 25, Curt Stern, Dunn to Stern, 5 Oct 1933.

Paul David in 1937 and the German émigré Ernst Caspari in 1938.¹⁰ John A. Detlefsen solicited Dunn's advice in 1946 when searching for a Head Scientific Consultant of Biological Sciences to the Department of Commerce. Dunn suggested Curt Stern.¹¹ Investigators in need of specimens for their laboratory research contacted Dunn, and he either sent some from his own stocks or suggested breeders who had the specimens desired.¹² Several biological researchers appealed to Dunn for his help with research specimens during and after World War II. He received a rare breed of wheezing mice in 1937 from J.B.S. Haldane who worried that his mice

¹⁰ APS, Dunn Papers, Series I, Box 18, B. W. Kunkel, Kunkel to Dunn, 19 May 1937 and 11 Jan 1938, Dunn to Kunkel, 29 Aug 1938. There are other examples of people asking Dunn for his recommendations on geneticists. Theodore Just, Professor in the Biology Department at Notre Dame, wanted suggestions for a professor in vertebrate zoology. Dunn recommended one of his students, Francis J. Ryan, who still had to finish his degree and was not ready to move on. He also mentioned Dr. Herbert Elftman who was a professor with the Department of Anatomy at the College of Physicians and Surgeons (APS, Dunn Papers, Series I, Box 17, Theodore Just, Sep 1940). Warren Deacon asked Dunn for names of scientists, especially geneticists, who could help to expand the biology department at Vanderbilt University. In another case, Dunn supplied S.C. Dellinger with the names and qualifications of four people for an opening in the genetics department at University of Arkansas (APS, Dunn Papers, Series I, Box 6, Warren Deacon, 1948 and S.C. Dellinger, 1950).

¹¹ APS, Dunn Papers, Series I, Box 6, John A. Detlefsen, 1946-47, Detlefsen to Dunn, 16 May 1946 and Dunn's reply, 23 May 1946. Dunn suggested others in addition to Curt Stern for the position.

¹² APS, Dunn Papers, Series I, Box 6, Charles Danforth; Box 21, C. C. Little; Box 26, Russell Wilder.

might become casualties of war in London,¹³ and Dunn sent *Drosophila* to Otto Mohr in Norway after fighting stopped in 1945.¹⁴

What was it about Dunn that caused scientists and others to turn to him for information and help? The explanation lies in his scientific reputation, administrative activities, and genial personality. Furthermore, those who personally knew Dunn recognized his efficiency and loyalty. Norwegian geneticist Otto Mohr thought Dunn's extensive participation in committee work came as a consequence of his skill at managing the tasks at hand: "Since you are such an efficient man, Dunn, everybody is likely to turn to you."¹⁵ Geneticist Richard Goldschmidt told Dunn that the Emergency Committee in Aid of Displaced Foreign Scholars should broach the problem of Japanese internment. Results seen within three months caused Goldschmidt to surmise that Dunn had heeded his suggestion: "As I noticed recently that this Council [the Emergency Committee] has taken up with our President a suggestion which I had conveyed to you by letter, I conclude that the way via Dunn is a very efficient one."¹⁶ Dunn had indeed informed the Emergency Committee about

¹³ "Rats, Art and Men," New York Times (4 December 1938): E8; "Odyssey of Rats in Exile Revealed," New York Times (15 August 1940): 18; "Reported from the Field of Science: Refugee Science," New York Times (8 September 1940): 62.

¹⁴ APS, Dunn Papers, Series I, Box 23, Thomas Hunt Morgan, Dunn to Morgan, 22 Sept 1945.

¹⁵ APS, Dunn Papers, Series I, Box 22, Otto Mohr, 1937-1939, Mohr to Dunn, 8 Apr 1937.

¹⁶ APS, Dunn Papers, Series I, Box 14, Richard B. Goldschmidt, 1939-1950, Goldschmidt to Dunn, 30 Apr 1942 and 5 Feb 1942. Goldschmidt had explained to Dunn that the Japanese internment problem needed immediate attention in February (Goldschmidt to Dunn, 5 Feb 1942).

the situation,¹⁷ and he also acted on the subcommittee to investigate the “Japanese Question” regarding Japanese-American interment.¹⁸ Geneticist Hermann J. Muller repeatedly asked Dunn for help because Dunn had proven trustworthy in the past: “Forgive me for bothering you with all this. You see that good deeds, once started, bring the initiator no end of trouble! So in a way you have yourself to blame. But I do appreciate it.”¹⁹ As demonstrated in the cases mentioned thus far, Dunn went to great lengths to help geneticists with scientific and non-scientific problems.

Dunn also demonstrated great diplomacy in his interactions with others, which explains why people tended to call on him for help and why he held several leadership roles. In 1926 Leon F. Whitney sent Dunn a manuscript of his book The Basis of Breeding for review and comment before publication. Dunn sent Whitney a list of suggested revisions, all of which Whitney decided to implement. “You have a way of making the suggestions,” Whitney told Dunn, “so that anyone can see that there is nothing but the best intentions in what you say and that, I believe, is a real

¹⁷ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid for Displaced German Scholars-Correspondence, 1942, Drury to Dunn, 3 Mar 1942. Betty Drury, Secretary to the ECADFS, asked Dunn to say something at the next meeting about the situation on the west coast.

¹⁸ NYPL, Box #167, Emergency Committee in Aid of Displaced Foreign Scholars, Subcommittee Records, Subcommittee on Japanese, 1942. Stephen Duggan, Chairman of the ECADGS, asked Dunn on 22 Apr 1942 to act on this subcommittee. Dunn agreed on 29 Apr 1942.

¹⁹ APS, Dunn Papers, Series I, Box 23, Hermann J. Muller, 1938-1945, Muller to Dunn, 7 Jan 1945. I believe that Dunn was helping Muller to find a permanent academic post in the United States. Muller had returned from the Soviet Union in 1938, but had yet to find a permanent position. Indiana University in Bloomington hired Muller later that year and he started working there in fall 1945.

art.”²⁰ When D.F. Jones handed off the managing editor duties of Genetics to Dunn in 1936, he warned Dunn: “You will have a good chance to exercise your well known tact and diplomacy.” The business manager, who produced the annual financial reports for the journal was hard to deal with, according to Jones.²¹ Dunn noted his need for tact while conducting field research on the Jewish Ghetto Community of Rome during the early 1950s. “The community study is now going very well altho’ to date it has required more diplomacy than science.” He wanted to draw blood samples from human beings, but had to appease the religious leaders before the scientific investigations could begin.²²

Dunn, indeed, had a sincere interest in those whom he viewed as part of his fraternity, which explains his efforts on their behalf and their respect for him. After aiding Curt Stern to relocate to the United States in 1933, Stern wrote to Dunn asking if he could help other German geneticists, specifically Richard Goldschmidt, Joachim Werner Braun, and Ernst Caspari. Dunn successfully helped to bring all three to the United States in the 1930s and 1940s.²³ Stern thanked Dunn in 1941 for his genuine friendship over the years,²⁴ and Braun thanked Dunn for helping him to relocate to

²⁰ APS, Dunn Papers, Series I, Box 26, Leon F. Whitney, Whitney to Dunn, 21 June 1926.

²¹ APS, Dunn Papers, Series IV, Box 30, *Genetics*-Managing Editor, 1935-1938.

²² APS, Dunn Papers, Series I, Box 20, Walter Landauer, Dunn to Landauer, 24 Nov 1953.

²³ APS, Dunn Papers, Series I, Box 25, Curt Stern, 1932; APS, Stern Papers, L.C. Dunn Folders #1 and #2 covering 1933-37.

²⁴ APS, Stern Papers, L.C. Dunn, Folder #4, Stern to Dunn, 22 April 1941. “You have taken such a kind interest in my situation here at Rochester that I should like to let

Goldschmidt's laboratory at University of California in Berkeley. "I rarely met anybody who is so kindly interested in the fate of other people as you are" wrote Braun in a letter to Dunn. "Let me thank you once more for all your help and advice, which you gave when I was still a 'greenhorn' in New York."²⁵ Theodosius Dobzhansky, Dunn's colleague at Columbia University from 1940 until Dunn's retirement in 1962, revered Dunn as a scientist and administrator. Dobzhansky told Dunn that he was considering giving up his scientific research when Dunn retired because he had no desire to be in New York without Dunn as his co-worker. Dunn convinced Dobzhansky to continue his scientific pursuits and offered to help him find a job elsewhere. Dobzhansky went to the Rockefeller Institute before relocating to University of California at Davis.²⁶

In several cases others' knowledge of Dunn's committee work and his scientific reputation suggested him as an informed resource. In 1929 Dunn accepted the Chairmanship of the Committee on Organization of the Genetics Sections of the American Society of Zoologists and the Botanical Society of America. This committee established the Genetics Society of America (GSA) and its members voted

you know that things have finally become settled here. I shall be Chairman of the department for a three year term beginning next fall."

²⁵ APS, Dunn Papers, Series I, Box 3, Werner Braun, 1936-1937, Braun to Dunn, 11 Mar 1937.

²⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky, Dobzhansky to Dunn, 6 July 1961; APS, Theodosius Dobzhansky Papers, L.C. Dunn, Dunn to Dobzhansky, 13 July 1961 and Dobzhansky to Dunn, 6 August 1961.

Dunn as its first president.²⁷ Dunn's role at GSA President acquainted him with American and foreign geneticists; for example, the newly established Genetics Society of America played a pivotal role in organizing the Sixth International Genetics Congress in Ithaca, New York in 1932. Dunn acted as the Congress's Chairman of the Transportation Committee, which arranged transportation to and in New York. Many of Dunn's foreign colleagues, in addition to asking him for research specimens after the war, also turned to him for scientific literature to replace documents destroyed or never received during the war years. Dunn sent books to Kornel Kőrösy who had to leave his home and abandon all his books in Budapest in February 1945. Kőrösy's son, Francis, told Dunn about his father's death in 1948 and expressed how much the books had meant to his father: "I thank you heartily in his and in my name for having sent him the books he needed and loved so much and for the Genetics [referring to the journal]... You did such a good deed with him."²⁸ Dunn sent scientific literature overseas to many scientists, especially geneticists, in the years after the war ended.

²⁷ APS, Dunn Papers, Series I, Box 14, Genetics Society of America, see folders covering 1927-1932.

²⁸ APS, Dunn Papers, Series I, Box 18, K. Kőrösy, n.d., Kőrösy to Dunn, 16 July [no year] and Francis Kőrösy, 1948, Francis Kőrösy to Dunn, 31 July 1948. Kornel Kőrösy told Dunn that he had lost his books and thanked Dunn for the ones that he had sent. Two books that Kőrösy especially appreciated were C.H. Waddington's Modern Genetics and Lancelot Hogben's Nature and Nurture. Francis noted that his father had passed away two weeks prior: "He never was quite himself since the fascist ordeal and suffered a heavy attack in 1945 but recovered remarkably [sic] well for two years."

Dunn's scientific reputation alone warranted reverence from his peers. William E. Castle, Dunn's advisor during graduate school at Harvard University, acknowledged Dunn's scientific work ethic and perseverance in the laboratory shortly after Dunn graduated: "Glad to hear about your problems. I am sure you will get something out of them, if you keep at it. You always do, and need no urging."²⁹ After relocating to University of California in Berkeley, geneticist Richard Goldschmidt informed Dunn that he used Principles of Genetics, the textbook written by Dunn and Edmund W. Sinnott, because of its excellence. "I am using again in my class with great advantage your text (ca. 220 students). Though I follow a different system and emphasize not always the same things I think that your book is by far the best text available."³⁰ In her eulogy to Dunn, his student and colleague Dorothea Bennett remarked that Dunn's scientific approach "reflected standards of integrity and honesty that were rarely matched" and that these principles infected his intellectual descendants.³¹ Indeed, Dunn's efforts through personal interaction and public activism influenced those who knew him. Furthermore, his commitment to people and causes influenced others and created a strong support group ready to respond when Dunn asked for help.

²⁹ APS, Dunn Papers, Series I, Box 4, William E. Castle, 1920-1929, Castle to Dunn, 1 Sept 1921. Dunn started at Storrs Agricultural Experiment Station in 1920 and conducted genetics experiments on poultry.

³⁰ APS, Dunn Papers, Series I, Box 14, Richard B. Goldschmidt, 1939-1950, Goldschmidt to Dunn. The letter was dated between 1939 and 1942. Goldschmidt compared Dunn and Sinnott's book to An Introduction to Genetics (1939) by A.H. Sturtevant and George W. Beadle, which he found "pedagogically impossible."

³¹ Bennett, 1-2.

Dunn reacted to local, national, and international events unfolding around him and through his activism he helped shape the future of both genetics and American society. He undeniably accomplished a lot during his lifetime. How was he able to achieve so much? He lived in New York and he had a lot of help from his friends. Although not the only factors, physical location and community networks are two pivotal circumstances contributing to Dunn's activism.

Dunn's physical location at Columbia University in the City of New York was instrumental to his participation in a wide range of scientific and extracurricular endeavors. New York City provided a unique milieu unmatched in the rest of the United States because it teemed with intellectuals and activists, as well as acted as a major port for incoming refugee scientists.³² Additionally, the density of scholars in the northeastern United States and their proximity to Washington D.C. provided a fertile environment for organizations because of its convenience for conducting business in meetings and by mail.³³

³² Although scholars discuss intellectual communities of New York City, they do not typically focus on scientists. Alexander Bloom, Prodigal Sons: The New York Intellectuals and Their World (New York: Oxford University Press, 1986); Thomas Bender, New York Intellect: A History of Intellectual Life in New York City from 1750 to the Beginnings of Our Own Time (New York: Alfred A. Knopf, 1987). Alexander Bloom focuses on Jewish intellectuals in New York City and argues that the intellectual community becomes active in the 1930s and disintegrates during the 1960s. Thomas Bender has three intellectual lives, one of which is academic culture, that occur simultaneously and having different degrees of power over time. Democracy is pivotal to Bender's analysis of intellectualism in New York City.

³³ For a general discussion about the importance of place in relation to the performance of scientific research, acceptance of scientific theories, and dissemination of scientific information, see: David N. Livingstone, Putting Science in its Place: Geographies of Scientific Knowledge (Chicago: The University of Chicago

Dunn relied on the constant and reliable support of his friends and colleagues; without them, he could not have participated in the many ventures that he did. Thus, it would be impossible to write about Dunn's life without mentioning the many people with whom and for whom he worked. He not only actively participated in organizations associated with his discipline, institution, and geographical locale, but held directorial positions in most of these organizations. As demonstrated above, Dunn's integrity and idealism guided his personal activities and infected those around him, and his personality and convictions attracted people to him, many of whom remained lifelong friends and confidants. Brief biographical accounts of those with whom he interacted supplement the discussion of Dunn's activities and life. These biographical sketches demonstrate the integral role of networks and community among scholarly activists as well as give information about those who Dunn worked with and for. Moreover, Dunn's personality is described through discussions of his interactions with others. All in all, Dunn hardly acted alone, and his interaction with others makes his role within a larger community a necessary focal point.

In my dissertation, I demonstrate that Dunn was a well-connected individual among scientists, politicians, and other important figures, which I believe provides access to a better understanding of geneticists as a community of intellectuals and

Press, 2003); An author considering her historical subject in terms of physical location is: Janet Browne, Charles Darwin: The Power of Place (New Jersey: Princeton University Press, 2002). Robert McCaughey has discussed the administrative history of Columbia University and its affiliates. Robert A. McCaughey, Stand, Columbia: A History of Columbia University in the City of New York, 1754-2004 (New York: Columbia University Press, 2003).

activists. United by their humanitarianism, these scholars coordinated their efforts undertaking private and public campaigns in an effort to combat what they viewed as society's injustices. Through their intellectual and political participation they shaped American science and society.

My dissertation is an exploration of Dunn's political activism and the scientific ideas and practices that aided him in his endeavors. It is a partial biography in which I use Dunn as a common denominator in order to explore political aspects in the history of genetics, connect scholars from diverse disciplines, and explore a volatile period of the world's history. In a few words, my dissertation focuses on complexities that Americans, and particularly American scientists, faced in the years surrounding World War II, while focusing on one man, who was central to them all, Leslie Clarence Dunn.

Literature Review

The views of Dunn's contemporaries demonstrate his importance as a historical figure in American science and politics. Historians have relied on personal perspectives because almost all biographical information devoted entirely to Dunn comes from him or his friends. He participated in Columbia University's oral history project by sitting through several interviews over two years that resulted in over 1000 pages of transcript. Dunn's reminiscences are not only autobiographical, but also present an international perspective of the history of genetics from roughly 1920 to

1960.³⁴ Theodosius Dobzhansky and Dunn made a pact that whoever lived longer would write a biographical sketch of his recently deceased friend. Dobzhansky outlived Dunn by a little less than two years and in that time fulfilled requests from the American Philosophical Society (APS) and National Academy of Sciences for biographical essays on Dunn. Dobzhansky's memoirs adequately cover Dunn's scientific and extracurricular activities and have provided many scholars with background on Dunn's life, career, and activism.³⁵ The geneticist Dorothea Bennett wrote about Dunn's scientific work on the T-locus, some of which they pursued jointly while she was Dunn's student and colleague in the 1950s and early 1960s.³⁶ Bentley Glass wrote Dunn's entry in the Dictionary of Scientific Biography, which incorporates information from Dunn's archival documents deposited at the American Philosophical Society.³⁷

Other than these short memoirs and biographical articles written by Dunn's colleagues, little has been written about him. Indeed, historians have not recognized Dunn as an important historical subject even though they use his archived papers to find valuable information. In terms of available published documentation on Dunn,

³⁴ Dunn, "Reminiscences," 1022.

³⁵ Theodosius Dobzhansky, "Leslie Clarence Dunn (1893-1974)," Yearbook of the American Philosophical Society (1974): 150-56; Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs 49 (Washington, D.C.: National Academy of Sciences, 1978): 79-104. About the pact: APS, Dobzhansky Papers, Louise Dunn, Dobzhansky to Louise, n.d. Dobzhansky's letter was written after Dunn's death on 19 Mar 1974 and before 7 May 1974 when Louise responded.

³⁶ Bennett, 1-12.

³⁷ Bentley Glass, "Leslie Clarence Dunn," Dictionary of Scientific Biography 17 (New York: Charles Scribner's Sons, 1990): 248-50.

references to him are most frequently found in footnotes, rather than in text. Part of the reason for this is the enormous quantity of documents that Dunn archived, which amounts to thirty-two boxes. He discarded some of his papers, but nonetheless what survives is a collection of correspondence and committee records that represent the core concerns of geneticists living in the United States, Europe, and Asia during the twentieth century. These documents span several decades that were pivotal to the development of genetics as a fundamental discipline within biology.³⁸

Dunn's decision to deposit his personal papers with the American Philosophical Society most likely resulted from two experiences, his writing of A Short History of Genetics (1965) and his giving an oral history to Columbia University (1958-1960).³⁹ Whatever the reason, Dunn committed himself to the task of convincing other geneticists to preserve their papers for posterity.⁴⁰ Glass remarked on the vastness of the American Philosophical Society's genetics collection and Dunn's role in its acquisition.

By far the greatest archive of original materials for the history of genetics exists in the Library of the American Philosophical Society. This collection was largely started and developed through the indefatigable efforts of Leslie C. Dunn, who spent much time during

³⁸ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1966, Dunn to Landauer, 5 May 1966: "I have sifted my correspondence & have discarded much but shall deposit several files bearing on special problems, journals, NRC committee, science bills leading to Nat Sci. Foundation, Emergency Committee, etc."

³⁹ L.C. Dunn, A Short History of Genetics, The Development of Some of the Main Lines of Thought: 1864-1939 (Ames: Iowa State University Press, 1965); L.C. Dunn, "The Reminiscences of Leslie Clarence Dunn," Oral History Collection of Columbia University, Interviews by Saul Benison, 1958-1960, 1022.

⁴⁰ APS, Dunn Papers, Series I, Box 20, Walter Landauer, Dunn to Landauer, May 1966; APS, Stern Papers, L.C. Dunn Folder #6, Dunn to Stern, 24 Jan 1967.

his later years in encouraging his geneticist friends to deposit their papers in the archives of the Society.⁴¹

Although Glass said this over twenty years ago, his assertion is still true today.

Due to the extensive quantity of sources in Philadelphia, historians frequently draw from the Genetics Collection at APS, and as mentioned they use Dunn's papers to find information about their historical subjects.⁴² Therefore, several historians have pulled out pieces of Dunn's life by accessing only a portion of his archival information. The result is that a fair amount has been written about Dunn and the activities that he engaged in, but it gives an incomplete picture of him and the information is scattered. Moreover, Dunn's role as an administrator coordinating the efforts of scientists (especially geneticists) has not been fully recognized. As demonstrated in my dissertation, he held a vital position motivating, organizing, and leading scientists.⁴³

⁴¹ Bentley Glass, "The Preservation of Historical Materials in Genetics," Bulletin of the History of Medicine 57 (1983): 98-105, 100. About Dunn's role in pioneering the genetics collection at the American Philosophical Society, see also the APS website's introduction to the Genetics Collection at <http://www.amphilsoc.org/library/guides/glass/> (3 Aug 2006).

⁴² Examples include Kenneth Ludmerer's Genetics and American Society (1972), William Provine's Sewall Wright (1986), Peter Kuznick's Beyond the Laboratory (1987), Robert Kohler's Lords of the Fly (1994), Nikolai Krementsov's Stalinist Science (1997), Jessica Wang's American Science in the Age of Anxiety (1999), Joe Cain's "The Columbia Biological Series, 1894-1974" (2001) and "Co-opting Colleagues" (2002), and Karen Rader's Making Mice (2004). Note that these authors have used Dunn's papers to write on science and politics and history of genetics and eugenics.

⁴³ In order to gain as complete a picture of Dunn's life as possible, I spent three months living in Philadelphia reading Dunn's archived materials from Boxes One to Thirty-two. Documents in the papers of other geneticists, such as Theodosius Dobzhansky, Curt Stern, Ernst Caspari, and Milislav Demerec and the anthropologist

Since Dunn's activities encompass a myriad of topics, I have been able to bring together assorted literatures and draw connections between these seemingly separate issues. As a consequence of the diverse nature of these historical subjects, a complete literature review of each individual topic is not possible here. Certain genres of literature guided me because they helped frame Dunn within a larger context. Of particular importance are those on history of genetics, eugenics, and scientific racism, as well as science and politics. Some other important perspectives are provided in histories about scientific communities and institutional histories.

Among historians who have analyzed the relationship between science and politics in the United States from the 1920s to 1960s are Peter Kuznick, Jessica Wang, and Nikolai Kremmentsov. Each of these authors used Dunn's papers and mentioned him. Peter Kuznick argues that scientists stepped Beyond the Laboratory in the 1930s when they acted as political activists fighting for democracy, intellectual freedom, and other virtues. Important to a study of Dunn are Kuznick's chapters on anthropologist Franz Boas's organization of scientists with the American Committee for Democracy and Intellectual Freedom and the role of American scientists in helping their colleagues in the Soviet Union. Kuznick limits his study to the 1930s, and many of the topics that Kuznick discusses are explored in my dissertation.⁴⁴ In what follows, I extend his timeframe by following activists who started their political

Franz Boas provide me with additional information, including correspondence written by Dunn.

⁴⁴ Peter J. Kuznick, Beyond the Laboratory: Scientists as Political Activists in 1930s America (Chicago: Chicago University Press, 1987).

activities in the 1930s and stayed involved in political and social affairs for the next two to three decades.

Jessica Wang discusses the effects that anti-communism and McCarthyism had on liberal scientists in the United States during the late 1940s and 1950s. Wang considers the cases of several scientists from various disciplines, focusing primarily on physics. She examines the consequences for scientists, giving particular attention to their run-ins with governmental organizations, such as the House Un-American Activities Committee (HUAC).⁴⁵ Dunn had problems with HUAC, as did the American-Soviet Science Society, which he presided over for its duration (1943-1948). One topic that Wang concentrated on was HUAC's accusations of espionage against physicist E.U. Condon. Dunn gathered evidence in an effort to exonerate Condon. Wang also explores the early stages of legislation that established the National Science Foundation, a task in which Dunn participated. My dissertation augments information provided by Wang in that I discuss similar events about scientists from a different discipline, genetics.

Nikolai Kremmentsov's histories of the Soviet Union have been essential resources for my work because political events in the Soviet Union greatly affected scientists in the United States. Kremmentsov has written about genetics in the Soviet Union focusing on the mid-twentieth century in his book Stalinist Science. Like other Soviet scholars, Kremmentsov attempts to gain a better understanding of Stalin's

⁴⁵ Jessica Wang, American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War, Chapel Hill: The University of North Carolina Press, 1999.

endorsement of Trofim Lysenko's agro-biology to the detriment of Mendelian genetics. In comparison to other scholars who have written about Lysenkoism as a national phenomenon, Krementsov takes a broader view by analyzing the effect that Lysenkoism had on genetics as an international scientific discipline and discussing the degree to which non-Soviet geneticists mobilized in hopes of helping their Soviet colleagues. He follows up on this sort of global examination in his more recent book, International Science between the World Wars: The Case of Genetics, in which he writes about the International Genetics Congresses, focusing especially on issues pertaining to the Soviets.⁴⁶ By means of complementing Krementsov's work, I try to give a fuller discussion of events in the United States that evolved in response to actions in the Soviet Union. I also analyze the political and professional consequences for geneticists in the United States who had helped facilitate scientific relations with the Soviet Union, but were forced to stop as fears of communism and the Cold War mushroomed in the late 1940s and 1950s.

Another important body of literature is the history of genetics, and its political counterparts, histories of eugenics and scientific racism. As a subject, the history of genetics has gained popularity in the past couple of decades and in recent years a greater number of biographies on geneticists and histories of genetics have been

⁴⁶ Nikolai Krementsov, Stalinist Science, New Jersey: Princeton University Press, 1997; Nikolai Krementsov, International Science between the Wars: The Case of Genetics, New York: Routledge, 2004. Books that discuss Trofim Lysenko and relegate the discussion to Lysenkoism's national aspects are Zhores A. Medvedev's The Rise and Fall of T. D. Lysenko (1969) and David Joravsky's The Lysenko Affair (1970).

written.⁴⁷ One plentiful, yet tangential area is the history of molecular biology, which deals with protein chemistry and structural chemistry in general. Specific topics are the structure of DNA, sequencing proteins and nucleic acids, and mapping genomes. Genetics and molecular biology have become important topics not only among historians, but also among philosophers, sociologists, and other scholars concerned with recent scientific and cultural developments involving the biological sciences.

William Provine, Kenneth Ludmerer, and Elazar Barkan have written about Dunn in their histories covering genetics, eugenics, and scientific racism, respectively. Provine's biography about Sewall Wright extensively explains Wright's scientific work into problems of evolutionary biology. Provine's exploration of Wright's research partnerships with Dobzhansky and others contribute to an understanding of networks between geneticists, and thereby provides a window into the genetics community. Provine provides substantial background pertinent to Dunn because Wright and Dunn were both graduate students of William E. Castle at the Bussey Institute of Harvard University. Moreover, Dunn succeeded Wright as Castle's assistant. Provine kept his focus on Wright's scientific training and

⁴⁷ A few notable examples are the following, chronologically listed biographies: Ronald Clark, J.B.S. Haldane (1968), Garland Allen, Thomas Hunt Morgan (1978); Elof Axel Carlson, Hermann J. Muller (1981); William Provine, Sewall Wright (1986); Nathaniel Comfort, Barbara McClintock (2001); Paul Berg and Maxine Singer, George Beadle (2003); Oren Solomon Harman, C.D. Darlington (2004); and Frederic Holmes, Seymour Benzer. Histories of genetics are Jonathan Harwood's Styles of Scientific Thought (1993), Robert Kohler's Lords of the Fly (1994), and Karen Rader's Making Mice (2004).

investigations and does not explore this scientific community as a political or social force.⁴⁸

Kenneth Ludmerer focuses on genetics and eugenics in American lawmaking during the first three decades on the twentieth century in his book Genetics and American Society. He draws extensively from conversations with the geneticists involved, including Dunn and others central to my project. Ludmerer also explores trends in the popularity of eugenics among scientists and laypeople, noting that geneticists withdrew from promoting eugenics at the same time that eugenic ideas gained popularity among the general public.⁴⁹

Elazar Barkan in The Retreat of Scientific Racism discusses the views of several anthropologists and biologists in the United States and Britain within their political context. Barkan looks at both sides of scientific racism, those promoting and undermining the use of science when speaking about racial differences. Barkan compares many scientists' views and achieves his aim as stated in the subtitle, to examine the Changing Concepts of Race in Britain and the United States between the World Wars. In his section on Dunn, Barkan notes that Dunn "assumed the role of

⁴⁸ William B. Provine, Sewall Wright and Evolutionary Biology (Chicago: The University of Chicago Press, 1986).

⁴⁹ Kenneth M. Ludmerer, Genetics and American Society: A Historical Appraisal (Baltimore: Johns Hopkins Press, 1972).

expert” on race in the United States during the years after the Second World War, using his expertise as a geneticist to speak authoritatively against scientific racism.⁵⁰

Since I focus on Dunn as a member of intellectual, scientific, and political communities, my dissertation features institutional histories and discussions of scientific networks. Historian Nikolai Krementsov has differentiated between communities and networks. Communities consist of many intertwining networks, and networks are the product of people interacting and making institutional decisions together. Krementsov uses internationalism and international networks to frame his book, International Science between the World Wars, and demonstrates that geneticists were an international community. Concentrating on events occurring in the Soviet Union and the nature of these international scientific networks, Krementsov gives a thorough analysis of the International Congresses of Genetics of 1927 (Berlin), 1932 (Ithaca), and 1939 (Edinburgh), as well as the cancelled Congress of 1937 (Moscow).

Krementsov also notes that institutional histories on philanthropies, research facilities and societies, are commonplace to discussions about internationalism and scientific communities.⁵¹ Indeed, Dunn’s efforts centered on committee work, and therefore most chapters include institutional histories exploring small-scale

⁵⁰ Elazar Barkan, The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars (New York: Columbia University Press, 1992).

⁵¹ Krementsov talks about institutions, ideologies, and networks on pages 4-6. Mark B. Adams, “Networks in Action: The Khrushchev Era, the Cold War, and the Transformation of Soviet Science,” Trondheim Studies on East European Cultures and Societies 3 (Oct 2000): 1-37.

associations in their initial stages. For example, he coordinated the founding of the Genetics Society of America and acted as its first president. He was an Executive member of the Emergency Committee in Aid of Displaced Foreign Scholars and American-Soviet Science Society throughout both organizations' durations. He helped UNESCO with their campaign against racism almost immediately after the project was launched. He and Dobzhansky started an interdisciplinary research organization, the Institute for the Study of Human Variation.

Krementsov's framework provided me with the idea to write about Dunn in terms of his membership within the scientific community. Dunn, however, belonged to an assortment of communities that overlapped in many ways. He was a member of the genetics community that Krementsov highlights in his book; however, Dunn was also a member of a broader scientific community. The nature of Dunn's activism was typically associated to political campaigns confronting scientists, and politics adds an additional dimension to Dunn's networks and his membership in these communities.

Methodology & Overview

My dissertation is a partial biography of Dunn concentrating on the ways that his activism on social and political campaigns intersected with his scientific beliefs. A main goal has been to highlight the interconnections between Dunn's various activities, which upon first glance appear to have no, or only tangential, connections. By using Dunn as the main thread I discuss topics usually treated separately by historians. My goals are to show that there is a significant overlap between the issues

tackled and the people involved and to blur some of the artificial lines that we, as scholars, have drawn between historical subjects. In order to do this, I have framed Dunn's campaigns in their widest contexts: discipline building, anti-fascism, war mobilization, post-war preparation, anti-racism, etc. Within these contexts I have discussed Dunn's activism on different scales (local, national, and international) and his role in terms of scientific and scholarly communities. Dunn is presented as a member of an intellectual community, a participant in committee work, and a public activist, as well as an administrator, leader, and friend. The aim is to investigate an underlining cohesion in Dunn's principles through a discussion of his political endeavors, which were not necessarily based on scientific research, and yet focused on science.

Recently four historians have analyzed a contemporary trend among historians of science toward an increased use of archival materials and a greater specialization in subject matter.⁵² Although these four authors have different interpretations of what

⁵² Some of the authors interpreted the "generalist vision" to mean writing histories that are accessible to more readers within and outside of the discipline. This is also an important point, but not one that I will discuss here. According to Robert Kohler, there has been a marked decline in publications that attempt to tell the reader something fundamental about science and scientists. This lack of general themes in the historiography of science has created what Paula Findlen has viewed as a break down in the "common conversation" between historians with different temporal and disciplinary specialties. Steven Shapin identifies a "crisis of pertinence," which he describes as a decrease in the historian's ability to write for a wide audience of laypeople or other historians. David Kaiser refers to the trend as "Balkanization" and indicates that historians' various definitions of "what science is" has created a pluralism not previously evident in our discipline. "Focus: The Generalist Vision in the History of Science," *Isis* 96 (June 2005): 224-51; Robert Kohler, "A Generalist's Vision," pp. 224-29; Paula Findlen, "The Two Cultures of Scholarship?," pp. 230-37;

a “generalist vision” is and whether or not one is needed, I think the concept of drawing broader conclusions from insular studies is an important one.⁵³ By focusing on one man, I have provided myself with a single theme. By examining him within the context of his scholastic, scientific, and social communities, I have attempted to broaden my investigation beyond one man’s life and demonstrate the power of networks and fraternity. Historical works on discrete topics are important, and in no way do I want to minimize their relevance because they have been instrumental to my writing. Through my analysis, however, I hope to show the degree to which historical events intertwined and to establish some connections that, I believe, have been overlooked. Drawing on concepts proposed by Frederick Lawrence Holmes, I try to demonstrate continuity over time.⁵⁴ Whereas Holmes demonstrated the connections within a scientist’s career, I have tried to highlight the relationships between seemingly disparate events within a succinct historical era.

Even more recently, four authors reflected on the lessons that they learned in the process of writing biographies in the History of Science. Two authors, Mary Jo Nye and Mary Terrall, present perspectives that resonate with my own thoughts. Nye considers writing biographies as a way to reach broader audiences and in her opinion,

Steven Shapin, “Hyperprofessionalism and the Crisis of Readership in the History of Science,” pp. 238-43; David Kaiser, “Training and the Generalist’s Vision in the History of Science,” pp. 244-51.

⁵³ I am thankful to the participants of Paul Farber’s seminar during fall quarter 2006, in which we discussed these articles. The conversation helped me to solidify some of my thoughts about this “Focus” section, which I had contemplated previously in terms of its value to my historiographical methodologies and goals.

⁵⁴ Frederic Lawrence Holmes, Investigative Pathways: Patterns and Stages in the Careers of Experimental Scientists, New Haven: Yale University Press, 2004.

“the most compelling scientific biographies are ones that portray the ambitions, passions, disappointments, and moral choices that characterize a scientist’s life.” In writing my dissertation, I not only discuss Dunn’s activities, but also convey his personality. Terrall considers science to be a way of living and therefore an integral part of the scientist’s personality. Science is so important and natural to most scientists that science serves as a guiding factor in the scientist’s life. This is my impression of Dunn. Furthermore, I agree with Terrall’s opinions about the benefits gained from writing a biography: “thinking about the place of biography in the discipline should lead us to think about the relation between the lives of individuals and historical arguments about culture, politics, intellectual movements, and so on.”⁵⁵ Thus, my aim is two-fold, to write about Dunn’s activities and personality while also placing Dunn within a broader context. My approach is not new, but it is an effective one.⁵⁶

The chapters of this thesis essentially follow Dunn’s life chronologically with some temporal overlap, and yet each chapter also maintains a thematic cohesion.

⁵⁵ “Focus: Biography in the History of Science,” *Isis* 97 (June 2006): 302-29. Joan L. Richards, “Introduction: Fragmented Lives,” 302- 05; Mary Terrall, “Biography as Cultural History of Science,” 306-13, quotation is from 307; Theodore M. Porter, “Is the Life of the Scientist a Scientific Unit?,” 314-21; Mary Jo Nye, “Scientific Biography: History of Science by Another Means?,” 322-29.

⁵⁶ Janet Browne, Charles Darwin: Voyaging (New Jersey: Princeton University Press, 1995); Janet Browne, Charles Darwin: The Power of Place (New Jersey: Princeton University Press, 2002); David C. Cassidy, Uncertainty: The Life and Science of Werner Heisenberg (New York: W.H. Freeman and Company, 1992); Thomas Hager, Force of Nature: The Life of Linus Pauling (New York: Simon and Schuster, 1995); Oren Solomon Harman, The Man Who Invented the Chromosome: A Life of Cyril Darlington (Cambridge: Harvard University Press, 2004).

Chapter One outlines Dunn's educational and political backgrounds, as well as his first genetics job at Storrs Agricultural Experiment Station in Connecticut. Four important events that colored Dunn's future career path are discussed: his military service during World War I, his relationship with Walter Landauer, the publication of the textbook Principles of Genetics, and his tour overseas to visit genetic stations in Europe in 1927. Chapter Two analyzes Dunn's efforts in establishing genetics as a credible discipline in the United States while also undermining eugenics' foothold. Chapter Three assesses responses to fascism by discussing Dunn's role in organizations that attempted to find jobs for European scholars at American institutions and his activism within groups that fought to protect citizen's civil liberties. Chapters Four through Six explore Dunn and his communities' responses to the events of World War II on three levels: national, local, and international. Mobilization during the war and preparation for the post-war world frame these three chapters. Scientists who had been politically active during wartime experienced a backlash from government agencies at all three levels in the onset of the Cold War, including Dunn and those with whom he collaborated. Moreover, geneticists in the Soviet Union suffered the ultimate blow to their research when Lysenko's agrobiology garnered Josef Stalin's total support at the expense of Mendelian genetics. The United States government's clampdown on individuals and organizations, as well as the efforts and reactions of American geneticists on the behalf of their Soviet colleagues, are the focus of Chapter Seven. Chapter Eight lays out Dunn's efforts to undermine scientific racism through publications and committee work. These include

Heredity, Race and Society, a book about science and social responsibility written with Dobzhansky, as well as his publications and committee work performed under the aegis of UNESCO. Chapter Nine examines the role of evolution in informing Dunn's views about race and explores his efforts to establish the Institute for the Study of Human Variation and research performed in conjunction with the Institute. The concluding chapter critically assesses Dunn, looking at his personality, achievements, faults, idiosyncrasies, and convictions.

Chapter 1

Preparing for a Life Devoted to Science, 1915-1928

Between 1915 and 1928 L.C. Dunn professionally established himself as a geneticist in the United States while also formulating political convictions that shaped his future endeavors. He was a graduate student in genetics at Harvard University under William E. Castle from 1915 to 1920 and then went to work at Storrs Agricultural Experiment Station. Dunn stayed at Storrs until 1928 before moving to Columbia University in the City of New York. Dunn cultivated his political views while at Storrs through the influence of Walter Landauer, who joined the Station's staff in 1924. Contributing to Dunn's reputation as a geneticist was the publication of his highly influential textbook Principles of Genetics with Edmund W. Sinnott.

In addition to obtaining a thorough education and establishing himself within his discipline, Dunn developed important insights that influenced his views for the rest of his life. He volunteered for the Army during World War I, where he served in France, and he traveled to Europe in 1927 to survey genetic laboratories for the International Education Board. While overseas Dunn visited the Soviet Union for the first and only time in his life. This trip had a profound impact on his future endeavors not only because he became infatuated with the Soviet Union, but also because he initiated contacts with geneticists throughout Europe that grew overtime. This chapter

explores these years and outlines the foundations of his later political endeavors and professional reputation.

The Bussey Institution and American Expeditionary Forces

Mammalian geneticist William E. Castle accepted Dunn into Harvard University's graduate program in 1915. At this time, Harvard University was one of the few institutions with a graduate program in genetics, and Castle was well-established. Other prominent scholars heading academic institutions with graduate training in genetics were T.H. Morgan and E.B. Wilson at Columbia University and H. S. Jennings and Raymond Pearl at Johns Hopkins University; all were in the northeastern United States. After finishing his doctoral dissertation in 1895 on embryological development of a simple marine species (*Ciona instestinalis*) in the ascidian order, Castle focused his research interests on elucidating problems of heredity and evolution. Two years after obtaining his doctoral degree from Harvard University, he accepted a job offer from his alma mater where he stayed until retiring in 1936. His research in 1900 dealt with sex determination and soon after the rediscovery of Mendel's laws Castle re-conceptualized his perspective. In 1903 Castle published his first paper situated within the rubric that would become known as Mendelian genetics. He remained actively engaged in investigating genetic questions until his death in 1962.¹

¹ L.C. Dunn, "William Ernest Castle, October, 25, 1867-June 3, 1962," Biographical Memoirs 38 (Washington, D.C.: National Academy of Sciences, 1965): 33-80: 37-40;

Castle worked in Cambridge at the Museum of Comparative Zoology during his initial decade at Harvard University, but sought roomier accommodations for his research and more responsibility as a graduate adviser. He managed in 1908 not only to relocate his operations, but also to establish a proper graduate school, the Bussey Institution for Applied Biology. The Bussey Institution was located at the Arnold Arboretum in Jamaica Plain less than ten miles away from Harvard's main campus. The Institution had been an undergraduate program in husbandry and gardening since 1871, but upon its closure Castle persuaded the dean of the Graduate School of Applied Sciences to give him use of the main building and to hire a botanical geneticist. E.M. East arrived in 1909.²

Genetics at the Bussey Institution thrived under the direction of Castle and East, and together they served as educators to the first few generations of geneticists by graduating roughly forty doctoral candidates during the Bussey Institution's duration from 1908 to 1936. Both men retired upon the closure of the Bussey Institution. Although not a complete list, the following are some of their more well-known students. Castle taught Dunn, E.C. MacDowell, C.C. Little, and Sewall

William B. Provine, Sewall Wright and Evolutionary Biology (Chicago: The University of Chicago Press, 1986): 35-37; H. Terry Taylor, "William Ernest Castle: American Geneticist. A Case-Study in the Impact of the Mendelian Research Program" (master's thesis, Oregon State University, 1984). I tried to get a dissertation about Castle, but Interlibrary Loan could not find a library willing to send it to Oregon State University: Michael Omorato Davia, "Of Mice and Men: the Genetics and Eugenics of William E. Castle" (diss., Harvard University, 1986).

² Dunn, "Castle," 51-52; Provine, Sewall Wright, 44-45; Karen Rader, Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955 (New Jersey: Princeton University Press, 2004): 31-32.

Wright. East educated R.A. Emerson, Karl Sax, R.A. Brink, and D.F. Jones. In addition to their scientific accomplishments, many of Castle and East's students played a major role in structuring genetics as a discipline within the United States, as will be discussed in the next chapter. In his biographical memoir of Castle for the National Academy of Sciences, Dunn remarked on Castle and East's effect on genetics as a discipline: "Castle and East and the Bussey thus exercised an important formative influence on American genetics and, through the foreign guest investigators, on other countries as well."³

Dunn reflected positively about the interactions between Castle and East and the atmosphere at the Bussey Institution. His reminiscences mirror those of other students at the Institution, such as Sewall Wright and Karl Sax. Botanical geneticist Karl Sax, recalled the "informal academic atmosphere" of the Bussey Institution, which focused on research and seminars. Castle and East often disagreed with each other during seminars and sat at opposite sides of the table defending their individual positions. Although Castle and East held different opinions, they agreed about the fundamental goal of genetics and encouraged their students to think of heredity in evolutionary terms and to keep a Darwinian perspective. Sewall Wright's biographer, William Provine, argues that Harvard graduate students in genetics imbibed the "deepest commitment to the Darwinian outlook to be found in the United States in the 1910s and 1920s." Dunn made a similar remark and included aspects in addition to

³ For the entire list, see Dunn, "Castle," 52-53; Dunn's quotation is taken from page 53.

evolution upon which his advisors agreed: “Castle and East were both interested in evolution, in animal and plant breeding, in agriculture, and in human social problems, and each was actively engaged in experimental analysis of problems which they conceived as of basic and general biological significance.”⁴ Dunn undoubtedly gained an interest in evolution as a student at the Bussey Institution from 1915 to 1920. His interests in human social problems for the most part were cultivated after he left graduate school.

Wright’s comments about Castle’s interaction with his students corroborate Dunn’s recollections. They remembered him as putting himself on an even footing with his students. When Dunn told Castle that the parasite affecting their rat colony lived on the scales of the rats’ tails, Castle matter-of-factly responded that they would have to remove the tails. Dunn and Castle cut off several hundred tails while talking about the linkage maps that they would construct from breeding those now-tailless rats. Wright, who was Castle’s graduate student between 1912 and 1915, had a similar recollection about Castle as a mentor. Wright spent his Saturdays minding after rats and guinea pigs during his first two years at Bussey and they often spoke about genetics on those weekend days. Wright recalled that Castle openly discussed

⁴ Dunn, “Castle,” 53-54; Provine, *Sewall Wright*, 46-47. Quotation in Provine’s book is from page 46. Karl Sax, “The Bussey Institution: Harvard University Graduate School of Applied Biology,” *Journal of Heredity* 57 (1966): 175-78. Dunn and Sax overlapped as students at Harvard University.

genetic issues with him whether they agreed or disagreed on the topic at hand, and Wright felt that these conversations had a profound effect on his education.⁵

Dunn's arrival at the Bussey Institution coincided with Wright's departure, and he replaced Wright as Castle's assistant. Wright trained Dunn during the summer of 1915 before moving to Washington D.C. for a job with the United States Department of Agriculture. Castle delegated several duties to his assistants including teaching seminar and laboratory classes that supplemented his genetics course. Dunn recalled that he was surprised to be given these responsibilities at the beginning of his first fall semester at Harvard University. He instructed laboratory sessions on breeding *Drosophila*, and he led discussion groups attended by graduate and upper-level undergraduate (senior year) students. Dunn felt unprepared intellectually for this task, doubting the adequacy of his knowledge of organic chemistry in order to discuss a week's book, Benjamin Moore's The Nature and Origin of Life. "He [Castle] probably told me to act as moderator of an orderly discussion which might help the students to exchange views and criticisms," stated Dunn. "At any rate that is all I could do."⁶ Dunn's statement is telling because it reflects a fundamental aspect of his personality that contributed to his reputation as a geneticist. He was a natural administrator, teacher, and moderator and rarely, if ever, doubted his ability to act as a leader.

⁵ Dunn, "Castle," 55; Provine, Sewall Wright, 64. Provine read Dunn's reminiscences and documents on Castle in order to write his book on Sewall Wright, and therefore it is not surprising that Dunn's views corroborate those in Provine's biography.

⁶ Provine, Sewall Wright, 47, 68, 99; APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 4.

In contrast to his confidence as an administrator, Dunn remained skeptical during his entire life about his capabilities as a zoologist. He had studied botany as an undergraduate at Dartmouth University and took only two courses in zoology (entomology and neuroanatomy) from John H. Gerould. Dunn believed that Gerould, who had attended Harvard University as a graduate student, helped him get accepted into Harvard's genetics program. An opening in Castle's laboratory also proved beneficial for Dunn's acceptance into the Ph.D. program at the Bussey Institution. Castle needed an assistant and had sixty dollars per month from the Carnegie Institute of Washington to hire someone. Dunn's future in zoology instead of botany was decided by a sequence of events that led him to Castle, who happened to be in need of an assistant to replace Wright. Dunn never lost interest in botany, which he fostered at Harvard University through interacting with E.M. East and plant morphologist I.W. Bailey.⁷

Dunn attended an array of science courses as a graduate student. He took sensory physiology from G.H. Parker, geology from Reginald Dailey, and paleontology from Percy Raymond. E. L. Mark directed histology and embryology laboratories, and W.H. Rand provided Dunn with courses in comparative anatomy and *Entwicklungsmechanik*. Dunn had started learning German while in grade school and continued these language studies for seven years (from the fourth to tenth

⁷ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 3-4.

grades). Dunn also cultivated his passion for literature by attending weekly seminars in modern German literature with Professor Howard.

While at Harvard University, Castle and Dunn developed a strong, yet professional relationship. After Dunn left Harvard University in 1920, they continued to correspond throughout Castle's long life about their professional work and shared their personal opinions about professional matters even when they disagreed. Castle gave Dunn advice about weighty matters pertaining to job offers and editorial posts, but not much on personal matters. The bulk of their dialogue concentrated on genetic and administrative issues.⁸

Dunn conducted most of his scientific investigations on mice while a graduate student at Harvard University producing evidence of gene linkage for hair color. Starting with color variation and its inheritance, Dunn argued in 1916 that red hair in mice was actually an intense shade of yellow hair. Furthermore, the process that produced red hair most likely resembled that which produced dark colorations in black-and-tan mice.⁹ Continuing investigations along these same lines during his several years at Harvard University, he explained an anomalous frequency of brown hair (double that expected) as a result of gene linkage of yellow and black haired

⁸ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 1-2, 4; APS, Dunn Papers, Series I, Box 4, William E. Castle, 3 folders of correspondence covering 1920-1961.

⁹ L.C. Dunn, "The Genetic Behavior of Mice of the Color Varieties "Black-and-Tan" and "Red," The American Naturalist 50 (Nov 1916): 664-75.

mice.¹⁰ He also returned to the 1916 problem four years later, presented his extensive data, but gave no conclusion, stating that the status of knowledge could not account for the information gathered.¹¹ He did, however, produce some definitive results by conducting experiments on mice with albinism and pink-eye and on rats with albinism and red-eye. He found that cross-over occurs in male and female mice at an equal rate, whereas female rats exhibit a greater degree of cross-over than male rats. This evidence of linkage proved to Dunn that genes are arranged linearly on a chromosome. Along with these experiments Dunn also provided evidence of independent, i.e. non-linked, genes, and located their relative positions on chromosomes based on whether or not they were linked. Dunn proposed four groups of color variation, of which only one, albinism and pink-eye, demonstrated linkage. The other three were independent: non-agouti, piebald spotting, and black-eyed white spotting.¹² In the final paper that Dunn wrote on his mice experiments as a graduate student, he primarily summarized the status of mice genetics through a discussion of his and C.C. Little's efforts for understanding white spotting. Additionally, Dunn shared his opinion about the most profitable avenue of genetic research: "...it is through the investigation of linkage and the localization of the hereditary determiners

¹⁰ L.C. Dunn, "Anomalous Ratios in a Family of Yellow Mice Suggesting Linkage between the Genes for Yellow and for Black," The American Naturalist 53 (Nov-Dec 1919): 558-60.

¹¹ L.C. Dunn, "The Sable Varieties of Mice," The American Naturalist 54 (May-Jun 1920): 247-61.

¹² L.C. Dunn, "Linkage in Mice and Rats," Genetics 5 (May 1920): 325-43; L.C. Dunn, "Independent Genes in Mice," Genetics 5 (May 1920): 344-61. Agouti mice have bands of light and dark hair. Piebald mice are spotted.

that the most exact knowledge concerning the nature of the hereditary material can be secured.”¹³

Also while a graduate student, Dunn published an insightful article in 1917 debating whether hereditary material resided in the cytoplasm or nucleus. He argued that the nucleus held the necessary information for heredity and development, whereas the cytoplasm supported the nucleus by providing a suitable environment. The “cytoplasmic” and “chromosome” theories of hereditary material, as Dunn referred to them, were the focus of an ongoing debate at the time. Historians point to C.B. Bridges’ 1916 paper in which he located genes within the chromosome as the decisive experiment in support of the chromosomal theory; however, the debate continued for over a decade after Bridges published his article. Dunn in 1917 did not cite Bridges’ finding, but rather contested the views held by E.G. Conklin, Jacques Loeb, and A.F. Shull, all of whom were about one generation older than he. Conklin, Loeb, and Shull proffered a compromise theory in which the cytoplasm contributed to inheritance and embryonic development.

Dunn neither conducted experiments to write his article, nor cited himself. Rather, he turned to experimental evidence gathered by others, especially Theodor Boveri who had provided support for the chromosomal theory by 1907. Boveri and other investigators sought a physical understanding of the cell, and by 1910 had proven to cytologists and geneticists that chromosomes were important heredity

¹³ L.C. Dunn, “Types of White Spotting in Mice,” The American Naturalist 54 (Nov-Dec 1920): 465-495, 489.

material. A biochemical and physiological understanding of the gene developed after 1930 through contributions such as George Beadle and E.L Tatum's "one gene-one enzyme" hypothesis. Dunn astutely observed that the status of knowledge in 1917 left many questions unanswered, and moreover that geneticists needed to broaden their understanding of cells in order to solve the problems arising from the cytoplasmic-chromosomal debate. All the while, he recognized the dependent relationship between the cytoplasm and nucleus as well as their independent roles:

Whatever relations may exist between the two [cytoplasm and nucleus], the fact remains that the cytoplasm is necessary. Without it the nucleus, deprived of its milieu, can not live, and development can not take place. The investigation of the finer physiological reactions which take place between the nucleus and cytoplasm is badly needed, and the restatement of them in terms of physics and chemistry. Such evidence as is available indicates that the importance of the cytoplasm is in the main subordinate to that of the nucleus.¹⁴

During World War I Dunn and several of his fellow students enlisted as volunteers for the military forces by joining the Harvard Regiment. Dunn signed on because of his agitation with Germany's unrestricted submarine warfare. He continued his academic studies until just before the United States entered World War I in April 1917, and he shortly thereafter left Boston for training camp in Plattsburg,

¹⁴ L.C. Dunn, "Nucleus and Cytoplasm as Vehicles of Heredity," The American Naturalist 51 (May 1917): 286-300. Quotation is from pages 298-99; John A. Moore, Science as a Way of Knowing: The Foundations of Modern Biology (Cambridge: Harvard University Press, 1993): 265-84, 303-27, 348-52; Joseph S. Fruton, A Skeptical Biochemist (Cambridge: Harvard University Press, 1992): 93-104. Entries in Dictionary of Scientific Biography were consulted for information on Edwin Grant Conklin (v. 3, 389-91), Jacques Loeb (v. 8, 445-46), and A.F. Shull (v. 12, 416-18). Charles C. Gillispie, ed., Dictionary of Scientific Biography (New York: Scribner's Son, 1971).

New York. He returned to Massachusetts as a Second Lieutenant of Infantry in September for six months of additional training at Camp Devons before going to France as a First Lieutenant and company officer with the American Expeditionary Force. The 76th Division out of Camp Devons left for France in July 1918 and was designated a Depot Division, which meant that most members did not engage in active warfare, which seems to have been the case with Dunn.¹⁵

Dunn did not tell his mother that he was about to be deployed to France while visiting her prior to his departure. Instead he wrote her a letter from the train explaining that he did not want their last moments before he left to be sad and he liked that they had parted cheerfully rather than in tears. Dunn spoke of having left his heart and soul at home and only having taken with him “the self which is trained and outfitted for war,” telling his mother that he would come back “the real me.”¹⁶ Dunn’s military stint stayed in his mind and came vibrantly into focus when his older son, Robert, decided to enlist for the armed forces during World War II, a topic that will be discussed in Chapter Five. Dunn failed to persuade Robert, who became an airplane pilot trained to bomb Germany, that pacifism was a better solution than war. Robert returned from World War II safely. L.C. Dunn himself returned safely from France and resumed his studies at Harvard University in March 1919. Prior to his

¹⁵ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., “Autobiographical Resume,” 11 pages, 5; Captain Shipley Thomas, The History of The A.E.F. (New York: George H. Doran Company, 1920): 188, 487.

¹⁶ APS, Dunn Papers, Series I, Box 7, Harriet A. Dunn, 1918-1947, n.d., Dunn to his mother, n.d.

departure Dunn married Louise Porter on 2 May 1918; Robert Leslie was born in 1921 and their other son, Stephen Porter, in 1928.¹⁷

Storrs Agricultural Experiment Station

After graduating from Harvard University in December 1920, Dunn started his first job at Storrs Agricultural Experiment Station in Connecticut. Storrs Agricultural Experiment Station was well-established location for agricultural research. It had started in 1881 based on the model of its nearby forerunner the Connecticut Agricultural Experiment Station in New Haven, which was the first American station, established in 1875.¹⁸ The Hatch Act passed in 1887 and offered \$15,000 per year to each state with an Agricultural Experiment Station. In 1906 the Adams Act doubled the annual grants and specified that Agricultural Experiment Stations conduct original research.¹⁹ Historian Charles Rosenberg argues that the Adams Act created job positions for geneticists because station administrators sought doctoral students rather than lesser trained personnel. Rosenberg notes that Agricultural Experiment Stations offered jobs to Thomas Hunt Morgan's students at

¹⁷ Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs 49 (Washington, D.C.: National Academy of Sciences, 1978): 79-104, 80-81.

¹⁸ Charles E. Rosenberg, "Science, Technology, and Economic Growth: The Case of the Agricultural Experiment Station Scientist, 1875-1914," No Other Gods: On Science and American Social Thought revised and expanded edition (1967, Baltimore: Johns Hopkins Press, 1997): 153-72, 154; "History," University of Connecticut website, <http://www.uconn.edu/about/history.php> (accessed 30 Aug 2006).

¹⁹ Rosenberg, "The Adams Act: Politics and the Cause of Scientific Research," No Other Gods, 173-84, 173.

Columbia University during the first two decades of the twentieth century.²⁰

Considering that Columbia's program focused primarily on *Drosophila*, it seems logical that stations would also approach students at the Bussey Institution. Under East and Castle, students investigated topics in botanical and mammalian genetics that would readily apply to agricultural needs.

That Dunn went to Storrs is not surprising considering that Agricultural Experiment Stations in Connecticut employed several biologists associated with Harvard University. E.M. East worked at New Haven for about five years before moving to the Bussey Institution. D.F. Jones, who was East's student, graduated from Harvard University in 1915 and went to the Connecticut Agricultural Experiment Station to continue the research program that East had started on corn. Edmund W. Sinnott attended Harvard University for his undergraduate and doctoral degrees, completing his Ph.D. in 1913. He remained at the Bussey Institution as an instructor until 1915 and then moved to Storrs where he stayed until 1928. Dunn and Sinnott wrote the first edition of Principles of Genetics when they worked at Storrs Agricultural Experiment Station together.²¹

²⁰ Rosenberg, "The Social Environment of Scientific Innovation: Factors in the Development of Genetics in the United States," No Other Gods, 211-24, 221.

²¹ For a biographical sketch about E.M. East at Storrs Agricultural Experiment Station, see: Provine, Sewall Wright, 45-46; Paul C. Mangelsdorf, "Donald Forsha Jones, April 16, 1890-June 19, 1963," Biographical Memoirs 46 (Washington, D.C.: National Academy of Sciences, 1975): 135-56, 136; George S. Avery, "Edmund W. Sinnott (February 5, 1888-January 6, 1968)," Bulletin of the Torrey Botanical Club 95 (Nov-Dec 1968): 647-52.

While employed at Storrs, Dunn did not have a connection with Connecticut Agricultural College (later the University of Connecticut) and therefore did not have any teaching responsibilities. Storrs hired Dunn with the agreement that he could devise his own research agenda focusing primarily on poultry genetics. Thus, Dunn diverted his attention from mice while at Storrs for nearly eight years, and he resumed his studies of mice when he relocated to Columbia University in 1928. Rosenberg suggests several reasons to explain scientists' attraction to Agricultural Experiment Stations. Stations regarded research and publication as primary concerns, and therefore offered attractive jobs. Moreover, scientists had limited opportunities for employment in the early twentieth century because few industrial jobs and academic centers existed in the United States.²²

In terms of his scientific investigations, Dunn performed most of his research on poultry, yet he also wrote a publication on cattle and continued his research on mice. He developed a technique for feeding poultry that investigators at other stations revered for its practicality in solving problems common when raising poultry in confinement. Dunn pointed to two factors lacking from indoor rearing of poultry: proper dietary and environmental conditions. With a good diet and suitable sunlight, indoor-chickens could be reared without exercise and develop similarly to those raised outside. Rickets was the main health problem plaguing chickens, causing them to lose leg strength. Dunn found that a chicken's rations should include 0.5 per cent

²² APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 5; Rosenberg, 167-68.

of cod liver oil and suggested storing the oil in its raw form rather than mixed with dry feed. Cod liver oil, according to Dunn, lost its therapeutic value when mixed with starch and then stored for six months. In its raw form the cod liver oil, moreover, could cure chickens exhibiting signs of leg weakness.²³

Dunn developed another practical method that saved breeders the expense of rearing more chickens than desired. He proposed determining the gender of poultry at hatching, a much younger age than previously possible, by monitoring the breeding crosses made and then separating males and females according to plumage. This method allowed breeders to rear male and female chickens knowing their ultimate destination. Females would become egg layers, whereas males could be killed, fattened, or sold at an earlier age. Dunn suggested that the application of his gender test based on sex-linked characters significantly decreased rearing costs. Moreover, Dunn explained that by crossing poultry strains in certain ways, one could produce more vigorous offspring. The key to optimal breeding was an understanding of the basic principles of Mendelian genetics and controlled crosses.²⁴

Researchers at Agricultural Experiment Stations were expected to devise practical techniques that farmers could apply. Dunn's efforts while at Storrs demonstrate his willingness to help farmers while also conducting experiments that

²³ L.C. Dunn, "Feeding Young Chickens in Confinement," Bulletin of the Storrs Agricultural Experiment Station 116 (March 1924): 3-16; L.C. Dunn, "Effect of Dry Storage on the Antirachitic Potency of Cod Liver Oil," Science 59 (30 May 1924): 485; L.C. Dunn, "The Effect of Cod Liver Oil in Various Amounts and Forms on the Growth of Young Chickens," Journal of Biological Chemistry 61 (1924): 129-36.

²⁴ L.C. Dunn, "A Method from Distinguishing the Sex of Young Chicks," Bulletin of the Storrs Agricultural Experiment Station 113 (March 1923): 245-80.

interested him personally. Dunn sought advice from his colleagues and farmers while employed at Storrs, especially in his initial years, because he had no previous agricultural experience. As Dobzhansky recalled, "...he had never handled a chicken before accepting this position..."²⁵

The data acquired during his tenure at Storrs Agricultural Experiment Station that was most significant to his later research dealt with vigor as displayed through egg "hatchability" of chicks. By 1924, after conducting experiments for several years, Dunn ascertained that mortality in chicken embryos resulted from internal rather than external factors. He turned his attention to genetic issues at this time after having successfully proven that a population of hens produces varying results, whereas individual hens demonstrate a consistency in "hatching quality" under "uniform and optimum conditions of incubation." Building on hybrid corn research conducted by E.M. East and D.F. Jones, Dunn corroborated their conclusions based on plants by producing similar results for animals.²⁶ Dunn's examinations proved the benefits of hybrid vigor, which will be discussed in Chapter Two with reference to genetics and eugenics.

²⁵ Dobzhansky, 1978, 81; APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 5-6.

²⁶ L.C. Dunn, "The Variation of Eggs in The Rate at Which They Lose Weight: Part IV, Relation between Loss of Weight and Hatching Quality," Poultry Science 3 (1924): 136-48. Parts I-III of this article also appeared in Poultry Science 2 (1922-1923): 45-58, 166-71, 199-204. Part IV gives an overview of Dunn's conclusions and the direction of his further research. A more encompassing discussion of Dunn's work and its importance to other animals and to plants is: L.C. Dunn, "Genetic Factors Involved in the Mortality of Chick Embryos," Second World's Poultry Congress (1924): 61-67. Quotation is from page 62. APS, Dunn Papers, Series I, Box 7, L.C. Dunn-Autobiographical data, n.d., "Autobiographical Resume," 11 pages, 6.

Dunn accomplished several of his scientific investigations with the help of Walter Landauer while at Storrs, and the two men developed a strong relationship that lasted throughout their lives. Their genetic research dealt with several developmental traits in poultry, including a morphological mutation in fowls. The mutation created some birds without proper derrières, a condition termed “rumplessness.” In a series of papers that were published between 1925 and 1936, Dunn and Landauer described two types of rumplessness. One was inherited, but the other was not and was termed accidental. Rumplessness resulted from missing vertebrae and was associated with the occasional suppression of an oil gland. Skeletal comparisons allowed them to ascertain that fowl with similar appearances (no tail and a more erect posture) actually possessed two different deformations. One or two vertebrae missing from the center of the synsacrum signified hereditary rumplessness, whereas a few vertebrae missing from the end denoted the accidental condition. They analyzed both types, but devoted more time to the genetic mutation.

Hereditary rumplessness provided them with intriguing research because this mutation switched from a dominant to recessive trait, and then practically disappeared from their stock. In 1926 they labeled it a dominant trait and concluded that most rumpless fowl were typically heterozygous for the trait. This finding clarified a misunderstanding about the condition. Was the condition due to developmental problems that occurred between roughly the ninth and twelfth days of incubation? Or was the information absent from the beginning and therefore the vertebrae never developed? After deeming hereditary rumplessness a heterozygous trait, they stated

that parents bore both rumpless and normal progeny, which settled the dispute. The vertebrae never developed in those that grew to be rumpless fowls. A minority of the progeny would have shown signs of developing vertebrae because they were normal fowls. By 1934 they had crossed normal and rumpless fowl, and they bred an intermediate rumpless fowl with a fleshy and mostly featherless tail. After breeding the mutated and normal fowl they concluded that the mutated condition switched from a dominant to recessive trait. “The change in the dominance of the rumpless gene,” they stated, “was assumed to have been brought about by the introduction of normal genes (plus modifiers for tail development) from certain normal fowls and by their accumulation by selection within the rumpless intermediate stock.”

Their results supported research conducted in Thomas Hunt Morgan’s laboratory on *Drosophila*, as well as Dunn’s unpublished experimental evidence on mice and rats. Dunn and Landauer also noted that tails of rodents and birds changed often and therefore were prone to genetic modification. In their final paper on the subject, published in 1936, they announced the end of their trials with rumpless birds. They had managed through their crosses to practically eliminate rumplessness from their stock. Heterozygote and normal birds were hard to differentiate and the homozygous recessive fowl with the rumpless gene were no longer totally rumpless. They had selectively bred better birds over eleven years.²⁷ In other words, they had

²⁷ Walter Landauer and L.C. Dunn, “Two Types of Rumplessness in Domestic Fowls: A Morphological Comparison,” Journal of Heredity 16 (1925): 153-60; L.C. Dunn and Walter Landauer, “The Genetics of the Rumpless Fowl with the Evidence of a Case of Changing Dominance,” Journal of Genetics 29 (1934): 217-43; Walter

demonstrated that by mating chickens with different genotypes, one could improve the whole population. Dunn later applied his findings on heterozygous vigor as demonstrated by poultry and mice to inform his views about human heredity, as will be discussed in Chapter Eight in reference to his attempts to combat the notion of pure races.

Before discussing the personal relationship between Dunn and Landauer, it is necessary to assess Dunn's scientific accomplishments at Storrs Agricultural Experiment Station. W.L. Slate, Director of Storrs from 1923 to 1938, noted the importance of Dunn's practical feeding methods in 1938 during the fiftieth anniversary celebration of Storrs Agricultural Experiment Station. Slate stated: "Dr. L.C. Dunn came to the station in 1919 to continue the inbreeding experiments with poultry begun by [Wm. F.] Kirkpatrick and [L.E.] Card. His work soon brought fame to the Station as well as to himself. Concerned primarily with the fundamentals of inheritance of chickens, he made such practical contributions as the method for feeding young chicks in confinement." R.E. Buchanan, Director of the Iowa Agricultural Experiment Station, said of Storrs in general and Dunn specifically: "The studies of Dr. Dunn and associates in poultry breeding, particularly the work in inbreeding, have their fruition in part with us [at Iowa Agricultural Experiment Station], and a decade or two hence will be as important relatively as your contributions to Iowa in corn breeding. You laid here the foundations for an

Landauer and L.C. Dunn, "Further Data on Genetic Modification of Rumplessness in the Fowl," Journal of Genetics 33 (1936): 401-05.

understanding of the complex problems of poultry nutrition; we are feeding our poultry on the basis of this work.”²⁸ I.M. Lerner, a poultry geneticist who entered the field after Dunn had completed his duration at Storrs Agricultural Experiment Station, admired Dunn’s “pioneering and versatility” and claimed that Dunn’s genetic research on poultry had not been matched. Moreover, in the 1940s and 1950s researchers realized that Dunn’s egg weight and hatchability examinations contributed to evolutionary theory. He had demonstrated that natural selection stabilizes and produces intermediate phenotypes. Lerner developed his ideas about genetic homeostatis by adding to Dunn’s experimental data. Although Lerner knew Dunn personally, his assessment of Dunn’s achievements warrants credence because Lerner was considered one of the most distinguished poultry geneticists when he made these comments following Dunn’s death.²⁹

While concurrently at Storrs from 1924 to 1928, Dunn and Landauer developed a strong personal relationship that produced lasting effects for them both. Landauer’s biographers labeled Dunn as a mentor to Landauer in poultry genetics, which was central to Landauer’s forty-year career as a geneticist at Storrs and a professor at its academic institution, University of Connecticut. Landauer was hired

²⁸ Bulletin of the Storrs Agricultural Experiment Station 228 (Oct 1938): 1-64. Each man made these statements during their talks delivered at the 50th Anniversary celebration on 28 July 1938: W.L. Slate, “Milestones and Monuments-An Historical Sketch of the Station,” 5-16, 14; R.E. Buchanan, “The Place of an Agricultural Experiment Station in the Economy of a Commonwealth,” 17-42, 42. Dunn started working at Storrs in January 1920 according to most sources, but in his quotation Slate said 1919. Storrs might have officially hired Dunn in 1919 or he might have begun research for them before moving to Connecticut.

²⁹ Dobzhansky, 1978, 81.

as an assistant geneticist in 1924 and was promoted to Dunn's role as lead geneticist of poultry husbandry when Dunn left in 1928. Dunn remained a consulting geneticist for several years after leaving Storrs which allowed him to finish and publish the results of some of his long-term investigations. While Dunn taught Landauer about science, Landauer shared his beliefs in socialism and democracy with Dunn and irrevocably shaped Dunn's political views.

Landauer attended University of Heidelberg for graduate school and obtained employment from the University after graduating in 1921 with a doctoral degree in zoology. Shortly after Landauer started his graduate studies in 1915, he took a two year leave of absence during World War I. Known to be a pacifist and conscientious objector, Landauer got a medical position with the International Red Cross. After the war ended, he returned to Heidelberg, resumed his studies, and began writing political articles. Of the nine broad topics that his biographers listed, two in particular pertain to Dunn. Landauer promoted "the need for true democracy" and "the virtues of socialism and the means to achieve it."

Germans experienced several tumultuous years following the end of World War I, and Landauer published his opinions in socialist papers. An astute observer, Landauer recognized his countrymen's tendency towards nationalism and racism as early as 1919, and their receptiveness of the National Socialist German Workers Party shortly after it started in 1920. By the time that the Nazi Party gained power, Landauer had already left Germany. He came from Jewish stock and the fate of his immediate family during the Nazi regime's reign reflects what is widely known about

Germany in the 1930s and early 1940s. His autistic brother, Heinz, lived in a German institution for the feeble-minded and at some point suffered the fate of euthanasia, common to many mentally ill individuals. Landauer described his younger brother as “a loveable creature, very gentle, the incarnation of good-naturedness” when he learned in 1941 of Heinz’s death. Walter’s mother was not sent to a concentration camp, but she nonetheless died during the war, alone and ostracized in Heidelberg. Two of his brothers left Germany; one went to China and another to Chile.³⁰ One of them, Fritz, had a hard time too. After his release from a concentration camp in spring 1943, Fritz found life hard and the cost of living extremely high. He needed to spend his entire monthly paycheck to buy leather shoes. Fritz initially wrote to Landauer threatening to “liquidate” his life and asking for money. Soon thereafter Fritz wrote again; he was going to kill himself and was giving Landauer custody of his young daughter.³¹

Gustav Landauer, Walter’s uncle, greatly influenced Landauer’s pro-socialism and pro-anarchism views. Gustav was twenty-six and actively engaged in political and intellectual pursuits when Walter was born in 1896. Gustav has been described by

³⁰ Hugh Clark, Julius Elias, and Peter Bergmann, “The Antecedents of Nazism: Weimar, The Political Papers of Walter Landauer,” Transactions of the Connecticut Academy of Arts and Sciences 56 (2000): 181-372, 193-232. The quotation from Landauer about his brother Heinz comes from Landauer to Dunn, 12 Feb 1941 (APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1941).

³¹ Landauer did not know why he was burdening Dunn with this news, but told him nonetheless. APS, Dunn Papers, Box 19, Walter Landauer, 1944, Landauer to Dunn, 9 Apr 1944. Fritz Landauer (1883-1968), the architect of Jewish synagogues and other buildings in Germany during the interwar period does not appear to be Walter Landauer’s brother (Judaism website: <http://www.judentum.net/kultur/landauer.htm>, 20 December 2006).

his biographers as a humanitarian who criticized Marxism, capitalism, militarism, and bureaucratic authority, and who favored a community-based utopia rather than urban, industrialized society. Gustav's romantic philosophy reflected the views of middle-class, left-leaning intellectuals who saw the benefits of democracy and faults of radicalism. In Munich in early May 1919, members of the "White Guard," i.e. troops of the German social-democratic republic, publicly beat to death Gustav and two of his compatriots that were and remain better well-known, Rosa Luxemburg and Kurt Eisner. At the time of his uncle's death, Walter had only recently started to engage in public political discussions. Evidence of Walter's reverence for his uncle's philosophy and activism is evident in Walter's own views as shared with Germans while he lived in Germany, through his influence upon Dunn after moving to the United States, and in the archival materials about Gustav Landauer that Walter deposited with his own documents.³²

Landauer and Dunn shared several commonalities in addition to and as a result of their scientific research and political views. Dunn had a deep appreciation for literature and Russia; Landauer was well-read, especially in Russian literature. After reading a book they typically sent it to the other and shared their opinions of it. Dunn's younger son Stephen suffered from severe physical handicaps, a similarity to the mental retardation of Landauer's brother. When problems began for Germans and

³² Clark, Elian, and Bergmann, 197-98; Eugene Lunn, Prophet of Community: The Romantic Socialism of Gustav Landauer (Berkeley: University of California Press, 1973): 3-10; Ruth Link-Salinger [Hyman], Gustav Landauer: Philosopher of Utopia (Indianapolis: Hackett Publishing Co., Inc., 1977): 2-4, 22, 51, 88. Walter Landauer's papers are at the Dodd Research Center at University of Connecticut.

Austrians in the 1930s, both men started programs to help refugees at their universities. Their correspondence spans fifty years from 1924 to Dunn's death in 1974 and is rich in historical information about their personal lives, professional research, and international political issues.³³

Genetics was a small discipline prior to 1930 and its adherents tended to keep in contact regularly. They corresponded through mail, attended annual genetics meetings held over winter break, and coordinated summer research trips to coastal towns such as Cold Spring Harbor and Bar Harbor. Dunn met many scientists with whom he would remain close for the rest of his life while at Harvard University and Storrs Agricultural Experiment Station. As mentioned above, Dunn worked with Sewall Wright for one summer at the Bussey Institution while learning how to take care of the laboratory specimens. Landauer became his lifelong friend at Storrs. Dunn sincerely appreciated the guidance he received from plant geneticist Donald F. Jones, also a colleague at Storrs. Jones in later years handed over to Dunn his posts as Secretary and Treasurer of the Joint Genetics Sections and managing editor of Genetics.³⁴

³³ Insights gained from reading archived letters between Dunn and Landauer, which are filed together. APS, Dunn Papers, Series I, Boxes 18-20, Walter Landauer, 1924-1974. My dissertation attests to the value of their correspondence because many personal details about Dunn come from letters between him and Landauer.

³⁴ APS, Dunn Papers, Series I, Box 17, Donald F. Jones, 1960, Dunn to Jones, 3 May 1960.

A Fundamental Textbook in Genetics and an Awe-inspiring Trip to Russia

In addition to Dunn's education, scientific research, and burgeoning network of colleagues, a major factor contributing to his prestige as a geneticist was his classic textbook, Principles of Genetics. Co-authored with botanical geneticist Edmund W. Sinnott, this textbook went through five editions from 1925 to 1958.³⁵ Dunn solicited Theodosius Dobzhansky's aid on the last two editions, by which time Sinnott no longer participated in the revision process. Howard Levene, a colleague of Dunn and Dobzhansky's at Columbia University, wrote a chapter on the use of statistics in genetics for the fifth and final edition. A sixth edition was planned and in the process of revision with an expected release date of 1962, but it never reached publication.³⁶

Several elements contributed to the book's success. The authors substantially revised each edition in order to incorporate the most current genetic knowledge. Since genetics was a relatively new field when they first began publishing Principles of Genetics, existing data quickly became outdated or supplemented. Their frequent revisions attempted to keep pace with the changing discipline; they rewrote sections in order to provide up-to-date information about new developments and deleted outmoded ideas. For example, Dunn and Sinnott had a chapter entitled "The Problems of Eugenics" in the first edition of 1925. In his review Castle noted that the authors failed to caution students that eugenicists base their conclusions on the assumption

³⁵ The editions and their copyright years are as follows: 1st edition, 1925; 2nd edition, 1932, 3rd edition, 1939; 4th edition, 1950; and 5th edition, 1958.

³⁶ APS, Dunn Papers, Series I, Box 22, McGraw-Hill Book Company Correspondence, 1931-1969.

that inheritance and not environment determines a person's traits. He warned that "They [Dunn and Sinnott] present eugenics as the eugenicists would have it." This chapter was eliminated from the second edition, published in 1932. (The next chapter will discuss the development of genetics as a discipline in the United States and Dunn's changing views about eugenics.) Ernst Caspari highlighted in a review that the fourth edition published in 1950 discussed human genetics throughout its pages, which he appreciated because the authors extended the laws of genetics to all living beings.³⁷ Over the years Principles of Genetics was translated into several languages including Spanish, Hebrew, Hindi, Russian, and a Serbo-Croatian language.³⁸

Dunn and Sinnott included questions at the end of each chapter of Principles of Genetics. This was a novel approach in genetic textbooks in the 1920s. Mathematical texts commonly incorporated a problem section in an effort to get students to implement lessons learned by reading the chapter. Dunn and Sinnott geared their text towards undergraduate students of genetics and most likely borrowed this technique from other disciplines. This feature faced skepticism early on, but over the years it became one of the book's noteworthy qualities, according to

³⁷ Edmund W. Sinnott and L.C. Dunn, Principles of Genetics: An Elementary Text, with Problems 1st edition (New York: McGraw-Hill Book Company, Inc., 1925); W.E. Castle, "Some New Books on Genetics," Science 62 (18 Dec 1925): 567-69; Albert F. Blakeslee, "Principles of Genetics: a Text-Book, with Problems; Recent Advances in Plant Genetics," Science 77 (17 Mar 1933): 284-85; Th. Just, "Principles of Genetics," American Midland Naturalist 22 (Nov 1939): 775; Ernst Caspari, "Principles of Genetics," Science 112 (15 Dec 1950): 725; F.H. Sobels, "Principles of Genetics," The Quarterly Review of Biology 34 (Jun 1959): 151.

³⁸ APS, Dunn Papers, Series I, Box 22, McGraw-Hill Book Company Correspondence, 1931-1969.

reviews written about the various editions. Castle in 1925 insisted that hypothetical problem-solving did not replace laboratory experimentation, whereas about twenty-five years later two instructors hailed the questions presented as “An indispensable feature of any such text.” Sinnott and Dunn noted in the preface of their first edition that they considered laboratory experience important to the learning process, and they seem to have headed off Castle’s objection. As teachers, they found that students learned certain principles better by drilling them repeatedly, which was the objective of their “Questions for Thought” and “Discussion, Problems and Reference Assignments.” Dunn held a non-teaching post at Storrs Agricultural Experiment Station, which may imply that it was Sinnott who found drilling a good tool for instructing students.³⁹

Dunn took a sabbatical from Storrs in 1927 with the plan that he and his family would spend the year mainly in Edinburgh and he would work with geneticist F.A.E. Crew. He arrived in Edinburgh in March 1927, and in June Dunn learned that the International Education Board (IEB) had awarded him a travel grant that he would use to tour laboratories in five European countries: Great Britain, Denmark, Norway, Germany, and the Soviet Union. The IEB gave Dunn \$1000 for living, traveling, and incidental expenses, and in return he reported to the IEB on his observations about the scientists and scientific centers that he visited. C.B. Hutchison, Director for Agriculture of the IEB, informed Dunn that the money did not make him an official

³⁹ Castle, “Some New Books on Genetics,” 568; S. Fogel and I.H. Herskowitz, “Principles of Genetics,” The Quarterly Review of Biology 27 (Jun 1952): 210-11, 211; Sinnott and Dunn, Principles of Genetics.

representative of the organization; nonetheless, it allowed Dunn to meet numerous geneticists in Europe with whom he remained in correspondence and to whom he sent care packages of documents and other scientific supplies from the 1930s through the 1940s.

Conceived by Wickliffe Rose and started with financial support from John D. Rockefeller, Jr. in 1923, the IEB sent American scientists to foreign laboratories in order to meet their disciplinary colleagues in other countries. The bulk of these tours occurred between 1923 and 1928 with the intention of promoting education in European and North American countries by funding scholars and institutions. The Rockefeller Foundation took the International Education Board into account when reorganizing its grant system starting around 1929, and officially absorbed the IEB in 1938. After this restructuring, the Rockefeller Foundation no longer funded individual people and organizations, but rather gave money to scientific projects.

The IEB's aims were not new; others had sponsored educational and research programs that enhanced buildings, equipment, and personnel. The novelty of the IEB's program was its international scope, which grew from Rose's desire to uphold democratic principles and to help with recovery from damages incurred during World War I. George Gray noted that the IEB chose to support programs at reputable institutions with capable leaders in the sciences and agriculture in an effort to maximize their endowment from John D. Rockefeller, Jr.⁴⁰ The IEB's goals mirror

⁴⁰ RAC, RF 6.1, Series 1.1, Box 11, Folder 117, Natural Science USA, 1926-1927, Dunn to Hutchison, April 1927, 5 pages and Hutchison to Dunn, 16 Jun 1927; George

Dunn's personal convictions and stand as a precursor to his own later attempts to aid his fellow geneticists and other scholars in the years surrounding World War II.

The IEB appears to have had little interest in the Soviet Union at the time, but Dunn wanted to go there, and therefore he wrote to Hutchison from Edinburgh shortly after his arrival on the continent and argued his point of view:

I have a great hankering to see what's going on at [Nikolai] Koltzoff's and [M.M.] Zawadovsky's Institutes at Moscow. There's been a series of remarkable papers emanating from there and rumors of many thrilling things in progress, [A.S.] Serebrovsky for example is reputed to have voluminous data on most of the characters in fowls with which I've been working. Muller brought back and published some of Serebrovsky's data four years ago and told me the bare bones of a long story about which I've been curious ever since, Crew and the other people here also feel that if we can't get the Russians out to tell us what is going on, then perforce we must go and see.⁴¹

As is evident, Dunn's excitement about the scientific developments in the Soviet Union had been piqued prior to going to the country, and he was too close in proximity to the Soviet Union to let the opportunity slip away. Moreover, in September Dunn attended the Fifth International Congress of Genetics held in Berlin

Gray with an introduction by Raymond B. Fosdick, Education on an International Scale: A History of the International Education Board, 1923-1938 (New York: Harcourt, Brace and Company, 1941): v-xiii; 3-15; Raymond B. Fosdick, The Story of the Rockefeller Foundation (New York: Harper & Brothers, 1952): ix-xiii; Robert Kohler, Partners in Science: Foundations and Natural Scientists 1900-1945 (Chicago: The University of Chicago Press, 1991): 233-39.

⁴¹ RAC, RF 6.1, Series 1.1, Box 11, Folder 117, Natural Sciences USA, 1926-1927, Dunn to Hutchison, April 1927, 5 pages: 2. F.A.E. Crew did not travel with Dunn to the Soviet Union, but rather Dunn wrote to Hutchison while visiting Crew immediately following his departure from Moscow.

during which he met many Soviet geneticists.⁴² After the Congress he traveled to Moscow.

Dunn followed through on his promise to comment on his impressions to Hutchison and the IEB and wrote two reports. In one, he discussed Great Britain, Denmark, Norway, and Germany (8 pages, dated 26 January 1928) and in the other, the Soviet Union (7 pages, dated 2 November 1927). Progress that was being made in the Soviet Union impressed Dunn phenomenally and in his excitement, he penned a glowing endorsement of what was being called the Soviet experiment immediately after his departure from Moscow. Dunn's report to Hutchison about the other four countries would be written almost three months later.⁴³

Dunn's positive perspective on the Soviet Union reflects an accurate portrayal when compared to historical surveys of this volatile period marked by revolution and modernization. Russians initiated two revolutions in 1917 resulting in the overthrow of the monarchy and establishment of Communism with Vladimir Lenin at the helm. The next three to four years, from 1917 to 1921, were defined by civil war. As the fighting subsided, the New Economic Policy went into effect lasting from 1921 to 1928. This period in the Soviet Union has been characterized as the most peaceful era between the 1917 revolutions and the end of Josef Stalin's reign in 1953.

⁴² APS, Dunn Papers, Series I, Box 18, Walter Landauer, Dunn writing from Dahlem to Landauer, 23 Sep 1927.

⁴³ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928, Report on Russia, 2 Nov 1927, 7-pages; Report on Great Britain, Denmark, Norway, and Germany, 26 Jan 1928, 8-pages.

The New Economic Policy devised a dual system based on socialistic and capitalistic economies in hopes of recovering from several years of revolution and the war that had begun in 1914 with World War I. Famine in 1921 and 1922 further exasperated the situation and led to importation of tractors and other Western machinery in the early 1920s in an effort to improve farming techniques and crop yields. Scientists, such as those that Dunn met in Moscow, experienced their best years during the New Economic Policy period. Funding from the government went to the Russian Academy of Sciences and reached scientists without consideration of their political leanings, nor were scientists forced into allegiance to Marxism. Historian Loren Graham has noted that the 1920s were also a period of cultural change in the Soviet Union, in which the younger generation came to resent the older generation of bourgeois scientists. In addition, nationalism grew during the decade and created skepticism about those who maintained contacts in other countries; in other words, scientists' internationalism was slowly becoming a suspect activity. In the years after Josef Stalin's ascension to power in 1928, nationalism and Marxism increased in importance and commitment to the Soviet Union often took precedence over scientific truths.⁴⁴

⁴⁴ I am indebted to Professor William Husband, who allowed me to audit two of his history courses on the Soviet Union (Science and Society in Russia and Stalin and Stalinism). These classes provided me with the background I needed in order to gain important historical context for this project. Catherine Evtuhov and Richard Stites, A History of Russia: Peoples, Legends, Events, Forces since 1800 (Boston: Houghton Mifflin Company, 2004). Chapters 15-19 of Evtuhov and Stites were consulted. Thomas P. Hughes, American Genesis: A History of the American Genius for Invention (New York: Penguin Books, 1989: 249-94; David Joravsky, Soviet

Although a peaceful period, the New Economic Policy years were far from luxurious for the Soviets, which Dunn witnessed to a certain extent. He noticed the simple living conditions of three Soviet researchers whose homes he visited.

Serebrovsky with a wife and three children has one room and lives about as an American day laborer. [S.S.] Tschetverikoff with his wife, mother, brother, and sister-in-law shares, with a workman's family of six, a five room apartment. Koltzoff has three rooms at the Institute for his family. All are poor and have no money for the amenities of life but on the whole seem to have adapted themselves to the new order and to be working in harmony with it. They seem to be happy, even though not nearly as comfortable as the American scientist of the same grade.⁴⁵

Their ability to remain content while living in cramped conditions contributed to Dunn's admiration for the Soviet Union and its people. Biologists' achievements made with the barest essentials also informed his views. He described their approach to research as familiar, yet modern and novel. Russian geneticists used *Drosophila* for their investigations, but asked unique questions and developed new techniques: "Their guiding idea is to study the geographic distribution of *Drosophila* varieties and mutants in the wild, and to sample a number of wild populations for type and frequency of mutations." He also expressed his admiration for the people that he had met and their accomplishments: "When one considers the amount and quality of the work which has continued without interruption through the famine years and the difficult conditions of life that now prevail in Moscow, one is continually

Marxism and Natural Science, 1917-1932 (London: Routledge and Kegan Paul, 1961): 65-66; Loren R. Graham, Science in Russian and the Soviet Union: A Short History (1993, New York: Cambridge University Press, 1994): 79-98.

⁴⁵ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928, Dunn to Hutchison, 2 Nov 1927, 7 pages, 3.

astonished.” He, moreover, hoped that the International Education Board would relieve some of the scientists’ needs: “It seemed to me that in Moscow more than in any other place I have visited the investment of a little sympathy and interest and perhaps a little money for fellowships or libraries would yield disproportionately high returns.”⁴⁶ Dunn hoped that the IEB would supply Soviet scientists with modern laboratory equipment to replace their “homemade” machines and augment their libraries with scientific books in German and English and journals from before 1925. Dunn qualified his overwhelming enthusiasm about genetic and agricultural investigations in the Soviet Union to a degree by reporting that he had low expectations prior to visiting Moscow and that the reality of what he saw surprised him.

Nikolai Koltzoff’s Institute of Experimental Biology in Moscow and its *Drosophila* research into population genetics received Dunn’s attention in particular, partly because he spent more time there, four days, than at any other laboratory. An especially bright investigator, S. Gershenson, caught Dunn’s attention while he was in Moscow. About one year after his departure, Dunn worked with Koltzoff to submit an application to the IEB for Gershenson to work in T.H. Morgan’s laboratory that had recently relocated to the California Institute of Technology. Geneticist Hermann J. Muller, who had introduced *Drosophila* as a laboratory specimen while in the Soviet Union in 1922, offered Gershenson a fellowship at University of Texas. Gershenson

⁴⁶ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928, Dunn to Hutchison, 2 Nov 1927, 7 pages. Quotations are from pages 1, 3 and 7, respectively.

received \$1800 to study in Austin and act as Muller's laboratory assistant with the money coming from Muller's Rockefeller Foundation grant, rather than from the one submitted to the IEB.⁴⁷ Gershenson would later translate Dunn and Sinnott's textbook into Russian for use in the Soviet Union.⁴⁸

Dunn spent only nine to ten days in Russia between the end of October and early November 1927, but it was long enough to convince him of the progress being made in the Soviet Union, and he felt compelled to inform others besides Hutchison about his time in the Soviet Union. "The most interesting of course was the people," Dunn wrote to his mother,

I talked mostly with scientists and found much extremely new and interesting work going on. Moscow is a great scientific center especially in exp. [experimental] Biology and genetics. They have worked steadily thru' the dark days of the Revolution and the terrible hunger years of '21-'23, and considering the primitive conditions under which they must live & work they have accomplished remarkable things. The intense and active scientific and artistic life of the city which goes on in unabated vigor is proof that for the Russians at any rate such cultural affairs are as necessary as bread & that they go on when nearly everything else has ceased...I left Moscow with real regret. It is poor & primitive, & in many ways uncomfortable but its spirit and its hospitality have not been equalled [sic] in any city in which I've been. I'm convinced that it is one of the cultural & scientific centers of the world.⁴⁹

⁴⁷ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928, Dunn to Hutchison, 2 Nov 1927, 8 pages; APS, Dunn Papers, Series I, Box 25, USSR-Correspondence with Geneticists, 1926-1930, Dunn to August Trowbridge of the International Education Board, 9 Nov 1928; Box 23, H.J. Muller, 1928-1937, Letters between Muller and Dunn, 7 Nov 1928, 13 Nov 1928, and 24 Jan 1929.

⁴⁸ APS, Dunn Papers, Series I, Box 14, S. Gershenson, 1932-1944, Dunn to Gershenson, 7 Mar 1932.

⁴⁹ APS, Dunn Papers, Series I, Box 7, Harriet A. Dunn, 1918-1947, n.d., Dunn to his mother, 30 Oct 1927.

Dunn's trip in 1927 came immediately prior to the beginning of the Great Break, which marked the first stage of increased regimentation. His experience in the Soviet Union was that of a thriving country devoted to scientific research. He saw a country that was celebrating its tenth anniversary of the October Revolution of 1917 and he witnessed the ever-increasing accomplishments that their struggle had achieved. By the time he left the Soviet Union, Dunn's conversations with Serebrovsky had fired him into a new passion for insuring the future of their discipline of genetics: "We have made a great plan for 1928-1929, drafted an application for the Int. Educ. Board, & prepared a manifesto on the theme "Workers of the (genetics) world – Unite!"⁵⁰ His awe-inspired view of a promising Soviet Union would color his opinions for years to come and influence him greatly. He would later make some minor decisions with major consequences based on his convictions about his Soviet colleagues' abilities and their country's potential without taking into account the political changes that occurred in the Soviet Union after 1927.

After returning from his European trip, Dunn finally accepted one of the many job offers that had come to him since he had graduated from Harvard University in 1920. Castle supported Dunn's decision to decline an offer from Texas (most likely University of Texas at Austin) in 1920 noting that Dunn's connection to Storrs currently gave him better financial and scientific prospects. One of Castle's chief concerns was the remoteness of Texas; most large-scale university, genetic programs

⁵⁰ APS, Dunn Papers, Series I, Box 9, Louis Dunn, 1927, Dunn to Louise, 25 Oct 1927 from the Anikowo Genetics Station in Russia.

were located in the northeastern United States.⁵¹ J.B. Johnston and William A. Riley tried repeatedly to lure Dunn to the Department of Animal Biology at University of Minnesota. Dunn declined their offer for the 1926-1927 academic year, but they remained persistent, and Johnston extended the offer for the next year. Dunn took the year for a sabbatical leave from Storrs and felt compelled to follow their protocols and return for a full year before taking another position. Riley hoped that after Dunn had spent one-year at Storrs that he might reconsider their offer, and Riley told Dunn that he would be willing to not permanently fill the position if Dunn wanted it left open for him. Riley expected Dunn to start in spring 1928 and asked Dunn to notify him if his plans changed.⁵² Dunn's plans did change in the intervening year and a half, and by fall 1928 he declined the offer from Minnesota and instead started his academic career at Columbia University in the City of New York.

⁵¹ APS, Dunn Papers, Series I, Box 4, William E. Castle, 1920-1929, Castle to Dunn, 30 Jun 1920 and 18 Jul 1920. University of Texas acquired Hermann J. Muller in 1920 as an associate professor. See, Elof Axel Carlson, Genes, Radiation, and Society: The Life and Work of H.J. Muller, (Ithaca: Cornell University Press, 1981): 117.

⁵² APS, Dunn Papers, Series I, Box 17, J.B. Johnston, 1926, Johnston to Dunn, 30 Oct 1926 and 22 Nov 1926; Box 24, William A. Riley, 1926-1927, 4 Nov 1926, 23 Nov 1926, and 7 Jan 1927.

Chapter 2

Making Genetics a Credible Discipline, 1925-1935

The early decades of the twentieth century were a precarious time for genetics as a discipline.¹ William Bateson coined the term “genetics” in 1906. By 1910 research had demonstrated that Mendel’s laws applied to plants, animals, and humans. Thomas Hunt Morgan’s research school laid the basis of the chromosomal theory in the mid-1910s, and researchers continued to develop the theory over the next two decades.² By the mid-1920s, geneticists recognized that their field of scientific inquiry was a sub-discipline within biology that unified botany and zoology. Academia, however, had not firmly established genetics within its system. Although

¹ Jan Sapp, Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics (New York: Oxford University Press, 1987): xiv, 32, 45-53. Jonathan Harwood discusses the emergence of genetics as a discipline in the United States in comparison to Germany; however Germany is his main concern. Jonathan Harwood, Styles of Scientific Thought: The German Genetics Community, 1900-1933 (Chicago: The University of Chicago Press, 1993). Historians of genetics take for granted that genetics had disciplinary cohesion almost immediately after the rediscovery of Mendel in 1900. Moreover, historians of American biology state that biologists established their discipline between 1880 and 1920. Genetics was part of the establishment of biology as a discipline, but geneticists had yet to firmly establish genetics as a sub-discipline. For discussion about the biology and discipline building, see: Ronald Rainger, Keith R. Benson, and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988).

² L.C. Dunn, A Short History of Genetics, the Development of Some of the Main Lines of Thought: 1864-1939 (New York: McGraw-Hill Book Company, 1965): 68-9, 87, 139-57.

many campuses had added genetics to their undergraduate curriculum, Harvard, Columbia, and Johns Hopkins Universities were among the hand-full of American academic institutions with programs employing multiple geneticists and offering graduate degree programs. Many geneticists with graduate degrees, moreover, found employment in the agricultural industry in the absence of academic positions.³

To establish genetics as a discipline, its members started a scientific society, the Genetics Society of America, attempted to professionalize one of their main periodicals, Journal of Heredity, and to demarcate between the professionals (geneticists) and the amateurs (eugenicists). Historians have used these parameters in order to demonstrate the processes taken by biologists to form a professional scientific discipline. Tobey Appel in particular mentions the importance of academic departments, graduate programs, societies, and journals for establishing a scientific discipline. She concludes that, “Numerous biological sciences were established in America but no unified science of biology” by the 1920s.⁴ Whereas she ends her investigation in 1923, I extend the time frame to the 1930s in order to corroborate her argument and explain geneticists’ activities as various parts of one process in the establishment of genetics as a unified and credible discipline during the 1920s and

³ Charles Rosenberg, “The Social Environment of Scientific Innovation: Factors in the Development of Genetics in the United States,” No Other Gods: On Science and American Social Thought revised and expanded edition (1967, Baltimore: Johns Hopkins Press, 1997): 211-24.

⁴ Rainger, Benson, and Maienschein, The American Development of Biology. Tobey Appel’s chapter: “Organizing Biology: The American Society of Naturalists and its ‘Affiliated Societies,’ 1883-1923” (87-120). The quotation is from page 113. See also Benson’s concluding remarks in “From Museum Research to Laboratory Research: The Transformation of Natural History into Academic Biology” (76-77).

1930s. Jan Sapp provides a preliminary discussion of the growth of American genetics as a discipline in Beyond the Gene in which he “argued that American geneticists established their authority by forming their own discipline with its own well-defined objectives, techniques, explanatory standards, doctrines, journals, and societies and by restricting their investigations to problems which could be effectively dealt with by Mendelian procedures.” He pinpointed that the rise of American genetics occurred between 1915 and 1932, and considered the establishment of Genetics (1916) and the Genetics Society of America (1932) as the bookends of this process. Sapp’s statements concentrated on American geneticists’ intellectual conceptualization of their discipline, and he did not give an in-depth discussion of the institutional histories.⁵ I agree with his argument and timeframe for the development of American genetics, and as a complement to Sapp’s analysis of the intellectual roots of American genetics, I discuss the geneticists who administratively contributed to building genetics into a recognized discipline in the United States.

Committee work and community networks constitute an important feature for the development and credibility of genetics as a scientific discipline during its formative years. Moreover, the diverging goals of genetics and eugenics became an overarching problem that many geneticists addressed during the same time period. American geneticists eventually managed to establish genetics as a discipline separate from eugenics and they did so by concentrating their energies in two directions, fortifying genetics and destabilizing eugenics. They professionalized their discipline

⁵ Sapp, xiv, 45-53. The quotation is from the introduction (xiv).

by strengthening its architecture through academic societies and journals.

Simultaneously, some American geneticists made a conscious effort to damage eugenicists' social aims by circulating a more complex view of Mendelian genetics than that proposed by eugenicists, as well as by attacking eugenicists' core American organization, the Eugenics Record Office. Scholars typically discuss these simultaneous activities separately, but in actuality the two campaigns reinforced one another.

Today genetics and eugenics are considered two separate disciplines; however, in the early twentieth century they were intertwined. Eugenics was originally considered complementary to genetics and based on the scientific study of human heredity. In the late 1920s eugenics took on a new definition after eugenicists succeeded at legally implementing their ideas. Many of them frequently disregarded and misrepresented the complexities of genetic principles in order to implement socio-political policies. Several geneticists recognized that their discipline could not continue its association with eugenics, and they fought to disconnect genetics from eugenics before eugenicists discredited them and their profession.

Broad scale histories of eugenics, more so than genetics, have received scholars' attention. A number of historians, furthermore, have discussed the changing views among geneticists in the 1920s and 1930s, and typically agree that most of the geneticists who distanced themselves from eugenics did so during this time period. Kenneth Ludmerer explores the dynamics between scientists' and laypeople's interest in eugenics and he demonstrates that geneticists initially began distancing themselves

from eugenics intellectually and then for socio-political reasons. At roughly the same time as American geneticists distanced themselves from eugenics, the general population in the United States started their active support of eugenics. On the whole the public did not re-evaluate their views until after World War II ended. Ludmerer interviewed Dunn and many other geneticists in the course of writing his book, as well as examined primary sources including Dunn's papers at the American Philosophical Society.

Daniel Kevles' In the Name of Eugenics remains one of the most comprehensive analyses on the changing definition of eugenics during the twentieth century. Whereas Ludmerer focuses on the American situation among geneticists, Kevles discusses medical and genetic researchers in the United States and United Kingdom. With reference to timing, Kevles also explains that people's views changed in response to actions taken in Nazi Germany during the 1930s. Kevles focuses on individuals and eugenics in his book, which allows him to adequately evaluate people's changing views of eugenics while also demonstrating that some geneticists held onto their eugenic beliefs.

Elazar Barkan describes anthropologists and biologists' increasing hesitation to use science in order to make racist claims. He also believes that the interwar years signified a time of change and by analyzing public statements, he describes scholars' opinions without noting the scientific basis behind this intellectual shift. Lastly, Garland Allen has stated that scientists' views underwent revision between 1915 and 1940, and he notes that a majority of geneticists joined the anti-eugenic campaign

after 1925. His several articles discuss the role of the Eugenics Record Office and its personnel in publicizing eugenic ideas to laypeople and lawmakers. Allen provides solid historical investigations of the pivotal agencies and people in the American eugenics movement, while also assessing the reasons behind their demise.⁶

My views on timing agree with other scholars' analyses, but my approach differs slightly. I, too, explore the divergence of research-based science from eugenicists' political uses of science; however, I view geneticists' attempts to undermine the integrity of eugenicists' aims as part of their efforts to establish genetics as a viable discipline. Although geneticists did not consciously comment that these two endeavors were connected, I believed that events occurring during the 1920s and 1930s can be better understood as two parts of one process. Geneticists could not build a credible scientific discipline in the presence of eugenics, especially in light of eugenicists' heightening misapplication of genetic principles. By undermining eugenics and its proponents, geneticists fostered the integrity of their discipline.

⁶ Kenneth M. Ludmerer, Genetics and American Society: A Historical Appraisal (Baltimore: Johns Hopkins Press, 1972); Daniel Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (Cambridge: Harvard University Press, 1985); Elazar Barkan, The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars (New York: Columbia University Press, 1992); Garland Allen, "The Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History," Osiris 2 (1986): 225-64, 250-54; Garland Allen, "The Misuse of Biological Hierarchies: The American Eugenics Movement, 1900-1940," History and Philosophy of Life Sciences 5 (1983): 105-28, 118-19.

Dunn's personal stance on eugenics altered over time from that of neutrality to negativity. The basis for his intellectual shift was not the result of a change in his views, but rather a change in the goals of eugenicists and in the definition of eugenics. The general population came to understand eugenics in the terms that eugenicists promoted it, which was a definition that increasingly relied on social application at the expense of scientific investigation. Dunn stated as much in 1935: "Eugenics has come to mean an effort to foster a program of social improvement rather than an effort to discover fact. Those familiar with the field recognize that both purposes are pursued, but the public does not make this distinction..."⁷ Dunn believed eugenics to have two parts to its definition: the study of human heredity and the application of the laws of heredity towards social ends. Throughout his lifetime, he supported scientific research into problems of human heredity. During the late 1920s and early 1930s, he grew to dislike the extent to which eugenicists used scientific laws for political means; and therefore, he made more rigorous statements against eugenics as the movement expanded.

There were three phases to geneticists' intellectual shift. An increased understanding of genetics by the 1920s confirmed that Mendelian laws were more complex than initially perceived, especially when applied to human beings. Secondly, American eugenicists more adamantly pursued policy and sidelined scientific principles starting in the 1920s, which caused geneticists to grow increasingly

⁷ APS, Dunn Papers, Series I, Box 3, Carnegie Committee, 1929-1936, Dunn to John C. Merriam, President of the Carnegie Institution of Washington, 3 Jul 1935.

apprehensive of eugenics. Lastly, in the 1930s international uses of eugenics, especially those employed by members of the National Socialist Party in Germany, solidified concerns that eugenicists had gained too much power guiding policy while continuing to claim that their suggestions were based on biological science. Geneticists' skepticism, hence, arose at first because of scientific reasons and later in response to political factors.⁸

In this chapter, I use Dunn as a focal point while analyzing this well-known shift in geneticists' opinions about eugenics. Furthermore, I explore geneticists' work in support of genetics and to the detriment of eugenics as concurrent aims in an effort to build genetics into a viable academic discipline. I discuss below Dunn's outlook on eugenics as a matter of his personal opinion and as a result of his genetic research. After developing an anti-eugenic stance, he worked to undermine eugenicists' efforts. He marginally succeeded at distributing ideas counter to eugenics, but his greater achievement was that he struck at the base of the eugenicist's edifice. He helped overhaul the Journal of Heredity during the 1930s because the journal's pro-eugenic and slapdash content concerned him and other geneticists. He also contributed to the closure of the Eugenics Record Office. Dunn, for the most part, participated on committees and conducted his own private campaigns addressing these issues with the ultimate goal of fortifying genetics and destabilizing eugenics; however I believe that he is representative of a larger trend among geneticists.

⁸ Ludmerer, 132-33.

A Divide Develops between Genetics and Eugenics

Dunn claimed to hold a “neutral” view of eugenics in the late 1920s. In response to a letter asking him to join the American Eugenics Society, Dunn told Leon F. Whitney, secretary of the American Eugenics Society, that he wished to stay “aloof from organized work in Eugenics” because he had no intention to join an association unless he planned to participate in it. The American Eugenics Society officially started in 1923, but had its origin in the Second International Eugenics Congress, a one-week conference held in New York City in September 1921. Dunn had been a member of the American Eugenics Society during its initial years, resigning his membership in 1925 or 1926. The Executive Committee of the American Eugenics Society persisted in their attempts to woo Dunn into rejoining and elected him unanimously to its Board of Directors in 1931 in their hopes of increasing the number of the Board’s geneticists. Dunn responded politely with a decline, and in stating his refusal asked them to stop soliciting him for membership. Not taking Dunn’s reply as final, Whitney asked him to think the decision over.⁹

Dunn’s distancing himself from the American Eugenics Society demonstrates that he changed his opinion in the years following the Society’s establishment in

⁹ APS, Dunn Papers, Series I, Box 26, Leon F. Whitney, Dunn to Whitney, 27 Nov 1928, and three letters between Whitney and Dunn, 9 May to 19 May 1931. For information about the 2nd International Eugenics Congress, see: Harry H. Laughlin, “Historical Background of the Third International Congress of Eugenics,” A Decade of Progress in Eugenics: Scientific Papers of the Third International Congress of Eugenics (Baltimore: The Williams & Wilkins Company, 1934): 1-14, 3-5; Mark H. Haller, Eugenics: Hereditarian Attitudes on American Thought (1963, New Brunswick: Rutgers University Press, 1984): 74.

1923. Was Dunn's claim that he was neutral about eugenics a tactful or truthful reply? It appears tactful, because immediately prior to his resignation from the American Eugenics Society he changed his perspective from one of neutrality to negativity in response to national developments. International events occurring in the early 1930s solidified his views and made him more outspoken about the problematic aspects of eugenicists' claims. A survey of his scientific publications and private letters provides an understanding of Dunn's personal opinion during the 1920s and 1930s about what eugenics was and should be.

Strong evidence demonstrating the fundamental change occurring in the definition of genetics can be found through an assessment of the Second and Third International Congresses of Eugenics held in New York City in 1921 and 1932, respectively. Charles B. Davenport, who established the Station for the Experimental Study of Evolution and Eugenics Record Office at Cold Spring Harbor, expressed his views during both congresses. In 1921, he argued that good science does not need propaganda, and he promoted both pure and applied scientific research. He also made an appeal for more field workers to help collect data about human beings to complement the laboratory research performed by scientists and physicians. Davenport hoped that experimental investigators would do more work on problems with direct applications to human beings. Overall, he conveyed a strong commitment to gathering as much information as possible and by every means possible in an effort to elucidate problems in human heredity. As president of the Congress in 1932, Davenport focused on the great accomplishments made in propaganda and policy

since 1921. Human genetics still served as a basis to eugenics, and moreover science and medicine had advanced eugenic knowledge through twin studies and hereditary diseases. Davenport's primary concern in 1932, however, lay in getting eugenic information to the masses in order "to secure the adoption and practice of the principles established."¹⁰ In other words, Davenport had come to view the laws of heredity as a solid, foundational doctrine on which eugenics could base its social and political suggestions.

Davenport's views reflect a fundamental change that occurred in eugenicists' concerns during the decade between 1921 and 1932. Eugenicists' confidence increased during the 1920s because they successfully influenced the passage of sterilization laws at the state level and of immigration legislation nationally. Eugenicists had, indeed, already made some headway in getting legislation passed prior to the 1921 International Congress on Eugenics; eighteen states passed laws permitting the sterilization of the unfit (i.e. institutionalized people) between 1905 and 1922. Eugenicists, however, succeeded in making their greatest impact on policy in the years following the second congress. Harry H. Laughlin, superintendent of the Eugenics Record Office, published Eugenical Sterilization in the United States in 1922, which not only allowed him to circulate his eugenic views but also established

¹⁰ Charles B. Davenport, "Research in Eugenics," Eugenics, Genetics and the Family: Scientific Papers of the Second International Congress of Eugenics vol. 1 (Baltimore: Williams and Wilkins Company, 1923): 20-28. Charles B. Davenport, "Presidential Address: The Development of Eugenics," A Decade of Progress in Eugenics: Scientific Papers of the Third International Congress of Eugenics (Baltimore: The Williams & Wilkins Company, 1934): 17-22: 19.

him as an expert on legal policies based on eugenic ideas. Starting in 1923 more states passed laws allowing sterilization. Laughlin often testified as an expert witness in court and at congressional hearings and contributed to the passage of sterilization laws and the racially-based Immigration Restriction Act of 1924. Several factors came into play during the 1920s that influenced policymaking, and thus Laughlin should not be viewed as the only propellant.¹¹ Laughlin, however, as superintendent of the Eugenics Record Office, had close contact with Davenport, and together these two men and their organization guided and promoted the eugenics movement in the United States.

From their institutions at Cold Spring Harbor, Davenport and Laughlin established eugenics as a fairly credible discipline and spearheaded a strong social campaign within the United States. Davenport, who was trained in biology, started the Station for Experimental Evolution in 1904 with funding from the Carnegie Institution of Washington (CIW). This organization studied heredity and evolution of plants and animals, excluding human beings. In 1910 Davenport obtained money from a wealthy widow, Mary Harriman, to establish the Eugenics Record Office (ERO). She supported the Eugenics Record Office until 1918 at which point the Carnegie Institution of Washington financed the ERO for its remaining twenty years. Laughlin, who oversaw the functions of the Eugenics Record Office, was educated in English literature and agricultural studies. He strongly supported eugenicists'

¹¹ Kevles, 102-04; Phillip R. Reilly, M.D., J.D, The Surgical Solution: A History of Involuntary Sterilization in the United States (Baltimore: The Johns Hopkins University Press, 1991): 41-70.

legislative aims and held ample sway at legal hearings. He regularly testified for the implementation of sterilization and immigration laws as practical methods for controlling societal problems. On 31 December 1939 the Eugenics Record Office closed, whereas the Station for Experimental Evolution continues to operate under the Carnegie Institution as its Department of Genetics.¹²

An examination of the papers delivered at the 1921 and 1932 International Eugenic Congresses provides evidence that eugenicists' and geneticists' concerns shifted during these eleven years. Of the over 100 papers delivered at the Second Congress in 1921, many well-known and revered biological scientists spoke, including Sewall Wright, Hermann J. Muller, Leo Loeb, and Dunn. Wright did not mention human beings or speculate on the possible eugenic application of principles gathered from his guinea pig research, whereas most speakers alluded to connections between their research and its meaning for human genetics. Muller discussed his work on mutations in *Drosophila*, which he argued was one factor driving evolution and therefore the mutation theory needed to be a major tenet of eugenic research. Although a definite connection between mutation and human deterioration had yet to be established, Muller felt that the "same general [mutational] thesis undoubtedly applies also to mankind." Through investigations into the effects of mutations on human beings, he hoped that a better understanding would be gained about the rate at which mutations act deleteriously on the human population. Leo Loeb, a pathologist investigating cancer in mice, presented twelve conclusions based on over ten years of

¹² Allen, "Eugenics Record Office," 227-36; Kevles, 50-51, 102.

research and surmised in his final conclusion that studying cancer in mice might provide an understanding of cancer in humans. Dunn gave a preliminary report on his assessment of racial mixture among humans in Hawaii. Although several papers dealt primarily with human beings, Dunn was one of the few geneticists discussing human beings based on human data. An anthropologist had collected the data with the intention that a geneticist would interpret it. More will be said below about Dunn's papers on Hawaiians and Hawaii's hybrid populations.¹³

In comparison to the numerous papers delivered by geneticists in 1921, few geneticists attended the next Eugenics Congress in August 1932. The Sixth International Congress of Genetics was also held in New York State (in Ithaca) and it started the day after the Third International Congress of Eugenics ended.¹⁴ Only a few geneticists, however, made the effort to present papers at the eugenics congress. Hermann J. Muller, a known supporter of eugenic ideas throughout his lifetime, presented a passionate paper on "The Dominance of Economics over Eugenics," in which he shared his opinions about the aims of eugenics. Muller's speech was not

¹³ All papers are found in Eugenics, Genetics and the Family: Scientific Papers of the Second International Congress of Eugenics vols. 1-2 (Baltimore: Williams and Wilkins Company, 1923). Sewall Wright, "The Effects of Inbreeding on Guinea Pigs," vol. 1, 266-69; H.J. Muller, "Mutation," vol.1, 106-12; Leo Loeb, "The Inheritance of Cancer in Mice," vol. 1, 182-85; L.C. Dunn, "Some Results of Race Mixture in Hawaii," vol. 2, 109-24. C.H. Danforth discussed human heredity in "The Frequency of Mutation and the Incidence of Hereditary Traits in Man," (vol.1, 120-28) and C.C. Little used data from the Eugenics Record Office to assess "The Inheritance of a Predisposition to Cancer in Man" (vol. 1, 186-90). Several physicians gave papers on human inheritance of mental and physical attributes.

¹⁴ The Third International Congress of Eugenics was held at the American Museum of Natural History in New York City from 21 to 23 August 1932. The Sixth International Congress of Genetics was held in Ithaca from the 24 to 31 August 1932.

scientific, nor was it pro-eugenic in the American sense of the movement at this time. Muller, according to his biographer Elof Axel Carlson, pointed out that American eugenicists relied on a simplified definition of the unfit and needed to consider the effects of other factors, such as environment and economics. Carlson captures the thrust of Muller's argument and some of its provocative features, but downplays his pro-eugenic statements. In light of the economic depression facing Americans, Muller argued that society had to undergo a fundamental restructuring before citizens could make educated eugenic decisions:

Only the impending revolution in our economic system will bring us into a position where we can properly judge, from a truly social point of view, what characters are most worthy of a man, and what will best serve to carry the species onward to greater power and happiness in a united struggle against nature, and for the mutual betterment of all its members.

As this passage shows, Muller ultimately advocated for a socialistic society. Carlson notes that this speech marked Muller's public admission of socialist sympathies and that it received ample attention from the press. What Carlson avoids is a discussion of Muller's eugenic vision. Muller promoted sterilization of the unfit at the outset of his speech: "That imbeciles should be sterilized is of course unquestionable." He ended by commenting on the promises that eugenics could contribute to a socialistic society: "The possibilities of the future eugenics under these conditions [i.e. socialism] are unlimited and inspiring."

Although Muller promoted eugenics, he did so in a manner unacceptable to the Congress's organizers, who attempted to impair the effectiveness of Muller's

speech in the two months before the Congress by cutting his speaking time short. After seeing a copy of Muller's talk, Davenport, one of the main organizers and president of the Congress, reduced Muller's allotted speaking time from one-hour to fifteen minutes, and then down to ten minutes. Indignant about the rebuff, Muller asked Dunn and other geneticists for help. Dunn obliged by writing to Davenport, who replied that Muller's talk "had very little of significance for applied eugenics." Although his letter cannot be consulted, it appears, based on Davenport's response, that Dunn disagreed with the Congress's disregard for free speech, especially when the ideas countered those of the Congress's organizers. Muller eventually received a fifteen minute slot.¹⁵

Dunn's Views in Print: Genetic Research and Eugenic Statements

A major change in geneticists' views about eugenics occurred between these two International Congresses on Eugenics of 1921 and 1932, and Dunn also underwent this transition. As mentioned, Dunn ideas about genetics did not change so much as the definition of eugenics. Nonetheless, the scientific work that he conducted

¹⁵ H.J. Muller, "The Dominance of Economics over Eugenics," A Decade of Progress in Eugenics: Scientific Papers of the Third International Congress of Eugenics (Baltimore: The Williams and Wilkins Company, 1934): 138-44, 143; APS, Dunn Papers, Series I, Hermann J. Muller, 1928-37, 3 letters to Dunn: Muller, 21 Jun 1932; Davenport, 24 Jun 1932; Muller, 7 Jul 1932; Elof Axel Carlson, Genes, Radiation, and Society: The Life and Work of H.J. Muller (Ithaca: Cornell University Press, 1981): 178-81. Carlson cited Muller's letter to Otto Mohr, but does not mention if Mohr helped Muller.

during the early 1920s solidified his opinion that eugenicists interpret Mendelian laws too strictly.

While at Storrs Agricultural Experiment Station, Dunn conducted postmortems on chicken embryos in order to learn the cause of death. He assessed the vigor of poultry through chick “hatchability,” i.e. whether or not a chick hatched from its egg. Acknowledging that his work had practical and scientific benefits, Dunn explained that although embryonic deaths could result from various genetic and environmental reasons, most died due to genetic abnormalities. Dunn’s explanation that heredity caused most of these deaths went against the common view held fifteen years earlier when breeders believed that they could increase the number of chicks hatching by manipulating the environment in which the eggs and hen lived. In his investigations, Dunn found that hatchability decreased with inbreeding and recognized that breeders typically searched for one cause for fewer eggs hatching. Dunn, however, warned agriculturalists against blaming all deaths on “vigor” because “vigor” was an ambiguous and loaded explanation that lumped together several factors. Over 3,000 postmortems had shown that in reality chick embryos died with a variety of deformities such as crossed beaks, missing parts (eyes, wings, and heads), a malformation of a neck muscle (*musculus complexus*), and exposed brain matter.¹⁶

Two of the abnormalities that Dunn had observed were lethals that adhered to Mendelian laws. Lethals (or lethal factors) is a term used to describe homozygous

¹⁶ L.C. Dunn, “Standpoint of Genetics,” *Scientific Agriculture* 4 (1923): 1-7, 4-6. The *musculus complexus* is the neck muscle that allows a chick to move its head and break open the shell.

embryos that formed, but died before hatching. He referred to one of these Mendelian abnormalities as “parrots,” because their deformities made them look like parrots more than chicks (shortened head, beaks, and legs). Progeny from a known parrot-producing cross yielded nineteen normal and five parrot chicks, which Dunn pointed out was almost exactly a three to one ratio. After the male used to produce the parrot chicks died, he bred the hen with a different male (the first one’s brother). This mating birthed thirteen normal chicks and convinced Dunn that recessive genes from the hen and her first male partner combined to produce lethals. Dunn noted that inbreeding increased the chances of producing lethal chicks resulting from the inheritance of recessive genes, whereas another investigator, Raymond Pearl, found that better hatchability was gained by out-crossing chickens. Dunn, moreover, noted that his breeding experiments with poultry yielded similar results to those observed among corn, rats, and guinea pigs. Hybrid corn experiments had been conducted by E.M. East and D.F. Jones, two members of the Bussey Institution, who conducted this work at Storrs Agricultural Experiment Station. Dunn had firsthand knowledge of East and Jones’ research because of his association with each man and both facilities. Dunn concluded by bringing attention to the potentiality of examining embryos and performing postmortem investigations and described embryology in poultry science as “a field of research which is almost untouched and full of promise.”¹⁷

¹⁷ Dunn, “Standpoint of Genetics,” 6-7. For information about East and Jones, see Diane Paul and Barbara Kimmelman, “Mendel in America: Theory and Practice, 1900-1919,” The American Development of Biology, eds. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press,

In reference to Dunn's statements about eugenics, several of his publications from the 1920s and 1930s can be consulted. In 1925 Dunn and Sinnott published the first edition of Principles of Genetics and included within it a chapter about eugenics. (The second edition was published in 1932 without the section on eugenics.)¹⁸ They defined and discussed eugenics "as the eugenicists would have it," according to William E. Castle, Dunn's mentor. Castle's assessment is accurate, but a bit misleading. Indeed, Dunn and Sinnott presented eugenics as defined by eugenicists. Moreover, they mentioned the unworthiness of mentally (but not physically) unfit individuals, such as the feeble-minded, paupers, and criminals. The following is one of several similarly phrased statements, "At the bottom of the human scale stands a group of individuals who are so poorly endowed that almost everyone would agree that the elimination of their stock from society would be desirable."¹⁹ Although Dunn and Sinnott recognized a need to purify the human population, they did not endorse eugenicists' practical applications wholeheartedly and without reservations. They felt that more should be learned scientifically before enacting drastic legal measures. Since they foresaw the potential benefits of eugenic methods for curbing those lesser

1988): 281-310, 296-301. William E. Castle and C.C. Little pioneered research into lethal factors at the Bussey Institution during the years following 1910 (L.C. Dunn, "William Ernest Castle, October 25, 1867-June 3, 1962," Biographical Memoirs 38 (Washington, D.C.: National Academy of Sciences, 1965): 33-80, 46.

¹⁸ Albert F. Blakeslee, "Principles of Genetics; A Text-book, with Problems; Recent Advances in Plant Genetics," Science 77 (17 Mar 1933): 284-85.

¹⁹ Edmund W. Sinnott and L.C. Dunn, "The Problems of Eugenics," Principles of Genetics (New York: McGraw-Hill Book Company, Inc., 1925): 402-17. Quotation about undesirables is from page 404, see also 406. W.E. Castle, "Some New Books on Genetics," Science 62 (18 Dec 1925): 567-69.

fortune within the population, Dunn and Sinnott told the students reading their textbook to “endeavor to assist in stimulating a thoughtful interest in the problem in order that its solution may be approached in a sane, unprejudiced and intelligent fashion.” Eugenic methods depended upon understanding genetics and its effects on democracy, war, immigration, and race. At the end of their chapter, Dunn and Sinnott noted that eugenics was one of many solutions to social problems and recognized that some of its proponents had gotten too extreme in their eugenic proposals and had forgotten that there are alternative tactics.²⁰

Dunn and Sinnott promoted an educated approach to eugenics, which was the vision that Dunn continued to share, and at about the same time Dunn related knowledge about Mendelian genetics of plants and mammals to clarify misinformation about human heredity circulating among the general population. His views in a speech delivered at a conference of the American Sociological Association in December 1924 are quite anti-eugenic. Dunn likely felt more comfortable relaying a radical point of view in a speech, than in a textbook that had the potential for high distribution. In his talk, Dunn addressed common fallacies about racial purity, immigration, and race antagonism. The status of genetic knowledge had by this time forced scientists to accept that both inheritance and environment shape a person’s development. Moreover, a black and white view of genetics failed to take into

²⁰ Sinnott and Dunn, Principles of Genetics (1925). Suggestion to students is on page 407.

account more recent evidence that proved Mendelian genetics had gray areas. Dunn unequivocally shared his interpretation in light of increasing data:

This more modern view of heredity, which is in accord with the most recent biological research, is more flexible, less dogmatic and lends less countenance to the fatalistic philosophy which either sponsored or was invoked by hasty conclusions from a partial knowledge of Mendelian inheritance.²¹

Dunn bolstered the chromosomal theory in which genes were considered to be discrete, unchangeable units that interacted with the environment and other genes. The expansion of data about hybrid vigor and the mechanisms guiding biological inheritance allowed Dunn to discredit common statements often made about racial mixing. Dunn asked, “Are human hybrids more vigorous or less than the parent types? Are they under any biological handicaps such as infertility? Are the new combinations of characters in hybrids disharmonious or incompatible?” His response: “Dogmatic answers can certainly not be given from the human data.”²² In other words, even though more information continued to be learned, there were still many unanswerable questions.

Dunn had no qualms about applying genetic knowledge about lower animals to human beings, and he applied his understanding of plant and animal hybrids to examples of racial mixing among humans. Drawing from experiments performed on animals by mouse geneticist C.C. Little, Dunn opined that pure breeding usually created numerous problems in comparison to intermixing. Little had been a student of

²¹ L.C. Dunn, “A Biological View of Race Mixture,” The Trend of Our Civilization: Publications of the America Sociological Society 19 (1925): 47-58, 48.

²² Dunn, “A Biological View of Race Mixture,” 54.

William E. Castle's before Dunn, and as a student at Harvard University, Little inbred mice with the intention of developing a pure line.²³ In reference to a common belief about racial mixing, data had not yet demonstrated that marriages between two people of the same race yielded superior children and that racial mixing created inferior progeny. Instead, Dunn stressed the need to think of each couple individually rather than to attempt to make stereotypical assessments based on race. Moreover, like the causes of death among lethal chicken embryos, Dunn believed that several factors merged when members of a race were grouped together under one ambiguous racial label: "The large problem of race mixture will become progressively broken up into single problems which will deal with each case of race mixture as a special case."

Dunn concluded that races are mentally and physically different, and then turned the common argument on its head. Instead of saying that racial differences create problems, Dunn suggested that human diversity was a factor contributing not only to biological evolution, but also to social evolution. Variation enhanced and drove human society in Dunn's view.²⁴ He continued to promote the social benefits of a biologically diverse population in his later publications on race including Heredity, Race and Society (1946).²⁵ In his statements about eugenics, most of which he made after this 1924 paper, Dunn presented similar notions about the status of scientific

²³ Dunn, "A Biological View of Race Mixture," 54; Karen A. Rader, Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955 (New Jersey: Princeton University Press, 2004): 37-40.

²⁴ Dunn, "A Biological View of Race Mixture," 47-58. Quotation is from page 56.

²⁵ L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society (New York: Mentor Books, 1946): 114-15.

knowledge of human heredity and warned people against deciding policy based on eugenicists' misapplication of genetics. In this lecture, however, Dunn is unusually direct and opinionated, and his speech reflects the meeting's theme, "The Trend of Our Civilization."

Personal experience assessing morphological data on human beings also helped to inform Dunn's knowledge about human heredity. He performed his first scientific analysis of a human population during the 1920s, which introduced him to methods in anthropology and human genetics. Anthropologist Alfred M. Tozzer had visited Honolulu during the summers of 1916 and 1920 to measure Hawaiians, Caucasians, and Chinese. Although the publications did not appear until after the mid-1920s, physical anthropologist E.A. Hooton, one of three renowned physical anthropologists in the United States during the first half of the twentieth century, had asked Dunn to participate on the project while he was a student at Harvard University.²⁶ Dunn helped Tozzer with the investigation by analyzing the data. He devised charts and reported extensively on Tozzer's measurements, but did not go to Hawaii to collect data. Thus, Tozzer measured and Dunn reported on body form (height, weight, arm span) and head and facial features (head shape, nose shape, thickness of lips, hair color). From sufficient data Hooton wanted Tozzer and Dunn to assess fertility, longevity, and mental capacity on pure and mixed races. Due to

²⁶ Stanley M. Garn and Eugene Giles, "Earnest Albert Hooton, November 20, 1887-May 3, 1954," Biographical Memoirs 68 (New York: National Academy of Sciences, 1995): 167-179, 175. Franz Boas and Ales Hrdlicka were the two other physical anthropologists.

inadequate records, generalizations could not be made, but a thorough analysis was performed nonetheless. A main goal was to ascertain the effect of racial mixture on physical appearance and mental facilities, as well as to learn about human heredity. The bulk of Dunn's publication described the typical, pure native Hawaiian and then compared Hawaiians to Caucasians and Chinese, as well as discussed hybrids formed through racial mixture from the mating of individuals from two different races.

When presenting his conclusions, Dunn explained that the vast variation found in their sample of Hawaiians was the result of partial data. Anthropologists at this time commonly conducted research in order to succinctly describe racial types. Dunn realized that his lack of data did not allow him to present a permanent racial standard, but he did outline the typical features among Hawaiians for the first time. He believed that more data would add clarity to his attempt to physically describe the Hawaiian race. In other words, he thought that a racial definition for Hawaiians could be made once there was enough information.²⁷ Dunn learned first-hand about anthropology and human genetics during his analysis of this data, and most likely the experience caused him to think not only about his own views about eugenics, but also those of others. E.A. Hooton wrote several popular books over his lifetime. Some promoted eugenic ideas and others did not. In Up from the Ape (1931), Hooton presented a rather progressive view of eugenics because he argued that evaluations

²⁷ L.C. Dunn, "An Anthropometric Study of Hawaiians of Pure and Mixed Blood," Papers of the Peabody Museum of American Archeology and Ethnology 11 (1928): 91-211, 91-129. A preliminary report was given seven years earlier in 1921 at the Second International Congress of Eugenics.

about the abilities of races had been made prematurely, before undertaking a “serious experimental investigation.” He proffered a pragmatic approach, reminiscent of Dunn’s contention, because he believed that scientific evaluations of races had to be performed before making judgments. Moreover, Hooton had not made up his mind and awaited scientific evidence about whether or not races were inherently unequal.²⁸ Six years later in 1937, he published Apes, Men, and Morons, and its contents indicate that he had come to view races as inherently different. He understood that racial propaganda kept anthropologists and geneticists from supporting eugenics; yet, he promoted negative eugenics through sterilization and birth control because he believed its benefits to outweigh its costs.

However vague and incomplete may be our knowledge of human heredity, and however potent the influence of environment may be, I cannot see that the status of man can be lowered by preventing inferior human animals from propagating, even under the improbable assumption that the resulting offspring will not be like their parents, but will be an improvement upon them.²⁹

These two disciplines, anthropology and human genetics, remained important to Dunn’s interpretation of social and scientific problems for years to come.

Although Dunn had interpreted data on human beings, he did not consider himself to be an expert on human heredity whereas his colleagues did. Botanical geneticist Alfred F. Blakeslee asked Dunn to speak about “the future of man in the light of his past” at the upcoming meeting of the American Society of Naturalists.

²⁸ Earnest Albert Hooton, Up from the Ape (New York: The Macmillan Company, 1931): 395-7, 591-97, 605. The quotation is from pages 594.

²⁹ Earnest Albert Hooton, “The Eugenics Bogy,” Apes, Men, and Morons (1937, New York: Books for Libraries Press, 1970): 229-36, 236.

Dunn declined because he doubted his ability to comment on the topic and suggested H.S. Jennings instead.³⁰ Two years later H.J. Muller, as vice-president of the American Society of Naturalists, wanted Dunn to take part in a session on “Heredity and Environment in Man.” Muller thought that Dunn was the “logical person to treat heredity in man—what kind of evidence is available, what characters are known to be inherited and in what manner.” Dunn again declined. This time he stated that one month was not enough time to prepare a paper on human heredity.³¹

In the years immediately following his partial analysis of Hawaii’s population, Dunn used the data in a small textbook, Heredity and Variation, in which he brought together plant, animal, and human genetics. Dunn also based Heredity and Variation on his larger textbook Principles of Genetics, yet he geared the scientific content in Heredity and Variation to students in non-science fields. Published in 1932, Dunn in this small textbook laid out the major points about genetics and throughout the book he drew on knowledge from botanical, animal, and human genetics to demonstrate that Mendel’s laws apply to all living beings. In the last chapter, he thus explores genetic contributions to improving crops, livestock, and human beings. Combining a discussion of hybrid corn and of progeny from a mixed Chinese-Hawaiian marriage, Dunn explains that hybrids usually demonstrate vigor and that breeders use the

³⁰ APS, Blakeslee Papers, B: B585, L.C. Dunn #1, Blakeslee to Dunn, 28 May 1930; Dunn to Blakeslee, 29 May 1930.

³¹ APS, Dunn Papers, Series I, Box 26, Alexander Weinstein, 1930-32, 1964, Weinstein to Dunn, 24 Nov 1932 and Dunn’s undated, handwritten note on Weinstein’s letter: “Unable to prepare Human heredity paper for symposium. Letter follows.”

technique to produce “bigger and better” results. Accordingly, Chinese-Hawaiians are physically and mentally superior human beings. Dunn warns against inbreeding because of its negative effects as displayed by marriages between cousins. Inbreeding increases the possibility that an offspring inherits a homozygous recessive trait, which as a rule is the most lethal combination for progeny to receive from parents. In most cases, Dunn mentioned some exception to the rule; for example, inbred rats can produce good and bad offspring.

In his discussion about eugenics Dunn divided the discipline into two parts, scientific research and its social application. He believed that biological eugenics, defined as the study of human heredity, was a valid enterprise that confirmed the extension of Mendel’s laws to human beings. Eugenic investigations had gathered and circulated valuable information about inheritance. For example, eugenicists demonstrated links between heredity and illness, such as some forms of epilepsy and insanity, and brought attention to the ill-effects of war, late marriage, and other socially-based decisions on the genetic constitution of the human population. The implementation of genetic laws in an effort to remedy social problems, however, required caution. Understanding heredity was tricky because traits segregated in simple and complex fashions. One could not undoubtedly forecast which couples would produce physically and mentally handicapped children, but one could surmise quite accurately in the case of some couplings: “Marriages between feeble-minded persons produce, with a few doubtful exceptions, only feeble-minded children.” On the other hand, many normal people unwittingly bear defective children. Dunn

worried about people's individual freedoms as a result of eugenicists' social successes such as passing laws to sterilize mentally impaired human beings, but he stated that all freedoms had not been lost, yet.

Those who recognize the nature of the biological factors involved but who still feel that the freedom of the individual is a good to be preserved may still hope to prevent the irruption of social, racial, and religious prejudice into the direction of this difficult and delicate problem.

In order to make educated decisions about social ills, citizens had to ask geneticists and eugenicists for advice and must not act hastily.³² A consistency among Dunn's statements is his belief in eugenics as a biological enterprise, and his cautious attitude towards socially applying eugenic solutions.

An important facet to recognize about Dunn's work on human beings and human races is that he used anthropological and scientific practices that were common in the 1920s, but he held irregular views about the interpretation of that data. His scientific investigations into hybrid vigor caused him to promote novel views not only among scientists, but also the general population. Dunn acknowledged that his view about hybrids demonstrating vigor was well-supported in folklore about animal breeding and human marriage: "That violent opinions have been held about inbreeding, or the mating of close relatives, is shown by the laws and customs forbidding the practice in civilized societies, and by the continual disagreement on

³² L.C. Dunn, Heredity and Variation: Continuity and Change in the Living World (New York: The University Society, Inc., 1932): 92-113. Quotation about the feeble-minded is from page 106.

this question among animal breeders.”³³ Nonetheless Dunn’s was a highly uncommon perspective when he promoted it.

A personal factor that undoubtedly contributed to Dunn’s developing disdain of the application of eugenic policies and likely influenced the statements he made in Heredity and Variation came about between 1928 and the early 1930s. Louise, Dunn’s wife, gave birth in 1928 to their second son, Stephen, who suffered from cerebral palsy. Throughout his lifetime, Stephen dealt with severe physical handicaps even though he had a sharp mind. Dunn and Louise knew that something was seriously wrong with Stephen by March 1929, but doctors had not yet provided an accurate diagnosis.

This has been a most disorganized and unhappy month – Stephen and Bob have both been ill – we took Stephen to the Babies Hospital for observation but have no diagnosis yet after ten days – altho’ he only stayed two days & seems better now. Whether it’s only a “normal” retardation in development or has some specific nervous disorder back of it we don’t know yet & it’s been most trying for Louise.³⁴

Dunn and Louise took Stephen to clinics hoping that he would someday walk and by the age of five Stephen underwent the first of many surgeries aimed at providing him with better muscle control. This particular surgery failed, forcing Dunn and Louise to go back to their “original method of building up new nerve paths by training and have

³³ Dunn, Heredity and Variation, 98.

³⁴ APS, Dunn Papers, Series I, Box 18, Walter Landauer, 1929, Dunn to Landauer, 21 Mar 1929; L.C. Dunn married Louise Porter in May 1918. She had attended Smith College in Northampton, Massachusetts. Theodosius Dobzhansky, “Leslie Clarence Dunn, November 2, 1893-March 19, 1974,” Biographical Memoirs 49 (Washington, D.C.: National Academy of Sciences, 1978): 79-104, 81.

to give up the hope of effects from canceling the opposing impulses.”³⁵ Stephen would use a wheelchair throughout his life and required constant aid from one or two people well beyond his formative years. Dunn and Louise took on a large portion of these daily tasks, such as feeding him and exercising his muscles.

Dunn’s Committee Work: Fortifying Genetics and Destabilizing Eugenics

In addition to his statements in print, Dunn participated on a handful of committees that helped to fortify genetics and destabilize eugenics. He privately offered his viewpoint on eugenics’ problematic aspects to John C. Merriam, president of the Carnegie Institution of Washington, when asked for his opinion about the future of the Eugenics Record Office. In 1929 and 1935 Dunn acted on an advisory committee for the CIW that evaluated the usefulness of the data collected by the Eugenics Record Office. The 1929 committee made conservative recommendations because Charles B. Davenport and Harry H. Laughlin were on the seven-member committee. The 1935 committee had the benefit of Davenport’s retirement the previous year and suggested closing the Eugenics Record Office.³⁶ The committee’s suggestions were eventually enacted and the Eugenics Record Office closed on 31 December 1939.

³⁵ APS, Dunn Papers, Series I, Box 18, Walter Landauer, 1932, Dunn to Landauer, 6 May and 25 Jun 1932.

³⁶ APS, Dunn Papers, Series I, Box 3, Carnegie Committee, 1929-1936, Memorandum for Dr. Merriam re Meeting of Advisory Committee on Eugenics Record Office, n.d.; A.V. Kidder to Dunn, 25 May 1935, Dunn to Merriam, 3 Jul 1935; Merriam to Dunn, 25 Jul 1935; Allen, “Eugenics Record Office,” 250-52; Kevles, In the Name of Eugenics, 355.

Dunn's suggestions in 1935 again reflect his support for eugenics as a method of scientific inquiry and his concerns about eugenicists' ever-increasing misapplication of science based on personal motive. A.V. Kidder, Chairman of the Division of Historical Research of the Carnegie Institution, had written the evaluation report for 1929 and asked Dunn to act on the second committee with E.A. Hooton, Robert Redfield, and A.H. Schultz. Kidder, Hooton, Redfield and Schultz were all anthropologists, thus making Dunn the only geneticist on the committee. Like Dunn, Hooton, Redfield and Schultz were professors at prestigious universities. Hooton, as mentioned above, was at Harvard University. Redfield and Schultz were associate professors at University of Chicago and Johns Hopkins University, respectively.³⁷ Since Dunn was on sabbatical in Oslo, he sent a letter expressing his opinions about eugenics, eugenicists, and the Eugenics Record Office, and he did not attend the meeting held in August 1935 at the facilities in Long Island.

Dunn knew how best to convey his opinions. Instead of replying to Kidder, he addressed his letter to John C. Merriam, President of the Carnegie Institution of Washington. The following points are a summary of that letter: Serious scientists had recently distanced themselves from eugenics because "eugenical research was not always activated by purely disinterested scientific motives, but was influenced by social and political considerations tending to bring about too rapid application of

³⁷ For information on each man's academic affiliations, see: J. McKeen Cattell and Jaques Cattell, eds., American Men of Science 5th ed. (New York: The Science Press, 1933). Their sketches are found on the following pages: Hooton, 533; Kidder, 613; Redfield, 916; Schultz, 986.

incompletely proved theses.” Eugenics needed cross-disciplinary input on human heredity gathered from researchers in several fields, such as genetics, medicine, anthropology, and experimental psychology. Eugenics, moreover, must act by the same rules that guide other sciences. Those collecting the data should be trained in experimental and quantitative analysis. Furthermore, formal methods for researching, publishing, and authenticating eugenic investigations should precede application of eugenic suggestions. If eugenics follows the example set by other scientific disciplines, then the application of its principles might one day be adopted.

Dunn did not explicitly advise Merriam to close the Eugenic Records Office, but he did question its employees’ motives and he proposed relegating its eugenic research to an institution whose primary concern was scientific research on human heredity. As mentioned, Dunn was writing from Norway. While overseas in 1935 Dunn saw the effects of the National Socialist Party because he visited Germany. He discussed his personal impressions in order to convey to Merriam the potential consequences of putting program before discovery and of not taking new scientific developments into account. Dunn was quite pessimistic about the future, but hoped that Americans would learn a lesson from the events unfolding in Nazi Germany and that Americans would “keep all agencies which contribute to such questions as free as possible from commitment to fixed programs.”³⁸ In addition to witnessing the effects

³⁸ APS, Dunn Papers, Series I, Box 3, Carnegie Committee, 1929-1936, Dunn to Merriam, 3 Jul 1935.

of Nazism firsthand, Dunn also kept abreast of political developments because he aided German refugees starting in 1933. This topic is the basis of the next chapter.

President Merriam valued Dunn's comments and agreed that interdisciplinary scientific research should serve as the foundation of eugenics; however, Merriam did not allude to his feelings about the fate of the Eugenics Record Office and the Carnegie Institution of Washington's future commitment to eugenics.³⁹ The other members met while Dunn was in Oslo on sabbatical, yet they independently came to similar conclusions as those expressed by Dunn. "When you see the report of the Committee," Kidder told Dunn, "you will realize how admirably your communication backed up our recommendations. Just what will be done by the Institution in this situation is problematical, but at least we are all on record and very definitely as to what ought to be done."⁴⁰

Merriam did not act on the suggestions he received in 1935; however his successor, Vannevar Bush quickly facilitated the closure of the Eugenics Record Office. Bush started as Director of the Carnegie Institution in January 1939 and at the end of December that same year the Eugenics Record Office closed. Bush recognized the problems caused by Harry H. Laughlin's promotion of eugenics, and therefore asked Laughlin to retire by telling him that eugenics contradicted the aims of

³⁹ APS, Dunn Papers, Series I, Box 3, Carnegie Committee, 1929-1936, Merriam to Dunn, 25 Jul 1935.

⁴⁰ APS, Dunn Papers, Series I, Box 3, Carnegie Committee, 1929-1936, Kidder to Dunn, 1 Aug 1935.

objective scientific investigation.⁴¹ After the Eugenics Record Office closed, genetic research continued at the Cold Spring Harbor laboratories and its geneticists cultivated strong relations with researchers at Columbia University throughout Dunn's tenure.

The bulk of this chapter thus far has been an exploration of Dunn's efforts in connection with eugenics and the genetic research informing his views; however, Dunn also attempted to establish genetics as a discipline in the United States at roughly the same time. His efforts concentrated on three projects. The National Research Council's Committee of Experimental Plants and Animals attempted to establish a storehouse for specimens with unique genetic attributes. The Journal of Heredity needed revamping to save it from disrepute and financial crisis. The Joint Genetics Sections of the American Association for the Advancement of Science reorganized to become the Genetics Society of America. Dunn acted on or chaired committees that addressed each of these issues. The timeframe for Dunn's participation on these disciplinary committees coincides with his decision to move to Columbia University and his initial five years there. He arrived in New York in fall 1928 as a professor in the Department of Zoology and by 1933 international issues increasingly preoccupied his energies.

⁴¹ G. Pascal Zachary, Endless Frontier: Vannevar Bush, Engineer of the American Century (New York: The Free Press, 1997): 89-93. Others have mentioned Dunn's role in bringing an end to the Eugenics Record Office. See: Allen, "Eugenics Record Office," 250-52 and Reilly, 69-70.

While acting on the Advisory Committee of the National Research Council in 1928, Dunn was asked for topics to be discussed at the upcoming meeting. Dunn proposed to William Crocker, the Council's Chairman of the Division of Biology, that they discuss "providing an adequate supply of experimental animals and plants for investigators, especially in genetics..." In particular, Dunn wanted to preserve mutant plants and animals that could be used in genetic research. He mentioned that in Thomas Hunt Morgan's laboratory at Columbia University, one person's full-time job was to keep and maintain mutant strains of *Drosophila*. Dunn, in comparison, sometimes obtained mice and other specimens from foreign countries. He conveyed the direness of the situation to Crocker by providing the following example about Norwegian sheep recently acquired by Storrs Agricultural Experiment Station:

The problem has been brought home to me several times in the past few years by inability to get in this country certain stocks of mice which are necessary in genetic work; and most recently by an experience which we have had in obtaining some ancon sheep from Norway. We wished to have available for study the only representative of this peculiar variation which are [sic] known: namely, of the descendents of one mutant ewe which was found in northern Norway several years ago. We tried to induce several zoological gardens to purchase these sheep for preservation, but did not succeed, and were forced to buy them ourselves in order to prevent their possible extinction.⁴²

⁴² APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, 1928, Dunn to Crocker, 31 Mar 1928.

Crocker supported Dunn's suggestion, and at the Council's next meeting on 21 April 1928 attendees adopted resolutions that put Dunn in charge of formulating a committee to attack this problem of managing biological specimens.⁴³

Dunn worked hard to put together a suitable and diverse membership as well as to devise an appropriate name for the committee. He approached several people including geneticists C.C. Little, R.A. Emerson and Helen Dean King. Additionally, Dunn extended invitations to William M. Mann of the National Zoological Park in Washington D.C., Robert W. Hegner of the School of Hygiene and Public Health at Johns Hopkins University, George R. La Rue of the University of Michigan's Zoology Department, and etymologist F.E. Lutz of the American Museum of Natural History in New York. Originally given an unwieldy name, "Committee for the Study of Problems of Provision and Preservation of an Adequate Supply of Experimental Animals and Plants for Investigation," Dunn shortened it upon request to Committee on Experimental Plants and Animals. By the end of 1928 all of the above people, except Dean, agreed to act on the committee and they had held their first meeting.⁴⁴

Dunn had brainstormed some ideas before solidifying the committee's members and during the first meeting in December 1928 those in attendance (Dunn, Hegner, Little, and Lutz) had devised a plan of action. About seven months before the meeting Mann agreed to act on the Committee, and Dunn immediately communicated

⁴³ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, 1928, Crocker to Dunn 4 April 1928 and 9 May 1928.

⁴⁴ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, 1928, several letters dated May to Oct 1928.

his vision that the National Zoological Park (i.e. Mann's place of business) would be a great stockroom for numerous rodents whose bills would be paid for by the National Research Council. Dunn made sure to note the benefit for Mann: "There is an excellent opportunity for exhibition and public instruction."⁴⁵ Karen Rader discusses C.C. Little's participation on this committee and states that it is unclear whether Dunn or Little formulated the idea of teaming up with the National Zoological Park in Washington D.C. Considering that Little agreed to act on the committee three months after Dunn's letter to Mann, most likely the notion came from Dunn, but it is possible that each man thought of the same idea independently. Mann agreed with Dunn's idea about a rodent house at his zoo and the committee proposed it as one of several suggestions to the new Chairman of the Council's Division of Biology and Agriculture, C.E. Allen of University of Wisconsin. Although Mann initially agreed to the rodent house, at some point he changed his mind and in 1929 began building a facility for reptiles instead.⁴⁶

Another of the committee's plans was surveying investigators in order to find out what sort of laboratory stocks they held and required. Dunn and the other members had devised questions for the survey and an extensive, yet general mailing list. The list of recipients not only included several academic departments including zoology, botany, physiology, and genetics, but also deans of medical schools, biologists employed at publicly and privately funded institutions, and curators at

⁴⁵ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, 1928, Dunn to Mann, 2 Jun 1928.

⁴⁶ Rader, 102-03.

museums and botanical gardens. The committee had also come to the decision that stocks be maintained by scientific and educational institutions, rather than commercial businesses. Given the green light from Allen, the Committee on Experimental Plants and Animals sent out their questionnaire.⁴⁷

The survey gave background to the committee and its goals, and it asked five questions. The questions covered three main facets. What specimens were needed at the respondent's location? What specimens did the respondent's institution use for experiments, maintain stocks of for future use, and might make available for distribution? Was the respondent willing to cooperate with the Committee by either maintaining certain stocks at his institution or by supporting a central stock house?⁴⁸ It appears that Dunn and his colleagues received a strong response to the questionnaire, although it is uncertain exactly how many surveys were sent out compared to how many were returned. Dunn received over forty replies in the initial five weeks, during May and June 1930; however, Dunn's attention became diverted and he let his commitment to the Committee on Experimental Plants and Animals lapse. Among the tasks occupying his efforts was organizing the Genetics Society of America, as is discussed below. Dunn presented the Committee's final report at the annual meeting in April 1931, and one month later the Committee was defunct. W.C.

⁴⁷ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, 1929, Dunn to C.E. Allen, 9 Dec 1929 and Allen to Dunn, 18 Dec 1929.

⁴⁸ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, Feb-May 1930, Copies of same letter from Dunn to LaRue, Mann, and Hegner, 13 Feb 1930.

Curtis, the Chairman of the Division of Biology and Agriculture, stated that “...although he [Dunn] expressed the belief that there were opportunities for a Committee of this nature, at the same time he felt that nothing could be done [v]ery actively at the present time at least...” Lutz had previously told Dunn that the committee should officially disband and members unofficially stay in contact with one another.⁴⁹

There were other active systems in place for mice geneticists, including the Mouse Club of America and its Mouse Newsletter. Little and those summering at the Station for Experimental Evolution at Cold Spring Harbor started the club around 1920; Dunn was a member. They informally circulated information about mice strains and experimental work through communal letters with wide distribution. In 1941 George D. Snell made the Mouse Newsletter into a more formal publication produced out of his organization, the Roscoe B. Jackson Memorial Laboratory in Bar Harbor.⁵⁰ The better-known *Drosophila* Information Service (DIS) served a similar purpose.

The Committee on Experimental Plants and Animals failed to accomplish the great visions that Dunn had originally perceived for it; however, evidence accumulated over the years demonstrating that the need had not disappeared. Walter

⁴⁹ APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, Feb-May 1930 and June 1930-Feb 1932, 41 letters dated between 23 May and 28 Jun 1930 and more received after June 1930. About the future of the committee, see: APS, Dunn Papers, Series I, Box 23, National Research Council–Committee on Experimental Animals and Plants, June 1930-Feb 1932, Dunn to Hegner, 19 March 1931, Lutz to Dunn, 23 March 1931, and Curtis to R.A. Emerson, 21 May 1931.

⁵⁰ Rader, 54-56, 170.

Landauer in 1939 suggested to Henry A. Wallace, the Secretary of Agriculture, an idea similar to Dunn's project, but it went nowhere. About five years later, Robert Griggs, the acting Chairman of the Division of Biology and Agriculture of the National Research Committee, asked Landauer to become Chairman of the Committee on Plant and Animal Stocks. Landauer agreed and held a meeting at Columbia University in 1945 hoping to establish an information center that kept track of genetic specimens in addition to the two, *Drosophila* and maize, for which there already were organized programs. In 1948 Landauer resigned from the chairmanship because he felt that he had accomplished as much as he could. He had managed to get the United Nations Interim Commission on Food and Agriculture to catalog wheat. Years later in 1969, Landauer still recognized the need for a storehouse for poultry specimens.⁵¹ George D. Snell in 1951 voiced the need for another storehouse than the Jackson Memorial Laboratory, so that geneticists could concentrate on research and not on rearing experimental mice. Snell had received letters from various geneticists requesting that his laboratory take in mutant stocks of specimens. The goal was to have the specimens in two laboratories at least, so that they could insure that the mutant specimens did not go extinct. Snell had reservations about taking this on even though he saw the value of it. Dunn sympathized with Snell, and made it evident that he would help the Jackson Laboratory in any way possible. Dunn still saw the need

⁵¹ Hugh Clard and Louis J. Pierro, "Walter Landauer-the Man and his Work," Issues and Reviews in Teratology 7 (New York: Plenum Press, 1994): 217-83, 233-34.

for a repository preserving mutant specimens. Concurrent with the mouse stock issues, Dunn and Snell were also trying to regulate mouse nomenclature.⁵²

While overseeing the Committee on Experimental Plants and Animals, Dunn assumed chairmanship of the Committee on Organization of Geneticists, which orchestrated the founding of the Genetics Society of America in 1932. The American Association for the Advancement of Science (AAAS) annually held its meeting around late December or early January. The Joint Genetics Sections had been started in 1921 as a bridge between geneticists associated with the American Society of Zoologists and with the Botanical Society of America, who attended separate AAAS sessions.⁵³ Dunn was elected secretary-treasurer beginning in January 1926. That same year members ruled that the post be extended from one to three years.⁵⁴ George H. Shull of Princeton University oversaw the original eight-person committee, conceived in 1927, which discussed whether there was a need for a genetics association. Two years later he presented its conclusions: American geneticists required better organization and a society to represent their collective needs.⁵⁵

⁵² APS, Dunn Papers, Series I, Box 25, George D. Snell, 1929-1972, Snell to Dunn, 9 Apr 1951 and Dunn to Snell, 17 Apr 1951.

⁵³ APS, Genetics Society of America, 575.06: G28p, GSA, History and Establishment, 1934, untitled and undated summary of the Genetics Society of America.

⁵⁴ APS, Genetics Society of America, 575.06: G28p, 1921-22 Record of Genetics Sections, Bound book with Minutes of the Joint Genetics Sections meeting, Kansas City, Missouri, Dec 29 and 30, 1925: 17-18.

⁵⁵ APS, Dunn Papers, Series I, Box 14, Genetics Society of America, 1928-June 1930, assorted correspondence and G.H. Shull's Committee on Organization of the Geneticists report, n.d. Dunn handwrote on it "excerpt from minutes of Genetics Sections meeting Des Moines, 1929." As noted on the bottom of Dunn's copy of

There were problems with the Committee on Organization's approach that Shull oversaw, and therefore a smaller four-member committee was appointed at Des Moines in 1929. In addition to Dunn as chairman, its other members were Edmund W. Sinnott, Sewall Wright, and R.A. Brink. The following February Dunn contacted Wright and Brink to remind them of their commitment. Sinnott and Dunn communicated in person since both worked at Columbia University. The main resolution left undecided involved enrollment for the new genetics society, and there was a great deal of debate among geneticists about who should be allowed into the society and how they should gain entrance. The main question boiled down to: how restrictive or unrestrictive should the society be? All four geneticists favored a more inclusive society than the Joint Genetics Sections, which relegated its membership to those associated with the American Society of Zoologists and Botanical Society of America. Cognizant that an association of geneticists was interdisciplinary by nature, the Committee on Organization proposed "the formation of a society which shall be independent to the extent of choosing its own members, and which shall yet retain close contact with the other biological societies from which its membership will be drawn." The committee canvassed biological societies for opinions but received little feedback; only six of twenty-six associations replied. Due to the low response, action

Shull's report, this information is also available in "Proceedings of the Twenty-Seventh Annual Meeting of the American Society of Zoölogists," Anatomical Record 44 (25 Jan 1930): 402-04.

was postponed until the 1931 AAAS meeting so that Dunn's committee could have another year to survey the situation.⁵⁶

A resolution passed during the 1931 AAAS meeting held in New Orleans established the Genetics Society of America (GSA). Dunn was elected its first president. Among its other elected officers were F.D. Richey, vice-president, and P.W. Whiting, secretary-treasurer. The GSA kept essentially the same three-member set-up and required the same length of representation as that used by the Joint Genetics Section. The presidency and vice-presidency were one-year commitments, whereas the secretary-treasurer held the post for three-years. To round out the executive committee, the past two presidents remained as advisors.⁵⁷ J.A. Weir has noted that men associated with Harvard University's Bussey Institution dominated the GSA's executive committee in its initial years. Its first seven presidents taught, learned, or conducted research at the Bussey Institution.⁵⁸ Since the Genetics Society

⁵⁶ APS, Dunn Papers, Series I, Box 14, Genetics Society of America, 1928-June 1930. Quotation is from Report of the Committee of the Joint Genetics Sections to study the organization of geneticists, presented at the Cleveland meeting of the Sections Dec 30, 1930.

⁵⁷ APS, Genetics Society of America, 575.06: G28p, 1921-22 Record of Genetics Sections. In this folder is a bound book with notes about the activities of the Joint Genetics Section and Genetics Society of America.

⁵⁸ J.A. Weir, "Harvard, Agriculture, and the Bussey Institute," *Genetics* 136 (Apr 1994): 1227-31, 1230. The presidents were Dunn, 1932; R.A. Emerson, 1933; Sewall Wright, 1934; D.F. Jones, 1935; P.W. Whiting, 1936; E.M. East, 1937; and L.J. Stadler, 1938. Weir correctly ascertained that men associated with the Bussey Institution also dominated the organizational committee of the Sixth International Genetics Congress in August 1932; however, he erroneously stated that "this carried over to the organization of the Genetics Society of American later in the same year." As I have demonstrated, the GSA took several years, to organize, from 1927 to 1932, and had already been started by the time of the 6th ICG in August 1932.

of America grew from the Joint Genetics Sections, it had 289 members immediately after its inception. The bulk of them belonged to the American Society of Zoologists and the Botanical Society of America. The Society sought to enlarge its membership by soliciting teachers, graduate students, and research assistants in genetics.⁵⁹ One of the first tasks undertaken by this newly-formed society was contributing to the organization of the Sixth International Congress of Genetics held in Ithaca in August 1932. The Genetics Society of America operated without producing a journal for over thirty years and eventually took over the publication of Genetics in 1963. Genetics was established in 1916 and was associated with several publishing companies prior to merging with the Genetics Society of America.⁶⁰

Following his committee work on the behalf of Experimental Plants and Animals and for the establishment of the Genetics Society of America, Dunn joined other geneticists in calling for an overhaul of the Journal of Heredity. Assorted reasons, including objectionable content and financial crisis, brought about Dunn's involvement in revamping the journal. The Journal of Heredity was the first academic serial devoted to genetics in the United States.⁶¹ The American Breeders Association started the American Breeders Magazine in 1910 and after four years renamed it the Journal of Heredity. Even though the American Breeders Association had also

⁵⁹ P.W. Whiting, "Genetics Society of America," Science 75 (Mar 1932): 303.

⁶⁰ Barry Ganetzky, Professor in the Laboratory of Genetics at the University of Wisconsin and the current President of the GSA, told me (in personal correspondence 11 Sep 2006) the year that GSA started overseeing the publication of Genetics. Ganetzky learned about the history of the GSA from discussions with James Crow, his colleague at University of Wisconsin.

⁶¹ Dunn, "William Ernest Castle," 60.

changed its name to the American Genetics Association, it had not altered its foci. The title page of the newly-minted Journal of Heredity noted that in addition to plant and animal breeding the publication continued its devotion to eugenics.⁶² The journal's association with eugenics remained strong for many decades. Kenneth Ludmerer discusses the sordid past of the Journal of Heredity and mentions that Thomas Hunt Morgan, as early as 1915, objected to its eugenic associations by taking a stance in private. Dunn and Castle discussed their distaste for the journal's social commentary in 1933, which is discussed below. In 1939 the Journal of Heredity published a statement about its commitment to distancing itself from eugenic articles and dropped the term from its subtitle.⁶³ Kathy Cooke has pointed out that the journal continued printing pro-eugenic articles during the 1950s and 1960s.⁶⁴

Dunn went straight to the source when he had a complaint; and therefore he listed his objections to the journal's content by writing initially to the journal's editor Robert Cook. In his letter to Cook on 3 January 1933, Dunn stated that the Journal of Heredity should strive to keep science and politics separate because personal opinions should not be promulgated in an academic journal.

...my criticism is chiefly toward involving any kind of political ideas in scientific questions. It has nearly always been found to be bad policy

⁶² James Wilson, "The New Magazine Has a Place," The American Breeders Magazine 1 (1910): 3-5; For information about the changes, see the title page of the first Journal of Heredity volume from 1914.

⁶³ Ludmerer, 82-83, 136-37. About T.H. Morgan also see: Garland E. Allen, Thomas Hunt Morgan: The Man and His Science (New Jersey: Princeton University Press, 1978): 228-229.

⁶⁴ Kathy Cooke, "Twisting the Ladder of Science: Pure and Practical Goals in Twentieth-Century Studies of Inheritance," Endeavour 22 (Mar 1998): 12-16, 15.

and to lead to an entangling alliance between propaganda and scientific judgement [sic] in which the latter, because it is more difficult to maintain and disseminate, usually loses out. It is not so much a question as to whether one agrees with your social and economic remarks as to whether one wants to have the Association sponsor ideas of this sort in general. Personally I am opposed to it.

Dunn also noted that data about human heredity had been collected in a less than scientific manner and therefore, in his opinion, undermined the credibility of eugenic claims. Having been on the 1929 committee that analyzed the information collected by the Eugenics Record Office, Dunn specifically mentioned its eugenic questionnaire in conjunction with the one passed out by the journal. In effect, Dunn told Cook that he solely wished to register his complaint; however, within a couple of months he chose to do more.⁶⁵

Dunn followed up his letter to Cook by writing to William E. Castle two months later. Castle had an invested interest in the Journal of Heredity because he had helped to restructure it from a magazine into an academic journal in 1913, and he had remained close to its editorial processes throughout his life. Dunn asked Castle in March 1933, “Don’t you think the time has come to seriously consider the conduct of the Journal and to try and improve it?”⁶⁶ Dunn was not alone in his concerns. Franz Boas, Edmund W. Sinnott and Milisav Demerec had spoken with him about the situation, most likely because Dunn was on the editorial board. Demerec realized that changes needed to occur, but did not appear to want to get involved even though he

⁶⁵ APS, Dunn Papers, Series I, Box 4, William E. Castle, 1930-1939, Dunn to Cook, 3 Jan 1933.

⁶⁶ APS, Dunn Papers, Series I, Box 17, Journal of Heredity, 1929-1933, n.d., Dunn to Castle, 14 Mar 1933.

too was on the editorial board.⁶⁷ Dunn acted on the situation immediately by writing a string of letters that resulted in a meeting of the Journal of Heredity's editorial board and council.

Among the problems that Dunn listed, the topmost was Robert Cook. He edited the journal poorly as evidenced through “wretched” tabular material and frequent mistakes in content and labeling. Cook made editorial comments too often that were “badly chosen, and flippantly irrelevant.” The journal’s monthly volumes, furthermore, contained what appeared to Dunn to be material used to fill the pages. He noted that geneticists chose to send their papers to other journals and therefore the problem of sufficient and noteworthy articles would only decline. “At present the journal is a hodge-podge,” Dunn stated and continued by summarizing its content as, “partly scientific report, partly newspaper, partly popular science and occasionally propaganda.” Dunn later expressed his personal perspective about the journal’s content in a more straightforward way, but was not as frank with Castle as he had been with Cook: “I have not always been in full sympathy with the editorial policy (choice of scientific articles, inclusion of discursive articles on controversial social

⁶⁷ APS, Demerec Papers, L.C. Dunn, Demerec to Dunn, 23 March 1933. I don’t think Demerec wanted to get involved in the matter because he wrote to Dunn: “I agree with everything you said in the letter. There is no doubt that the Journal of Heredity could be greatly improved by better editing and you will do a good service to genetics if by your efforts an improvement in the journal can be accomplished.” Dunn had sent Demerec a copy of his letter to Castle (14 Mar 1933) with a note written at the top: “Demerec, Boas & two students have recently asked me whether I agreed with Cook’s conduct of the Journal & these criticisms seem to fall on all of us & call for consideration.” This note was most likely written to Castle.

and economic questions, character of news articles and the too patent appearance of a Mission)...”

The journal, according to Dunn, had some merits and served a unique purpose nonetheless. It attempted to circulate biological information to a wide audience by incorporating photographs with its articles. In actuality, the original intent had been to develop a publication similar to, and as successful as, National Geographical Magazine; however, the Journal of Heredity missed in achieving this aim.⁶⁸ Castle, for the most part, agreed with Dunn’s assessment of the journal’s problems with reference to subject matter. Castle specifically mentioned his distaste for the journal’s eugenic content and his opinion that it should exclude eugenic articles. Since Cook had made the journal successful, Castle thought that he should not be replaced. Cook, according to Castle, needed help from an editorial board that participated in the editorial process.⁶⁹ Moreover, the journal had fallen on hard times financially in part because the United States legislative branch had radically reduced the budgets given to learned institutions.⁷⁰ The United States was in the midst of a severe economic depression, and the journal suffered from its impacts too.

When Dunn wrote to Cook again, he reflected Castle’s stance that the journal should try to overcome its financial problems rather than to end publication. Dunn

⁶⁸ APS, Dunn Papers, Series I, Box 17, Journal of Heredity, 1929-1933, n.d., Dunn to Castle, 14 Mar 1933 and Dunn to Raymond Pearl, 1 May 1933.

⁶⁹ APS, Dunn Papers, Series I, Box 4, William E. Castle, 1930-1939, Castle to Dunn, 20 Mar 1933.

⁷⁰ APS, Dunn Papers, Series I, Box 17, Journal of Heredity, 1928-33, n.d., Cook to H.H. Newman of Department of Zoology at University of Chicago, 29 Apr 1933.

offered Cook constructive suggestions by highlighting the journal's best features, which he felt should become a fundamental aspect of the journal. His comments reiterate his previous statements about its numerous photographs and wide appeal. His suggestions provide a method of attacking the journal's financial and content problems in unison: cut down the amount of material presented in each volume by printing the best material and curtailing superfluous matter.⁷¹ By combining the two issues, Dunn managed to attack the problem of the journal's content by approaching it as an extension of its financial situation. During a meeting of the journal's editorial board and council, those in attendance decided to ask for a financial grant from the National Academy of Science's Committee on Scientific Publications. When Dunn presented the funding request to geneticist Raymond Pearl, he noted some of the changes agreed upon at the meeting. These changes in the journal's structure reflect the opinions expressed earlier by Castle and Dunn. The editorial board would support and advise Cook, who would remain managing editor. Moreover, "if properly edited and supported it could fill a unique and necessary place in the literature of genetics by publishing readable scientific articles in which the chief value and emphasis inheres in the illustrations." Pearl sent the appeal to the committee and expressed his hope

⁷¹ APS, Dunn Papers, Series I, Box 17, Journal of Heredity, 1928-1933, n.d., Dunn to Cook, 12 Apr 1933. Dunn sent his suggestions to Castle before finalizing them and mailing his letter to Cook (see Castle's note to Dunn on Castle to Cook, 6 Apr 1933).

that its members would aid the journal.⁷² The American Genetics Association continues to publish Journal of Heredity today.

During the mid-1920s Dunn evaluated his ideas about eugenics as a consequence of several factors and subsequently stood against eugenics. He became more outspoken and specific about the issues involved in applying Mendelian laws to human beings, but for the most part his opinion did not change; he continually cautioned or advised against the premature application of science towards social and political ends. The change that occurred was not within Dunn, but rather within society. The definition of eugenics had taken on a more precise meaning as eugenic ideas circulated through the general population. Eugenics no longer had two parts to its definition, one scientific and the other about application. Instead eugenics signified the implementation of scientific principles for the improvement of society. Dunn held to his original opinion that much would be gained from the scientific study of human heredity for his entire life. In the 1950s, as will be discussed in Chapter Nine, he attempted to pursue valid scientific investigations into questions about human heredity when he started the Institute for the Study of Human Variation. Indeed, after the Second World War his examinations turned to questions about biology, evolution, and race; however, the underlining principles guiding his research were the same and he continued to explore racism through scientific analysis.

⁷² APS, Dunn Papers, Series I, Box 17, Journal of Heredity, 1928-33, n.d., Cook to H.H. Newman of Department of Zoology at University of Chicago, 29 Apr 1933, Dunn to Pearl, 1 May 1933, and Pearl to Dunn, 5 May 1933. Cook wanted Newman to write to Raymond Pearl about the journal's legitimacy.

Dunn played an important leadership role in establishing genetics as a viable discipline. His efforts from roughly 1925 to 1935, however, do not provide an exhaustive exploration of geneticists' attempts at demarcating their discipline. Dunn, nonetheless, provides an entry into understanding the connections between geneticists' campaigns at this time, and thereby an analysis of Dunn's activities provides historians with a more comprehensive view. Genetics was a new field of inquiry in the 1920s and one that was closely tied to eugenics. By the mid-1930s geneticists consciously strove to distance their scientific research from the socio-political campaigns of eugenicists and had laid the basis for establishing genetics as a credible discipline in the United States.

Chapter 3

Aiding Refugees & Protecting Civil Liberties, 1933-1945

Americans responded quickly in 1933 after the Nazi Party in Germany passed its first law that targeted Jews. As troops mobilized in Europe throughout the 1930s, citizens of the United States created an assortment of organizations to aid European refugees and protect Americans' civil liberties, especially those suppressed under fascism such as democracy and intellectual freedom. The US government was reluctant to help these political refugees and had immigration restrictions to support their inaction.¹

Among the concerned American citizenry, university professors were a unique group in terms of their common profession and diverse specialties. Scholars were among the first to lose their jobs in Germany, and also scholars constituted a significant portion of the constituency of some of the United States very active refugee and anti-fascist organizations. American academics wished to help their foreign counterparts and protect freedom of expression at home.²

The year 1933 marks a major turning point in Dunn's activism because that year he became involved with these organizations that were forming as the result of political and social events. As the decade progressed his political activities increased.

¹ Gerhard Falk, "The Reaction of Americans to the Persecution of the European Scholars during the Nazi Era, 1933-1941," International Review of History and Political Science 9 (1972): 78-98, 78-79.

² Scholars in other countries such as Great Britain, France, and Belgium reacted similarly.

Indeed, Dunn's political consciousness had been growing, and he expressed his concerns about world events to Walter Landauer in February 1932:

For the first time in years I have been avidly reading the newspapers. Never have I felt so depressed and discouraged over the state of the world. I think in the back of my mind I really fear a general cataclysm of some sort. It even seems as though war in the East, an economic collapse in the U.S. and revolution in Germany and Spain might all be upon us at almost any minute. It will surely be increasingly difficult for any of us to preserve the physical detachment from the fate of other people which in the years just past was so easy that we adopted it as a distinct attitude. Now will arise the struggle between our desire to act and our foreknowledge of the probable futility of what we should do, and of the loss of peace and freedom which action in concert so often means. Between the alternatives I foresee increasing unhappiness for most of us.³

Science and scientists were Dunn's primary concern and initially lay at the center of his campaigns against fascism. Yet, Dunn also broadened his scope to include academicians of various disciplines and non-academic refugees. His efforts on the behalf of scientists, and the experiences of some of these refugees, especially geneticists and other biologists, are the core of this chapter.

Dunn participated on several anti-fascist organizations from 1933 to the end of war in 1945, and moreover he usually served as a member of their executive committees. In reference to groups formed to aid refugees, Dunn was on the Executive Board of the Emergency Committee in Aid of Displaced Foreign (formerly, German) Scholars for its entire duration from 1933 to 1945. The Emergency Committee helped to relocate European scholars into American

³ APS, Dunn Papers, Series I, Box 18, Walter Landauer, 1932, Dunn to Landauer, 4 Feb 1932.

universities who had lost their academic posts as a result of Nazi laws.⁴ Dunn was Secretary of a similar organization, the Faculty Fellowship Fund at Columbia University, which raised money from its local community and placed refugees into its faculty. He was also on the Executive Committee of the American Committee to Save Refugees, which focused on aiding any refugee without reference to profession and coordinated the actions among these various groups.⁵ These three organizations had similar aims, but performed them on different scales (at the local, national, and international levels) and for people of different professional backgrounds.⁶

As time passed, Dunn continued working for these organizations and joined others. In the late 1930s to early 1940s Dunn re-evaluated his efforts and decided that he no longer wished to find solutions to each individual's problem, but wanted to strike at the core of the problem. Anti-fascist organizations attempted just that and Dunn actively participated in local groups, especially those giving aid to Spain. These associations later formed the basis of the American Committee for Democracy and Intellectual Freedom founded in 1939. In this chapter Dunn's two anti-fascist campaigns are discussed, as well as the Faculty Fellowship Fund at Columbia University, the Emergency Committee in Aid of Displaced Foreign Scholars, and the

⁴ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid of Displaced German Scholars, 1933 and Box 11, Emergency Committee in Aid of Displaced German Scholars-Minutes, 1933.

⁵ APS, Dunn Papers, Series I, Box 1, American Committee to Save Refugees, 1940-1942, "Objectives of the American Committee to Save Refugees."

⁶ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1933-1935, Interim Report of Faculty Fellowship Fund, written by Dunn, Secretary, 27 Nov 1933.

several organizations that culminated to form the American Committee for Democracy and Intellectual Freedom.

Scholars have previously examined the relocation of scientists during the 1930s and 1940s. Two edited volumes provide the most comprehensive assessment of the situation for scientists. They are Donald Fleming and Bernard Bailyn's The Intellectual Migration and Mitchell Ash and Alfons Söllner's Forced Migration and Scientific Change. Biologists, however excluding medical scientists, have received little attention. Many physicist and chemist refugees contributed significantly to the war effort by working on the Manhattan Project, and their situations have been discussed more often than biologists. Social scientists have also received scholars' attention.⁷ A handful of books on intellectual refugees were written by those who helped to relocate them and these accounts draw significantly on information gathered from refugees. One of these, The Rescue of Science and Learning, is about the Emergency Committee in Aid of Displaced Foreign Scholars and was written by two of its most knowledgeable members, its Chairman Stephen Duggan and Secretary Betty Drury.⁸ Gerhard Falk briefly surveyed four refugee organizations, one of which was the Emergency Committee.⁹

⁷ Donald Fleming and Bernard Bailyn, eds., The Intellectual Migration: Europe and America, 1930-1960 (Cambridge: Belknap Press, 1969); Mitchell G. Ash and Alfons Söllner, eds., Forced Migration and Scientific Change: Émigré German-Speaking Scientists and Scholars after 1933 (New York: Cambridge University Press, 1996); Lewis Coser, Refugee Scholars in America: Their Impact and Their Experiences (New Haven: Yale University Press, 1984).

⁸ Stephen Duggan and Betty Drury, The Rescue of Science and Learning: The Story of the Emergency Committee in Aid of Displaced Foreign Scholars (New York: The

In their Introduction, Ash and Söllner assess the literature written about refugees and suggest some new perspectives to consider. They wish to move away from the argument that Germany's loss was another country's gain. They do not use the term "Brain Drain," but that is the concept to which they refer. Refugees, according to Ash and Söllner, should be seen as more than their intellects. They were scholars who had to adjust in several ways because they lost their livelihoods, language, and culture. For those who adjusted well, they gained new ideas and opportunities in their new environments. As will be discussed below, some refugees failed to accommodate to their new lives. Ash and Söllner accurately stated that "Discipline membership...could be an anchor of stability in the personal and career crises that befell many émigrés..."¹⁰ Indeed, that was true for some geneticists, as

MacMillan Company, 1948); Maurice R. Davie, Refugees in America: Report of the Committee for the Study of Recent Immigration from Europe (New York: Harper & Brothers Publishers, 1947); Norman Bentwich, The Rescue and Achievement of Refugee Scholars: The Story of Displaced Scholars and Scientists, 1933-1952 (Netherlands: Maritinus Nijhoff, 1953).

⁹ The other three were the Rockefeller Foundation, University in Exile at the New School for Social Research, and the Academic Assistance Council. Falk overstated the Emergency Committee's hands-off approach. In theory the Emergency Committee wanted Universities to come to them with requests for a scholar. In reality, though, members of the Emergency Committee solicited invitations and asked for formal requests and privately informed American academics which scholars needed jobs. Gerhard Falk, "The Reaction of Americans to the Persecution of the European Scholars during the Nazi Era, 1933-1941," International Review of History and Political Science 9 (1972): 78-98.

¹⁰ Ash and Söllner, 1-19. Quotation is from page 12. Lewis Coser recently substantiated Ash and Söllner's point about other levels of the refugees' situations. Coser argued that human agency plays a significant role in the flow of ideas resulting from refugee relocations. Coser wanted the history of ideas to be rooted in a history of people and human interaction and he believed that the information passed between people through personal contact and communication holds an important place. "To be

will be shown, but not true for all. Each individual scholar had personal and professional attributes that also determined his or her desirability as a candidate for relocation and ability to adjust. Moreover, the power of international scientific networks played a major role in coordinating and expediting a refugee's relocation.

Establishing Refugee Organizations Locally and Nationally

The Faculty Fellowship Fund started as the result of a luncheon discussion between Dunn, Franz Boas, John Dewey, and Wesley C. Mitchell of Columbia University and Alvin Johnson of the New School for Social Research. (Each is discussed in fuller detail below). They had gathered at the Columbia Faculty Club in May 1933 with the express purpose of devising a plan after they had learned the names of those in Germany who had been ousted from academic positions for political reasons.¹¹ The German law had been passed in early April 1933, dismissing Jewish civil servants and teachers at private institutions.¹²

sure," writes Coser, "a great deal of cultural transmission comes about via impersonal channels such as the printed media and, more recently, by electronic means. However, such impersonal processes have always been supplemented by personal contacts between human agents" (Coser, xi).

¹¹ Charles Weiner, "A New Site for the Seminar: The Refugees and American Physics in the Thirties," *The Intellectual Migration: Europe and America, 1930-1960* eds. Donald Fleming and Bernard Bailyn (Cambridge, Harvard University Press, 1969): 190-234, 212. Weiner interviewed Dunn and reprinted the announcement from the *Manchester Guardian* on 19 May 1933 that named 196 German scholars who had lost their academic posts.

¹² Peter M. Rutkoff and William B. Scott, *New School: A History of the New School for Social Research* (New York: The Free Press, 1986): 91.

As their first action, Dewey, Dunn and three other members of Columbia's faculty surveyed their colleagues' opinions about helping German scholars. In the questionnaire, they stated the problem: "The outstanding fact which is of interest to all of us is that hundreds of scholars in all fields of knowledge, including seven Nobel Prize Winners[,] have lost their positions for reasons irrelevant to their scientific qualifications." They also proposed a solution: "It is important both for the name and the progress of science that arrangements should be made to provide working facilities for the persons thus affected." Moreover, they noted that other institutions of higher learning were mobilizing in order to aid these intellectual refugees, and at Columbia University they proposed "to establish temporary fellowships and to arrange for their tenure in a suitable university." Two questions were devised for a letter to their colleagues:

- A. Would you sympathize with the idea of establishing such fellowships at Columbia for outstanding German investigators who are at present without scientific homes?
- B. Would you contribute toward funds to be raised for this purpose?¹³

The questionnaire ascertained that there was a general desire to help foreign colleagues, and the Faculty Fellowship Fund was officially established in July 1933. (By this time, Dunn was an Executive member of the Emergency Committee in Aid of Displaced Foreign Scholars, which is discussed below.)¹⁴ Through this campus

¹³ The five faculty members were John Dewey, Joseph P. Chamberlain, Hans T. Clarke, Robert MacIver, and Dunn. UACL, Subject Files: Faculty-Faculty Fellowship Fund (for German Scholars), Questionnaire to the Columbia University faculty, n.d.

¹⁴ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records-L.C. Dunn, 1933, Dunn to Edward R. Murrow,

organization, members raised money for temporary fellowships to be given to displaced academics, who received an invitation to Columbia University. In other words, an academic department invited a scholar as a fellow, and their salary came from the Faculty Fellowship Fund. The University had no monetary commitment to the Faculty Fund fellows, but agreed to recognize the invitee by bestowing the scholar with a title, such as visiting professor, lecturer, associate, or fellow.¹⁵

Several prominent faculty members were associated with the Faculty Fellowship Fund. Philosopher John Dewey, “whose staunch defense of freedom to learn and teach was known to all” according to Dunn, oversaw the Faculty Fund by taking on its chairmanship. Dunn became secretary, a post he held until January 1941.¹⁶ Anthropologist Franz Boas and economist Wesley C. Mitchell, in addition to formulating the idea for the organization, also acted on the committee. Boas, who turned seventy-five years old in 1933, had securely established his reputation by this point and he consciously chose to focus on political issues during the 1930s. Dunn

Assistant Director of Institute of International Education, 12 Jul 1933. Murrow left the Emergency Committee in 1935 to join the Columbia Broadcasting System (CBS) and had an illustrious career as a CBS news reporter, setting the standard for TV news reporting. He was in his mid to late 20s while working with the Emergency Committee, and during World War II he reported from London (Duggan and Drury, 178). For information on his work with the ECADFS, also see: A.M. Sperber, Murrow: His Life and Times (New York: Freundlich Books, 1986): 51-66.

¹⁵ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1933-1935, Interim Report, 27 Nov 1933.

¹⁶ APS, Boas Papers, B:B61, L.C. Dunn #4, Minutes from Faculty Fellowship Fund meeting, 10 Jan 1941. Prof. James W. Angell took over the Secretary post from Dunn (immediately) who wanted to step down after 8 years (in the minutes). Rabi and Hecht had been elected onto the committee. Same minutes are also in Dunn Papers, Columbia University-Faculty Fellowship Fund, 1940-1941, n.d.

and Boas developed a strong personal relationship during the 1930s and 1940s, which will be discussed in greater depth below.

By the end of 1933, a total of twelve faculty members participated on the committee, several of whom were scientists including physicist George B. Pegram, physical chemist Victor K. LaMer, and biochemist Hans T. Clarke.¹⁷ Other scientists who joined the committee later were chemist Harold C. Urey, physicist I.I. Rabi, and biophysicist Selig Hecht. A pivotal member of the Faculty Fund from its beginning was Joseph P. Chamberlain, a professor of public law, who was appointed by President Franklin Delano Roosevelt to the International High Commission for Refugees. Chamberlain acted on several refugee committees in the 1930s and 1940s and undoubtedly kept members of the Faculty Fund abreast of what he knew about the situation in general.¹⁸

¹⁷ Jaques Cattell, ed., "Hans T. Clarke," American Men of Science: The Physical and Biological Sciences 10th ed., vol. 1 (Tempe: Arizona State University, 1960): 687; Jaques Cattell, ed., "Victor K. LaMer," American Men of Science: The Physical and Biological Sciences 10th ed., vol. 3 (Tempe: Arizona State University, 1961): 2299.

¹⁸ Gerald Benjamin, "Joseph Perkins Chamberlain," American National Biography 5, eds. John A. Garraty and Mark C. Carnes (New York: Oxford University Press, 1999): 633-35; APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1933-1935, Dunn's Interim Report, 27 Nov 1933. The other four of twelve original members were: minister and educator Henry Sloane Coffin, education specialist John J. Coss, sociologist and political scientist Robert M. MacIver, and economist and social scientist Edwin R.A. Seligman. Others joined later including Edmund Beecher Wilson, Carleton J.H. Hayes, historian Isaac L. Kandel, and sociologist Robert S. Lynd. About those who joined later see, APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1950, 1967, Dunn's Final Report, n.d.; APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1938, Faculty Fellowship Fund, 12 Dec 1938.

Columbia University's faculty responded to the European situation quickly after receiving news from their British colleagues. Boas received two cables informing him that Lord William Henry Beveridge and George Macaulay Trevelyan were forming a national committee in Great Britain. Other members mentioned were J.W. Mackail, Lord Cecil of Chelwood, Lord Rutherford, and Lord Grey. The British group originally called itself the Academic Assistance Council of England and later changed its name to the Society for the Protection of Science and Learning.¹⁹ The first cable explained the British association's functions and plans: "to rais[e] funds for fellowships for scientists stop immediate necessity to create neutral international board of scientist and scholars to coordinate plurality of existing movement and assume responsibility for fellowship awards stop." The second cable mentioned that scholars in France and Belgium had started similar national organizations to help German refugees and asked Boas to organize a comparable movement in the United States: "Would you consider creating an American group[?]"²⁰ Boas was in close contact with the United States' national group, the Emergency Committee in Aid of Displaced Foreign Scholars, but he did not act on it.

¹⁹ Information compiled by the Society for the Protection of Science and Learning can be found in: Norman Bentwich, The Rescue and Achievement of Refugee Scholars: The Story of Displaced Scholars and Scientists, 1933-1952 (Netherlands: Martinus Nijhoff, 1953). Beveridge wrote a book about the refugees and British organizations: Lord Beveridge, A Defence of Free Learning (London: Oxford University Press, 1959). An article on refugees in Britain: Walter Adams, "The Refugee Scholars of the 1930s," Political Quarterly 39 (1968): 7-14.

²⁰ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced Scholars-Correspondence, n.d. Cables from Liebowitz and Ben. Although the copies of these cables are undated, it is evident that they were sent immediately after implementation of the April 1933 laws in Germany. Duggan and Drury, 5.

The Emergency Committee in Aid of Displaced German Scholars officially started in June 1933. A little over five years later on 9 November 1938, the Executive Board voted to change the organization's name by replacing German with Foreign because only a fraction of the refugees needing relocation by this time were actually Germans.²¹ As stated in the organization's name, members restricted their activities to placing foreign scholars into academic jobs throughout the United States. To achieve its aims, the organizers laid out three goals at its onset: "to serve the personal needs of the refugees, to preserve their great attainments for the benefit of scholarship in the United States, and particularly to assist the institutions of higher education to absorb the refugee scholars without disturbance in administration or conflict with personnel." In order to bypass immigration restriction laws, the Emergency Committee invited scholars before they arrived to the United States. Jewish immigration quotas did not apply to a refugee with an invitation in hand. Due to the overwhelming number of foreign academics looking for American positions and monetary aid, members of the Emergency Committee achieved their goals by gradually adopting the following guidelines: They solicited requests from American universities and then found a suitable refugee to fill the open position. They confined their efforts to mature refugees, ranging roughly from ages thirty-five to fifty-eight, who had strong academic reputations. Lastly, they acted favorably on requests from institutions that

²¹ An announcement of the Emergency Committee appeared in the *New York Times*, "German Scholars to be Aided Here," *New York Times* (13 July 1933): 6. The Emergency Committee changed its name at the 9 November 1938 meeting of the Executive Board. Duggan and Drury, 7.

agreed to pay the refugee's entire salary within one to two years time. Some exceptional scholars who fell outside of these parameters also were considered.²²

When Stephen Duggan, Director of Institute of International Education, originally invited Dunn to join their "provisional Committee that has been organizing the activity in favor of the ousted German professors," Duggan's role as Director of the Institute of International Education made him and his organization an obvious choice for coordinating the American effort to find fellowship opportunities for foreign displaced academics. The Institute of International Education had started after World War I in order to facilitate good will and better international relations by sending American scholars overseas on educational exchanges.²³ Dunn hesitantly accepted Duggan's invitation by questioning whether the Emergency Committee would benefit from his acceptance: "Since I am willing to forward the work of the committee in any event, it might be better to add someone whose name would add prestige to the committee and have it understood that I may be called upon whenever needed." Duggan made sure that Dunn knew the day and time of the next meeting and also informed Dunn of the organization's temporary name, the Emergency Committee in Aid of Exiled German Scholars.²⁴ "Exiled," however, as we have seen above, would not be adopted. By 27 June 1933 they had chosen the name that is

²² APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced German Scholars, Correspondence, 1941, Annual Report 1 Jun 1941, page 1-2; Falk, 86-88.

²³ Duggan and Drury, 6.

²⁴ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records-L.C. Dunn, 1933, Duggan to Dunn, 3 Jun 1933 and Dunn to Duggan, 7 Jun 1933, and Murrow to Dunn, 12 Jun 1933.

familiar today. The exchange between Duggan and Dunn marked the beginning of Dunn's active membership on the Executive Board of the Emergency Committee, and he remained active until its liquidation in 1 June 1945, almost twelve years later.²⁵

In addition to Dunn there were six other members of the Executive Board when the Emergency Committee started in 1933. Each man was a prominent scholar or citizen in the New York area. Anthropologist Livingston Farrand, who was President of Cornell University, acted as the Emergency Committee's Chairman until his death in 1939 at which point Duggan, the Emergency Committee's secretary from 1933-1939, became chairman. Fred M. Stein, President of Montefiore Hospital, was the Emergency Committee's treasurer. The other members in 1933 were Alfred E. Cohn of the Rockefeller Institute for Medical Research, Nelson P. Mead, a leader in New York City philanthropies and professor at College of the City of New York, and lawyer and philanthropist Bernard Flexner. Flexner raised the money used by the Emergency Committee in Aid of Displaced Foreign Scholars. A large portion of that money initially came from the Rockefeller Foundation, which matched the money given to each refugee from the Emergency Committee. Both of Bernard's brothers, Simon and Abraham Flexner, worked closely with the Rockefeller Foundation and helped the Emergency Committee. Simon was an influential advisor for the Rockefeller Foundation during the 1920s and continued to have a powerful voice even after he stopped acting as a trustee in 1930. Abraham organized medical fellowships for the Rockefeller Foundation in the 1920s. Over the years more people

²⁵ Duggan and Drury, 64.

joined the Executive Board, such as Frank Aydelotte, Director of the Institute for Advanced Study in Princeton, Alvin Johnson of the New School for Social Research, and astronomer Harlow Shapley of Harvard University.²⁶

The Emergency Committee started in response to what was considered an emergency and therefore a temporary situation. Indeed, many Americans thought that the situation in Germany would be short-lived and had no interest in contributing money to aid refugee scholars since the Great Depression had created financial problems for universities and academics. For example, Dunn sent circulars in May 1933 asking several geneticists, including Raymond Pearl, G.H. Parker and Ross G. Harrison, to contribute funds in order to set up fellowships for refugee scholars, and each declined for similar reasons. Pearl of Johns Hopkins University thought that the project was too ambitious because universities were having a hard time functioning financially. Furthermore, Pearl doubted the severity of the German scholars' situation and was very optimistic that the situation was short-lived: "I have a strong feeling that the present attitude of the German administration regarding such men will be only temporary, and that as a matter of fact most such men will gravitate back to their old

²⁶ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid for Displaced German Scholars-Minutes, 1936, 4 Jun 1936 and Correspondence, 1941, Annual Report 1 Jun 1941; Duggan and Drury, 6-7. There were four other Executive Board members: Charles J. Lieberman, President of the Refugee Economic Corporation, Henry Allen Moe, trustee of the Oberlaender Trust, Hertha Kraus, and Charles A. Riegelman. About Simon and Abraham Flexner, see: Robert Kohler, Partners in Science: Foundations and Natural Scientists 1900-1945 (Chicago: The University of Chicago Press, 1991): 78, 106, 291; Thomas Neville Bonner, Iconoclast: Abraham Flexner and a Life in Learning (Baltimore: The Johns Hopkins Press, 2002): 264-65; Rutkoff and Scott, 94.

positions within a year at most.” Parker, Director of the Zoological Laboratories of Harvard University, told Dunn that he was “not disposed at this moment to have my name used on committees or to agree to contribute funds” to the Emergency Committee because “the situation is extremely hazy and I am not in a position to pass judgment on the universities, the Nazi regime or the professors. When the atmosphere clears a bit I hope to have a more certain judgment of the situation.” Harrison of Yale University’s Osborn Zoological Laboratory agreed that universities were “paralyzed for lack of funds.” In contrast to Pearl and Parker, however, Harrison was extremely pessimistic and doubted that Americans could actually rectify the problem to any degree: “I quite sympathize with the move but fear that the problem is one of such magnitude that anything we can do will not be of great help.”²⁷

As time passed, the European situation coalesced and it became clear to many (including Dunn) that the German National Socialist Party and fascism were formidable enemies with enduring power. Members of the Emergency Committee developed their guiding principles overtime in order to deal with what had become a long-term reality. By 1935, the Executive Board of the Emergency Committee for Aid of Displaced Foreign Scholars reacted to the situation in Europe by setting limitations for their organization resulting in the guidelines discussed above. In summary, from the beginning it was agreed to only help established scholars and not other refugees. The Executive Board quickly restricted its functions by requiring a

²⁷ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid for German Scholars-Correspondence, Apr-Aug 1933, Pearl to Dunn, 24 May 1933, Parker to 26 May 1933, and Ross G. Harrison to Dunn, 26 May 1933.

long-term financial commitment from American institutions that wanted to hire a refugee scholar.

In the months after the war began, the number of refugee scholars looking to come to the United States grew significantly. Not only was fighting underway in Europe, but there was a growing number of scholars who needed to relocate. The Emergency Committee responded to these new developments by reviewing its guidelines and activities. Dunn expressed his views on the matter, requesting that the Emergency Committee return to its original guidelines. In particular, he wished to curtail the Emergency Committee's interactions with refugees by dealing strictly with American universities and institutions, and he wished to continue restricting aid to universities that agreed to pay half of the refugee scholar's salary. Giving up one of these tenets was feasible, but disregarding both was problematic. Furthermore, Dunn preferred giving greater sums of money to better qualified, senior refugees, rather than giving less money to more applicants.²⁸ From 1940 until the Emergency Committee's dissolution in 1945, Dunn continued to voice similar opinions evaluating the organization's original intentions versus its actions.

In order to anticipate the needs of refugee scholars, Executive Board members of the Emergency Committee in Aid of Displaced Foreign Scholars sought updates about the situation in Europe from a variety of sources and had some of the latest information available. In July 1933 Dunn received a letter from biophysicist Selig

²⁸ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced German Scholars-Correspondence, Jul-Dec 1940, Dunn to Duggan, 18 Nov 1940.

Hecht, his co-worker in the Department of Zoology at Columbia University. Hecht, who was visiting Germany, knew that Americans had started the Emergency Committee and relayed to Dunn that Germans needed to know that there were people who were willing to help them. Thus, Hecht suggested that Dunn's committee develop a pamphlet and circulate information about the organization to academics in Germany.²⁹ Years later, in the early 1940s, Dunn convinced Hecht to act on the Faculty Fellowship Fund.³⁰

In 1934-1935, Dunn went to Europe during his sabbatical leave and saw the situation firsthand. Dunn shared his pessimism with members of the Emergency Committee.

I'm just back from Germany and although you probably have good means of knowing conditions there, I must tell you that any impressions of conditions in the Universities were pretty terrible. Externally and physically, things look very well. New institutes, splendid equipment and a lot of activity. But these things and the right to work at all are bought at the price of complete submission to a regime which clearly intends to destroy all free intellectual life; and the personal lives of most thinking people are being destroyed by this contradiction and the hypocrisy and dishonesty which are sure to follow upon it.³¹

²⁹ APS, Dunn Papers, Series I, Box 15, Selig Hecht, 1933, n.d., Hecht to Dunn, 23 Jul 1933.

³⁰ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1940-1941, n.d., Minutes of the Meeting of Faculty Fellowship Fund Committee, 10 Jan 1941.

³¹ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1933-1935, Dunn to Murrow, 3 Jul 1935.

While Dunn was on this leave of absence in 1934-1935, Franz Boas oversaw the Faculty Fund's operations.³² In 1937, members of the Emergency Committee invited Dr. Fritz Demuth, who was on the Executive Committee of the Academic Assistance Council (the British equivalent of the Emergency Committee) to update them on the situation in Germany and to forecast what would be Adolf Hitler's primary and secondary concerns. Demuth reported that war was not an immediate objective because "the Hitler regime could not, in the next few years, gain any victories in its economic and foreign affairs." He then conjectured that Hitler would act "even more ruthlessly" against educators because he "would concentrate on its ideology and proceed more rigorously than ever against all those individuals and groups in Germany that were not 'coordinated.'" Demuth optimistically remarked that 900 German scholars had already been placed outside of Germany. This information did not appease members of the Emergency Committee's Executive Board, who could not fathom how they would accommodate the additional 1,250 people who Demuth foresaw losing their posts in Germany in the upcoming years.³³

Dunn's efforts placed him in a good position for receiving the latest information. While visiting the State Department in Washington D.C. during June 1940, Dunn learned that the newly-implemented British blockade of France increased

³² There is not a statement that Boas officially took over as Secretary of the Faculty Fellowship Fund, but that appears to be the case from the correspondence. New York Public Library, Emergency Committee of Displaced German Scholars, Box 111, Colleges & Universities-Columbia University, 1934 and 1935.

³³ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid for Displaced German Scholars-Minutes, 1937, 21 April 1937, pages 1-2.

the difficulty of getting refugees out of France. “The State Department said the only way to get out of France was by plane via Lisbon for London and this of course was impossible for most of the refugees.”³⁴ One last example demonstrates the quick flow of information to those who aided refugees. Dunn informed Frank D. Fackenthal, Provost of Columbia University, that physicist Paul Langevin was in “grave danger” having been arrested the day before.³⁵

Assisting Foreign Scholars

Since Dunn participated on the Emergency Committee and Faculty Fund, the two organizations wound up working closely together.³⁶ For example, some refugees initially coming under the purview of the Faculty Fellowship Fund eventually received monetary aid from the Emergency Committee in Aid of Displaced Foreign Scholars. When the first laws against Jewish academicians went into effect in Germany in April 1933, mathematician Felix Bernstein of the University of Göttingen was in the United States and chose to remain in New York. During the 1920s Bernstein had used the Hardy-Weinberg equilibrium principle to analyze human blood groups and demonstrated that blood groups A and B are dominant, and AB and O are not. A few years later, in 1931, he devised an algebraic method to measure the

³⁴ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1940, Dunn to Landauer 22 Jun 1940.

³⁵ UACL, Central Files 1.1.236, Box 382, Folder 1, L.C. Dunn (Jul 1940-Jun 1941), Dunn to Fackenthal, 23 Nov 1940.

³⁶ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1950, 1967, Dunn’s Final Report, n.d.

frequency of gene linkages among the human population.³⁷ In May 1935 Boas, who was interested in Bernstein's work, strove to find money to keep him at Columbia University. In his attempt to receive the Emergency Committee's help, Boas wrote to Secretary Stephen Duggan and described Bernstein as "a man of such great value to us that I am exceedingly anxious to see him retained."³⁸ Previously, Duggan had acquired money from the Emergency Committee that contributed to Bernstein's relocation to Columbia University.³⁹ Dunn reflected later that he and Boas had persuaded the Department of Mathematics to take on Bernstein.

The Faculty Fellowship Fund, however, never actually gave money to Bernstein because he received a \$4,000 salary paid in equal parts by the Emergency Committee and Rockefeller Foundation. In his effort to find monetary aid to stay in the US, Bernstein approached the Rockefeller Foundation, requested money for one or two assistants, and informed the Foundation's administrators that Boas and Dunn had gone to great lengths on his behalf: "B. [Bernstein] feels that Boas and Dunn have already made considerable sacrifices of time and energy in providing for him, and that he would not feel it proper to ask them to draw upon their departmental budgets for this aid." By May 1934 Warren Weaver of the Rockefeller Foundation heard negative details about Bernstein's personality and tenure at Columbia

³⁷ Daniel J. Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (1985; Cambridge, Harvard University Press, 1995): 195-97.

³⁸ APS, Boas Papers, Stephen Duggan #8, Boas to Duggan, 20 May 1935 and 29 May 1935

³⁹ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1933-1935, Dunn's Interim Report, 27 Nov 1933.

University. The Rockefeller Foundation would not renew their grant, but the Emergency Committee did. Bernstein stayed at Columbia University until June 1936 at which point he went to New York University.⁴⁰

Philosopher and theologian Paul Tillich of Frankfort had a similar experience to that of Bernstein. Presbyterian minister and educator Henry Sloane Coffin, who was President of the Union Theological Seminary and facilitated its strong relations with Columbia University, recruited Tillich to join the Union Theological Seminary. Tillich received a fellowship paid by the Faculty Fund, but soon thereafter learned that the Emergency Committee had agreed to sponsor him. He benefited by receiving money from both organizations.⁴¹ Tillich spent the rest of his career associated with Columbia University.⁴² Bernstein and Tillich were established academicians with

⁴⁰ RAC, RF 1.1 200D, Box 130, Folder 1603, Columbia University-Felix Bernstein (Refugee Scholar, Mathematics) 1933-1939, Notes of a conversation with Felix Bernstein, 14 Nov 1933 and President Nicholas Murray Butler to Raymond Fosdick of Rockefeller Foundation, 6 Nov 1939. Many documents in this folder cover the details of Bernstein's request and the Rockefeller Foundation's deliberations and grant. APS, Dunn Papers, Series I, Box 3, Felix Bernstein, 1952, Dunn to Dr. George B. Pegram, 24 Jun 1952.

⁴¹ APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1933-1935, Dunn's Interim Report, 27 Nov 1933; Bradley J. Longfield, "Henry Sloane Coffin," American National Biography 5, eds. John A. Garraty and Mark C. Carnes (New York: Oxford University Press, 1999): 142-43. Members on the Faculty Fellowship Fund used the organization to their advantage by sponsoring refugees that they desired to have as associates. Dunn was no exception. Some of the proceeds collected by the Faculty Fellowship Fund paid for short lectureships for two biologists, biophysicist Eugene Rabinowitch and zoologist Ernst Caspari (APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1936-1938, Letter to the Officers of Columbia University, 5 Dec 1938).

⁴² For more information about Tillich, see: Karen J. Greenberg, "The Refugee Scholar in America: The Case of Paul Tillich," Forced Migration and Scientific Change, eds. Mitchell G. Ash and Alfons Söllner (New York: Cambridge University Press, 1996);

strong professional reputations, which contributed to their ability to receive monetary aid.

Members of the Faculty Fund sought other scholars to invite to Columbia University upon learning of the Emergency Committee in Aid of Displaced Foreign Scholars' generosity and in cases when they agreed to sponsor a refugee who subsequently chose not to leave Germany. For example, anthropologist T.W. Danzel of Hamburg was to receive funding from the Faculty Fund, but stayed in Germany after he regained his position there. As Secretary, Dunn asked the committee to use the money set aside for Danzel to pay Julius Lips's salary. Julius Lips, Professor of Ethnology and Sociology and Director of the Rautenstrauch-Joest Museum at the University of Cologne, received a post and salary organized by members of the Faculty Fund. Boas had suggested Lips, and also gave Lips a place in the Anthropology Department. When money became scarce, Boas took on a significant amount of the burden himself by anonymously paying half of Lips's salary in 1935. Dunn knew that Boas paid half of Lips's salary and shared this information with Columbia colleagues Dewey and Mitchell: "Although he [Boas] does not wish it to be known, he is now paying half of Lips's salary out of his own pocket."⁴³ As demonstrated by the situations involving Bernstein and Lips, Dunn and Boas personally strove to aid refugees.

Paul Tillich, "The Conquest of Theological Provincialism," The Cultural Migration: The European Scholar in America (Philadelphia: University of Pennsylvania Press, 1953).

⁴³ APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1933-1935, Dunn to John Dewey and Wesley C. Mitchell, 12 Dec 1935.

The Emergency Committee aided 288 scholars and the Faculty Fund fourteen academicians from a variety of disciplines. Only those to whom Dunn gave special attention are discussed below. Three scholars, Victor Jollos, Hugo Iltis, and Friedrich Oehlkers were associated with genetics. Jollos and Iltis received money from the Emergency Committee and Oehlkers from the Faculty Fellowship Fund. The case of Jollos is one that demonstrates the hardship that some émigrés faced by relocating to the United States. Jollos immigrated in 1933, became an American citizen in 1939, and died in 1941 without ever having gained a permanent position. Iltis, a biology professor, arrived in the United States between late 1938 and early 1939. He considered himself Gregor Mendel's official biographer, recreated his Brunn museum that explained Mendel's life and ideas, and quickly found a job at Mary Washington College of the University of Virginia. Oehlkers, in contrast to Jollos and Iltis, stayed in the United States for only four months during 1939 as a recipient of a fellowship from the Faculty Fund and then returned to Freiburg. His situation demonstrates the leverage that Dunn had by the late 1930s in finding quick solutions for certain refugees. Oehlkers decided to return to Germany for personal, not professional, reasons.

It is informative to consider the cases of Jollos, Iltis, and Oehlkers in more detail, in order to gauge the successes and failures of Dunn's work on these refugee committees. Dunn's correspondence and papers provide a great deal of information and insight into the lives of émigré scientists. Through Dunn's efforts in their behalf,

personal stories about the refugees can be told that illustrate the vast differences in refugees' ability to adjust to their new lives in America.

In November 1933, Richard Goldschmidt of the Kaiser Wilhelm Institute for Biology wished to find his student, Victor Jollos, a position in the United States. Goldschmidt personally wrote to L.J. Cole at the University of Wisconsin asking him to give Jollos a job. Through extensive letters between November 1933 and April 1934, Cole and Dunn coordinated the details for Victor Jollos to go to Wisconsin. Dunn then met Jollos at the harbor in New York when he arrived on the *Deutschland*.⁴⁴ Jollos, who was forty-five years old when he came to the United States, remained at the University of Wisconsin for four years even though there was not a permanent position for him. Jollos had received aid from the Emergency Committee before the Executives decided to limit its financial aid to scholars at Universities that would eventually absorb the refugee's entire salary. Although the Emergency Committee stopped funding Jollos in fall 1935, members of the Executive Committee, including Dunn, continued to find a solution for the precarious nature of Jollos's residence in the United States. Dunn felt personally responsible for Jollos's wellbeing and thought that members of the Emergency Committee should not abandon him. Feeling liable, Dunn and Cole did everything they could for Jollos,⁴⁵

⁴⁴ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid for Displaced German Scholars-Correspondence, Sep-Dec 1933 and 1934, Letters between Cole and Dunn from 16 Nov 1933 to 10 Feb 1934.

⁴⁵ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1933-1935, Dunn to Murrow, 3 Jul 1935.

who had managed to piece together a living by getting grants from several philanthropies including another refugee organization, the National Coordinating Committee for Aid to Refugees and Emigrants Coming from Germany.

Dunn did not give up on Jollos and devoted many years to finding Jollos a permanent position. In 1936 the Emergency Committee tried to place him at the University of Puerto Rico, but Dunn reported that it fell through because again there was no guarantee of a permanent post. Dunn requested at the next meeting that the Emergency Committee give the National Coordinating Committee a \$500 advance for Jollos to go on a lecture tour. They voted to give the National Coordinating Committee the money, which doled it out to Jollos as he needed it. About one year later Dunn continued to search for a solution for Jollos. Attached to the agenda for the 5 January 1938 meeting was a “Suspense List” on which Victor Jollos appeared with the following note: “Professor L.C. Dunn says the University of Colorado is going to apply to us for him.”⁴⁶ Dunn also had Jollos’s co-worker in the Zoology Department at University of Wisconsin, Professor Lowell E. Noland, tell Dr. Harry Sobotka of Mount Sinai Hospital in New York City about Jollos.⁴⁷ Neither University of Colorado nor Mount Sinai Hospital hired Jollos.

Jollos’s personality and ambition contributed in part to his placement problem. He had a broad knowledge of the biological sciences and was an

⁴⁶ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced German Scholars-Minutes 1936-1938, 3 Dec 1936, 20 Jan 1937, 2 Mar 1937, and 5 Jan 1938.

⁴⁷ APS, Dunn Papers, Series I, Box 17, Victor Jollos, 1934-1938, Noland to Sobotka, 25 Jan 1938.

accomplished researcher in parasitology and tropical diseases, but Jollos had problems with teaching. When a position opened at University of Wisconsin for a lecturer in zoology, Cole did not feel confident about putting Jollos in front of a large class of undergraduates and therefore filled the position with someone else. Moreover, Jollos had recently changed scientific fields. He was a protozoologist, but wished to do research in genetics and evolution. His peers did not doubt his abilities as a scientist, but they were not convinced that he had mastered genetic techniques and materials. Even Dunn felt that Jollos needed some additional training and suggested sending him to Thomas Hunt Morgan's laboratory in Pasadena to gain better foundations in genetics. Dunn followed up on this idea by talking with geneticist Calvin Bridges who worked in Morgan's laboratory, but Bridges's reply was not encouraging. Furthermore, Jollos added to his estrangement by supporting scientifically radical ideas that other scientists found unconvincing.⁴⁸

Jonathan Harwood notes the controversy surrounding Jollos's ideas in the 1930s. Many tried unsuccessfully to duplicate Jollos's experiments using heat to cause mutation. Two researchers finally managed to achieve replication in the mid-1930s; however, most biologists continued to doubt Jollos's theoretical claim that his experiments provided evidence of directed mutations. Jollos's reputation secured him a job at the University of Edinburgh from 1933 to early 1934, at which point the University of Wisconsin post opened to him. Moving from location to location

⁴⁸ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1933-1935, Dunn to Murrow, 3 Jul 1935, Dunn to Duggan, 7 Oct 1935, and Dunn to Drury, 11 Oct 1935.

interrupted Jollos's experiments and he spent most of the 1930s without the incubators needed to conduct his experiments. Harwood named Dunn as one of three scientists who supported Jollos's scientific reputation through the 1930s. Jollos's main problem was that German and American geneticists had different perspectives. Jollos was a developmental geneticist (as was Dunn, but this was true of few other American geneticists) and Jollos's ideas stood outside the mainstream in the United States.⁴⁹

Jollos and his wife also had problems adjusting to living in the United States in part because they had enjoyed a better quality of life in Germany. In addition to Jollos's eccentricities, his wife caused problems for the family. She had alienated most of the faculty's wives at the University of Wisconsin by being tactless and taking advantage of their hospitality. Their two daughters, who were ages sixteen and thirteen in 1937, adjusted more readily than their parents; they performed well in school and made friends easily.⁵⁰ As is evident in the case of Victor Jollos, he was not alone and decisions about his future depended on him and his family. Moreover, temperament commonly hindered refugees' adjustment to their new life in America.

By 1936 the family had significant financial troubles. Cole told Dunn that, "At the moment Professor Jollos is absolutely destitute, except for a little money they

⁴⁹ Jonathon Harwood, Style of Scientific Thought: The German Genetics Community, 1900-1933 (Chicago: The University of Chicago Press, 1993): 122-26. The other two scientists named were H.S. Jennings and Tracy Sonnenborn.

⁵⁰ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced German Scholars-Minutes, 20 Jan 1937, Mildred Lasker Kahn of the Joint Committee for the Readjustment of German Jews to Cecilia Razovsky of the National Coordinating Committee, 16 Feb 1937.

have been able to raise on the basis of some of Mrs. Jollos's jewelry. Professor Jollos is expecting payment soon for a manuscript of a popular book which he recently submitted to a German publisher."⁵¹ The Jollos family's situation deteriorated quickly. They were close to eviction from their home in January 1937 and about to apply for public relief. Dunn wanted to avoid bad press: "If they go on relief, they face deportation. If they are deported, our Committee will be in an ugly mess."⁵² The National Coordinating Committee managed to extend the Jollos family's stay in their home for an additional two weeks. Dunn got Jollos an invitation to go on a lecture tour speaking at the Rockefeller Institute, Princeton, Yale, and Johns Hopkins Universities. Dunn hoped that Jollos would receive \$500-\$600 for this lecture tour. Jollos sent Dunn the last of his Persian rugs to auction for money to live on. Bernard Flexner told Dunn to not sell Jollos's Turkish and Persian rugs, but after a few months Dunn had no choice. The rugs were appraised at \$400-500, and Jollos had already been given the money. Dunn personally had given Jollos \$100 and the University of Wisconsin gave him the other \$300. Dunn knew that he was going beyond his regular duties as a member of the Emergency Committee and remarked as much: "This is obviously apart from the work of the committee, but I can't feel right

⁵¹ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1936, Cole to John Whyte, who had joined the Emergency Committee in Dec 1935 and was acting as Assistant Secretary, 6 Mar 1936.

⁵² NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1937-1940, Telegram from Cole to Dunn, 30 Jan 1937.

in abandoning a colleague who through no fault of his own has fallen upon bad times.”⁵³

Victor Jollos suffered two heart attacks in 1937 and died on 5 July 1941 at the age of fifty-four leaving behind his wife and two daughters.⁵⁴ In reporting his death to members of the Executive Board, Betty Drury, the Emergency Committee’s secretary, accurately summarized Jollos’s situation: “This poor soul had a ghastly time. The Committee dropped him some years ago (there had been some criticism) and eventually he went on relief. Case of bad family adjustment which hampered him greatly.”⁵⁵ R.A. Brink, who was a Professor of Genetics at University of Wisconsin and worked with Jollos there, wanted Dunn to know that his efforts in the behalf of Jollos were appreciated and that Jollos’s inability to find a permanent post had nothing to do with Dunn. Dunn reflected that Jollos lacked resiliency and surmised that he embraced an end to his dismal situation: “He had many fine qualities and easy adaptability to the repeated misfortunes which he suffered was not one of them. I gathered from a letter from him that he had not much to live for and I suppose he welcomed death.”⁵⁶

⁵³ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1937-1940, Dunn to Whyte, 8 Feb 1937, Bernard Flexner, 10 Feb 1937 and Dunn to B. Flexner, 23 Apr 1937.

⁵⁴ Harwood, 126.

⁵⁵ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1937-1940, Drury to Duggan, 7 Jul 1941.

⁵⁶ APS, Dunn Papers, Series I, Box 17, Victor Jollos, 1938-1941, Brink to Dunn, 7 Jul 1941 and Dunn to Brink, 15 Jul 1941.

Dunn believed that the Emergency Committee was at fault to some extent because the organization had brought Jollos to the United States and then changed its procedures by only supporting scholars at institutions that agreed to take over the refugee's entire salary in one to two years time. The Emergency Committee's failure to follow-through with Jollos undoubtedly influenced Dunn's actions and he stepped outside of his role as a member of the Emergency Committee when dealing with Jollos's predicament. John Whyte, a member of the Emergency Committee, noted Dunn's commitment by saying in 1937, when Jollos was still alive, that Dunn "has tried through every means at this disposal to assist Professor Victor Jollos."⁵⁷

Hugo Iltis was another refugee to whom Dunn gave special attention over many years. Iltis relocated to the United States around early 1939, but Dunn began his search for a place for Iltis in May 1938. From then until the mid-1950s, Dunn helped Iltis and his family to adjust to living in Virginia and sought an established museum to purchase Iltis's Mendel memorabilia. Hugo Iltis had published a biography of Gregor Mendel in 1924 and had some of Mendel's personal items, including photostats of Mendel's autobiography and dried and pressed pear leaves labeled in Mendel's handwriting. Iltis also owned a portrait of Mendel painted by Joseph O. Flatter, a renowned cartoonist.⁵⁸ Although Flatter was born after Mendel died, he lived in Brünn, acquired photographs and portraits of Mendel, and used some of

⁵⁷ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 16, Series I, Scholars Receiving Aid, Victor Jollos, 1937-1940, Whyte to Dr. Jacob Billikopf, Executive Director of National Coordinating Committee, 2 Jul 1937.

⁵⁸ I have been unable to locate a rendering of Flatter's portrait of Mendel. The original is in the University of Illinois archive.

Mendel's living relatives as models in order to paint an accurate portrayal of Mendel in the Brunn countryside. Iltis and Flatter shared political views in their opposition to the Nazi regime.⁵⁹

With Mendel's items, Iltis developed an elaborate, anti-eugenic museum display incorporating biographical material on Mendel and educational explanations of Mendelism. Iltis opened a museum in Fredericksburg, where he lived and taught biology at Mary Washington College. After Iltis's death in 1952, his wife worked with Dunn to find a suitable home for the museum display. Dunn and his former colleague botanical geneticist Marcus M. Rhoades arranged for Rhoades's institution, the University of Illinois, to purchase the collection in its entirety, which is where the items remain today. Upon acquiring the artifacts, it was noted that the objects in Iltis's museum were most likely the only surviving "letters, photographs, experiment notes, and personal papers of Gregor Mendel" because whatever had been left in Czechoslovakia appeared to be destroyed or lost during the war.⁶⁰

⁵⁹ Hugo Iltis, Gregor Mendel: Leben, Werk, und Wirkung (Berlin: J. Springer, 1924). The biographical portion of this book was translated into English: Hugo Iltis, Life of Mendel, trans. Eden and Cedar Paul (New York: W.W. Norton and Company, 1932). Two articles provide information about the museum and its items: Hugo Iltis, "The Mendel Museum at Mary Washington College," The Scientific Monthly 56 (Apr 1943): 386-87; Rebecca Marston, "Museum to Mendel," The Sunday Star: Washington's Pictorial Magazine (1 Feb 1948): 10. The article tells a sensational story of Iltis's role in saving Mendel's items from destruction: "It [the museum] is a successor to his world-famous shrine at Bruenn, Czechoslovakia, confiscated by the Germans and destroyed by one bomb-whether a German or Russian is not known."

⁶⁰ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1953-1956, Mrs. Hugo Iltis to Dunn, 6 Jan 1956 and Dunn's reply, 2 Mar 1956. "Mendeliana," Science 127 (10 Jan 1958): 77; Information about the Iltis Mendeliana Collection, 1841-1984 can be

Fredericksburg was not an ideal location for the museum's items, according to Dunn, who sought a prestigious, metropolitan, central location for them. Dunn, however, was unable to convince Iltis during his lifetime to part with them. In 1946, Dunn contacted Ernst Mayr at the American Museum of Natural History in New York City because Mayr was interested in borrowing some of Iltis's materials for exhibition.⁶¹ Iltis, however, wanted someone from the American Museum of Natural History to visit his museum in Virginia. When no one made the trip to Fredericksburg, the plan fell through. Two years later, Dunn very kindly tried to persuade Iltis to let people borrow the museum's items, and again Dunn tried to arrange an agreement between Iltis and the American Museum, but to no avail. In 1950, Dunn convinced Iltis to loan some of the items to D.C. Rife of the Institute of Genetics at Ohio State University, and there was a long string of correspondence in which Dunn and Rife discussed the possibility of Rife's organization buying some of the Mendel documents from Iltis. The Institute of Genetics offered to purchase the museum from Iltis and to give him a job as curator after his retirement from Mary Washington College in 1952. Mary Washington College complicated things by presenting a counteroffer in which Iltis was "offered an appointment at Mary Washington after his retirement provided that he signs all rights of ownership of the

accessed through the University of Illinois at Urbana-Champaign Archive website: <http://web.library.uiuc.edu/ahx/archon> (19 Nov 2006).

⁶¹ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1946-1949, Dunn to Iltis, 20 Feb and 22 May 1946.

museum to the college.”⁶² Negotiations between Iltis and Ohio State University and Mary Washington College continued during 1951 and 1952. Moreover, Dunn tried again to get Mayr and the American Museum of Natural History interested in the items.⁶³ Dunn recognized that one source of these problematic negotiations was Iltis’s inflated view of his museum’s value. Only a few items of memorabilia were priceless pieces, according to Dunn, who considered the manufactured part of the display replaceable and dispensable.⁶⁴

The Iltis family adjusted well to life in the United States. In 1944, Iltis reported to the Emergency Committee: “I am building up the Mendel Museum in Fredericksburg. I am just going to apply for citizenship. Both my sons are serving in the U.S. army.”⁶⁵ Iltis and his family lived comfortably in Virginia, and he remained there for the rest of his life. On 22 June 1952 shortly after his retirement, Hugo Iltis died suddenly and painlessly. Hugh Iltis, one of Hugo’s sons, asked Dunn to write Iltis’s obituary and explained to Dunn that those considering writing it did not understand his father as Dunn did. Hugh considered his father’s liberalism “the mainspring of his whole life” and something that was not understood by R.C. Cook

⁶² APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1946-1949, Iltis to Dunn 3 Oct 1948 and Dunn’s reply, 6 Oct 1948, Dunn to Iltis, 20 Dec 1950, and Dunn to Rife, 20 Dec 1950.

⁶³ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1950-1951, several letters; Hugo Iltis, 1952, Dunn and Ernst Mayr correspondence from 28 Jan 1952 to 12 Feb 1952.

⁶⁴ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1946-1949, Dunn to M.R. Irwin, Department of Genetics at University of Wisconsin, 27 Apr 1949; Hugo Iltis, 1950-1951, Dunn to D.C. Rife, 17 Oct 1950.

⁶⁵ APS, Dunn Papers, Series I, Box 11, Emergency Committee in Aid of Displaced German Scholars, Excerpts from Letters from Grantees and Fellows, February-June 1944, 6 pages.

and W.J. Spillman. Dunn held a different and special place in Iltis's life, and Hugh wanted his father remembered by someone who understood him: "My father told me last summer that he felt that you came closer in sharing his viewpoints and feelings than anybody else he knew."⁶⁶ In a heartfelt obituary that appeared in Science, Dunn wrote of Iltis's honesty and dedication:

In reviewing the life of a colleague and friend, one sees the essence of the scientific calling itself...one cannot judge the grist by the number of pages or the space occupied, because it is part of a larger whole that is maintained not merely by addition of facts and theories, but by the acquisition of a spirit in which courage, integrity, and devotion, like those of Hugo Iltis, are essential ingredients.⁶⁷

After Iltis's death, his wife Anne became curator of the museum at Mary Washington College and worked with Dunn over several years to sell the museum's items.⁶⁸

Botanist Friedrich Oehlkers received aid from the Faculty Fellowship Fund for spring semester 1939, which appointed him as a Visiting Professor of Genetics at Columbia University. Oehlkers was not Jewish, but his wife was and as a result he was going to lose his academic post at the Botanical Institute of the University of Freiburg unless he divorced his wife. Not willing to end his marriage, Oehlkers sought another solution and wrote to Dunn. Dunn sprang into action by explaining to Columbia University's President Nicholas Murray Butler that there was little time to

⁶⁶ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1952, Hugh Iltis (Hugo's son) to Dunn, 26 June 1952. R.C. Cook was one of the people who considered writing Hugo Iltis's obituary.

⁶⁷ L.C. Dunn, "Hugo Iltis: 1882-1952," Science 117 (2 Jan 1953): 3-4, 4.

⁶⁸ APS, Dunn Papers, Series I, Box 16, Hugo Iltis, 1952 and 1953-1956, several letters between Anne Iltis, Dunn, and those that Dunn contacted to find a home for the museum.

make arrangements for Oehlkers, who had to obtain a leave of absence before a new law took effect three days later on 1 January 1939. Dunn had spoken with botanist Edmund Sinnott, his co-author of Principles of Genetics and they felt confident that Oehlkers would find a permanent post in the United States, but that he needed somewhere to go immediately while a post was sought. The Faculty Fellowship Fund agreed to give Oehlkers \$600 and a furnished vacant apartment, contributed by a professor on leave. Dunn stressed to President Butler that Oehlkers's appointment was temporary. Even though he had not yet received a response from Butler, Dunn wrote to Oehlkers on the same day inviting him to be a Visiting Professor at Columbia University. The next day, President Butler agreed to give Oehlkers an appointment at Columbia University with a salary of \$600 for one semester. Dunn cabled Oehlkers, who accepted the offer on 2 January 1939.⁶⁹ In one month, Dunn had heard about Oehlkers's troubles and arranged for his appointment at Columbia University. Since faculty members managed the Fellowship Fund and not administrators, Dunn made all of the arrangements and told Oehlkers of his appointment before Butler had officially granted it. Dunn was not only efficient, but he also held a great amount of power as Secretary of the Faculty Fund committee. His understanding with the University allowed him to act quickly and before getting affirmation from the University's President.

⁶⁹ APS, Dunn Papers, Series I, Box 23, Friedrich Oehlkers, 1938-1939, Oehlkers to Dunn, 4 Dec 1938, Dunn to Butler, 29 Dec 1938, Dunn to Oehlkers, 29 Dec 1938, and Fackenthal to Dunn, 30 Dec 1938.

While at Columbia University for the semester Oehlkers's ability to speak English improved greatly, and four institutions were interested in him: University of California, Indiana University, McGill University, and the Carnegie Institute of Washington at Cold Spring Harbor. Geneticist Ralph E. Cleland of the Department of Botany at Indiana University had collaborated with Oehlkers in the 1920s and wished to reestablish a working relationship with him. Dunn advised Cleland to try to get funding from the Rockefeller Foundation for Oehlkers, rather than from the Emergency Committee. Even though Dunn conveyed his opinion that the Emergency Committee would most likely give money to Oehlkers, its members had difficulties raising money and Oehlker's reputation made it possible for him to get money from the Rockefeller Foundation. C. Leonard Huskins of McGill University asked his institution about a post for Oehlkers, but was not optimistic about it coming through and noted that Oehlkers's interests matched those of others at University of California. Huskins tried to "stir them to action there" in Berkeley. Money and a post were not found for Oehlkers in the United States, however, and on 26 April 1939 Oehlkers boarded a ship and went back to Germany and his wife. He wrote to Dunn from Freiburg the next month saying that he had arrived safely and his laboratory was still intact.⁷⁰

⁷⁰ APS, Dunn Papers, Series I, Box 23, Friedrich Oehlkers, 1938-1939, Dunn to Ralph E. Cleland, 13 Jan 1939 and 15 Mar 1939, Huskins to Dunn, 10 Jan 1939, Oehlkers to Dunn, 21 Mar 1939 and 17 May 1939; Harwood, 79. Oehlkers's and his family were harassed during the 1930s. Eventually, his wife was taken to an extermination camp and most likely died there, and his son committed suicide during

Oehlkers had a broad and synthetic knowledge of biology. Moreover, he headed a research program in Germany, known as the “Black Forest School,” which strengthened and grew beginning in the mid-1930s. American geneticists voiced doubts about his belief in cytoplasmic inheritance, on which he lectured while visiting the United States during 1939. Like Jollos, Oehlkers’s scientific ideas fell outside of the American mainstream. Unlike Jollos, Oehlkers returned to Germany and lived until 1971, well beyond the war years.⁷¹

In addition to the scholars that he helped through the Emergency Committee in Aid of Displaced Foreign Scholars and the Faculty Fellowship Fund, Dunn also aided some of the Germany’s most accomplished geneticists, including Curt Stern, Joachim Werner Braun, Richard Goldschmidt, and Ernst Caspari. These men readily adapted to life in the United States in part due to their reputations and credentials, which allowed them to find jobs easily and to start work quickly once they relocated. Dunn’s role in helping each man was minimal in comparison to those mentioned previously, but it is evident that Dunn held a significant role in the relocations of Stern, Caspari, Braun, and Goldschmidt. These German men kept in close contact while in the United States and helped one another professionally.

Curt Stern lived for a few years in the United States before moving permanently in 1933. He spent two years as a postdoctoral fellow in Morgan’s laboratory in the late 1920s, came to the United States in 1932 to attend the Sixth

the war. Wikipedia, Deutsch: http://de.wikipedia.org/wiki/Friedrich_Oehlkers, 20 December 2006.

⁷¹ Harwood, 79-82, 195, 249-52.

International Genetics Congress held in Ithaca and stayed for six weeks as a lecturer at Case Western University. During this time he married an American woman.⁷² Stern returned to Germany for about one year before permanently relocating to the United States around May 1933. Stern went to Cold Spring Harbor before finding a permanent job, Sewall Wright asked Dunn to write a letter of recommendation for Stern. Stern was hired for the position, which was at University of Rochester,⁷³ and he stayed there until 1947 when he replaced Richard Goldschmidt, who retired from University of California. Three years before he retired, Goldschmidt started coordinating Stern's relocation to Berkeley.⁷⁴ As pointed out by Harwood, Stern adjusted easily to his life in the United States.⁷⁵ Additionally, Stern helped his fellow countrymen by telling Dunn that two of Alfred Kühn's students, Braun and Caspari, needed help relocating from Germany to the United States.⁷⁶

Joachim Werner Braun applied to medical school at University of Rochester, where Stern worked, but due to Jewish quotas he was not accepted. Richard Goldschmidt, who was still in Germany at the Kaiser Wilhelm Institute for Biology, wanted the Emergency Committee in Aid of Displaced German Scholars to consider

⁷² APS, Stern Papers, Ms Coll 5, Congress of Genetics, Sixth International (Ithaca, NY) 1932, Dunn, Chairman of Transportation Committee to Stern at Kaiser-Wilhelm Institut in Berlin-Dahlem, 3 Jun 1931; Harwood, 308-09.

⁷³ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid of Displaced German Scholars, 1933, Sewall Wright to Dunn, 11 May 1933 and Dunn to B.H. Willier, 15 May 1933.

⁷⁴ APS, Stern Papers, Ms Coll 5, Richard Goldschmidt, Folder 5, Goldschmidt to Stern, 22 May 1945 and Stern to Goldschmidt, 31 May 1945; Folder 7, Goldschmidt to Stern, 24 Dec [no year].

⁷⁵ Harwood, 309.

⁷⁶ Harwood, 87, 264.

Braun. Braun had strong credentials having been a student of Russian geneticist N.W. Timofeeff-Ressovsky (while Timofeeff was in Berlin) and German biologist Alfred Kühn of University of Göttingen. Braun was young, only nineteen or twenty years old, which put him outside of the Emergency Committee's parameters. As a result they did not fund him when he came to the United States in 1936.⁷⁷ Braun initially went to Ann Arbor where he worked with Franklin Shull, and he thanked Dunn for helping to find him a place to do research: "May I thank you very much for all your efforts in helping me to come here. I am really happy to have now a nice and interesting working place." From 1937 to 1941, Braun worked in Goldschmidt's laboratory at Berkeley. After Berkeley, Braun spent over twelve years from 1942 to 1955 at the University of California, Davis before moving to the Institute of Microbiology at Rutgers, where he remained until his death in 1972. When Stern needed advice about relocating to Berkeley in 1947, he turned to Braun for information about the housing market.⁷⁸

Richard Goldschmidt had been offered a position at Stanford University in 1932-1933, but chose to stay in Germany hoping that the situation would improve. In 1935, prior to officially losing his position, Goldschmidt asked Dunn to help him

⁷⁷ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid of Displaced German Scholars, 1934-1936, Richard Goldschmidt, 27 Mar 1934; ASP, Stern Papers, Ms Coll 5, L.C. Dunn, Folder 1, Stern to Dunn, 20 Apr 1934; Timofeeff-Ressovsky lived in Berlin and worked at the Kaiser Wilhelm Institute from 1925 to 1945 (Harwood, 55).

⁷⁸ APS, Dunn Papers, Series I, Box 3, Werner Braun, Braun to Dunn, 14 Oct 1936; APS, Stern Papers, Ms Coll 5, Werner Braun, several letters between Braun and Stern, 1940 and 1947; APS, Caspari Papers, Ms Coll 1, Joachim Werner Braun #1, partial obituary of Braun written by Caspari and William Firshein, n.d.

move to the United States, saying “What a fool I was to believe that it would be possible to stay in Germany and to wait for a recovery of sanity.”⁷⁹ Dunn had suggested a lecture tour to Goldschmidt while he was visiting Germany earlier that year, and indeed Dunn, with help from Stern and Wright, coordinated an American lecture tour for Goldschmidt. They arranged for him to deliver the Sigma Xi lectures at the University of Chicago in early December 1935. After that was in place, Dunn wrote to geneticists in the Northeastern United States asking them to have Goldschmidt speak at their institutions for a nominal fee. In addition to Columbia, Goldschmidt went to Cornell and Yale Universities.⁸⁰ While Dunn was making these lecture plans, Goldschmidt telegraphed to say that he had been “flunked” and that his trip to the United States was now open-ended.⁸¹ Dunn wrote to Alan Gregg of the Rockefeller Foundation and to Livingston Farrand, Chairman of the Emergency Committee, to see what could be done for Goldschmidt. Even after Goldschmidt arrived, Dunn continued to organize his lecture tour, which he considered a vehicle for Goldschmidt to receive an offer for a permanent job. Goldschmidt returned to Germany for about three months in 1936 before receiving a job offer from Berkeley,

⁷⁹ APS, Dunn Papers, Series I, Box 14, Richard Goldschmidt, 1928-Oct 1935, Goldschmidt to Dunn, Sep 1935 is handwritten with a question mark at the top of the letter.

⁸⁰ APS, Dunn Papers, Series I, Box 14, Richard Goldschmidt, Dunn to Sewall Wright, 14 Oct 1935, Stern to Dunn, 29 Oct 1935, Carl R. Moore of University of Chicago to Dunn, 29 Oct 1935, and Dunn to Ross G. Harrison at Osborn Zoological Laboratory, 29 Oct 1935.

⁸¹ APS, Dunn Papers, Series I, Box 14, Richard Goldschmidt, Nov-Dec 1935, Goldschmidt to Dunn, letter on 9 Nov and telegram on 11 Nov 1935.

where he started on 30 July 1936. Immediately after getting to Berkeley, Goldschmidt began his efforts to bring Werner Braun to Berkeley.⁸²

In March 1934, Alfred Kühn, who remained in Germany during the war, wrote to Stern in New York. Kühn hoped that Stern could help his student Ernst Caspari to relocate, and Stern immediately wrote to Dunn. Stern did not know Caspari, and neither did Dunn, but they eventually brought him to the United States.⁸³ Caspari conducted innovative genetic research for his dissertation project between 1931 and 1933, which contributed to his successful relocation. Knowing that normally *Ephestia* (a moth) have black eyes and its mutant red-eyes, Caspari compared other organs and proved that they also differed in color. He then transplanted the normal gonads of *Ephestia* into the mutant, red-eyed *Ephestia*, and bred ones with black-eyes and normal colored gonads. Caspari concluded that a hormone guided organ development.⁸⁴

Caspari spent from 1933 to 1938 as an Assistant in Microbiology at the University of Istanbul before arriving to the United States on 29 October 1938. He immediately went to Lafayette College in Pennsylvania as a Fellow in Biology and three years later was promoted to Assistant Professor of Zoology. Dunn arranged Caspari's fellowship and for Caspari to visit Columbia University from about two

⁸² APS, Dunn Papers, Series I, Box 14, Richard Goldschmidt, 1936-1938, Goldschmidt to Dunn, letters from Berlin dated April and May 1936, Goldschmidt to Dunn about Braun, 4 Aug, 18 Sep, and 16 Dec 1936.

⁸³ APS, Stern Papers, Ms Coll 5, Alfred Kühn, Kühn to Stern, 17 Mar 1934; APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid for Displaced German Scholars, 1934-1936, Stern to Dunn, 29 March 1934.

⁸⁴ Harwood, 87-89.

weeks in mid-December 1938. Dunn found him "...an interesting man and a very good teacher as judged from his seminars, even after only two months practice in English." Caspari remained at Lafayette College until 1944, leaving to work with Curt Stern at the University of Rochester and on the Manhattan Project for two years. Caspari then went to Wesleyan University as an Associate Professor of Biology (1949-1960) before returning to University of Rochester as a full professor and Chairman of the Biology Department (1960-1966).⁸⁵ Dunn not only helped Caspari to relocate, but also did research and published with him during the war years.⁸⁶

Dunn evidently held a pivotal position in these refugee committees, which allowed him to act decisively and get results. His efforts on behalf of refugees were accomplished behind-the-scenes and not as public campaigns. Even though he sought funding from his local community at Columbia University, he was not publicly advocating for aid. Nonetheless, Dunn facilitated the relocation of biologists and other scholars with a range of reputations and accomplishments who experienced a variety of situations after arriving to the United States.

⁸⁵ APS, Dunn Papers, Series I, Box 18, B.W. Kunkel, 1937-1948, Kunkel to Dunn, 1 Sep 1938; Box 19, Walter Landauer, 1938, Dunn to Landauer, 29 Nov 1938 and 26 Dec 1938. APS, Caspari Papers, Ms Coll 1, Caspari-Bibliography and Curriculum Vitae, Caspari's CV as of 1958; APS, Dunn Papers, Box 23, National Academy of Science, 1963-1972, n.d., Caspari career biography; An example of Caspari and Stern's Manhattan Project work: Ernst Caspari and Curt Stern, "The Influence of Chronic Irradiation with Gamma-rays at Low Dosages on the Mutation Rate in *Drosophila Melanogaster*," Genetics 33 (1948): 75-95; Harwood, 87-89.

⁸⁶ Ernst Caspari and L.C. Dunn, "Close Linkage between Mutations with Similar Effects," Proceedings of the National Academy of Science 28 (1942): 205-10; Ernst Caspari and L.C. Dunn, "A Case of Neighboring Loci with Similar Effects," Genetics 30 (1945): 543-68.

Dunn's assistance to scholarly refugees illustrates the degree to which scholarly networks operated among refugees and between the displaced and American scientists. In the case of the German circle (Goldschmidt, Stern, Caspari, and Braun), German refugees wished to help their fellow countrymen, whether they knew the refugee or not, and to preserve ties to the homeland after relocating. These German refugees managed to foster new networks among themselves as a result of being ousted from Germany, and developed relations with those in the United States who willingly assisted them. Dunn acted as a fulcrum for these four German geneticists, as he did for others, not only while they sought positions but also while they adjusted to their new lives in the United States. Thus, Dunn fostered new networks and expanded his intellectual community through the addition of refugees. Moreover, these biologists witnessed firsthand Dunn's efficient manner and his devotion to those of his scientific fraternity. They gave Dunn their loyalty in return.

Reflections and Liquidations

By the end of the war Dunn ascertained that fourteen scholars had been aided by the Faculty Fellowship Fund. Of these fourteen, only four of the Faculty Fellowship Fund recipients successfully adjusted to their new careers and remained at Columbia University. Julius Lips and Felix Bernstein had personal trouble adjusting and left Columbia University, but not the United States. All in all, however, the Faculty Fellowship Fund had successfully achieved its goal, in Dunn's own view, and

Columbia University had benefited institutionally from helping academic refugees by giving them money for living expenses and providing them with a place to work.⁸⁷

The Faculty Fellowship Fund was able to provide appointments based on the amount of money that its members raised. They collected almost \$4,000 by the end of 1933, which is commendable considering the financial crisis facing Americans at this time. This money came primarily from Jewish faculty members, who contributed whatever amounts they could spare. It was augmented with money from the Emergency Committee in Aid of Displaced Foreign Scholars, which sometimes matched the salary given to visiting scholars from the Faculty Fund. For the most part, grants ranged from \$1,000 to \$1,200 covering one-year of salary, and allowed a handful of refugees to come to Columbia University between 1933 and 1935, including mathematician Stefan Warschawski of Göttingen and archeologist Margarethe Bieber of Giessen.⁸⁸ Warschawski was a temporary visitor and returned to Europe, whereas Bieber, a senior scholar upon her arrival, was absorbed into Columbia University's faculty.⁸⁹

⁸⁷ APS, Dunn Papers, Series I, Box 13, Frank D. Fackenthal-Columbia University, 1943-1949, Dunn to Fackenthal, 21 Nov 1945.

⁸⁸ APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1933-1935, Dunn's Interim Report, 27 Nov 1933; Columbia University, Faculty Fellowship Fund, 1936-1938, Letter to the Officers of Columbia University, 5 Dec 1938.

⁸⁹ APS, Dunn Papers, Series I, Box 13, Frank D. Fackenthal-Columbia University, 1943-1949, Dunn to Fackenthal, 21 Nov 1945. The four were Margarethe Bieber, Paul Tillich, the Professor of Public Law Arthur Nussbaum, and biochemist Rudolf Schoenheimer.

Response to the Faculty Fellowship Funds fundraising in 1933 was significantly more generous than their later efforts because by December 1935 only \$4,600 had been raised, which included only \$600 collected during 1934 and 1935. By late 1935, however, members of the Faculty Fund recognized that what had been viewed as a temporary problem was turning into a long-term situation. This inspired members to make the Faculty Fund a permanent agency at Columbia University and revise their tactics for collecting money. In their January 1936 pledge drive, they asked for donations paid not only in lump sums, but also over one, two, and three years either monthly or annually.⁹⁰

Almost three years later, in late 1938 a two-page letter was sent to faculty members requesting their donations once again so that the Faculty Fellowship Fund could rebuild its balance, which had almost completely been dispensed, leaving under \$100.⁹¹ This fundraising effort received a response comparable to 1933 and allowed the Faculty Fund to again aid a handful of refugees. When Dunn prepared the Faculty Fellowship Fund's final report in 1952, he calculated that they had collected almost \$14,000 over ten years from 1933 to 1942. Most of this money, \$12,725, went to

⁹⁰ APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1936-1938, Report of Activities to January 1, 1936. When the Faculty Fellowship Fund was raising money again in 1938, Dunn noted that they were not as successful as they had been in 1933-1934 when they had raised \$7,000. This figure is significantly more than I have seen in the documentation (APS, Dunn Papers, Series I, Box 17, Walter Landauer, 1938, Dunn to Landauer, 26 Dec 1938).

⁹¹ APS, Dunn Papers, Series I, Box 5, Columbia University, Faculty Fellowship Fund, 1936-1938, to Officers of Columbia University, 5 Dec 1938.

salaries for refugees, only \$170 went to administrative costs, and about \$1,100 remained unspent.⁹²

The two bursts of donations came from the local community and can be seen as reactions to events overseas. The first windfall was received immediately after the Faculty Fund started and was in response to the first Nazi laws passed in April 1933, which called for the forced retirement of non-Aryans holding jobs as civil servants and professors. The second outpouring was in December 1938 in the aftermath of *Kristallnacht* on the night and morning of 9 to 10 November 1938. On 3 November 1938, Hitler banned Jews from theaters, concerts, high schools, and universities. Less than one week later, a Jewish teenager killed a Nazi official in Paris and Hitler fined the Jews in Germany one billion marks, which precipitated subsequent events. Literally meaning “night of broken glass,” on *Kristallnacht* Nazi rioters demolished Jewish synagogues and shops and shortly thereafter Jewish pogroms started in earnest.⁹³ In their December 1938 fundraising letter, the Fund’s committee remarked on the growing intensity and broadening nature of the situation in Europe.

Recent events in Germany, Austria, Czechoslovakia, Italy and Spain indicate that the need which called our Faculty Fellowship Fund into being has very greatly increased. Whereas in 1933, professors were

⁹² APS, Dunn Papers, Series I, Box 5 Columbia University-Faculty Fellowship Fund, 1950, 1967, Dunn’s Final Report, n.d. The precise amounts are \$13,964.98 collected, and \$12,724.96 for salaries, \$171.48 for administrative costs, and \$1,068.54 left unspent.

⁹³ Information about *Kristallnacht* comes from: Anthony Read and David Fisher, *Kristallnacht: The Nazi Night of Terror* (New York: Random House, 1989); Pauline Maier, Merritt Roe Smith, Alexander Keyssar, and Daniel J. Kevles, *Inventing American: A History of the United States, from 1865* vol.2 (New York: W.W. Norton & Company, 2003): 794; Ash and Söllner, 1.

dismissed from their posts in Germany chiefly for reasons of “non-Aryan” descent or because they were women or politically opposed to National Socialism, today the proscription applies not only to these classes, but to Catholics, to Protestants, and to all those who do not actively support the new regimes in the several countries.⁹⁴

The response to the Faculty Fellowship Fund’s requests for donations indicate that aid to refugees was not constant, but rather concentrated in particular periods in time, and furthermore that Columbia’s faculty gave generously in response to dire events overseas.

Undoubtedly, Americans responded to refugees in a variety of ways throughout the twelve years of 1933 to 1945. This chapter has explored a select group, who were benevolent toward these dispossessed foreigners. Many Americans, however, resented the influx of refugees to the United States. Some of this animosity stemmed from the economic crisis and the perception that refugees were taking jobs away from American citizens, a problem that the Emergency Committee recognized and attempted to defuse.⁹⁵ For example, Dunn listed three methods that the Emergency Committee used to alleviate hostility toward refugees. One: support refugees with unique qualifications. Two: do not support refugees who are in a field that is already saturated in the United States. Three: put refugees into American institutions that are in need of someone with their specialization and that have

⁹⁴ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1936-1938, FFF committee to Officers of Columbia University, 5 Dec 1938.

⁹⁵ APS, Dunn Papers, Series I, Box 13, Emergency Committee in Aid of Displaced German Scholars-Minutes, Aug-Sep 1940, Memorandum of Program, 12 Aug 1940; Ash and Söllner, 10.

suitable resources for the refugee's professional work.⁹⁶ When asked their thoughts about the United States, refugees remarked on their astonishment at the prevalence of anti-Semitism and racial prejudices. Specific complaints stressed the incompatibility of observable discrimination and American democracy, which was evident in colleges' Jewish quotas and in jobs that explicitly were not open to Jews.⁹⁷

As secretary, Dunn reflected on the Faculty Fellowship Fund's accomplishments on a couple of occasions. In April 1943 after the Faculty Fund's first decade of operations, Dunn penned a report for President Butler. The United States was actively fighting and it was impossible to guess the war's outcome. Yet, with the benefit of hindsight Dunn summed up the threat that fascism had been presenting to higher learning over the previous decade.

In April 1933 the first lists of professors dismissed from the German universities reached this country. They were long and impressive lists; and they made it evident that learning, wisdom, and attachment to the advancement and diffusion of knowledge were no longer the criteria by which scholars were to be judged. Instead, support of a new state theory, adherence to a racial doctrine without any scientific justification, and subservience to a fanatical leader became primary qualifications of all teachers and members of the universities.

From Germany these ideas spread elsewhere in Europe and Asia, according to Dunn, and the destruction of intellectuals, libraries, and scientific institutions demonstrated that fascist leaders viewed freedom of thought and learning as a threat to their absolute power.

⁹⁶ APS, Dunn Papers, Series I, Box 23, Friedrich Oehlkers, 1938-1939, Dunn to R.E. Cleland, 13 Jan 1939.

⁹⁷ Davie, 63-69.

Dunn's original goal for the report had been to summarize the logistics and technicalities of the Faculty Fund by listing the individual names of scholars and the amount of money each received; however, more philosophical issues kept invading his thoughts and Dunn ended up attempting to answer a few fundamental questions that he thought begged attention.

How could this great tragedy of cultural destruction have been avoided?

How can its victims be rescued and restored?

What part in the world will be played by the surviving and reconstituted universities?

What is the duty of learning in such a war-torn world?

Dunn resolved that Columbia University should sponsor an international conference in autumn 1944 at which scholars address these issues. American and displaced scholars living in the United States would be invited, as well as academicians in occupied and unoccupied countries. Dunn's goals were "reawakening confidence in mind and spirit" and bringing new hope to members of universities throughout the world since the same problems plagued them all. Dunn stressed his opinion that British, Russian, and Chinese representatives should be invited and every effort made to have them attend. President Butler liked the idea and by June 1943 Dunn chaired a committee and oversaw the planning of the World University Convocation. Two others involved with planning the Convocation had also participated with Dunn on

the Faculty Fellowship Committee: Joseph Chamberlain and historian Isaac L. Kandel.⁹⁸

About one decade later in his final report of 1952, Dunn summarized the Faculty Fellowship Fund's intention, to act as a local organization on behalf of political refugees, and discussed its relationship to national and international efforts in behalf of European political refugees. Dunn explicitly stated that a national agency, the Emergency Committee in Aid of Displaced Foreign Scholars, started its activities after the Faculty Fellowship Fund and moreover, that he became a member of the Emergency Committee "with the result that the two bodies worked closely together." Thus, Dunn's participation on both the local and national committees created a bond between the two organizations. But as he noted, he did not join the Emergency Committee in order to facilitate better results at a local level; rather his commitment to both refugee organizations made collaboration easy and resulted in the two agencies having close ties.⁹⁹

Dunn wrote this final report in 1952 because he wished to use the remaining fund balance toward paying for lecturers to speak on "Migrations of Scholars in the 20th Century" at Columbia University's upcoming 200th anniversary celebration.

Dunn contacted psychologist Wolfgang Köhler of Swarthmore College. "My hope,"

⁹⁸ UACL, Central Files, 1.1.236, Box 382, Folder 2, L.C. Dunn (Jul 1942-Jun 1943), Dunn to Butler, 27 Apr 1943 and 26 May 1943. Although there is no disruption in the numbering of archival folders, there appears to be missing documents between Dunn and administrators among Columbia University's records because there is nothing covering July 1943 to August 1945.

⁹⁹ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1950, 1967, Dunn's Final Report, n.d.

wrote Dunn, “would be to have the university world reminded of what has happened to scholars, not only in 1933 (although that would, I suppose, receive chief emphasis) but at other times as well as early victims of intolerance and political instability.”¹⁰⁰

In spring of 1952, Köhler gave a lecture at the University of Pennsylvania on refugee migrations titled, “The Scientists and their New Environment,” which was then published in The Cultural Migration: The European Scholar in America. Köhler declined Dunn’s offer and it is unclear who, if anyone, spoke on this issue.¹⁰¹

Thirteen of the 288 scholars, who received financial assistance from the Emergency Committee in Aid of Displaced Foreign Scholars, went to Columbia University, which is a large number in comparison to most institutions, but not the largest. The New School for Social Research created the University in Exile giving twelve scholars residence in 1933-1934 and aiding a total of 178 in all. Alvin Johnson of the New School not only gave them a place to go, but also supplied the necessary invitations and accommodations to satisfy immigration laws, and he raised money. Funds from the Emergency Committee, Rockefeller Foundation, and other organizations supported the University in Exile’s scholars. Twenty-one academicians went to the New School aided by the Emergency Committee. Palestine’s Hebrew University gave fifteen scholars jobs, while the Institute for Advanced Study in

¹⁰⁰ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1950, 1967, Dunn’s Final Report, n.d.; Box 17, Wolfgang Köhler, 1952-1953, Dunn to Köhler, 19 Nov 1952 and Köhler’s reply, 26 Jan 1953.

¹⁰¹ Wolfgang Köhler, “The Scientists from Europe and Their New Environment,” The Cultural Migration: The European Scholar in America (Philadelphia: University of Pennsylvania Press, 1953): 112-37.

Princeton and the Institute of Social Research in New York each sponsored eleven academics. Like Columbia, New York and Harvard Universities each supported thirteen scholars. Some universities gave between three and nine scholars a post, but most offered only one to two positions to refugees.¹⁰² Eighty-one of the Emergency Committee's 288 scholars specialized in the natural sciences, of which eight were biologists.¹⁰³ Two of these eight displaced scholars were discussed above: Victor Jollos and Hugo Iltis.

The Emergency Committee formed a Subcommittee on Liquidation in 1943 that quickly re-evaluated its name after several members questioned whether liquidation was the correct mode of action. Alan Gregg of the Rockefeller Foundation expressed his opinion to Chairman Duggan that the tasks for which the Emergency Committee was started had ceased to exist. In 1940 members of the Rockefeller Foundation joined the Emergency Committee's Executive Committee in order to respond to the influx of refugees as a result of the onset of the war. By late 1943 that need had been exhausted. Since its onset the Emergency Committee restricted its grants to scholars producing a high caliber of scholarship and qualified scholars no

¹⁰² Duggan and Drury, 200-04. Appendix VII is a table of the "Geographical Distribution of Emergency Committee Scholars, Rosenwald Fellows, and National Research Associates." Those who were given aid at two institutions were counted twice so that some overlap in the numbers occurs. Rutkoff and Scott, 84-86.

¹⁰³ Duggan and Drury, 193, Appendix I is a table of the "Fields of Specialization of Emergency Committee Scholars, National Research Associates, and Rosenwald Fellows." The other major categories and the number of scholars receiving aid were Humanities, 137; Social Sciences, 110; and Medical Sciences, 22.

longer sought money.¹⁰⁴ By way of comparison, the Emergency Committee's English counterpart had transformed from the Academic Assistant Council to the Society for the Protection of Science and Learning in 1936, and according to Lord Beveridge, their goal from inception was to protect teachers' freedom. The Society for the Protection of Science and Learning had a broader aim defining its purposes than the Emergency Committee, and it continues to operate today.¹⁰⁵

Dunn stated immediately that they should have liquidated in 1940 and stood by this opinion throughout the discussions. Dunn summarized the Emergency Committee's efforts in three fields. He thought that the grants-in-aid, the organization's original purpose that intended to help outstanding scholars, had ended three years prior. The amount of money given to scholars since 1940 proved to Dunn that the scholars in need of help more recently were of a lesser caliber than those aided during the 1930s. Two aspects of the Emergency Committee's activities still warranted consideration, according to Dunn: the fellowship program and the National Research Associates.¹⁰⁶ Dunn, however, was not a member of the Subcommittee on

¹⁰⁴ Members of the Rockefeller Foundation interacted with those working for the Emergency Committee beginning in 1933, but it was not an official partnership and the Rockefeller Foundation kept quiet about giving financial aid to the Emergency Committee. Duggan and Drury, 61-66.

¹⁰⁵ Beveridge, ix. London School of Economics and Political Science, Academic Assistance Council, http://www.lse.ac.uk/resources/LSEHistory/academic_assistance_council.htm (3 Dec 2006).

¹⁰⁶ National Research Associates were older scholars (over 58 years old) who received money from the Emergency Committee in order to relocate and were given access to libraries and laboratories at an American university. In 1939 Harlow Shapley had started a program at Harvard University, called the Harvard National Research Associates, and two years later he put the money that he had raised into a

Liquidation. Fred M. Stein, Alfred E. Cohn, and Nelson P. Mead acted on this subcommittee. Mead offered a less extreme alternative. He wanted to curtail the amount of work done by the Emergency Committee and suggested that no more grants-in-aid be given unless it was for “an extraordinarily brilliant scholar with a well recognized international reputation.” He hoped to dissolve the organization when the war ended.¹⁰⁷

After much debate, three options were presented and eventually a decision made. The choices included Dunn’s opinion that the reasons for which the Emergency Committee had been established had come to an end and therefore the organization should be liquidated. A second option was to secure funds from new sources in order to continue helping refugees. Lastly, they discussed tapering off their duties over one year, which would allow scholars to receive advice and get their questions answered. Some modified this third alternative by approaching the next year with an open mind since no one knew what lay ahead. They decided to continue their organization in a modified form for one year from 1 February 1944.¹⁰⁸

Members of the Emergency Committee’s Executive Board re-evaluated their options nine months later, and still they disagreed about the organization’s future.

trust and turned over the program to the Emergency Committee (Duggan and Drury, 84-85, 196).

¹⁰⁷ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box #167, Subcommittee Records, Subcommittee on Liquidation, 1943-1944, Executive Committee Meeting Minutes 1 Dec 1943, Duggan to Mead, 7 Dec 1943. Appendix to Minutes of 27 Jan 1944.

¹⁰⁸ APS, Dunn Papers, Series I, Box 13, Emergency Committee in Aid of Displaced German Scholars-Minutes, 1944, 27 Jan 1944.

Nelson P. Mead wanted to continue taking on new tasks, whereas Duggan wished to continue in order to see what was going to happen to the educational situation during the post-war era. A vote was taken in which five members wished to continue until the end of the European war and six, including Dunn, chose to dissolve the Emergency Committee. Since the vote was so close, no official decision was reached; however, it was determined that no new grants were to be awarded from November 1944 to June 1945.¹⁰⁹ In early 1945, members decided to close the office as soon as possible after 1 April 1945. Two projects remained unfinished, writing a history of the Emergency Committee in Aid of Displaced Foreign Scholars and making a report for the Rosenwald Family Association.¹¹⁰ Dunn obviously called for the organization's end as the war wound down and he distanced himself from the organization. He attended meetings irregularly beginning in 1943, which was the same year that he took on new tasks that focused on post-war reconstruction of science, such as Chairmanship of the American-Soviet Science Society and drafting legislation for what would become the National Science Foundation.

¹⁰⁹ APS, Dunn Papers, Series I, Box 13, Emergency Committee in Aid of Displaced German Scholars-Minutes, 20 Nov 1944.

¹¹⁰ APS, Dunn Papers, Series I, Box 13, Emergency Committee in Aid of Displaced German Scholars-Minutes, 1945, 29 Jan 1945 and 12 March 1945. Dunn did not attend either of these meetings. The Rosenwald Fellowships were given to refugees pursuing arts, music, architecture, and other creative professions. The money and provisions for these fellowships came from William Rosenwald of the Rosenwald Family Association. The Emergency Committee chose the recipients and performed the administrative tasks (Duggan and Drury, 72-73).

Other Dimensions of Dunn's Efforts

As time passed, Dunn joined other organizations and started new campaigns in order to help foreign refugees outside and potential enemies inside of the United States. Dunn's later activities demonstrate a change in his thinking and approaches to dealing with the fascist threat. In 1940 he broadened his efforts to help all refugees and stepped into a more political and public sphere as an Executive Board member of the American Committee to Save Refugees by going to Washington D.C. to work with politicians on the behalf of refugees. He also acted on the Emergency Committee in Aid of Displaced Foreign Scholars's Subcommittee of Japanese Scholars, which dealt with Japanese internment in California.

The American Committee to Save Refugees (ACSR) coordinated the efforts of various refugee organizations and acted on behalf of all refugees from its headquarters on Fifth Avenue in New York City. Dunn was on the Executive Board from its inception on 19 July 1940, but his actions predate its founding. In actuality, Dunn and chemist Harold Urey inspired the establishment of the American Committee to Save Refugees when they sent out a letter in June explaining the plight of anti-fascist refugees in France. Miss Persis Miller, who had been aiding refugees for quite some time at this point, called for the formation of a committee to save anti-fascist refugees. Dunn undertook what the ACSR considered one of the

organization's first actions when he went to Washington D.C. to visit the State Department.¹¹¹ Dunn's intention was

- (1) to get anti-fascist refugees out of French concentration camps and out of France;
- (2) to smoothe [sic] the way for the entrance of British scientists who might be evacuated.

Dunn doubted that the first goal could be achieved because of Americans' fears of communism and the inability to differentiate between communists and non-communists. France had a large Communist Party and its members called for an end to the war with Germany toward the end of 1939. Nonetheless, Dunn and others planned to pressure politicians into action and targeted their activities toward President Franklin D. Roosevelt, the French Embassy, and the Red Cross.¹¹² Dunn also wrote letters to his American and British colleagues in an attempt to develop a plan for a possible evacuation of scholars from Britain and France.¹¹³

¹¹¹ APS, Dunn Papers, Series I, Box 1, American Committee to Save Refugees, 1940-1942, Report of the Work of the American Committee to Save Refugees, 28 Nov 1940.

¹¹² APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1940, Dunn to Landauer, 22 Jun 1940. This is one of few formal, typed letters that Dunn sent to Landauer and it focused on political information with few personal updates. Gerhard L. Weinberg, A World at Arms: A Global History of World War II (Cambridge: Cambridge University Press, 1994): 104.

¹¹³ APS, Dunn Papers, Series I, Box 3, British Scientists-plans for evacuation, 1940, 26 letters to and from Dunn, dated between 17 Jun 1940 and 23 Oct 1940. Dunn suffered a heart attack during this summer (see chapter 5 for more information), which might have occurred between 22 and 30 June 1940. There is a break in Dunn's outgoing correspondence between 22 June and 7 Aug 1940, and in a letter dated 30 June 1940 Persis Miller wished Dunn "a good rest and speedy recovery" from his illness.

The American Committee to Save Refugees summarized its general purpose in terms of cooperation among and coordination of the various organizations in support of foreign refugees.

It is believed that by cooperation among them [organizations concerned with various special categories of refugees] and by drawing in the interest of as many individuals and local bodies as possible, the maximum effect will be achieved in this great emergency need. Our policy therefore shall be to carry on the widest possible campaign for aid for all anti-fascist refugees and to cooperate with all organizations having a common purpose.

The ACSR outlined seven specific goals to guide its actions, including releasing prisoners, aiding evacuations, directing governmental relief efforts, providing asylum in American countries, and collecting funds to perform these operations. Their actions were directed at political leaders in North and South America, the French, German, and Italian governments, as well as existing organizations that gave aid overseas, such as the Red Cross and Quakers.¹¹⁴ In acting on the ACSR's Executive Board, Dunn was among friends from Columbia University: anthropologist Franz Boas, sociologist Helen Merrell Lynd (wife of Robert S. Lynd), and economist Walter Rautenstrauch.¹¹⁵

Discrimination against refugees had erupted in California in the months after Pearl Harbor. Goldschmidt wrote what he called an "urgent appeal" on behalf of the European "enemy aliens," i.e. Germans and Italians, living in California who had not

¹¹⁴ APS, Dunn Papers, Series I, Box 1, American Committee to Save Refugees, 1940-1942, Objectives of the American Committee to Save Refugees, 19 Jul 1940.

¹¹⁵ APS, Dunn Papers, Series I, Box 1, American Committee to Save Refugees, 1940-1942, letterhead. Walter Rautenstrauch was chairman of the ACSR.

yet been in the United States long enough (five years) to have become naturalized. Local governments harassed alien Germans and Italians now that the US had gone to war. Goldschmidt was one of many people expected to leave their houses within the next three weeks. Goldschmidt asked Dunn to work with Ross G. Harrison and the members of the Emergency Committee to rectify the problem. Calling the situation un-American, Goldschmidt lamented “that Adolf Hitler will gloat when he hears that his victims are again being victimized,” and noting “that immediate action is urgent especially on the West Coast.”¹¹⁶

Japanese Internment in California also alarmed Goldschmidt, who had a Japanese graduate student, Masuo Kodani, who was to be interned. Goldschmidt’s students collected money for Kodani hoping to find him a scholarship, and Goldschmidt wanted to know whether the Emergency Committee might take up the problem of Japanese internment. During the week before Goldschmidt sent his letter, Dunn had been asked and agreed to act on a sub-committee dealing with the Japanese Question. The sub-committee’s two other members were Alfred E. Cohn and Charles J. Liebman. Dunn took Goldschmidt’s letter to the next Board meeting of the Emergency Committee thereby bringing Kodani’s situation to the attention of those not on the Subcommittee on Japanese Scholars.¹¹⁷

¹¹⁶ APS, Dunn Papers, Series I, Box 14, Richard B. Goldschmidt, 1939-1950, Goldschmidt to Dunn, 5 Feb 1942. The underlining was Goldschmidt’s emphasis.

¹¹⁷ APS, Dunn Papers, Series I, Box 14, Richard B. Goldschmidt, 1939-1950, Goldschmidt to Dunn, 30 Apr 1942 and Dunn’s reply, 14 May 1942; NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box #167,

By May 1942, Kodani was interned and Goldschmidt was trying to get him a position at University of Missouri. Dunn understood that Japanese internment was not within the scope of the Emergency Committee's aims, but saw parallels between the situation of Japanese in the United States and foreign scholars in fascist controlled countries.

My own position is that Japanese scholars displaced from their positions in the western coast universities seem to be in the precisely same positions as scholars displaced, through no fault of their own, from European universities. And if our main purposes in all of this work are to preserve academic freedom and the continuity of scholarship, I believe we should make the same efforts in behalf of all scholars suffering from the revolutionary situation created by the Axis.¹¹⁸

When validating the Emergency Committee's involvement in the Japanese question, Dunn was possibly cognizant of a potential criticism and one that Harlow Shapley later expressed. Shapley did not understand the correlation between the Emergency Committee's goals and Japanese internment. The Committee had been established as a way to help foreigners who resided outside of America, and Shapley thought that as an organization they should not interfere with problems internal to the United States.¹¹⁹ For Dunn a larger problem loomed and he planned to protect Americans' civil liberties, including saving scholars from persecution.

Subcommittee Records, Subcommittee on Japanese, 1942, Chairman [Duggan] to Dunn on 22 Apr and Dunn's reply, 29 Apr 1942.

¹¹⁸ APS, Dunn Papers, Series I, Box 4, Alfred Cohn, 1936-1942, Dunn to Cohn 27 May 1942.

¹¹⁹ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box #167, Subcommittee Records, Subcommittee on Japanese, 1942, Shapley to Fred M. Stein, 4 Jun 1942.

Protecting Civil Liberties

Beginning in the early to mid-1930s, Dunn and Boas developed a close working relationship that revolved around their humanitarianism and similar political leanings. They interacted with one another on two committees started at Columbia University during the 1930s, the Faculty Fellowship Fund and University Federation for Democracy and Intellectual Freedom. The University Federation developed into a national organization with a similar name, the American Committee for Democracy and Intellectual Freedom. This American Committee was one of many anti-fascist campaigns that occupied Dunn's time from 1936 to the early 1940s. He contributed to the cause by attending rallies, giving radio addresses, going to Washington D.C. as a delegate, and signing telegrams sent to President Franklin D. Roosevelt.¹²⁰

As members of the Faculty Fellowship Fund, Boas and Dunn each suggested refugees who could potentially benefit from the Faculty Fund and they voted on whose salary to subsidize with the group's money. Boas funneled suggestions through Dunn in hopes that the Emergency Committee in Aid of Displaced Foreign Scholars would act favorably.¹²¹ Occasionally they did, as was discussed through the cases of ethnologist Julius Lips and mathematician Felix Bernstein. Likewise, Dunn asked Boas to suggest Alfred Kühn as someone in need for the Emergency Committee's aid.

¹²⁰ APS, Dunn Papers, Series I, Box 1, American Committee of Democracy and Intellectual Freedom, 1940.

¹²¹ APS, Boas Papers, B: B61, L.C. Dunn, Dunn to Boas about Julius Lips, 2 Mar or May 1935 from Oslo, and Dunn to Boas, 12 Jul 1936.

Dunn most likely wanted the nomination to come from someone who was not on the Executive Committee.¹²² Moreover, Boas oversaw the Faculty Fund when Dunn was taking his sabbatical leave in 1934-1935.¹²³ In addition to Dunn, Boas asked favors of other members of the Emergency Committee; for example, Boas had a longstanding relationship with anthropologist Livingston Farrand, who was Chairman of the Emergency Committee and President of Cornell University.¹²⁴

In addition to their mutual desire to help refugees, Boas and Dunn's physical proximity and political opinions also brought them together. Not only were they on the same campus, but their departments shared the same building, Schermerhorn Hall.¹²⁵ Politically, both men were sympathetic to socialism.¹²⁶ With reference to race, Boas and Dunn used scientific knowledge to counteract scientific racism.

Scholars have noted the degree to which Boas's perspective influenced anthropology

¹²² APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Dunn to Boas, 20 Jun 1938. Dunn told Boas to make sure the Emergency Committee knew that the University of North Carolina had \$750 for Kuhn's salary and that Swarthmore was second option. Kuhn remained in Germany throughout the war.

¹²³ There is not a statement that Boas officially took over as Secretary of the Faculty Fellowship Fund, but that appears to be the case from the correspondence. New York Public Library, Emergency Committee of Displaced German Scholars, Box 111, Colleges & Universities-Columbia University, 1934 and 1935.

¹²⁴ APS, Boas Papers, B:B61, Livingston Farrand, 4 folders of correspondence dating from 1899 to Farrand's death in 1939.

¹²⁵ APS, Boas Papers, B:B61, F.D. Fackenthal #10. Boas had his office at on the 4th floor (400s) of Schermerhorn Extension around 1930-1932. There are references to that Dunn and his colleagues had space on the 6th floor and Dunn's office was on the 8th floor of Schermerhorn Hall. The two wings are connected.

¹²⁶ For information about Boas's socialist outlook, see: Margaret M. Caffrey, Ruth Benedict: Stranger in this Land (Austin: University of Texas Press, 1989): 289.

as a discipline and people's understanding of racial differences in general.¹²⁷ Even though Dunn had been hesitant about eugenicists' application of genetic principles since the mid-1920s, his views solidified with the advent of Nazi laws, and he more or less adopted a Boasian view about race.

Whereas Dunn initially acted on committees that worked privately to aid refugees following the advent of Nazi laws in 1933, Boas quickly entered the public sphere with a political agenda. He was about seventy-five years old and frequently ill, and yet Boas remained fervent and active until his death one decade later.

Preservation of democracy and civil liberties formed the core of Boas's political activism during these ten years. He supported freedom of thought in particular.¹²⁸ The culmination of his energies produced a national organization in 1939, which he chaired, called the American Committee for Democracy and Intellectual Freedom.

Boas's student, Ruth Benedict, and Dunn were members of its Executive Committee.¹²⁹

As a result of new issues facing Americans in the late 1930s, Dunn consciously decided to combat fascism and preserve academia in any way possible,

¹²⁷ Harvey A. Levenstein, "Franz Boas as Political Activist," Kroeber Anthropological Society Papers 29 (1963): 15-24; Vernon J. Williams Jr., Rethinking Race: Franz Boas and His Contemporaries (Lexington: The University Press of Kentucky, 1996); Claudia Roth Pierpont, "The Measure of America: How a Rebel Anthropologist Waged War on Racism," The New Yorker (8 Mar 2004): 48-63.

¹²⁸ Marshall Hyatt, Franz Boas, Social Activist: The Dynamics of Ethnicity (Connecticut: Greenwood Press, Inc., 1990): 144-45.

¹²⁹ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1937-1939, Press Release from American Committee for Democracy and Intellectual Freedom, dated Monday, November 20 [1939].

and he wished to do more than simply aiding refugee scientists. To this end, Dunn decided to give more time to the kinds of problems being addressed by these newly-established organizations and he resigned from quite a few committees beginning in January 1938.¹³⁰ In a letter expressing his intention to resign to members of the Emergency Committee in Aid of Displaced Foreign Scholars in 1938, Dunn stated that he wished to devote his time to “the conditions which have led to the suppression of intellectual freedom in so many countries.” He then summarized his new strategy, by saying: “For me the time has come when to attack the disease itself seems more important than to rescue its victims.”¹³¹

Alfred Cohn, who was on the Executive Board of the Emergency Committee with Dunn, pleaded with Dunn to continue aiding refugees. Though their political collaborations, Cohn developed an admiration for Dunn, which he expressed in an attempt to get Dunn to reconsider his resignation: “Getting to know you, and the seriousness and generosity and resourcefulness with which you approach these problems has been to me very impressive and a source of very deep gratitude.” In addition to his admiration, Cohn felt more intellectually compatible with Dunn than with other members of the Emergency Committee. Dunn was persuaded to stay on the Emergency Committee’s Executive Board and relayed his commitment to Cohn on 6 Feb 1938. It is evident from Cohn’s reply that personal problems, intellectual fatigue,

¹³⁰ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1938, Dunn to Landauer, 20 Jan 1938.

¹³¹ New York Public Library, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records for Duggan and Dunn, L.C. Dunn 1934-1938, Dunn to Drury, 6 Jan 1938.

and new passions had convinced Dunn that he needed to resign.¹³² Dunn's inability to follow-through with the resignation demonstrates his compassion and dedication, while also pointing to one of his flaws. Dunn had a hard time saying no, especially if the consequences meant overworking and hurting himself.

Before developing into a national organization, the American Committee for Democracy and Intellectual Freedom had its roots in a campus organization formed in 1936, Columbia University's Faculty Committee for Medical Relief to Spain. The medical group at Columbia University was not unique. A similar, more inclusive New York-based campaign, the American Friends of Spanish Democracy, started in response to atrocities resulting from the Spanish Civil War. Its members raised enough money to send sixteen medical personnel, four ambulances, and \$20,000 in medical supplies to Spain.¹³³ In February 1937, Dunn wrote to Secretary of State Cordell Hull upon learning that a restriction impeded Americans and a second envoy of medical supplies from going to Spain. The blockage defeated humanitarian purposes, according to Dunn, who hoped the decision would be reversed.¹³⁴ Dunn also asked Boas to sign an appeal for those in Spain by relating the issues facing Spaniards to those facing Boas's fellow countrymen in Germany. Boas agreed to sign

¹³² APS, Dunn Papers, Series I, Box 4, Alfred Cohn, 1936-1942, Cohn to Dunn, 3 Feb and 14 Feb 1938. Dunn replied on 6 Feb 1938 which is a letter that I have not located. From Cohn's statements on 14 Feb 1938, Dunn had changed his mind and would not be resigning from the Emergency Committee.

¹³³ Caffrey, 286-87; APS, Dunn Papers, Series I, Box 25, Aid to Spain, 1937, n.d., Roger Chase, General Secretary of Medical Aid to Spain.

¹³⁴ APS, Dunn Papers, Series I, Box 25, Aid to Spain, 1937, n.d., Dunn to Hon. Cordell Hull, 19 Feb 1937.

it.¹³⁵ The appeal to which Boas lent his name was most likely a telegram sent from faculty members at Columbia University to General Cabanellas, President of the Junta, which read: “The undersigned members of the Columbia University in New York urge the commutation of the death sentence passed on Dr. Leopoldo Alas President of Oviedo University.”¹³⁶ For Dunn, this action was in line with his efforts in behalf of refugees, who were also politically persecuted scholars.

By fall 1937, Columbia University had started a Faculty Committee for Aid to the Spanish People. The academicians associated with the Committee for Aid to the Spanish People participated on several of the same campaigns as Dunn, including Boas, Mitchell, Lynd, Clarke, and Rautenstrauch, who also signed the appeal mentioned above. Additional members of the Faculty Committee for Aid to the Spanish People included Ruth Benedict, Selig Hecht, John Dewey, Robert MacIver, Harold Urey, and Mark Van Doren. This Faculty Committee collaborated with scholars outside of Columbia University too. For example, Boas, Dunn, and eighteen other prominent American citizens signed an appeal that was sent to military leaders in Spain asking them to stop executing civilians, especially Government officials and

¹³⁵ APS, Boas Papers, B:B61, L.C. Dunn #3, Dunn to Boas, 5 Feb 1937 and Boas’s reply, 8 Feb 1937.

¹³⁶ APS, Dunn Papers, Series I, Box 25, Aid to Spain, 1937, n.d., Telegram sent 11 Feb 1937 from CU to General Cabanellas, President of the Junta. In addition to Dunn and Boas among those who signed the appeal were W.C. Mitchell, Robert S. Lynd, Hans T. Clarke, Walter Rautenstrauch, J.P. Chamberlian, and George B. Pegram

professors. Another cable went to President Franklin D. Roosevelt requesting that the United States actively help these Spaniards.¹³⁷

By December 1937 seven members of the Faculty Committee for Aid to the Spanish People had recognized a need to diversify their goals.

The undersigned members of the Committee, however, feel that groups interested in the problems created by Fascism in Europe and South America, and in intellectual freedom generally, must make common cause and concentrate their efforts against the spread of Fascism.

The signers were Boas, Dunn, Selig Hecht, Robert S. Lynd, Walter Rautenstrauch, Harold Urey, and Willystine Goodsell. They asked faculty and students to attend a meeting on 17 December in Schermerhorn Hall, which resulted in the formation of the University Federation for Democracy and Intellectual Freedom.¹³⁸

The University Federation for Democracy and Intellectual Freedom was a campus organization that recognized the general threat of fascism. Harold Urey was the University Federation's Chairman. Dunn was vice-chairman of the Federation representing Columbia's faculty and he chaired its Committee in Aid of Spain.

Benedict acted as the organization's secretary and oversaw its Membership Committee. Boas was Chairman of the University Federation's Committee on

¹³⁷ APS, Boas Papers, B:B61, Franz Boas #8, Press Release written by Boas for release on 5 Sep 1937. Others who signed both appeals were Albert Einstein, Walter B. Cannon, and Senator Gerald P. Nye of North Dakota.

¹³⁸ APS, Boas Papers, B:B61, Faculty Committee for Aid to the Spanish People, letter to Faculty and Student Organizations, 8 Dec 1937; APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1937-1939, Benedict's summary of 17 Dec 1937 meeting, n.d. She wrote the summary before 14 Jan 1938, the date of the University Federation's first official meeting.

Intellectual Freedom. The University Federation summarized its membership and goals in the following manner:

The Federation is made up of faculty, alumni, and students who believe that learning and democracy can flourish only with the guarantee of civil liberties and intellectual freedom. In a world where these guarantees are being laid aside the Federation accepts an obligation to assist victims and to oppose extension of such aggressions.¹³⁹

One of Dunn's main objectives as a member of the University Federation concentrated on his role as chairman of the Committee in Aid of Spain. He organized an exhibit through Columbia University's Teachers College on changes in education in Spain.¹⁴⁰

It is hard to ascertain the degree of Dunn's involvement with the University Committee and its national counterpart, the American Committee; however, he obviously believed in their tenets and lent his name to their campaigns. Immediately after the University Federation was established, Dunn wrote his colleague at Columbia University, W.C. Mitchell, who was acting President of the American Association for Advancement of Science (AAAS), about the University Federation's activities. The AAAS had circulated a resolution about its commitment to intellectual freedom that Dunn noted aligned with the goals devised by the University Federation

¹³⁹ Peter J. Kuznick, Beyond the Laboratory: Scientists as Political Activists in 1930s America (Chicago: The University of Chicago Press, 1987): 181-83; APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Pamphlet with UFDIF's By-laws, no date.

¹⁴⁰ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Dunn to Prof. Federico de Onis of Columbia University's Department of Romance Languages, Hispanic Languages, 12 March 1938 and Dunn to George S. Counts of the Teachers College, 19 Apr 1938.

at roughly the same time.¹⁴¹ Mitchell later became Chairman of the University Federation for Democracy and Intellectual Freedom.¹⁴² When this campus organization developed into a national group in February 1939, Dunn and Boas were elected onto a five person subcommittee that was to “carry on routine matters between meetings of the Executive Committee.”¹⁴³

The goals of the American Committee for Democracy and Intellectual Freedom built on those of its forerunner and averred that adversity can be overcome by educating the masses and cooperation. “We are convinced,” read one of their mission statements, “that a concerted effort to present to the American people the truth about our traditions and history, about races and nationalities, about education and culture, about war and peace can contribute much to weaken the effectiveness of anti-freedom, anti-alien, and pro-war propaganda.”¹⁴⁴ Peter Kuznick in Beyond the Laboratory has recognized that Boas, Dunn, and other faculty at Columbia University were pivotal to scientists’ initial forays into the political sphere during the 1930s, and

¹⁴¹ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1937-1939, Dunn to Mitchell, 21 Feb 1938.

¹⁴² APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Letterhead, n.d. Dunn was no longer Vice-Chairman at this point.

¹⁴³ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Press Release, 20 Nov 1939, and Minutes of the National Executive Committee Meeting, 9 Dec 1939. The other three members were Ned H. Dearborn, Dean of General Education at New York University, President Frank Kingdon, University of Newark, and Dr. William M. Malisoff of Polytechnic Institute of Brooklyn.

¹⁴⁴ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Nov 1939-Statement of Record Program’s Needs, 9 pages, 1.

his book has chapters on Boas's efforts to mobilize scientists and the American Committee for Democracy and Intellectual Freedom.¹⁴⁵

One of Boas's main campaigns in the late 1930s and early 1940s was broaching the subject of race and undermining scientific racism. Boas started a committee in 1937 that engaged in interdisciplinary studies of race. Dunn and several other members of Columbia's faculty participated on it. In addition to anthropology and genetics, two other disciplines, sociology and psychology, were represented.¹⁴⁶

Overwhelmed with committee work, Dunn eventually told Boas that "the [committee's] questions are sufficiently far removed from my immediate interests so that it would take more time than I have to advise intelligently concerning them."¹⁴⁷

Dunn continued his activism in other arenas. Tackling the race issue in general was not a focal for Dunn in the late 1930s, but it would be after the war ended. The American Committee undertook Boas's anti-racism campaign having one of the most renowned American geneticists, Secretary of Agriculture Henry Wallace, speak about

¹⁴⁵ See Kuznick's chapter, "Franz Boas Mobilizes the Scientists against Fascism" (171-194) and "Scientists Establish the American Committee for Democracy and Intellectual Freedom" (195-226), in *Beyond the Laboratory*.

¹⁴⁶ Kuznick, 180. The group engaged in "Studies in the Determination of Population Qualities by Genetic and Environmental Factors." The other members from Columbia University were Robert M. MacIver, Gardner Murphy, and Otto Klineberg.

¹⁴⁷ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1938-1939, Boas to Dunn, 17 Oct 1938 and Dunn's reply, 18 Oct 1938.

“Racial Theories and the Genetic Basis of Democracy” at the American Committee’s inaugural event in February 1939.¹⁴⁸

Conclusion

At some point, Dunn handwrote his personal views about the refugee situation and anti-fascist campaigns, which is reproduced in its entirety below. In it, Dunn shares his opinion about the university’s role, and therefore the academician’s function, in addressing the political upheaval of the 1930s and 1940s.

Professors can have no illusions as to their aloofness from controversial issues in the current world. In Germany, Italy, and Fascist Spain free thought in the university was one of the first things [unreadable word] which Fascist authority struck. And now Japan is reported to be systematically bombing Chinese universities. If universities have any function in a world of controversy it is that of providing an island of free thought where the divergent points of view on which social change depends for effective guidance can receive open and sympathetic discussion. For people interested in keeping the critical intellectual function alive, a policy of drift and hope as regards “academic freedom” is no longer tenable. We know what drift involves in the midst of present power forces. The present move of American academic people to organize in behalf of the common responsibility which they share with their colleagues in the universities of the world is therefore no empty gesture. The interest and future of academic people and of democracy are not merely similar; they are identical. Germany, Spain, Italy, China – they are in a very urgent sense our own problem.¹⁴⁹

¹⁴⁸ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1937-1939, New Bulletin on the Activities of the Local Committees in Arranging Lincoln Birthday Meetings.

¹⁴⁹ APS, Dunn Papers, Series I, Box 25, Aid to Spain, 1937, n.d., Dunn’s handwritten notes on the backside of letterhead for the Columbia University Faculty Committee for Aid to Spanish People.

Although Dunn continued his committee work to aid refugees until after the war ended, he transferred his attention in the late 1930s and early 1940s to fighting fascism in a more general way, as was discussed above. In addition to this shift, Dunn also turned his attention to preparing America for battle in the event that the United States entered the war. War mobilization and post-war preparation served as other major rallying points around which Dunn acted, and these two aspects serve as the focus of the next few chapters.

Chapter 4

Funding Science: National Responses to War, 1938-1945

“It’s because I believe we still have power to influence the future that I have been turning more and more to political activity.”

L. C. Dunn, 27 July 1940¹

Prior to the German invasion of Poland on 1 September 1939 the United States started mobilizing for a possible war. Military and governmental agencies as well as industrial companies expanded in an effort to prepare personnel and supplies that would be needed should the United States enter into combat. After Japan attacked Pearl Harbor in December 1941, the United States officially entered the Second World War and American citizens increased their attention to war-related needs. Some American scientists, especially physicists, contributed their expertise early on by beginning basic research in pertinent fields. The Manhattan Project and its development of the atomic bomb is a well-known example. Ultimately, a significantly greater number of scientists worked for the government and the amount of federal money devoted to science increased substantially in comparison to pre-war years.²

¹ APS, Dunn Papers, Series I, Box 19, Walter Landauer, Dunn to Landauer, 27 July 1940.

² V.R. Cardozier, The Mobilization of the United States in World War II: How the Government, Military and Industry Prepared for War (North Carolina: McFarland &

Mobilization and the war presented American scientists with new challenges. Some contributed by immediately developing weapons and devices that would help in the fight; others sought recognition from the United States government because they wanted to contribute to the war effort. As a result two public discussions started nationally among scientists: one focused on the maximum utilization of scientists from all disciplines during the war, and the second concerned itself with the relationship between science and government during and after the war. Although typically debated in the domestic context, there were also regional and international dimensions to Americans' preoccupations with World War II mobilization and post-war preparation.

In reference to mobilization, scientists not only argued for the full utilization of American scientists in aiding the war effort, but also tried to facilitate scientific cooperation with scientists in other Allied countries. Science had been an international endeavor in the years prior to World War II, and as war projects would prove, some scientists preferred cooperation to confidentiality. Furthermore, World War I had taught many scientists about potential problems in war mobilization, and

Company, Inc., Publishers, 1995); Daniel S. Greenberg, The Politics of Pure Science (New York: The New American Library, Inc., 1967); Thomas G. Paterson, J. Garry Clifford, and Kenneth J. Hagan, American Foreign Relations: A History since 1895 vol. 2 (Boston: Houghton Mifflin Company, 2000): 130-31; Melvyn P. Leftler, The Specter of Communism: The United States and the Origins of the Cold War, 1917-1953 (New York: Hill and Wang, 1994): 3-32; Lawrence Badash, Scientist and the Development of Nuclear Weapons: From Fission to the Limited Test Ban Treaty, 1939-1963 (New Jersey: Humanities Press, 1995): 24-35.

some used their previous experiences to inform their suggestions in response to World War II.³

After the United States' entry into the Second World War brought these issues to the foreground, Dunn questioned the degree to which biologists and other scientists were effectively employed in war work for the government. He, for the most part, did not join a campaign to mobilize nationally, but instead focused his attention on local and international aspects of mobilization. At Columbia University he responded to wartime needs by joining campus-wide committees and coordinating war work in the Zoology Department. Internationally, he facilitated cooperation with the Soviet Union by presiding over the American-Soviet Science Society, an organization devoted to establishing a regular exchange of scientific information between these two Allied nations.

In addition to fueling mobilization, the war also caused scientists to consider their future needs in the post-war world. Scientists drew from their experiences during and before the war to inform their visions for the future, which for many American scientists meant forming a well-defined relationship between government and science. Not agreeing on the details, scientists and politicians debated during the 1940s about what would become the National Science Foundation in 1950. In light of the national debate about funding for science, the war also prompted academic scientists and administrators to evaluate the science performed on their campuses and its financial sources. In addition, scientists recognized the consequences that war had

³ Greenberg, 68-96.

on the internationalism of science and called for international scientific cooperation. Concerns about international cooperation informed scientists' suggestions for a national foundation and inspired them to re-establish collaboration with scientists in other nations. Furthermore, many American scientists helped foreign colleagues to rebuild their laboratories and libraries after the war ended by sending various supplies overseas.

Preparing for the post-war world was something that occupied Dunn's attention and consumed hours of his time. He helped to write science legislation for Senator Harley M. Kilgore, and compiled information for Vannevar Bush's Science, the Endless Frontier. He acted as the main speaker for biologists during Senate hearings held in 1945 to discuss the formation of a national science foundation, and he urged scientists to collaborate with the politicians drafting these bills. At Columbia University he surveyed faculty about their visions for the post-war era and proposed a Council for Research in the Natural Sciences to act as a coordinating body with outside funding agencies. As Chairman of the American-Soviet Science Society, he tried to educate Americans about Soviet scientists and the Soviet Union and to establish cooperative relations between scientists in the two countries. He also supported the reconstruction of genetics in other countries.

In three chapters these various levels of responses to World War II are discussed with Dunn as a central figure. This chapter examines national aspects of American scientists' attempts at mobilizing during the war and preparing for the post-war era. Chapter Five discusses responses to the war in the personal and regional

contexts, and Chapter Six examines international affairs. L.C. Dunn and his family demonstrate some of the ways that the war personally affected Americans. Columbia University in the City of New York and its Zoology Department are examined as localized case studies. Studies of the genetics community and American-Soviet Science Society demonstrate some international dimensions of scientists' responses to World War II.

The war years greatly influenced Dunn's activities, during which he shifted his activism to accommodate new pressures facing Americans, and especially American scientists. In 1940, he made a conscious decision to redirect his attention: "I am involved now in the only defense work which means anything to me – the examination of the future of our institutions and what instruments can be devised to cope with the new world."⁴ However, this transition took place slowly over the next few years so that by the end of the war, Dunn's campaigns had different foci from his earlier activism and they also meant more to him personally. Previously, a feeling of responsibility spurred Dunn's activism as he reacted to the world situation. Things shifted in reference to the war and for Dunn personally around late 1943. Conquering Nazi Germany seemed inevitable although not immediate at this time, and President Franklin D. Roosevelt optimistically spoke of America overcoming fascist powers after Italy surrendered in summer 1943. President Roosevelt, Soviet Premier Josef Stalin, and British Prime Minister Winston Churchill met in Tehran towards the end

⁴ APS, Dunn Papers, Series I, Box 19, Walter Landauer, Dunn to Landauer, 24 Nov 1940.

of the year to coordinate the defeat of Nazi Germany and the Axis forces.⁵ Dunn also began looking to the future and formulating a post-war world in which he wanted to live. In these campaigns of the mid- to late 1940s, Dunn participated with enthusiasm and a positive outlook.

Dunn Mobilizes, Reluctantly

Concerns for the government's effective usage of scientists increased gradually during the early 1940s in response to the mobilization of American scientists in contributing to the war overseas. Scientists wanted to help fight the war and hoped the United States government would fully utilize their expertise. However, many scientists recognized that the US government employed only a small portion of American scientists by hiring a disproportionately larger number of physical scientists than biological scientists.⁶ The American Association of Scientific Workers (AASW) was particularly vocal on the subject of mobilizing scientists for the war in part

⁵ Edward M. Bennett, Franklin D. Roosevelt and the Search for Victory: American-Soviet Relations, 1939-1945 (Delaware: S.R. Books, 1990): 101-107; Catherine Evtuhov and Richard Stites, A History of Russia since 1800: Peoples, Legends, Events, and Forces (Boston: Houghton Mifflin Company, 2004): 406-07.

⁶ J.E. Hawkins, "Biology and Medicine in the War," Science 95 (15 May 1942): 507-08; Chas. H. Behre, Harry Grundfest, and Elvin A. Kabat, "The Full Utilization of Scientific Personnel," Science 96 (3 Jul 1942): 16; Harry Grundfest, "The Complete Utilization of Scientifically Trained Personnel," Science 96 (2 Oct 1942): 318-19.

because its membership in the early 1940s was predominantly scientists not employed in war work.⁷

Created in 1937 by chemist K.A.C. Elliott upon a British model, the American Association of Scientific Workers brought together liberal-minded Americans working in the sciences. The AASW's organizers aimed to use science to address societal issues, and as Peter J. Kuznick points out they hoped to reorganize science and society.⁸ The founding members envisioned the AASW as a necessary complement to the American Association for the Advancement of Science (AAAS) and other organizations for scientists, such as the American Federation of Teachers (AFT) and American Association of University Professors (AAUP). In creating the AASW its founders aimed to address the economic interests of scientists as a group, rather than as individuals. A July 1938 progress report describes the uniqueness of the AASW in comparison to these other groups:

We are of the opinion that no other organization occupies itself with such special concerns of the scientists as the promotion of the application of science for the welfare of society, the provision of adequate and regular funds for research, and the combatting [sic] of pseudo-scientific theories and anti-social applications of science.

⁷ Elizabeth Hodes, "Precedents for Social Responsibility Among Scientists: The American Association of Scientific Workers and the Federation of American Scientists, 1938-1948," University of California at Santa Barbara, diss. 1982: 76-80.

⁸ Peter J. Kuznick, Beyond the Laboratory: Scientists as Political Activists in 1930s America (Chicago: University of Chicago Press, 1987): 227-28.

The organization's main headquarters were located in Philadelphia, where the AASW started. They quickly established a local branch in Boston and planned others in cities with major universities, such as New York, Chicago, Baltimore, and Berkeley.⁹

The author of the report, Phillip M. Field, sent Dunn a copy and requested that he join the AASW, but Dunn declined the offer. He recognized the need for an organization for scientific workers outside of universities, such as those employed by the government, hospitals, and other institutions. For himself, he preferred the American Federation of Teachers "because of its trade union affiliation."¹⁰ Five months later Dunn received a letter from Irvin M. Korr, Secretary of the New York branch of the AASW. Korr asked Dunn to join the New York chapter and to speak on 10 March 1939 at their open forum. Dunn declined Korr's offers and enclosed a copy of his earlier letter to Field in explanation for his refusal.¹¹ Soon thereafter, however, Dunn changed his mind about collaborating with the AASW because he spoke at a symposium for the New York Branch in November 1939. He may have also changed his mind about joining the AASW.¹²

⁹ APS, Dunn Papers, Series I, Box 1, American Association of Scientific Workers, 1938-39, n.d., Phillip M. Field, Secretary, Executive Committee of AASW, "Progress Report, The American Association of Scientific Workers," No. 1 (Philadelphia, July 1938): 4 pages.

¹⁰ APS, Dunn Papers, Series I, Box 1, American Association of Scientific Workers, 1938-39, n.d., Dunn to Field, 23 Sept 1938. Peter Kuznick discussed Dunn's response and concerns (Kuznick, 230).

¹¹ APS, Dunn Papers, Series I, Box 1, American Association of Scientific Workers, 1938-39, n.d., Irvin M. Korr, Secretary of the New York Chapter of AASW to Dunn, 19 Feb 1939 and Dunn's reply, 23 Feb 1939.

¹² Kuznick, 240.

Historians have mentioned that Dunn was a member of the AASW, but it is unclear when he officially joined the organization. Three factors are clear: he participated in activities sponsored by the New York branch of the AASW starting at the end of 1939, he was on a committee at Columbia University in 1943 made up of members of the AASW, and in the early 1950s he acted as one of eight vice-presidents.¹³ However, as mentioned above, he twice declined offers in the late 1930s to become a member even though joining would seem an obvious decision for him.¹⁴

The organizers of the AASW hoped to attract members who held liberal political

¹³ Betty Drury, the secretary of the Emergency Committee in Aid of Displaced Foreign Scholars, asked Dunn about the AASW in 1941 and he responded that he was quite familiar with the organization and recommended that the ECADFS forge a cooperative relationship with the AASW (NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records, L.C. Dunn, 1941, Drury note to files after a phone conversation with Dunn, 30 Jan 1941). The New York Times announced a meeting of the AASW's New York Branch in which Dunn participated in "Events Today," New York Times (15 Dec 1944): 26; HUA, Shapley Papers, HUG 4773.10, Box 29B, Papers, c. 1921-1965, American Association of Scientific Workers, Helen Dean, Secretary of AASW to Shapley 12 Feb 1951. In 1943 Elvin A. Kabat asked Dunn to work with the New York branch of AASW "to help bring to the American scientists and public, the realization of the necessity for completer [sic] and all out mobilization of science for winning the war." Kabat might have been asking Dunn to join the AASW, or Dunn might have been a member of the AASW who was not actively engaged in their mobilization campaigns (APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May-June 1943, Kabat to Dunn, 4 June 1943). The committee at Columbia University had five members, all of whom were also members of the AASW, and it produced Ruth Benedict's The Races of Mankind. Violet Edwards, "Note on *The Races of Mankind*" Race: Science and Politics revised edition (1940; New York: The Viking Press, 1945): 167-68.

¹⁴ Dunn had two students in 1941 who were active members. Lester G. Barth was National Secretary and H. Burr Steinbach was Treasurer (UACL, Central Files, Box 382, Folder 1, L.C. Dunn July 1940-June 1941, Dunn to Fackenthal, 4 March 1941). Elizabeth Hodes and Jessica Wang mention that Dunn was a member of the American Association of Scientific Workers, but neither specifies when he joined (Hodes, 324; Wang, 1995, 150).

views and who wanted science to be used for the public's benefit. These attributes describe Dunn's views and activism; therefore, it is not surprising that he was asked to join the organization. The AASW's aims also aligned with Dunn's activities during the late 1930s and early 1940s, especially those addressed by the American Committee for Democracy and Intellectual Freedom. Kuznick points out that Franz Boas joined the AASW as a national sponsor because its aims reflected those of the American Committee. In addition to Boas, other members of the AASW were people with whom Dunn interacted during the late 1930s and 1940s in political and social campaigns: Walter B. Cannon, Barry Commoner, Leo Loeb, Hermann J. Muller, Harlow Shapley, and Harold C. Urey, to name a few.¹⁵

When members of the AASW initially circulated their views about mobilization in the early 1940s, they did so by publishing rather innocuous summaries about symposiums that they had recently held.¹⁶ By 1942, members of the AASW increased the stridency of their pleas by calling for action on the part of their colleagues. Thus, in July 1942, three members of the New York branch of the AASW, Charles A. Behre, Harry Grundfest, and Elvin A. Kabat, asked the older generation of scientists to use their prominent positions and reputations to initiate mobilization: "the full utilization of our country's resources in scientific and technical

¹⁵ Hodes, 64-65; Kuznick, 231, 235.

¹⁶ Examples of meeting summations are Harry Grundfest, "The American Association of Scientific Workers," *Science* 93 (14 Feb 1941): 158-60 and Hawkins, 1942 (cited above). Grundfest recapitulated the talks given in late December 1940 at the symposium "The Scientist and American Democracy," and Hawkins reported on a symposium held in April 1942 titled, "Biology and Medicine in the War."

personnel requires decisive action by our national scientific authorities and by our senior scientists.” The authorities to whom they appealed were defined as members of the American Association for the Advancement of Science and National Academy of Sciences, as well as scientists employed by the United States government.¹⁷

Neurophysiologist Harry Grundfest, one of the authors, published several articles in the early 1940s addressing the mobilization of scientists. As National Secretary of the AASW, he reported that members had sent suggestions to the National Academy of Sciences and to Vannevar Bush of the Office of Scientific Research and Development (OSRD) outlining the ways to better utilize American scientists during the war. Historian Elizabeth Hodes has noted that members of the American Association of Scientific Workers failed to establish strong relationships with preeminent scientists. Moreover, their inability to develop a fruitful association with elite scientists, especially Bush of the OSRD, proved detrimental for the American Association of Scientific Workers in the long run.¹⁸ Grundfest, drawing from personal experience, also told scientists to coordinate themselves, develop their own projects, and to take initiative by presenting their ideas to government officials, as he and other members of the AASW’s New York branch had done. Lastly, he mentioned that several branches of the AASW had people who could assist “in formulating and presenting projects for war research.”¹⁹

¹⁷ Behre, Grundfest, and Kabat, 16.

¹⁸ Hodes, 80-82.

¹⁹ Hawkins, 508; Grundfest, 1942, 318-19. Quotation is from page 319.

Grundfest had studied at Columbia University in the 1920s where he received his Ph.D. in zoology and physiology in 1930. After moving between jobs for five years, he took a position with the Rockefeller Institute, where he stayed until 1943 at which time he committed himself primarily to war work. From then until the end of the Second World War he conducted research for the United States Army at the Climatic Research Unit at Fort Monmouth Signal Laboratories in New Jersey and the Wound Ballistic Unit at Princeton University. He continued to work closely with the American Association of Scientific Workers throughout the war. After the war ended, Grundfest returned to New York and worked at Columbia University's College of Physicians and Surgeons until he retired in 1976. His scientific work covered a wide range of physiological matters pertaining to the nervous system and focusing on cells and membranes. Politically, Grundfest held leftist views and openly shared them. He was also active with Jewish groups, such as the Weizmann Institute of Science, Hebrew University, and Hadassah Medical Organization.²⁰

At the same time that Grundfest spoke out on the mobilization of scientists to assist in the war effort, democratic Senator Harley M. Kilgore of West Virginia submitted his first Senate bill addressing full utilization of science and technology during the war. Kilgore had a law degree and prior to entering the Senate in 1941 he had been a judge. Drawing from his pro-labor views, he drafted a bill in the spirit of New Deal politics. It received little attention; he revised and resubmitted it six months

²⁰ John P. Reuben, "Harry Grundfest, January 10, 1904 – October 10, 1983," Biographical Memoirs of the National Academy of Sciences 66 (Washington, D.C.: National Academy Press, 1995): 151-66; Hodes, 74-78.

later. The new legislation (S. 702) sparked more discussion about the utilization of scientists during the war and about establishing a permanent governmental agency for science. Kilgore referred to his revised legislation as the Science Mobilization Bill and, as pointed out by historians, Kilgore's initial bills dealt with war mobilization more so than a post-war policy. As years passed without adoption of his legislation, Kilgore rewrote his bills to increasingly focus on the structure between science and government after the war ended.²¹

Kilgore promoted a close relationship between the US government and science. He based his legislation on New Deal politics by proposing minimal corporate monopolization of scientific developments. Initially he proposed that the government draft scientific and technological workers to conduct investigations applicable to wartime needs. He revised this language in his subsequent bill of 1943 to allow for voluntary rather than mandatory involvement on the behalf of scientists. He suggested an Office of Scientific and Technical Mobilization, a governmental agency coordinating science and technology during and after the war, and he believed that the government should own all patent rights for processes developed with its

²¹ Kilgore proposed his first bill in 1942, but most disliked it. He revised the bill and submitted it to the Senate six months later (Wang, 1999, 26). Daniel Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945, A Political Interpretation of *Science – The Endless Frontier*," *Isis* 68 (1977), reprinted in *The Scientific Enterprise in America*, ed. Ronald L. Numbers and Charles E. Rosenberg (Chicago: University of Chicago Press, 1996): 297-319, 300-01, 303; Robert Franklin Maddox, "The Politics of World War II Science: Senator Harley M. Kilgore and the Legislative Origins of the National Science Foundation," *West Virginia History* 41 (1979): 20-39, 20-22, 31; Hodes, 111; Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Durham: Duke University Press, 1995): 83.

financial support. Even though Kilgore's 1943 bill scaled back the government's power, many critics noted that Kilgore's suggestions caused scientists to lose their independence.²² The United States government eventually passed legislation that established the National Science Foundation in 1950 following years of debate that started in response to Kilgore's bills.

The activities of the AASW's members during the early 1940s demonstrate the close relationship between discussions of scientists' roles in war mobilization and their support of Senator Kilgore's Congressional bills for public funding of science. Elliott and Grundfest expressed their support of Kilgore's revised bill, S.702, by noting that it would allow for the full usage of scientists during times of war and peace. They believed that Kilgore's proposal for an Office of Scientific and Technical Mobilization, in which scientists participated, would give them more say in governmental issues involving science and technology. Furthermore, Kilgore's bill represented a step towards increased democracy in the United States and they urged scientists to familiarize themselves with the bill's contents. As prominent members of the AASW, Elliott and Grundfest saw their vision for a close relationship between American science and society in this bill.²³

Dunn initially showed his support by entering the public discussion about Kilgore's bill as a concerned scientist who had no connection to the federal side of

²² Maddox, 25-27; Kevles, 303; Wang, 1995, 141-42.

²³ K.A.C. Elliott and Harry Grundfest, "The Science Mobilization Bill," *Science* 97 (23 Apr 1943): 375-77. Elliott was Chairman of the Philadelphia Branch of AASW and Grundfest was National Secretary of the AASW.

the matter. Chemist Gustav Egloff, Director of Research for the Universal Oil Products Corporation and President of the American Institute of Chemists, published a disparaging commentary in response to Elliott and Grundfest's article in Science. Dunn wrote a retort bringing into question Egloff's motives for denouncing Kilgore's bill by pointing out that private companies, such as Egloff's employer, would lose their monopoly on rights to scientific discoveries with the passage of S. 702. Kilgore proposed measures guaranteeing that the public and small businesses reap the benefits of scientific developments, and not private corporations.²⁴

Egloff's approach for getting his point across concerned Dunn immensely. According to Dunn, Egloff gave unsubstantiated and untraceable statistics, as though they were facts. He questioned Egloff's contentions that ninety-five per cent of scientists contributed to the war effort and that almost all laboratories were working for the government. In an effort to undermine Egloff's statement, Dunn noted that most biologists, geologists, and mathematicians, as well as other scientists, were not employed by the government and that many laboratories had no relationship with the government. Dunn, like Elliott and Grundfest, asked his readers to peruse the bill for themselves.²⁵ In contrast to Elliot and Grundfest, Dunn did not take a stance on the issue of the need or extent of scientific mobilization. He simply noted that Egloff had

²⁴ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May to June 1943, Nathan Robertson, "Science Bill Fought by Official of Firm Being Probed by U.S." This article did not have a newspaper name or date attached to it. It was released in Washington on 8 Nov [no year].

²⁵ Gustav Egloff, "The Kilgore Senate Bill," Science 97 (14 May 1943): 442-43; L.C. Dunn, "The Opposition to the Kilgore Bill," Science 97 (4 Jun 1943): 510-11.

exaggerated the degree to which American scientists were being utilized for the war effort.

Before going to press, the editor of Science, J. McKeen Cattell, wrote to Dunn saying that he wished to change some of Dunn's language. Cattell disliked Dunn's accusation that Egloff was motivated by his private profit and Cattell told Dunn that he wished to delete several words in Dunn's rejoinder. Dunn responded that "It isn't Dr. Egloff's private profit that I have in mind but the profit of his corporation. If you wish, change the word 'private' to 'corporation.'"²⁶

Egloff submitted a response to Dunn's accusations, in which he supplied citations for his previous statements. Wishing to have equal time to Egloff, Dunn sent an article to Science, but Cattell decided to end the debate and did not publish Dunn's second article.²⁷ Not satisfied with Cattell's negative response, Dunn resubmitted with the request that Cattell balance the tally sheet: "it would even the score at two for Egloff and two for Dunn which seems fairer than 2:1."²⁸ Dunn did not get his say in Science, and instead he distributed on his own accord a document consisting of his second article and two letters to Cattell, which undoubtedly did not receive the wide circulation that his published article would have.

²⁶ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May to Jun 1943. Cattell to Dunn, 19 May 1943 and Dunn's reply 20 May 1943. Gustav Egloff, "The Science Mobilization Bill," Science 97 (18 Jun 1943): 552-54.

²⁷ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May to Jun 1943, Cattell to Dunn, 24 June 1943; Leonard Carmichael, "The National Roster of Scientific and Specialized Personnel: A Progress Report," Science 93 (7 Mar 1941): 217-19.

²⁸ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May to Jun 1943, Dunn to Cattell, 28 June 1943.

Dunn received several letters from colleagues, who either expressed their belief that Science should have published his second article or from readers who disagreed with his first article.²⁹ Those who disagreed with Dunn felt that Egloff's views better represented the true nature of the situation.³⁰ Kabat and Grundfest, who as mentioned above had co-authored an article on mobilization with Behre, wanted Dunn to get involved with the mobilization effort spearheaded by the New York branch of the AASW. Dunn responded to Grundfest that he wished to be on record for supporting legislation of this kind, but that he did not want to get extensively involved.³¹ One and one half years later, Dunn and Grundfest were both still engaged in the public discussion about legislation for a science bill and delivered speeches on "Post-War Science Policy in the United States" at a meeting held by the New York Branch of the AASW. In the end Dunn committed himself beyond what he initially

²⁹ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, Jul to Dec 1943. Correspondents supporting Dunn's views were Robert K. Enders of Washington D.C. (8 July 1943), Lyman Chalkley (14 June 1943 and 27 July 1943), F. R. Moulton of American Association for the Advancement of Science (31 July 1943), and Harry Grundfest (6 Aug 1943). Curt Stern wrote a letter to Cattell and sent a copy to Dunn (30 July 1943).

³⁰ F.C. Whitmore, Dean of Pennsylvania State College (5 June 1943) and Ford W. Harris of the Law Offices of Harris, Kiech, Foster & Harris in Los Angeles (9 June 1943) disagreed with Dunn's opinions (APS, Dunn Papers, Series I, Box 17, Kilgore Bill, Jul to Dec 1943).

³¹ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May-Jun 1943, Kabat to Dunn, 4 Jun 1943; APS, Dunn Papers, Series I, Box 17, Kilgore Bill, Jul to Dec 1943, Grundfest to Dunn, 6 Aug 1943, and Dunn to Grundfest, 12 Aug 1943.

intended by actively pursuing legislation for a national science foundation until he left the United States for sabbatical in 1946.³²

Realizing that the statistical aspects of his debate with Egloff would not be resolved without investigation, Dunn proposed that Cattell find an expert to explore the matter, and then write an article presenting information on the data under contestation. He specifically suggested Leonard Carmichael, Director of the National Roster of Scientific and Specialized Personnel. Carmichael and others with the National Roster collected information from American scientists and technologists in order to compile a list of America's "brain power." Cattell heeded Dunn's advice and contacted Carmichael, who wrote "The Number of Scientific Men Engaged in War Work." Carmichael did not present definitive statistics, explaining that it was impossible to estimate the number of scientists mobilized because of the multitude of definitions for "scientific man" and "war work." Although Carmichael offered vague estimates, he argued that a large percentage of scientists had contributed to fighting the war in some manner. He appears to have supported Egloff's stance rather than Dunn's.³³

Dunn was more concerned with the nature of articles opposing Kilgore's bill, such as Egloff's, than with trying to garner support for Kilgore's bill at this point. According to Dunn, Egloff's poorly-researched article threatened the credibility of

³² "Events Today," New York Times (15 Dec 1944): 26. Dean Raymond Kirk also spoke at the meeting, which was held on the Columbia University campus.

³³ Leonard Carmichael, "The Number of Scientific Men Engaged in War Work," Science 98 (13 Aug 1943): 144-45.

science: "...for unless scientists confine themselves to statements which can be tested by evidence, even where their personal and economic interests are concerned, the public credit of all scientists will be destroyed."³⁴ As can be seen in this example, Dunn carried his scientific approach into the political sphere, and drawing from over a decade of experience he advised other scientists to uphold their scientific integrity when broaching political topics. Within six months, news spread that the United States Justice Department was investigating Egloff for alleged violations of anti-trust laws.

In the early years of World War II, Dunn's concern about Egloff's approach and statements in opposition to Kilgore's bill caused him to discuss mobilization, but he did not promote any particular agenda at this time. He simply urged scientists to come to their own conclusions about the bills after reading them. During 1944 and 1945 Dunn took on a more active role in these matters, administratively and publicly. He supported establishing a governmental science foundation for the post-war era, extensively participated in gathering information and drafting bills, and voiced his opinions about specific aspects of the legislation under discussion.

³⁴ Gustav Egloff, Science 97 (18 June 1943): 552; APS, Dunn Papers, Series I, Box 17, Kilgore Bill, May to June 1943, "A Controversy about Scientists in War Work." In this four-page document, Dunn reproduced his two letters to Cattell of Science and his second article in response to Egloff. He circulated this document to those who asked him the status of their debate. For example, William E. Castle thanked Dunn for sending him a copy of the document and gave Dunn his opinions on the matter. APS, Castle Papers, L.C. Dunn #2, 1939-1962, Castle to Dunn, 28 Jul 1943.

Funding Science after the War

Legislators and scientists drafted multiple science bills during the 1940s in which they proposed a new relationship between American science and the United States government. The advent of public funding with the establishment of the National Science Foundation in 1950 was a watershed for American scientists, who had previously approached private philanthropies, such as the Rockefeller and Ford Foundations, for financial assistance. After the mid-twentieth century, public and private monies contributed to scientific research in the United States, and science expanded considerably as a result. Although most scientists desired for the United States government to support scientific research, passing a bill through the United States Senate was not achieved easily. Throughout the 1940s scientists debated the fine points for the structure of a governmental body and its procedures for controlling public funding of science. Scientists, such as Dunn, got involved because they wanted their concerns represented. He participated in drafting, discussing, and debating legislation, particularly during 1944 and 1945.

Scholars have written rather extensively on the establishment of the National Science Foundation by giving their greatest attention to the events from 1945 onward. These publications cover Vannevar Bush's Science, an Endless Frontier, and the legislation designed from Bush's report and submitted by Senator Warren G. Magnuson, as well as Senator Harley M. Kilgore's competing bill. From this point of origin, scholars have dissected the legislative histories important to the passage of these bills. Alice Kimball Smith focused on atomic scientists and discussed

Congress's simultaneous consideration of two comparable legislative issues about harnessing atomic energy, the McMahon and May-Johnson bills.³⁵ J. Merton England noted that the National Science Foundation lauds Vannevar Bush for his role in the agency's establishment. In an attempt to re-examine Bush's legacy, England and later Nathan Reingold discussed his position as director of the Office of Scientific Research and Development and the committees he designed that contributed to Science, an Endless Frontier.³⁶

Dunn collaborated with Kilgore for almost two years before participating in the senate debates held in October 1945. Whereas some historians and political scientists begin their discussions about a national science foundation with the publication of Vannevar Bush's Science, the Endless Frontier in July 1945, the bulk of Dunn's participation occurred prior to the release of that report. A few historians have examined the events beginning with the submission of Kilgore's first senate bill in 1942. Daniel Kevles supported the claim that Vannevar Bush's report shaped the final legislation passed in 1950, which established the National Science Foundation. In comparison, Robert F. Maddox examined Kilgore's role in drafting science

³⁵ Alice Kimball Smith, A Peril and a Hope: The Scientists' Movement in America: 1945-47 (Chicago: University of Chicago Press, 1965); Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited," Historical Studies in the Physical and Biological Sciences 26 (1995): 139-66.

³⁶ J. Merton England, A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-57 (Washington, D.C.: National Science Foundation, 1982); Nathan Reingold, "Vannevar Bush's New Deal for Research: The Triumph of the Old Order," Historical Studies in the Physical and Biological Sciences 17 (1987): 299-344.

legislation and concluded that he deserves recognition for his part in drafting the initial bills from which others based their counterproposals. Elizabeth Hodes examined the American Association of Scientific Workers and its members' support for Kilgore's bills in contrast to the Federation of American Scientists and its members' views on legislation for atomic energy. She also analyzed why the AASW was less successful than the Federation of American Scientists as an organization and in getting legislation passed. Daniel Lee Kleinman wrote a thorough evaluation of Kilgore and Bush's proposals while also examining the reasons behind each of their views.³⁷

Some scholars, who have written about the debates concerning a national science foundation, premised their investigations in contexts pertinent to Dunn. Toby Appel has analyzed the formation and first twenty-five years of the National Science Foundation from the perspective of biologists. Although she does not give a detailed analysis of the period from 1942 to 1945, she thoroughly investigates matters important to biologists in which Dunn holds an integral place.³⁸ Jessica Wang's analysis of the debates from a liberal-left perspective gives a broader understanding of the issues at stake during the 1940s by analyzing them with reference to their political context. She argued that politically the liberal-left lost support during the decade and that the outcome for this legislation reflects the shift towards more

³⁷ Kevles, "The National Science Foundation," 297-319; Maddox, 20-39; Hodes, 1982; Kleinman, 76-92.

³⁸ Toby A. Appel, Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975 (Baltimore: Johns Hopkins Press, 2000): especially Appel's introduction and Chapter One.

conservative political thinking induced by the early Cold War years.³⁹ Kevles also has touched on America's conservatism in reference to legislation about atomic energy. He noted that the military's views gained greater representation in the bills between 1946 and 1950 because of Cold War politics developing from a concern for national security.⁴⁰ Understanding the liberals' perspective and America's growing conservatism during the late 1940s are two important factors in analyzing Dunn's actions and role in these debates. America's conservative turn not only had ramifications for the passage of a Congressional bill establishing a National Science Foundation, but also created problems for people and organizations espousing liberal views too closely associated with communism.

Senator Kilgore submitted a science bill late in 1942 that focused on mobilizing American personnel and resources during the war. The idea for the bill developed out of discussions with Herbert Schimmel, a doctorate in physics with experience as a congressional aid. Schimmel and Kilgore held similar views about the requirements for a governmental agency overseeing all scientific and technical mobilization in wartime, and they quickly drafted and submitted a bill to the Senate. Their legislation addressed their concerns about private corporations monopolizing governmental monies and abusing patent laws, as well as shortages of natural resources. Since the legislation grew from these ideas, it outlined restrictive usage of developments funded from governmental money and mandatory cooperation by

³⁹ Wang, 1995, 163-65.

⁴⁰ Daniel J. Kevles, The Physicists: The History of a Scientific Community in Modern America 1971 (New York: Vintage Books, 1979): 359, 365-66.

American citizens with scientific and technical expertise.⁴¹ The Senate held hearings to discuss the bill, which pointed out some of its failings. One of the more pronounced problems was a lack of feedback from scientists and engineers, an aspect Kilgore promptly addressed by enrolling the American Association of Scientific Workers as proponents for his revised bill.⁴²

Kilgore redrafted and submitted his bill (S. 702) in early 1943, which created quite a discussion among scientists. Whereas the AASW bolstered it, many other scientific groups thought it too restrictive even though Kilgore had loosened some of its parameters.⁴³ S. 702 proposed an Office of Scientific and Technical Mobilization (OSTM) as a centralized body coordinating the government's various agencies with scientific workers during and after the war. Accommodating for the post-war years was one of the major revisions that Kilgore added in response to the hearings. The OSTM's main functions nationally would be to act as a source for funding scientific investigations, training and educating scientists, circulating information about new developments, and advising the government on scientific topics. It would also guarantee that scientists addressed the public's needs and would foster international scientific cooperation. Even though Kilgore took scientists' views into account, he still did not make them central to the OSTM's daily functions. He proposed a head administrator appointed by the United States President, a six-member Board with two

⁴¹ Kevles, 1977, 301; Maddox, 22-26.

⁴² Kevles, 302-03; Maddox, 24; Hodes, 107-08; Kleinman, 76-79, 81.

⁴³ Hodes, 107-13; Kleinman, 87. A few of the organizations that disliked the bill were the American Association for the Advancement of Sciences, American Chemical Society, American Institute of Chemists, and American Institute of Physics.

scientists, and a twenty-five to thirty member advisory committee with five scientists. The other executives would represent small businesses, agriculture, industry, and laborers. A more problematic aspect of S. 702 was its clause on patents because it gave the OSTM rights over scientific and technological developments accomplished with governmental funding since 1941.⁴⁴

Dunn disagreed with one fundamental aspect of Kilgore's Office of Scientific and Technical Mobilization, and he wrote to Kilgore expressing his concerns in July 1943. Kilgore's bill allowed for extensive input from technicians and engineers, which Dunn disliked. Ultimately, Dunn worried that the implementation of Kilgore's OSTM as outlined in S. 702 would cause a few to benefit at the expense of many, which was the same issue that concerned him with Gustav Egloff's point of view. Dunn wanted pure scientists to receive greater representation in Kilgore's centralized body because he thought that they could be more objective as decision makers. Specifically, he suggested that the OSTM utilize existing scientific organizations, such as the National Academy of Sciences and the National Research Council, and he noted that their inclusion into the bill would be a politically savvy move because it would garner their members' support. Dunn fully agreed with one of the more controversial aspects of Kilgore's bill, his stance on patents. As mentioned above, Kilgore proposed that the US government retain patent rights for all innovations developed from scientific and technical work funded with governmental money. Dunn, however, realized that the bill would most likely not pass a vote in the Senate

⁴⁴ Hodes, 104, 110-11; Kevles, 1977, 303; Wang, 1995, 141-42, Kleinman, 83-84.

because of the clause on patents. He, thus, suggested to Kilgore that he consider formulating a separate bill for patents as a means of getting the bill passed.⁴⁵

In Kilgore's absence his consultant, Herbert Schimmel, responded to Dunn and through their exchange of letters Dunn began a collaborative relationship with Kilgore that grew over the next few years. Schimmel realized immediately the "extremely constructive" nature of Dunn's suggestions, and therefore sent him a copy of an article Kilgore had drafted for submission to Science, an article that addressed the exchange between Dunn and Gustav Egloff. Dunn made two major comments for revision: delete all mention of Adolf Hitler and write for a scientific audience. Kilgore most likely did not receive Dunn's critiques before submitting his article, and he definitely had not incorporated Dunn's suggestions. Kilgore demonstrated that Egloff used a tactic supported by Hitler in Mein Kampf – persuade readers' opinions by constant repetition. In reference to his second critique, Dunn suggested that the article be written in a manner more appealing to scientists' way of thinking and told Schimmel that, "Refutation by citing facts would be more persuasive." Kilgore instead used the article to undermine Egloff, summarize his legislation, and campaign for scientists' political support.⁴⁶

⁴⁵ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, July to Dec 1943, Dunn to Kilgore, 27 Jul 1943.

⁴⁶ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, July to Dec 1943, Schimmel to Dunn, 2 Aug 1943 and Dunn's reply, 9 Aug 1943. Harley M. Kilgore, "The Science Mobilization Bill," Science 98 (13 Aug 1943): 151-52. I do not have a copy of the article sent to Dunn and therefore could not compare it with the one published in Science.

Four months later in January 1944 Dunn reinitiated contact with Kilgore and his staff, which marks the beginning of his steady collaboration with politicians on drafting bills about governmental funding of science. Dunn had read and found engaging the exchange in Science between Kilgore and Vannevar Bush in the previous months. Bush, as director of the Office of Scientific Research and Development, oversaw a successful, temporary agency serving functions similar to those proposed by Kilgore. However, Kilgore's bill outlined an agency that would function very differently from how the OSRD operated. Bush found the proposed system problematic and therefore explained to Kilgore his reasons for disagreeing with S. 702.⁴⁷

The increased attention and strong reactions to Kilgore's bill proved to Dunn that the topic was important to scientists. Furthermore, he believed that scientists should take advantage of Kilgore's interest in the matter, which is exactly what Dunn did. He suggested to Kilgore that the fundamental questions raised by the discussion of a relationship between science and government be presented to a few prominent American scientists, who had remained silent up to this point. Dunn hoped that through such a method Kilgore would learn what general concerns scientists had and then apply them to his legislation.⁴⁸ Dunn often consulted the best authority when gathering information, as is the case with this suggestion to Kilgore. Schimmel

⁴⁷ Vannevar Bush, "The Kilgore Bill," Science 98 (31 Dec 1943): 571-77, 572.

⁴⁸ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson Bills, 1944-45, Dunn to Kilgore, 20 Jan 1944; APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Dunn to Karl Sax, 5 Feb 1944.

promised to send Dunn a list of American Nobel Laureates and a revised version of Kilgore's bill.⁴⁹ Kilgore thanked Dunn for his help and noted the need for opinions from scientists not in administrative positions.⁵⁰

Dunn wrote to several prominent scientists and presented them with two principal problems and several questions. He asked them for their candid views, which he planned to summarize and submit to Kilgore.⁵¹ Dunn received a fair number of responses that ranged from advocating minimal to extensive government involvement in scientific endeavors. For example, chemist G.N. Lewis felt that science should conduct itself as a "free enterprise," whereas physiologist Walter B. Cannon believed that constant support from the government was imperative. Astronomer Harlow Shapley of Harvard University endorsed Dunn's project, but chose not to respond formally. He felt that his official positions with scientific organizations restricted him from speaking freely and he therefore preferred to say

⁴⁹ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Schimmel to Dunn, 24 Jan 1944.

⁵⁰ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson Bills, 1944-45, Kilgore to Dunn, 28 Jan 1944. Hodes connected Dunn's Nobel Laureate survey to his leadership of the New York Branch of the American Association of Scientific Workers. I think she overstated Dunn's affiliation to the AASW (116).

⁵¹ Dunn sent the letter to seventeen people: Carl D. Anderson, Walter B. Cannon, A.J. Carlson, Arthur A. Compton, R. Courant, Clinton J. Davisson, Albert Einstein, E.O. Lawrence, G.N. Lewis, Harlow Shapley, Wendell M. Stanley, Harold C. Urey, Robert A. Millikan, Irving Langmuir, G.R. Minot, W.P. Murphy, G.H. Whipple. APS, Dunn Papers, Series I, Box 17, Kilgore 1944, Dunn to Schimmel, 23 Feb 1944. He told Minot that he planned to summarize the responses and send them to Kilgore. Same folder, Dunn to Minot, 10 April 1944

nothing at all. He was President of the National Society of Sigma Xi and a member of the National Science Fund, a committee of the National Academy of Sciences.⁵²

The two basic problems in need of addressing, according to Dunn, were the following. One: How can the public's interest be protected since their money paid for the research? In reference to patent rights, Dunn asked whether research accomplished with federal money should mandate that the results belong to the public. Two: Should the government continue to finance scientific research after the war ended, and if so, how much money should be committed annually? This problem had many parts to it and Dunn asked several subordinate questions. The main issues were what kind of scientific research should be funded (fundamental or applied), how should funding be doled out, and should there be a central agency in control.

Dunn helped Kilgore in several ways besides collecting information from scientists, as noted above. He attended meetings with Kilgore to discuss the legislation, gathered ideas from scientific administrators, and arranged for Kilgore to meet scientists. Dunn went to Washington D.C. in early March 1944 to help redraft Kilgore's bill – his second trip since the beginning of the year.⁵³ In early May he met

⁵² APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, G.N. Lewis, 5 May 1944, Walter B. Cannon, 28 March 1944, and Harlow Shapley, 27 March 1944. The National Society of Sigma Xi promotes scientific cooperation and collaboration (<http://www.sigmaxi.org/about/overview/index.shtml> 9 Feb 2006). The National Science Fund dealt with issues relevant to those presented in Dunn's survey (from Shapley's letter).

⁵³ The following letters refer to trips that Dunn made to Washington D.C.: Schimmel thanked Dunn for his visit and Dunn mentioned an upcoming journey. APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Schimmel to Dunn, 24 Jan 1944 and Dunn to Schimmel, 23 Feb 1944.

with Alan Gregg, Director of the Division of Medical Sciences of the Rockefeller Foundation and gathered some good ideas from him. Specifically, Gregg suggested formulating a committee to draft a twenty-five year prospectus for public and private funding of science in the United States as a basis for developing a long-range plan.⁵⁴ Later that month when Kilgore and Schimmel visited New York, Dunn planned a luncheon so that they could meet with local men closely involved with science. In attendance were chemist Wendell M. Stanley of the Rockefeller Institute for Medical Research, Richard Courant, Head of Department of Mathematics at New York University, Waldemar Kaempffert, Editor of the Science Section of the New York Times, W.J. Robbins, Director of the New York Botanical Garden, chemist H.C. Urey of Columbia University, and physicist George B. Pegram, Dean of Graduate Faculties at Columbia University.⁵⁵ The luncheon went rather well. Kilgore and Schimmel found it beneficial and enjoyed themselves, and Dunn's other guests grew increasingly interested in the legislation.⁵⁶

Of the men Dunn invited to the luncheon, a few contributed significantly to the promotion and circulation of information about science legislation. Urey in conjunction with Harlow Shapley headed up the Committee for a National Science Foundation, which started later in the year. After the Senate failed to reach a decision

⁵⁴ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Dunn to Kilgore, 4 May 1944.

⁵⁵ Urey and Pegram were performing investigations for the US government which contributed to the Manhattan Project at this time.

⁵⁶ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Dunn to Schimmel, 10 May 1944 and Schimmel to Dunn, 27 May 1944. APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Dunn to Kilgore, 14 Nov 1944.

before recessing, Urey and Shapley sent a letter on 28 Dec 1945 to the six Senators actively involved with drafting this legislation. The letter outlined the main points of agreement made by scientists at the senate hearings held during the previous two months and they gathered over 200 signatures from scientists who supported a national science foundation. In brief, the statement informed legislators that a compromise bill could be achieved and that they were willingly to help draft such a bill.⁵⁷ Kaempffert wrote a pamphlet in 1946 for the Public Affairs Committee, a non-partisan organization that printed inexpensive publications explaining American policy. In his easily accessible pamphlet, “Should the Government Support Science?,” he not only answered this question affirmatively, but also declared it “a national necessity.”⁵⁸ Kaempffert had been involved in these legislative matters since Kilgore had submitted his first bill. He testified at the 1942 Senate hearings urging for legislation that would establish a permanent agency, a suggestion Kilgore heeded when he revised his bill. Kaempffert had also written an article about Kilgore’s revised bill, S.702.⁵⁹

The situation in Washington D.C. grew more urgent as 1944 progressed, according to Dunn. Liquidation of the Office of Scientific Research and Development had begun and there was no replacement. Dunn worried that enthusiasm would wane

⁵⁷ Howard A. Meyerhoff, “Science Legislation and the Holiday Recess,” Science 103 (4 Jan 1946): 10-11; “Committee for a National Science Foundation,” Science 103 (11 Jan 1946): 45, 62-63.

⁵⁸ Waldemar Kaempffert, “Should the Government Support Science?,” Public Affairs Pamphlet 119 (July 1946).

⁵⁹ Kevles, 300-01; Maddox, 24; Kleinman, 81. Waldemar Kaempffert, “The Case for Planned Research,” American Mercury 57 (October 1943): 442-47.

if too much time passed without approving a national organization for science. As a solution, Dunn suggested that Kilgore hire Dunn's former student, Barry Commoner, as an aid. Commoner was young, only twenty-seven, but had made significant accomplishments since graduating from Columbia University as an undergraduate and receiving his doctorate in physiology from Harvard University. He had taught at Queens College in New York, entered the navy at the onset of World War II, and was currently a Lieutenant working at the Naval Air Station in Maryland. In addition to being a highly competent scientist, he was also gifted when dealing with people and administrative matters. Furthermore, Dunn noted that Commoner had helped the Mayor and police department of New York by developing and teaching New York educators emergency measures in case of an air raid.⁶⁰ Kilgore told Dunn that he was not looking to hire anyone at present, and Commoner informed Dunn that he was busy with another project.⁶¹ Nonetheless, about one year later, Commoner joined Kilgore's work force and asked Dunn for his help in the upcoming Senate hearings.

In fall 1945 United States Senators heard from scientists about two bills before them, Senator Warren Magnuson's S. 1285 and Senator Kilgore's S. 1297. Kilgore drafted a new version of his bill based on ideas gathered from Dunn and others, which he submitted in response to Magnuson's bill. Magnuson wrote his bill

⁶⁰ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Dunn to Kilgore, 14 Nov 1944.

⁶¹ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Kilgore to Dunn, 21 Dec 1944 and Commoner to Dunn, 18 Nov 1944.

based on Vannevar Bush's report, Science, the Endless Frontier.⁶² President Roosevelt had asked Bush, as the war was ending, to make suggestions on the post-war relationship between government and science. Bush formed committees of scientists to report on their ideas and then produced Science, the Endless Frontier from their feedback.⁶³

Magnuson and Kilgore's bills made different suggestions about the same issues. Ultimately, Bush, and therefore Magnuson's bill, proposed a structure that preserved scientists' autonomy, whereas Kilgore stressed a need for getting scientific developments to American citizens.⁶⁴ Bush outlined a structure for the relationship between government and science, which called for a federal organization directed by a board of laymen and scientists appointed by the President of the United States. Magnuson's board, according to this idea, acted as the topmost decision-making body with a subordinate administrator, who reported to the United States President. Kilgore, in contrast, called for a governmental body controlled by a director, who was appointed by the US President and oversaw the board. The national science or research foundation proposed in Bush's report had no power over other governmental bodies, such as military branches. Kilgore suggested an organization that acted as a supervisor of and coordinator for all scientific bodies of the national government. Magnuson's bill excluded the social sciences, whereas Kilgore's included them.

⁶² Wang, 1999, 27; Appel, 18-24; Vannevar Bush, Science, the Endless Frontier (Washington D.C.: U.S. Government Printing Office, 1945).

⁶³ Kevles, 309-11; Kleinman, 92-96;

⁶⁴ Kevles, 309; Hodes, 104; Kleinman, 73, 98-99.

About patent policy, Magnuson's bill made no specific statements about the use of patents and delegated decision-making powers to the advisory board. Kilgore kept a similar patent policy to that proposed previously; his bill stated that research paid for by the United States government would be governmental property and under governmental control.⁶⁵ Magnuson promoted pure scientific research more so than Kilgore, who promoted fundamental research as a vehicle for applied science.⁶⁶

Several factors influenced scientists' decisions about which bill to support. Liberally-minded scientists tended to back Kilgore's bill, whereas those wanting a more conservative policy chose Magnuson's.⁶⁷ Biologists disliked Magnuson's categorizing of their discipline because it seemed to fall under the medical, rather than the natural sciences. For this reason, biologists and social scientists typically bolstered Kilgore's bill.⁶⁸ (Dunn confirms these generalities because he was a liberally-minded biologist who backed Kilgore). Physicists and other atomic scientists were preoccupied with legislation pertaining to their discipline, according to historian Alice Kimball Smith, and thus they did not enter extensively into discussions about a national science foundation until 1946 at which point Magnuson and Kilgore had developed a compromise bill. Wang, on the other hand, states that the Federation of

⁶⁵ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Open letter to the President of the United States from the scientists who contributed to Bush's report, 24 Nov 1945; England, 26. Kilgore noted in the report from which he drafted S. 1297 that although patents needed more discussion, the results from federally funded projects should be free to the public (Kleinman, 91).

⁶⁶ Kilgore, 152; Appel, 18-24; Kleinman, 78.

⁶⁷ Wang, 142; Kevles, 319.

⁶⁸ Appel, 20.

American (formerly, Atomic) Scientists preferred Kilgore's ideas.⁶⁹ Bush had the backing of scientists who contributed to his report, many of whom held high-level administrative posts, such as the Dean, President, or Director of universities and corporations.⁷⁰ Also as a result of directing the Office of Scientific Research and Development for the government, Bush received backing from non-scientists important to the passage of legislations, such as military men and Congressmen.⁷¹ Scientific organizations also tended to favor certain bills. Members of the National Academy of Sciences preferred Magnuson's proposal, whereas members of the American Association for the Advancement of Sciences and the American Association of Scientific Workers sided with Kilgore.⁷²

The Senate hearings on the science mobilization bills were held in October and November 1945. Senators heard from several biologists on 24 October. In preparation for the hearings, Commoner and Kilgore asked Dunn to act as their main speaker from the biological sciences. They thought that he would be the best person to give an overview: "Frankly it is our hope," explained Commoner "that you will be able to do the major job that day and show the breadth and importance of the problem, i.e. the need for federal support of biological research." Kilgore and

⁶⁹ Wang, 1999, 37; Smith, 437-44. A principal legislative matter about atomic energy was the May-Johnson bill submitted on 4 October 1945, which proposed instituting the Atomic Energy Commission (Smith, 128).

⁷⁰ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Open letter to the President of the United States from the scientists who contributed to Bush's report, 24 Nov 1945. Kleinman provides an explanation of scientific elite (Kleinman, 56-59).

⁷¹ Kleinman, 66-69.

⁷² Wang, 1999, 37.

Commoner had enrolled other biologists to share their views on specific subjects, including Selman A. Waksman (penicillin), George W. Beadle (physiological genetics), and Wendell M. Stanley (virus biology). Commoner also told Dunn that the situation in Washington D.C. was highly political; Magnuson's faction had been soliciting support for his bill.⁷³

Dunn on Public Funding of Science

Dunn accepted the request that he act as the main speaker at the hearings, a task for which he was considerably prepared. Prior to the hearings, he gave two talks on "Organization and Support for Science in the United States," one of which was for the New York branch of the American Association of Scientific Workers.⁷⁴ He also developed his lecture into an article that was eventually published in two places, both of which appeared after the hearings. Dunn conducted a fair amount of research; for example, he learned about the relationships of science and government within other countries. He had read about the Kaiser Wilhelm Gesellschaft in J.G. Crowther's book The Social Relations of Science (1941), but did not find it very critical of the

⁷³ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, Commoner to Dunn, 9 Oct 1945; APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson Bills, 1945-46, 1950, Kilgore to Dunn, 10 Oct 1945.

⁷⁴ UACL, Subject Files, Instruction-Department of Zoology, Department of Zoology News Letter Number 7, Feb 1945 and News Letter Number 8, May 1945. Dunn gave his talk to the New York branch of the AASW on 15 Dec 1944 and also for Sigma Xi on 3 May 1945 at University of Rochester. Appel identified Dunn as the biologists' main speaker in her book, Shaping Biology (on page 22).

Institute's weaknesses and thus only marginally helpful.⁷⁵ He was also well-read on the situation of the Russian Academy of Sciences and the Soviet government because of his role as Chairman of the American-Soviet Science Society and his general interest in the subject, and he recommended books about the Soviet Union to a colleague interested in Soviet institutions. J.G. Crowther's Soviet Science (1936) was his top choice. He also mentioned a volume edited by Joseph Needham, Science in Soviet Russia (1942), J.D. Bernal's The Social Function of Science, and A.F. Joffe's Development of the Exact Sciences in the USSR.⁷⁶ Dunn's interest in the Soviet Union seems to have motivated him to read books on that nation as opposed to other countries.

Dunn also drew from his knowledge about the relationship between science and government in the Soviet Union to inform his opinions about an American science foundation. The Soviet government funded science by providing money to their Academy of Sciences, and the Academy distributed the funds. Dunn felt that the Soviet system did not stifle "scientific spirit" and described their biological research as having: "vitality and activity, and the atmosphere of eagerness, modernity and novelty." Dunn knew that the Soviet model was an example of extreme centralization,

⁷⁵ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1944, Dunn to Landauer, 23 Sept 1944. Dunn also mentioned this book by Crowther to Robert S. Lynd (see the next footnote.)

⁷⁶ APS, Dunn Papers, Series I, Box 21, Lynd to Dunn, 23 March 1944 and Dunn's reply, 28 March 1944. Dunn recommended these books to sociologist Robert S. Lynd when asked about books on the "effects of politicizing science" in the Soviet Union. Lynd was preparing to give a 12-week course on Soviet Institutions. The National Council of American-Soviet Friendship produced Joffe's publication as a pamphlet.

and he argued that the United States represented the other end of the spectrum because of its lack of governmental monies for science. He hoped that in the United States a compromise could be developed between these two extremes. Through the Soviet example, Dunn aimed to demonstrate that central funding of science can produce scientific and social benefits. He, thus, directly challenged one concern held by many American scientists, that governmental involvement restricted intellectual freedom.⁷⁷

At the hearings Dunn spoke specifically about the need for federal funding of biology and focused on several key points. One: Basic biological investigation had to be funded because it formed the foundation for applied research. Two: Biologists needed the guarantee of adequate long-term funding. Three: Biology would benefit from a science foundation acting at the national level, as opposed to state-run agencies. In reference to this last point, Dunn envisioned a national organization that funded cross-disciplinary projects, established a breeding facility to supply laboratories with research specimens, and coordinated international cooperation.⁷⁸

Although Dunn sided with Kilgore, he never entirely agreed with any of the bills before the Senate. His motivation to contribute to these proceedings stemmed

⁷⁷ L.C. Dunn, "Soviet Biology," *Science* 99 (28 Jan 1944): 65-67; L.C. Dunn, "Organization and Support for Science in the United States," *Science* 102 (30 Nov 1945): 548-54.

⁷⁸ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, L.C. Dunn, "Statement for Hearings on S. 1297," (23 Oct 1945): 10 pages. After the hearings, Kilgore and Schimmel held a dinner that Dunn attended along with some of his colleagues from the American-Soviet Science Society: H.C. Urey, Harlow Shapley, and E.U. Condon (Smith, 190).

from his belief that the United States required one national body to oversee the distribution of public money for scientific research. During the hearing he proposed his own view for the relationship between the administrators of the science foundation and the US President, in which a director worked for the President and the people of the United States. The director in his scheme sat on the President's cabinet as Secretary of Scientific Research and Development. Scientists needed high ranking governmental access so that they could contribute to national and international decisions.⁷⁹ In addition to siding with Kilgore's stance on the foundation's administrative structure, Dunn also favored other aspects of Kilgore's bill. He, thus, promoted a board comprised of scientists, laypeople, and members of other governmental departments. Dunn felt that scientists should be in charge of science within the US government, arguing that self-government was the best solution, as evidenced by democratically-based systems.⁸⁰ He agreed with Kilgore's plan for inclusion of the social sciences, but differed from Kilgore on the issue of patents⁸¹ by preferring the exclusion of patents from the bill in hopes that the legislation would get passed more quickly.⁸²

⁷⁹ APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-45, L.C. Dunn, "Statement for Hearings on S. 1297," (23 Oct 1945): 10 pages. "Science Secretary Urged in Cabinet," New York Times (25 Oct 1945): 38.

⁸⁰ Dunn, "Organization," 551.

⁸¹ Dunn, "Organization," 552-53; APS, Dunn Papers, Series I, Box 26, Robert M. Yerkes, 1945, Dunn to Yerkes, 14 Dec 1945.

⁸² APS, Dunn Papers, Series I, Box 17, Kilgore Bill, July to Dec 1943, Dunn to Kilgore, 27 Jul 1943.

Dunn also developed some of his opinions by analyzing Columbia University's expenditures for scientific and technical research. At the request of Vannevar Bush, he compiled rough statistics and information provided by six of the University's science departments. Dunn found that scientists and engineers at Columbia University conducted mostly basic research. His calculations suggested that between forty to sixty percent of the science departments' budgets went to research and that no money was provided for purchasing reprints or traveling to conferences. Seventy-five percent of the money paying for the research performed came from Columbia University's general income, and only twenty-five percent came from grants provided by private foundations.

Dunn elaborated his stance on patent policy in his article "Organization and Support for Science in the United States." He thought that scientists doing research with public funds essentially conducted scientific investigations for the people of the nation, which meant that their work should not become private property. This did not mean that it was impossible to patent developments achieved with public money since he envisioned the patenting process for public discoveries as a gradient. Fundamental scientific work had minimal access to private rights, whereas the products from applying fundamental methods could be patented.⁸³ Federal funding for scientific research solved the issue of patents in Dunn's mind: "My present view is that if work done at government expense is required to be freely published (except when public security is in danger) difficulties about patents and monopoly ownership of results

⁸³ Dunn, "Organization," 551.

will be avoided.”⁸⁴ Big business monopolies continued to worry Dunn and many of Kilgore’s supporters, who saw them as a threat to scientists, inventors, and small businesses.⁸⁵

When sharing these solutions in “Organization and Support for Science in the United States,” Dunn also presented scientists with reasons for supporting a governmental body for science, and urged them to collaborate with legislators. He viewed freedom as a mandatory principle in order for science to be successful. Scientists had to be free to pursue problems of their choosing in the manner they desired, and to be able to publish their investigations without censorship. Recognizing that scientists valued their intellectual freedom and disliked losing any degree of control over their pursuits, Dunn urged scientists to get involved. They had to voice their opinions in order for their concerns and needs to be represented in the legislation being debated in the Senate.⁸⁶

Dunn, furthermore, argued that science had to address social needs and social purposes, which was a major aspect of the leftist-liberal view and Kilgore’s bill. Science had become a major aspect of modern society, according to Dunn, and therefore a system had to be developed in which the public received the benefits of scientific research. Speaking to this point, Dunn asked, “What is science for?” and

⁸⁴ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Dunn to William S. Taylor, Department of Psychology at Smith College in Massachusetts, 14 Feb 1944.

⁸⁵ Kevles, 1977, 303.

⁸⁶ Dunn, “Organization,” 553-54. He saw his hope for greater scientist involvement in legislative matters with the organization of the Committee for a National Science Foundation, which is discussed below.

responded, "...scientists will agree that science exists for man and not for itself alone." Furthermore, World War II had accentuated the imperative role of science for citizens of the United States, and also revealed the need for an organized plan allowing scientists to help the war effort by drawing on their expertise. Dunn hoped that scientists, who either wished to contribute or had contributed to fighting the war, would realize the need to support the establishment of a governmental body of science. His statements explain the impetus compelling him to participate.⁸⁷

Conclusion

After the hearings held in October and November 1945, many worried about another delay in passing legislation. Kilgore's opposition organized a Committee Supporting the Bush Report and drafted a letter to President Truman explaining their views. Truman had endorsed Kilgore's bill a month earlier in October 1945.⁸⁸ Harold Urey and Harlow Shapley urged scientists to put aside their differences about the details and focus on the bigger problem at hand. They formed the Committee for a National Science Foundation in December 1945 and outlined the six key aspects important to a majority of scientists as learned from the Senate hearings. First, 116 scientists out of 117 agreed that science warranted governmental support in addition to private funding. Second, freedom in scientific research had to be upheld. Third, all

⁸⁷ Dunn, "Organization," 548-49.

⁸⁸ Appel, 21; Smith, 438; APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson, 1944-1945, Open letter to the President of the United States from the scientists who contributed to Bush's report, 24 Nov 1945.

sciences should be supported. Fourth, developments made with federal monies should be made public through publication. Fifth, recipients should be chosen based on merit and all institutions should qualify for funding. Sixth, scientists disagreed most on whether a director or board should hold the power. They hoped that a compromise could be reached on this sixth point. Urey and Shapley sent their letter with signatures from over 200 scientists to the US Senators involved in developing the legislation. The statement also offered the signers' assistance in drafting a compromise bill that would please a majority of the scientists and legislators involved.⁸⁹ Shortly thereafter, Magnuson and Kilgore made concessions and jointly submitted another bill, S. 1720. Although a compromise had been reached, more bills were drafted and submitted before legislation passed in 1950.⁹⁰

Dunn supported Urey and Shapley's campaign and signed their letter, but began to distance himself from the debates over the establishment of a national science foundation.⁹¹ Two factors contributed to Dunn's withdrawal, the first of which was that he needed a break. Dunn had been offered leaves of absence from

⁸⁹ Meyerhoff, 11.

⁹⁰ Kevles, 316-19; Kleinman, 124-39; Wang, 1999, 37.

⁹¹ "A National Science Foundation," *American Naturalist* 80 (Jan 1946): 98; Meyerhoff, 11. Dunn urged Robert M. Yerkes and Charles C. Adams to get involved with the Committee for a National Science Foundation: APS, Dunn Papers, Series I, Box 26, Robert M. Yerkes, 1945, Yerkes to Dunn, 8 Dec 1945 and Dunn's reply, 14 Dec 1945; APS, Dunn Papers, Series I, Box 17, Kilgore and Magnuson Bills, 1945, Dunn to Charles C. Adams, 10 Jan 1945. Dunn must have written to Adams in January 1946.

Columbia University during the war, but chose to postpone his sabbatical.⁹² He finally left in early summer 1946 for a sixteen month hiatus and in planning for this leave he slowly decreased his commitments after the new year began. Secondly, as Elizabeth Hodes has pointed out, scientists debated the structure for a governmental science agency until 1946, at which point they more or less reached a consensus. From 1946 on, politicians debated the issues before passing legislation in 1950.⁹³ Dunn's withdrawal from drafting and debating legislation for a national science organization coincides with this shift from a debate among scientists to one between politicians. Since Dunn typically focused on social and political issues that pertained to the scientific community, it is not surprising that he withdrew after scientists had obtained a relative consensus. Furthermore, Dunn most likely felt that he had accomplished one of his primary goals because more scientists had gotten involved and they planned to get legislation passed.

Dunn responded to World War II on a national level through behind-the-scenes development of and public campaigning for what would become the National Science Foundation. Dunn's participation grew during 1943 and over the next two years he regularly engaged in activities pertaining to a establishing a governmental

⁹² Dunn asked for a leave of absence for spring session of the 1942-1943 academic year. It was given to him (APS, Dunn Papers, Series I, Box 4, Columbia University-Frank D. Fackenthal, 1929-1945, Fackenthal to Dunn, 2 Feb 1942). He elected to postpone his leave, which Fackenthal acknowledged. (UACL, Central Files, Box 382, Folder 2, L.C. Dunn, July 1942 to June 1943, Fackenthal to Dunn, 25 Jan 1943). Fackenthal informed Dunn two years in a row during the early 1940s that he was eligible for sabbatical (APS, Dunn Papers, Series I, Box 13, Frank D. Fackenthal of Columbia University, 1934-1949, Fackenthal to Dunn, unknown dates).

⁹³ Hodes, 143-44.

organization for science. National issues, as described in this chapter, were only one aspect of Dunn's concern. He simultaneously responded to matters that developed at the local and international levels as a result of the war.

Chapter 5

Close to Home: Personal and Regional Responses to War, 1940-1945

World War II affected Americans' daily lives at multiple levels. In the previous chapter, Dunn's responses on a national scale were explored, but that is only one aspect of American scientists' mobilization during the war and preparation for the post-war years. Americans also dealt with the changes occurring around them at a personal and local level. In other words, families and workplaces experienced the impacts of war and had to accommodate to these developments. The Dunns' experiences represent one family's attempts to grapple with the ever-changing conditions on their daily lives, only some of which resulted from the war. Dunn's workplace, Columbia University, created new personal and professional challenges for Dunn, and he partook in campus-wide and departmental work prompted by war. In 1940, Theodosius Dobzhansky started his twenty-two year tenure with the Zoology Department at Columbia University, which had a long-term effect on both Dunn and Dobzhansky. This chapter explores events that happened simultaneously with those discussed in the previous chapter and provides a better understanding of the physical and emotional toll that the war years had on Dunn, as well as provides some background to his post-war endeavors.

The Dunn Family in the early 1940s

Dunn's personal life presented him with tribulations during the early 1940s. Robert, the Dunn's elder son fought in the war, which worried Dunn and his wife, Louise. Their younger son, Stephen, who suffered from a physical handicap throughout his life, went through his adolescence during the war years. Attending to Stephen and his special needs took a lot of devotion from Dunn and Louise. Dunn willingly adapted his professional tasks to accommodate the family's needs, while also increasing his extracurricular campaigns and attending to his usual scientific tasks.

Dunn had his own health to consider during the 1940s. He ushered in the decade at forty-six years of age, and in the summer of 1940 he suffered a heart attack.¹ Specific information about this episode and his other health problems is scant except for frequent and brief comments about sicknesses and operations.² Dunn's colleague at Columbia University, sociologist Robert S. Lynd, worried about Dunn not only because of his ill health, but also his tendency to take on too much. Lynd

¹ UACL, Central Files, Box 378, Folder 1, Robert S. Lynd, Jun 1934 to Jul 1935 and Mar 1937 to Dec 1940, Lynd to Fackenthal, 21 Sep [1940] and Fackenthal, Frank D. to Lynd, 23 Sep 1940. Fackenthal noted that his letter was in reply to Lynd's of 21 September: "Thank you very much for your letter of September 21. I knew of an illness but did not know the nature. I shall see what can be done about it."

² Dunn mentioned his illnesses to Walter Landauer (APS, Dunn Papers, Series I, Boxes 18 to 20). For example in one letter Dunn mentioned a staphylococcus infection in his hand followed by a bout with bronchitis (Dunn to Landauer, 6 Dec 1939). He missed the 7th International Genetics Congress because of a routine operation for a hernia during summer 1939 (NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, L.C. Dunn 1938-1941, Betty Drury of ECADFS to Miss Davis, Secretary to Dunn, 19 Jun 1939 and Duggan to Dunn, 22 Jun 1939).

hoped that the University's Provost would consider reassigning the chairmanship of the Zoology Department to someone else during the 1940-1941 academic school year.

He is high-mettled and hard-working, and it would probably be hard to persuade him to relinquish the departmental chairmanship. But just because he takes on responsibility so seriously and works so hard, some of the rest of us ought to try to re-structure his official situation so that he can't go on and kill himself.³

Dunn confirmed Lynd's premonition by acting as the Zoology Department's chairman during the 1940-1941 academic year, which was his first year that Dunn was assigned this responsibility. He held this post throughout the war, stepping down in order to take a much overdue sabbatical during the 1946-1947 academic year.⁴ Dunn, indeed, overexerted himself during the war years, which eventually caught up with him. When he left he was exhausted and after recovering for more than six months, he commented to Dobzhansky that he still felt a lack of energy. Dobzhansky worried about Dunn's remark and hoped that Dunn would return revived from the desert. "Vitality is one of the greatest gifts which the Gods can confer on a mortal, and surely you are one of those who possessed this gift in abundance! The last decade

³ UACL, Central Files, Box 378, Folder 1, Robert S. Lynd, Jun 1934 to Jul 1935 and Mar 1937 to Dec 1940, Lynd to Fackenthal, 21 Sep [1940].

⁴ APS, Dunn Papers, Series I, Box 13, Frank D. Fackenthal (Columbia University) 1934-1949, Fackenthal to Dunn, 4 March 1940 and Fackenthal to Dunn, 4 Oct 1943. Dunn was designated Executive Officer of the Zoology Department at a Faculty Meeting on 4 March 1940 for the period 1 July 1940 to 30 June 1943. Fackenthal informed Dunn on 4 Oct 1943 that he was appointed "Executive Officer of the Department of Zoology for three years from July 1, 1943."

you abused yourself, but Arizona must restore it all back again!”⁵ When Dunn left New York in the summer of 1946, he had intended to travel to Uppsala in the fall. Due to poor health, he stayed in the United States until February 1947 at which point he finally traveled to Scandinavia for a six month stay spent primarily with Gunnar Dahlberg in Norway. Louise and Stephen accompanied him during this sabbatical year.⁶

Louise Dunn supported her family resolutely by attending to their needs, and by working with Dunn on some of his campaigns. Dunn and Louise were joint treasurers of their local Russian War Relief fund. Together, they distributed a fall fund drive letter in 1942 and received about \$2,000, of which half came from one source. Dunn noted how remarkable the response was compared to the year before in which they received almost no response, except for a few “caustic remarks.”⁷ The difference in people’s reactions reflects Americans’ changing attitude towards the Soviet Union as a result of the United States having entered the war in alliance with the Soviet Union. Louise also experienced some adverse health conditions including

⁵ APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence #5, 1946-47, Dobzhansky to Dunn, 2 Oct 1946 and 23 Jan 1947. Dobzhansky underlined “must” twice.

⁶ RAC, RF. 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 12 Feb 1947. According to a letter from Dunn to Walter Landauer, Dunn and his family might have still been in the United States as late as March 1947 (APS, Dunn Papers, Box 19, Walter Landauer, 1947, Dunn to Landauer, 30 March 1947).

⁷ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1942, Dunn to Landauer, 21 Oct 1942.

an operation in early 1939,⁸ and occasional minor ailments.⁹ Dunn remarked most frequently of her devotion to Stephen, which is discussed below.

The well-being of his sons caused Dunn great concern and consumed a fair amount of his time during the 1940s. Facing the prospect of a possible war, Dunn and Robert, the older of his two sons, disagreed about America's involvement. Dunn wished the United States to keep the peace by remaining out of the war, whereas Bob wanted the US to enter so that the war would end more quickly.¹⁰ Bob was in his early twenties when he entered the armed forces and trained as a pilot for advanced aerial gunnery. In March 1944 his squadron left the US to bomb Germany, and a few months later he suffered and quickly recovered from some wounds. Needless to say, Bob's deployment and stint worried Dunn. Soon thereafter Bob completed fifty flights and received furlough during August and September 1944. Dunn put most of his commitments on hold in order to go to Maine and enjoy his son's visit.¹¹

⁸ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1939, Dunn to Landauer, 7 Feb 1939.

⁹ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1944, Dunn to Landauer, 23 Sep 1944. Louise had a boil that was due to staphylococcus, which had to be drained and then reappeared.

¹⁰ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1940, Dunn to Landauer, 29 April 1940.

¹¹ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1943-1944, Dunn to Landauer, 27 Jun 1943, 6 Mar 1944, 25 Jul 1944, 31 Aug 1944 and 8 Sep 1944. Dunn cancelled a trip with Walter Landauer to visit geneticist C.C. Little and his laboratory in Bar Harbor. He went to Maine with his family instead (Landauer to Dunn, 1 Sept 1944; Landauer reported to Dunn on the facilities at Bar Harbor.) Dobzhansky remained at Columbia during the summer of 1944, while Dunn was away (APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence, 1943-1945, various correspondence Jun to Aug 1944).

Stephen, Dunn's younger son, had a severe form of cerebral palsy,¹² which required him to use a wheelchair for mobility. His physical handicap presented Dunn and Louise with several challenges, especially during the 1940s. Stephen, a teenager during the war, started school near the Columbia University campus in 1941 where Dunn could easily assist him. Stephen required regular exercise care and underwent many operations over his lifetime. Stephen's operations sometimes decreased his ability to perform everyday tasks; for example, Dunn fed Stephen lunch and Louise attended to him at dinner while he recovered from an operation on his hands in 1943. It took Stephen a long time to recover from this particular operation, as noted by Dunn three-months later: "Recovery is slow business & Louise and I are simply extending the summer programs of keeping time clear for him – taking turns with exercises care & school attendance."¹³ Helping Stephen was a two-person job and although they usually had someone to assist with him, Leslie and Louise seemed to have a hard time finding caretakers during the war years. After the war ended, Stephen started college at Columbia University and eventually completed a doctorate

¹² Ethel Dunn, "Stephen P. Dunn," *Anthropology Newsletter* 40, 6 (June 1999): 48. The author of this obituary was Stephen's wife, Ethel Dunn. She also suffered from cerebral palsy, but had less severe handicaps. They got married in 1953, and together published on anthropological and sociological topics. Stephen lived to be 71 years old.

¹³ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1943. Dunn told Landauer about Stephen's hand operation on 27 June 1943. Stephen had experienced some troubles after the surgery (Dunn to Landauer on 13 Aug 1943). Dunn made the remark quoted above in a letter Landauer on 27 Sept 1943.

in anthropology.¹⁴ Years later, Dunn described his daily routines during Stephen's high school and college years (i.e. the 1940s).

I recall now that I was in & out of his school (3 blocks from my lab at Columbia) nearly every day & when he entered college[.] I at first brought him to Columbia each day then when he went to live in a dormitory for most of his course I had a habit of lurking in the background to be sure he got to his classes; & he formed the habit of dropping into my office for study & talk.¹⁵

Dunn stayed close to home during the war years for several reasons. Stephen and Louise needed his help, and therefore he restricted his travel to short trips and local destinations.¹⁶ He postponed his sabbatical until after the war ended because most destinations that interested him were in Europe, and because he had several commitments keeping him in New York.¹⁷ Remaining close to New York undoubtedly shaped Dunn's activities because he could regularly participate in committee meetings in the northeastern United States, particularly in New York and Washington D.C., while also attending to his daily activities.

¹⁴ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1940s: Stephen was accepted at Lincoln School near Columbia University, 12 June 1941; Louise needed Dunn to help with Stephen, so he could not leave New York as planned, 6 Oct 1943 and 25 July 1944; Stephen started at Columbia College, 23 Sep 1945.

¹⁵ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1970, Dunn to Landauer, 21 May 1970.

¹⁶ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1943 and 1944, various correspondences.

¹⁷ Dunn planned to take a sabbatical in 1941 (APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1938, Dunn, "Report to the Editorial Board [of Genetics] for the period November 30, 1937 to November 30, 1938). Dunn did not want to take his sabbatical for second session of 1942-1943 year (UACL, Central Files, 1.1.236, Folder 2, L.C. Dunn, Jul 1942-Jun 1943, Dunn to Fackenthal 22 Jan 1943).

Genetics & Development: Theodosius Dobzhansky Comes to Columbia

Dunn had two biological families to look after. In addition to his wife and sons, he also made the future of genetics at Columbia University one of his major concerns beginning in the late 1930s. By this time he had been with Columbia's Zoology Department for roughly one decade. Dunn worried about the fate of the biological sciences on campus due to decreased enrollment and shifts in priorities as a result of the war. Yet, he also had grand plans for a cooperative program focusing on genetic research, and one of his main goals was to acquire another geneticist. In 1940, which was a little less than two years after Dunn initially conceived of asking Theodosius Dobzhansky to come to Columbia University, Dobzhansky arrived as Professor of Zoology. He would remain at Columbia University until Dunn retired in 1962, relocating to Rockefeller Institute (later renamed Rockefeller University) because he did not want to remain at Columbia without Dunn.¹⁸

Dunn approached Dobzhansky about joining the Zoology Department at Columbia University after gaining an intimate knowledge of Dobzhansky's latest theoretical ideas. Dunn had been editing Dobzhansky's Genetics and the Origin of Species, a book that is considered pivotal to initiating the Modern Evolutionary

¹⁸ Dobzhansky's biographical material in this section comes from the following sources. Garland Allen, "Theodosius Dobzhansky," American National Biography 6 (1999): 663-65; Francisco J. Ayala, "Theodosius Dobzhansky, January 25, 1900-December 18, 1975," Biographical Memoirs of the National Academy of Sciences (Washington D.C.: National Academy Press, 1985): 163-213; Jaques Cattell Press, ed., "Theodosius Dobzhansky," American Men and Women of Science 12th edition, v.2 (New York: Jaques Cattell Press, 1972): 1475.

Synthesis.¹⁹ Modern Synthesis, a term used by Julian Huxley, has come to represent the theoretical developments drawing on evolutionary theory and unifying sub-disciplines in the biological sciences beginning in the late 1930s.²⁰

Dobzhansky wrote this revolutionary book as the result of giving a series of lectures at Columbia University in October 1936. Dunn invited Dobzhansky in April 1936,²¹ and, as Joe Cain notes, Dunn proposed that Dobzhansky write his lectures into a book and retroactively revived a defunct Columbia lecture series dating back to the 1890s called the Jesup Lectures. Traditionally the lecturers had written books based on their lectures, which were published as volumes in the Columbia Biological Series. Dobzhansky's Genetics and the Origin of Species was volume eleven in the series, and it was published more than twenty-five years after the volume preceding it. After Dunn restarted the series, fifteen lectures were given from its resumption with Dobzhansky's talks in 1936 to the final book produced in the Columbia Biological Series, Richard Lewontin's The Genetic Basis of Evolutionary Change (v. 25, 1974). Dunn edited volumes eleven to twenty-one, whose authors were not only his friends, but also revered scientists. Speakers in this series followed Dobzhansky's

¹⁹ Dunn was editing Dobzhansky's book in April 1937. APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1937, Dunn to Landauer, 19 Apr 1937. Theodosius Dobzhansky, Genetics and the Origin of Species (New York: Columbia University Press, 1937).

²⁰ Julian Huxley, Evolution: The Modern Synthesis (New York: Harper Brothers Publishers, 1943).

²¹ APS, Dobzhansky Papers, B: D65, Leslie Clarence Dunn #3, Dunn to Dobzhansky, 17 Apr 1936. Dunn told Dobzhansky that Lancefield, Schrader, MacGregor and he hoped that Dobzhansky would visit Columbia in October to give a series of lectures. Dunn planned to get money for Dobzhansky's visit.

example and produced prestigious books contributing to a new evolutionary perspective, including Ernst Mayr, Systematics and the Origin of Species (v. 13, 1942); G.G. Simpson, Tempo and Mode in Evolution (v. 15, 1944); and G. Ledyard Stebbins, Variation and Evolution in Plants (v. 16, 1950).²² Cain notes that after Dunn retired in 1962 only two more volumes were produced before the Columbia Biological Series ended again. William Provine discussed “The Origin of Dobzhansky’s Genetics and the Origin of Species” in 1994, and Cain in his article “Co-opting Colleagues” revisited and corrected some issues raised by Provine. These two authors thoroughly explore the events from 1936 to 1937 in reference to Dobzhansky’s book, the Jesup lectures, and Columbia Biological Series, and their articles should be consulted for more information.²³ Below is a detailed discussion of

²² Dunn asked H.J. Muller to give a Jesup Lecture in 1955, but Muller had to turn down Dunn’s offer because he was suffering from some health problems: “It really wrings my heart to be declining the opportunity you and the others have extended to me.” (APS, Dunn Papers, Series I, Box 23, H.J. Muller, 1946-1967, Correspondence between Muller and Dunn dated from 26 Jan to 19 Feb 1955).

²³ Joe Cain discusses Dobzhansky’s lectures and the resulting book, and he demonstrates that Dunn revived the lecture series after Dobzhansky’s book was in the final editing phases in 1937. Joe Cain, “Co-opting Colleagues: Appropriating Dobzhansky’s 1936 Lectures at Columbia,” Journal of the History of Biology 35 (2002): 207-19; Joe Cain, “The Columbia Biological Series, 1894-1974: A Bibliographic Note,” Archives of Natural History 28 (2001): 353-66. Cain’s articles respond to some issues raised by William Provine: William B. Provine, “The Origin of Dobzhansky’s *Genetics and the Origin of Species*,” The Evolution of Theodosius Dobzhansky: Essays on His Life and Thought in Russia and America, ed. Mark B. Adams (New Jersey: Princeton University Press, 1994): 99-114. Ayala, 167. Ayala credits Dobzhansky with initiating the synthesis in biology that Mayr, Simpson, and Stebbins contributed to by writing books for their respective fields of systematics, paleontology, and botany.

how Dobzhansky's lectures led to his employment as Professor of Zoology at Columbia University.

Around the same time that Dobzhansky delivered the Jesup Lectures, Dunn conceived of revamping genetics at Columbia University by setting up a Department or Division of Genetics. Dunn would not achieve his goal, but instead established a Genetics Laboratory in 1938 that he would later refer to as "largely a paper organization." When the joint laboratory began, a press release remarked on its novelty: "...the first [laboratory] in any university to merge the activities of scientists in the two fields [botany and zoology] in an attack upon the common problems of genetics."²⁴ Dunn had a tendency to think big at first, re-evaluate a situation, and eventually achieve a less lofty, but more realistic goal than originally intended. His hope for a Genetics Department, which is discussed below, is an example of just such a case, and also demonstrates his understanding of the pivotal role that genetics played within biology.

In spring 1937 Dunn began in his attempts to bring together geneticists who were associated with Columbia University, especially himself and Edmund W. Sinnott, his co-author of Principles of Genetics. Sinnott worked at Barnard College, the women's college associated with Columbia University. The idea of a joint program actually came about because University of Chicago was trying to hire

²⁴ APS, Dunn Papers, Box 5, Columbia University-Genetics Laboratory, 1938-1940, "Columbia Launches Genetics Laboratory to Unite Botanical, Zoological Research," Press Release, 11 Jun 1938. RAC, RF 1.1.200D, Box 132, Folder 1631, Dunn to Weaver, 29 Nov 1947. Dunn called it a paper organization in his letter to Weaver.

Sinnott, and Columbia's Dean of Sciences asked Dunn's advice on how to keep Sinnott from leaving Barnard College. Dunn envisioned the establishment of a division that merged scientists from two different departments, Botany and Zoology, who researched genetics problems. He and Sinnott asked for land to plant Sinnott's crops and a greenhouse on campus, as an initial request.

Dunn achieved establishing a Genetic Laboratory in which the geneticists stayed associated to their respective departments, while sharing facilities, seminars, students, and assistants. He also obtained the Nevis Estate north of New York City along the Hudson River in Irvington-on-Hudson. Another cooperative aspect of Dunn's vision for genetics at Columbia University was associating it with the Carnegie Institution of Washington's Department of Genetics at Cold Spring Harbor.²⁵ The Eugenics Record Office and Station for Experimental Evolution closed in 1939 and reorganized in 1940.

Later, Dunn felt that a proper department or institute of genetics was necessary as a result of disciplinary changes in biology. He was convinced that

²⁵ APS, Dobzhansky Papers, B:D65, Leslie Clarence Dunn #3, Dunn to Dobzhansky 24 Apr 1938; RAC, RF 1.1.200, Box 132, Folder 1630, EB memo about conversation with Dunn, 21 Apr 1937; Warren Weaver interview with Dunn, 30 Oct 1939; Dunn to Frank Blair Hanson of Rockefeller Foundation, 27 Mar 1940 and Memorandum with Regard to the Proposed Institution for Genetics and Development; APS, Dunn Papers, Box 19, Walter Landauer, 1942, Dunn to Landauer, n.d. (filed with documents dated Nov 1942), A Program for An Institute of Genetics of Columbia University and the Carnegie Institute of Washington. For the history of Columbia University in relation to Barnard College, see: John William Robson, A Guide to Columbia University: With Some Account of Its History and Traditions (New York: Columbia University Press, 1937): 125-35. Information about Nevis: APS, Dunn Papers, Box 23, Nevis Newsletter, 1970. Dorothea Bennett, "Nevis Biological Station," Nevis Newsletter 5, 1 (Feb 1970): 1-2.

“genetics has grown up and won’t be restrained by old boundaries and that we are now quite as important as any field of biology and may even be at the center of it.”²⁶

But he did not consider at this time (or any other time, as far as I can tell) the possibility of a Biology Department. The Botany and Zoology Departments at Columbia University remained separate entities until about six years after Dunn retired. In 1968, administrators integrated these two departments to form the Department of Biological Sciences. In addition to changing the administrative structure, the two departments were also united physically with the erection of a sixteen-story building.²⁷

Columbia University’s President Nicholas Murray Butler supported the idea of Dunn’s Institute for Genetics and Development, and forwarded Dunn and Sinnott’s plan to the Rockefeller Foundation. Dunn had written a memorandum with the intention that Butler read it. It was not yet a grant proposal to be submitted to the Rockefeller Foundation, but Dunn and Sinnott had that ultimate goal in mind. Dunn wrote the majority of the original memorandum, which was supplemented with more information and a budget requesting \$100,000. Dunn debated for a long time about whether or not to pursue the Institute of Genetics at Columbia. He opposed the idea because genetics might be weakened if separated from the Botany and Zoology

²⁶ RAC, RF 1.1.200D, Box 132, Folder 1631, Dunn to Weaver, 29 Nov 1947.

²⁷ UACL, Subject Files, Instruction-Life Sciences, “New Tower to Bring Life Sciences Together,” The Columbia Chronicle, Jan-Feb 1968. Most of the information for this article came from a booklet, “The Life Sciences: The \$200 Million Campaign.” The booklet mentioned that one building would have “classrooms, laboratories and offices where students and teachers can easily communicate with each other” (21).

Departments. President Butler supported the new institute because he saw it as a way to bring in research money.²⁸ Although the Great Depression was coming to an end, the Rockefeller Foundation's budget was tight at this time, 1939-1940, and Warren Weaver had to inform President Butler in January 1940 that the Rockefeller Foundation could not accommodate the request for \$100,000.²⁹

Before the Rockefeller Foundation made that decision, however, Dunn succeeded at establishing a joint genetics laboratory. The shared laboratory for geneticists, which was operating by June 1938, realized Dunn's goal to bring botanists and zoologists together "in closer collaboration." Geneticists followed two trends, according to Dunn. They either segregated from other disciplines in an effort to carve out a place for a unified genetics, or they used genetics as a unifying idea to deal with various issues concerning living matter. Dunn supported the latter, and expressed as much in a campus publication:

Progress in natural science has come to depend more and more on collaboration between fields which have been traditionally separate. This will be particularly true of the genetics of today and tomorrow; for it stands at the crossroads where many biological problems meet. The provision of the new laboratory at Columbia has greatly improved the prospects for productive research and better teaching in this traditionally Columbia subject.³⁰

²⁸ APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence, Dunn to Dobzhansky, 1 May 1940.

²⁹ RAC, RF, 1.1.200D Box 132, Folder 1630, Columbia University-Genetics, 1937-1941, Weaver to Butler, 4 Jan 1940. See correspondence starting in October 1939 for discussions about the grant.

³⁰ L.C. Dunn, "Genetics at Columbia," Columbia University Quarterly 30 (Dec 1938): 258-66, 263-66. The copy of the article that I read was provided to me by the University Archives and Columbiana Library at Columbia University.

Dunn referred to Thomas Hunt Morgan's pioneering work with his colleagues of the "fly room" at Columbia University as the origins of this long tradition. The Genetics Laboratory became a casualty of war and was not developed, which prompted Sinnott to leave Columbia University in 1940 for the Emerson Sterling Professorship and Botany Department Chairmanship at Yale University.³¹

While going through the process of reorganizing genetics at Columbia University, the Zoology Department offered *Drosophila* geneticist Alfred H. Sturtevant a professorship, but the California Institute of Technology's counteroffer proved too enticing. Sturtevant chose to stay at Caltech accepting promotion to Director of the Kerckhoff Laboratories upon Thomas Hunt Morgan's retirement. Sturtevant stayed at Caltech for the rest of his academic career, and in 1947 he was promoted from Professor of Genetics to T.H. Morgan Professor of Biology. Dunn's alternate choice was Dobzhansky, who was also at Caltech and had been working with Morgan since 1927.³²

³¹ UACL, Central Files, Box 382, Folder 4, L.C. Dunn (1946-49, 1951-53), Dunn to Unknown, n.d. Dunn mentioned the budget for 1948-1949, and based on other letters it appears to have been written between 3-14 Apr 1948. "Edmund W. Sinnott," American Men of Science 11th edition, ed. The Jaques Cattell Press (New York: R.R. Bowkes Company, 1967): 4947. The details about the Genetics Laboratory and Dunn and Sinnott's efforts on its behalf can be found in Dunn's correspondence with Sinnott between 1938 and 1940. APS, Dunn Papers, Box 25, Edmund W. Sinnott, 1934-1968.

³² RAC, RF 1.1.200, Box 132, Folder 1630, Warren Weaver interview with Dunn, 30 Oct 1939. "A.H. Sturtevant," American Men of Science 11th edition, ed. The Jaques Cattell Press (New York: R.R. Bowkes Company, 1967): 5244.

In early 1938, Dunn desired to hire Dobzhansky,³³ and he conveyed his hope to Dobzhansky in confidence and told him about some of the changes already underway.

Sinnott and I only have discussed one plan which is to put forward your name, not only because we like both you and your work, but because a genetics laboratory should combine interests in the two main lines of the future, evolutionary and developmental genetics.

Dunn had not consulted with his departmental colleagues about this plan because he wished for them to come up with it, seemingly, on their own. Moreover, he was writing in order to ask Dobzhansky his thoughts on two points: a description of the kind of genetics laboratory that should be established and the relationship that Dobzhansky would like to have with “such a lab & specifically one at Columbia.”³⁴

Dobzhansky responded positively and thoroughly. He liked the idea of moving to the east coast, especially New York, because he found Pasadena “a small town” and “provincial,” and he wanted to go somewhere in order to invigorate his thoughts. Having good relations with other biological sciences was important, and he did not think it mattered whether the genetics laboratory was its own department or not. Among the other pros was having Dunn as a colleague: “I want to tell you that nothing attracts me more than the prospective of having you as one of my immediate

³³ Dunn mentioned in February 1938 that he wanted Dobzhansky to fill an upcoming vacancy in the Zoology Department. APS, Dunn Papers, Box 19, Walter Landauer, 1938, Dunn to Landauer, 23 Feb 1938.

³⁴ APS, Dobzhansky Papers, B:D65, Leslie Clarence Dunn #3, Dunn to Dobzhansky, 24 Apr 1938.

colleagues.”³⁵ A little less than nine months later, Dobzhansky had accepted Dunn’s offer and they were discussing specifics, such as Dobzhansky’s course schedule and laboratory equipment needs;³⁶ however, Dobzhansky did not start his career at Columbia University until fall 1940. During the interim, Dobzhansky and Dunn corresponded regularly and together made decisions about who to hire as a co-worker specializing in botanical genetics. They convinced Marcus Rhoades to accept the position after negotiations with L.J. Stadler fell through.³⁷

Dobzhansky asked Dunn to delay announcing his arrival because he wanted Morgan to believe that he was considering staying at Caltech and he did not want to be rude to Morgan. Dobzhansky knew that Morgan was going to make a counteroffer to try and get him to stay at Caltech. Nonetheless, Dobzhansky told Dunn that his mind was made up to go to Columbia University. While Dunn was keeping quiet, as requested by Dobzhansky, he found out that the Rockefeller Foundation had turned down the request for \$100,000 to support an Institute of Genetics at Columbia University. Two months later Dunn must have been free to discuss Dobzhansky’s

³⁵ APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence #2, 1938-1939, 2 May 1938. Dobzhansky reiterated this point on other, later occasions including 14 Jan 1939 and 30 Jan 1940.

³⁶ APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence #2, 1938-1939, 14 Jan 1939.

³⁷ APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence #3, 1940-1942, many letters dated from 30 Jan 1940 to 17 May 1940; Box 5, Columbia University-Genetics Lab, 1938-1940, “Professor Named: Dr. Dobzhansky to take Post at Columbia Soon,” New York Sun (15 May 1940): no page. This article announced that Dobzhansky would be Professor of Zoology, Marcus M. Rhoades would be Associate Professor of Botany and Dunn would become head of the department on 1 July 1940.

decision to relocate because he called Frank Blair Hanson to tell him about it. Hanson made the following notes:

With Dobzhansky coming into the Department next year, and also some minor personnel [changes], D. [Dunn] raises the question of support for the genetics work at Columbia. The proposed setup in genetics will be presented to the Columbia University Trustees in about a month, and if their approval is received, D. wishes to discuss with RF [Rockefeller Foundation] officers the matter of some major support.

Dunn continued to speak with Hanson over the next year, but did not immediately receive funding from the Rockefeller Foundation.³⁸ Genetics at Columbia University obtained a three-and-one-half year grant starting January 1945 for \$75,000. It supported “researches on variation in genetic constitution in relation to growth and development under the direction of Professor L. C. Dunn.”³⁹

Columbia University during the War

As the Columbia University faculty responded to the world’s situation, Dunn was one of the more active faculty members in the early 1940s. His prior participation during the 1930s in University organizations responding to fascism set a trend that continued throughout the war. With the beginning of the 1940-1941 academic year, Dunn’s campus roles increased immensely. He added to his university-wide

³⁸ RAC, RF 1.1 200D, Box 132, Folder 1630, Columbia University-Genetics, 1937-1941, Notes in F.B. Hanson’s diary made after Dunn’s call, 18 Mar 1940. See correspondence from March 1940 to January 1941 for Dunn and Dobzhansky’s conversations with members of the Rockefeller Foundation.

³⁹ RAC, RF 1.1 200D, Box 132, Folder 1630, Columbia University-Genetics, 1937-1941, Norma S. Thompson, Secretary at Rockefeller Foundation to Butler, 6 Dec 1944. See also, Rockefeller Foundation Annual Report, 1945...

responsibilities by participating on several committees, such as the University Committee on National Defense and the Committee on Program. He also became Executive Officer of the Zoology Department, which had important ramifications for his campus activities.⁴⁰ He attempted to ensure the future of the Zoology Department during the war and to improve the morale of its faculty members. His efforts as Chairman of the Zoology Department between 1940 and 1946 demonstrate his administrative capabilities. This section analyzes scientific work conducted by Zoology Department faculty members and to a lesser extent by other scientists at Columbia University during the war years. Dunn's efforts as Chairman, his role in campus committees, and his knowledge of classified scientific research conducted on the campus are also explored.

In addition to overseeing the Zoology Department, another career-related accolade greatly increased Dunn's scientific prestige. The National Academy of Sciences elected him and Dobzhansky to membership in 1943.⁴¹ This achievement by two members of Columbia's Zoology Department attested to their personal scientific capabilities and the caliber of Columbia's genetics program. By 1944, the Zoology

⁴⁰ In a 1938 letter to Dobzhansky, Dunn stated that "The faculty has stuck me again with the chairmanship, this time for a five year term." (APS, Dobzhansky Papers, B: D65, L.C. Dunn #3, Dunn to Dobzhansky, 24 Apr 1938). I am uncertain if this means that he chaired the department, and if so, how the chairmanship differs from being Executive Officer of the Zoology Department.

⁴¹ Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs of the National Academy of Sciences 49 (1978): 79-104, 87.

Department had a total of four faculty members who were also members of the National Academy of Sciences.⁴²

Within the Zoology Department, Dunn encouraged faculty members to conduct investigations to help with matters pertinent to the war. Indeed, during the war the Zoology Department's budget was reduced, and Dunn noted that governmental contracts would augment the department's funds. Four faculty members of the department, Lester G. Barth, Robert Ballentine, Francis J. Ryan, and Selig Hecht, applied their scientific work to war problems.⁴³ Associate Professor Lester G. Barth, who was an active member of the American Association of Scientific Workers, investigated tissue regeneration and hoped to apply his researches to problems of wound healing, especially limb regeneration. Barth wrote a textbook on Embryology (1949) in which he discussed limb regeneration in amphibians and proposed a new theory based on greater control of regeneration. According to Barth, certain circumstances blocked regeneration and from his experiments it appeared possible to chemically induce regeneration, not only in amphibians, but possibly other vertebrates.

Why does regeneration occur in some forms and not in others? Why do we find limbs regenerating in salamanders and not in other

⁴² RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University-Genetics 1942-1947, Frank Blair Hanson notes after visiting Columbia University's Zoology Department, 17 Oct 1944. The other two were Franz Schrader and Selig Hecht (elected 1944).

⁴³ UACL, Central Files, Box 382, Folder 2, L.C. Dunn, July 1942-June 1943, Dunn to Nicholas Murray Butler, President of Columbia University, 30 Nov 1942. Information discussed below about the war work conducted in the Zoological Laboratory at Columbia University comes from this letter.

vertebrates? There is no theoretical reason for the lack of regeneration in other forms. Moreover, if we apply principles of early development, it is possible to stimulate regeneration in forms which never normally regenerate. And this is important, because it give us a control over regeneration.⁴⁴

Barth did not mention human beings, but the possibility appears to have motivated his war work. Robert Ballentine taught at Columbia University in the 1940s before moving to Johns Hopkins University where he stayed for the remainder of his long career. Ballentine had recently graduated from Princeton University in 1940 and at Columbia investigated therapies for combating gas gangrene.⁴⁵ Francis J. Ryan graduated from Columbia University earning a doctorate in developmental biology in 1941. After spending one-year at Stanford University with George W. Beadle and E.L. Tatum conducting experiments with *Neurospora*, Ryan returned to Columbia University as an assistant professor in fall 1942. Ryan's research interests continued to focus on genetics after his return and his war-related research examined vitamins and amino acids of *Neurospora*.⁴⁶ Ryan collaborated with Ballentine on these investigations.

⁴⁴ Lester George Barth, *Embryology* (1949; New York: Holt, Rinehart, and Winston, 1953): 338.

⁴⁵ Emil Venere, "Robert Ballentine, Hopkins Biologist since 1949," *The Johns Hopkins Gazette* 27 (26 Jan 1998). Found at: <http://www.jhu.edu/~gazette/janmar98/26obits.html> (9 March 2006).

⁴⁶ Francis J. Ryan was a conduit that brought together three investigators, who later shared a Nobel Prize. George W. Beadle and E.L. Tatum conducted important experiments on *Neurospora* while Ryan was at Stanford University on a National Research Council Fellowship. Beadle and Tatum shared the Nobel Prize in Physiology and Medicine of 1958 with Joshua Lederberg. Ryan mentored Joshua Lederberg from 1942 to 1944, which was immediately after his return from Stanford University. Lederberg left Columbia University in 1944 at Ryan's insistence to obtain

Selig Hecht, a Professor of Biophysics, investigated physiological aspects of vision, paying particular attention to colorblindness. The Air Corps, Signal Corps, and Canadian Navy employed him to devise and build an apparatus for testing human eyes. Hecht described his war research as “red hot” because of the interest of the armed forces in it. Of his own work done for the Naval Aviation Examination Station, Hecht reported that he had examined 500 men of whom 175 were colorblind. He noted that he was not interested in statistical numbers regarding the frequency of color blindness, but rather was looking at its properties.⁴⁷ Hecht was elected to the National Academy of Sciences in 1944, one year after Dunn and Dobzhansky received this same honor.⁴⁸

Dunn grappled with whether he should contribute to the war effort by using his genetics knowledge for scientific purposes.

I am torn in two directions by the war as most of us must be 1) to find any job which might contribute in however small a way to shortening it & bringing about a secure peace. There doesn't seem much chance that many of us in our field will find such jobs. 2) to keep the work of science going & particularly to train some young men so there won't be so great a gap in the continuity.⁴⁹

his PhD under E.L. Tatum at Yale University (“Francis J. Ryan, 1916-1963” from <http://www.fgsc.net/fgn/nn4/4obit.pdf> on 8 March 2006 and <http://nobelprize.org/medicine/laureates/1958/> on 9 March 2006).

⁴⁷ APS, Stern Papers, Ms Coll 5, Selig Hecht, Hecht to Curt Stern on 25 Nov 1942 and 2 Dec 1942.

⁴⁸ APS, Stern Papers, Ms Coll 5, Columbia University-Department of Zoology Newsletter #6, May 1944.

⁴⁹ APS, Ernst Caspari Papers, Ms Coll 1, L.C. Dunn #2, Dunn to Caspari, 22 Dec 1942.

Option two describes the route that Dunn took. He chose to direct his war-related work to political and scientific campaigns that did not involve scientific experimentation. Ultimately, through the campaigns that he undertook, Dunn attempted to preserve the pursuit of science through the war.

Most of the senior members of the department, including Dunn, continued their research without accommodating it to war time issues.⁵⁰ Dunn, Theodosius Dobzhansky, Franz Schrader, Arthur W. Pollister, and William K. Gregory decided to work collectively on one grand genetic problem that drew from their individual specialties. They loosely defined their proposed research subject in fall 1942 to Columbia University's President Nicholas Murray Butler as "...studies to elucidate the structure and functioning of the hereditary materials, in transmission from generation to generation, in the development of the individual and in the differentiation of species."⁵¹ Two years later Dunn submitted a grant proposal to the Rockefeller Foundation in which he summarized the department's overlapping research as dealing with growth, development, and heredity.⁵² A broad and ambitious goal, Dunn proposed that it might warrant its own cross-disciplinary institution in the

⁵⁰ Elizabeth Hodes noted that Dunn worked on war projects for the government (82). Although Dunn worked on committees that dealt with war-related issues, he did not alter his scientific research. Also, he adapted his courses to train military men, but the US government did not hire him.

⁵¹ UACL, Central Files, 1.1.236, Box 382, Folder 2, L.C. Dunn (Jul 1942-Jun 1943), Dunn to Provost Frank D. Fackenthal, 11 Nov 1942 and Dunn to President Nicholas Murray Butler, 30 Nov 1942; APS, Stern Papers, Columbia University, Department of Zoology News Letter Number 4, 1 June 1943.

⁵² RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University – Genetics 1942-1947, Dunn to Frank Blair Hanson, 7 Oct 1944.

future. In years to come, Dunn and Dobzhansky remained committed to this goal and marginally achieved it by narrowing their focus to human beings and founding the Institute for the Study of Human Variation.

Dunn found that formulating this research project in 1942 helped to improve morale because it caused the faculty to think beyond the war's end.⁵³ Moreover, he hoped that the program would guarantee the continuation of genetics throughout the war, even if there were no students to teach. Dunn focused on educating the general public and hoped that this aspect would help to sell the idea: "I suppose it may seem presumptuous to try to set up such an institute in war time – but I believe the public justification which we hope to give it by interpreting genetics for non-specialists will gain the support of our president." Dunn also capitalized on the location of Columbia University by noting that the New York region had "the largest concentration of geneticists representing nearly all the different approaches to the central problem stated in the purpose of the institute."⁵⁴

As mentioned above, in 1944 Dunn sought money from the Rockefeller Foundation for the Zoology Department's research program, and successfully received a \$75,000 grant to be distributed over 3 ½ years, beginning 1 January

⁵³ Dunn commented on some of the problems facing the Zoology Department in 1942, which affected morale. "Unfortunately we have had to reduce our colony to a little over half its size & to abandon some stocks entirely." Dunn also noted that they did not have as many people in the lab to help care for the animals. APS, Ernst Caspari Papers, L.C. Dunn #2, Dunn to Caspari, 22 Dec 1942.

⁵⁴ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1942, Dunn to Landauer, 16 Nov 1942. UACL, Central Files, 1.1.236, Box 382, Folder 2, L.C. Dunn (Jul 1942-Jun 1943), Dunn to Provost Frank D. Fackenthal, 11 Nov 1942. Dunn referred to President Nicholas Murray Butler of Columbia University.

1945.⁵⁵ The Rockefeller Foundation awarded Columbia University this grant based on Dunn's leadership. Frank Blair Hanson of the Rockefeller Foundation visited the Zoology Department before his organization awarded the grant, and wrote the following about the changes in the Zoology Department since Thomas Hunt Morgan and E.B. Wilson had left:

...with Wilson's retirement and Morgan's removal to California Institute of Technology, both events occurring in 1928, there followed a difficult period under several chairmen...In recent years, however, this picture has substantially changed. Under the direction of Prof. L.C. Dunn, and with the encouragement and support of the University, a highly competent staff has been assembled, composed principally of young men and of those in their early prime...⁵⁶

In order to ascertain what other geneticists thought of Dunn and the Zoology Department at Columbia University, Hanson contacted geneticists Curt Stern and Hermann J. Muller, who gave glowing reports of Dunn and the department.⁵⁷

⁵⁵ The research conducted by the Zoology faculty as a result of this grant will be discussed in Chapter 9. RAC, RF 1.1 200D, Box 132, Folder 1630, Columbia University – Genetics, 1937-1941 (misfiled), Norma S. Thompson, Secretary at Rockefeller Foundation to President Nicholas Murray Butler of Columbia University, 6 Dec 1944. The letter said that the Columbia University Department of Genetics received \$75,000 from 1 Jan 1945 to 30 Jun 1948 “for researches on variation in genetic constitution in relation to growth and development under the direction of Professor L. C. Dunn...” The Zoology Department received a five year extension (from 1948 to 1953) for an additional \$82,500 of this Rockefeller Foundation grant.

⁵⁶ RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University – Genetics 1942-1947, Frank Blair Hanson's notes about his visit to Columbia University's Zoology Department, 17 Oct 1944.

⁵⁷ RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University – Genetics 1942-1947, Frank Blair Hanson interview with Curt Stern, 19 Oct 1944. Stern reported good things about the capabilities of the investigators in the Zoology Department. About Dunn, Hanson recorded: “The opinion seems to be that Dunn, the new Chairman of the Dept., has done an excellent job in his appointments and in the general administration of the Dept. S. [Stern] says it is one of the leading departments

Faculty of the Zoology Department conducted the following scientific research on growth, development, and heredity in 1944. Dunn and Salome Gluecksohn-Schoenheimer investigated growth and differentiation in mutant strains of mice. Dobzhansky analyzed aspects of population genetics by continuing his experiments on *Drosophila*. Schrader examined abnormal growth in chromosomes as a method for understanding the normal processes of mitosis. Pollister collaborated with A.E. Mirsky of the Rockefeller Institute on investigations of protein changes in mitosis and meiosis in order to learn about the role of genes and chromosomes in development. Barth, Ryan, and Ballentine continued to conduct their investigations pertinent to war issues, which also aligned with the research program suggested in Dunn's grant application. There were two people in the department whose work did not fit in with this unified program, Selig Hecht's research on vision and William K. Gregory's work in paleontology.⁵⁸

The Zoology Department adjusted their academic courses to address issues presented by the war. The whole of Columbia University opened itself to new recruits of the United States Naval Reserve Midshipmen's School, the Navy V-12 Program,

of the country and he recommends RF support." In the same file is the following notes by Frank Blair Hanson based on an interview with H.J. Muller (at Amherst), 3 Dec 1944. "The Dept. of Zoology, Columbia U., under L. C. Dunn, has set up an Advisory Committee of prominent zoologists outside of Columbia to meet in the Columbia Zoology Dept. once or twice a year to advise on research programs, teaching courses, methods, etc. M. [Muller] has been appointed a member of this Advisory Committee to Columbia Zoology, and highly recommends the proposed RF appropriation to Columbia."

⁵⁸ RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University-Genetics 1942-1947, Dunn to Frank Blair Hanson, 7 Oct 1944 and Frank Blair Hanson's notes about his visit to Columbia University's Zoology Department, 17 Oct 1944.

and the US Navy School of Military Government and Administration.⁵⁹ Through this “Navy Program” the Zoology Department’s professors trained pre-medical students of the Navy V-12 program by teaching one year of biology and zoology to approximately 150 cadets. These naval students also chose either Comparative Anatomy or Embryology as a half-year elective.⁶⁰ Dunn taught a course in heredity to Navy undergraduates and other students during the fall of 1943. The Zoology Department’s newsletter noted the changes induced by the war: “The war has of course affected the teaching of the Department. This is most noticeable in the increased pace of undergraduate training and the altered schedule imposed by the Navy program.” The graduate program witnessed fewer changes than the undergraduate program.⁶¹

Two professors from the Zoology Department, Dunn and Hecht, also taught at the New School for Social Research in New York City. The New School offered professors from the local area a chance to teach courses and give lectures to wider audiences. It advertised its courses as good preparation for the post-war world because its “curriculum has been made up of alive, provocative and practical courses in the culture, politics and affairs of the world” and it “has been training and

⁵⁹ UACL, World War II Collection, From the Historical Note about the Collection, Series V. Educational and Military Training Programs (1940-1946).

⁶⁰ APS, Stern Papers, Columbia University-Department of Zoology News Letter Number 4, 1 Jun 1943.

⁶¹ APS, Stern Papers, Columbia University-Department of Zoology News Letter, Number 5, Dec 1943.

furnishing leaders to industry, government and social industry.”⁶² As discussed previously in Chapter Three about refugees, Alvin Johnson the Director of the New School arranged for his academy to offer teaching posts to refugees during the 1930s and early 1940s and eventually joined the Executive Committee of the Emergency Committee in Aid of Displaced Foreign Scholars. Thus, Dunn and Johnson not only knew one another, but also held similar aspirations.

Dunn and Hecht independently taught courses at the New School and jointly participated in an evening discussion panel in November 1942 on “The Nature of Life.”⁶³ Hecht started working in Columbia University’s Zoology Department in 1926, two years before Dunn arrived. They developed a strong and close friendship over the twenty years that they were colleagues, and participated on similar campaigns. Hecht wrote to Dunn from Europe in 1933 and updated Dunn on the political situation. Dunn used his correspondence with Hecht to inform members of the Emergency Committee in Aid of German Scholars during its first months.⁶⁴ In the late 1930s they collaborated to help people in Spain fight against fascism,⁶⁵ and Dunn

⁶² Advertisement, New York Times (16 Jan 1944): SM41.

⁶³ Advertisement, New York Times (8 Nov 1942): X2. The dialogue between Dunn and Hecht was held on 12 Nov 1942.

⁶⁴ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid for German Scholars-Correspondence, April-August, 1933, Hecht to Dunn, 20 June 1933 from Germany and 28 June 1933 on route to Italy; APS, Dunn Papers, Series I, Box 15, Selig Hecht, 1933, n.d., Hecht to Dunn, 23 July 1933; NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records, L.C. Dunn, 1933, Hecht to Dunn, 15 Aug 1933 from Paris.

⁶⁵ APS, Boas Papers, Federation of Faculty Committees for Aid to the Spanish People. Dunn and Hecht were both members of this Federation.

later invited Hecht to join the Faculty Fellowship Fund, which placed qualified refugees into academic position at Columbia University.⁶⁶

Although they held their courses independent of each other at the New School, both men aimed at reaching a general audience by teaching there. Additionally, each produced a book on a technical topic for non-scientific readers based on their New School course. Dunn taught a course on “Heredity and Society” in the early part of 1944 and repeated it two years later.⁶⁷ A large attendance of about fifty people per lecture in 1944 astounded Dunn.⁶⁸ While teaching this course, Dunn and Dobzhansky were writing the first edition of their book, Heredity, Race and Society (1946),⁶⁹ which addressed similar matters as Dunn’s lectures. Dunn, who supported using science to inform one’s general knowledge, warned against basing absolute political decisions on science.

It is true there have been attempts to examine scientifically these questions about the unlikenesses [sic] of men, but it is also true that knowing the answers to the questions will not necessarily determine what we shall do about them.

⁶⁶ APS, Dunn Papers, Series I, Box 5, Columbia University-Faculty Fellowship Fund, 1940-1941, n.d., Minutes of the Meeting of Faculty Fellowship Fund Committee, 10 Jan 1941.

⁶⁷ Dunn taught “Heredity and Society” at the New School for Social Research from 3 Feb to 6 Apr 1944. APS, Stern Papers, Columbia University-Department of Zoology Newsletter, News Letter Number 5, Dec 1943 and News Letter Number 10, March 1946.

⁶⁸ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1944, Dunn to Landauer, 6 Mar 1944.

⁶⁹ L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society (New York: The New American Library, 1946). More will be said about Dunn and Dobzhansky’s book in a later chapter.

Issues pertaining to Nazi German informed Dunn's perspective.⁷⁰

Selig Hecht spoke at the New School in 1945 on topics pertaining to his specialization in the physiology of vision,⁷¹ and later gave lectures on the atomic bomb. Hecht developed a book titled Explaining the Atom (1947) from his course at the New School and speeches elsewhere about atomic energy in an effort to make the information accessible to laypeople.⁷² Hecht, who had trained in zoology and physiology, did not help to develop the atomic bomb and was not a physicist. He came to his understanding of atomic energy by reading the information available in the public record and made it a point to tell his readers that the data was not confidential. Hecht, like Dunn, aimed to convey information to laypeople in hopes of informing their knowledge about political questions: "Only by understanding the basis and development of atomic energy can one judge the legislation and foreign policy that concern it. I hope that this book will help to make intelligent voters."⁷³

⁷⁰ APS, Dunn Papers, Series I, Box 8, L.C. Dunn – Lecture: "Heredity and Politics," 1943-1944, 10 pages, 6-7. Dunn delivered this talk in January 1944, which was one month before he started teaching "Heredity and Society" at the New School. Dunn gave this lecture at Bard College, Annandale-on-Hudson, New York, which is associated with Columbia University. Dunn called eugenics "a gentler doctrine than the extermination recommended (and practiced) by the Nazis."

⁷¹ APS, Stern Papers, Columbia University – Department of Zoology Newsletter, News Letter Number 9, Nov 1945. Hecht titled his lecture, "The World and the Senses."

⁷² APS, Stern Papers, Columbia University – Department of Zoology Newsletter, News Letter Number 10, March 1946; George Wald, "Selig Hecht, February 8, 1892 – September 18, 1947," Biographical Memoirs of the National Academy of Sciences 60 (1991): 81-100, 91; Selig Hecht, Explaining the Atom (New York: Viking Press, 1947).

⁷³ Hecht's book was published the year that he died, and later in 1954 Eugene Rabinowitch, who started the Bulletin of the Atomic Scientists, revised and added

Hecht died suddenly in 1947, the year that his book was first published. Dunn wrote his obituary for the Columbia University faculty.⁷⁴

Columbia University as a whole had many researchers engaged in war work, in particular physicists and chemists who contributed to the Manhattan Project. As the National Defense Research Committee developed into the Office of Scientific Research and Development in 1941, Columbia University's Substitute Alloy Materials Laboratory (referred to as SAM Labs) had five contracts with these governmental agencies.⁷⁵ Physicists initiated research on fission and a chain reaction in early 1939 prior to receiving money from the government. Enrico Fermi continued these investigations working with physicists Leo Szilard, John R. Dunning, and George B. Pegram, who was also Dean of the Graduate Faculties. Fermi had recently arrived at Columbia University after permanently leaving fascist Italy when he received a Nobel Prize in Physics in 1938. Fermi, a few of his colleagues, and their equipment left New York in early 1942 for the Metallurgical Laboratory (Met Lab) at University of Chicago. Fermi also spent about one-and-one-half years at Los Alamos before returning to the University of Chicago where he remained for the rest of his career.

four chapters to it. Selig Hecht, Explaining the Atom, rev. Eugene Rabinowich (1947; New York: Viking Press, 1963): xviii. Hecht's quotation comes from his preface to the first edition. Hecht mentioned a couple of times how he came to learn about atomic energy and that the information was not a secret (xvii-xviii, 168-70). Hecht noted that he had no training in physics (xvii) and Rabinowitch reiterated this point in his revised edition (xiv).

⁷⁴ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-"Selig Hecht," 1947.

⁷⁵ Kleinman, 63.

Chemist Harold C. Urey, who had received a Nobel Prize for Chemistry in 1934 for his discovery of heavy hydrogen, oversaw about 700 people as an administrator for the Manhattan Project at Columbia University. He drew from his knowledge of deuterium and heavy water to develop a way to control chain reactions. He also investigated gaseous diffusion with Dunning and others researching the separation of Uranium isotopes U-238 and U-235. Urey asked to be relieved of his administrative responsibilities with SAM Labs by early 1944, and remained at Columbia University until 1945, at which point he also relocated to Chicago's Met Lab. Dunning's group was originally located in Pupin Hall on the Columbia campus, but when they needed more room they used space available in Schermerhorn Hall where the Zoology Department was located.⁷⁶

Although Dunn was in steady contact with Urey, Pegram, and others working on the Manhattan Project, it is not evident to what degree he knew specific details about their research. Dunn, Pegram, and others at Columbia University started the Faculty Fellowship Fund in 1933, which was still active during the war. The Faculty

⁷⁶ Henry DeWolf Smyth, Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government, 1940-1945 (1949; Stanford: Stanford University Press, 1989); Richard G. Hewlett and Oscar E. Anderson, Jr., The New World, 1939-1946: A History of the United States Atomic Energy Commission Vol. 1 (University Park: Pennsylvania State University Press, 1962); Alice Kimball Smith, A Peril and a Hope: The Scientists' Movement in America: 1945-47 (Chicago: University of Chicago Press, 1965): 59-60; Badash, 24-35; Emilio Segrè, "Enrico Fermi," Dictionary of Scientific Biography 4, ed. Charles C. Gillespie (New York: Charles Scribner's Sons, 1971): 576-583, 580-82; Albert B. Costa, "Harold Clayton Urey," National American Biography 22, eds. John A. Garraty and Mark C. Carnes (New York: Oxford University Press, 1999): 123-26, 123-24.

Fellowship Fund gathered monetary donations from faculty members and then used the money to hire academic war refugees to teach at Columbia University.⁷⁷ Dunn and Pegram also supported some of the same initiatives sponsored by the American Committee for Democracy and Intellectual Freedom in the late 1930s.⁷⁸ Before the United States entered the war and after Columbia had initiated war-related research for the government, Urey, Pegram, and Fermi asked Dunn to help them organize the 2nd International Congress of Pure and Applied Science (Physics, Chemistry, and Biology) to be held in the United States. They hoped to capitalize on America's neutrality in order to "bring to the attention of scientific laymen the International character of Science and its peaceful objectives at a time when these things need such emphasis." Dunn declined because he did not feel he could give his time to the event and was pessimistic that it would convene considering the tumultuous state of the world. Dunn's doubts proved valid. Urey, Pegram, and Fermi planned for the 2nd

⁷⁷ NYPL, Emergency Committee in Aid of Displaced Foreign Scholars, Box 156, Executive Committee Records, L.C. Dunn, 1933, John Dewey, Franz Boas, J.P. Chamberlain, Hans T. Clarke, W.C. Mitchell, George B. Pegram, Dunn and others to Columbia University's Faculty, no date. Feedback from faculty members had convinced them to start the Faculty Fellowship Fund.

⁷⁸ APS, Dunn Papers, Series I, Box 25, Aid to Spain, 1937, n.d., Telegram from Columbia University Faculty to General Cabanellas, President of the Junta, 11 Feb 1937. Dunn, Franz Boas, W.C. Mitchell, Robert S. Lynd, Hans T. Clarke, Walter Rautenstrauch, J.P. Chamberlain, M. Handler, George B. Pegram and others sent a telegram saying: "The undersigned members of the Columbia University in New York urge the commutation of the death sentence passed on Dr. Leopoldo Alas President of Oviedo University."

International Congress of Pure and Applied Science to meet at Columbia University in September 1940, but announced its postponement one month beforehand.⁷⁹

A few years later Fackenthal asked Dunn to have dinner at the University's Faculty Club on 27 November 1944 with Urey, Pegram, I.I. Rabi, and Marcus Rhoades to discuss the future of pure science at American Universities and Columbia University, in particular.⁸⁰ I.I. Rabi of the Physics Department aided the Manhattan Project as an associate director of the Radiation Laboratory at Massachusetts Institute of Technology and continued to work closely with the military throughout his career at Columbia University.⁸¹ Dunn and Dobzhansky wooed botanist and geneticist Marcus Rhoades to Columbia in 1940 and collaborated with him on projects merging botanical and zoological genetics.⁸² Fackenthal most likely knew about Dunn's concurrent work with Congressmen in Washington D.C. to set up federal funding for science.⁸³

⁷⁹ APS, Dunn Papers, Series I, Box 4, Columbia University-Frank D. Fackenthal, 1929-1945, Harold Urey, President of the Congress, George Pegram, member of the Executive Committee, and Enrico Fermi, Scientific President of the Section of Physics to Dunn, 16 May 1940 and Dunn to Urey, 29 May 1940. Also in this folder at APS is an announcement for the Congress. For an announcement about postponing the Congress: "Scientific Notes and News," *Science* 92 (16 Aug 1940): 146.

⁸⁰ APS, Dunn Papers, Series I, Box 4, Columbia University-Frank D. Fackenthal, 1929-1945, Fackenthal to Dunn, 17 Nov 1944.

⁸¹ William A. Nierenberg, "I.I. Rabi," *American National Biography* 18, eds. John A. Garraty and Mark C. Carnes (New York: Oxford University Press, 1999): 50-52.

⁸² APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky – Dunn Correspondence #3-6, 1940-1949. Dunn and Dobzhansky mention Rhoades in several letters from 1940 to 1949.

⁸³ Fackenthal definitely knew about Dunn's commitment to establishing a National Science Foundation by early 1945. He read Dunn's address on "the organization and support of scientific research" and wanted ten to a dozen reprints if it was published.

The day after their dinner, Dunn formally suggested starting a Council for Research in the Natural Sciences. Fackenthal thought it an “excellent suggestion” and soon thereafter asked Dunn for a list of faculty for the Council’s membership. Anticipating that the US government would be contributing more money to research in the future, Dunn believed that the Council should oversee the allocation of research funds for Columbia’s science departments as well as learn about funding sources. He also suggested that each scientific discipline have one member on the Council to represent its needs and mentioned that he would consult with Urey and others in the science faculty about the Council’s purpose.⁸⁴ Networking with members of the science faculties at Columbia University was only one aspect of his campus efforts. Dunn also conducted committee work that resulted from war needs.

Columbia University President Nicholas Murray Butler appointed Dunn to the University Committee on National Defense at its onset in July 1940. This committee cooperated with the American government, especially the US National Defense Research Committee, in an effort “to protect and defend those fundamental principles of American economic, social and political life.”⁸⁵ The US government started the National Defense Research Committee in June 1940 and one year later broadened its

APS, Dunn Papers, Series I, Box 13, Frank D. Fackenthal (Columbia University), 1934-1949, Fackenthal to Dunn, 8 Jan 1945.

⁸⁴ APS, Dunn Papers, Series I, Box 4, Columbia University-Frank D. Fackenthal, 1929-1945, Dunn to Fackenthal, 28 Nov 1944, Fackenthal to Dunn, 5 Dec 1944 and Dunn to Fackenthal, 19 Jan 1945.

⁸⁵ APS, Dunn Papers, Series I, Box 4, Columbia University-Columbia Defense Committee 1940-1941, Frank D. Fackenthal to Dunn, 5 July 1940 and Butler’s announcement of the University Committee on National Defense, 17 July 1940: 13 pages. Quote is from page 1 of Butler’s announcement.

responsibilities and renamed it the Office for Scientific and Technical Development. Vannevar Bush directed these organizations and acted as a scientific administrator for the government throughout the war.⁸⁶ The main purpose of these two governmental organizations was to coordinate American scientists, technologists, and resources for the development of war weapons.⁸⁷ Dunn was a likely candidate for University Committee on National Defense because he was quite familiar with war related committee work, through his participation on the Emergency Committee in Aid of Displaced Foreign Scholars. He was also familiar with Columbia University faculty because he acted as Secretary of the Faculty Fellowship Fund from 1933 to 1941.⁸⁸ He had also participated on committees for the National Research Council since the 1920s that had a similar goal of coordinating scientists at a national level.⁸⁹ The

⁸⁶ Vannevar Bush also played an important role in suggesting a structure for the National Science Foundation that was in competition with Senator Harley M. Kilgore's, as discussed in the previous chapter.

⁸⁷ Irvin Stewart, Organizing Scientific Research for War (New York: Arno Press, 1980): 7-8.

⁸⁸ APS, Dunn Papers, Series I, Box 5, Columbia University – Faculty Fellowship Fund, 1940-1941, n.d., Minutes of the Meeting of Faculty Fellowship Fund Committee, January 10, 1941.

⁸⁹ APS, Dunn Papers, Series I, Box 23, National Research Council, 1922-1927 and National Research Council-Committee on Experimental Plants and Animals, 1928-1932; Later, Dunn was on the National Research Council Committee on Human Heredity as noted in: "Scientific Notes and News," Science 92 (16 Aug 1940): 145-47, 146; Dunn reported to Landauer on his recent trip to Washington D.C. during which he had attended "tedious" meetings with the National Research Council. The doorman told Dunn it was his 20th Anniversary of attending meetings to which Dunn said "I shudder to think of all the weekends I have spent in that place." He also visited with Senator Kilgore and discussed the national science foundation bills (APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1944, Dunn to Landauer, 6 Mar 1944).

National Research Council, established under the National Academy of Sciences during World War I advised the United States government on scientific matters.⁹⁰

The University Committee on National Defense at Columbia examined national and international problems and then determined and organized the University's response.⁹¹ For example, Committee members devised accommodations for students attending the university who could not complete an academic session because they were called to fight in the war. Students drafted into active duty received partial tuition refunds and course credit. The University also offered courses providing technical training in an effort to prepare students for wartime jobs. Lastly, faculty taught classes that discussed war-related issues, especially those facing Americans.⁹²

Early on, Dunn thought, enthusiastically, that he could accomplish great things through this Committee of "stiffnecked," automaton members: "I know that these people think so little, that a plan presented with enthusiasm and the appearance of reason can succeed even when opposed to their purposes."⁹³ However, his enthusiasm proved premature, and within months he criticized the approach taken by the University Committee on National Defense. He felt that the Committee's first bulletin focused too narrowly on military matters and urged its revision. According to

⁹⁰ Greenberg, 78.

⁹¹ "Columbia Moves to Aid U.S. Defense," New York Times, 4 Jul 1940: 8; APS, Luria Papers, Series IIa, Columbia University, "Report" on Columbia Queries, n.d.

⁹² "Columbia Offers Military Training," New York Times, 13 Sep 1940: 9 and "Defense to Rule Columbia Opening," New York Times, 22 Sep 1940: D6.

⁹³ APS, Dunn Papers, Series I, Box 19, Walter Landauer, Dunn to Landauer, 27 July 1940.

Dunn, the Committee members needed to define their goal by outlining the principles they intended to defend, and state their purpose by explaining the university's unique role in offering wartime defense. Moreover, Dunn argued that the Committee should serve as "a moulder [sic] of public opinion and protector of intellectual endeavor." Drawing from his involvement with the American Committee for Democracy and Intellectual Freedom, Dunn called for a more authoritative stance and increased public role on the part of this Committee.⁹⁴ About one year later, Dunn started serving on the newly-formed Committee on Program, which attempted to plan for the post-war world. The Committee on Program, which grew out of the University Committee on National Defense, came closer to his vision for action.

The Committee on Program started in 1941 with an eye to the future. The Committee on Program had a slow start, gained some impetus, and quickly fizzled out. Philip Jessup accepted the chairmanship and asked Dunn to join the committee. At this point in time, Jessup practiced law at a New York law firm, taught international law at Columbia University, and gave legal advice to Federal officials. While Jessup is known for his diplomatic and international work with the US government, in the early 1940s he had yet to establish this reputation. Jessup had a vague notion of the Committee's intent when he explained to Dunn that it would analyze "the changed conditions which it seems likely that the University, in common

⁹⁴ APS, Dunn Papers, Series I, Box 4, Columbia University-Columbia Defense Committee 1940-1941, Dunn to Fackenthal, 5 Sep 1940.

with all other persons and institutions, is likely to face in the next decade or two.”⁹⁵

Dunn joined as well as some of his close colleagues: Sociologist Robert Lynd, writer and English professor Mark Van Doren, and Provost Frank D. Fackenthal.⁹⁶ Jessup took a leave of absence from Columbia University shortly thereafter, which lasted longer than originally planned. If Jessup returned over the next year, then he did not make the Committee his priority. Instead, he focused on advising governmental officers in Washington D.C.⁹⁷ By April 1941 Committee members had drafted some recommendations. Dunn had two immediate goals for the Committee: “We need to clarify our own minds before we can enlighten others.” He then suggested some recommendations and discussion topics for the Committee to ponder.⁹⁸ Over the next

⁹⁵ APS, Dunn Papers, Series I, Box 5, Columbia University Library Planning Committee 1940-1950, Jessup to Dunn, 30 Dec 1940. Philip Jessup is not related to the Jessup Lectures that were mentioned previously.

⁹⁶ The Committee on Program had the following members: P.C. Jessup, J. C. Bonbright (Finance), L.C. Dunn (Zoology), Frank D. Fackenthal, Robert Lynd (Sociology), Roswell Magill, E.G. Miller, Jr., Nathaniel Peffer (International Relations), and Mark Van Doren (English). From APS, Luria Papers, Series IIa, Columbia University, “Report,” 8 pages, 1.

⁹⁷ APS, Dunn Papers, Series I, Box 17, Philip C. Jessup 1941, Correspondence between Jessup, Dunn, Bonbright’s Secretary (Hynd), and Fackenthal, dated Sep to Dec 1941. Biographical information on Philip C. Jessup is from: Eric Page, “Philip C. Jessup Dies; Helped End Berlin Blockade,” New York Times (1 Feb 1986): 13; Donna Grear Parker, “Philip C. Jessup,” American National Biography (Oxford University Press, 2000).

⁹⁸ APS, Dunn Papers, Series I, Box 4, Columbia University-Suggested Recommendations of Committee on Program 1940, “Suggested Recommendations of Committee on Program,” n.d. (most likely April 1941), 5 pages. APS, Dunn Papers, Series I, Box 5, Columbia University-Library Planning Committee 1940-1950, n.d., Correspondence between Committee members dated Feb to Apr 1941.

year, they developed and distributed queries, and by October 1942 Dunn noted that the Committee lost momentum.⁹⁹

A series of “Columbia Queries” gathered feedback from the university’s personnel on matters facing Americans as a result of the war. Committee members summarized two main problems with which they grappled:

- (1) To state some of the fundamental issues which our society and our university will be called upon to face in the future;
- (2) To discover methods by which the members of the university might give collective consideration to such long range problems.

Using an ingenious layout they developed query booklets that proposed a multi-point resolution. On each page of the booklet was a statement with which the reader was asked to agree or disagree. The idea was to read a page and if the respondent answered yes, then he or she proceeded to the next page. If he disagreed with the statement, then there was no need to continue with the questionnaire. The query progressively presented more specific statements; therefore, the point at which the respondent disagreed indicated the degree of his approval. The Committee on Program sent out and tabulated results for three queries before holding a forum to discuss them on 12 May 1942. The topics for the three queries were the need for international cooperation through an international organization, the emergence of

⁹⁹ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1942, Dunn to Landauer, 21 Oct 1942. Dunn told Landauer that the Committee on Program had fallen apart. He had foreseen this outcome as early as the spring. Dunn was laid up with bronchitis yet again.

close ties between political and economic power, and the decline of individual representation within governments.¹⁰⁰

Eight faculty members, including Dunn, also drafted an educational program to assist in the process of demobilization. The program's three goals were prepare veterans for civilian life, give them something to do while demobilizing, and continue their education that was interrupted by the war. Serving on a sub-committee with Dunn and Van Doren was Ralph Linton, Chair of the Anthropology Department. Together, they brainstormed two instructional approaches, both of which were meant to instill a liberal arts-based knowledge that would cultivate intellectual and humane pursuits and appeal to the students' technical inclinations. For the less literate, they suggested prompting discussion by using visual and audio media and teaching mechanics and history of invention. For those with more education, they proposed a more individualistic approach for instructing traditional academic subjects in mathematics and humanities. At this point they had not devised concrete details about course length or developed a method for administering the information. They discussed employing techniques used by home study and extension courses, which implies that the students would not be coming to the campus.¹⁰¹

¹⁰⁰ APS, Luria Papers, Series IIa, Columbia University, "Report," 8 pages. For each query they distributed 1500 booklets and the following number of people responded per query: #1: 681; #2: 576; #3: 406.

¹⁰¹ UACL, Central Files, Box 382, Folder 2, L.C. Dunn, Jul 1942-Jun 1943, "Draft Memorandum," summary of luncheon meeting held 24 Nov [1942]: 8 pages. Only three of the eight faculty members were named: Dunn, Mark Van Doren, and Ralph Linton.

Conclusion

Dunn grappled with several issues at once during the early 1940s pertaining to his personal life and professional career, yet he still found time to take war issues into consideration. He participated at the local level in various ways including campus committees and offering classes through the New School for Social Research. He adapted his professional schedule to accommodate his family's needs by helping Stephen on a regular basis and taking one-month off when Robert had furlough. Furthermore, Dunn looked to the future by developing a plan for returning servicemen and formulating a program incorporating the research interests of his closest colleagues. Dunn took some important steps during the war that had long-term effects for him and the Zoology Department at Columbia University. After Dobzhansky relocated to Columbia University, Dunn and Dobzhansky started on a mutual research path that continued into the 1950s, which will be explored to a greater extent in Chapter Nine. Looking to the future became quite important to Dunn as the war came to a close. This is evident in his wartime activism and will be even more evident in the next chapter in terms of Dunn's international responses to the Second World War.

Chapter 6

Overseas Colleagues: International Responses to War

Dunn disliked the way in which war threatened the internationalism of science, and he reacted by reaching out to his fellow scientists in other countries. He attempted to respond to the many requests he received asking for his aid because he wanted to get scientific information and supplies to scientists overseas who needed them. When he received requests, he either sent the items himself or asked someone else to help. He also promoted better communication between the Soviet Union and United States using science as a means for non-political relations. This chapter focuses on Dunn's strengthening relations with Soviet geneticists and his involvement with the National Council of American-Soviet Friendship, which resulted in his becoming president of the American-Soviet Science Society. For his activism on an international scale, Dunn would suffer consequences during the Cold War years, which will be discussed in the next chapter. This chapter also explores Dunn's efforts during the war on the behalf of scientists' in war torn countries, especially Soviet scientists.

Soviet Colleagues & the International Genetics Congresses

Dunn established relationships with foreign geneticists and a scientific reputation internationally not only through his correspondence and publications, but also through personal interaction with foreign geneticists. In 1927 while on sabbatical

leave from Storrs Agricultural Experiment Station, Dunn went to Europe and toured agricultural centers in Great Britain, Denmark, Norway, Germany, and the Soviet Union.¹ He also attended the Fifth International Congress of Genetics in Berlin, as did many Soviet geneticists. Since Dunn had already visited with geneticists in Britain and was about to go to Germany, he spent most of his time during the Congress speaking with Russian geneticists, in particular Aleksandr Serebrovsky, Nikolai Vavilov, Nikolai Kol'tzoff, and Georgii Karpechenko. Relationships between Americans and Russians blossomed, according to Dunn, who believed that three similarities explained why geneticists from these two countries got along so well. One, most of the Americans and Russians were between the ages of thirty and forty. Two, many Americans either agreed with the Soviet Union's political system or were at least sympathetic to the Soviet experiment. Three, geneticists in the two nations conducted analogous research:

More specifically, the same kind of genetics is being cultivated with the same enthusiasm in America & Russia – in both places a radically experimental post-Morganatic method prevails – and the latest premises are accepted. In other words, we speak the same genetics language.

Dunn and Serebrovsky immediately developed a friendship: “Serebrovsky is staying on in Dahlem for a time & we've become very good friends. He's anxious to come to

¹ APS, Dunn Papers, Series I, Box 9, L.C. Dunn-Report on Observations in Russia and Europe, 1927-1928, Report on Russia, 2 Nov 1927, 7-pages; Report on Great Britain, Denmark, Norway, and Germany, 26 Jan 1928, 8-pages.

America for a year if he can get a fellowship, a visum, etc.”² After returning to the United States, Dunn corresponded with Soviet geneticists and even accepted Anton Zhebrak as a graduate student at Columbia University in 1930. Kol'tsov of the Timiriazev Agricultural Academy in Moscow recommended Zhebrak to Dunn.³

Dunn valued the internationality of these Congresses, which can be seen through his participation on the organizing committee for the Sixth International Congress of Genetics, which was held in Ithaca in 1932. Dunn acted as transportation coordinator. The other eight Americans organizing the Congress were T.H. Morgan, E.M. East, Robert Cook, C.C. Little, Charles B. Davenport, R.A. Emerson, Milislav Demerec, and D.F. Jones.⁴ Over two years prior to the Congress in Ithaca, Dunn began asking American universities to host a visiting lecturer from a foreign country. He considered it an important gesture that promoted a “feeling of international cordiality,” gave American students access to new ideas, and provided money for foreigners traveling to Ithaca.⁵ Dunn had difficulties coordinating travel due to the economic depression and explained to German geneticist Hans Nachtsheim that the Congress took on new meaning in light of the financial crisis:

Changes which I suspect will be more or less permanent are occurring every day, and education and research are bound to suffer severely. Some of us thought this provided an additional reason for maintaining

² APS, Dunn Papers, Series I, Box 18, Walter Landauer, Dunn writing from Dahlem to Landauer, 23 Sep 1927.

³ Nikoali Kremmentsov, International Science between the World Wars: The Case of Genetics (London: Routledge, 2005): 32-33.

⁴ Kremmentsov, International, 36.

⁵ APS, Pearl Papers, B: P312, L.C. Dunn, 1930, Dunn to Pearl at Johns Hopkins University, 12 May 1930.

the continuity of international intellectual [sic] intercourse even in the face of bad conditions and influenced our decision to hold the Congress this summer.⁶

Postponing the Congress had been considered and rejected.⁷

The American committee invited about twenty Soviet geneticists to the Sixth International Congress of Genetics, and many had planned to attend but only Nikolai Vavilov did. Soviet geneticists, especially Vavilov, fought hard to get permission from the Soviet government to travel to New York. Historian Nikolai Kremmentsov thoroughly discusses the details of the interactions between Soviet geneticists and their domestic patrons, as part of his three-prong analysis of the International Congresses of Genetics in his recent book International Science between the World Wars: The Case of Genetics. In addition to Soviet geneticists and their domestic patrons, Kremmentsov's third prong is the Soviet's foreign peers. Kremmentsov often mentions Dunn as a member of a genetics network operating on an international level.⁸ Vavilov, who was Vice President of the Ithaca Congress, presented the International Organizing Committee with the proposal that the Soviets host the next Genetics Congress in Moscow, a proposal endorsed by the Soviet government. The

⁶ APS, Dunn Papers, Box 23, Hans Nachtsheim, 1932-1961, Dunn to Nachtsheim, 26 Mar 1932.

⁷ Kremmentsov, International, 36.

⁸ Kremmentsov, International, 2. This book looks at the International Congresses of Genetics from 1899 to 1937, paying particular attention to the events surrounding the cancellation of the Seventh International Congress of Genetics in Moscow, which was supposed to be in 1937. It was held in Edinburgh in 1939 instead. Kremmentsov is a historian of Soviet science and therefore focuses on Soviet geneticists. He frames his book in terms of their international networks between geneticists, which has provided me with perspective and useful information.

International Organizing Committee decided to have the next Congress in 1937, but did not choose a location.⁹

Due to the numerous controversies surrounding the Seventh International Congress of Genetics, several historians have analyzed it. The general information is that the International Organizing Committee eventually asked the Soviets to host the Seventh Congress in Moscow in 1937, and it was approved by the Soviet Politburo. The Soviets began planning for it and then, suddenly, the Politburo cancelled the Congress on 14 November 1936. The news reached geneticists outside of the Soviet Union in early December and was reported in the New York Times on 14 December. Geneticists from many countries wrote to their Soviet colleagues to find out more information, and soon the news from the Soviet Union changed. The Congress had not been cancelled, but postponed because Moscow was not ready to accommodate foreign geneticists. More correspondence between geneticists in the Soviet Union, United States, and European countries resulted and eventually a decision about the Seventh Congress had to be made. Would they wait for their Soviet colleagues to provide a new meeting date for the Congress? Or would they reschedule the Congress for a different year and location? The latter course of action was taken and the Seventh International Congress of Genetics was held in Edinburgh in 1939.

The above is only one of the controversial matters plaguing this Congress. Trofim Lysenko's agro-biology and his growing power concerned several geneticists inside and outside of the Soviet Union, which historians have related to the

⁹ Krementsov, International, 34-42.

cancellation. Also, about thirty geneticists from the United States proposed a discussion on the Nazis' eugenic practices (i.e. racial hygienics) in Moscow. This point has also been connected to the cancellation of the Congress by some historians, including Nils Roll-Hansen who points out that Vavilov told foreign geneticists that the Soviet Politburo forbade a discussion on race problems.¹⁰

Moreover, monumental international events occurred during the Seventh International Congress of Genetics, which was held in Edinburgh from 23 to 30 August 1939. On August 23rd, the first day of the Congress, Germany and the Soviet Union signed a Non-Aggression Pact and many German geneticists had to leave Edinburgh immediately. Geneticists from other countries chose to leave, especially those from Britain, Hungary, Switzerland, and Scandinavia. On the day that most attendees had planned to leave, August 30th, many ships leaving British ports were cancelled. The next day World War II officially started.¹¹

Dunn actively corresponded with geneticists throughout North America and Europe, including the Soviet Union, upon hearing about the cancellation of the International Genetics Congress in Moscow in early December 1936. Otto Mohr was the President of the International Organizing Committee for the Seventh Congress

¹⁰ Elof Axel Carlson, Genes, Radiation, and Society: The Life and Work of H.J. Muller (Ithaca: Cornell University Press, 1981): 225-28; Nils Roll-Hansen, The Lysenko Effect: The Politics of Science (New York: Humanity Books, 2005): 187, 204, 224-25, 237-38; Zhores, A. Medvedev, The Rise and Fall of T.D. Lysenko, trans. I. Michael Lerner (New York: Columbia University Press, 1969): 51-52.

¹¹ My overview is a summary based on: Krementsov, International, 42-72. Landauer and Dunn were among the geneticists supporting a discussion on eugenics and racial hygienics (45).

and therefore had to sort out the Congress's arrangements.¹² Mohr and Dunn were close personal friends, and Dunn had spent part of his 1934-1935 sabbatical leave visiting Mohr in Norway. In November 1935, shortly after Dunn left Oslo, Mohr finished arranging for the Congress to be held in Moscow and wrote to inform Dunn.¹³

News of the cancellation came from the American Hermann J. Muller, who was living in the Soviet Union at the time. He sent letters to Mohr and Dunn in early December 1936.¹⁴ The day after the New York Times reported the cancellation, Dunn acted. He sent a telegram to T.H. Morgan with news of the cancellation and the arrest of Nikolai Vavilov. Dunn asked Morgan to write the Soviet Ambassador in Washington D.C. requesting news about Vavilov and two other Soviet geneticists rumored to have been arrested. He spoke with Edwin G. Conklin, the President of the American Association for the Advancement of Science, who also sent a telegram to the Soviet Ambassador and agreed to write an official statement if the Ambassador corroborated the rumor. Dunn then contacted R.A. Emerson with the same request and also asked Emerson to speak with Secretary of Agriculture Henry Wallace about contacting the Ambassador. Dunn also knew of other scholars' actions.

Anthropologist Franz Boas sent a telegram directly to the Ambassador, and geneticist

¹² APS, Dunn Papers, Box 16, International Congress of Genetics, 1935-1937 and 1938-1939, many letters about the cancellation, postponement, and rescheduling of the Seventh International Congress of Genetics; APS, Dunn Papers, Box 22, Otto Mohr, 1937-1939; Krementsov, International, 42.

¹³ APS, Dunn Papers, Box 22, Otto Mohr, 1929-1936, Mohr to Dunn, 16 Nov 1935.

¹⁴ APS, Dunn Papers, Box 16, International Congress of Genetics, 1935-1937, Muller to Mohr, 8 Dec 1936 and Muller to Dunn, 9 Dec 1936.

Albert Blakeslee asked President John C. Merriam of the Carnegie Institute of Washington to do the same. Dunn had provided this information to Milislav Demerec, Secretary-Treasurer of the Genetics Society of America who immediately penned an official statement from the Society that incorporated the information from Dunn and reproduced the GSA's cable sent to the USSR Ambassador. Morgan and Emerson carried out Dunn's requests.¹⁵ Within days, Dunn had stirred several American colleagues into action in the hopes of not only finding out information, but also aiding Soviet geneticists.

Dunn's actions landed him on a committee appointed by the Genetics Society of America to find out information about events in and news from the Soviet Union. The two other members were H.S. Jennings of Johns Hopkins University, who acted as its chairman, and R.A. Emerson. Demerec told Dunn that the three men would update other geneticists at the Genetic Society of America's annual meeting in January. Their report summarized the events beginning with the 14 December 1936 New York Times article and reproduced the following resolution, which was written by Jennings, Dunn, and Emerson, and passed by the Society:

The Genetics Society of America records its regret that it appears impossible to hold the meeting of the Seventh International Congress of Genetics in the summer of 1937 as planned; and in view of the importance of maintaining a friendly contact among the geneticists of all countries through regular international congresses it urges that the

¹⁵ APS, Dunn Papers, Box 16, International Congress of Genetics, 1935-1937, Dunn to Demerec, 15 Dec 1936; Statement from Demerec as Secretary-Treasurer of the Genetics Society of America, 16 Dec 1936; Morgan to Dunn, 16 Dec 1936; R.A. Emerson to Dunn, 17 Dec 1936.

International Committee arrange for holding the Seventh International Congress at the earliest practicable time.¹⁶

Dunn told Muller of the plan to reschedule the Congress and hold it as soon as possible in a different country.¹⁷ Muller pushed for holding the Congress in the Soviet Union arguing that it would give support to Soviet geneticists. Lysenko, who is discussed in the next chapter, gained considerable power during the late 1930s having aligned himself and his ideas with Josef Stalin and the Soviet political system. Thinking like a Soviet politician, Muller told Dunn that relocating the Congress would be considered politically motivated, and using Soviet terminology he explained to Dunn one possible consequence:

The action would give Soviet government authorities the impression that geneticists abroad are anti-Soviet i.e. reactionary, bourgeois; and the natural inference from this in turn would be that “classical genetics” is a “bourgeois” science, and that Russian geneticists supporting the “classical genetics” of Western countries are deviating from the proper path of proletarian science. This might cause very grave injury to the developing science of genetics in the U.S.S.R.¹⁸

The decision to relocate the Seventh Congress to Edinburgh in 1939 took a long time, and was not finalized until September 1938. During 1937 and 1938, Mohr wrote letters to Dunn asking his opinion and advice about how to diplomatically handle the situation. “Ought we to go to Moscow in order to give support to the

¹⁶ APS, Dunn Papers, Box 16, International Congress of Genetics, 1935-1937, Dunn to Demerec, 15 Dec 1936; Statement from Demerec as Secretary-Treasurer of the Genetics Society of America, 15 Jan 1937.

¹⁷ APS, Dunn Papers, Box 16, International Congress of Genetics, 1935-1937, Dunn to Muller, 7 Jan 1937.

¹⁸ APS, Dunn Papers, Box 23, Hermann J. Muller, 1928-1937, Muller writing from Texas to Dunn, 17 Jun 1937.

decent geneticists who are in a critical situation?” asked Mohr, noting that “This argument carries for me a heavy weight.”¹⁹ Dunn wanted the Congress to be in USSR, but did not feel it right to sway others’ opinions: “Muller’s letter has some good reasoning in it but I’m afraid does not persuade me to active propaganda in favor of congress in U.S.S.R. For myself I should vote for it so, and should go, but to persuade others somehow seems to involve promising something and I haven’t faith enough to do that in this case.”²⁰ After hearing from Mohr that the Congress had been rescheduled for Edinburgh, Dunn expressed disappointment.

Dunn had planned to go to Edinburgh for the Seventh Congress, but could not because he was still recovering from a surgery earlier that summer.²¹ At the Congress, several attendees, including Muller, signed the Geneticists’ Manifesto, a statement about race and eugenics. The Geneticists’ Manifesto averred that eugenics overstepped biology’s territory by making claims about improving the human population. Its signers called for economic and social equality as a first step toward understanding human inequities and stated that nationalism and racism would have to

¹⁹ APS, Dunn Papers, Box 22, Otto Mohr, 1937-1939, Mohr to Dunn, 8 Apr 1937.

²⁰ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1937, Dunn to Landauer, 8 Jul 1937.

²¹ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1939, Dunn to Landauer, 28 May 1939. Dunn told Landauer that he was to have surgery in order “to get some muscles sewed up – repair of an inguinal hernia that will keep me off my feet for a month or more.” New York Public Library also has information about Dunn’s minor surgery. NYPL, Manuscripts and Archives, Emergency Committee in Aid of Displaced Scholars, Box 156-Executive Committee Records, Drury to Miss Davis, Secretary to Dunn, 19 Jun 1939, Duggan to Dunn, 22 Jun 1939, and Dunn to Duggan, 10 Jul 1939. Dunn said in his July letter that he had recovered.

be eradicated before eugenic measures could be implemented fairly.²² Had Dunn attended the Congress, he most likely would have signed it, too.

Origins & Goals of the American-Soviet Science Society

After the United States entered World War II, American scientists concerned with defeating fascism mobilized on an international level by cooperating with scientists from other Allied nations. Scientists considered internationalism to be an important aspect of science, and it was a major feature of scientific relations in the years prior to the war. During the war American security became an important matter for the United States military and government, as did confidentiality about scientific investigations contributing to winning the war.²³ These two developments, restricted internationalism and increased secrecy, bothering many American scientists and prompting them to act. Some responded in 1943 by starting the American-Soviet Science Society, which improved international cooperation through an increased exchange of scientific information between the United States and Soviet Union.

American views about the Soviet Union fluctuated greatly during the 1930s and early 1940s. The New Deal and labor movements had a strong following in the United States until the late 1930s. Known as the Popular Front, supporters of the New

²² Ruth Benedict, Race: Science and Politics, revised edition (1940; New York: The Viking Press, 1947): 198-99; Peter J. Kuznick, Beyond the Laboratory: Scientists as Political Activists in 1930s America (Chicago: University of Chicago Press, 1987): 205.

²³ Daniel S. Greenberg, The Politics of Pure Science (New York: The New American Library, Inc., 1967): 68-96.

Deal and labor movements were closely associated with the American Communist Party. Conservatives used anti-communist rhetoric to garner support in opposition to the Popular Front and by 1938 the United States had become more conservative. The Non-aggression Pact of 1939 between Nazi Germany and Soviet Union hurt the already ailing Popular Front and strengthened Americans' anti-communist views. For the next two years a period referred to by scholars as "the little red scare" witnessed pro-communist accusations against the United States government and a dramatic decrease of membership in the American Communist Party. Germany invaded the Soviet Union in 1941 causing the Nazi-Soviet Pact to disintegrate and soon thereafter the United States entered the war. Even though the United States and Soviet Union joined forces in the war against Germany and the other Axis countries, many Americans remained skeptical about the Soviet Union.²⁴

The American-Soviet Science Society's members took advantage of newly-opened lines of communication with the Soviet Union when they started their organization. The Nazi-Soviet Pact of 1939 disrupted the communication that Americans had established with Soviets during the 1920s and 1930s. Soviet scientists had proven their competence in many scientific disciplines and American scientists had fostered relations with Soviets during the two decades prior to World War II. The breakdown of the Non-aggression Pact between Germany and the Soviet Union

²⁴ M.J. Heale, American Anticommunism: Combating the Enemy Within, 1830-1970 (Baltimore: The Johns Hopkins University Press, 1990): 113-129.

allowed for the Grand Alliance of 1941, which in principle reopened communication between Great Britain, United States, and Soviet Union.

The Grand Alliance united countries fighting against a common enemy, Nazi Germany and the Axis powers. It was not based on similar visions for a post-war policy and many Americans distrusted the Soviets. President Franklin D. Roosevelt and some of his close advisors sought cooperative relations with the Soviet Union in fighting the war and formulating a peaceful post-war era. Some of his other advisors from the Department of State and the military, however, advised against basing Soviet diplomacy on cooperation because they did not trust Stalin. They thought that eventually a conflict would develop between the United States and Soviet Union, and therefore advocated limited sharing of information.²⁵

Historian E. H. Beardsley has analyzed the dynamic relationship of scientific exchange between these three Allied countries. After the USSR joined the Allied forces, each country's government continually evaluated and revised their degree of interchange with the other nations. For the most part, from 1942 to 1945, relations between British and Soviet scientists had fewer obstacles and tensions than did the American-Soviet partnership. Both countries had signed pacts with the Soviet Union in which they agreed to share information about any weapons and devices that would aid the defeat of Nazi Germany. The British tried to get the Americans to enter into a technology exchange with the Soviets, but the United States government resisted.

²⁵ Edward M. Bennett, Franklin D. Roosevelt and the Search for Victory: American-Soviet Relations, 1939-1945 (Delaware: S.R. Books, 1990): 42-43, 84, 88.

Eventually lines of communication reopened between the Soviet Union and United States. The official turning point came mid-year 1944, after it was estimated that the war with Germany would most likely end within one year, but that the Pacific war would not end for another three years. Realizing that Great Britain would focus on reconstruction and not continue to fight a war with Japan, the United States recognized its need for the Soviet Union's aid after Germany was defeated. This estimation of events stimulated the American government to increasingly restore scientific communications with the Soviet Union.²⁶

A conscious effort was made during 1942 and 1943 to improve Americans' feeling toward the Soviet Union and to promote cooperation with Soviets.²⁷ For example, several American citizens, who were engaged in governmental and military work, sought better and more open communication with the Soviet Union. In November 1942, about 2,000 Americans congregated at Madison Square Garden for a two-day Congress of American-Soviet Friendship that presented information on the Soviet Union's past achievements in science and medicine, as well as by their military forces during the war.²⁸ Vice-President Henry Wallace expressed admiration for his countrymen's mobilization to aid their new ally: "This meeting demonstrates just one thing – the desire and the determination of the American people to help

²⁶ E.H. Beardsley, "Secrets between Friends: Applied Science Exchange between the Western Allies and the Soviet Union during World War II," Social Studies of Science 7 (1977): 447-73.

²⁷ Heale, 130.

²⁸ "Notes on Science-A Tribute to the Russians," New York Times (25 Oct 1942): E9.

Russia and help her now.”²⁹ Other politicians in attendance were New York Mayor Fiorello La Guardia, New York Governor Herbert Lehman, and Senator Claude Pepper of Florida. Lieutenant General Dwight D. Eisenhower was traveling to Africa and sent a telegram that was read at the Congress. He applauded the Soviet army’s capabilities and reassured Americans that the Allies would win the war. Former Ambassador to the Soviet Union Joseph E. Davies, who acted as honorary chairman at the November rally, looked to the future and optimistically remarked on American-Soviet affairs. He hoped for peaceful international relations for years to come, and urged Americans to learn about and cooperate with the Soviet Union.³⁰ Lieutenant General L.J. McNair of the US Army Ground Forces spoke at the rally and afterwards sent a congratulatory letter to its chairman Corliss Lamont remarking: “There is no question that untold good resulted from the Congress, and you have performed a public service which deserves national and international recognition.”³¹ The politicians and military men who contributed to the rally believed that the war could be won through complete mobilization of the Allied Forces and supported the intent of the Congress to improve Americans’ understanding of Soviets.

²⁹ NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 1, Folder 2, Congress of the American-Soviet Friendship, 1942, “Tribute to Russia,” speech by Vice-President Henry Wallace.

³⁰ “Full Unity Urged with the Russians,” *New York Times* (8 Nov 1942): 36.

³¹ NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 1, Folder 2, Congress of the American-Soviet Friendship, 1942, L.J. McNair, Lt. Gen., wrote from the Headquarters of the Army Ground Forces, Office of the Commanding General, in Washington, D.C. to Corliss Lamont, 23 Nov 1942.

As a direct result of the Congress in Madison Square Garden, Americans started the National Council of American-Soviet Friendship, an organization geared to facilitating a cooperative relationship between the two countries.³² The Friendship Council outlined three aims, the second of which reflects the role that World War II had on the organization's creation.

- a) To strengthen friendly relations between the United States and Union of Soviet Socialist Republics through the promotion of better understanding between them.
- b) To educate the American people to the need for such better understanding and friendly relations between the United States and the Union of Soviet Socialist Republics as essential to victory in the present war against the Axis Powers and to the establishment of world-wide democracy and enduring peace.
- c) To act as a coordinating body and headquarters for organizations and groups of individuals subscribing to the above purposes, and to arrange for the affiliations of such groups.³³

The Friendship Council subsequently established cultural committees in areas such as art, dance, and architecture. The two main scientific groups were the American-Soviet Medical Society and the American-Soviet Science Society.³⁴

The National Council of American-Soviet Friendship held a three-day Congress in New York in November 1943, which focused on issues pertinent to

³² NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 1, Folder 7, Chronology: NCASF Activities, n.d., 7-page list of events covering 1942 to 1953.

³³ APS, Dunn Papers, Series I, Box 1, American-Soviet Friendship Council Executive Board Minutes, 1944-1948, "Revised By-Laws, National Council of American-Soviet Friendship, Inc., March 15, 1944," page 1.

³⁴ Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs of the National Academy of Sciences 49 (1978): 79-104, 86; David B. Buehrens, "The US-Soviet Encounter: An Interview with Richard Morford, NCASF," New World Review 40 (1972):66-77, 66-67.

wartime relations between the two nations. The journal Science and the New York Times announced the upcoming conference and some of the prominent speakers scheduled. New York's Mayor Fiorello La Guardia spoke at the rally along with Secretary of the Interior Harold L. Ickes, who was an active sponsor of the Friendship Council.³⁵ Several scientists also delivered talks, including Dunn, and during the Congress Harry Grundfest suggested starting a scientific committee, as had been discussed by some scientists prior to the Congress.³⁶ Harry Grundfest, who was an active member of the American-Soviet Science Society and American Association of Scientific Workers, was born in Minsk, Russia and moved with his family to the United States before turning ten years old.³⁷ Those present voted in affirmation of his resolution and thus began the Science Committee of the National Council of

³⁵ "The American-Soviet Scientific Congress," Science 98 (22 Oct 1943): 359; "American-Soviet Conference," Science 98 (5 Nov 1943): 403; "3-Day Congress to Honor Soviet," New York Times (6 Nov 1943): 4; "Events Today: Conference, National Council of American-Soviet Friendship," New York Times (6 Nov 1943): 11; NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 5, Folder 1, Alice Barrows, Executive Secretary of the NCASF to Robert S. Lynd, 16 Feb 1943.

³⁶ HUA, Shapley Papers, HUG 4773.10, Box 29B, Papers, c. 1921-1965, American-Soviet Friendship- Science Committee of the Congress, Edwin S. Smith, Executive Director of NCASF to Shapley, 28 Sept 1943; NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 1, Folder 26, Executive Board Minutes, 1943, Minutes of Meeting of the Congress Committee [of NCASF] held August 26, 1943. Grundfest suggested that there be a scientists' session during the Congress chaired by prominent scientists. The three scientists that he proposed, Walter B. Cannon, G.N. Lewis, and Ernest O. Lawrence, had been elected to the Academy of Sciences in the USSR in 1942 and agreed to act as honorary chairmen. See: "The American-Soviet Scientific Congress," Science 98 (22 Oct 1943): 359; Nikolai Kremontsov, Stalinist Science (New Jersey: Princeton University Press, 1997): 115.

³⁷ John P. Reuben, "Harry Grundfest, January 10, 1904-October 10, 1983," Biographical Memoirs of the National Academy of Sciences 66 (Washington, D.C.: National Academy Press, 1995): 151-66; 152.

American-Soviet Friendship, which developed into the American-Soviet Science Society one-and-one-half years later.³⁸

The American-Soviet Science Society had two main purposes. First, its members desired to resume and strengthen the scientific exchange interrupted by the war. Second, they wanted to cultivate a relationship between the two countries that was “not affected by political, economic, and social differences.”³⁹ Indeed, Science Society members consciously kept the organization free of politics by focusing on science. Dunn headed the Science Committee from its inception to May 1946 and remained pivotal to its actions even after he resigned the chairmanship in 1946.⁴⁰ He outlined the group’s goals, which demonstrate the organizers’ desire to promote cooperation between the two countries.

It is the objective of this committee to foster and promote closer relations, cooperation and exchange between American and Soviet scientists, to facilitate translating and abstracting of scientific literature and to arrange exchange visits of scientific professors and students.

Scientists who had not spoken at the Congress were invited to share their sentiments about the renewal of the American-Soviet relationship. Grundfest expressed an

³⁸ “Life is Restored to Dead Animals,” New York Times (8 Nov 1943): 21, 32. Resolution to start the Science Committee is on page 32.

³⁹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-46, n.d., “American-Soviet Science Society, The Constitution,” no date. Their membership pamphlets also had this information.

⁴⁰ According to an obituary notice about Cannon, he was the first Chairman of the Science Committee, a post he held at the November 1943 conference. MCZ, Ernst Mayr Library, “Walter B. Cannon,” American-Soviet Science Society’s Science Bulletin I, 5 (Oct 1945): 1.

attitude held by others in support of the Friendship Council when he called for a peaceful association with the Soviet Union after the war.⁴¹

At the Congress in November 1943, Dunn had requested that biologists facilitate the exchange of information with the Soviet Union, rather than continue to allow political ideologies to interrupt the internationalism of science. As discussed above, he had a personal affinity for the Soviet Union since visiting the Institute of Experimental Biology in Moscow in 1927 and starting regular correspondence with Soviet geneticists. During that visit, he became enamored with the Soviet Union's "youth and originality," which he found created a refreshing approach to scientific investigations. He also liked the country's close ties between science and society, and expressed the view to his listeners that science thrives under socialism in the Soviet Union.⁴²

Dunn drew from his visit of 1927 during his talk at the Congress and focused on one main point, which related to the goals of the Friendship Council's meeting. He hoped for international cooperation through greater collaboration between the two countries now that communication had been re-established. In reference to his own discipline, he stressed the importance of keeping Soviet geneticists informed of current developments because in his opinion the most important genetic contributions to date had been from the United States and Soviet Union. He specifically requested

⁴¹ L.C. Dunn "Foreword," Science in Soviet Russia (New York: Arno Press, 1975): iii-iv, iv. Grundfest's statement is on pages 96-97 of the same book.

⁴² Theodosius Dobzhansky, "Leslie Clarence Dunn (1893-1974)," Yearbook American Philosophical Society (1974): 150-56, 154; Dobzhansky, 1978: 79-104.

copies of journals and new stocks of *Drosophila* to replace those destroyed by the Nazis. According to Dunn, the Soviet Union and United States had too many common scientific interests and goals to not collaborate.

For the sake of biological science itself, we biologists should use all our efforts to see that the barriers which separated Soviet biology and biologists from us should never again be allowed to prevent the free flow of persons and ideas, both scientific and social, on which the progress of science and of society depends.

Dunn circulated these statements to a larger audience by publishing his lecture in Science in January 1944.⁴³

The Science Society's Membership and Achievements

During 1944 and early 1945, the Science Committee built itself into a fairly well-established organization. As mentioned, Dunn assumed the chairmanship. K.A.C. Elliott and Harry Grundfest of the American Association of Scientific Workers were members of the Executive Committee along with over twenty others. All but five of these officers and Executive Committee members resided in the northeastern United States. Although it was an American and Soviet organization, the general membership consisted primarily of American scientists and a few Canadians.⁴⁴ The Science Committee successfully established communication

⁴³ L.C. Dunn, "Soviet Biology," Science 99 (28 Jan 1944): 65-67. The first and last quotations are from page 67 and the second is from 66.

⁴⁴ Of the thirty officers and executive committee members, ten lived in New York, six in New Jersey, three in Pennsylvania, two in Maryland, and one in each of the following Washington, D.C., Connecticut, Massachusetts, New Hampshire, Alabama, Texas, Missouri, California, and eastern Canada. APS, Demerec Papers, National

between American and Soviet scientists by contacting governmental agencies and individual scientists in the Soviet Union, but there was no counterpart group in the Soviet Union.

A lack of funds continually plagued the American-Soviet Science Society. In mid-1944 Dunn asked Alan Gregg of the Rockefeller Foundation about the possibility of receiving financial aid from the foundation. Gregg's reply dissuaded Dunn from submitting a grant application.⁴⁵ At their January 1945 executive committee meeting the officers instated a three-dollar annual membership fee to cover some of the operating costs.⁴⁶ Over the next year and a half, they continued their search for funding. Alfred E. Mirsky of the Rockefeller Institute and Chairman of the Finance Committee unsuccessfully sought contributions from philanthropic groups and targeted industrial companies next.⁴⁷ Their attempts over the next year procured only minimal donations, which Samuel Gelfan of Columbia University's College of Physicians and Surgeons and their new financial officer, noted would not sustain the

Council of American-Soviet Friendship, Inc., Science Committee (Folder 1), "Members of Executive Committee of Science Committee of National Council of American-Soviet Friendship, Inc., 5 July 1944. A list of the names of these officers and executive committee members is also available in: L.C. Dunn, "Scientific Interchange between the United States and Soviet Russia," *Science* 101 (23 Feb 1945): 200-01.

⁴⁵ APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 1), Dunn to Members of the Science Committee, 5 July 1944.

⁴⁶ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d. "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship, January 17, 1945:" 4 pages, 3.

⁴⁷ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 12 March [1945]: 4 pages, 3.

Society. According to Gelfan, their three-dollar membership dues did not cover their minimum budget and they had to secure a stable financial backer. Gelfan noted that with 1000 members each paying ten-dollars in fees per year, they would be solvent.⁴⁸ The Science Society had significantly fewer than 1000 members at that time, only 166. By their next meeting, two months later, they had increased their membership considerably with the addition of 54 people, bringing the total membership to 220.⁴⁹

The acquisition of new members demonstrates that the Science Society offered a service desired by scientists. Their difficulty in finding monetary backers reflects the public's attitude towards the Soviet Union. Dunn recognized this dilemma:

The Russian business booms, except the most important part, i.e. exchange of persons. That will not progress until there is less suspicion on both sides & there won't be less suspicions until Russians & Americans know each other personally – a kind of impasse which can be loosened a little bit but not overcome.⁵⁰

Three months prior to this statement, Dunn had written to A.M. Frumkin of the Soviet Union's Society for Cultural Relations with Foreign Countries (VOKS) inviting Soviet scientists to visit the United States. He hoped for extended exchanges of six

⁴⁸ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the American-Soviet Science Society," 19 Feb 1946: 3 pages, 2. The Science Society approached the National Research Council for financial assistance, too: APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-1946, n.d., Zlotowski to Dunn, 26 Oct 1945 and Dunn's reply, 2 Nov 1945.

⁴⁹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the American-Soviet Science Society," 25 April 1946: 3 pages, 1.

⁵⁰ APS, Dunn Papers, Series I, Box 19, Walter Landauer 1945, Dunn to Landauer, Sept 1945.

months to one-year, and mentioned the benefits for both nations. Each country could learn more about the other, scientists could share their knowledge, and together they could build better relations between the two countries.⁵¹ The Science Society successfully established communication and document exchanges with the Soviet Union, but never achieved their goal for exchanges of scientific personnel.

Increasing membership caused the Executive Committee to consider disassociating from the Friendship Council in spring 1945, at which time they chose to become the American-Soviet Science Society for functional reasons. They thought that the organization would have more flexibility as a Society. Dunn noted that the organization would be more successful in soliciting money for operating expenses if they disassociated from the Friendship Council and its political purposes. Initially, the Science Society's members neither physically moved their office away from the Friendship Council's, nor broke ties with the parent group. They, instead, consciously chose to cultivate their relationship with the Friendship Council even though they had become an independent association.⁵²

The Science Society increasingly made it a point to focus their work on scientific discussions and to keep their efforts free of politics. Political statements made by members of the National Council of American-Soviet Friendship convinced members of the Science Society to completely disassociate their organization from

⁵¹ APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, Dunn to Frumkin, 5 June 1945.

⁵² APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 1 May 1945: 4 pages, 1-2.

the Friendship Council.⁵³ Dunn notified Richard Morford, Executive Director of the Friendship Council that the Science Society's disassociation was to be effective 1 April 1946.⁵⁴ There is no clear statement in the Science Society's records explaining the exact nature of the Friendship Council's political actions.⁵⁵ However, Karl T. Compton, President of Massachusetts Institute of Technology, resigned from the Friendship Council because of "the general trend toward becoming a political pressure group, both national and international." Compton believed that the Soviet Union had spies in the United States and disliked the fact that the Friendship Council chose to deny Soviet espionage and to attack the American agencies trying to prevent infiltration.⁵⁶ After mid-1946 several people resigned from the Friendship Council, most likely in response not only to changes in the Friendship Council's motives but also the rise of anti-communism in the United States.⁵⁷ Langmuir wanted to stay a

⁵³ The American-Soviet Science Society gave no explicit explanation for their disassociation from the Friendship Council. APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 25 Apr 1946: 3 pages, 1.

⁵⁴ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-46, n.d., Dunn to Morford, no date.

⁵⁵ The Rockefeller Foundation's resolution to give the American-Soviet Science Society a grant for \$25,000 mentioned that political issues prompted the Science Society's members to distance their organization from the National Council of American-Soviet Friendship because "it was feared that connection with the National Council involved political flavor." RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Rockefeller Foundation Resolution 46100, 21 Jun 1946.

⁵⁶ NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 5, Folder 2, Compton to Executive Director of NCASF, 22 March 1946.

⁵⁷ NYU, Tamiment Library, National Council of American-Soviet Friendship, Box 5 Folder 2, Sponsors and Members Resignations, 1946-1955, various letters. Karl T.

member in order to receive the Friendship Council's publications but, like Compton, asked for his name to be taken off the letterhead as a "sponsor." Langmuir thought that the Soviet Union had become confrontational and aggressive and therefore the United States had to react with conviction, whereas the Friendship Council in Langmuir's opinion favored appeasing the Soviet Union. Dunn resigned from the Friendship Council's Board of Directors by 1947, but kept his membership in the organization until 1950. Dunn did not feel that the Council's intentions were problematic politically and greatly appreciated their role in fostering the American-Soviet Science Society.⁵⁸

Members of the Executive Committee of the Science Society were conscious that problems would result if the organization admitted members with motives incompatible with the Society's intentions. The Science Society, therefore, restricted its membership to scientists, technologists, and scientific teachers and students as a precaution against infiltration. The Executive Committee required that each potential

Compton no longer wanted to be a "sponsor" as of 22 March 1946, but remained supportive of the Friendship Council's original aspirations (Karl T. Compton to Executive Director of NCASF, 22 March 1946). Harold Ickes resigned on 18 Oct 1946 and Irving Langmuir on 31 March 1947 (Langmuir to Rev. William Howard Melish, NCASF, 31 March 1947).

⁵⁸ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 12 Feb 1947. Dunn told Weaver: "I do not believe the Council's purposes are political or propagandistic in the bad sense." Dunn waited until March 1950 to resign from the National Council of American-Soviet Friendship. Prior to then, he resigned from its Board and told Condon that he "as a member of the Executive Board of the Council, saw no reason for suspecting it of political machinations. I eventually disagreed with the shift in emphasis of the Council from cultural relations to educational campaigns on more immediate issues." (APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Dunn to Condon, n.d.

member be sponsored by two existing members. If a potential member did not know any existing members, then they would be referred to the Membership Committee.⁵⁹

One of the American-Soviet Science Society's greatest accomplishments during its short, five-year duration was assisting post-war reconstruction of American-Soviet cooperation. Establishing a reciprocal exchange of written documents was one aspect of this success. Dunn reported to members in July 1944 that they had received journals from the Soviet Union through their Academy of Sciences and the Society for Cultural Relations with Foreign Countries (VOKS). They had also received several manuscripts by Soviet scientists for submission to American journals.⁶⁰ Natalie Dobzhansky, Theodosius Dobzhansky's wife and one of the Science Committee's coordinators of publications, reported in January 1945 that they had received 113 books and manuscripts from the Soviet Union to date and that American scientific journals had accepted fifteen of the manuscripts for publication. After the war ended the number of periodicals received by the Science Society increased significantly as did communication with Soviet scientists.⁶¹ Historian

⁵⁹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 1 May 1945, 4 pages, 2; APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., Constitution of the American-Soviet Science Society, no date, 4 pages, 1.

⁶⁰ APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 1), Dunn to Members of the Science Committee, 5 July 1944. Enclosed with this letter was the membership list.

⁶¹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d. "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 17 Jan 1945, 4 pages, 1; "Minutes of the Meeting of

Nikolai Kremmentsov has noted the significant increase of communication between Soviet and American scientists in 1943 and mentions the role that VOKS played in the Soviet Union to mediate information.⁶² As a Russian historian, Kremmentsov provides a thorough analysis by focusing on the information about the Soviet Union. He does not mention, however, the American-Soviet Science Society's vital role as the body coordinating communications in the United States.

After attending to the administrative issues, the Science Committee started actively collecting documents to be sent to the Soviet Union. Dunn appealed to the American scientific community through another article in Science in which he asked scientists to donate published materials (such as articles, journals, and textbooks) to be shipped to their Soviet colleagues. This article published in early 1945 noted the great need for recent publications dating from 1941. "It must be remembered," Dunn wrote, "that not only was normal communication interrupted, but many libraries in western Russia have been destroyed, and the scientific collections must be replaced."⁶³ Public solicitations, such as Dunn's, successfully gathered books, reprints, and journal copies for shipment.⁶⁴ The US State Department started

the Science Committee of the National Council of American-Soviet Friendship," 13 Nov 1945, 5 pages, 1-2.

⁶² Kremmentsov, 1997, 115.

⁶³ Dunn, "Scientific Interchange," 200-01.

⁶⁴ The Science Society solicited their members' help during its 1946 book campaign. As mentioned previously, Dunn wrote two articles in Science in which he requested literature for scientists. Dunn, "Soviet Biology," 66-67; Dunn, "Scientific Interchange," 200-01; APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-1946, n.d., Marvin E. Oakes of ASSS, Chairman of Book Campaign to ASSS members, 21 May 1946; APS, Dunn Papers, Series I, Box

transferring books to European countries by March 1945, at which point the Science Society utilized this service to offset some of their expenditures. Although the State Department provided for shipping books, the Science Society had to find a different means for transporting journals, reprints, and other materials.⁶⁵ Fortunately, the International Exchange Service of the Smithsonian Institute and the consulate in New York provided these other services.⁶⁶

The Science Society's poor financial situation impinged on their efficiency, yet they found creative solutions for exchanging information.⁶⁷ Members enlisted and accepted help to accomplish the duties at hand because without money or a secretary tasks piled up.⁶⁸ Milislav Demerec informed Dunn that the Carnegie Institution of Washington would cooperate with them by sending copies of publications from their Institution to libraries in Europe, including the Soviet Union.⁶⁹ Demerec worked at

25, U.S.S.R. Correspondence with Geneticists, 1944-1945, Dunn to Gershenson, 27 July 1945.

⁶⁵ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 12 March [1945]: 4 pages, 1

⁶⁶ APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, Dunn to Gershenson, 27 July 1945 and Annette Terzian, Dunn's Secretary to H.W. Dorsey of Smithsonian Exchange Service, 12 Nov 1945. Terzian sent Dorsey two packages with reprints to be sent to USSR.

⁶⁷ The National Council of American-Soviet Friendship gave the Science Committee \$10,000 per year for three years. These three years spanned from 1943 to 1946, but there are not exact dates. RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Rockefeller Foundation resolution to give the grant to American-Soviet Science Society, 21 June 1946.

⁶⁸ APS, Dunn Papers, Series I, Box 19, Walter Landauer 1944, Dunn to Landauer, 13 Oct 1944.

⁶⁹ APS, Demerec Papers, National Council of American-Soviet Friendship, Inc. Science Committee (Folder 1), Demerec to Dunn, 2 March 1945.

the Carnegie Institution's Department of Genetics at Cold Spring Harbor, which had a close association with the Zoology Department at Columbia University.⁷⁰ Other expenses involved hiring translators. The Science Society arranged to sidestep one cost associated with publishing translations. The two countries discussed not assessing a copyright fee, but instead employing a barter system based on the understanding that scientists in both nations would translate books from the other country. Dunn noted that Soviets preferred a system based on equal trade, rather than selling rights because their country did not have copyright laws. However, the Soviet government and American State Department had not established an official policy by November 1945.⁷¹

In order to circulate information received from the Soviet Union, the Science Society produced a Science Bulletin. They published ten volumes only and at random intervals, dated between early 1945 and September 1946.⁷² The Science Bulletin had an extensive bibliography of the Soviet publications available from the Science Society upon request, and articles about scientific happenings in the Soviet Union.

⁷⁰ APS, Demerec Papers, Columbia University, Folders 1-3, Columbia University's Secretary to Demerec, yearly renewals. Demerec held a non-salaried position with Columbia University as an Associate in Genetics from 1942 to 1964.

⁷¹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 1 May 1945, 4 pages, 4 and 13 Nov 1945, 5 pages, 2; APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-1946, n.d., Dunn to James S. Thompson, President of McGraw-Hill Book Company, 13 August 1945.

⁷² MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 1 (no date): 11 pages. The second volume was dated March 1945. APS, Dunn Papers, Series I, Box 24, Science Bulletin 1945-1946, Science Bulletin, I, 10 (Sep 1946): 15 pages.

The articles contain a wide range of content. For example, the Science Bulletin excerpted Soviet physicist Peter Kapitsa's report to the USSR Academy of Sciences (given on 18 May 1943). Kapitsa, Director of the Institute of Physical Science, worked closely with party officials after the mid-1930s, which helped him obtain a prominent position in the Soviet Union's atomic bomb project that started in 1945.⁷³ The Science Bulletin also announced biologist Vladimir Komarov's resignation as President of the Academy of Sciences and physicist Sergei Vavilov's appointment to its Presidency in 1945. Komarov died at the age of seventy-five shortly after resigning from the presidency of the Academy of Sciences; the Science Bulletin published an obituary.⁷⁴ There were also obituaries for two American scientists, Walter B. Cannon and Gilbert N. Lewis, honoring their attempts at building cooperative relations with Soviet scientists. Both men were Honorary Chairmen of the American-Soviet Science Society and members of the USSR Academy of Sciences.⁷⁵

Announcements about new scientific techniques and findings on an eclectic mix of topics were a frequent feature of the Science Bulletin's latter volumes. Early volumes tended to discuss non-technical scientific matters, such as medal recipients

⁷³ MCZ, Ernst Mayr Library, "Kapitsa on Science in the USSR," American-Soviet Science Society's Bulletin, I, 2 (Mar 1945): 6 pages, 3-4; Kremontsov, 275-79.

⁷⁴ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin, I, 5 (Oct 1945): 32 pages, 14-15 and Science Bulletin, I, 6 (Jan 1946): 37 pages, 2-5.

⁷⁵ Walter B. Cannon's obituary: MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 5 (Oct 1945): 32 pages, 1. G.N. Lewis died 23 March 1946 and his obituary appeared in Science Bulletin I, 8 (May 1946): 17 pages; Kremontsov, 115.

and job promotions. The Bach Institute of Biochemistry of the Academy of Sciences reported investigations conducted on plant enzymes, muscle and respiratory enzymes, vitamins, and the structure of proteins.⁷⁶ In other volumes they carried articles on cosmic rays, paleontology,⁷⁷ astronomy, human eyes,⁷⁸ and archeology. The last volume gave a detailed description of a Meniscus telescope used for astronomical observations.⁷⁹ Articles discussing topics too technical for the Science Bulletin were submitted to scientific journals.⁸⁰

The Science Society received numerous articles from Soviet scientists hoping to be published in American journals. The publications slated for American journals were typically written in Russian or poor English. Before submitting a publication to a journal, the Science Society found someone to translate it, proof-read it, and then gave it to a scientist who specialized on the subject covered in the article. If the

⁷⁶ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 8 (May 1946): 18 pages, 1-2.

⁷⁷ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 8 (May 1946): 18 pages: "Cosmic Rays Studies in Pamirs," 3-4 and "Paleontological Discoveries," 4.

⁷⁸ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 9 (Jun 1946): 15 pages: "Soviet Astronomy During Recent Years," 1-3 and "The Light Sensitivity of the Eye," 3-4.

⁷⁹ APS, Dunn Papers, Series I, Box 24, Science Bulletin, 1945-1946, American-Soviet Science Society's Science Bulletin I, 10 (Sep 1946): 15 pages: "Achievements of Archeological Investigations in the U.S.S.R.," 3-7 and "A New Type Astronomical Telescope," 7.

⁸⁰ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 13 Nov 1945, 5 pages, 2.

Russian manuscript was acceptable, then it would be sent to an American journal.⁸¹

Thus, the process for publishing Soviet manuscripts in American journals consumed much time from those committed to the task.

The Science Bulletin gave an extensive discussion of the Soviet Academy of Sciences' celebration for its 220th Anniversary. The Science Society and event attendees thought that the Anniversary celebration signified an important step in scientific cooperation between the two countries. In June 1945 fourteen American scientists, many of whom were members of the American-Soviet Science Society, flew to the Soviet Union for a one-month trip. Among the delegation were astronomer Harlow Shapley, chemist Irving Langmuir, biophysicist Detlev Bronk, and physical chemist Duncan MacInnes.⁸² MacInnes of the Rockefeller Institute for Medical Research represented the American-Soviet Science Society during the delegation's visit.⁸³ Dunn had declined the invitation⁸⁴ and hoped that E.U. Condon would go as a

⁸¹ Early in the Society's existence, its officers discussed whether or not they wanted to take on the task of getting Russian articles into American journals. Dobzhansky argued that Soviet scientists needed to publish abroad and Dunn mentioned ways to improve the manuscripts so that they would be more appealing for American journals. The Science Society chose to "not discourage the sending of manuscripts" and elected to translate and edit the Soviet articles. APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder #1), "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 29 Nov 1944, 5 pages, 3.

⁸² Dunn declined the invitation to visit the Soviet Union for the 220th Anniversary of the Soviet Academy of Sciences. APS, Dunn Papers, Series I, Box 4, Columbia University – F.D. Fackenthal 1929-1945, Dunn to Frank D. Fackenthal, 24 May 1945.

⁸³ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin: "MacInnes: Close Cooperation between Pure and Applied Science," Science Bulletin I, 5 (Oct 1945): 8-9, 8.

representative of the Society.⁸⁵ Condon had planned to go to Moscow for the 200th Anniversary jubilee, but General Leslie Groves, coordinator of the Manhattan Project, stopped him from attending at the last minute.⁸⁶ While in the Soviet Union, the American delegates attended lectures and went on guided tours of laboratories, museums, and Soviet neighborhoods. Most of the Americans' time in the Soviet Union seemed to be planned and structured. Langmuir had been told that he could not tour Moscow by himself, which he disregarded by leaving one of the meetings and spending about six hours walking around Moscow.⁸⁷ The Soviets paid the foreign delegates' expenses during their stay and treated them extravagantly.⁸⁸

The Science Society hosted a dinner reception on 21 August 1945 in honor of the delegates' return, which it viewed as a major accomplishment. Three hundred people attended filling the Columbia University Men's Faculty Club to capacity.

⁸⁴ APS, Dunn Papers, Series I, Box 4, Columbia University – F.D. Fackenthal, 1929-1945, Dunn to Fackenthal, 24 May 1945.

⁸⁵ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-1946, n.d., Dunn to Hon. Anatol V. Gromov of USSR Embassy in Washington D.C., 24 May 1945.

⁸⁶ Thomas C. Lassman, "Government Science in Postwar America: Henry A. Wallace, Edward U. Condon, and the Transformation of the National Bureau of Standards, 1945-1951," *Isis* 96 (Mar 2005): 25-51, 34-35; Lassman's statements are corroborated by James Walter, who stated that Condon asked to go to Russia for 220th Anniversary, but was told he could not go. The Army asked that he not be allowed to go and Walter presumed that Leslie Groves had some part in that decision (RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, James Walter, "House Unit to Quiz Dr. Condon on Reds' A-Bomb 'Know-How,'" *Times Herald* Washington D.C. Thursday (no date), attached to letters dated July 1947).

⁸⁷ MCZ, Ernst Mayr Library, American-Soviet Science Society's *Science Bulletin*: Irving Langmuir, "Soviet Government Give High Priority to Pure Science," *Science Bulletin* I, 5 (Oct 1945): 3-4, 3.

⁸⁸ Krementsov, 1997, 116.

Members of the Science Society considered the reception an “indication of the interest that not only scientists, but the general public also feels about the Soviet Union” and recognized that “This meeting did a great deal of good and the press gave it an excellent report.”⁸⁹ Presided over by Dunn, the evening stressed the importance of cooperative scientific relations with the Soviet Union,⁹⁰ especially since the Soviets had proven themselves to be on par scientifically with the United States.⁹¹ Major themes discussed by the delegates were the Soviet Union’s extensive funding for and coordination of science. Shapley alluded to discussions in the United States about a national science foundation: “Only recently our government became conscious of the fact that one must build with an awareness of science, even if one doesn’t build as tightly as the Russians.”⁹² Many also remarked on their surprise that

⁸⁹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., “Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship,” 13 Nov 1945: 5 pages, 1.

⁹⁰ MCZ, Ernst Mayr Library, American-Soviet Science Society’s Science Bulletin: “American Scientists Report on Scientific Developments in the Soviet Union,” Science Bulletin I, 5 (Oct 1945): 1-12. The following mentioned cooperation by noting that Soviet scientists also wanted cooperation, or by urging Americans to facilitate cooperation: Edwin Smith, Director of the National Council of American-Soviet Friendship (2), Langmuir (3), Arpad L. Nadai, Consulting Mechanical Engineer at Westinghouse Electric Company Laboratories (8), Physical Anthropologist Henry Field, Library of Congress (10), I.M. Kolthoff, Head of Analytical Chemistry Department at University of Minnesota (11), and James Church of the Agriculture Experiment Stations in Reno, Nevada (12).

⁹¹ “U.S., Soviet Seen Leading in Science,” New York Times (22 Aug 1945): 16; MCZ, Ernst Mayr Library, American-Soviet Science Society’s Science Bulletin: Irving Langmuir, “Soviet Government Give High Priority to Pure Science,” Science Bulletin I, 5 (Oct 1945): 3-4, 4.

⁹² MCZ, Ernst Mayr Library, American-Soviet Science Society’s Science Bulletin: Harlow Shapley, “Astronomy in the Soviet Union,” Science Bulletin I, 5 (Oct 1945): 5-6, 5. Edwin Smith, Director of the National Council of American-Soviet Friendship

Soviet scientists investigated pure science as much as applied science.⁹³ They also noted how friendly the Soviets were and how highly regarded science was in their country.⁹⁴

Between the time that the Soviet delegates returned from Moscow and held the reception in New York, the United States dropped two atomic bombs on Japan. Members of the American-Soviet Science Society viewed the use of the bombs as “a serious deterioration in the relations of the United States and other countries, especially the Soviet Union.” Harry Grundfest suggested that the Science Society send a telegram to President Harry S Truman expressing their concerns about atomic energy and nuclear bombs. In keeping with the Science Society’s goal to forge peaceful post-war relations, they urged the President to not exploit the bomb by using it to intimidate other countries, but rather to share information about atomic energy

and Merrill Bernard, Hydrolic Director of the U.S. Weather Bureau commented on the Soviets’ coordination and planning of science (2, 9). Jacob Heiman, Assistant Professor of Cancer Research at Columbia University noted that Americans could learn from the Soviets about organizing and planning of science (7).

⁹³ MCZ, Ernst Mayr Library, American-Soviet Science Society’s Science Bulletin: “American Scientists Report on Scientific Developments in the Soviet Union,” Science Bulletin I, 5 (Oct 1945): 1-12. See: Langmuir (3-4), MacInnes (9), Charles Kellogg, Chief of the Division of Soil Survey of the U.S. Department of Agriculture (10), and I.M. Kolthoff, Head of Analytical Chemistry Department at University of Minnesota (11).

⁹⁴ MCZ, Ernst Mayr Library, American-Soviet Science Society’s Science Bulletin: “American Scientists Report on Scientific Developments in the Soviet Union,” Science Bulletin I, 5 (Oct 1945): 1-12. See: Smith (2), Arthur Upham Pope, Director of the Iranian Institute (2), Shapley (5), Detlev Bronk, Professor of Biophysics at University of Pennsylvania (6), Arpad Nadai, Consulting Mechanical Engineer at Westinghouse Electric Company Laboratories (8), and Kolthoff (11).

through the United Nations Organization.⁹⁵ Dunn at this time conveyed a prevalent sentiment shared by scientists to an audience in New York and summarized his motive for speaking out on the use of bombs: “The problems raised by the use of the atomic bomb have finally shown us that henceforth there is to be no separation of moral and ethical problems from so-called scientific ones.”⁹⁶ The deterioration of diplomatic relations between the United States and Soviet Union that developed in the wake of the atomic bombs greatly affected the Science Society and is discussed in the next chapter.

Dunn was primarily concerned with establishing communication with Soviet geneticists, which proved difficult. After almost one year of activity through the Science Society, Dunn noted that he had heard from only two Soviet geneticists: S. Gershenson and Anton Zhebrak.⁹⁷ Dunn and Gershenson had built a strong relationship over several years. He had shown Dunn around the Soviet Union during Dunn’s 1927 visit and translated Dunn and Sinnott’s textbook, Principles of Genetics into Russian for distribution in the Soviet Union in the early 1930s.⁹⁸ Gershenson wrote to Dunn from Kiev in 1942 asking for his help and in essence outlined the goals

⁹⁵ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., “Minutes of the Meeting of the American-Soviet Science Society,” 13 Nov 1945: 5 pages, 4-5. The telegram’s message was replicated in these minutes on pages 4-5 and paraphrased in the New York Times: “Bomb Secrecy Opposed,” New York Times (17 Nov 1945): 4. The quotation is from the telegram to President Truman.

⁹⁶ APS, Dunn Papers, Series I, Box 9, L.C. Dunn – “Science and the Future of Society,” (Lecture) 1945: 19 pages, 1-2.

⁹⁷ APS, Dunn Papers, Series I, Box 19, Walter Landauer 1945, Dunn to Landauer, Sept 1945.

⁹⁸ APS, Dunn Papers, Series I, Box 14, S. Gershenson, 1932-1944, Dunn to Gershenson, 7 Mar 1932.

of the American-Soviet Science Society prior to its existence. He requested that Dunn send him *Drosophila* stocks and reprints of American publications. He also wished to submit six articles to American journals and hoped that Dunn would proofread them first. Dunn responded by getting Demerec to help him collect books and reprints, which they sent to Gershenson through the Consul General of the U.S.S.R in New York. Dunn noted to Gershenson that they would ask others to send additional literature.⁹⁹ Even though the Science Society started after Gershenson's request reached Dunn, they announced the shipment of scientific literature to Gershenson as the organization's first package.¹⁰⁰ Dunn continued to send shipments of documents to Gershenson and his colleagues in Kiev over the next few years. They thanked Dunn and the Science Society for the biological literature:

We express our most sincere thanks to you and to the staff of the Zoological Department of Columbia University for the sending of a large number of books, sets of journals and reprints, which we have just received. Apart from the high value of this sending for our work, we were greatly touched with it as a real token of the friendly attitude of American scientists towards Soviet science. We firmly hope, that the scientific connections between our countries will become still closer and will be an important factor in the progress of scientific work both in America and in the USSR.¹⁰¹

⁹⁹ APS, Dunn Papers, Series I, Box 14, S. Gershenson, 1932-1944, Gershenson to Dunn, 10 Dec 1942 and Dunn's reply, 4 May 1943.

¹⁰⁰ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 1 (no date): 3.

¹⁰¹ MCZ, Ernst Mayr Library, American-Soviet Science Society's Science Bulletin I, 6 (Jan 1946): 37; APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, D.K. Tretiakow, S. Gershenson, and V.I. Troitskaya to Dunn, 22 Sept 1945.

Gershenson also remarked that these supplies helped to rebuild their library in Kiev.¹⁰² Anton Zhebrak, who had studied with Dunn in the early 1930s, visited the United States during the summer of 1945 and reported that correspondence could be reinstated with geneticists and promised to send their addresses after his return to the Soviet Union.¹⁰³ In 1946 Dunn eventually contacted Serebrovsky, whose institute he had visited in Moscow in 1927.¹⁰⁴ Dunn and other geneticists, many of whom were also members of the Science Society, found it hard to establish regular contact with geneticists. Mendelian genetics had been under attack in the Soviet Union since the late 1930s by Soviets promoting Trofim Lysenko's approach to the discipline. The Science Society was able to establish communication more easily with scientists from disciplines other than genetics. American geneticists' attempts at undermining Lysenkoism from afar will be discussed in the next chapter.

Post-war Reconstruction of Genetics

Tactics used by the American-Soviet Science Society for shipping materials overseas informed other groups interested in furnishing materials to war-torn countries in Europe. For example, after Hermann J. Muller became chairman of the

¹⁰² APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, Gershenson to Dunn, 28 Dec 1945.

¹⁰³ APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, I. Michael Lerner to Barbara McClintock (GSA President) with cc: Dunn, 27 June 1945.

¹⁰⁴ APS, Dunn Papers, Series I, Box 25, USSR Correspondence with Geneticists, 1944-1945, Dunn to Serebrovsky, 14 Dec 1945; APS, Dunn Papers, Series I, Box 25, A.S. Serebrovsky, 1924-1947, Serebrovsky to Dunn, 21 April 1946. The correspondence between Serebrovsky and Dunn was infrequent during the 1940s.

Committee on Aid to Geneticists Abroad, a group organized under the Genetics Society of America, he asked Curt Stern for suggestions on the types of assistance that this new committee should provide and how to perform those duties. Drawing from personal experience, Stern told Muller that he had sent a package to Italy, which after six to nine months had not arrived or been returned. With so few reprints and so many scientists desiring them, Stern recognized the need for establishing secure shipment channels. Thus, he suggested to Muller in June 1945 that he borrow ideas from Dunn's processes for shipping documents to the Soviet Union by contacting European embassies and the US State Department.¹⁰⁵ Delays resulted because some countries had not resumed mail delivery between individuals. Demerec informed Albert F. Blakeslee in June 1948 that they had finally shipped all of his reprints packaged for foreign countries. Blakeslee's reprints labeled for Japan sat for several years in the US awaiting secure delivery.¹⁰⁶

Geneticists in other countries turned to Dunn for help with reconstructing their libraries and laboratories. Geneticist C.C. Tan received Dunn's full endorsement for his enterprise to re-establish biology in China. Tan and his colleagues managed to continue their genetic research during the war, even though they moved to a temporary location. Shortly after the war's end, the Zoology Department at Columbia University hosted Tan as a visiting fellow. Tan had completed his doctorate in the

¹⁰⁵ APS, Stern Papers, Hermann J. Muller (Folder 6), Muller to Stern, 8 June 1945 and Stern's reply, 12 June 1945.

¹⁰⁶ APS, Blakeslee Papers, M. Demerec #2, Demerec's Secretary to Blakeslee, 19 June 1948.

1930s in Thomas Hunt Morgan's laboratory at California Institute of Technology working closely with Dobzhansky, who he revered as a mentor. Dunn found Tan determined and capable of rehabilitating Chinese libraries and the Genetics Institute that he directed at Chekiang University in China. Dunn wrote to the Secretary of State twice on Tan's behalf. He first proposed that the Office of International Information and Cultural Affairs help Tan start an international journal and then asked that books be sent to Chekiang. In addition to helping his Chinese colleagues, the prospects for international cooperation spurred Dunn's sponsorship of Tan's plan:

I believe it would contribute materially to the development of science in China and particularly to the kind of scientific cooperation between Chinese, Indian, British, American and other scientists upon which the general progress of science will increasingly depend.

Dunn also wrote Warren Weaver a letter expressing his faith in Tan's abilities after Tan informed Dunn that he had asked the Rockefeller Foundation for monetary aid to develop his laboratory and department in China.¹⁰⁷ Tan already received money from the Rockefeller Foundation for his personal research, but the situation in China was dire. After returning to China in 1946, Tan reported on issues facing China and his own status. China was experiencing post-war reconstruction, economic inflation, and civil unrest. Tan's university suffered great physical devastation during the war. Most of the University's budget was slated for rebuilding the campus. Tan planned to use grant money to revamp his work space: "I have struggled very hard to get my laboratory reestablished and hope to start some work in the spring. I am expecting

¹⁰⁷ APS, Dunn Papers, Series I, Box 25, C.C. Tan 1946-1948, Dunn to Secretary of State, 7 March 1946 and 8 March 1946 and Dunn to Warren Weaver, 1 May 1946.

some support of equipment from the Rockefeller Foundation, and this will help a great deal.”¹⁰⁸ Shortly after Tan returned to his country in 1946 China came under Lysenko’s influence, which forced Tan to redirect his scientific investigations from genetics to evolutionary problems until the late 1970s. When China re-embraced genetics, Tan played an important role in its reconstruction and today is recognized as “one of the founders of modern genetics in China.”¹⁰⁹ When Y. Ogura of Japan requested reprints in 1948 after his country’s period of isolation ended, Dunn sent five copies of all reprints published by his department since 1940.¹¹⁰ Whether Tan and Ogura contacted Dunn because of his activities during and after the war or because he was a personal friend cannot be ascertained. He had a reputation for helping people in need and for that reason he received many requests for help.

Indeed, Dunn was a valuable resource for many geneticists because he did what he could and delegated tasks beyond his means. Hans Grüneberg, a mouse geneticist at University College in London, received aid from Dunn and other American geneticists after the war. Dunn not only sent Grüneberg a packet of reprints from himself and Schoenheimer, but also asked Dobzhansky, Marcus Rhoades,

¹⁰⁸ APS, Stern Papers, Ms Coll 5, C.C. Tan, Tan to Stern, 12 Jan 1946.

¹⁰⁹ James F. Crow, “C.C. Tan: A Life of Peaks and Valleys,” *Genetics* 164 (May 2003): 1-4; Professor Tan moved to Shanghai’s Fudan University in 1952. Their School of Life Sciences reveres him for his contributions to improving genetics in China (<http://life.fudan.edu.cn/english/cctan.asp> 7 Feb 2006).

¹¹⁰ APS, Dunn Papers, Series I, Box 23, Y. Ogura, 1948, Ogura to Dunn, 3 April 1948 and Dunn’s reply, 27 May 1948.

Milislav Demerec, and E.C. MacDowell to send copies of their reprints.¹¹¹ Boris Ephrussi, a geneticist who relocated from France to the United States in 1941, returned to his country after the war and requested supplies for students in France. Dunn contacted H.E. Setterfield, National President of Alpha Epsilon Delta, and asked him to help Ephrussi. Setterfield offered to distribute supplies.¹¹² Otto Mohr requested *Drosophila* in order to resume experiments. Dunn sent him flies through the Foreign Information Research Division and also asked Thomas Hunt Morgan to send some too, but he did not because the weather was too warm.¹¹³

Conclusion

American scientists responded during the Second World War in two ways, mobilization for war and postwar preparation, and these two responses were intimately connected. Dunn and some other scientists mobilized on regional, national, and international levels in order to shape the war's immediate and long-term outcomes. They not only wanted the Allies to win, but also desired peaceful relations for years to come. Harry Grundfest eloquently expressed these tandem inspirations for scientific collaboration on a global scale in early 1945 when he remarked, "The immediate problems of international scientific co-operation are quite specific, being

¹¹¹ APS, Dunn Papers, Series I, Box 15, Hans Grüneberg 1937-1955, n.d., Dunn to Grüneberg, 31 Oct 1945.

¹¹² APS, Dunn Papers, Series I, Box 25, H.E. Setterfield 1948, Setterfield to Dunn, 20 May 1948. Alpha Epsilon Delta was a National Honorary Pre-Medical Fraternity based at Ohio State University.

¹¹³ APS, Dunn Papers, Series I, Box 23, Thomas Hunt Morgan, Dunn to Morgan, 22 Sept 1945 and Morgan's reply, 5 Oct 1945.

determined by the war we are fighting and the peace we aim to achieve.”¹¹⁴ Scientists participated in a variety of wartime activities in addition to working on projects for the government, and adapting their scientific investigations and academic courses to accommodate needs produced by the war. They also drew from their scientific expertise in order to advise politicians and acted as international diplomats using science as their method for peaceful communications. Chapters Four, Five and Six have demonstrated the degree to which regional, national, and international matters overlapped and were simply different scales of activism in response to the war. Dunn, as one of many scientists concerned with the war’s outcome, participated in activities that preserved American ideals threatened by the war and established institutions considered necessary as a result of the war.

All in all, issues concerning the Second World War permeated every aspect of the lives of these academics, scientists, and politicians. A multifaceted analysis of American scientists’ activities that developed in response to the war produces a better understanding of the war’s invasiveness into every aspect of their daily lives. The end of the war did not bring an end to these issues, as seen by Dunn’s work and expressed by him in September 1945: “...the post war world is upon us with many changes foreseen during the war which now have to be carried into effect.”¹¹⁵

¹¹⁴ APS, Dunn Papers, Series I, Box 24, Science in the USSR, 1944-1945, “Science Education in the USSR,” 14 Oct 1944. This document is Dunn’s overview and introductory comments at a panel discussion. Harry Grundfest, “The Scientists’ Postwar Problems,” *Scientific Monthly* 60 (Feb 1945): 130-40, 135.

¹¹⁵ APS, Dunn Papers, Series I, Box 19, Walter Landauer 1945, Dunn to Landauer, 23 Sept 1945.

Chapter 7

Science & Politics: The Onset of the Cold War

During World War II communication between the United States and Soviet Union reopened, and through the American-Soviet Science Society, American scientists facilitated the exchange of information between the two countries. Fears of the Soviet Union existed in 1943 when the Science Society began, and as years passed, these apprehensions coalesced. Members of the Science Society suffered many setbacks, especially during the group's final two years, as a result of governmental agencies that looked skeptically upon the Science Society's activities. Particularly damaging to the Science Society were accusations made against the organization during an inquiry into the loyalty of physicist E.U. Condon. Some of the Society's executive committee members, nonetheless, came to Condon's aid during his hearings with the House Committee on Un-American Activities (HUAC).

At the same time that the American-Soviet Science Society came under the watch of the HUAC, geneticists of the Science Society also fought against a threat to Soviet genetics, Trofim Lysenko's agro-biology. The Science Society had particularly good membership for combating Lysenkoism. As head of the society, Dunn enrolled many other geneticists into the organization in hopes of undermining Lysenko's

growing prestige.¹ The official end of the American-Soviet Science Society and of Mendelian genetics in the Soviet Union occurred within weeks of one another in the summer of 1948. After that time, members of the Science Society continued to discuss their organization's demise and the endorsement of Lysenko's theories in the Soviet Union. Dunn, as president of the American-Soviet Science Society, played a pivotal role in these events and suffered greatly in the wake of their dismal outcomes. As demonstrated below, Dunn's work behind-the-scenes played a major role in these years. This chapter describes Dunn's public and private efforts on the behalf of Soviet geneticists and analyzes intersections of Cold War politics and biological science in the United States and Soviet Union during the 1940s.

Two historians have recently provided thorough examinations of Soviet genetics and Lysenkoism. Nikolai Krementsov analyzes the history of Soviet genetics in a broader context than previous scholars. He not only demonstrates the effect that international diplomatic affairs had on science, but also the role that foreign geneticists played in combating Lysenko's prestige in the Soviet Union. In this vein Krementsov has analyzed the efforts made by Dunn and other American geneticists in the United States in their efforts to undermine Lysenko's prestige in the Soviet Union. In The Lysenko Effect Nils Roll-Hansen shows how Lysenko's claims initially fit well within scientific discussions of the early 1930s. Lysenko's opinions changed

¹ Nikolai Krementsov, Stalinist Science (New Jersey: Princeton University Press, 1997): 118-19; Nikolai L. Krementsov, "A 'Second Front' in Soviet Genetics: The International Dimension of the Lysenko Controversy, 1944-1947," Journal of the History of Biology 29 (1996): 229-50.

over time, however, causing some of his initial supporters to become his staunchest critics. Roll-Hansen's book extensively examines the late 1920s to 1939, nicely complementing Krementsov's focus on the late 1930s to 1948.²

It should be noted that I am concerned with situations as they unfolded in the United States, and I provide descriptions of events in the Soviet Union that pertain to American geneticists' involvement. The aim of this chapter is to analyze how American geneticists perceived what was happening, rather than to explain in minute detail what was occurring in the Soviet Union with reference to Lysenkoism. I analyze how American geneticists gathered and circulated information about Lysenko's agro-biology in the United States and the views that they presented. Furthermore, the American-Soviet Science Society's work in the United States is my main focus rather than the organization's effect in the Soviet Union. Therefore, national issues influencing the Science Society's efficiency and the group's domestic campaigns are examined. The chapter provides a better understanding of the early Cold War years in the United States by viewing Lysenkoism in its international context and examining situations facing American scientists domestically.

² For publications by Nikolai Krementsov see footnote 1. Nils Roll-Hansen, The Lysenko Effect: The Politics of Science (New York: Humanity Books, 2005). Also see Zhores A. Medvedev, The Rise and Fall of T.D. Lysenko trans. I. Michael Lerner, ed. Lucy G. Lawrence (New York: Columbia University Press, 1969); David Joravsky, The Lysenko Affair (Cambridge: Harvard University Press, 1970); Valerii Soifer, Lysenko and the Tragedy of Soviet Science (New Jersey: Rutgers University Press, 1994).

Trofim Lysenko & Agro-Biology

Trofim Lysenko promoted a non-Mendelian based biology as a viable agricultural research program for the Soviet Union. He had first performed experiments in the late 1920s and believed that vernalization improved crop production. Vernalization, by Lysenko's definition, refers to a process used on plants and seeds to quickly acclimatize them to new environments. According to Lysenko, a plant could be adapted to grow in a different temperature and seeds could be grown more quickly by using this process. Through ideas such as this, Lysenko promoted a hereditary theory in competition with Mendelian genetics and claimed that his theory was useful for solving problems facing Soviet society. For example, in extremely cold areas of the Soviet Union, crops were oftentimes damaged when temperatures dropped. Lysenko presented a solution to famine by suggesting the exposure of seeds to a cold and moist environment before planting them as a means for speeding up their gestation.³

Vernalization was not a new concept and eventually many Soviet scientists found it ineffective; however, coming to this negative conclusion was a process. Lysenko successfully revived vernalization as a credible research avenue and potential solution to the Soviet Union's agricultural problems in the early to mid-1930s. Soviet geneticists bolstered Lysenko's practical claims because of their

³ Medvedev, 12-17; Loren Graham, Science in Russia and the Soviet Union: A Short History, Cambridge: Cambridge University Press, 1993: 124-25.

potential benefits, if true, and simultaneously tested the validity of his ideas. He later espoused a genetic theory based on the inheritance of acquired characteristics, which received criticism shortly after he presented it. Proponents of the inheritance of acquired characteristics believe that environmental factors affecting one generation pass from parent to progeny, and therefore living organisms quickly adapt to their surroundings. Lysenko reinforced his scientific theory of inheritance of acquired characteristics by aligning it with dialectical materialism, the philosophical basis of a socialist state.⁴

Dialectical materialism has had many meanings in the Soviet Union during the twentieth century. Materialists believe that everything in nature consists of matter and that nature is infinite. Those who subscribe to dialectical philosophy promote that all parts in nature, history, and thought are connected and in motion. Dialectical materialists, therefore, consider evolution to be a basic tenet that explains how substances develop from simple to more complex. In reference to Lysenko's agrobiolgy, Russian historian Loren Graham argues that Lysenko did not align his ideas with dialectical materialism until after he started collaborating with I.I. Prezent. In 1935, Lysenko under Prezent's influence published his first paper that supported dialectical materialism and disputed Mendelian genetics.⁵

⁴ Roll-Hansen, 113-92.

⁵ For discussions about the Soviet Union and dialectical materialism, see: Gustav A. Wetter, Dialectical Materialism: A Historical and Systematic Survey of Philosophy in the Soviet Union trans., Peter Heath (New York: Frederick A. Praeger, Publishers, 1958); David Joravsky, Soviet Marxism and Natural Science, 1917-1932 (London: Routledge and Kegan Paul Ltd, 1961); Loren R. Graham, Science, Philosophy, and

Lysenko's shrewd tactics to promote himself and his ideas contributed greatly to his influence over Soviet biology. He fit his agricultural work into the political ideology approved by Stalin, which won him political support. His main proponents in the long run were not his fellow scientists, but rather Lysenko gained political backing, which proved quite important for success under Josef Stalin's rule. Lysenko used a shrewd political maneuver to promote his ideas by referring to his science as Michurin biology in honor of Soviet horticulturalist Ivan Michurin who had previously endorsed similar methods. Michurin had no formal scientific training and through experience had proved himself a talented fruit breeder.⁶ Attributing intellectual heritage to a Soviet plant breeder was shrewd because Stalin promoted the tradition of the worker scientist as a method of national propaganda. Born a peasant, Lysenko also could style himself as fulfilling the goal of Soviet society, as envisioned by Vladimir Lenin after the 1917 revolution, to modernize the peasantry into accomplished workers contributing to the socialist state. Together, Lysenko's philosophical and scientific theories, complemented by his social origins, gave him considerable advantage in the political struggle of the 1930s. It is notable that Lysenko himself did not discuss any direct implications of his agro-biology for human improvement.⁷

Human Behavior in the Soviet Union (New York: Columbia University Press, 1987); Frederic L. Bender, ed., The Betrayal of Marx (New York: Harper & Row, Publishers, 1975).

⁶ Theodosius Dobzhansky, "The Suppression of a Science," Bulletin of the Atomic Scientists 5 (May 1949): 144-46.

⁷ Medvedev, 4; Graham, 100-01, 123-24.

Lysenko gained a significant foothold in the Soviet Union during the late 1930s, which coincided with the Great Terror and purges of 1936 to 1938. Many scientists holding high positions lost their posts and were replaced by those conforming to the party's agenda. Lysenko's power increased as a result, and he became president of the Lenin All-Union Academy of Agricultural Sciences in 1938. Lysenkoists assailed Mendelians by using political and personal attacks during the scientific congresses, and Lysenko won considerable support after an October 1939 meeting convened to discuss genetics. The report produced from the meeting fashioned Lysenko's agro-biology in a positive light by calling it revolutionary and labeling Mendelian genetics as counter-revolutionary. Soviet biologists continued to debate the scientific and political validity of the two perspectives for heredity after the 1939 meeting. Nikolai Krementsov demonstrates that Lysenko's prestige fluctuated during the next decade until 1948 when his genetic ideas received complete endorsement in the Soviet Union. Even though his power fluctuated, Lysenko and his agro-biology remained a constant threat to Soviet geneticists during the 1930s and 1940s. All in all, Lysenko held a prominent position guiding Soviet agriculture and genetics from the 1930s to 1960s.

The internationally respected geneticist Nikolai Vavilov argued in support of Lysenko's ideas early-on and remained supportive of him longer than most Mendelian geneticists. Vavilov believed that vernalization might have value, but he disagreed with Lysenko's theory based on inheritance of acquired characteristics. Yet, Vavilov vocalized his criticisms of Lysenko to a lesser extent than his co-workers as

late as 1936. Vavilov was president of the Lenin All-Union Academy of Agricultural Sciences (VASKhNIL) before Lysenko held the post, but was replaced by someone with views more in line with the Communist Party in 1935.⁸

Nikolai Vavilov was arrested in 1940 and died in a Siberian concentration camp in 1943.⁹ Information about his whereabouts and death was unknown in the United States during the early to mid-1940s. Sometime after 1941 Dunn wrote to the Soviet Ambassador requesting information on whether his friend Nikolai Vavilov had been arrested at some point. Dunn also mentioned Vavilov's scientific credentials and asked for the Ambassador's help in abating the rumors that circulated in the United States.

His work is so well-known and his personality so valued that his fate is a matter of international scientific importance concerning which his colleagues all desire authentic information. May I beg of you, Mr. Ambassador, to inquire concerning the whereabouts of Prof. Vavilov and either to inform me or to make a public statement and thus set at rest the rumors now circulating in scientific circles in this country and England?¹⁰

It does not appear that Dunn received an answer to his request for verification about Nikolai Vavilov. In 1945 Karl Sax of Harvard University averred that Vavilov was

⁸ Krementsov, 1997, 54-91; Graham, 128-30; Roll-Hansen, 171-94.

⁹ Conway Zirkle, Death of a Science in Russia: The Fate of Genetics as Described in *Pravda* and Elsewhere (Philadelphia: University of Pennsylvania, 1949): 88; Roll-Hansen, 264. Zirkle and Roll-Hansen give two different years for Vavilov's death 1942 and 1943, respectively.

¹⁰ APS, Dunn Papers, Series I, Box 26, Nikolai Vavilov, 1934, 1941, Handwritten copy of letter from Dunn to Ambassador of USSR, Washington D.C., no date. Although, it is unclear exactly when Dunn wrote his letter, it was after 1941 because he mentioned Vavilov's arrest in the summer of 1940 and that Vavilov was still in custody in 1941.

dead, and geneticist Robert C. Cook, managing editor of the Journal of Heredity, produced contradictory information after following up on Sax's statement. Cook stated that Nikolai Vavilov's death had not been confirmed, and that it was Sergei Vavilov, Nikolai's brother and President of the USSR Academy of Sciences, who had died.¹¹ Cook had erroneous information because in actuality, Sergei died about five years later in 1951.¹² This discussion about Vavilov's death demonstrates a common problem among American scientists about events occurring in the Soviet Union. Lots of contradictory statements circulated in the United States and made it difficult for American geneticists to figure out what was true and what was not.¹³

Coordinating American Geneticists' Attack on Lysenko

Before becoming an independent organization on 1 April 1945, the American-Soviet Science Society operated as a Science Committee under the National Council of American-Soviet Friendship. When Dunn first spoke about the need for scientific exchanges between the United States and Soviet Union, he did so at the 1943 meeting that established the Science Committee. In his speech, "Soviet Biology," Dunn talked about his impressions of the Soviet Union from over fifteen years earlier when he had

¹¹ Robert Cook, "The Question about Dr. Vavilov's Death," Journal of Heredity 37 (Jan 1946): 28.

¹² Krementsov, 305.

¹³ Nils Roll-Hansen, 288. Roll-Hansen also used Nikolai Vavilov's death as an example to demonstrate the misunderstandings between what Americans heard and what was happening in the Soviet Union. Information about the cancellation/postponement of the 7th International Genetics Congress originally scheduled for Moscow in 1937 is another example (Krementsov, 2005, 46-53).

visited there in 1927. Dunn gave a promising picture of the Soviets' pursuit of science and not only urged improved relations between the two countries, but also geared his speech to address the debates about establishing a national science foundation in the United States.¹⁴ According to Dunn, the relationship between scientists and the Soviet government proved that governmental funding produced quality science. He said:

The progress of biological research in the Soviet Union has taught us a very valuable lesson. It is that control and organization of science by and for the whole community does not kill the scientific spirit or initiative nor submerge the individual scientist in a dead level of anonymity.¹⁵

Dunn addressed in this statement a hotly contested issue – whether or not scientists would be able to freely pursue their scientific investigations if they were funded with governmental money. Shortly after making his speech, Dunn submitted it for publication in Science and it appeared in print in January 1944.

A few months later, botanist and geneticist Karl Sax of Harvard University severely criticized Dunn for ignoring the true state of Soviet genetics. He directly countered Dunn's statements about the freedom of scientific inquiry in the Soviet Union by discussing T.D. Lysenko's influence on Soviet biology.

The author [L.C. Dunn] did not discuss the most significant trend of biological research in the U.S.S.R. – the subservience of science to

¹⁴ Historian Oren Solomon Harman has shown that C.D. Darlington discussed Lysenkoism in order to speak about the relationship between science and government in England. Dunn made similar statements. Oren Solomon Harman, "C.D. Darlington and the British and American Reaction to Lysenko and the Soviet Conception of Science." Journal of the History of Biology 36 (2003): 309-52; Oren Solomon Harman, The Man Who Invented the Chromosome: A Life of Cyril Darlington (Massachusetts: Harvard University Press, 2004): 140-52.

¹⁵ L.C. Dunn, "Soviet Biology," Science 99 (28 Jan 1944): 65-67, 67.

social and political philosophy. It is important that this aspect of Soviet science should be generally known and understood because it is not confined to Russia. It could happen here.¹⁶

Sax represents the other side of the argument about possible ramifications resulting from a relationship between science and government. He worried that the agency paying for the scientific investigations would also direct the science being performed.

Dunn and Sax knew one another, although they did not have a close personal relationship. They had attended Harvard University for graduate degrees at the same time, and thus overlapped at the Bussey Institution for about three years.¹⁷ Sax studied plant genetics with E.M. East, while Dunn studied mammalian genetics with William E. Castle.¹⁸ Sax made a valid point when he mentioned that Dunn failed to discuss Lysenko and his influence on Soviet biology. Dunn chose to not mention Lysenko because he doubted at the time the durability and extensiveness of Lysenko's influence.¹⁹

When Sax mentioned Lysenko's influence over Soviet biology in 1944, Lysenko indeed had a fair amount of credibility in the Soviet Union. American scientists, however, knew few details due to the interruption of communication

¹⁶ Karl Sax, "Soviet Biology," *Science* (14 Apr 1944): 298-99.

¹⁷ Dunn attended Harvard University from 1915 to 1920, taking time off during World War I (Dobzhansky, 1978, 80-81). Sax attended Harvard University from 1916 to 1922 (American Men and Women of Science, 12th ed. 1971, 5524).

¹⁸ Carl P. Swanson and Norman H. Giles, "Karl Sax, November 2, 1892-October 8, 1973," *Biographical Memoirs of the National Academy of Sciences* 57 (Washington, D.C.: National Academy Press, 1987): 373-97. L.C. Dunn, "William E. Castle, October 25, 1967-June 3, 1962" *Biographical Memoirs of the National Academy of Sciences* 38 (Washington, D.C.: National Academy Press, 1987): 33-80, 52-53.

¹⁹ APS, Dunn Papers, Series I, Box 17, Kilgore Bill, 1944, Dunn to Sax, 5 Feb 1944.

between the two countries. The opening of discourse between scientists and the exchange of publications monitored by the American-Soviet Science Society, however, allowed for the circulation of information. Dunn as chairman of the American-Soviet Science Society regularly acquired news about and publications by Lysenko.

Dunn received Lysenko's book Heredity and Its Variability in 1944, the year after it was published in the Soviet Union. Unable to read Russian, Dunn asked his colleague Theodosius Dobzhansky to translate it, which Dobzhansky completed during summer 1945. When sending it to Dunn, Dobzhansky noted that: "Translating it has been one of the most unpleasant tasks I had in my whole life, and surely I would never undertake a thing like that for money – it can be done only for a 'cause.'"²⁰ Dunn also organized the logistical end of publishing Lysenko's book. After receiving Dobzhansky's translation, he had it edited for "style and sense" before submitting it to publishers.²¹ He hoped that McGraw-Hill Company would publish Lysenko's book, but the Editorial Committee rejected the proposal.²² Dunn

²⁰ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky – Dunn Correspondence, 1943-45, Dobzhansky to Dunn, 31 July 1945.

²¹ Professor Carl Epling of University of California edited Dobzhansky's translation: ; APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1945-46, Dunn to Silver 17 Aug 1945.

²² APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-46, n.d., Dunn to Walter Bara of the McGraw-Hill Publishing Company, 26 May 1945 and Bara's reply, 1 June 1945; L.C. Dunn, interview, "The Reminiscences of Leslie Clarence Dunn," transcript, Columbia University Oral Histories, 1958-1960, 747-48. Interviews conducted by Dr. Saul Benison. Kremontsov has written about the process of getting Lysenko's book published in the United States after Dobzhansky had translated it: Kremontsov, 1997, 121-22.

then approached Henry Silver of the King's Crown Press, who agreed to publish the book as an inexpensive pamphlet costing fifty cents.²³

During this time, Dunn also collected information about Lysenko in preparation for reviewing the book after Dobzhansky's translation was published. Secretary of Commerce Henry A. Wallace proved to be a particularly valuable source for information. Wallace had grown up on a farm in Iowa and contributed to the family's journal, "Wallace's Farmer." He studied genetics while an undergraduate at Iowa State College and received an Honorary Doctor of Science degree from his alma mater in 1934. Wallace was Secretary of Agriculture from 1933 to 1940 and Vice-President under Franklin D. Roosevelt during his third term. Wallace traveled to the Soviet Union in 1944 on official business²⁴ and during his trip collected Soviet publications, including some written by Lysenko. Wallace supplied Dunn with English translations of Lysenko's publications dating from 1939, which helped Dunn and his associates to write informed reviews of Lysenko's claims.²⁵

²³ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence, 1943-45, Dunn to Dobzhansky, 18 Aug 1945; APS, Dunn Papers, Series I, Box 21, Lysenko Controversy-Correspondence, 1945-46, Dunn to Silver 17 Aug 1945 and King's Crown Press to Dunn, 11 Sep 1945; T.D. Lysenko, Heredity and Its Variability, trans. Theodosius Dobzhansky (New York: King's Crown Press, 1946).

²⁴ Edward L. Schapsmeier and Frederick H. Schapsmeier, Henry A. Wallace of Iowa: The Agrarian Years, 1910-1940 (Ames: The Iowa State University Press, 1968): 20-24, 166, 264-65; Edward L. Schapsmeier and Frederick H. Schapsmeier, Prophet in Politics: Henry A. Wallace and the War Years, 1940-1965 (Ames: The Iowa State University Press, 1970): 87-91.

²⁵ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1945-46, Wallace to Dunn, 21 Aug 1945, Dunn to Wallace, 31 Aug 1945. Wallace

Dunn not only organized the publication process, but also coordinated geneticists' critiques of Lysenko's Heredity and Its Variability for scholarly journals. In addition to his own review slated for Science, he successfully lined up Dobzhansky to review it for Journal of Heredity and G. Ledyard Stebbins for Chronica Botanica. Dunn asked L.J. Stadler to write a review for American Naturalist and provided him with galley proofs since the book had not yet been released. Dunn also told Stadler the purpose for translating the book and how to frame the review's contents:

It is very important that the book be reviewed by persons who understand the implications of his [Lysenko's] work and who will not use it merely for an attack on Soviet science in general. The book was translated in order that Americans could judge at first hand what Lysenko's ideas really are.

Stadler declined,²⁶ and Curt Stern wrote the review instead.²⁷

Dunn explained to Waldemar Kaempffert, Science Editor for the New York Times that there were two opinions about how to handle Lysenko's book. The first perspective demonstrates Stadler's response to Dunn: Do not validate Lysenko's

had copies of addresses made by Lysenko and a verbatim account of the Lysenko-Vavilov debates from Pod Znamenem Marksizma.

²⁶ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy-Correspondence, 1945-46, Dunn to Stadler, 22 Dec 1945 and Stadler's reply 28 Dec 1945. Kremenstov quoted a similar passage made by Dunn on 29 June 1945 to I. Michael Lerner in Stalinist Science (122).

²⁷ APS, Stern Papers, Hermann J. Muller (Folder 7), Stern to Muller, 14 Feb 1946. Curt Stern, rev. of "Heredity and Its Variability," by T.D. Lysenko, The American Naturalist 80 (1 Mar 1946): 241-43. Kremenstov discussed Dunn's role in coordinating the translation and book reviews of Lysenko's Heredity and Its Variability as well as a pamphlet by P. Hudson and R. Richens that scientifically analyzed Lysenko's claims (Kremenstov, 1997, 121-22). The Hudson and Richens book is: P.S. Hudson and R.H. Richens, The New Genetics in the Soviet Union (Cambridge: School of Agriculture, 1946).

pseudo-science by discussing his views. The other opinion explains Dunn's goal: Understand Lysenko's work before criticizing it. Dunn wanted to subject Lysenko's work to the same scrutiny that any other scientific investigation would receive before taking a stance on its validity.²⁸ Kaempffert asked Dunn for information because he planned to discuss Lysenko and Vavilov in a Sunday edition of the New York Times in early 1946. Dunn sent him the book reviews written by himself, Stebbins, and Dobzhansky. In his review of Lysenko's book Kaempffert concentrated on and criticized Lysenko's scientific views. He also noted that the Soviet Union's political system had to be considered in order to understand Lysenko.²⁹

Dunn and his fellow geneticists consciously kept politics to a minimum and focused on Lysenko's scientific approach in their book reviews.³⁰ Curt Stern explained Lysenko's ideas in comparison to those of Mendelian genetics. He concluded that Lysenko presented "hunches and vague generalities" and disregarded

²⁸ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy-Correspondence, 1945-46, Dunn to Kaempffert, 31 Jan 1946. Kremenstov has written about Dunn and others' coordination in an effort to undermine Lysenko's prestige in the Soviet Union (Kremenstov, 1997, 121-28). Dunn expressed similar motives to I. Michael Lerner: APS, Dunn Papers, Series I, Box 25, USSR-Correspondence with Geneticists, 1944-45, Dunn to Lerner, 29 Jun 1945. Dunn wrote to Lerner, "We believe that the best way to deal with Lysenko's influence is to make known his ideas and evidence in a form in which he himself has published them. We have no doubt that the judgment of Americans will be adverse and that this will strengthen the hands of those in the Soviet Union who oppose him."

²⁹ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1945-46, Correspondence between Dunn and Kaempffert dated 29 Jan 1946 to 13 Feb 1946. Waldemar Kaempffert, "Man and his Milieu" rev. of Heredity and Its Variability by T.D. Lysenko, New York Times (3 Mar 1946): BR4.

³⁰ Kremenstov states that the book reviews coordinated by Dunn focused on science and either presented a neutral or positive opinion of socialism (1997, 122).

fifty years of genetic research.³¹ Dobzhansky noted that Lysenko reiterated outdated, erroneous theories and told his readers where to find information that countered Lysenko's claims, which he said were "to put it mildly, improbable."³² According to Dunn, Lysenko offered little evidence and presented nothing new. Lysenko's aim, moreover, differed from that of geneticists and other scientists because he wanted to control, rather than understand, nature. Furthermore, Dunn attributed Lysenko's significant influence to the "promise" that his theories gave to the Soviet Union's agricultural problems.³³ Dunn and his network, therefore, organized their responses in order to present similar opinions about Lysenko's worth. However, Dunn was not the only one who coordinated geneticists in an effort to undermine Lysenko.

Hermann J. Muller also asked geneticists to make public statements, and Krementsov notes that the two most active organizations in coordinating American geneticists' against Lysenko were headed by Dunn and Muller. These organizations were, respectively, the American-Soviet Science Society and the Genetic Society of America's Committee in Aid of Geneticists Abroad.³⁴ The Genetic Society of America formulated its committee mid-year 1945.³⁵ Muller had spent many years living in the Soviet Union during the 1920s and 1930s and consciously refrained from

³¹ Stern, 1946, 243.

³² Theodosius Dobzhansky, "Lysenko's 'Genetics:' A Review," rev. of Heredity and its Variability by T.D. Lysenko, Journal of Heredity 37 (Jan 1946): 5-9. The quotation is from page 8.

³³ L.C. Dunn, rev. of Heredity and Its Variability, by T.D. Lysenko, Science 103 (8 Feb 1946): 180-81.

³⁴ Krementsov, 1997, 121.

³⁵ APS, Stern Papers, Hermann J. Muller (Folder 6), Muller to Stern, 8 June 1945 and Stern's reply, 12 June 1945.

activities dealing with the Soviet Union because of the possible backlash for himself and those he knew in the Soviet Union. For example, in 1944 Muller declined Dunn's request that he join the American-Soviet Science Society, noting that he had to be discrete until he obtained a permanent academic post in the United States.

I am not free to do what I normally should, without the risk of paying too great a price. I know that this sort of thing is watched by the administrators, in deciding whether or not to invite a man, and in my case they would be especially wary in watching for traces of the continuance of a pro-Soviet attitude once so openly expressed.³⁶

Shortly thereafter Muller accepted a job at Indiana University in Bloomington and started teaching there in fall 1945.³⁷

Muller shrewdly asked Curt Stern to write a rejoinder to an anonymous article in the "Science" section of Time on 11 February 1946 that gave the impression that there was something of value in Lysenko's work. Since the Time article reviewed Dobzhansky's translation of Lysenko's book Muller believed that he, Dobzhansky, and Dunn should not write a response. If written by one of these men who had close associations to the Soviet Union, it would be considered a personal reaction rather than a scientific one. Stern wrote a remark about the book review in Time, in which he noted the fallacy of Lysenko's methods and his lack of proof and concrete evidence.³⁸ After seeing the photograph of Lysenko that accompanied the anonymous

³⁶ APS, Dunn Papers, Series I, Box 23, Hermann J. Muller, 1938-1945, Dunn to Muller, 22 March 1944 and Muller's reply, 7 May 1944.

³⁷ Elof Axel Carlson, Genes, Radiation, and Society: The Life and Work of H.J. Muller (Ithaca: Cornell University Press, 1981): 288-290.

³⁸ "How to Liquidate Heredity," rev. of Heredity and its Variability by T.D. Lysenko, Time 47 (11 Feb 1946): 60-61; Curt Stern, "Vague, Amateurish Lysenko," Time 47

article, Richard Goldschmidt wrote to Dunn: “Did you see Lysenko’s picture in Time? Doesn’t he look like a madman?”³⁹

Salvador Luria also coordinated geneticists’ efforts in an attempt to help his Soviet colleagues. Luria, a refugee from Italy, had yet to conduct his experiments with Max Delbrück on bacteria, which would prompt the establishment of the “Phage Group” and win him a shared Nobel Prize in 1969. However, scientists highly praised his scientific capabilities, which allowed him to relocate from fascist Italy to Columbia’s College of Physicians and Surgeons in fall 1940. Three years later, Luria relocated to Indiana University in Bloomington; hence, he and Muller were employed simultaneously at the same institution by the Departments of Bacteriology and Zoology, respectively.⁴⁰ In 1946 Luria wrote a letter to British geneticist J.B.S. Haldane that was co-signed by Stern, Dobzhansky, Dunn, Demerec, and Muller. The six geneticists who signed the letter strategically asked Haldane to make a public statement denigrating Lysenko’s science, knowing that Haldane was a revered geneticist, member of the USSR Academy of Sciences, and known political ally of the Soviet Union. Haldane declined saying that he did not know enough about

(11 Mar 1946): 6-7. APS, Dunn Papers, Series I, Box 14, Richard B. Goldschmidt, 1939-1950, Goldschmidt to Dunn, 15 Feb 1946.

³⁹ APS, Dunn Papers, Series I, Box 14, Richard Goldschmidt, 1939-50, Goldschmidt to Dunn, 15 Feb 1946.

⁴⁰ RAC, RF 1.1 200D, Box 133, Folder 1641, Columbia University – Salvador Luria, (Refugee Scholar, Surgery) 1940-1941, Memorandum RA-NS 4044, 15 Nov 1940; David Y. Copper, “Salvador Edward Luria,” American National Biography 14, eds. John A. Garraty and Mark C. Carnes (New York: Oxford University Press, 1999): 144-45; Carlson, 291-92 (see above footnote).

Lysenko's claims to make a statement.⁴¹ Haldane's reply "really shocked" Stern; whereas Muller said he "had expected something like this."⁴²

Dunn and his network of activists had an agenda. They consciously chose not to attack the Soviet system because they did not want to risk alienating the Soviets. Indeed, they wanted to undermine Lysenko's influence, but without disturbing the newly opened communication between the Soviet Union and its wartime Allies in the United Kingdom and United States. Ultimately, they desired free scientific exchange between countries and attempted to regain an aspect of science lost during the war, internationalism.

Concerned about the harm that their efforts might cause for Soviet geneticists, American geneticists devised some alternative methods in hopes of helping Soviet geneticists, as was the case for Nikolai Timofeeff-Ressovsky. Timofeeff-Ressovsky had cultivated strong relationships with foreign colleagues while in Germany at the Kaiser Wilhelm Institute for Brain Research. He had gone to Berlin in 1925 and did not return to the Soviet Union until after the war ended. In summer 1946, Peo I. Koller of the Royal Cancer Hospital in London informed Demerec that Timofeeff-Ressovsky and another scientist "are on trial for their lives. The charge is that they

⁴¹ APS, Dunn Papers, Series I, Box 23, Hermann J. Muller, 1946-1967, Muller to Dunn, 5 June 1946. Muller received the letter from Haldane on 15 May 1946. Haldane said: "I regret that I have not read Lysenko's book, and am therefore clearly not in a position to do anything about the matter. You will agree that publication in the U.S.A. does not mean availability in this country." Kremontsov, 1997, 115, 123.

⁴² APS, Stern Papers, Hermann J. Muller (Folder 7), Muller, Stern, Dobzhansky, Dunn, Demerec, and Luria to Haldane, 17 April 1946, Muller to Stern, 5 Jun 1946, and Stern to Muller, 12 Jun 1946; Kremontsov, 123.

did not return to Russia when they were requested to do so. Time is very short and no stone must be left unturned in order to help them.”⁴³ Koller asked Demerec to tell Dunn. Dunn, however, was on sabbatical at this time and stayed estranged from these issues until after his return in 1947.

Demerec took two main courses of action after receiving Koller’s letter. He showed the letter to Bentley Glass, who wrote to Muller about the situation. Glass, a geneticist in the United States, worried that “any word from foreign geneticists will probably aggravate rather than alleviate their danger,” and he offered an alternative method for aiding Timofeff-Ressovsky. In order to deflect the true source and motive, Glass suggested that a prestigious American society, specifically the National Academy of Sciences, bestow an honor on Timofeff-Ressovsky. Muller, then, wrote to Curt Stern asking for his opinion about honoring Timofeff-Ressovsky.⁴⁴ Demerec also contacted Ignace Zlotowski of the American-Soviet Science Society asking if the organization could make a gesture on Timofeff-Ressovsky’s behalf. Since the Science Society had good relations with the Soviet Union, Demerec and some of the other geneticists spending the summer at Cold Spring Harbor thought that perhaps the Science Society could state that future relations with the Soviets would be compromised because of Timofeff-Ressovsky’s trial. Max Delbrück, who had worked with Timofeff-Ressovsky in 1937, was in Cold Spring Harbor that summer

⁴³ The other scientist was Zarakin. APS, Demerec Papers, B: D394, National Council of American-Soviet Friendship, Inc., Science Committee, Folder 3, Koller to Demerec, 27 July 1946.

⁴⁴ APS, Stern Papers, Ms Coll 5, Hermann J. Muller, Folder 7, Muller to Stern, 5 Aug 1946.

and offered to provide the Science Society with personal information about Timofeeff-Ressovsky.⁴⁵

This particular case reiterates the point that American geneticists were concerned about their Soviet colleagues and they understood the predicaments facing Soviet scientists. It also demonstrates the type of networking taking place not only among American geneticists, but also between American and foreign scientists. All of them wanted to aid their Soviet colleagues in a manner that would produce the best possible outcome. Other geneticists continued to share information, but Dunn, while on sabbatical, was not part of these exchanges and appears to have kept distant from the Lysenko issue, for the most part, until the final outcome in the dispute between Lysenko's agro-biology and Mendelian genetics was passed down in the Soviet Union during the summer of 1948.⁴⁶ As will be discussed below, Dunn was preoccupied with domestic issues involving the American-Soviet Science Society after returning from sabbatical.

⁴⁵ APS, Demerec Papers, B:D394, National Council of American-Soviet Friendship, Inc., Science Committee, Folder 3, Demerec to Zlotowski, 1 Aug 1946.

⁴⁶ Dunn was on sabbatical from May 1946 to September 1947, during which he wrote a review of P.S. Hudson and R.H. Richen's The New Genetics in the Soviet Union (APS, Dunn Papers, Box 6, Theodosius Dobzhansky-Dunn Correspondence #4, 1943-45, Dobzhansky to Dunn, 26 Sep 1946, page 3 of 4); L.C. Dunn, book review, "The New Genetics in the Soviet Union" Science 104 (18 Oct 1946): 377-78. In Dunn's Papers at the APS, there is nothing in the Lysenko folder between the dates April 1946 and 23 September 1948, at which time Franz Kallmann of New York State Psychiatry Hospital solicited Dunn for help in writing an article about Lysenko. Dunn agreed. APS, Dunn Papers, Box 21, Lysenko Controversy-Correspondence, 1948-53, Franz Kallmann to Dunn, 28 Sep 1948 and Dunn's reply, 28 Sep 1948.

Saving Genetics in the Soviet Union

In the Soviet Union scientists had been experiencing more autonomy during the war and immediately following its end than in the periods before and after those years. World War II shifted the regime's focus from combating domestic enemies to fighting foreign ones, and scientists benefited from the regime's preoccupation. Scientists also profited during the years immediately following the war's end when international relations between the United States and Soviet Union were good. Geneticists felt fairly secure that they could undermine Lysenko's prestige and diminish his power over their discipline from 1945 to 1947. Dunn's initiative for the Western publication and critical reaction to Dobzhansky's English translation of Lysenko's book was meant to bolster the position of geneticists in the Soviet Union. Soviet geneticists were gaining confidence that Lysenko's power was waning.⁴⁷

Dunn and his cohort worked together to help their genetic colleagues in the Soviet Union because they thought that Lysenko's prestige was on the decline. Soviet geneticist Anton Zhebrak visited the United States in May 1945 and informed American geneticists that Lysenko was losing his authority. Zhebrak headed a division of Agitprop, the Communist Party's Administration of Agitation and Propaganda, and when in the Soviet Union he used his position to gather support for genetics from Communist party members. He also oversaw a genetics department at Timiriazev Agricultural Academy. He had received a scientific education and studied

⁴⁷ Krementsov, 1997, 100-115.

with Dunn at Columbia University in 1930-1931.⁴⁸ Dobzhansky wrote to Dunn on 25 November 1945 with news of “the first clear testimony of Lysenko’s star declining.” Dobzhansky’s proof was that Nikolai Dubinin had extolled the greatness of Vavilov and Georgii Karpechenko in an article about Soviet theoretical genetics.⁴⁹

E.B. Babcock and I. Michael Lerner, professors of genetics at University of California, wrote to Dunn in summer 1945 to notify him of Zhebrak’s visit to Berkeley. Zhebrak spoke extensively with Lerner, a Russian émigré, about the Soviet Union and Soviet genetics and told him about the extensive destruction to the Universities in Kiev and Minsk during the war and their needs for reconstruction.⁵⁰ Babcock hoped that Zhebrak could give Dunn valuable information about genetics in the Soviet Union, and Lerner tried unsuccessfully to coordinate a meeting between Zhebrak and Dunn. Zhebrak was called back to the Soviet Union earlier than expected.⁵¹

Zhebrak paid close attention to the discussion that had occurred between Dunn and Sax in Science during 1944 and tried to respond to Sax’s article. Zhebrak

⁴⁸ Roll-Hansen, 179-80. Roll-Hansen mentions that Zhebrak studied at Columbia University with Dunn’s students too. Kremenstov, 1997, 105-06. Kremenstov states that information gathered from Zhebrak and by the American delegation that went to the Soviet Union in summer 1945 for the 220th Anniversary of the USSR Academy of Sciences convinced American and British geneticists to aid their Soviet colleagues (119-20).

⁴⁹ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #5, 1946-1947, Dobzhansky to Dunn, 25 Nov 1946.

⁵⁰ Kremenstov, 1997, 119-21; APS, Dunn Papers, Series I, Box 25, USSR – Correspondence with Geneticists, 1944-45, Babcock to Zhebrak, 1 June 1945 and Babcock to Dunn, 2 June 1945.

⁵¹ APS, Dunn Papers, Series I, Box 25, USSR-Correspondence with Geneticists, 1944-45, Correspondence between Lerner and Dunn, Jun and Jul 1945.

submitted an article to Science when he was in the Soviet Union, but it had not been published. After arriving in the United States, Zhebrak sent it to Dunn for final editing before submission. The last paragraph of Sax's article had done considerable damage in the Soviet Union, according to Zhebrak in correspondence.⁵² Sax had said:

Our admiration for the Russian people and the military might of the Soviet Republic should not blind us to the fact that science has not been free in the totalitarian states where science must conform to political philosophy.⁵³

In Zhebrak's view, Sax's statement provided Lysenko with proof that geneticists, such as Sax, were anti-Soviet. In contrast, Zhebrak tried to convey a relatively positive depiction of Soviet genetics and agro-biology by writing that Lysenko had contributed valuable practical ideas, but that Soviet geneticists largely disregarded his work in genetics. The Soviet government "has never interfered" in scientific debates in genetics, Zhebrak maintained. In addition to his glowing report about the Soviet situation, Zhebrak not only denounced Sax, but also complimented Dunn for appreciating the Soviet perspective and for trying to increase communication between the two countries.

Professor Sax does not understand the essence of the Soviet conception of the bonds between pure science, its application and philosophy, according to which the materialist philosophy of nature can only develop on the basis of the development of the various sciences; he therefore incorrectly states that in our country "science must conform to political philosophy."⁵⁴

⁵² APS, Dunn Papers, Series I, Box 25, USSR-Correspondence with Geneticists, 1944-45, Zhebrak to Editors of Science, 13 Jun 1945 and Lerner to Dunn, 27 Jun 1945.

⁵³ Sax, 1944, 299.

⁵⁴ Anton Zhebrak, "Soviet Biology," Science 102 (5 Oct 1945): 357-58, 358.

Sax wrote a response to Zhebrak, which appeared two months later in Science, asking “Where is Vavilov?” and then responded to his own question with “Vavilov is dead.”⁵⁵ Vavilov had indeed died, but his death had not yet been confirmed in the United States.

The discussion in Science among Dunn, Sax, and Zhebrak, which played out between January 1944 and December 1945, did not stop there or with them. A little less than two years later, the interchange received attention in Pravda, a major Russian newspaper, and triggered what one observer described as “a twisted ending.” On 2 September 1947 Ivan Laptev, editor of Pravda’s agricultural column and supporter of Lysenko, criticized Zhebrak and his Science article. Laptev’s article garnered a great amount of response in support of Zhebrak from Soviet geneticists. Laptev’s claim that Zhebrak had committed “unpatriotic acts” led to the Timiriazev Academy’s calling Zhebrak before an honor court less than one month after Laptev’s article appeared. Zhebrak was exonerated, a result which Krementsov uses to demonstrate that Mendelian genetics still had support within the regime in late 1947.⁵⁶

Almost one year later, in July and August 1948, the Central Committee of the Communist Party endorsed Lysenko’s agrobiolgy as the only valid form of biological research in the Soviet Union and purged Mendelian geneticists from

⁵⁵ Karl Sax, “Soviet Biology,” Science 102 (21 Dec 1945): 649.

⁵⁶ Krementsov, 1997, 146-49. Laptev’s article was reprinted in Socialist Agriculture the following day.

academic positions. Zhebrak and others fought the Communist Party at first, but were quickly censored. By mid-August, Zhebrak recanted his belief in Mendelian genetics, stated that his old work was based on erroneous ideas, and vowed to uphold the party's finding.⁵⁷

Laptev's commentary marked the beginning of the end for two lines of genetic investigation in the Soviet Union, according to Conway Zirkle. Zirkle, professor of botany at University of Pennsylvania and friend to Dunn and other geneticists, wrote a book, Death of a Science in Russia, analyzing the situation almost immediately after these events occurred. Zirkle described Laptev as a "pyrotechnist" whose article started "an astonishing display of fireworks"⁵⁸ beginning with Zhebrak's trial in front of the honor court. Moreover, Zirkle surmised that Laptev's article might have been the first step in a process of deterioration, ending with the Soviet regime's decision in 1948 to suppress Mendelian genetics.⁵⁹ Therefore, the outcome from efforts by Dunn and his friends, according to Zirkle's interpretation, accomplished the opposite of what they intended. They had attempted to undermine Lysenko's Michurin biology by disputing his scientific claims in American publications; however, Lysenko won

⁵⁷ Zirkle reprinted Zhebrak's recantation, pages 277-79.

⁵⁸ Zirkle, 1949, 49-50. The statement about Laptev made by Zirkle is from page 50. Krementsov states that Laptev should not be seen as the beginning of the end because the party officials did not allow Zhebrak and Dubinin to receive criticism. Rather, the Laptev article and repercussions demonstrate the Party's growing influence over the scientific community (1997, 149).

⁵⁹ Zirkle, 50. Zirkle concludes: "Perhaps no controversy has ever been brought to a conclusion as decisively as this one. When Laptev had finished there was nothing that anyone else could say. The incident was closed. But the incidence itself may have been a trigger which set off a greater reaction."

undisputed control of biology in the Soviet Union by August 1948.⁶⁰ Moreover, Zhebrak personally suffered after his trip to the United States when efforts made by American geneticists in hopes of helping Zhebrak and other Soviet geneticists backfired.

Zirkle's perspective is important since he interacted with Dunn, Muller, and Dobzhansky in the United States and with geneticists in other locales in order to compile his book. Whether or not the Dunn-Sax-Zhebrak debate had the powerful effect in the Soviet Union to which Zirkle ascribed it, those outside of the Soviet Union seemed to have thought that it did. Dunn and his network, therefore, most likely felt responsible, to some degree, for the decisive overturn of Mendelian genetics that occurred in the Soviet Union in 1948.

Dunn continued to address the Lysenko controversy, especially in 1948 and 1949, and as mentioned he contributed to Zirkle's book, Death of a Science in Russia. Between September and December 1948, which was immediately after the Soviet edict against Mendelian genetics was passed, Dunn attempted to translate and publish information on the events as they had been recorded in Pravda. He asked G. Ledyard Stebbins, Jr. his opinion because Stebbins had been reading the articles. Stebbins had similar plans and they corresponded about the project. During this time, Zirkle contacted Dunn because he planned to put together his book, as mentioned above. Dunn gladly allowed his papers to be reprinted and expressed to Zirkle his opinion on what major questions remained to be answered.

⁶⁰ Krementsov, 1997, 146-49.

The attitude of the Soviet government toward science has been clearly expressed and made evident in budgets, buildings and every form of physical support. Some treatments of this case would have us believe that all that changed over night. I don't believe it has, but the important question remains – what are the mainsprings of action in this particular case?

Dunn and Stebbins chose to let Zirkle handle the project and Stebbins gave his information from Pravda to Zirkle, who included it in his book.⁶¹

Dunn sent a copy of his letter to Dobzhansky, who had a definite answer to Dunn's question, "what are the mainsprings of action in this particular case?"

Dobzhansky summarized it as a case of "frauds backed by political chikanery [sic]" and then described the Soviet situation in fuller detail:

...a contemptible cheat has not only obtained backing for his prescientific and at best 19th century ideas, but has also succeeded in murdering some and bouncing other scientists who were doing first class work and who dared to oppose his charlatanism. This is the core of the situation, and all else is materials for dissertations of future historians. Well, I am all in favor of giving food to future historians...

Unsure, but pretty certain that Dunn wanted to continue speaking cautiously about the events in the Soviet Union, Dobzhansky had decided that it was time to stop "pussyfooting" and time to start showing respect for those who had lost either their livelihood or lives as a result of Lysenko's ascension. The events called for "violent language," and no matter how opposed Dunn was to the idea, Dobzhansky had decide

⁶¹ APS, Dunn Papers, Box 1, American Council of Learned Societies, 1948, Dunn to Mr. Mortimer Graves of ACLS, 15 Sep 1948; APS, Dunn Papers, Box 21, Lysenko Controversy-Correspondence, 1948-1953, two letters from Dunn to Stebbins, 6 Oct and 18 Nov 1948, two letters from Stebbins to Dunn, 8 Oct and 15 Nov 1948, Zirkle to Dunn, 1 Dec 1948, Dunn to Zirkle, 6 Dec 1948, Stebbins to Zirkle, 8 Dec 1948. Quotation from Dunn is in his letter to Zirkle on 6 Dec 1948.

to finally speak his mind.⁶² Their united campaign against Lysenko ended after Lysenko won.

In the years after the Soviet regime chose Lysenko's Michurin biology over Mendelian genetics, Dunn and his colleagues discussed this outcome publicly. Krementsov uses the term "second front" to describe American geneticists' efforts to undermine Lysenko's prestige in the years immediately prior to the 1948 decision. As discussed above, during this time, Dunn, Muller, Dobzhansky and other geneticists organized their efforts and used a scientific approach of collecting and analyzing data in order to objectively discuss the matter. In giving their personal views more readily after 1948, these men no longer coordinated their efforts, but tried to come to terms with the situation in their own individual ways.

The main question asked post-1948 was: What factors contributed to the decision made by the Soviet regime? Dobzhansky, who had lived and trained in the Soviet Union before going to T.H. Morgan's laboratory in 1927, pinpointed an anti-genetics tradition in his country of birth dating from its disciplinary beginnings in the early twentieth century. Kliment Timiriazev avidly promoted evolution, but not genetics. In an article titled "Repulse of the Mendelians," Timiriazev called genetics reactionary, which caused others to doubt its validity. Ivan Michurin later reinforced Timiriazev's distaste for Mendelian genetics. It was pointed out to Michurin that genetics disproved some of his scientific beliefs, such as his contention that newer

⁶² APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #6, 1948-1949, Dobzhansky to Dunn, 12 Dec 1948.

varieties of fruit trees have greater genetic flexibility than older varieties. The newer variety had the ability to adapt to environmental influences until after its first year of bearing fruit. Michurin responded by saying that he was right and genetics was wrong. Lysenko credited Michurin with formulating the basis of his ideas and according to Dobzhansky, Lysenko's success in the Soviet Union rested on two factors. First, the regime revered Russian intellectual tradition and considered Russian views infallible. Second, Lysenko exploited the regime's devotion to nationalism in order to promote himself politically.⁶³ As years passed, events in the Soviet Union convinced Dobzhansky of the validity of his opinion. In 1952 he again stated that the success of Lysenkoism boiled down to two factors. The first explanation built on his earlier statements: the Soviet regime distrusted Mendelian genetics and based their views on ideas from Timiraizev and Michurin. His second argument took a long-term assessment of how the Soviet Union had changed over time. Dobzhansky demonstrated that after World War II ended, the Soviet Union grew increasingly nationalistic. Not only did the regime insist on using only their language in scientific journals, but they also sought examples to demonstrate their country as being the first to accomplish major feats. For Dobzhansky, the regime's shift in the years following the war helped explain the allure of Lysenko's views.⁶⁴

Dunn and Muller had presented coordinated responses prior to 1948, whereas after the Soviet Union clamped down on Mendelian genetics they approached the

⁶³ Dobzhansky, 1949, 144-46.

⁶⁴ Theodosius Dobzhansky, "Lysenko's 'Michurinst' Genetics," Bulletin of the Atomic Scientists 8 (Feb 1952): 40-44.

situation differently. They agreed on two basic factors contributing to the decision made in the Soviet Union. Politics guided the outcome, and Lysenko's "science" was not scientific. Muller did not believe that anything more could be done for biology and biologists in the Soviet Union, and he called for an "autopsy" to determine the cause of death. Muller blamed Soviet politicians, who since 1936 had orchestrated the end of genetics by holding scientific debates in which politicians decided the outcomes.⁶⁵ Muller's need to condemn Soviet politicians did not interest Dunn, who wanted to discuss the problem rather than point fingers. Dunn hoped to understand how the Soviets could make an absolute decision to the detriment of agriculture after decades of trying to erect a society based on science.⁶⁶ Dunn believed that political reasons dictated the decision made by the Soviet regime and noted that Lysenko's claims fit well with socialism's aim to change society quickly.⁶⁷ Both men noted that

⁶⁵ Muller's biographer, Elof Axel Carlson, noted that Muller's tone in this article caused him trouble because his detractors, some of whom sided with Lysenko, felt that he should have used a more scientific approach. Muller decided that he had to improve on how he conveyed his views (Carlson, 229-230). Hermann J. Muller, "The Destruction of Science in the USSR," The Saturday Review of Literature 31, 49 (4 Dec 1948): 13-15, 63-65; Hermann J. Muller, "The Crushing of Genetics in the USSR," Bulletin of the Atomic Scientists (Dec 1948): 369-71. The Bulletin of the Atomic Scientists' article is a shorter version of what appeared in The Saturday Review of Literature on 4 Dec 1948 (as cited above) and a second article published one week later: Hermann J. Muller, "Back to Barbarism-Scientifically," The Saturday Review of Literature 31, 50 (11 Dec 1948): 8-10.

⁶⁶ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy-Correspondence, 1948-1953, Dunn to Muller, 17 Feb 1949.

⁶⁷ L.C. Dunn, "Science and Politics in Russia," Bulletin of the Atomic Scientists (Dec 1948): 368, 383.

their personal relationship would remain warm and undisturbed even though their opinions differed about the Soviet situation.⁶⁸

Muller, according to Dunn, had lost some of his objectivity in reference to matters involving Lysenko and the Soviet Union. Muller drew from his personal experiences from talking with Soviet biologists and from living for several years in the Soviet Union. Dunn in comparison to Muller had spent only ten days in the Soviet Union, and used only published materials to dissect the scientific problems pertaining to the Lysenko controversy.⁶⁹ Dunn indeed continued his scientific, objective examination in search of answers to unsolved questions about the demise of genetics in the Soviet Union. In May 1949 the Bulletin of the Atomic Scientists devoted an entire issue to “The Russian Purge of Genetics” in which Dunn argued that nothing could be gained by condemning the science performed in the Soviet Union.⁷⁰ He had by this point changed his mind about dissecting Lysenko’s scientific claims and demonstrating their erroneousness. He instead urged scientists to consider why the Soviet Communist Party supported only Lysenko’s genetics and what this situation could tell Americans about the Soviet Union’s political system. In his estimation, the answer to this perplexing development lay in “understanding how, in a revolutionary

⁶⁸ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1948-1953, Muller to Dunn, 17 Jan 1949 and Dunn to Muller, 17 Feb 1949.

⁶⁹ APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1948-1953, Dunn to Zirkle, 6 Dec 1948.

⁷⁰ Other geneticists who contributed to the Bulletin of the Atomic Scientists’ volume “The Russian Purge of Genetics” were Dobzhansky, Karl Sax, Sewall Wright, M.B. Crane, and Richard B. Goldschmidt.

state, all parts of life are connected by a political lifeline.”⁷¹ In other words, Dunn hoped an analysis of party politics would shed some light on the subject.

Many years later, Dunn still had questions about the fate of genetics in the Soviet Union. In light of the Soviets’ recent accomplishments, most notably Sputnik, Dunn wanted to know why some sciences thrived and others were condemned.⁷² The events that occurred in 1948 in the Soviet Union remained unsettled in Dunn’s mind for many years; however, by 1950 he seemed to have resolved that Soviet genetics should be left in the past. That year, the Genetics Society of America honored the 50th anniversary of the rediscovery of Johann Gregor Mendel’s work. Dunn, as editor of a volume produced from this Golden Jubilee, stated that a conscious decision had been made to discuss only productive disciplinary developments. Moreover, he called attention to the scientific validity and importance of Mendelian genetics as opposed to hereditary theories valued for their non-scientific features.

It was clearly the intent of the Genetics Society of America to survey only the constructive events in the development of this science. An examination of the factual foundations and applications of genetics would be of service everywhere regardless of their political palatability.⁷³

⁷¹ L.C. Dunn, “Motives for the Purge,” Bulletin of the Atomic Scientists 5 (May 1949): 142-43. Quotation is from 143. Dunn made similar statements prior to this article in a letter to Richard Morford, Executive Director of the National Council of American-Soviet Friendship: APS, Dunn Papers, Series I, Box 21, Lysenko Controversy – Correspondence, 1948-1953, Dunn to Morford, 14 Jan 1949.

⁷² L.C. Dunn, “A Firsthand Account of the Lysenko Quackery,” rev. of The Rise and Fall of T.D. Lysenko by Zhores A. Medvedev, trans. by I. Michael Lerner, The Journal of Heredity 60 (May-Jun 1969): 110-112.

⁷³ L.C. Dunn, “Introduction,” Genetics in the 20th Century: Essays on the Progress of Genetics during its First 50 Years, ed. L.C. Dunn (New York: The Macmillan Company, 1951): ix-xi, ix.

Dunn undoubtedly conveyed his point without mentioning the Soviet Union or Lysenkoism.

Dunn never really reconciled himself to what happened to Soviet genetics and geneticists. As late as 1965, when he published A Short History of Genetics he virtually ignored the Soviet Union. This book covered the period from 1864 to 1939, the timeframe during which Soviet genetics thrived; and yet, he rarely discussed genetic developments in the Soviet Union. What makes this surprising is that Dunn gave an oral history at about the same time that he was writing the book, in which he extensively discussed the history of genetics in the Soviet Union and United States. A scholarly explanation for why he chose to limit his discussion about Soviet genetics in the book could be that he did not have access to the materials necessary to write an accurate history. However, as former chairman of the American-Soviet Science Society, Dunn knew about the information received from Soviet geneticists and where to retrieve these scientific documents. On a more personal level, it could have been that he continued to discuss only “constructive” genetics.

Saving the American-Soviet Science Society

During its third year of operation in 1946, decisions made by employees of the United States Treasury incapacitated the American-Soviet Science Society, and in 1948 the Science Society’s members regretfully announced its termination. Several events occurring in the United States precipitated the American-Soviet Science

Society's end, such as a controversy involving a Rockefeller Foundation grant, the House Un-American Activities Committee's investigation of physicist E.U. Condon, and the Science Society's need for a competent leader. Moreover, members of the American-Soviet Science Society recognized that their troubles stemmed from American apprehensions about communist countries and Soviet sympathizers, and undoubtedly the political atmosphere that developed in the United States after World War II contributed greatly to the Science Society's decision to disband.

Diplomatic relations between the United States and Soviet Union started declining in 1946 and by late 1948 the atmosphere in the United States had changed dramatically. During this timeframe, a shift occurred in Congress from a Democratic to Republican Party majority and the Popular Front crumbled. Americans' reservations coalesced in early 1947 as a result of the Truman Doctrine, which responded to Britain's decision to pull out of Greece and Turkey. Not wanting the Soviet Union to broaden its sphere of influence, Truman proposed a plan for Americans to aid the two countries. In order to get support for his plan, Truman and his backers instilled fear into Americans about the threat of the Soviet Union and Communists. Americans grew more conservative in response to fears of the Soviet Union and anti-communism grew much stronger by late 1947. For example, the House Committee on Un-American Activities received a four-fold budget increase between 1945 and 1948.⁷⁴ The American-Soviet Science Society offers a unique

⁷⁴ Mary Sperling McAuliffe, Crisis on the Left: Cold War Politics and American Liberal, 1947-1954 (Amherst: The University of Massachusetts Press, 1978): 22-34;

mode for examining effects of early cold war politics on a scientific organization and scientists. Ultimately, a history of the Science Society demonstrates a crisis developing in the United States as a result of international affairs. Moreover, a fundamental difference of perspectives present since World War II intensified. Scientists tried to hold on to their traditional views of science based on internationalism, while the US government established new rules based on military procedures and confidentiality.⁷⁵

Before Dunn left for his sabbatical leave in June 1946, a Nominating Committee of the American-Soviet Science Society attempted to find someone to replace him as Chairman. Four long-time members were appointed to the Nominating Committee: Dunn, Carl O. Dunbar, A.E. Mirsky, and Ignace Zlotowski.⁷⁶ Members of the Science Society's Executive Committee asked physicist E.U. Condon twice to succeed Dunn, but both times Condon refused because of his governmental position with the National Bureau of Standards.⁷⁷ The committee unanimously nominated

M.J. Heale, American Anti-communism: Combating the Enemy Within, 1830-1970 (Baltimore: The Johns Hopkins University Press, 1990): 132-44.

⁷⁵ Jessica Wang and Nikolai Kremontsov have examined the effects of cold war politics on scientists. Wang focused on physical scientists in American Science in an Age of Anxiety. Kremontsov in Stalinist Science studied how the situation affected Soviet scientists, especially geneticists.

⁷⁶ APS, Demerec Papers, National Council of the American-Soviet Friendship, Inc., Science Committee (Folder 3), "Minutes of the Executive Committee Meeting of the American-Soviet Science Society, 22 Jan 1946: 5 pages, 5.

⁷⁷ APS, Dunn Papers, Series I, Box 5, Condon Case – Related Materials, 1946-1952, Dunn to Carl Dunbar of Dept. of Geology at Yale University, 28 March 1946.

chemist Irving Langmuir of General Electric Company.⁷⁸ Langmuir, only two years away from retirement, had a solid scientific reputation with his many accolades including the 1932 Nobel Prize in Chemistry.⁷⁹ He traveled with the Science Society's delegation to Moscow in 1945 for the 220th Anniversary of the Russian Academy of Sciences and regularly wrote articles endorsing collaboration with Soviets. Dunn noted to Langmuir the importance of filling the chairmanship "during the present critical time,"⁸⁰ but Langmuir could not be persuaded. Meeting minutes have no information about who took over the post, and during Dunn's last meeting there was no discussion of his successor.⁸¹ Dunn, later, remarked that the Science Society appointed a three-person Steering Committee of Duncan MacInnes, Alfred E. Mirsky, and Theodore Shedlovsky. All three worked at the Rockefeller Institute and

⁷⁸ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 25 Apr 1946: 3 pages, 1.

⁷⁹ Charles Süsskind, "Irving Langmuir," *Dictionary of Scientific Biography* 8, ed. Charles C. Gillispie (New York: Charles Scribner's Sons, 1973): 22-25.

⁸⁰ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society Correspondence, 1945-1946, n.d., Dunn to Langmuir, 18 April 1946.

⁸¹ APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 3), "Minutes of the Executive Committee Meeting," 28 May 1946: 2 pages, 2.

were long-time members of the Science Society.⁸² Physical chemist Duncan MacInnes eventually acceded to the presidency out of necessity.⁸³

The timing of Dunn's departure could not have been much worse for the Science Society. The organization needed someone strong and determined to guide it through the upcoming months fraught with a steady decline of diplomatic relations between the two countries. Josef Stalin and President Harry S Truman had met multiple times in the latter half of 1945 to devise a post-war structure. Relations between the two nations had become strained and cooperative diplomatic relations were no longer an option by January 1946 when Dunn announced his upcoming sabbatical. By fall the situation between the United States and Soviet Union had developed into its future antagonistic course, and the Science Society would suffer domestic ramifications resulting from this breakdown in international affairs.⁸⁴ When Dunn left on his sabbatical in June 1946 he said to his friend and confidant, Walter Landauer, that he feared the upcoming, post-war era more than the one that had just

⁸² RAC, RF 1.2 200 D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 12 Feb 1947. Theodore Shedlovsky, the most recent member, joined the Executive Committee in January 1945 (APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Meeting of the Science Committee of the National Council of American-Soviet Friendship," 17 Jan 1945: 4 pages, 1).

⁸³ L.C. Dunn, "Position of Science Society," New York Times (19 Mar 1948): 22.

⁸⁴ David Holloway, "The Atomic Bomb and the End of the Wartime Alliance," The Rise and Fall of the Grand Alliance, 1941-1945, eds. Ann Lane and Howard Temperley (New York: St. Martin's Press, Inc., 1995): 207-25, 218-25; Norman A. Graebner, "Yalta, Potsdam, and Beyond: The British and American Perspectives," The Rise and Fall of the Grand Alliance, 1941-1945, eds. Ann Lane and Howard Temperley (New York: St. Martin's Press, Inc., 1995): 226-54, 245-46.

passed.⁸⁵ Troubles, indeed, were about to begin for Dunn and the American-Soviet Science Society.

The Science Society finally thought that their financial problems were resolved after receiving a \$25,000 grant from the Rockefeller Foundation for one year starting 1 July 1946. Executive Committee members planned to spend the bulk of the grant to develop the Science Bulletin into a journal called the American Review of Soviet Science and the remaining money on secretarial assistance and office expenses.⁸⁶ Dunn in his letter requesting the grant reassured Warren Weaver about the American-Soviet Science Society's intentions. He explained that the Science Society conducted scientific exchanges of a "purely objective and scientific nature" in hopes of establishing better relations between the two countries, and that their organization had already developed good relations with groups in the Soviet Union. He also mentioned that the Science Society had no connections to political organizations in either country, and ultimately expressed the Society's impartiality: "We know that science is the safest bridge over which international exchange and understanding can pass and we want to guard the neutral character of the bridge." Dunn expected the grant to be temporary because he hoped that an organization, such as the United Nations, would eventually undertake the services provided by the

⁸⁵ APS, Dunn Papers, Series I, Box 19, Walter Landauer 1946, Dunn to Landauer, 28 April 1946.

⁸⁶ The Rockefeller Annual Report 1946 (New York: Rockefeller Foundation, 1946): 169-70; APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 3), "Minutes of the Executive Committee Meeting" of the American-Soviet Science Society, 28 May 1946, and Norma S. Thompson, Secretary of the Rockefeller Foundation to Dunn, 21 Jun 1946.

Science Society.⁸⁷ Dunn had not yet left for his sabbatical when the Society learned that it had received the grant, and he most likely felt assured about the Science Society's future knowing that the Rockefeller Foundation had approved the application.⁸⁸

Before the Science Society's members could gain access to their grant, they needed the United States Treasury to give them tax-exemption status. The Science Society had been a tax-exempt organization while associated with the Friendship Council, but effective 1 April 1946 the Executive Committee of the American-Soviet Science Society chose to break all ties with the National Council of American-Soviet Friendship. Science Society members wanted their group to remain clear of politics and feared that affiliation to the increasingly political Friendship Council might impinge on their organization's reputation.⁸⁹

⁸⁷ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 24 May 1946. At the last Executive Committee meeting of the Science Society that Dunn attended before leaving for sabbatical, he made a similar remark about the Science Society's temporary need. He envisioned that UNESCO or the National Science Foundation would take over the Society's responsibilities at a future date (APS, Demerec Papers, National Council of American-Soviet Science Society, Inc., Science Committee (Folder 3): 2 pages, 1.

⁸⁸ The Science Society stopped their activities during the summers. Dunn left New York in early summer, but did not leave the United States until February due to illness. News about the grant from the Rockefeller Foundation came in late June: APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 3), Norma S. Thompson, Secretary at Rockefeller Foundation, to Dunn, 21 June 1946. Dunn informed Weaver that he was well enough to go to Uppsala: RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 12 February 1947.

⁸⁹ APS, Dunn Papers, Series I, Box 2, American-Soviet Science Society, 1944-1946, n.d., "Minutes of the Executive Committee Meeting," 25 April 1946; RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Note in

Shortly after the Science Society became independent, the Executive Committee submitted the grant to the Rockefeller Foundation.⁹⁰ Weaver told Dunn that the Science Society had to receive tax-exemption status before getting money from the Rockefeller Foundation, to which Dunn replied that he had no problems making the grant contingent on the Society receiving tax-exemption. Dunn figured that the US Treasury would act on the Science Society's application "in due course." Weaver double-checked whether or not the Science Society was a good investment by contacting the State Department. Mr. Charles E. Thompson replied that the State Department had "no objections whatsoever to the grant."⁹¹

News that the American-Soviet Science Society received a grant from the Rockefeller Foundation remained confidential, as was desired by the grantor and grantee, for about eight months. Then in February 1947, the grant became front-page news in the New York Times. The anonymous author in a fairly objective article noted that the American-Soviet Science Society remained the sole vehicle by which scientific information passed between the two countries. Whether or not the author hoped to stir up some controversy is not clear. However, mention was made that the

Weaver's diary, 14 May 1946: "They [the American-Soviet Science Society] were associated with the National Council of American Soviet Friendship. This Council is supposed to be concerned with cultural affairs but they got over into the political field. The American Soviet Science Society broke off relations with the National Council fearing that they would get an unfortunate political flavor."

⁹⁰ APS, Demerec Papers, National Council of American-Soviet Friendship, Inc., Science Committee (Folder 3), "Minutes of the Executive Committee Meeting" of the American-Soviet Science Society, 28 May 1946: 2 pages, 1.

⁹¹ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 24 May 1946; Weaver to Mr. Charles E. Thompson of the State Department, no date; Dunn to Weaver, 19 June 1946.

Science Society had been dormant while awaiting a response from the US Treasury, which was expected within days.⁹²

Dunn received a copy of the newspaper article and wrote to Weaver from Arizona expressing his strong views and feeling.⁹³ Dunn appreciated the role that the National Council of American-Soviet Friendship played in developing the Science Society, and therefore desired for nothing bad to befall the Friendship Council. Dunn had resigned from the Friendship Council's Board because he disliked the Board members' decisions on their future direction. They chose to improve American-Soviet relations through mass education of the public instead of using slower methods, such as those performed by the Science Society. Dunn, however, stayed associated with and appreciative of the Friendship Council. Americans in general, according to Dunn, conflated the Soviet Union with communism and persecuted every American-Soviet group no matter their intentions. Dunn viewed the situation as one in which misrepresentation caused the abandonment of worthy enterprises in the United States. This angered Dunn, who told Weaver that he would stand up for the Friendship Council should the situation escalate.⁹⁴ Dunn left for Scandinavia about two weeks later and distanced himself from this matter until his return in the fall.

⁹² "Rockefeller Gift Helps Scientists Open Door to Russian Knowledge," New York Times (8 Feb 1947): 1, 10.

⁹³ Although Dunn left for sabbatical in May 1946, he postponed his departure for Europe due to illness.

⁹⁴ RAC, RF. 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Dunn to Weaver, 12 Feb 1947.

Publicity remained steady, but relatively benign for the next year. Dunn got involved after his return when the furor started again.

Warren Weaver, who was quoted in the article, tried to explain to Dunn how someone learned about the grant. The reporter had not mentioned that he was a journalist, knew a fair amount about the grant already, and was persistent in getting information. At some point in his conversation with the reporter, Weaver decided that it would be better to talk frankly with the man and to explain the value of the deeds accomplished by the American-Soviet Science Society. He told the man to contact the Science Society for more information. The reporter found no one associated with the Science Society at its office because the organization was relatively inactive at this time. Moreover, Dunn could not be found for comment because only a few of Dunn's closest friends knew his whereabouts. Inability to contact anyone from the Society made the story even more sensational to the journalist, according to Weaver.⁹⁵

The Science Society waited almost one year before receiving a response from the Treasury Department, and during the interim members of the Science Society considered alternative solutions. They hired lawyers and got their society incorporated in early 1947, which meant that they could legally accept contributions from individuals and corporations. Weaver, however, reiterated that they could not access the Rockefeller Foundation's grant until their organization had proper tax-

⁹⁵ APS, Dunn Papers, Series I, Box 26, Warren Weaver, 1933-1955, Weaver to Dunn, 19 Feb 1947. Others found this story intriguing and later sought Weaver for comment. He mentioned that he used the same candid approach with reporters from Time and the Tribune contacted him.

exemption status.⁹⁶ Hoping “to revive and catalyze” the American Soviet Science Society, Harlow Shapley suggested that its members consider associating with the “non-political” American-Russian Institute. The American-Russian Institute had tax-exemption status and would allow the Science Society to act autonomously.⁹⁷ MacInnes disliked Shapley’s suggestion because he feared that their organization would be expected to take on projects outside of their purview. MacInnes noted that they were “actively” trying to get tax exempt status and would consider Shapley’s suggestion at a later date if the need arose. MacInnes accepted and seemed almost complacent about the Treasury’s delay,⁹⁸ but as time passed his ire grew.

The US Treasury finally responded to the Science Society’s application in April 1947, but had not acted on it. In his letter E.I. McLarney, Deputy Commissioner of the United States Treasury Department, noted fairly new developments to explain the continuing delay. The Treasury viewed the Science Society as a communist-front group, a decision based on second-hand information.

It appears from newspaper articles recently published that the Committee on Un-American Activities of the House of Representatives proposes to investigate the matter of whether your activities and those of certain of your leaders are detrimental to the interests of the United States. Under these circumstances a definite

⁹⁶ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Rabkin & Johnson to the American-Soviet Science Society, 11 Feb 1947 and Weaver to Gerald Oster, 24 Mar 1947.

⁹⁷ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Harlow Shapley to Weaver, 9 Mar 1947.

⁹⁸ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, MacInnes to Weaver, 24 Mar 1947 and 16 Apr 1947.

ruling on your status for Federal income tax purposes is being deferred pending further development of facts.⁹⁹

The recent newspaper articles mentioned in the Treasury's letter were those accusing Edward U. Condon of espionage.¹⁰⁰ Condon's situation with reference to the Science Society will be examined extensively following a discussion about the Science Society's actions in response to McLarney's letter.

The US Treasury's reply provoked members of the Science Society's Executive Committee to consider their next move. The Treasury Department's letter "shocked" chemist I.M. Kolthoff of University of Minnesota, who found it alarming that the Treasury based its decision on unfounded suspicions. He suggested that the Science Society ask for help from scientific organizations engaged in social and political matters, such as the American Association for the Advancement of Science, the American Association of Scientific Workers, and the Federation of American Scientists. Carl O. Dunbar, Director of the Peabody Museum of Natural History at Yale University considered the government's antagonistic policy toward the Soviet Union "a tragic mistake." Dunbar missed the meeting at which the Executive Committee discussed joining their organization with the American-Soviet Medical

⁹⁹ APS, Dunn Papers, Series I, Box 24, Science Bulletin 1945-1946, E.I. McLarney, Deputy Commissioner, Treasury Department to American-Soviet Science Society, 28 April 1947.

¹⁰⁰ Jessica Wang has noted that in March 1947 the Washington Times-Herald accused Condon of having communist ties. Listed among his dubious activities was his association with the American-Soviet Science Society (Wang, 1999, 131). These two articles were: William Odlin, Jr., "Condon Duped into Sponsoring Commie-Front Outfit's Dinner," Washington Times-Herald (23 Mar 1947): 1, A-2; William Odlin, Jr., "Condon Facing U.S. Probe into Soviet Society Affiliation," Washington Times-Herald (25 Mar 1947): 2.

Society. He opposed the idea stating that the Science Society members should wait and hope for the Treasury Department to provide their organization with tax-exempt status. Merging the two organizations, according to Dunbar, was disadvantageous because the range of topics covered by the two organizations would have to be compromised.¹⁰¹

A few members of the Science Society's Executive Committee eventually met with someone from the US Treasury Department to discuss the situation. MacInnes felt that they had conveyed the value of the American-Soviet Science Society, but correctly ascertained that the Treasury would not act on their tax-exempt application as a result of the meeting.¹⁰² Dunn talked with Warren Weaver after returning from his sabbatical to discuss the situation impinging on the Science Society's ability to function. The matter greatly upset Dunn, whom Weaver described as "indignant." By this point MacInnes had also become quite riled. Members of the Science Society decided to force the Treasury to make a decision on their tax-exemption application "as a matter of principle." Dunn knew that whatever the outcome of their application, the situation in the United States would force the Science Society to end because they

¹⁰¹ APS, Dunn Papers, Series I, Box 24, Science in the USSR, 1944-1948, I.M. Kolthoff to Gerald Oster, 21 May 1947 and Carl O. Dunbar to Gerald Oster, 28 May 1947.

¹⁰² RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes by Warren Weaver after talking with Duncan MacInnes, 17 Jun 1947. Four men from the Science Society, Duncan MacInnes, Stuart Mudd, Gerald Oster, and Watson Davis, attended a hearing with Mr. Hill of the US Treasury Department on 12 June 1947.

could not accomplish their goals in light of the current political atmosphere.¹⁰³ In addition to the problem of increased caution in the United States, the Soviet Union had passed a decree a few months earlier in which sharing confidential scientific information held a weighty punishment of eight to twenty years in a labor camp.¹⁰⁴

The American-Soviet Science Society grant created several problems for Warren Weaver and the Rockefeller Foundation. Raymond B. Fosdick of the Rockefeller Foundation wanted to cancel the grant with the Science Society and proposed that the Foundation never again give a grant to an organization without tax-exemption status. Members of the Science Society used the Rockefeller Foundation's acknowledgment as proof of their group's worthiness in discussions with the US Treasury, which angered Fosdick. Furthermore, Fosdick did not know what to believe about the Science Society's alleged political associations with the Soviet Union and Communism. He found it as easy to trust MacInnes, who said that the Science Society did not engage in communist activities, as it was to believe those espousing the opposite opinion.¹⁰⁵ Weaver wanted to let events play their course rather than take action because in his opinion things would most likely develop beneficially for the

¹⁰³ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes by Warren Weaver after talking with L.C. Dunn, 3 Nov 1947.

¹⁰⁴ Kremmentsov, 1997, 141, 187. The USSR Supreme Soviet announced on 14 June 1947 its edict, "On Responsibility for Disclosure of State Secrets," which had a specific section on scientific work. Americans knew of the decree by the end of June.

¹⁰⁵ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Raymond B. Fosdick to Weaver, Inter-office Correspondence, 7 Oct 1947. Fosdick did not name any of the people who said that the Science Society had communistic associations.

Rockefeller Foundation. Therefore, Weaver suggested that the Foundation allow the grant to expire on 30 June 1948, and he told Fosdick that he would dissuade the Science Society from taking the money should the situation present itself. He believed this to be a remote possibility.¹⁰⁶

Physicist E.U. Condon got involved in many of the same organizations as Dunn during the 1930s and early 1940s. He was affiliated, but not particularly active as an Executive Committee member of the American-Soviet Science Society, and with the American Committee for Democracy and Intellectual Freedom. Unlike Dunn, Condon worked on the Manhattan Project at MIT's Radiation Laboratory and in New Mexico and was later employed by the United States government. Condon acted as associate director at Los Alamos for a few weeks in 1943 before leaving because of a fundamental difference of opinion with the Manhattan Project's coordinator, General Leslie Groves. Groves enforced stricter security measures after joining the project in late 1942 and Condon disliked Groves's stringent regulations.¹⁰⁷ The feud between Groves and Condon caused Condon trouble years later. As

¹⁰⁶ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Weaver Inter-office Correspondence to RBF, 28 Oct 1947. Weaver notified MacInnes about the grant's extension for one year (RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Weaver to MacInnes, 19 June 1947). Weaver also worked to exonerate Condon, and grappled with his desire to help a personal friend while respecting his role as an employee of the Rockefeller Foundation. Weaver wrote Condon a personal letter saying that the Rockefeller Foundation could do little in response to his case with J. Parnell Thomas and HUAC, but that he would do what he could in a personal way. (RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Weaver to Condon, 1 Aug 1947).

¹⁰⁷ Wang, 1999, 133.

mentioned in the previous chapter, Groves stopped Condon from attending the 220th Anniversary of the USSR Academy of Sciences during the summer of 1945.¹⁰⁸ Henry A. Wallace personally selected Condon to oversee the United States Bureau of Standards, a position that he held from 1945 to 1951.¹⁰⁹ Condon's strong friendship with Wallace also created problems for him later because Wallace was portrayed as trying to revitalize communism when he ran as a third party candidate for President of the United States in 1948.¹¹⁰

Condon's association with the American-Soviet Science Society was the main piece of evidence against him.¹¹¹ Newspaper articles began appearing in March 1947 and escalated over the next year. J. Parnell Thomas, Chairman of the House Un-American Activities Committee, made explicit accusations about Condon's affiliation with the Friendship Council and Science Society in June 1947. Condon had previously sent a form letter to some of his co-workers at the National Bureau of Standards inquiring whether they wished to support the American-Soviet Science Society's endeavors. Thomas cited this letter as Condon's attempt to dupe

¹⁰⁸ Thomas C. Lassman, "Government Science in Postwar America: Henry A. Wallace, Edward U. Condon, and the Transformation of the National Bureau of Standards, 1945-1951," *Isis* 96 (2005): 25-51, 35. As mentioned in the previous chapter, Dunn had asked Condon to act as the American-Soviet Science Society's representative while in Moscow.

¹⁰⁹ Lassman, 27; Wang, 1999, 133.

¹¹⁰ Heale, 141-44.

¹¹¹ Wang, 131-38.

unsuspecting Americans to take part in a communist organization.¹¹² MacInnes described Thomas's articles as "very inaccurate and unfair."¹¹³

Members of HUAC believed that the Science Society retained its associations with the Friendship Council and used their alleged relationship as evidence to incriminate Condon of espionage. Accusations against Condon appeared on the front page of the New York Times on 2 March 1948 with quotation of the following statement released by HUAC:

There is no evidence in our possession that Dr. Condon is a member of the Communist party. But as a member of the executive committee of the American-Soviet Science Society, which is affiliated with the National Council of American-Soviet Friendship, Inc., and which was recently cited as a subversive organization by Attorney General Clark, he has lent his name and influence to one of the principal Communist endeavors in the United States.¹¹⁴

Dunn tried to salvage the reputations of E.U. Condon and the American-Soviet Science Society by responding to the accusations against Condon. He pointed out that the above quotation from HUAC had three details wrong.

(1) The American-Soviet Science Society has no connection with the National Council of American-Soviet Friendship; (2) it has not been cited as a subversive group by the Attorney General; (3) it has at no time had anything

¹¹² RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, MacInnes told Weaver about two articles, which Weaver recorded in his interview notes after talking with Duncan MacInnes, 17 Jun 1947. J. Parnell Thomas, "Reds in our Atom Bomb Plants," Liberty (32 Jun 1947); J. Parnell Thomas, "Russia Grabs out Inventions," American Magazine (Jun 1947). Wang also mentions Thomas's articles on page 131.

¹¹³ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes by Warren Weaver after talking with Duncan MacInnes, 17 Jun 1947.

¹¹⁴ William S. White, "Soviet Spy Links Laid to Dr. Condon, High Federal Aide," New York Times (2 March 1948): 1, 3.

to do with any “Communist endeavors” or with any except strictly scientific purposes.

Dunn was livid about the accusations against Condon and the Science Society as well as about situation in general, and he did not hold back his opinions. Dunn, thus, criticized the governmental agency’s interference and declared that “...the Committee on Un-American Activities must be restrained or abolished before it does irreparable harm not only to the reputation of a fine scientist [Condon] who is serving his country but to the whole future of science in the United States.”¹¹⁵ As noted by Dunn, the American-Soviet Science Society was never on an official list.¹¹⁶ The Attorney General and House Un-American Activities Committee had however made official statements in 1947 against the National Council of American-Soviet Friendship, which was the Science Society’s former parent group. In the end, the American-Soviet Science Society and some of its members suffered the same fate as many people during the McCarthy era: guilty by association.

Dunn’s article in support of Condon created additional problems for himself and the American-Soviet Science Society. A respondent pointed out that the Manhattan Telephone Directory listed the same phone number and address for the Science Society and Friendship Council.¹¹⁷ Other newspaper articles appeared elsewhere, in which the authors attacked Dunn personally. One article supplied a long

¹¹⁵ L.C. Dunn, “The Case of Dr. Condon,” New York Times (6 March 1948): 12.

¹¹⁶ President Truman’s Executive Order 9835 of March 1947 called for loyalty oaths for employees of the United States government and asked the Attorney General to develop a list of subversive groups within the United States (McAuliffe, 27).

¹¹⁷ Lambert Fairchild, “Coincidence of Telephone Listing,” New York Times (18 Mar 1948): 26.

list of alleged Communist organizations with which Dunn was affiliated, such as the American Committee for Democracy and Intellectual Freedom, the American Committee to Save Refugees, and the American-Russian Institute. Dunn was associated with all of these groups, but they were not Communist organizations. Another article written by journalist George E. Sokolsky questioned Dunn's statements and scientific capabilities. Sokolsky read Dunn's article and dialed 411 for updated telephone directory information. The Science Society, according to the operator, was still connected with the Friendship Council. Furthermore, Sokolsky sent a photographer to the Friendship Council's offices and produced evidence that the Science Society occupied one of the Friendship Council's three rooms.¹¹⁸ Dunn tried to clarify the misinformation. He explained the timing of the Science Society's disassociation from the Friendship Council and the Science Society's reasons for applying for the Rockefeller Foundation grant. Dunn telephoned the Friendship Council and asked to be connected with someone from the Science Society, and noted that he was told that the information in the phone book was outdated.¹¹⁹

Sokolsky's article and Condon's impending hearing before HUAC created a flurry of activity among Dunn, MacInnes, Condon, and Weaver. Dunn and MacInnes searched for documents among the American-Soviet Science Society's records that

¹¹⁸ APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Copies of two newspaper articles. One article had no citation information. It was printed by the King Features Syndicate, Inc., copyrighted 1948, and mentioned an upcoming meeting on 4 April 1948. The other article was: George E. Sokolsky, "Exposing a Defender of Dr. Condon," Columbus Evening Dispatch (16 Mar 1948).

¹¹⁹ L.C. Dunn, "Position of Science Society," New York Times (19 Mar 1948): 22. Jessica Wang briefly mentioned the dispute about the telephone number (137).

could be used during Condon's hearing to help exonerate him. Weaver poignantly summed up the problems facing Condon because of the Science Society's lackadaisical approach to running an organization.

It was always a part-time enthusiasm of individuals who were busy with their main jobs as scientists, and very inexperienced and awkward and foolish about handling the affairs of the Society, never stopping to think, apparently, that it was important to be very business-like and legal. It is humorous, and ridiculous, and tragic – but it seems doubtful that they even have any formal minutes recorded. The records are all mixed up – and of course Mr. [J. Parnell] Thomas can make a great successful howl over this fact. Condon said that those responsible for the Society have acted like damn fools – honest and innocent and well-intentioned damn fools, but still damn fools.

Dunn and MacInnes found records one day later establishing their disassociation from the Friendship Council in 1946. From Weaver's notes, it does not appear that they located the Science Society's Executive Committee meeting minutes.¹²⁰

Dunn supplied Condon's defenders with other information about the American-Soviet Science Society in an attempt to demonstrate the irrationality of invoking the Science Society in claims against Condon. Thus, Dunn provided Democratic Representative Chet Holifield of California with an historical statement about the Science Society. Holifield fought for Condon in the House of Representatives, and Dunn thanked him for making sure "that the true facts are known and that justice is done."¹²¹ Dunn also gathered letters that the American-

¹²⁰ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes made by Weaver from conversations with Condon and Dunn, 24 and 25 Mar 1948.

¹²¹ APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Dunn to Holifield, n.d.

Soviet Science Society had received after publishing its last Science Bulletin, which demonstrated the Society's worthiness. People wrote either requesting to receive the Science Bulletin or asking why they no longer received it.¹²² Dunn also coordinated his efforts with Gerald Oster, a physical chemist at the Rockefeller Institute for Medical Research and member of the Science Society's Executive Committee since late 1945. Oster spoke for the Science Society about Condon's case in order to alleviate the possibility of multiple members giving contradictory statements to the press.¹²³

Dunn also supported Condon in other ways. A.N. Richards, the President of the National Academy of Sciences, circulated a statement that he planned to send from members of the Academy (and not the Academy itself) in defense of Condon. The statement informed the government that no person should be portrayed as guilty before having a chance to respond to the accusations. Furthermore, it suggested that the government's treatment of Condon might alienate scientists and stop them from

¹²² APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Prof. Felix Rawitscher of the University of Sao Paulo, 13 June 1947; Haig Ajamian, Periodicals and Documents Division of Brooklyn College, 5 Dec 1946 and 10 Jan 1947; University of Illinois Library, 15 May 1947; Samuel A. Talbot, Johns Hopkins University's School of Medicine, 27 May 1947; Marguerite E. Smith of Oklahoma Agricultural and Mechanical College, 17 Feb 1947. There are about 20 more of these letters and requests in the file from places within the US (including Iowa, New York, and Massachusetts) and other countries too (including Uppsala, Sweden).

¹²³ Gerald Oster's name regularly appears on the Minutes starting at the 13 November 1945 meeting. APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Oster to Dunn, 27 Mar 1948.

entering into collaborative associations with the government.¹²⁴ Dunn wanted a more strongly worded statement, and desired the National Academy of Sciences “not merely to prevent the intimidation of scientists” but also, “to protest most vigorously this misuse of authority to undermine the fundamental rights and dignity of a citizen who is also a fellow member [of the NAS].”¹²⁵ Dunn and seven other members of the National Academy of Sciences circulated a statement made by three members of the American Academy of Arts and Sciences that presented and disputed the claims made against Condon and then gave a short synopsis of their position. In some ways, they took a similar stance as that of Richards: The House Committee on Un-American Activities presented inadequate evidence and Condon should be given the opportunity to respond. In comparison to Richards’ statement, the members of the American Academy stated that HUAC accused scientists of disloyalty because they shared scientific information internationally. They, furthermore, pointed out that international collaboration is a normal part of the scientific enterprise.¹²⁶

¹²⁴ APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Richards to NAS members, “A Statement by Members of the National Academy of Sciences Concerning a National Danger,” 31 Mar 1948. Also in APS, Demerec Papers, E.U. Condon.

¹²⁵ APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Dunn’s reply to Richards of the National Academy of Sciences, 2 Apr 1948. Dunn’s references to Condon as “our fellow member” and “one of our members” garnered a response from Richards. He told Dunn: “The Academy is not a club. I do not conceive that the Academy has any more duty to rush to the defense of Condon than it would have to defend a scientist whom none of us knows” (APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Richards to Dunn, n.d.).

¹²⁶ APS, Demerec Papers, E.U. Condon, Letter from eight members of the National Academy of Sciences in response to Dr. Richards’ letter, 5 Apr 1948. The eight members were Dunn, Louis P. Hammett, Victor K. LaMer, L.G. Longworth, D.A.

Dunn hoped that Condon could see the greater good that his case represented, and reassured Condon that he, Dunn, would stand by him throughout the episode. Dunn, therefore, wrote to Condon saying that he would “do anything in my power at any time to help you get the correct position established and the facts put before Congress and the people.” Dunn also lamented that Condon was the one to have to go through this debacle, but noted that Condon and his case represented something larger: “you have now come to stand for something which we simply cannot lose – so that your work and time serves a very large purpose indeed.”¹²⁷ Dunn correctly assessed that Condon’s case had become representative of a larger problem; one in which scientists and other organizations rallied in support of Condon. Kevles remarks on the tremendous assistance that Condon received from other physicists, especially those in the American Physical Society. Wang mentions the efforts undertaken by the Federation of American Scientists, Emergency Committee of Atomic Scientists, and other groups. She argues that Condon provided a specific situation around which scientists could share their opinions about the government’s restrictions on their freedoms.¹²⁸

MacInnes, I.I. Rabi, M.M. Rhoades, and D.D. Van Slyke. The three men who wrote the American Academy of Arts and Sciences letter were mathematician and natural philosopher P.W. Bridgman and law professor E.M. Morgan of Harvard University and physicist J.C. Slater of Massachusetts Institute of Technology.

¹²⁷ APS, Dunn Papers, Series I, Box 5, Condon Case-Related Materials, 1946-1952, Dunn to Condon, n.d.

¹²⁸ Kevles, 1971, 379-80; Wang, 1999, 139-40.

Dunn, as mentioned above, had told Weaver late in 1947 that the Science Society could not rise above current affairs plaguing American-Soviet relations¹²⁹ and by mid- 1948 the Science Society's Executive Committee sent a letter to members announcing its end. Oster noted that the Executive Committee did not plan to make a formal announcement because they did not wish to give J. Parnell Thomas and his committee that satisfaction.¹³⁰ About one month later, the New York Times learned about the letter and reported the Science Society's decision to disband.¹³¹ The Science Society waited about two more months before issuing its terminal announcement.

Dunn and his colleagues formally announced the end of the American-Soviet Science Society in September 1948. Dunn, MacInnes, Harlow Shapley, pathologist Leo Loeb, and toxicologist Alice Hamilton lamented that the US Treasury had sat on their application for over two years. All five had held positions as Chairman or Honorary Vice-Chairman of the Science Society during its short five-year duration. Clearly upset with the general situation plaguing American society, the authors expressed their opinion about the effects of the American government on scientific organizations:

¹²⁹ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes by Warren Weaver after talking with L.C. Dunn, 3 Nov 1947.

¹³⁰ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Interview notes from a conversation with Gerald Oster, 28 Jun 1948 and Dunn to Members and Subscribers of American-Soviet Science Society, 29 Jun 1948. Oster told Weaver that the Science Society decided to give all of its literature collected from the Soviet Union to the American-Russian Institute's library. He also mentioned the irony that many governmental agencies requested information from the Society about scientific developments in the Soviet Union.

¹³¹ "Science Group to End Soviet Exchange," New York Times (21 Jul 1948): 25.

It is a sad commentary on the administration of a government department that it prefers unsubstantiated insinuations to these solid evidences of the scientific standing of the Society and its value to American Science.

Dunn and his colleagues mentioned four “solid evidences” that they believed proved the American-Soviet Science Society’s worthiness. It was incorporated as an educational organization. It was recognized by the Rockefeller Foundation and the State of New York. Lastly, government organizations desired its services.¹³²

The House Committee on Un-American Activities had already investigated some of those who wrote the letter in Science, but ultimately HUAC’s accusations against the American-Soviet Science Society do not appear to have been a sincere concern. HUAC held a closed meeting with Harlow Shapley in November 1946 and continued to investigate him until the early 1950s. Shapley experienced relatively few repercussions because he had a strong reputation and the support of his scientific community.¹³³ As late as January 1954, Condon experienced consequences from his earlier loyalty accusations. He noted in retrospect that HUAC had not investigated any other members of the American-Soviet Science Society, which he interpreted to mean that HUAC targeted him and not the Society. If the Science Society had truly

¹³² L.C. Dunn, Harlow Shapley, Alice Hamilton, Leo Loeb, and Duncan A. MacInnes, “The American Soviet Science Society,” Science 108 (10 Sep 1948): 279. The Science Society had informed its members over two months prior to this public announcement that they had decided to disband. See: “Science Group to End Soviet Exchange,” New York Times (21 Jul 1948): 25.

¹³³ Wang, 1999, 118-30, 145-47.

been a security risk, then those more involved than he would have received greater criticism.¹³⁴

Aftermath: Disillusion & Distrust

The dissolution of the American-Soviet Science Society in the United States coincided temporally with the end of Mendelian genetics in the Soviet Union. In the summer of 1948, the Central Committee of the Communist Party endorsed Lysenko's Michurinist genetics as the only valid form of genetic research in the Soviet Union and beginning in early August the party purged Mendelian geneticists from academic positions. In the United States the government's crusade against organizations with contacts and sympathies with the Soviet Union forced the American-Soviet Science Society to announce its end. The Science Society's initial letter to its members came less than one month prior to the purge of Mendelian geneticists in the Soviet Union, and a more detailed announcement of the Society's end appeared in Science about one month afterwards.¹³⁵

The concurrent episodes of 1948 must have had devastating residual effects for the American scientists intimately involved. With regards to Lysenkoism, many American geneticists, indeed, experienced the effects on a personal level because of their strong ties to Soviet science and Soviet scientists. Lysenko's ascension signified

¹³⁴ RAC, RF 1.2 200D, Box 174, Folder 1599, American-Soviet Science Society, 1946-1958, Condon to Weaver, 10 Jan 1954.

¹³⁵ "Science Group to End Soviet Exchange," New York Times (21 Jul 1948): 25; Dunn, et al., 1948, 279. The official announcement in Science was made on 10 September 1948.

a fundamental problem for scientific freedom in the Soviet Union. Domestic issues in the United States added power to the punch because the House Committee on Un-American Activities had demonstrated through the Condon case that it too planned to redefine democracy and freedom. Furthermore, both governments relayed to their constituents that science as an international, cooperative endeavor was a thing of the past. During six months in 1948, March through September, a developing international crisis hit its climax and among those hardest hit domestically were the America-Soviet Science Society and its members. In half a year scientists, especially geneticists, witnessed a major turning point. Whereas, they had previously kept political aspects out of their scientific endeavors, they were now unable to discuss the pursuit of science without mentioning the political factors that impeded them.

These events greatly affected Dunn, and he was not able to recover quickly after the Cold War climate had set in at home. While on sabbatical during 1946-1947, prior to the final outcome of these events, Dunn and his family stayed for awhile with Gunnar Dahlberg in Uppsala. In June 1949, well after the Dunn family had returned, Stina, presumably Dahlberg's wife, expressed her concern that Dunn had not written. His silence conveyed to her that Dunn had either thrown himself into his work or was depressed about current events. Dobzhansky also knew of and understood Dunn's disheartened state:

What [Russian dramatist Anton] Chekhov called "twilights of life" in the postwar world get onto your nerves more than on those of many others, perhaps because your skin is thinner than for example mine, perhaps because you have suffered a much greater disillusionment.

You used to have faith in the goodness of people and their essential collective wisdom...

In this same letter of 12 May 1949, Dobzhansky attempted to uplift Dunn's spirits while also suggesting a new course of action.

Human evolution is not over yet. And by all means let us not despair, just withdrawn [sic] into an ivory tower (I have done some ivory tower propaganda with you for years but you would not listen) and let's venture out of it when opportunity offers.

Stina mentioned events at Columbia University, specifically that some radical professors had been dismissed, whereas Dobzhansky knew that tensions in the Zoology Department had reached a crisis. Dunn and Dobzhansky were considering leaving Columbia University. Stina understood that Dunn refrained from writing about that which depressed him. Similarly, Dobzhansky noted that Dunn typically disliked bothering others with his misery, and in response to hearing about Dunn's grief, Dobzhansky shared his desire to absorb some of it.¹³⁶

Although 1948 marked the end of both Mendelian genetics in the Soviet Union and of the Science Society in the United States, that year marked only the beginning of the Cold War. Dunn continued to experience repercussions as a result of the animosity between these two countries. The Soviet regime condemned Principles of Genetics, the textbook that Dunn wrote with Edmund W. Sinnott. While this textbook was popular in the United States, in the Soviet Union it sold over three times as many copies of the first three editions, which had been published in the late 1920s

¹³⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #6, 1948-1949, Dobzhansky to Dunn, 12 May 1949; Box 5, Gunnar Dahlberg, 1949-1950, Stina to Dunn, 27 June 1949.

and 1930s prior to Lysenko's triumph.¹³⁷ By 1949 Dunn noted that Lysenko condemned his textbook for teaching the wrong genetics.¹³⁸ Dunn later recalled that the regime blamed the textbook for poisoning young Soviet minds.¹³⁹

Dunn also experienced a number of personal setbacks as a result of the US State Department responding to his degree of his involvement with the American-Soviet Science Society (and other suspect organizations). In the early 1950s, the House Un-American Activities Committee investigated him. Ruth Shipley of the Department of State denied his application for a passport in 1953 because of his alleged communist activities.¹⁴⁰ Dunn had planned to travel to London in the summer of 1948 to act as a Scientific Attaché in London. Before departing, Dunn was informed that his appointment was not confirmed "due to curtailment of funds for scientific liaison work." Initially, he did not suspect that his political views influenced the outcome, but six months later the State Department filled the position with someone else. After finding this out, Dunn said that he drew the obvious conclusion.¹⁴¹

Dunn's public activism during the 1940s offers a lens with which to view the ironies in one scientist's attempts to bridge the gap between Soviet and American science. Dunn headed the American-Soviet Science Society with the hopes of

¹³⁷ Dunn, 1944, 66; Dunn, 1975, 777.

¹³⁸ Dunn, 1949, 142.

¹³⁹ Dunn, 1975, 777.

¹⁴⁰ APS, Dunn Papers, Series I, Box 8, L.C. Dunn-Passport Correspondence, 1953, R.B. Shipley to Dunn, 9 Apr 1953.

¹⁴¹ Dunn, 1975, 795-97; APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1948, Dunn to Landauer, 20 May 1948.

opening communication; however, as cold war tactics coalesced, communication between the United States and Soviet Union deteriorated. Furthermore, Dunn not only saw the connection between central funding and free inquiry collapse in the Soviet Union, but also witnessed in the United States a government using comparable tactics to those implemented abroad. For example, both countries censored communication. The United States government implemented loyalty oaths for governmental employees, whereas the Soviet regime threatened severe punishment for sharing national secrets. Both countries widely publicized to their citizens that actions have consequences. In the United States, the House Un-American Activities Committee demonstrated its power by conducting security investigations of a high-profile scientist, E.U. Condon. The Soviet regime used show trials to instill fear in its constituents.¹⁴² As for Dunn, he withdrew from discussions about the Soviet Union and Soviet genetics shortly after 1950, redirecting his attention to other lines of inquiry.

¹⁴² Nikolai Kremontsov demonstrates the role that the trial of professors Nina Kliueva and Grigorii Roskin had in the Soviet Union during 1947. Kremontsov briefly discussed the “KR affair” in Stalinist Science (129-57) and more extensively in his book, The Cure. Nikolai Kremontsov, The Cure: A Story of Cancer and Politics from the Annals of the Cold War (Chicago: University of Chicago Press, 2002).

Chapter 8

Science & Society: Dunn's Attack on Racism, mid-1940s to 1952

Dunn gradually became more political in thought and action after he started his tenure at Columbia University in 1928. As discussed in previous chapters, he counteracted the effects of fascism by aiding European refugees and safeguarding Americans' civil liberties, and he reacted to the Second World War by mobilizing for war and preparing for its end. The advent of these international political problems redirected Dunn's attention, pulling him away from questions about human genetics. Indeed, Dunn had been gravitating toward scientific problems involving human beings before these activities engaged his attention. He had analyzed anthropological data on people of pure and mixed Hawaiian descent in the 1920s, and he participated in anti-eugenic campaigns from roughly 1929 to 1935. He continued to speak against racial discrimination into the 1940s, although it was not his main focus.

As the war was ending Dunn returned to issues involving human heredity similar to those broached in the 1920s and early 1930s, and he initiated a full-scale effort to undermine racism and study human races. In 1946 he and Theodosius Dobzhansky published Heredity, Race and Society in which they scientifically explained that human variation is the product of both inheritance and environment and argued that the biological sciences did not support racism. By 1952 they had revised and expanded their book and during the interim Dunn worked closely with the

United Nations Educational, Scientific, and Cultural Organization (UNESCO) to produce pamphlets discussing the biology of human variation and addressing the fallacy of racial prejudices. Furthermore, he acted as Rapporteur in 1951 to the committee that criticized and revised UNESCO's 1950 Statement on Race.¹ In addition to circulating his views internationally through publications, Dunn also augmented his laboratory research on mice with investigations of human beings. To do research on human evolution, Dunn and Dobzhansky sought money from private foundations and established the Institute for the Study of Human Variation at Columbia University with Dunn as its Director. During its short duration from 1951 to 1958, the Institute's associates tackled questions on human evolution by using a cross-disciplinary approach. As part of the Institute's research, Dunn traveled to Italy in 1953 to collect data on an isolated Jewish population in Rome, and after his return he carried out a similar study on an African-American community living on James Island near South Carolina.

Dunn's views about race remained rather consistent over time. He believed that race was a valuable biological category; however, he fought against the use of

¹ L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society (New York: New American Library of World Literature, Inc., 1946); L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society, 2nd ed. (1946, New York: The New American Library of World Literature, Inc., 1952); L.C. Dunn, Race and Biology (Paris: UNESCO, 1951); What is Race?: Evidence from Scientists (Paris: UNESCO, 1952). The 1950 and 1951 Statements on Race are reproduced in What is Race?: Evidence from Scientists. Dunn's Race and Biology was part of a series called "The Race Question in Modern Science," and was reprinted with the other pamphlets and both the 1950 and 1951 Statements on Race. UNESCO, Race and Science: The Race Question in Modern Science (New York: Columbia University Press, 1961).

biology in making statements about racial superiority and inferiority.² During these years, from about 1946 to 1953, Dunn underwent a transformation from using science to speak about racism to conducting research on human beings in order to learn more about society and culture. This chapter explores Dunn's efforts and his attempt to undermine racism, and Chapter Nine discusses his scientific research on human variation by investigations involving isolated human populations.

The Anthropological Roots of Dunn's Race Work

Dunn's scientific and political views brought him into close contact with several anthropologists at Columbia University during the 1930s, most notably Franz Boas and Ruth Benedict. Boas and Benedict presented models that guided Dunn's opinions and tactics, and their influence can be seen in Dunn's post-war work addressing biological aspects of race and disputing scientific racism. Whereas Boas's anthropological outlook and political activism informed and motivated both Benedict and Dunn, it was Benedict's publications on race that served as concrete examples for Dunn when he worked for UNESCO. It is reasonable to draw connections between the perspectives and accomplishments of Boas, Benedict, and Dunn in reference to

² For general information about issues of race and scientific racism, see: Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (Connecticut: Archon Books, 1982); Audrey Smedley, *Race in North America: Origin and Evolution of a Worldview* (Boulder: Westview Press, 1993). For discussions about the issue of race in the United States after World War II, see: Mary L. Dudziak, *Cold War Civil Rights: Race and the Image of American Democracy* (New Jersey: Princeton University Press, 2000); Thomas Borstelmann, *The Cold War and the Color Line: American Race Relations in the Global Arena* (Massachusetts: Harvard University Press, 2001).

their efforts of studying race and undermining scientific racism while they were faculty members at Columbia University. Furthermore, it is evident that Dunn made decisions while working on his race publications that drew from his prior experiences with Boas and Benedict.

Boas devised and supported cultural relativism, meaning that different human groups have parallel cultures and that one group's worthiness cannot be evaluated against another's level of civilization. Boas argued in the mid-1920s that more needed to be learned about the mental and physical abilities of human beings and that scientific research should be conducted without biases. He told investigators to consider whether their conclusions were applicable to other races, and whether the attributes that they described resulted from environmental or inherited factors.³ By the mid to late 1930s, Boas had become politically active, and he pushed other people to act. One of his main campaigns was making scientific information available to the public. His own activism served as an example for many because he headed committees, gave speeches, and wrote passionate appeals in order to counteract Nazi propaganda and to support democracy and intellectual freedom.

Ruth Benedict, Boas's student, followed closely in his footsteps academically by holding to Boas's tenets intellectually and helping him administratively. She managed the Anthropology Department at Columbia University for him during the

³ Franz Boas, "What is a Race?" The Nation 120 (28 Jan 1925): 89-91. For information on and by Franz Boas, see: George W. Stocking, Jr., Race, Culture, and Evolution: Essays in the History of Anthropology (New York: The Free Press, 1968); George W. Stocking, Jr., ed., The Shaping of American Anthropology, 1883-1911. A Franz Boas Reader (New York: Basic Books, Inc., Publishers, 1974).

1930s, and often counseled his students. Additionally, she carried out Boas's educational agenda to inform the public about race and racism by writing Race: Science and Politics, which was published in 1940. Geared toward a general audience, her book served as the basis for two shorter publications: a pamphlet, The Races of Mankind (1943) and a children's book, In Henry's Backyard (1948).⁴

Dunn interacted regularly with Boas and Benedict during the 1930s and early 1940s. When Dunn was a member of the Executive Boards of the Emergency Committee in Aid of Displaced Foreign Scholars and the Faculty Fellowship Fund at Columbia University, Boas frequently asked Dunn to act on behalf of particular refugees. Moreover, while Boas chaired the American Committee of Democracy and Intellectual Freedom, Dunn and Benedict acted on its executive board and subcommittees. Dunn juggled many campaigns during the 1930s and early 1940s in addition to aiding refugees and fighting fascism. He also contributed to legislation that eventually established the National Science Foundation and facilitated communication between scientists in the United States and Soviet Union as President of the American-Soviet Science Society. These activities kept Dunn from addressing race and racism to the same degree as Boas and Benedict. He did, however, gain some valuable insights while collaborating with them. For example, he interacted with Boas, witnessed Boas in action, and learned about Boas's views on race and

⁴ Ruth Benedict, Race: Science and Politics (1940, New York: The Viking Press, 1945); Ruth Benedict and Gene Weltfish, The Races of Mankind (Public Affairs Pamphlet No. 85, 1943); Ruth Benedict and Gene Weltfish, In Henry's Backyard: The Races of Mankind (New York; Henry Schuman, 1948).

racism.⁵ Dunn collaborated with Benedict on several campus and national campaigns during the mid to late 1930s, but it was her race publications from the 1940s that served as models for his publications on race, especially those produced for UNESCO in the 1950s.

Ruth Benedict's relationship with Boas during the 1930s provides important background explaining why she wrote her race publications. Benedict finished her doctoral degree under Boas in 1923 and stayed at Columbia University to teach, obtaining a long-awaited assistant professorship in 1931. Also in the 1930s, Benedict took on a significant amount of administrative responsibilities in the Anthropology Department beginning with overseeing the department while Boas was on sabbatical in 1930. After he returned, Benedict continued to manage the department even though Boas was still its chair. She also oversaw the department during the 1936-1937 academic year because Boas had retired and a replacement had yet to be found.⁶ Scholars, including Benedict's intimate personal friend and colleague Margaret Mead, often mention the closeness of Boas and Benedict's professional relationship during the 1930s.⁷

The decade was pivotal for Benedict scientifically and politically. She established herself as an anthropologist by publishing Patterns of Culture in 1934, and

⁵ For a detailed discussion see Chapter Three.

⁶ Margaret Mead, "Ruth Fulton Benedict, 1887-1948," American Anthropologist 51 (1949): 457-68, 458-59; Margaret Mead, An Anthropologist at Work: Writings of Ruth Benedict (Boston: Houghton Mifflin Company, 1959): 341-55; Caffrey, 259-63, 276.

⁷ Herbert S. Lewis, "The Passion of Franz Boas," American Anthropologist 103 (2001): 447-67; Mead, An Anthropologist at Work, 341-55; Caffrey, 259.

about two years later, she started her political activism by working on many of the same anti-fascist campaigns as Boas and Dunn. However, she was not as radical as Boas and Dunn, who were both sympathetic to socialism.⁸ In 1939-1940 she took a sabbatical year leave during which she wrote Race: Science and Politics. The idea for a book along the lines of Benedict's had been in Boas's mind since at least the mid-1930s. Boas, who was born in Germany to a Jewish mother, doubted that statements coming from him would have the credibility that the same sentiments would have if made by someone who was of neither German nor Jewish heritage. He, therefore, wanted an anthropologist to write an international appeal addressing the misuses of the term race. In 1935, Boas offered to provide anthropologist Livingston Farrand with ideas for a document discussing people's misinformed views of human races. Farrand was President of Cornell University and Chairman of the Emergency Committee in Aid of Displaced Foreign Scholars at the time. Farrand declined Boas's offer,⁹ and eventually Benedict took on a project along these lines. According to Elazar Barkan, Boas instigated Benedict to write the book, and she received extensive advice from Boas when writing it.¹⁰

In Race: Science and Politics as well as in her subsequent publications, Benedict argued that race is a valid biological category that can be discussed

⁸ Caffrey, 289.

⁹ APS, Boas Papers, B:B61, Livingston Farrand #4, Four letters between Boas and Farrand dated from 12 Sep 1935 to 18 Sep 1935.

¹⁰ Elazar Barkan, The Retreat from Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars (New York: Cambridge University Press, 1992): 333.

scientifically; whereas, racism has no scientific basis, and instead has developed through historical circumstances. In the first section of Race: Science and Politics, she drew on biological evidence, from anthropology and to a lesser extent genetics, in order to address the science of race. In the book's second section, Benedict discussed racism and used historical examples to demonstrate that politics had shaped people's views about other races when compared to their own race. She pithily summarized her view by stating: "Race, then, is not the modern superstition. But racism is."¹¹

With her book Benedict sought to reach a broad audience and educate the public, which were goals that Boas had laid out for the American Committee for Democracy and Intellectual Freedom. Race: Science and Politics was too expensive at \$2.50 per book to permit the kind of circulation that she desired; however, she accomplished her goal by developing it into the widely-read pamphlet The Races of Mankind. Co-authored with anthropologist Gene Weltfish, The Races of Mankind reached the broad audience that Benedict had hoped because it cost only ten cents per pamphlet. It was translated into several languages, distributed in foreign countries, and made into a filmstrip.

Also helping the pamphlet's distribution was the notoriety it gained after the United States Army banned its distribution. The War Department bought 55,000 copies in order to educate enlisted men, but then stopped its distribution.

Congressman Andrew J. May of Kentucky disliked the pamphlet because it argued

¹¹ Benedict, Race: Science and Politics, 97-99. Quotation is from page 98. John P. Jackson, Jr. and Nadine M. Weidman, Race, Racism, and Science: Social Impact and Interaction (New Brunswick: Rutgers University Press, 2006): 168.

that Northern African-Americans had intelligence quotients (IQs) comparable to Southern whites. The House Military Affairs upheld the ban stating that The Races of Mankind had several flaws ranging “all the way from half-truths through innuendos to downright inaccuracies.”¹² The Army’s action contributed significantly to its circulation, according to Benedict’s biographer Margaret M. Caffrey, who notes that almost one million copies of The Races of Mankind sold in the initial ten years after the hullabaloo with the US Army. Moreover, a children’s book titled, In Henry’s Backyard, was eventually produced which presented a simplified form of Benedict and Weltfish’s views.¹³

Benedict and Weltfish came to co-author The Races of Mankind as the result of joining a small committee devoted to producing a Public Affairs Pamphlet on race. In addition to Benedict and Weltfish, the group overseeing the pamphlet’s publication consisted of anthropologist Marion Smith, psychologist Otto Klineberg, and Dunn. All five members belonged to the American Association of Scientific Workers and were also on the Columbia University faculty.¹⁴ This committee convened in 1943 to oversee the publication of The Races of Mankind, and around the same time Dunn

¹² The Associated Press, “Army Drops Race Equality Book; Denies May’s Stand Was Reason,” New York Times (6 Mar 1944): 1; “Hits ‘Races of Mankind’: House Group Says Book Army Used has Misstatements,” New York Times (28 Apr 1944): 7; Caffrey, 298.

¹³ Caffrey, 292, 297-300.

¹⁴ For information about the committee at Columbia University and its connection to the American Association of Scientific Workers, see: Violet Edwards, “Note on *The Races of Mankind*” Race: Science and Politics revised edition (1940; New York: The Viking Press, 1945): 167-68; Caffrey, 297.

and Dobzhansky had agreed to write what would become Heredity, Race and Society.¹⁵

Heredity, Race and Society

Heredity, Race and Society had a publication date of 29 November 1946, a date that was long in coming according to Dobzhansky who felt that it should have been published two years earlier.¹⁶ In May 1943, Dobzhansky mentioned their joint book, yet he admitted to Dunn that he had not started writing its text.¹⁷ During the summer of 1945, Schuster Publishers rejected it, which prompted Dobzhansky to refer to it as "...our seemingly ill-fated book." Then, two weeks later Dunn happily informed Dobzhansky that Penguin Books had agreed to publish it. Dobzhansky allowed Dunn to make the decisions without his input since he was across the country in California doing field research. He did however relay one stipulation: "I am not interested in royalties...to have it published by a publisher who may distribute it as widely as possible will give me a much greater satisfaction than to have it bring royalties."¹⁸

¹⁵ Dobzhansky admitted to Dunn that he had not yet written anything for their joint book. APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #4, 1943-45, Dobzhansky to Dunn, 16 May 1943.

¹⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #5, 1946-47, Dobzhansky to Dunn, 25 Nov 1946.

¹⁷ See footnote 13.

¹⁸ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #4, 1943-45, Dobzhansky to Dunn, 31 Jul 1945, 20 Aug 1945 and 4 Sep 1945.

Dobzhansky appears to have gotten his wish. A copy of the book cost only twenty-five cents when it first came out in 1946. At that price a fair number of people would have been able to afford it.¹⁹ Moreover, it was translated into six European languages as well as Arabic, Persian, Burmese, Urdu, Bengali, and Indonesian. Dobzhansky even ran across a pirated copy in Alexandria. Considering that their book sold over one-half million copies in several languages, its contents seemingly reached a wide readership.²⁰ Gerhart Sander believed that revising Heredity, Race and Society must have been quite an undertaking, and well worth the time due to its decent circulation.²¹

When the book was published in 1946, Dobzhansky was ecstatic and compared his elation to the feeling he had after the publication of his first scientific paper. He ordered 100 copies with the intention of distributing it as Christmas presents to people who did and did not usually receive a gift from him. He also started making corrections in one copy, just in case they decided to revise it someday,

¹⁹ The book went up in price over the years. A copy of the first edition printed in 1950 cost \$0.35 and the revised and enlarged edition cost \$0.60 in 1952.

²⁰ Theodosius Dobzhansky, "Leslie Clarence Dunn, November 2, 1893-March 19, 1974," Biographical Memoirs 49 (Washington, D.C.: National Academy of Sciences, 1978): 79-104, 82; APS, Dunn Papers, Series I, Box 8, L.C. Dunn-Heredity, Race and Society (with Dobzhansky) Correspondence, 1958-1964, 15 Nov 1961 Memorandum of Agreement between Dunn and Dobzhansky with Franklin Publications of New York. Dunn reported the pirated copy to their publishing company, The New American Library on 15 Sep 1958. The most recent tally in Dunn's papers dates from 30 June 1963 with sales totaling 459,617 copies (APS, Dunn Papers, Series I, Box 8, L.C. Dunn-*Heredity, Race and Society* (with Dobzhansky) Correspondence, 1958-1964, Royalties and Subsidiary Rights, #2).

²¹ APS, Dunn Papers, Series I, Box 24, Gerhard Sander, 1950-52, Sander to Dunn, 3 Sep 1952. Dunn sent the revised edition to Sander, one of his scientific collaborators around the time of its publication in 1952.

which they did do in the early 1950s. Furthermore, he was proud of their accomplishment because he felt that they had “honestly” tried to enlighten others.

In Heredity, Race and Society Dunn and Dobzhansky debunked several common social beliefs by presenting scientific evidence to counter prejudicial claims. Specifically, they attempted to explain the complexities of human heredity to a general reader, while also pointing out problematic aspects of racist thought, especially those substantiated by scientific racism and eugenics. They gave a broad, biological definition of races based on relatively new knowledge resulting from what is now referred to as the Modern Synthesis, “Races can be defined as populations which differ in the frequencies of some gene or genes.”²² To prove their point, they compared the distribution of the four human blood groups (O, A, B, and AB) present in individuals of various countries and populations, and they pointed out that “races differ in blood group type only in a relative way.” They similarly thought that racial categories were arbitrary, stating that it was impossible to create static racial groups and promoted a flexible view of human racial categories.²³ As pointed out by John P.

²² Dunn and Dobzhansky, 1952, 118; They gave a similar definition later in the book, “Races are populations which differ in the relative commonness of some of their genes” (124).

²³ L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society (New York: New American Library of World Literature, Inc., 1946); L.C. Dunn and Theodosius Dobzhansky, Heredity, Race and Society, 2nd ed. (1946, New York: New American Library of World Literature, Inc., 1952): 117-35. The definition of race is in both editions (1946, 101; 1952, 118), as is the other quotation (1946, 104; 1952, 120). Although originally published by Penguin Books, changes in the company’s infrastructure during 1948 created The New American Library of World Literature, Inc. and its subdivision, Mentor Books (http://en.wikipedia.org/wiki/New_American_Library; 5 Oct 2006).

Jackson and Nadine M. Weidman, Dunn and Dobzhansky presented a view similar to Ruth Benedict, in that they denied the validity of racism, but retained race as a biological category.²⁴ Indeed, Dunn and Dobzhansky did not want people to cease using race as a method for biological classification; however, they wanted people to be aware that defining various races was difficult and the lines drawn to differentiate races were arbitrary and ambiguous.²⁵

Dunn and Dobzhansky denied the notion of a pure race or pure blood lines by noting that “Mankind has always been, and still is, a mongrel lot.”²⁶ They also explained that biological evidence showed that hybrids, people who have inherited their genes from biologically different populations, may express “increased vigor” and they cannot be considered inferior to their parents.²⁷ This work built on Dunn’s extensive experiments and innovative perspective on the decreased vitality displayed by inbred poultry and the increased vigor of intercrossed poultry. He had conducted these investigations while at Storrs Agricultural Experiment Station in Connecticut during the 1920s.²⁸ In his 1934 textbook Heredity and Variation, Dunn stated that human progeny born from inter-racial marriages displayed hybrid vigor.²⁹

Dunn and Dobzhansky stated throughout their book that inherited traits interact with the environment: “what is inherited is not this or that trait, but the

²⁴ Jackson and Weidman, 168-73.

²⁵ Dunn and Dobzhansky, 1952, 124-26.

²⁶ Dunn and Dobzhansky, 1946, 98.

²⁷ Dunn and Dobzhansky, 1946, 114.

²⁸ Dobzhansky, 1978, 79-104.

²⁹ L.C. Dunn, Heredity and Variation: Continuity and Change in the Living World (New York: The University Society, 1934): 99.

manner in which the developing organism responds to its environment.” Different traits have different degrees of variability; for example, the blood groups are much more static than a person’s skin color or intelligence. Putting these two ideas together Dunn and Dobzhansky argue at the end of their book that a person’s mental ability depended on how that individual interacted with society at large. The plasticity of intelligence “guaranteed the success of mankind as a biological species, and has, in turn, permitted progressively more advanced cultural developments.” Being part of “a mongrel lot” held a special significance for Dunn and Dobzhansky, because man’s variation, plasticity, and educability propelled culture.³⁰ Moreover, they wanted their readers to embrace mankind’s diversity, rather than wasting time quibbling over racial hierarchies because “There is no common measure applicable to the works of a poet, an artist, a philosopher, a scientist, and the simple kindness of heart of a plain man. Humanity needs them all.”³¹ These are the last two sentences of their book, and in them Dunn and Dobzhansky adapt Boas’s argument of cultural relativism between populations in order to make a statement about individuals in society, thereby making Boas’s and their point easily understandable to readers.

³⁰ This point is much clearer in the 1952 edition, than in the original one because Dunn and Dobzhansky had incorporated the ideas of educability and plasticity. Both are present in the 1950 and 1951 “Statements on Race” and a source, if not the source, of Dunn and Dobzhansky’s exposure to using this argument with reference to difference in human intelligence.

³¹ Dunn and Dobzhansky, 1952, 131-35. Quotation is from 134. Dunn and Dobzhansky added this section on race and intellectual capacity to their revised edition of Heredity, Race and Society (131-34).

Dunn and Dobzhansky recognized that during different historical periods racism existed under various guises, such as Social Darwinism and Eugenics. These two topics were discussed in the first edition within the same chapter, “Group Differences and Group Heredity,” and were interspersed with scientific sections discussing population genetics and the human gene pool. This one chapter underwent extensive revision and in the next edition its contents formed two chapters: “Man as a Product of Evolution” and “Controlling Man’s Heredity.”

In “Man as a Product of Evolution” Dunn and Dobzhansky explained natural selection and Social Darwinism and argued that tolerance and cooperation were as prevalent as exploitation and competition. They urged their readers to recognize the “duality of man,” that he was subject to both biological and cultural forces. Human beings undergo biological modification through evolutionary processes in the same manner as other living species (such as apes). *Homo sapiens*, however, differs from other species because culture and society also affect human evolution. To truly learn about human beings, one had to know about the biological and cultural forces shaping humans because, “in man, biological fitness largely coincides with his fitness to live in the society of which he is a member.” Different cultures value different attributes and whether competition or cooperation guided a particular society, man had the ability of adjust himself accordingly. Furthermore, Dunn and Dobzhansky were not willing to give up on two main tenets of biology. In addition to their view that races

still exist, they also argued that natural selection still acts on human populations.³² Accompanying their discussion of the “duality of man” were explanations of the biology of the human gene pool, the mathematics of the Hardy-Weinberg equilibrium principle, and variation as a result of mutations. They framed the chapter within their hope for the future, that one day evolution could be “subjected to some measure of control by man,” and thereby minimize, or better yet eliminate, harmful mutations and hereditary diseases.³³

The next chapter, “Controlling Man’s Heredity,” clarified some problematic features associated with eugenics and inheritance of acquired characteristics. In their discussions about eugenics, they used new terminology to reiterate a sentiment that Dunn had argued previously. Beginning in the late 1920s, Dunn had supported studies of human heredity and castigated eugenicists’ drastic applications of genetic laws. In the 1950s, he supported scientifically-sound studies of the human gene pool, but called eugenics a pseudoscience when used to promote social inequalities and oppression.³⁴ Dunn and Dobzhansky criticized the scientific fallacies in Lysenko’s promotion of the inheritance of acquired characteristics as well, and they also blamed the Soviet Union’s political endorsement of the doctrine. As the title of the chapter suggests, “Controlling Man’s Heredity,” they couched their points within a discussion of the pros and cons of man’s ability to control his own heredity, and as in the previous chapter, they again stated that humans would one day be able to manipulate

³² Dunn and Dobzhansky, 1952, 68-69.

³³ Dunn and Dobzhansky, 1952, 63, 82.

³⁴ Dunn and Dobzhansky, 1952, 63, 101.

inheritance in a useful manner. Using methods similar to artificial selection and breeding practices, humans could control human heredity; however, for the most part, sociological factors rather than biological ones presently interfered with success in human engineering.³⁵

A comparison of the 1946 and 1952 editions yields a marked increase in their discussion of evolution, which represents a shift simultaneously occurring in their research interests. By this time Dunn and Dobzhansky had established the Institute for the Study of Human Variation at Columbia University, on paper at least and were working toward constructing a physical space for the Institute. Dunn was conducting chemical analyses of blood and other bodily fluids through the Institute, whereas Dobzhansky was performing twin studies. Their Institute for the Study of Human Variation is discussed in the next chapter.

How the public received Heredity, Race and Society was a matter that concerned Dobzhansky, and he dreaded the book reviews believing that “our efforts will be appreciated much more in Harlem than on Park Avenue, and inhabitants of Harlem seldom write reviews or at any rate seldom have them published.”³⁶ He quickly learned that his pessimism was unnecessary because he received relatively

³⁵ Dunn and Dobzhansky, 1952, 83-106.

³⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #5, 1946-47, Dobzhansky to Dunn, 25 Nov 1946 and 20 Dec 1946. Few people knew Dunn’s whereabouts; Dobzhansky had a vague idea since he had received postcards from Carlsbad and Tucson.

positive feedback.³⁷ The few published reviews that do exist were written over a period of several years covering both the 1946 and 1952 editions of the book. These reviews on the whole are positive. Bentley Glass commended Dunn and Dobzhansky's book twice. In 1947 he stated that "This little book is outstanding for the clarity and simplicity of its exposition," and in 1954 he noted that "the excellence of a clear and pungent style is thoroughly maintained" in the revised edition. Bessie Bloom Wessel of Jewish Social Studies called it "simple, adequate, and forthright." She appreciated that two men with high-quality scientific backgrounds in genetics and evolution were promoting views that were sound in light of sociology and cultural anthropology.³⁸

Geneticists Robert Cook and Jay L. Lush, on the other hand, had many problems with what Dunn and Dobzhansky said, and did not say, in the first edition of their book. They wrote a rejoinder, more than a book review, in which they depict Dunn and Dobzhansky as misleading John Q. Busrider, an unsuspecting reader, by denying the beneficial uses of genetics for human races and society. John Q. was liable to gain a "pessimistic or hostile attitude toward eugenics" as a result of reading

³⁷ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #5, 1946-47, Dobzhansky to Dunn, 5 Dec 1946. Dobzhansky told Dunn, "Our dear little booklet has so far produced only expressions of approval – I hope this does not mean that people are withholding criticisms for politeness's [sic] sake only." Dobzhansky had his wife, Natasha, send Gunnar Myrdal's book The American Dilemma (1944) to Dunn.

³⁸ Bentley Glass, "Heredity, Race and Society," The Quarterly Review of Biology 22 (Jun 1947): 152; Bentley Glass, "Heredity, Race and Society," The Quarterly Review of Biology 29 (Dec 1954): 360-61; Bessie Bloom Wessel, "Heredity, Race and Society," Jewish Social Studies 11 (1949): 187-89.

Heredity, Race and Society, and therefore the reviewers spent almost seven pages clarifying a few points.

Cook and Lush thought that deleterious genes could be removed at a much quicker rate (i.e. over fewer generations) than argued by the authors. And although Dunn and Dobzhansky demonstrated that “biological suicide” was a far off fate, they failed to address “cultural suicide.” According to Cook and Lush, biological defects caused by mutations affected the human population at a significantly slower rate than culturally-based phenomena. Cultural factors were influencing human reproduction in a negative way because civilization allowed those with poor genetic constitutions to reproduce. Selection had the ability to act in a negative manner and to result in a marked growth in the number of deleterious genes. Cook and Lush worried most about intelligence and drew from data collected by Frederick Osborn in the United States and Cyril Burt in England to conclude that heredity and society interact negatively. Birthrates showed a decrease in the human population’s intellectual caliber, and a less intelligent human population was susceptible to totalitarianism, according to Cook and Lush:

An intelligent population may not have a democratic form of government, but it is hard to imagine that an increasingly unintelligent population will maintain a democratic tradition. A feebleminded population begs for the domination of a dictator. Here heredity and society touch very intimately; and yet this whole matter is passed over without comment.

For Cook and Lush there was a lot at stake because over the past 100 years, natural controls had ceased to act on the human population and new methods needed to be

devised. Extreme eugenic practices were not acceptable, and neither was giving up on eugenics simply because “fanatics get queer notions.” Eugenics could operate to the benefit of mankind as long as it functioned through education and voluntary, cooperative approaches.³⁹ Cook and Lush’s arguments were not novel, and summarize the sort of prevalent sentiment that instigated Dunn and Dobzhansky to write Heredity, Race and Society. Cook was managing editor of Journal of Heredity, the pro-eugenic publication in which this review article appeared, and when Dobzhansky learned beforehand of Cook’s intentions, he informed Dunn to expect a bad evaluation with “some swell headlines by Cook!!!”⁴⁰

Dunn and Dobzhansky called attention to Cook’s claims by writing a new section “Are People Getting More Stupid?” for the revised edition. In addition to providing a concrete example of Scottish children’s improved performance on an intelligence quotients (IQ) test, Dunn and Dobzhansky also explained why it is that people with fewer children might be more intelligent. People who start bearing children at an early age have less time to obtain a formal education and possibly suffer from a lack of access to social upward mobility. For Dunn and Dobzhansky, the problem was not that a portion of the population (i.e. a particular race or the more fertile and less wealthy classes) had an innate inability to learn. Rather, intelligence was a matter of inheritance, time, and access; and therefore, genetic, social, and

³⁹ Robert Cook and Jay L. Lush, “Genetics for the Million, An Unfinished Story,” Journal of Heredity 38 (Oct 1947): 299-305. The long quotation is on page 302, and the shorter one following it is on 305.

⁴⁰ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence, 1946-47, Dobzhansky to Dunn, 20 Dec 1946 and 8 Jan 1947.

economic factors had to be taken into consideration. As they argued throughout this book and in their other publications addressing racism: Biology was not the only factor.⁴¹

Dunn said of writing Heredity, Race and Society, “It made me realize how difficult it is to explain these matters on a plane which is scientifically sound, yet simple enough for the intelligent layman.”⁴² Even though Dunn found writing science for a general audience a tricky task, he did not shy away from additional opportunities to educate people. As discussed below, he undertook several additional projects that allowed him to share his views internationally in an attempt to clarify what biological and social scientists know and do not know about human races, and to argue that scientists have no right to evaluate races’ (physical, mental, or other) potentials by speaking in racist terms.

Educating the World: Dunn’s UNESCO Pamphlets

Dunn’s other publications about race and racism shared views not far from those expressed by him and Dobzhansky in Heredity, Race and Society. Moreover, their joint book persuaded members of UNESCO to ask Dunn in 1950 to write an educational pamphlet on Race and Biology, and he agreed. This technical, yet accessible booklet was part of a series called “The Race Question in Modern

⁴¹ Dunn and Dobzhansky, 1952, 99-101. See pages 97-99 for background on social classes and fertility. The also added a subsection in their chapter on “Race” that dealt with intelligence, “Do Human Races Differ in Mental Capacity?” (131-34).

⁴² APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1946, Dunn to Landauer, 15 Dec 1946.

Society.” UNESCO proposed to several scholars, like Dunn, who were leaders in their scientific field and had previously promoted social equality of races, that they write a pamphlet for inclusion in the series. As a result, New York scholars associated with Columbia University wrote several of the UNESCO pamphlets that dealt with other aspects of the racial problem. Otto Klineberg, who wrote on Race and Psychology, had been a psychology student at Columbia University in the mid to late 1920s, taking classes from Franz Boas. He left New York for about five years, returning to Columbia University for a thirty-year career as a member of the Psychology Department.⁴³ Harry Shapiro, Chairman of the Department of Anthropology at the American Museum of Natural History contributed two volumes, Race Mixture and The Jewish People: A Biological History. Scholars trained in anthropology authored a fair number of these pamphlets, including Race and History by Claude Lévi-Strauss and Racial Myths by Juan Comas.⁴⁴

When Alfred Métraux of UNESCO’s Department of Social Science asked for a summary of Race and Biology, Dunn referred him to the third paragraph:

The modern view of race, founded upon the known facts and theories of heredity, leaves the old views of fixed and absolute biological differences among the races of man, and the hierarchy of superior and inferior races founded upon this old view, without scientific justification. Races appear in the new view to be biological sub-groups

⁴³ Benjamin Harris, “Otto Klineberg,” American National Biography 12 (1999): 792-93.

⁴⁴ UNESCO, Race and Science: The Race Question in Modern Science (New York: Columbia University Press, 1961). Dunn revised Race and Biology in 1960 before it was published in this book and drew on his field work, which is discussed in the next chapter. Shapiro was a co-worker of Dunn’s at Columbia University and through the Institute for the Study of Human Variation.

within the single species, *Homo sapiens*, in which the similar heredity which the whole species has in common far outweighs the relative and minor ways in which the sub-groups differ. This change in biological outlook has tended to restore the view of the unity of man which we find in ancient religions and mythologies, and which was lost in the period of geographical, cultural and political isolation from which we are now emerging.⁴⁵

Prior to this, when Métraux received Dunn's initial draft, he exalted: "Your first version was welcome by the people in the Information Department, who have taken much inspiration from it for their propaganda on race." Even more exciting to Métraux was the upcoming visit of the "the team Dunn-Dobzhansky" to UNESCO's Paris offices.⁴⁶

In addition to UNESCO's enthusiastic response, reviewers reacted favorably to Dunn's Race and Biology over the years. Maurice R. Davie thought Dunn's booklet shined in comparison with the other four pamphlets that he reviewed from the UNESCO series and stated that Dunn's was, in his opinion, "the best of the series."⁴⁷

⁴⁵ Dunn, Race and Biology, 7-8. There is a sentence at the beginning of this paragraph that Dunn did not reproduce in his letter to Métraux: "The judgement [sic] of biology in this case is clear and unequivocal." APS, Dunn Papers, Series I, UNESCO, 1951, Dunn to Métraux, 17 Oct 1951.

⁴⁶ APS, Dunn Papers, Series I, Box 25, UNESCO, 1951, Métraux to Dunn, 21 Feb 1951.

⁴⁷ Maurice R. Davie, book review, "The Race Question in Modern Science," American Sociological Review 17 (1952): 393. The other four pamphlets were Racial Myths by Juan Comas, Race and Psychology by Otto Klineberg, Race and Culture by Michael Leiris, and The Roots of Prejudice by Arnold Rose. Davie might have had a personal bias in favor of Dunn because they were both extremely active in helping refugees during the 1930s. Davie, Staff Director of the Committee for the Study of Recent Immigration from Europe, authored Refugees in America (1947) interacting frequently with Dunn's colleagues from the Emergency Committee in Aid of Displaced Foreign Scholars. Alvin Johnson of the New School of Social Research was the committee's chairman. Stephen Duggan was also on the committee and Betty

Much later, Hugh H. Smythe of City University of New York reviewed Race, Science and Society (1975), which reproduced ten of UNESCO's booklets, and he mentioned only Dunn by name, calling attention to Dunn's qualifications as a geneticist and an authority on race.⁴⁸

The United Nations Educational, Scientific, and Cultural Organization had only recently been established when it started this serial project combating racial misconceptions. The roots of UNESCO stem from 1942 and the Conference of Allied Ministers of Education, a group organized to help the Allied Powers to deal collectively with rebuilding education after the war ended. Reforming curriculum and teaching racial tolerance guided the Conference's principles. In addition to borrowing the Conference of Allied Ministers' goals, the founders also modeled UNESCO on the Organization of Intellectual Cooperation of the League of Nations and the International Bureau of Education. From these origins, the name United Nations Educational and Cultural Organization (UNESCO) was suggested in 1944. One year later, after scientists had argued for their inclusion by mentioning the inherently cooperative and international character of science, science was added and the organization formed as UNESCO. The United Nations officially started on 24 October 1945, and less than one month later UNESCO began operations with Julian Huxley as the first Director-General. After another year, UNESCO had gained the

Drury was a member of Davie's staff. Maurice R. Davie, Refugees in America: Report of the Committee for the Study of Recent Immigration from Europe (New York: Harper & Brothers Publishers, 1947).

⁴⁸ Hugh H. Smythe, "Race, Science, and Society," Contemporary Sociology 6 (Jan 1977): 77-78.

minimum twenty states needed in order to ratify its Constitution and operate as an official agency of the United Nations.

In 1948 UNESCO decided to tackle racial prejudice and racial discrimination by collecting and disseminating information on an international scale. Resolutions adopted in 1949 and 1951 put this policy into effect; however, UNESCO's minimal budget greatly restricted its initial activities. In 1947, UNESCO operated on about \$7 million and over the next four years its budget modestly increased to \$8.2 million. Since UNESCO's operational funds come from contributions made by its member states, its budget grew significantly in subsequent years with the addition of new affiliates. For example, UNESCO's budget grew eight to nine-fold during its first twenty years, amounting to \$62 million in 1967-1968. It was during these two decades, the 1950s and 1960s, that the pamphlets in this series on "The Race Question in Modern Society" were published.⁴⁹ As discussed above, UNESCO asked Dunn to write his booklet in 1950, which makes Race and Biology one of the first to be produced, and moreover it was issued during UNESCO's initial years while the organization functioned under financial constraints.

Dunn's publications on race and his collaboration with UNESCO provide further evidence connecting his efforts to those of Ruth Benedict and Franz Boas. Dunn agreed to write a pamphlet for UNESCO in the years after both Boas and

⁴⁹ UNESCO on the Eve of its Fortieth Anniversary (Paris: UNESCO, 1985): 11-21, 81-82, 129-31; UNESCO: Twenty Years of Service to Peace, 1946-1966 (Paris: UNESCO, 1966): 13-14, 45; Sagarika Dutt, UNESCO and a Just World Order (New York: Nova Science Publishers, Inc., 2002): 1-21. Each book gives a different initial budget ranging from \$6.25-7.7 million.

Benedict had passed away. Boas died in 1942 at the age of eighty-four. In spring 1948, Benedict accepted an invitation from UNESCO to lecture at a summer seminar on Childhood Education in Czechoslovakia.⁵⁰ She died shortly after returning from overseas.

Dunn offered a perspective in his UNESCO pamphlet Race and Biology that was similar to Benedict's statements in her race publications during the 1940s. He provided a scientific discussion of genetics, blood groups, and other biological aspects of race and myths about race, as well as mentioned the mutual roles that heredity and environment play in shaping a person's physical attributes. He drew from his specialties by analyzing genetic and metabolic factors and arguing that "Heredity determines the nature of the internal trigger which the stimulus from the environment may release to produce a given effect."⁵¹ In comparison, Benedict drew from data typical of her discipline, such as descriptions of height, shape of head, and skin color. Like Dunn, she also mentioned ABO blood groups, but Dunn's explanation was much more scientifically technical than hers.⁵² Benedict and Weltfish in The Races of Mankind explained that racial prejudices are the result of fear, fears that can be overcome through the acceptance of racial equality, and stated

⁵⁰ Mead, An Anthropologist at Work, 437-38. The seminar produced "The Influence of Home and Community on Children under Thirteen Years of Age," Towards World Understanding VI (Paris, UNESCO, n.d.); Caffrey, 319, 337. Otto Klineberg, a student of Franz Boas, worked with UNESCO and interacted with Dunn regularly.

⁵¹ Dunn, Race and Biology, 15-19. Quotation is from page 17.

⁵² Dunn, Race and Biology, 26-34. In comparison to Benedict, Dunn drew on information about the distribution of blood groups in order to support his claim that civilization has created barriers other than geographical ones that keep populations from mixing.

that all human beings belong to one human race.⁵³ Dunn ended on a similar note by stating that “Biologically, then, all men belong to one mating circle.” He also noted that “There is, thus, no biological justification for race hatred or prejudice” and that “we should extend to all other groups the tolerance and sympathy which we extend to our own people.”⁵⁴

Similar socio-political opinions and professional circumstances brought Dunn and Benedict together over the course of about ten years during the 1930s and 1940s. Columbia University situated them amidst a political center with rich racial diversity and a growing immigrant population. Furthermore, their physical proximity to each other made collaborating convenient. When asked to write a pamphlet for UNESCO on race, Dunn thought of the one person’s work with which he had an intimate familiarity, Ruth Benedict’s Science: Race and Politics and The Races of Mankind. Dunn can be seen as carrying out a Columbia University campaign started twenty-five years earlier because he continued a project initiated by Franz Boas and picked up by Ruth Benedict. In the 1930s Boas had urged others to write the kind of publications that Benedict produced in the 1940s and Dunn wrote in the 1950s. Yet, since he was a geneticist, Dunn produced a very different book than that written by Benedict, who was an anthropologist. Nonetheless, Benedict and Dunn each drew on their own disciplinary specialty to present roughly the same general argument about

⁵³ Benedict and Weltfish, The Races of Mankind. The point about one human race is made repeatedly on pages 3-5, 12, and 31. The section on fear and racial prejudices is on pages 25-26.

⁵⁴ Dunn, Race and Biology, 38-39.

racism – an argument that Franz Boas devised during his prestigious career as an anthropologist at Columbia University.

Ruth Benedict's model also prompted Dunn to propose that Race and Biology include cartoons similar to those in The Races of Mankind. He received the reply that educational illustrations were desired, but not cartoons because they did not make sense to people living in all of the countries where the publication would be distributed.⁵⁵ Instead, a smaller brochure, What is Race?: Evidence from Scientists, was produced from Race and Biology that targeted a general, non-technical readership.⁵⁶ The larger booklet contains scientific tables and diagrams, but no other images, whereas What is Race? relies on illustrations to convey its points. Dunn, however, only edited What is Race?; he did not write the text. Diane Tead, of UNESCO's Mass Communication Department, penned it by combining information from Dunn's booklet with that of two other pamphlets in the same series: Otto Klineberg's Race and Psychology and Michel Leiris's Race and Culture.⁵⁷

⁵⁵ APS, Dunn Papers, Series I, Box 25, UNESCO, 1950, n.d., Robert A. Angell, Acting Head, Social Sciences Department of UNESCO to Dunn, 11 May 1950 and 22 May 1950.

⁵⁶ Up until the final edits, the UNESCO brochure What is Race? was going to be called "What Science Says about Race." I do not know who chose to change the name, nonetheless What is Race? is quite similar in title to an article written by Franz Boas that was published in The Nation in 1925. Boas's article, "What is a Race?" was the first of a series of articles on "The Nordic Myth" written by various authors. Franz Boas, "What is a Race?," The Nation 120 (Jan 1925): 89-91. APS, Dunn Papers, Series I, Box 25, UNESCO, 1951, Diane Tead, Mass Communication Department of UNESCO to Dunn, 10 Oct 1951.

⁵⁷ Dunn, What is Race?, backside of title page.

Tead grew up in New York. Her father, Ordway Tead, taught personnel administration as an adjunct professor in industrial relations at Columbia University between 1920 and 1956. He was President of the New York City Board of Higher Education from 1938 to 1953. Moreover, Ordway helped Franz Boas and the American Committee for Democracy and Intellectual Freedom by speaking at the 1939 rally that served as a kick-off event establishing the organization. In the 1940s he was Chairman of the Public Affairs Committee, which produced Benedict's Public Affairs pamphlet, The Races of Mankind. By the late 1940s, he was a member of the United States National Commission for UNESCO, which operated through the US Department of State and oversaw the United States involvement in UNESCO. Diane Tead's personal views appear to have closely followed those of her father who was described as a civil libertarian.⁵⁸

What is Race?: Evidence from Scientists was the product of a second attempt at producing a volume that conveyed the UNESCO message by using pictures. Its precursor, What Science Says about Race, which Diane Tead penned, received limited distribution in 1951 and created an intense situation that played out over six months before it was revised and renamed What is Race? Dunn had a hand in producing both of these illustrated pamphlets. He helped Tead to edit the original brochure, What Science Says about Race, while he visited UNESCO's Paris offices

⁵⁸ APS, Dunn Papers, Series I, Box 25, University Federation for Democracy and Intellectual Freedom, 1937-39, News Bulletin on the Activities of the Local Committees in Arranging Lincoln Birthday Meetings, n.d. Also, see: "Diane T. Michaelis: Producer of Movies and TV Programs," The New York Times (18 Dec 1981); "Dr. Ordway Tead, Educator, 82, Dies," New York Times (17 Nov 1973): 38.

during summer 1951.⁵⁹ After its publication, some members of the US National Commission, the State Departments' division that closely collaborated with UNESCO, reacted negatively towards the booklet. Dunn defended What Science Says about Race and received a laudatory note from Métraux that expressed his appreciation and that of members of UNESCO's staff. Métraux was aware of political tensions plaguing American scientists in the post-war years as a result of growing anti-Communism in response to Joseph McCarthy and the House Un-American Activities Committee. Métraux described Dunn's endorsement as a "courageous stand...In a period of weakness and fear..." Evidence for the US National Commission's exact opposition is unknown; however, the nature of the problem seems to stem from what Métraux referred to as a rumor that What Science Says about Race had been renounced by scientists working closely with UNESCO.⁶⁰

Dunn actively supported the illustrated booklet after the rumor compromising its reputation started in the early part of 1952. He reiterated his confidence in the pamphlet's soundness in a letter to Richard H. Heindel, Deputy Director of the US Department of State's UNESCO Relations Staff. Dunn and Heindel had corresponded the previous year after Heindel learned of Dunn's participation in the debates that resulted in drafting a second Statement on Race. Heindel, himself, had attended the Sixth General Conference of UNESCO, which convened a few days after Dunn's

⁵⁹ APS, Dunn Papers, Series I, Box 25, UNESCO, 1951, Tead to Dunn, 10 Oct 1951. Diane thanked Dunn for having helped with the brochure while he had been in Paris.

⁶⁰ APS, Dunn Papers, Series I, Box 25, UNESCO, 1952, Métraux to Dunn, 11 Mar 1952 and Tead to Dunn 19 Mar 1952.

conference ended. Upon his return to the United States Heindel was excited about UNESCO's program. He knew of Dunn's cooperation with UNESCO and therefore asked Dunn to contact him. Dunn offered to meet with him the next time he went to Washington D.C.⁶¹

Dunn demonstrated diplomacy and strategy when handling the issue about the pamphlet's credibility. Instead of calling attention to the rumors about What Science Says about Race, Dunn informed Heindel that he finally had had a chance to thoroughly peruse the booklet, and he had found it a good educational resource and offered to help with its distribution. Not everything in the brochure was stated in the exact manner that Dunn would have chosen, but he stood by its overall message and the scientific validity of its statements. Moreover, he noted that the brochure drew mainly from his Race and Biology and that as Rapporteur of the committee writing a second "Statement on Race" he felt assured that the committee's members would also support the illustrated booklet.

Heindel appreciated Dunn's comments which he felt addressed the biological information presented, but Heindel needed to get other opinions. Thus, he sought feedback from social scientists since some of the material pertained to their expertise. The situation between Dunn and Heindel appears to have ended amicably, and Heindel offered to share other responses that he had received in reference to the

⁶¹ APS, Dunn Papers, Series I, Box 25, UNESCO, 1951, Heindel to Dunn, 17 Jul 1951 and Dunn's reply, 25 Jul 1951.

pamphlet's reliability.⁶² One year later, Dunn would have his passport application denied by Ruth Shipley of the US State Department. Most likely, his ties to the Soviet Union during the 1940s were the predominant cause. Yet, Dunn also corresponded with Heindel from the summers of 1951 to 1952 about UNESCO and his work on race issues, and although they started out in hopes of cooperation, tensions arose over the course of that year. Nevertheless, Heindel and Dunn seem to have been on good terms at the end of March 1952.

The commotion caused by What Science Says about Race convinced Métraux to make a trip to the United States, arriving on 22 May 1952. Three days after his arrival, Dunn wrote to Jaime Torres Bodet, Director-General of UNESCO, that he and Métraux had revised and improved the controversial pamphlet. Still standing by his opinions from when he first signed on to these race projects with UNESCO, Dunn expressed his confidence in using illustrations to convey a message:

I was dismayed when I learned that UNESCO might abandon its first attempt to reach the broad public, especially by the pictorial method which promises to be so useful for mass education. Your resolve to proceed in spite of all difficulties to give this method a fair trial will now give encouragement to those who believe that public education in science is an urgent matter and one of the most tangible contributions which an international organization can make toward world peace.⁶³

Dunn and Métraux must have shown their revisions to Otto Klineberg because Klineberg wrote to Bodet on the heels of Dunn. Klineberg and Leiris's pamphlets

⁶² APS, Dunn Papers, Series I, Box 25 UNESCO, 1952, Dunn to Heindel, 13 Mar 1952 and Heindel's reply, 20 Mar 1952.

⁶³ APS, Dunn Papers, Series I, Box 25 UNESCO, 1952, Métraux to Dunn, 12 May 1952, and Dunn to Bodet, 25 May 1952.

were used to write What is Race?: Evidence from Scientists. Michel Leiris, like Dunn, had supported the original booklet, but Klineberg had not approved of its psychological and anthropological content. Klineberg regularly gave lectures on UNESCO's activities and therefore felt the need to convey his disapproval of the original booklet. Revising the illustrated pamphlet had been a wise choice in Klineberg's estimation, and he now felt able to continue giving UNESCO his unflagging endorsement in the United States. The revised pamphlet accomplished Dunn's main goal, which was to convey scientific information in support of the UNESCO message through illustrations thereby making it easy for laypeople to understand.⁶⁴

⁶⁴ What is Race? rhetorically asked readers, "Which of the following would you call races?" and listed twelve options. Half of the choices qualified as races: Mediterraneans, Negroes, Whites, Nordics, and Caucasians, as well as Australians, which were considered both a race and not a race. Seven options did not qualify as races: Australians, Aryans, Chinese, Latins, Russians, Semites, and Jews. Australians of European descent were not a race; Australian's "original inhabitants" (i.e. aborigines) were hard to classify because they fit within both the white and black races. Semites were categorized as a linguistic group speaking a family of languages including Babylonian, Hebrew, Arabic and Ethiopian, whereas Jews were identified as a religious and cultural group. The pamphlet upheld the view that there were three major races in the world (Caucasians, Negroes, and Mongoloids) and acknowledged that some scientists used more than three racial categories. Anthropologists preferred the term Caucasian, rather than White and they included Nordics as a sub-group within the Caucasian race. Mediterraneans were classified as a race, but it does not say which of the three races they fit into. Mediterraneans were defined as people of Spain, Portugal, southern France, Italy, North Africa and parts of Asia Minor, Arabia, Pakistan, and India (37, 72-73).

A Communal Effort: UNESCO and its Statements on Race

While Dunn initially edited these publications, Alva Myrdal, Director of UNESCO's Department of Social Sciences, invited him to be part of a panel that would "re-examine and discuss the definition of race and the question of ethnic differences as expressed in UNESCO's 'Statement on Race'" of 1950.⁶⁵ Alva was the wife of Gunnar Myrdal, who wrote The American Dilemma (1944), a book that called attention to the incongruity between the American Creed (life, liberty, and the pursuit of happiness) and racial segregation in American towns. Métraux had also been trying to get Dunn and Dobzhansky to visit Paris and act as American representatives at this same meeting. Dunn attended the conference held in June 1951, and then he spent the next year-and-a-half circulating draft copies of a new proclamation, "Statement on the Nature of Race and Race Differences" and coordinating the details of getting the new Statement published. He finally accomplished this feat in September 1952 after extensive revision efforts.⁶⁶ (At times the revised Statement will be referred to by year, 1951, which is the year the meeting was held and not the year that it was published.)

Ashley Montagu headed a committee made up of mostly sociologists, who together wrote the first "Statement on Race" that was published in 1950. Dunn, Dobzhansky, Gunnar Dahlberg, and others commented on it, and Montagu addressed

⁶⁵ APS, Dunn Papers, Series I, UNESCO, 1951, Métraux to Dunn, 21 Feb and 1 Mar 1951; Alva Myrdal to Dunn, 12 Mar 1951.

⁶⁶ APS, Dunn Papers, Series I, Box 25, UNESCO, 1951 and UNESCO, 1952. Most of the correspondence during these two years discusses the "Statement on Race."

their criticisms before publishing. Yet, all of the people mentioned above acted on the committee that wrote a new Statement. In his introduction to the 1951 Statement, Dunn acknowledged that geneticists and physical anthropologists disagreed with some views in the 1950 Statement, and furthermore that scholars, who performed research in the biological and anthropological sciences that dealt with race, should also be heard.⁶⁷

Dunn recognized that the two Statements basically provided the same conclusion: biology does not support racism or race prejudice. Montagu, who acted on both the 1950 and 1951 committees, believed that both Statements essentially conveyed the same opinions. Dunn, on the other hand, disliked the confident perspective from which the 1950 “Statement on Race” was written and pinpointed this one aspect as its main problem. For example, the 1950 version claimed that mental tests demonstrate all human groups to possess equal cognitive ability as long as they are tested in their native environmental conditions. There was no proof supporting this notion, according to those who wrote the 1951 Statement, and therefore they chose a different tactic. They presented the scientist’s common belief by using less definite terminology:

...there is good evidence that, given similar opportunities, the average performance (that is to say, the performance of the individual who is representative because he is surpassed by as many as he surpasses), and the variation round it, do not differ appreciably from one race to another.

⁶⁷ Dunn wrote a section that introduced the “Statement on the Nature of Race and Race Differences.” Dunn, What is Race?, 81; Montagu, 139.

Usually, however, the 1951 committee assessed race by pointing out absences of evidence used to support racially discriminatory claims, instead of presenting absolute facts. For example point seven begins: “There is no evidence for the existence of so-called “pure” races.”⁶⁸

The publication process was slow not due to the Statement’s length, but because it proved difficult to get cooperation in integrating numerous opinions. At one count, Métraux had collated comments drawn from sixty letters and was trying to make sense of them.⁶⁹ Dunn described some of the problems that he encountered while assembling the 1951 Statement.

Whether these [the Statement and a report of the meeting written in non-technical language] get accepted & published in time to do any good is something about which I cannot guess since the inertia of bureaucracy is almost crippling at Unesco. This one small statement has been almost smothered by a mountain of paper & memos & telegrams & counter telegrams.⁷⁰

Dunn wrote this in December 1951 around the time that he had been in Paris and gave UNESCO the original copy of the 1951 statement, which he felt was ready for publication. Unfortunately, more hurdles quickly came about. Métraux circulated a draft of the 1951 Statement and found that many geneticists disagreed with one

⁶⁸ What is Race?, 76-86; Montagu, 137-38. In 1964 UNESCO issued its third statement, “Proposals on the Biological Aspects of Race,” which believed had the same flaw as the 1950 “Statement on Race.” APS, Dunn Papers, Series I, Box 24, Roger Pineau, 1964, Dunn to Roger Pineau of the Department of State’s Bureau of Educational and Cultural Affairs, 17 Oct 1964.

⁶⁹ APS, Dunn Papers, Series I, Box 25, UNESCO, 1952, Métraux to Dunn, 15 Feb 1952.

⁷⁰ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1951, Dunn to Landauer, 12 Dec 1951.

paragraph in particular (discussed below) and postponed the Statement's publication so that more revisions could be made. By the end of February, Dunn expressed his concerns and also sought additional edits.⁷¹ As a result of many scientists making concessions in an effort to present a united voice, the final product left a bit to be desired because Dunn received feedback that his Race and Biology presented a more straightforward explanation than the joint Statement of 1951.⁷²

The paragraph in question and in need of revision had a sentence about mental capacities reminiscent of the inflammatory sentence from The Races of Mankind. Benedict and Weltfish had said that “income, education, cultural advantages, and other opportunities” determined intelligence and not one's race. To prove their point, they reproduced a table demonstrating that Northern Negroes achieved higher median scores on intelligence tests than Southern Whites. This point, as mentioned above, invoked Congressman Andrew J. May to discredit the pamphlet and caused officials of the United States Army to ban it. Those formulating the 1951 UNESCO Statement also compared the intelligence of two racial groups, but came to a different conclusion, and as is evident from the following excerpt, they phrased their points in more general terms:

Even those psychologists who claim to have sometimes found the greatest difference in intelligence between groups of different racial origin, and have contended that they are hereditary, always report that

⁷¹ APS, Dunn Papers, Series I, Box 25, UNESCO, 1951 and UNESCO, 1952, Dunn to Métraux, 28 Dec 1951; Métraux to Dunn, 25 Jan and 15 Feb 1952; Tead to Dunn, 19 Feb 1952; and Dunn to Métraux, 26 Feb 1952.

⁷² APS, Dunn Papers, Series I, Box 24, Gerhard Sander, 1950-52, Sander to Dunn, 20 Dec 1952.

many members of the group of inferior performance not merely surpass the lowest ranking members of the superior group, but most of its members.

Reviewers did not ask for the sentence's deletion, but rather requested a clarification of two vague and improperly used words: "many" and "most." It is possible that the commotion resulting from Benedict and Weltfish's The Race of Mankind sensitized them to the potential uproar that such a sentiment could create and, thus, some hoped to head off a crisis before it erupted. Nonetheless, Curt Stern and Dr. Marshall T. Newman, a physical anthropologist with the Smithsonian Museum, independently complained. The usage of the two terms misled readers, according to Stern, because "many" was a relative term, whereas "most" was absolute. Newman pointed out a similar disparity: "many" indicated quantity and "most" proportionality. Newman suggested providing rough percentages to appease literal-minded readers. Hans T. Clarke also objected and proposed a few simple changes in wording and Dunn, himself, offered a similar suggestion.⁷³ In the end the statement resembles a compromise between Clarke and Dunn's revised language, which replaced "many" with "some" and changed the end of the sentence, which ultimately clarified the sentence's meaning. The published version reads: "...some members of the group of inferior performance surpass not merely the lowest ranking member of the superior

⁷³ APS, Dunn Papers, Series I, Box 25, UNESCO, 1952, Métraux to Dunn, 25 Jan 1952 and Dunn's reply, 7 Feb 1952. A copy of Stern's comments that he sent to Métraux are available in their entirety in Stern's Papers. APS, Stern Papers, Ms Coll 5, Curt Stern, "Comments to UNESCO statement on Race, 1951." For biographical data on, see: Michael H. Crawford, "Marshall T. Newman (1911-1994)," Human Biology 67 (Oct 1995): 683-87.

group, but also the average of its members.”⁷⁴ It was coordinating and coming to agreement on decisions such as these that delayed the “Statement on the Nature of Race and Race Differenced” for eighteen months.

Dunn got so exasperated by the process that he drafted an entirely new statement geared to the general public that was based on Race and Biology and the Statement of 1951. Dunn wished for Métraux to enclose his “Reformulation” when re-circulating a revision of the Statement based on some of the comments discussed above. Moreover, Dunn suggested allowing the delegates to choose between the two statements. Métraux decided against giving members a choice because he believed that they were close to finalizing the Statement of 1951, and they were. A copy of the Statement distributed in April is not exactly the same as the final version published in September 1952, but it is close.⁷⁵

UNESCO published the 1951 “Statement on the Nature of Race and Race Differences” and a lengthy discussion about the document in The Race Concept. Dunn’s “Reformulation of the Statement on the Concept of Race” accompanied his comments about the 1951 Statement. Dunn developed his comments after grappling

⁷⁴ Clarke suggested using “...some members of the group of inferior performance may surpass the average of the superior group.” Dunn proposed: “...some members of [the] group of inferior performance surpass many members of the ‘superior group’.” See Métraux to Dunn, 25 Jan 1952 and Dunn’s reply, 7 Feb 1952, which are cited in the footnote directly above. The “Statement on the Nature of Race and Race Differences” has been published in several publications. See, What is Race? (81-86) and Ashley Montagu, Statement on Race: An Annotated Elaboration and Exposition of the Four Statements on Race Issued by the United Nations Educational, Scientific, and Cultural Organization 3rd ed. (London: Oxford University Press, 1972): 137-47.

⁷⁵ APS, Dunn Papers, Box 25, UNESCO, 1952, Dunn to Métraux, 26 Feb 1952 and Métraux’s to Dunn, 11 Mar and 18 Apr 1952.

for many years with the “race concept” and its misuses, and therefore provided the best summary of his opinions at this time. Race is strictly a biological concept that tends to get misconstrued because people conflate biological with social and cultural factors. Social and cultural attributes, such as nationality, language, and religion, are much more malleable than biological constitution. Since “race and culture arise from quite different origins and are affected by different factors” differentiating between race and culture (Boas’s main argument) was the first step in making sense of human society.⁷⁶

Finding consensus proved difficult, according to Dunn, because they attempted to make scientific information known while also justifying an ethical position. It was UNESCO’s goal to use scientific facts to make ethical claims,⁷⁷ and the first committee adhered to this guiding principle, whereas Dunn and his committee were unwilling to give the impression that science had proven what it had not. A more practical approach, according to Dunn, would have been to clearly differentiate between the scientific data and the conclusions drawn by scientists from that information. “It is our duty,” Dunn explained to Métraux, “as scientists to make the facts as clear as possible and to relate them to the evidence. We act in another

⁷⁶ The Race Concept: Results of an Inquiry (Paris: UNESCO, 1953): 83. Dobzhansky’s comments (73-75) are followed by Dunn’s “Reformulation” (75-78) and comments (78-83). Dobzhansky also provided his comments. Dunn and Dobzhansky agreed on one essential point. There are two ways to study human diversity, and therefore there are two ways in which race is used. First, it is a form of classification and second, it explains human variation. Dobzhansky’s discussion about human diversity is on page 73, and Dunn’s similar discussion about race is on page 80.

⁷⁷ Jackson and Weidman, 197-98.

capacity when we draw moral and ethical lessons from them, and I think these two aspects ought to be made clear.”⁷⁸

Dunn and Dobzhansky had obtained money for their Institute for the Study of Human Variation by this time, and Dunn began focusing his attention on a new endeavor, studying human populations firsthand. He wanted to be the one drawing scientific conclusions based on evidence that he had collected. Indeed in 1947, soon after completing the first edition of Heredity, Race and Society, Dunn started learning techniques that would allow him to conduct field research on human beings. His field and laboratory investigations dealing with questions about human heredity, races, and evolution are the subject of the next chapter.

⁷⁸ APS, Dunn Papers, Box 25, UNESCO, 1952, Dunn to Métraux, 26 Feb 1952.

Chapter 9

Human Heredity and Evolution, 1949-1959

After over six years of battling against racism from a geneticist's point of view, L.C. Dunn turned his attention away from the ethical dimensions and towards scientific studies of human races. Getting into the field in 1953 to collect bodily fluids from human beings required a few years of preparation and much effort on Dunn's part, as well as help from his friends and colleagues. He began learning laboratory techniques of serological analysis in 1947 and approached the Rockefeller Foundation for money to support a research institute in 1950. Dunn, Dobzhansky, and others received a grant the following year, which allowed them to organize the Institute for the Study of Human Variation. With the Institute's sponsorship, Dunn studied two isolated communities, Jews in Rome and African-Americans on James Island, South Carolina. As the culminating and synthesizing project from his investigations of human beings, he wrote Heredity and Evolution in Human Populations, which first appeared in 1959. The Institute for the Study of Human Variation and its members endured many setbacks during the 1950s as a result of decisions made by administrators at Columbia University. Ultimately, the University did not follow through in a timely manner or adhere to their promises to provide facilities and money for the Institute. As a consequence, the Institute for the Study of Human Variation operated for about seven years and during 1957 its affiliates started wrapping up their projects. It officially closed down operations during the following year.

Preparing to Conduct Studies on Human Beings

Upon finishing Heredity, Race and Society, Dunn developed an interest in personally conducting human genetics studies, especially by way of blood group analyses. To cultivate his new focus, he spent about six months of his 1947 sabbatical leave in Sweden with Gunnar Dahlberg of the State Institute of Human Genetics at the University of Uppsala. Dahlberg's book, Race, Reason, and Rubbish: An Examination of the Biological Credentials of the Nazi Creed, which was first published in 1942, prompted Dunn's decision to go to Uppsala.¹ Dahlberg concentrated mainly on science and history of science, and in his book's last few chapters he gave a thorough explanation of race and racism by pointing out that it was erroneous to believe in a pure Nordic race and to develop biased opinions based on anti-Semitism. Dahlberg kept his analysis focused on discrediting typical statements made by Nazis about Jews. For example, he said, most people relied on an antiquated understanding of race that was based on the blending of traits, rather than on Mendelian genetics, in order to promote Aryan superiority. Pure races, according to Dahlberg, were a "hypothesis with little scientific basis."² In terms of anti-Semitism, Dahlberg referred to the Jewish question as one that has its roots in religious,

¹ "The Reminiscences of L.C. Dunn," Columbia University Oral History Project, Interviews with L.C. Dunn conducted by Saul Benison in 1960; Gunnar Dahlberg, Race, Reason, and Rubbish: An Examination of the Biological Credentials of the Nazi Creed trans. Lancelot Hogben (1942; London: George Allen & Unwin Ltd., 1943).

² See Dahlberg's chapter to "Race" (193-218); Dahlberg, 209.

economic, social, and racial factors and he gave historical explanations for the development of prejudices against Jews.

All in all, Dahlberg refrained from broadening his subject matter and stayed on his specific topic – the situation between Nazis and Jews. Yet he drew conclusions that applied to issues involving race and racism in general. Eugenics and sterilization were not solutions to their proponents' concerns, and moreover these options were full of problematic aspects, such as the inability to determine standards that act as guidelines for implementing positive and negative eugenic practices. Dahlberg firmly believed that variety was good for society, and provided the rationale that eugenic practice could lead to deleterious consequences. The world required people of various talents (politicians, laborers, artists, etc.) to fulfill the diverse tasks necessary for the continuation of everyday civilization. Moreover, science changed regularly with the expansion of scientific information over time. Dahlberg cautioned against the desire for immediate practical applications based on theoretical ideas. In his opinion the suppression of scientific conclusions contrary to eugenic biases would never work because "...no opinion can stand up to the unanimous verdict of science."³ Dunn's later publications were more encompassing than Dahlberg's book, but their views are virtually the same.

Dahlberg's views as laid out in Race, Reason and Rubbish, and shared with Dunn during his visit, influenced not only Dunn's opinions but also his research. Dahlberg believed that ongoing processes continually break down barriers isolating

³ Dahlberg, 239-40.

populations from one another. Some small, isolated groups contend regularly with congenital defects as a result of consanguineous (cousin) marriages, but deterioration is not an inevitable result of inbreeding. Each population's genetic constitution determines its propensity as a group to bear individuals with favorable and detrimental attributes. In Dahlberg's opinion modern technology and social activities have brought diverse peoples into contact and eliminated geographical, social, and racial obstacles that had previously kept people apart: "...the social instruments which make for racial betterment are the bicycle and the omnibus, the flivver and the jazz band-in short, the collective enjoyments of town life."⁴

Going on sabbatical was a much needed vacation for Dunn. His tireless efforts before, during, and immediately after the war had taken their toll on him physically. He spent the first nine or so months of his sabbatical recuperating in the American desert, and postponed his planned departure for Europe. He appears to have been emotionally stressed and mentally exhausted, as well. Sweden and Dahlberg offered Dunn a leisurely and calm retreat: "I have found a very congenial atmosphere here at Dahlberg's Institut...for me, a laboratory in an old house enclosing a garden full of lilacs and fruit trees, quiet, and comfortable, in which coffee & cakes appear for the two professors every few hours – didn't belong in this world at all. The whole of Uppsala somehow belongs to an era that has died or been killed elsewhere in

⁴ Dahlberg's chapter on "Isolate Effect" (180-92).

Europe.”⁵ Dunn valued that some places on the continent had managed to preserve a few of the past pleasures lost in other locales, and he returned to New York reinvigorated and desiring to pursue new research avenues.

Dunn, however, did not find peace at home. As discussed in Chapter Seven, he learned of the problems facing the American-Soviet Science Society before leaving the United States for Sweden. The Science Society’s situation grew more dismal during his sabbatical and escalated in the months after his return. By March 1948 Dunn was among those scientists trying to rescue E.U. Condon, Condon’s reputation, and the reputation of the Science Society from accusations made by the House Un-American Activities Committee. National events stemming from the onset of the Cold War were only one facet of what troubled Dunn. At Columbia University, he and Dobzhansky struggled with colleagues in the Zoology Department.

In November 1947 Dunn informed Warren Weaver of the Rockefeller Foundation about the uncertain future of genetics at Columbia University. Although he had only been back in the department for a couple months, Dunn recognized that if he did not act quickly then he might lose a lot. Dobzhansky had been offered the opportunity to start a Division of Human Genetics for the National Institutes of Health. Dunn did not seem too concerned that Dobzhansky would take this particular job because Dobzhansky avoided and disliked administrative work. Yet, Dobzhansky was at the height of his career and desired to do research and train students, which

⁵ APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1947, Dunn to Landauer, 23 Jun 1947.

implies that Dunn foresaw how easily Dobzhansky might be wooed elsewhere if the offer were right. In order to accommodate Dobzhansky and other geneticists' needs at Columbia University, Dunn requested Weaver's opinion about the possibility of obtaining a Rockefeller Foundation grant for geneticists which would allow them to become independent of the Zoology and Botany Departments.

Ten years prior to this Dunn had chosen for the geneticists to retain their posts in separate departments while working together in a genetics laboratory; however, he now changed his opinion and sought independence for geneticists. Informing Dunn's new view was the discipline's increasing prominence: "I suppose genetics has grown up and won't be restrained by old [disciplinary] boundaries and that we are now quite as important as any field of biology and may even be the center of it."⁶ Dunn refrained at this time from sharing the true root of the problem. There was a divide in the Zoology Department that was creating tension for Dunn and Dobzhansky. Over the next few months, Dunn kept in contact with Weaver and when it came time to submit for renewal of the Zoology Department's grant, the Zoology Department's two factions sent in separate applications. Their research interests had diverged, according to Dunn, from the time when he had submitted the original application, which had funded the Zoology Department for the past three-and-one-half years.⁷

⁶ See the subsection on Dobzhansky in Chapter Five for information about the Zoology Department during the late 1930s and early 1940s. RAC, RF 1.1 200D, Box 132 Folder 1631, Columbia University-Genetics, 1942-1947, Dunn to Weaver, 29 Nov 1947.

⁷ Most information about the internal turmoil of the Zoology Department is recorded in the notes of members of the Rockefeller Foundation. RAC, RF 1.1 200D, Box 132

Dunn requested money for his, Dobzhansky, and Marcus Rhoades' investigations into "the role of genes and chromosomes as structural and functional units in heredity, development, and evolution." Separately, Franz Schrader asked for \$46,500 as a continuation of the previous grant for research conducted by him, Arthur W. Pollister, Lester G. Barth, and some junior members of the department.⁸ The Rockefeller Foundation approved both applications listing them as one grant with two overlapping projects focused on Genetics and Experimental Zoology.⁹ Genetics received forty percent and Zoology sixty percent of the \$82,500 that covered five years starting in July 1948.¹⁰

The two factions co-existed amicably enough for about one year, but by February 1949 Dunn had reached his limit and contacted Dobzhansky in Brazil for his opinion about what course of action they should take. Dobzhansky felt that they must leave either the Zoology Department or Columbia University. In hindsight, the turning point had come after Selig Hecht died in September 1947, which was roughly when Dunn arrived back from his sabbatical leave. Hecht, a senior member of the Zoology Department, had typically sided with Dunn and Dobzhansky, and as a result of Hecht's absence other departmental members started making things difficult for

Folders 1631, 1632 and 1633, Columbia University-Genetics, 1942-1947, 1948-1949, and 1950-1952. See letters starting with 29 Nov 1947. The original Rockefeller Grant covered three-and-one-half years starting January 1945.

⁸ RAC, RF 1.1 200D, Box 132 Folder 1632, Columbia University-Genetics, 1948-1949, Dunn to Weaver, 8 Jan 1948 and Schrader to Weaver, 14 Jan 1948.

⁹ "Columbia University Genetics and Experimental Zoology," Rockefeller Foundation Annual Report 1948 (New York: Rockefeller Foundation, 1948): 182-84.

¹⁰ RAC, RF 1.1200, Box 132 Folder 1631, Grant Report Resolution RF 48076, 18 Jun 1948.

Dunn and Dobzhansky. The problem's source, in Dobzhansky's opinion, was the need for a departmental administrator and the fact that all tenured zoologists wanted the post. (Dobzhansky himself disliked administrative matters, as mentioned earlier, and balked when Dunn suggested that he accept nomination as a candidate for Executive Officer. If Dunn thought it necessary, then Dobzhansky would allow his name to be put forward, but the idea horrified him. "My personality is not suited for the job," argued Dobzhansky. "I often hurt people's feelings without wishing to do so and without realizing that that is what I am doing.") Dunn considered retiring because departmental issues had gotten so problematic, but Dobzhansky would not hear of it, and that is when he informed Dunn of their two choices: "So, let us go on, if the University lets us. If not, let us look for places elsewhere, and if possible together... It's too late for us to change the present department, we must try to separate ourselves from it – within the University if possible, outside if necessary." The two main troublemakers were Francis J. Ryan and John A. Moore. Moore obtained the coveted position as Chair and told Weaver in October 1949 that relations between the zoologists and geneticists had improved. Moore kept everyone on good terms with each other by polling them individually rather than holding departmental meetings.¹¹

¹¹ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #6, 1948-1949, Dobzhansky to Dunn on 3 Feb, 6 Feb, and 22 Feb 1949. Dobzhansky's quotation about his administrative abilities comes from the 22 Feb 1949 letter, and the other from 3 Feb 1949; RAC, RF 1.1 200D, Box 132, Folder 1632, Weaver's diary entry about a talk with John Moore, 20 Oct 1949.

Dunn actually found a way to escape Columbia University temporarily; he accepted a guest appointment at Harvard University for fall 1949. After his semester at Harvard, President James B. Conant and others at the University entered negotiations with Dunn. They needed a strong administrator to revamp their Genetics Department, which began declining after E.M. East's death and had practically ceased to exist upon William E. Castle's retirement in 1939. Negotiations with Dunn fell through in part because those at Harvard acted slowly and misconstrued Dunn's needs by making them more exorbitant than he requested. Dunn informed Alfred Blakeslee that he could still be persuaded to leave Columbia for Harvard in 1952. Dunn considered going to Harvard University to start an institute focusing on human and mammalian genetics while Dobzhansky stayed at Columbia University to establish an institute on population genetics, including the human population. Blakeslee campaigned on Dunn's behalf by explaining the misunderstanding about money extensively to Karl Sax and succinctly to Conant; however, Conant continued to use money as the reason for not extending an offer to Dunn.

Although not stated outright, the outcome of this situation was more than likely determined by Dunn's political views and past. Beginning in 1949, Harvard University's President Conant had to deal with anti-communist attitudes extensively, and he grew more rigid over the next few years before resigning in January 1953. (He resigned knowing that the Senate Internal Security Subcommittee planned to investigate Harvard's faculty.) In the months leading up to his departure from Harvard University, Conant discouraged his faculty members from using the Fifth

Amendment by averring that it would be self-incriminating for anyone who invoked the Fifth Amendment by not naming names. More pertinent to Dunn's situation is the antagonistic relationship between Conant and Harlow Shapley. Conant considered Shapley politically naive, yet refrained from overtly using Shapley's political views against him. Conant nonetheless managed to stop Shapley from obtaining a full professorship in 1951, and one year later Shapley resigned as Director of the Harvard Observatory.¹² Dunn and Shapley held similar political and social views, as well as worked together as members of the Emergency Committee in Aid of Displaced Foreign Scholars and American-Soviet Science Society in the 1940s. Although complete conjecture, it is possible that Conant linked Shapley and Dunn, and therefore Dunn's close association to Shapley made him an undesirable candidate.

At some point Dunn and Dobzhansky conjured up a new research agenda to establish their own interdisciplinary institute, and by early March 1950 they had received approval from Columbia University to start an Institute of Population Genetics of Man. There was one hitch, according to Harry Miller of the Rockefeller Foundation: "Columbia has now given the project the green light, but (as usual) no

¹² RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, Blakeslee's comments recorded by Warren Weaver, 15 May 1952; Arnold Arboretum Horticulture Library, Karl Sax Papers, Sax to James B. Conant, 9 Feb 1950 and Blakeslee to Sax, 16 May 1952 and 12 Mar 1954; James G. Hershberg, James B. Conant: Harvard to Hiroshima and the Making of the Nuclear Age (New York: Alfred A. Knopf, 1993): 461, 606-37.

funds.”¹³ Members of the Rockefeller Foundation learned from Dobzhansky that they wished to build a \$100,000 building on the Nevis Estate in Irvington-on-Hudson. Columbia University owned the property, which is located about thirty miles north of New York City, and had given Dunn access to it years earlier in 1937. He housed mice on the property in barns and other buildings. Warren Weaver and Marston Bates of the Rockefeller Foundation had no intention of financing a new building, but offered to discuss with others at the Rockefeller Foundation the possibility of supplying the newly-conceived Institute with operational funds.

Dunn and Dobzhansky secured \$50,000 from a Ford Foundation grant in the University’s possession, which was earmarked for renovating the Nevis mansion to suit their purposes.¹⁴ They did not obtain enough money for a new building, as originally desired, but nonetheless by early 1951 they had the physical location at Nevis Estate for the Institute and money to renovate the mansion. Dobzhansky received the news unhappily and lamented that the mansion was “unsatisfactory” for laboratory work. Their prospectus warranted a “new and luxurious” laboratory comparable to others being built in the United States because their proposed agenda, in Dobzhansky’s opinion, offered “better scientific returns.” In order to draw on the Ford Foundation money, the Institute had to procure another source of funding. Dunn

¹³ RAC, RF 1.1 200D, Box 132, Folder 1632, Columbia University-Genetics, 1948-1949, Dobzhansky called Weaver, but Miller took the call because Weaver was out, 14 Apr 1950 and Warren Weaver diary entry, 6 Jun 1950.

¹⁴ Dunn later noted that they received \$57,000 from the Ford Foundation. UACL, Central Files, Box 382, Folder 4, L.C. Dunn, 1946-1947 and 1951-1952, Dunn to Grayson Kirk, 2 Feb 1953.

and Dobzhansky submitted a grant to the Rockefeller Foundation with two of their colleagues, Howard Levene of the Departments of Mathematical Statistics and Zoology, and W.H. Sheldon of the Department of Medicine. Sheldon, however, left the project before learning if the grant was approved.¹⁵

Members of the Rockefeller Foundation debated at length the merits of funding Dunn and Dobzhansky's scheme. One concern was the location. Dunn and Dobzhansky proposed an Institute to synthesize disciplines, yet planned to remove themselves from the city by going to the "country far away from the medical school (which is one chief concentration of knowledge of human biology), from the zoology department, for the psychologists, the anthropologists, etc., etc., etc." Dunn's optimism that he could obtain appointments at Columbia University for researchers associated with the Institute created doubts, too. Members of the Rockefeller Foundation hesitated to believe Dunn on this point because of their extensive familiarity with internal tensions at Columbia University. Funding the institution could create more divisions among Columbia's research departments. Weaver, moreover, disliked Dunn and Dobzhansky's tone, and yet he considered them the application's best feature:

...it may be worth while to forget the rather pretentious language of their proposal and concentrate on the fact that Dunn and Dobzhansky

¹⁵ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, Warren Weaver Diary Entry, 7 Feb 1951. Dobzhansky's statements are from: APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 12 Jul 1950.

are excellent geneticists interested in human evolution, whose work over the next 10-12 years would be well worth supporting.¹⁶

Robert S. Morison of the Rockefeller Foundation's Medical Sciences Division reiterated Weaver's point about Dunn and Dobzhansky's abilities and the importance of the Institute's overarching goal: "There is no getting around the fact, however, that here are two unusually good investigators at work on extremely significant problems." Dunn and Dobzhansky revised their proposal, making its goals less lofty by following Weaver's advice that they primarily concentrate their proposal for an interdisciplinary institute on genetics. Part of the problem was that the grant fit into three of the Rockefeller Foundation's divisions: Natural Sciences, Medical Sciences, and Social Sciences.¹⁷ Undoubtedly, Dunn and Dobzhansky's credentials and vision lent weight to the grant proposal and contributed to the Rockefeller Foundation's decision to give \$90,000 over three years starting 1 July 1951 to support the Institute for the Study of Human Variation.¹⁸ In all, the Rockefeller Foundation provided the Institute with three three-year grants that totaled \$157,000.¹⁹

¹⁶ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, an unsent letter drafted by Weaver to Dunn and Dobzhansky, 22 Jun 1950.

¹⁷ RF 1.1 200D, Box 132 Folder 1633, Columbia University-Genetics, 1950-1952, letters between Dunn and Weaver, Mar to May 1951.

¹⁸ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, "Proposal regarding the Establishment of an Institute for the Study of the Biological Basis of Human Evolution," n.d; Box 132, Folder 1632, Columbia University Genetics, Resolution RF 51069, 31 May 1951; Rockefeller Foundation Annual Report 1951 (New York: Rockefeller Foundation, 1951): 226-28.

¹⁹ Rockefeller Foundation Annual Report 1954 (New York: Rockefeller Foundation, 1954): 136-37; Rockefeller Foundation Annual Report 1957 (New York: Rockefeller Foundation, 1957): 124; RAC, RF 1.1 200D, Box 132, Folder 1631, Grant Resolution RF 54063, 21 May 1954 and Grant Resolution RF 57036, 21 Feb 1957.

Dunn and Dobzhansky's keenness to get their Institute and new research running can best be understood from their remarks to each other. In the same month that they gained access to the Rockefeller Foundation grant money, Dobzhansky reminded Dunn, "Now, you know how terribly enthusiastic I am for this Institute, and what great hopes I have for its eventual success and glory. I do feel that here we have a chance of creating something really useful, which may outlive ourselves and go on growing and expanding." "I agree whole-heartedly," Dunn replied, "about the need for us both to get actual research started on human material... I can return to some anthropological interests and do some actual work in human heredity."²⁰ In terms of Dunn looking forward to returning to anthropology, his only previous project had been completed during the 1920s when he analyzed Tozzer's data on Hawaiians and Chinese-Hawaiians. It had been a long hiatus.

Dunn and Dobzhansky put together a distinguished administrative board while waiting to hear from the Rockefeller Foundation, which included themselves and the following eight biologists, anthropologists, and physicians.²¹

- George Gaylord Simpson, Professor of Vertebrate Paleontology
- Otto Klineberg, Professor of Psychology
- A. L. Kroeber, Visiting Professor of Anthropology
- Harry Shapiro, Professor of Anthropology
- R. F. Loeb, Professor of Medicine

²⁰ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 15 Jul 1951 and Dunn's reply, 27 Jul 1951.

²¹ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, "Petition to Establish an Institute for the Study of the Biological Basis of Human Evolution," 2-pages, only has '50 as a date. The petition requested acknowledgement of the Institute for the Study of the Biological Basis of Human Evolution so that they could "arrange for financial support."

- Wm. H. Sheldon, Assistant Professor of Medicine
- Earl T. Engle, Professor of Anatomy
- Richard L. Day, Associate Professor of Pediatrics

James H. MacGregor, Professor Emeritus of Zoology at Columbia University, had agreed to work with the Institute as well. Dunn headed the project and was Director of the Institute for the Study of Human Variation.

Institute for the Study of Human Variation: Operations

Dunn enthusiastically pursued the establishment of the Institute, which seemed to encounter problems throughout its short duration from 1951 to 1958. Choosing a name proved to be their first obstacle. They most likely changed the name from Institute of Population Genetics of Man because there was no longer a need to merge geneticists from different departments. Their botanist Marcus Rhoades had left Columbia University for a position at University of Illinois in 1948.²² Their second choice for a name, the Institute for the Study of the Biological Basis of Human Evolution was descriptive, but cumbersome. They voted to change the name to the Institute for the Study of Human Variation and for Research on the Biological Factors Causing Evolutionary Changes in Populations. For convenience, members dropped

²² RAC, RF 1.1 200D, Box 132, Folder 1632, Columbia University-Genetics, 1948-1949, Dunn in a phone conversation to Harry Miller, 2 June 1948, and Rhoades to Harry Miller, 7 Jun 1948.

the subtitle and referred to the organization as solely, the Institute for the Study of Human Variation.²³

Human evolution provided the Institute for the Study of Human Variation with its principal framework and guiding questions. A novel feature of the Institute was its staff's cross-disciplinary approach to human evolution, which brought together specialists trained in social, biological, medical, and mathematical sciences. Recent scientific developments provided new techniques that allowed investigators to analyze human beings from these several perspectives, and the Institute gave them a place to coordinate interdisciplinary investigations. Collaborators integrated field, laboratory, and mathematical analyses in order to gain a comprehensive understanding of individuals and populations. The Modern Evolutionary Synthesis acted as a main conceptual pillar informing the Institute's approach. Genetic principles united plants, animals, and human beings, and therefore, genetics stood at the heart of their interdisciplinary scheme. Moreover, medicine, anthropology, and the social sciences stood to gain a better understanding of human beings by incorporating genetics into their investigations. Statistical methods offered a solution for overcoming the greatest obstacle in studying human beings, the inability to conduct experiments. All in all, incorporating mathematical techniques with genetic, anthropological, and medical data, human individuals and populations could be

²³ RAC, RF, 1.1 200D, Box 133, Folder 1634, Columbia University-Genetics, 1953-1954, Meeting Minutes, Institute for the Study of Human Variation, 19 Jan 1953.

subjected to analysis.²⁴ In essence, through the Institute for the Study of Human Variation, Dunn and Dobzhansky proposed integrative and cutting-edge research. Furthermore, the Institute offered a crucial and missing element to studies in human evolution because the United States had no institutions devoted to human biology, yet more and more students wanted to learn it.²⁵

The United States did not have a strong academic tradition in either human genetics or medical genetics at the end of the 1940s. Previously, Laurence Snyder had tried to establish a Human Genetics Department at Ohio State University, but abandoned the endeavor in 1947 after seventeen years due to a lack of financial support. He had approached both the Carnegie Institution of Washington and Rockefeller Foundation for funding, but was unsuccessful. James V. Neel was one of the few American scientists pursuing human genetics in the years immediately after World War II. In 1945 the University of Michigan offered Neel a position in human and medical genetics, which he accepted but did not make a priority at first. Beginning in 1946 he conducted short and long-term studies in Hiroshima and Nagasaki for the Atomic Bomb Casualty Commission. The Heredity Clinic at University of Michigan started in 1940, and one year later a comparable program began at the Dight Institute for Human Genetics at the University of Minnesota.

²⁴ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, "Proposal regarding the Establishment of an Institute for the Study of the Biological Basis of Human Evolution," n.d.

²⁵ RAC, RF 1.1 200D, Box 132, Folder 1633, Brochure of the Institute for the Study of Human Variation, 1952-1953; Folder 1633, Columbia University-Genetics, 1950-1952, "Proposal regarding the Establishment of an Institute for the Study of the Biological Basis of Human Evolution," n.d.

Geneticist Sheldon C. Reed oversaw the Dight Institute beginning in 1947. Johns Hopkins University had several investigators interested in human genetics during the early 1950s, including Victor McKusick, Barton Childs, Bentley Glass, and C.C. Li. It is evident that more and more physicians turned to human genetics throughout the decade and that by 1959 the number of medical geneticists studying hereditary diseases had grown phenomenally.²⁶ Hence, when Dunn and Dobzhansky proposed their Institute most of the above mentioned Universities were also developing programs, but did not have established ones. As late as 1956, Dunn validated the numerous visiting professors brought in each year from abroad by noting that “The number of persons in the United States competent in human genetics, for example, is very limited.”²⁷

Undoubtedly, Dunn and Dobzhansky provided a novel approach through the Institute for the Study of Human Variation because they sought to conduct research

²⁶ Sheldon C. Reed, “A Short History of Human Genetics in the USA,” American Journal of Medical Genetics 3 (1979): 282-95, 288-89. Reed outlined the important scientists and programs in human genetics. Daniel J. Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (1985; Cambridge, Massachusetts: Harvard University Press, 1995): 209-10, 223-25, 233, 253; James V. Neel, MD, PhD, Physician to the Gene Pool: Genetic Lessons and Other Stories (New York: John Wiley & Sons, 1994): 24-25; M. Susan Lindee, Suffering Made Real: American Science and the Survivors at Hiroshima (Chicago: The University of Chicago Press, 1994). Lindee’s book is about the Atomic Bomb Casualty Commission and in it she briefly discusses the status of human genetics as a discipline in the 1940s and 1950s (76-79). Neel was a student of Curt Stern’s at University of Rochester graduating in 1939, and spent one academic year, 1941-1942, at Columbia University as a National Research Fellow working on *Drosophila* under Dobzhansky and Dunn’s direction (Lindee, 67-68; Neel, 12-14).

²⁷ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to John A. Krout, 4 Jun 1956.

not only into problems in human heredity, but also human evolution. Although investigators were becoming increasingly interested in medical and human genetics, very few turned their attention to human evolution prior to the 1960s. Arthur E. Mourant of the Lister Institute spent his career collecting blood samples from peoples all over the world and produced The Distribution of Human Blood Groups in 1954. Spencer Wells, one of today's leading evolutionary geneticists, believes that Mourant's book initiated modern human genetics because of its focus on experimental population genetics. Wells also attributes the development of using blood polymorphisms to learn about relationships between modern humans to Luigi Luca Cavalli-Sforza's work in the 1950s. After 1960, Linus Pauling and Emile Zuckerkandl suggested their Molecular Evolution Clock, which compared amino acid sequences in mammalian species in an attempt to roughly ascertain the date at which two species diverged from a common ancestor. Their innovative proposal serves as the basis for molecular evolution as a discipline.²⁸

Furthermore, those interested in human genetics at the various academic centers listed above did not attempt to collaborate with anthropologists, which was one of Dunn and Dobzhansky's main objectives. James V. Neel conducted his first field research on an Amerindian tribe with an anthropologist in 1962. Later, in 1966, he teamed up with anthropologist Napoleon A. Chagnon to study the Yanomamö,

²⁸ Spencer Wells, The Journey of Man: A Genetic Odyssey (New Jersey: Princeton University Press, 2002): 15-27; Gregory J. Morgan, "Emile Zuckerkandl, Linus Pauling, and the Molecular Evolutionary Clock, 1959-1965," Journal of the History of Biology 31 (1998): 155-78.

which has since become a well-known anthropological case study and controversial matter.²⁹ As is discussed below, Dunn and his son, anthropologist Stephen P. Dunn, performed joint field research in 1953; however, their results pale in comparison to those achieved by Neel and Chagnon.

Physical anthropologists embarked on a new trajectory incorporating genetics and evolution into their research around the time that Dunn and Dobzhansky turned their attention to interdisciplinary studies. In 1951, Sherwood L. Washburn predicted the prospects of the Modern Synthesis for anthropology and pushed anthropologists to focus on evolution and to collaborate with geneticists and other scientists.

According to Washburn, the change occurring in physical anthropology was a switch in emphasis from anthropometric techniques to evolutionary processes: “The old physical anthropology was primarily technique...The new physical anthropology is primarily an area of interest, the desire to understand the process of primate evolution and human variation by the most efficient techniques available.” Recently

²⁹ Neel, 117-34. Its reputation has two sources. Napoleon Chagnon wrote a fundamental anthropology textbook based on the studies and journalist Patrick Tierney sparked a major controversy with his book Darkness in El Dorado. Tierney blamed researchers for presenting fraudulent data, abusing aboriginal tribes, and introducing disease to the people of South America. The International Genetic Epidemiology Society (IGES) quickly refuted Tierney’s charges against Neel, which appeared the same year that Neel died and were presented in the New York Times before the release of Tierney’s book. Napoleon A. Chagnon, Yanomamö, 4th ed. (1968; Fort Worth: Harcourt Brace Jovanovich College Publishers, 1992); Patrick Tierney, Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon (New York: W.W. Norton, 2000). For information about the controversy, see: “IGES Resolution Concerning Recent Allegations against James V. Neel,” Genetic Epidemiology 19 (2000): i-ii; Rex Dalton, “Anthropologists in Turmoil over Allegations of Misconduct,” Nature 408 (23 Nov 2000): 391.

anthropologist Rachel Caspari argued that eliminating the “race concept” was a pivotal factor contributing to the shift that occurred in physical anthropology during the 1960s. Washburn’s new physical anthropology, however, relied on conducting investigations in terms of populations and not steering away from race as the principal category used by investigators.³⁰ A handful of biologists aided Washburn to formulate his ideas, including three who contributed fundamentally to the Modern Synthesis: Sewall Wright, Ernst Mayr, and Dobzhansky.³¹

Dobzhansky invested himself whole-heartedly into the promising future of interdisciplinary investigations of evolution. Dobzhansky and Washburn had organized the Fifteenth Annual Cold Spring Harbor Symposium held during the summer of 1950, at which attendees devised the “new physical anthropology.” Through the Symposium, “Origin and Evolution of Man,” the two organizers aimed to bring their two fields, genetics and physical anthropology, together and forge cooperative investigations. Biological anthropology grew from these beginnings.³² Dunn attended the symposium.³³

³⁰ Rachel Caspari, “From Types to Populations: A Century of Race, Physical Anthropology, and the American Anthropological Association,” American Anthropologist 105 (2003): 65-76.

³¹ S.L. Washburn, “The New Physical Anthropology,” Transactions of the New York Academy of Sciences, Series II 13, 7 (1951): 298-304; Gabriel W. Lasker, “The ‘New’ Physical Anthropology Seen in Retrospect and Prospect,” Centennial Review 9 (1965): 348-66.

³² Rachel Silverman, “The Blood Group ‘Fad’ in Post-War Racial Anthropology,” Kroeber Anthropological Society Papers 84 (2000): 11-27, 22.

³³ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 28 Jun 1950.

The main goal of the Institute for the Study of Human Variation mirrored that of “the new physical anthropology” because the Institute also proposed to “bring the research efforts of specialists in the several fields to bear on common problems of human evolution.” In order to accomplish their overarching aim they held seminars, taught courses, accepted graduate students, and invited visiting scholars. Dunn and Dobzhansky quickly implemented these aspects after getting approval from Columbia University’s administrators to start the Institute.

By early 1951, the Institute’s members had organized a Seminar on Human Variation. These seminars started out small, but gained in popularity and importance. Originally meeting once per month, by the third year the seminars met bi-monthly on the first and third Mondays of the month at 8 o’clock in the evening and drew students and scholars from a variety of disciplines. Regular attendees included scholars from a variety of local institutions including three members of the Institute’s administrative board: anthropologist Harry Shapiro of the American Museum of Natural History, Dr. William Sheldon of the College of Physicians and Surgeons’ Department of Medicine, and psychologist Otto Klineberg of Columbia University.

Dunn joked that it was amazing for academics of diverse personalities to come together and argue without bloodshed, and moreover that the same members repeatedly returned. This was progress, according to Dunn, and even though the group sometimes seemed to get nowhere during their discussions, ultimately the seminars were valuable. For example, an important feature of these seminars was their methodological contribution; attendees debated the conditions for evaluating

data. The seminar's topics varied according to members' interests, but covered two main themes. Researchers either presented results from work completed and in progress, or they presented a general problem for discussion.³⁴

These seminars were one feature that continued after the Institute ended in the late 1950s. The seminars started in conjunction with the Institute and eventually became part of the University Seminars series at Columbia. The University Seminars originated with Frank Tannenbaum in 1945 and developed over the years into a series of interdisciplinary seminars meeting regularly throughout the academic year. By their twentieth anniversary, participants of the various University Seminars recognized the novelty of the series. Tannenbaum reflected that his wish to counteract increasing specialization among academics had inspired the idea to bring faculty members together through evening seminars. Tannenbaum did not have a concrete recollection about the beginning of the series; however, by 1965 he was able to reflect on possible reasons for their success. The eight aspects that he listed stress the importance of intellectual and organizational freedom, voluntary participation, and camaraderie. Tannenbaum confidently stated that the Seminars would be a long-term

³⁴ RAC, RF 1.1 200D, Box 132, Folder, 1633, Columbia University-Genetics, 1950-1952, Brochure-Preliminary Announcement of the Institute for the Study of Human Variation, 1952-1953; Folder 1634, Columbia University-Genetics, 1953-1954, Dunn to Warren Weaver, 24 Nov 1953; Folder 1635, Columbia University-Genetics, 1955-1958, Institute for the Study of Human Variation-Report on Research Activities, 30 Nov 1956. UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to John A. Krout, 4 Jun 1956.

feature at Columbia University, and one that inspired other academic centers to follow their example.³⁵

As early as November 1953, Dunn had been asked to link the Institute's seminars with the University Seminars. He acknowledged that the Institute for the Study of Human Variation modeled its seminars on the University's example, yet he and other members of the Institute's bi-monthly meetings chose to keep their independence for many years. Dunn felt that the University Seminars would limit them, and he chose not to subject the participants to "too many rules from outside the group." On this issue, Dunn had a different opinion than Tannenbaum about the degree of freedom given to members of the University Seminar series.

Members of the Institute for the Study of Human Variation officially linked their meetings with the University Seminars in the mid to late 1950s. With the Institute nearing its end, and its seminars continuing to thrive, the Seminar of Human Variation changed its name to the Seminar on Genetics and the Evolution of Man. When Dunn reflected in 1965 on the Seminars of Human Variation, he summarized their original intent as providing a forum for anthropologists, demographers, blood group serologists, hematologists, medical practitioners, mathematical statisticians, geneticists, and the occasional psychologist, sociologist, medical geographer, and linguist. The Seminar acted as central feature for the Institute, according to Dunn, who valued the Seminars because scholars of all levels learned something by

³⁵ Frank Tannenbaum, "Origin, Growth, and Theory of the University Seminar Movement," *A Community of Scholars: The University Seminars at Columbia* ed. Frank Tannenbaum (New York: Frederick A. Praeger, 1965): 3-45, 3-4, 41-5.

attending. Dunn, who was retired by this time, ended his reflections in 1965 with the following advice to the University: “If a university stands for continuity in the growth of knowledge, it could well adopt just such a means as the University Seminars, and make it a central activity.”³⁶ He had tried to do this through the Institute for the Study of Human Variation, but failed to receive the necessary support from Columbia University, and was forced to close the Institute by 1958. Dunn was evidently still upset seven years later.

By early 1951 the Institute had accepted three students to study human evolution: Richard H. Osborne, L.D. Sanghvi, and William Pollitzer. And after all three graduated, Dunn remarked that he was sending them into “a rather unreceptive world.”³⁷ Richard H. Osborne conducted twin studies and graduated in 1956 with a PhD in genetics after writing about Hereditary and Environmental Factors in Body Build: A Study of 100 Pairs of Adult Twins. Osborne, a human geneticist and anthropologist, produced a book based on data collected from 1952 to 1956 while he was a student and research associate with the Institute. His book, Genetic Basis of Morphological Variation: An Evaluation and Application of the Twin Study Method,

³⁶ RAC, RF 1.1 200D, Box 132, Folder 1634, Columbia University-Genetics, 1953-1954, Dunn to Warren Weaver, 24 Nov 1953; UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to John A. Krout, 4 Jun 1956; Leslie C. Dunn, “The Seminars and the University,” A Community of Scholars: The University Seminars at Columbia ed. Frank Tannenbaum (New York: Frederick A. Praeger, 1965): 111-17, 114-17. Quotation is from page 117.

³⁷ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1957, Dunn to Landauer, 4 May 1957.

was co-authored with Frances V. De George. He later taught at the medical schools of Cornell University and the University of Wisconsin.³⁸

L.D. Sanghvi, who specialized in the distribution of blood groups in India, had already produced promising results in 1951: the caste system kept individuals of differing classes segregated to a greater degree than thought.³⁹ His results that religious rules among communities of the Brahm caste in Bombay display different gene frequencies for blood groups served as the topic of his dissertation, completed in 1954.⁴⁰ In 1959 he teamed up with anthropologist Irawati Karve of Deccan College in Pune and they embarked on a seven year field study collecting data on “a single cultural linguistic region,” analyzing ten to fifteen populations of Tamil Nadu in southeastern India.

Sanghvi described himself as a mathematician and human geneticist with specialized training in blood group serology, normal and abnormal hemoglobin, and distance analysis using computer technologies.⁴¹ Throughout his career, Sanghvi continued to study Indians primarily, but also wrote a book on Australian Aborigines.

³⁸ “Richard Hazelet Osborne,” American Men and Women of Science, 12th ed., vol. 4 (New York: R. R. Bowker Company, 1972): 4700-701.

³⁹ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, Notes in Weaver’s diary, 7 Feb 1951.

⁴⁰ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to John A. Krout, 4 Jun 1956. L.D. Sanghvi, Comparison of Genetical and Morphological Methods for a Study of Biological Differences diss., Columbia University, 1954; RAC, RF 1.1 200D, Box 132, Folder 1635, Columbia University-Genetics, 1955-1958, Institute for the Study of Human Variation-Report on Research Activities, 30 Nov 1956.

⁴¹ L.D. Sanghvi, V. Balakrishnan, and Irawati Karve, Biology of the People of Tamil Nadu (Calcutta: The Indian Anthropological Society, 1981): v-vi.

Sanghvi helped to establish the Indian Society of Human Genetics in 1973 and acted as its first president. The Indian Society of Human Genetics honors Sanghvi for his pivotal role in establishing the discipline through a lifetime achievement award that carries his name: “The oration is intended as a tribute to the pioneering role of Dr. L.D. Sanghvi in the field of Human Genetics in India and to show the appreciation of the Society for any sustained and significant contribution made to the field of Human Genetics in India.”⁴²

William Pollitzer, a less advanced student without a dissertation project in 1951, was trained in anthropology and genetics. He collaborated with Dunn to perform his dissertation research on the Negroes of Charleston, S.C.: A Study of Hemoglobin Types, Serology, and Morphology, and he graduated with a PhD in Human Variation from Columbia University in 1957. Like Sanghvi, Pollitzer studied a population that he had previously encountered and even lived among. Pollitzer was born in Charleston, and in 1973 he returned to the area to teach at the University of North Carolina at Chapel Hill, an institution that he remained associated with until his death in 2002.

Throughout his life, Pollitzer continued researching African-Americans living along the coast of South Carolina and on James Island and recently wrote a book on

⁴² L.D. Sanghvi, ed., Human Population Genetics in India: Proceedings of the First Conference of the Indian Society of Human Genetics (India: Sangam Press Ltd, 1974): 289. The history of the Indian Society of Human Genetics and a description of the Dr. L.D. Sanghvi Oration is available on the Society’s website, <http://www.ccmb.res.in/ishg/index.html> and <http://www.ccmb.res.in/ishg/newawards.html> (15 Nov 2006).

these populations, The Gullah People and Their African Heritage (1999). Pollitzer adhered to the tenets of the Institute's interdisciplinary goals and research approach as is evident in his 1999 book. He brings together genetics, anthropology, history, and other disciplines to give a multidimensional view of the Gullah tribe and presents a well-rounded description of the people, their origins, and culture by describing the Gullah tribe's population source in Africa, genetics, health, language, arts, belief system, family structures, and daily lives. In his preface, Pollitzer reflected on his years as a student at Columbia University and although he does not mention Dunn, his recollections demonstrate that the Institute succeeded in its intentions.

...my studies of genetics and anthropology at Columbia University, reinforced by changing attitudes of a postwar nation, brought insights and questions. Population genetics especially gave me a new concept of "race," not so much a distinct segment of mankind delineated by physical features, but a group breeding among themselves so long in some area of the world that they shared a common "gene pool." The migration and intermixture of people could be estimated better by the frequency of their genes than by the measurement of their physical features.⁴³

The Institute for the Study of Human Variation accepted additional doctoral candidates in the years following the admittance of these three students. Faculty associated with the Institute trained only a small number of students, but its members' impact on them seems to have been profound.

⁴³ "William Sprott Pollitzer," American Men and Women of Science 17th ed., vol. 5 (New York: R.R. Bowker, 1989-1990): 1073; William Pollitzer, The Gullah People and Their African Heritage (Athens: University of Georgia, 1999): xvii-xx, xvii. Biographical information on Pollitzer is from the American Association of Physical Anthropology website: <http://www.physanth.org/annmeet/prizes/pollitzer.html> (15 Nov 2006). Dissertation titles for Osborne, Sanghvi, and Pollitzer came from the online catalog through Columbia University.

Dunn and Dobzhansky brought several prominent visiting fellows in association with the Institute for the Study of Human Variation. One of the first people they hoped to attract was James V. Neel, but other commitments kept him from coming to the Institute until he gave a lecture there in 1953. Two others, who gave talks and were also involved in blood group analyses, were immunobiologist William C. Boyd of Boston University's School of Medicine and Arthur E. Mourant of the Lister Institute in England.⁴⁴ During 1955 and 1956, seven fellows visited for stays of one to nine months. Two women visited from London: Ruth Sanger of the Lister Institute and Sylvia Lawler of the Galton Laboratory. Two men from Italy, Ruggero Ceppellini, Head of Hematology at University of Milan and Vito Borgia, Instructor from the Albanesi Academy in Sicily, who had met Dunn during his sabbatical (which will be discussed below), also visited the Institute. Dunn noted positively on the unplanned increase in visiting investigators, and he felt that the Institute performed a valuable function by providing facilities and cooperation among

⁴⁴ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, Dunn's updated proposal to Weaver, 11 May 1951 and Dunn to Weaver, no date (date stamped 9 Jul 1951); Folder 1634, Columbia University-Genetics, 1953-54, two lists of speakers for Nov 1952 to Feb 1953 and Feb to Apr 1953. On 11 May 1951 Dunn mentioned that he wanted to get James V. Neel on board as one of the initial instructors, but had not yet asked him. Later Dunn updated Weaver that Neel could not make it and that they now hoped to entice Lionel Penrose of University College London. Neel and Penrose were by far two of the most respected medical geneticists in the early 1950s. Anthropologist Ruth Landes and immunogeneticist M.R. Irwin also spoke during the 1952-1953 school year.

scholars.⁴⁵ During the 1956-1957 academic year, the Institute persuaded J.B. Birdsall of the Anthropology Department at University of California, Los Angeles to come to New York as a Visiting Professor of Human Genetics.⁴⁶

Courses accentuated the strengths of visiting fellows and reflected the interests of its members. In spring 1953, several people lectured in the course, Human Heredity with Special Reference to Blood Differences, which had two serologists acting as the main professors: Research Associate Philip Levine, and Visiting Professor Arthur E. Mourant. Most likely this is the course that Dunn summarized as covering “serology, genetics, and geography of the human blood groups,” a course that Columbia University’s School of General Studies wanted to adopt. Dr. H. Harris of the Galton Laboratory taught Human Heredity with Special Reference to Biochemical Variation during the winter of 1953-1954.⁴⁷

In addition to studying human beings, the Institute’s staff also derived information about human variation and evolution from other species. At the Institute’s onset, Dobzhansky’s studies of *Drosophila* populations served as a model for analyzing human data, and thus he continued and augmented his *Drosophila* research. Dunn and his colleagues’ analyses of mice populations demonstrated

⁴⁵ RAC, RF 1.1 200D, Box 132, Folder 1635, Columbia University-Genetics, 1955-1958, Institute for the Study of Human Variation-Report on Research Activities, 30 Nov 1956.

⁴⁶ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, Dec 1954-Jun 1957, Dunn to Herpers, 21 Oct 1955 and Dunn to Office of the Secretary, 19 Dec 1955. Ruggero Ceppellini spent eight months at that Institute between Dunn’s return from Italy in fall 1954 and a report written two years later in November 1956.

⁴⁷ RAC, RF 1.1 200D, Box 132, Folder 1634, Columbia University-Genetics, 1953-1954, Dunn to Warren Weaver, 24 Nov 1953.

parallel development to that of humans. Knowledge obtained from mice trials about genetic constitution and its adult expression, moreover, far exceeded what was known about humans. They hoped to draw conclusions about human beings by simultaneously observing the development of abnormalities in mice and humans.⁴⁸ Beginning in the late 1950s, Dobzhansky and Dunn continued their independent studies into fly and mice population genetics, respectively, receiving money from the Atomic Energy Commission (AEC). Dobzhansky's grant covered studies into "the genetic structure of natural populations" and Dunn's was for an "ecological and genetical study of evolutionary forces acting on populations of *Mus Musculus*," a species commonly known as the house mouse.⁴⁹

On a couple of occasions Dunn summarized the Institute's research practices for learning more about human evolution. Investigations fell into three main categories: metabolic (urine and saliva), serological (blood antigens), and morphological (physical anthropology). Moreover, investigators used these methods in three ways: biological studies of socially isolated communities, the evolutionary role of physiological variation, and twin studies. Blood group distribution was used to study four isolated communities: Indian castes by Sanghvi, Roman Jews by the Dunns, Gullah Negroes by Pollitzer, and Black Caribs by Dr. I. Lester Firschein of

⁴⁸ RAC, RF 1.1 200D, Box 132, Folder 1633, Columbia University-Genetics, 1950-1952, "Proposal regarding the Establishment of an Institute for the Study of the Biological Basis of Human Evolution," n.d.

⁴⁹ Lindee, 105. Dunn performed this research with Dorothea Bennett (APS, Dunn Papers, Series I, Box 2, Atomic Energy Commission Grant AT (30-1) 1804, 16 folders covering 1960-1973, n.d.).

the Department of Anatomy of Columbia University's College of Physicians and Surgeons.⁵⁰

The other two areas of physiological variation and twin studies overlapped in some trials. Members of the Institute collected and analyzed urine in order to determine metabolic variation. One test determined that some substances in urine samples differ to a greater degree in fraternal twins than in identical twins. Upon further tests, it was found that the variation in urine of identical twins was unaffected whether the two lived in the same or different households, whereas for fraternal twins different environmental and dietary factors greatly affected the amount of variation. Fraternal twins living together under similar conditions had less variation in their urine than those living apart. The hypothesis driving additional work was that genetic factors influenced dietary and behavioral choices. Psychologist Edward Nolan, a graduate student associated with the Institute, took on the next step, which was to evaluate personality patterns among identical and fraternal twins. Another physiological study based on twins analyzed one element found in urine, Beta-amino-isobutyric acid (BAIB) because it demonstrated high inheritability. Comparative studies, not based on twins, indicated that BAIB could be used to study races. Six species of primates did not have Beta-amino-isobutyric acid, and the percentage of

⁵⁰ Firschein's research appears to have been only tangentially related to the Institute for the Study of Human Variation. He did, however, acknowledge Dunn and Levene for "their patience during the course of this study and for their critical evaluation of the manuscript." I. Lester Firschein, "Population Dynamics of the Sickle-Cell Trait in the Black Caribs of British Honduras, Central America," American Journal of Human Genetics 13 (Jun 1961): 233-54, 251.

BAIB in humans differed according to heredity: Caucasians (5-10 percent), New York African-Americans (15 percent), Black Caribs (30 percent), and Apache Indians (40 percent). Other twin studies examined the amino acid constitution of blood in healthy individuals, deafness, and biological variations of children with autism.⁵¹

A little more than one year before the Institute ended its operations, Dunn foresaw no problems for the Institute's future, and indeed its closure came rather abruptly. Although various reasons influenced Dunn's decision to end operations, each contributing factor centered around one central theme. Columbia University inadequately supported the Institute for the Study of Human Variation. Although Dunn saw the need for continued research, he eventually realized that the Institute would not be engaged in these analyses, and by 1958 Dunn and his colleagues had officially closed the Institute, even though they still had funding from the Rockefeller Foundation and the United States Public Health Service.⁵²

From the Institute's onset and throughout its duration, administrative decisions made by the University impinged on the Institute's capabilities. Obtaining an adequate physical facility was the first major problem. As mentioned above, Dunn and Dobzhansky originally acquired the Nevis Estate, which was about thirty miles north of Columbia University. When Dobzhansky found out that the University planned to charge the Institute rent in order to use the Nevis Estate, he reacted

⁵¹ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to John A. Krout, 4 Jun 1956; RAC, RF 1.1 200D, Box 132, Folder 1635, Columbia University-Genetics, 1955-1958, Institute for the Study of Human Variation-Report on Research Activities, 30 Nov 1956.

⁵² UACL, Central Files, Box 382, Folders 4-6, L.C. Dunn, 1946-1959.

indignantly and described the University's decision as "verging on immorality." Dunn and Dobzhansky discussed the option of keeping the Institute on the eighth floor in Schermerhorn Hall; however, this proved problematic because there was no room for expansion. Going to Nevis Estate would be far from the center of things, but it would allow expansion. Ultimately, Dobzhansky did not think that they could stay in Schermerhorn Hall. Even though they were unsure of what to do, Dobzhansky reassured Dunn that their graduate students would follow them and Howard Levene to Nevis Estate. Dunn continued to feel pessimistic because money also caused him concern. They had plans to remodel the Nevis Estate to accommodate their needs, but without proper funds this seemed impossible.⁵³

Apparently, the \$50,000 from the Ford Foundation would not allow the extensive renovations desired, and provisions in the Rockefeller Foundation's grant explicitly forbade paying construction costs. The Rockefeller Foundation money covered scientific equipment and operational expenses. The mansion's basement and first floor needed remodeling to accommodate scientific equipment for research. Three constant-temperature rooms needed constructing in the basement for Dobshanky and other investigators' *Drosophila*, as well as all sterilization, preparation, and washing equipment for *Drosophila* experiments. Dunn wished to enhance the barns already being used as laboratories and storerooms for mice and

⁵³ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Several letters between Dobzhansky and Dunn, from 19 Jun to 25 Aug 1951.

other experimental animal stocks.⁵⁴ They kept some rooms on the upper floors as apartments, which gave investigators a place to live. This feature allowed the Nevis Estate to function as a summer laboratory in the traditions of Cold Spring Harbor and Wood's Hole, but on a much smaller scale.

Eventually a five-story building adjacent to the Columbia University campus at 413 117th Street was acquired and converted into a laboratory, library, and offices.⁵⁵ This development upset Dunn because it divided their research facilities. The animal population investigations remained at Nevis Estate throughout the Institute's duration, and the human population work stayed close to campus, moving from Schermerhorn Hall to the house on 117th Street. Columbia University guaranteed Dunn this new Morningside Heights' location, but acted slowly to implement its acquisition and failed to inform the house's second and third floor tenants that they had to vacate it. In February 1953, Dunn had no idea when the Institute could begin renovating the facility, and when they finally started remodeling the first floor in May, Dunn did not have the heart to throw the upper-floor tenants out on the street.⁵⁶ Dobzhansky relayed his agitation to Dunn: "For two years they [administrators at Columbia University] have done nothing and I for one am not

⁵⁴ UACL, Central Files, Box 382, Folder 4, L.C. Dunn, 1946-1949 and 1951-1953, Dunn to W.E. Gentzler, 24 Sep 1951.

⁵⁵ UACL, Central Files, Box 382, Folder 6, L.C. Dunn, 1957-1959, Dunn to John A. Krout, 18 Dec 1957.

⁵⁶ UACL, Central Files, Box 382, Folder 4, L.C. Dunn, 1946-1947 and 1951-1953, Dunn to Grayson Kirk, 2 Feb 1953; RAC, RF 1.1 200D, Box 132, Folder 1634, .

willing to wait much longer.”⁵⁷ Columbia University’s actions not only thwarted and delayed Dunn and Dobzhansky’s ability to obtain physical facilities, but also destroyed their main goal, to centralize the Institute’s various disciplinary researches at one location.

In 1953, Dunn sought a promise from Columbia’s administrators about the Institute for the Study of Human Variation’s future because the Rockefeller Foundation grant needed renewing in the upcoming months. Dunn feared that the University’s attitude toward the Institute greatly damaged their chances for obtaining additional grant money, and he conveyed to Columbia’s President Grayson Kirk that the University had the power to decide the Institute’s fate.

I, at least, would struggle with temporary inconveniences if the future of this work, at least for five or ten years, seemed bright, and especially if the University regarded it as important. In the end it is the University and not only a group of those specially interested in these problems which must decide whether or not we should persist.⁵⁸

Dunn was able to obtain a Rockefeller Foundation grant in 1954 for another three years, and their facility problems subsided for the time being.⁵⁹

Institute operations went relatively smoothly for the next few years. They sought several small grants to fund specific research, and received some monetary aid in this way. Their application to the National Science Foundation focused on their

⁵⁷ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 10 Feb 1953.

⁵⁸ UACL, Central Files, Box 382, Folder 4, L.C. Dunn, 1946-1947 and 1951-1953, Dunn to Grayson Kirk, 2 Feb 1953.

⁵⁹ Rockefeller Foundation Annual Report 1954 (New York: Rockefeller Foundation, 1954): 136-37; RF 1.1 200D, Box 132, Folder 1631, Grant Resolution RF 54063, 21 May 1954.

urine and blood experiments of human beings, and would allow them to perform some follow-up trials. They received \$13,000 for the National Science Foundation covering 1956-1957. Another successful request went to the United States Public Health Service for blood and anemia studies on the Gullah tribe of South Carolina. Their research had produced a novel finding that they planned to follow up on. “Unexpectedly we found two hereditary anemias in this community,” Dunn told the Vice-President of Columbia University, “one of them certainly from the African heritage, the other for the first time in an African community. The relation between these two and the other hereditary blood factors offers some fundamental problems which lie at the bottom of our field of work.” Dr. Gartler had found something that related to cancer research and was applying for money from the Damon Runyon Fund for the Institute to perform further investigations. Dunn applied on the Institute’s behalf for money from the Wenner-Gren Foundation to pay for field work in British Honduras to complete biological observations as well as collect demographic and social information concerning the community.⁶⁰

Serological and metabolic research thrived in the 1950s, especially investigations pertaining to genetic diseases; therefore, this was a good and rewarding time to be interested in these questions. A recent scientific finding influenced laboratory and clinical investigators to turn to the study of hereditary diseases. It was the 1949 article, “Sickle Cell Anemia, a Molecular Disease,” written by Linus

⁶⁰ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Several letters from Dunn to Krout, Vice-President of University between 18 Dec 1955 and 12 Mar 1956.

Pauling, Harvey A. Itano, S.J. Singer, and Ibert C. Wells. The authors described sickle cell anemia as a genetic disease that resulted from a molecular difference in the hemoglobin of people with normal and sickle cell blood, and they introduced the electrophoresis machine as a diagnostic and investigative tool for serological research.⁶¹ Following their publication, the study of blood became quite popular. Dunn summarized the application to evolution of their discovery and subsequent work on hemoglobinopathies in his book Heredity and Evolution in Human Populations. Furthermore, he connected their finding to the evolutionary synthesis and stated that "...the picture now emerging from the rapidly growing knowledge of the hereditary anemias...has given the most encouragement to students of evolution...The evidence is compelling that natural selection is the paramount force determining the frequencies of these genes." Secondly, in 1952, the structure of DNA was figured out by James Watson and Francis Crick. Dunn surmised that abnormal structure impinged on proper function. "These changes in gene function are in turn probably traceable to localized changes in the pattern of structure, the genetic code, in the molecule of deoxyribose nucleic acid which constitutes the material basis of hereditary transmission."⁶² By the time that Dunn and other members of the Institute for the Study of Human Variation turned to these questions, the Pauling, et. al. paper

⁶¹ Melinda Gormley, "It's in the Blood: The Varieties of Linus Pauling's Work on Hemoglobin and Sickle Cell Anemia," M.S. thesis, Oregon State University, 2003.

⁶² L.C. Dunn, Heredity and Evolution in Human Populations 2nd ed. (1959, New York: Atheneum, 1973): 49, 53.

had been published, but it would be a few years before Dunn assessed its meaning it such a modern way.⁶³

After the Institute had acquired facilities and money for research, Dobzhansky had misgivings about having spent the Institute's money on investigations that he thought had led nowhere and decided to resign in 1955.

The principal reason is, of course, that I feel very keenly my responsibility for having wasted a lot of money by betting on the wrong horse called "Metabolic Variation." Never before had I such a fiasco. If I would have spent a fourth part of that on *Drosophila* populations, then results would have been, I know, worth while.

Dobzhansky decided to return permanently to *Drosophila* and apologized to Dunn for abandoning their "brainchild."⁶⁴ Dobzhansky resigned at the beginning of one of his years in Brazil at the University of São Paulo. Dunn was not easily convinced and tried appealing to Dobzhansky's paternal pride of the Institute, but to no avail. The experience heavily affected Dobzhansky. He questioned his scientific contributions to human genetics, especially after learning that he had not been invited to a conference

⁶³ L.C. Dunn, "Old and New in Genetics," May 1964. Dunn remarked on the Pauling, et al finding; "In 1949, application of methods of physical chemistry directly to the study of a protein produced by a mutated gene led Pauling, Itano, Singer and Wells to identify the specific change in the protein brought about by the gene. The discovery of the first of the abnormal human hemoglobins which they described as causing a "molecular disease"-sickle cell anemia-was followed the identification of a large number of other proteins, each of which owed its difference from normal structure to a mutated gene. Ingram then showed that the change due to the mutation, in the case of each of two abnormal hemoglobins, was confined to a single amino acid residue at one point in one of the polypeptide chains composing the globin. There could be no doubt that genes controlled protein structure by specifying the sequence of amino acid residues in the polypeptide chains. The assumed basic functional correspondence was then altered from "one gene-one enzyme" to "one gene-one polypeptide."

⁶⁴ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #8, 1954-1955, Dobzhansky to Dunn, 16 Jul 1955.

on genetics of race, a subject to which he had contributed in a significant way. It also convinced him that those who doubted their endeavor might have been right.

According to Dobzhansky, Weaver had discounted *Drosophila* and mice as important to human genetics, as did Neel, who believed that scientists could only learn about human genetics by working with human beings.⁶⁵

Dunn reintroduced the matter a year later when Dobzhansky was about to return to New York from São Paulo. Dobzhansky had not altered his opinion about his association with the Institute for the Study of Human Variation, but he offered to discuss the situation more fully when he returned. Dobzhansky summarized his desires: “As I wrote you last July, almost a year ago, I have tentatively decided to withdraw from active membership to a position of a very friendly observer only, a member of the board of directors or whatever you call it.”⁶⁶ Dobzhansky appears to have followed this course of action. He resigned as an active researcher associated with the Institute for the Study of Human Variation, but acted on its advisory board.

Italy and the Isolated Jewish Community of Rome

Dunn contributed to the corpus of data collected by the Institute for the Study of Human Variation by undertaking field work in Rome. In August 1953, after fighting to get his passport renewed, Dunn set off for Italy with his son Stephen and

⁶⁵ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #8, 1954-1955, Dobzhansky to Dunn, 14 Aug 1955.

⁶⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #9, 1956-57, Dobzhansky in reply to Dunn, 23 May 1956.

wife Louise in search of a small isolated community that would become the subject of genetic and anthropological research. By October the Dunns had chosen a Jewish ghetto community in the center of Rome among whom they lived for nine months while doing their field research.⁶⁷ Dunn had a sabbatical leave and chose his destination with the intention of spending a productive year collecting blood, urine, and saliva samples from human beings. He was going into the field to analyze human evolution through population genetics as part of the Institute's endeavors. Stephen was a graduate student of social anthropology at Columbia University and would be conducting dissertation research by analyzing the community's history, economy, and social structure. Louise participated, too, by acting as "diplomatic head-of-mission" and gathering demographic information such as surname, residence, and occupation from members of the Roman Jewish community.⁶⁸

Several aspects of Dunn's life converged in Italy while he and his family lived among the Jewish community. He went there as a result of racial misconceptions present in the United States in the years following World War II. What he got out of his time in Rome was data demonstrating the existence of an isolated, biologically

⁶⁷ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 22 Oct 1953; Series V, Box 32, Rome Jewish Community-"A Genetical Study of a Jewish Community, the Old Ghetto in Rome," n.d., page 9 of 27. For a good overview of their study and its results see: L.C. Dunn and Stephen P. Dunn, "The Jewish Community of Rome," *Scientific American* 196 (March 1957): 118-24. The term "ghetto" was originally used to designate these locations in Italy where Jews lived. It has since developed a more widespread definition to designate a dilapidated area inhabited by a minority or several minorities (Oxford English Dictionary).

⁶⁸ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-"Heredity and the Study of Small Communities," Madison Lecture Series, 1955.

unique community living in the midst of a larger population. Combining his serological findings with those of his son's anthropological survey of the community's social habits, Dunn stated that social, cultural, and political factors influenced people's decision-making about marital partners, and therefore these three forces (social, cultural, and political) drove human evolution.

Dunn, who conceived and initiated the family's investigative vacation, united several interests through this endeavor. Science undoubtedly fueled the research, but only to a certain extent because social and political factors underlined Dunn's scientific motivations. The nature of the Dunns' research trip demonstrates the overlap between his personal and professional lives and shows how he was able to bring together his motivations, experiences, and opinions that had been developing for the past thirty years. What emerges is a tangled mass of scientific, social, and political factors surrounding the question: "What is Race?"

Dunn did not consider Jews to be a race, but he believed that many people in the world misunderstood the differences between race, religion, cultural group, and language. Dunn's efforts on the behalf of refugees provided him with an intimate knowledge of the consequences wrought by the successive laws passed by the Nazi party between 1933 and the early 1940s.⁶⁹ Moreover, Dunn recognized that Nazi propaganda about a "pure" Aryan race had left a lasting impression. As discussed in the previous chapter, he wrote several publications between 1946 and 1952, in which

⁶⁹ APS, Dunn Papers, Series I, Box 10, Emergency Committee in Aid of Displaced German Scholars, 1933 and Box 11, Emergency Committee in Aid of Displaced German Scholars-Minutes, 1933.

he scientifically explained heredity to a non-technical audience while simultaneously undermining scientific racism and racial prejudices.⁷⁰

Evolution, the driving force behind the Institute for the Study of Human Variation, provided Dunn with an explanation for the development of human races. In his race publications, Heredity, Race and Society and Race and Biology, Dunn explained that human races are a product of five evolutionary processes: mutation, selection, adaptation, migration, and isolation. Of these five processes, isolation provided the best historical explanation for the development of human races.⁷¹ Isolation, according to Dunn, could be either geographical or social. Civilization, a recent historical development, brought humans into greater contact with one another and broke down the geographical barriers separating human beings. A result of civilization was an increase in racial inter-mixing and a decrease in clear racial distinctions. Geographical isolation of distinct biological races was becoming rarer as time passed; however, social and cultural factors implemented by choice or by force continued to segregate communities.⁷²

Having a good idea of the kind of research project that he wished to undertake, Dunn focused next on a location and chose Italy for several reasons. Italy had more isolated communities than many other modern countries. He considered

⁷⁰ There other editions in 1963 and 1970. I am only speaking about the 1946 and 1952 editions, which appeared before Dunn traveled to Italy to study the Jewish community of Rome.

⁷¹ Dunn, 1951, 22-26.

⁷² Dunn and Dobzhansky, 1946, 105-115; Dunn and Dobzhansky, 1952, 121-135; Dunn, 1951, 36-37.

nuclear communities a manageable research project in that they have small mating groups and typically exhibit inbred disorders. Marriages seldom occur between a member of the small isolated community and the community surrounding it, and Dunn found this a particularly interesting aspect because it is true only of *Homo sapiens*. Lastly, he wanted to go to Italy.⁷³

Dunn and Stephen enjoyed a very close father-son relationship so it is no surprise that Dunn proposed a joint study of a Jewish community to his son Stephen by early 1952. Stephen was not interested initially, but eventually changed his mind.⁷⁴ Stephen could not have accomplished field research for his doctoral dissertation without the help of his father and mother. Cerebral palsy required him to use a wheelchair throughout his seventy-one years of life because numerous operations failed to correct the alignment of Stephen's hands and feet. Although Stephen had extreme physical handicaps, he had a sharp mind and learned several languages, read extensively, studied anthropology and sociology, and wrote poetry. Stephen knew five languages by the time that he earned his undergraduate degree. He first learned the languages that Dunn knew: English, French, and German. When the Dunn family

⁷³ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-"Heredity and the Study of Small Communities," 1955. Dunn's lecture delivered at the Madison Lecture Series on 1 April 1955.

⁷⁴ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1952, Dunn to Landauer, 18 Jan 1952.

went to Oslo for Dunn's 1934-1935 sabbatical, Stephen learned Norwegian, his fourth language. In the 1940s, Stephen started learning Russian.⁷⁵

The Soviet Union was another common interest shared by father and son. During the 1940s Dunn and Stephen spent a fair amount of time together partly due to circumstances created by the war. Dunn and Louise had a hard time finding someone to help them with Stephen, and therefore they shared in aiding Stephen with his daily needs. Stephen attended school near Dunn's office on the Columbia University campus, and they met regularly throughout the day. Stephen started learning to speak and read Russian in 1943. During the same year, Dunn actively worked towards improving communication with scientists in the Soviet Union by starting the American-Soviet Science Society. Moreover, Dunn and Dobzhansky, a Russian émigré, had developed a close friendship.⁷⁶ A majority of Stephen's later sociological research conducted with his wife focused on Soviet society, which is discussed below.

Poetry and literature also captivated father and son. Dunn had seriously considered a career in literature instead of zoology, but was convinced to pursue biology, reasoning that a scientist can enjoy literature, whereas a writer cannot do science. Starting in the 1930s the family produced books of poetry together published

⁷⁵ For information about the languages that L.C. and Stephen Dunn knew, see: APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1934, Dunn to Landauer, 15 Sept 1934. About Steven learning Russian, see: APS, Dunn Papers, Series I, Box 6, Dobzhansky, Theodosius-Dunn Correspondence #4, 1943-45, Dobzhansky to Dunn, 6 Aug 1943.

⁷⁶ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #4, 1943-1945, Dobzhansky to Dunn, 6 Aug 1943.

under the name of Coalbin Press. Dunn had acquired a printing press in 1904, and from the age of eleven through his college years he had a small printing business. Eventually he gave the press to his two sons. Stephen had a sincere love of verse, and after returning from Italy printed 500 copies of his poetry book titled Some Watercolors from Venice (1956), which was the fourth book published by Coalbin Press.⁷⁷

In his dissertation, Stephen acknowledged his parents' role in helping him to graduate. "Last, but not least," wrote Stephen in his acknowledgments, "my parents provided me with the opportunity to do the field-work in the first place. Whatever value this monograph may have, they are quite largely responsible for."⁷⁸ The trip was onerous for Stephen, but they made light of it. The family had access to only cold water, which proved problematic to Stephen's physical condition and provided the rest of a family with an invigorating experience.

...the achievement of a real steamy satisfaction is the event for which we all wait. The chief sufferer is Stephen. We others clamber into that icy tub (*privatim et seriatim et maledictim*) turn on the hand spray, and by dint of scrubbing and scraping and making loud noises we get the dirt off and get our exercise to boot. But this is no good for Stephen,

⁷⁷ APS, Dunn Papers, Series I, Box 4, Coalbin Press, 1956-1958, n.d, Dunn to James Lamar Weygand, n.d.; Box 7, L.C. Dunn – Autobiographical data, n.d. Stephen P. Dunn, Leslie and Louise's son, is not the Pulitzer Prize winning poet Stephen Dunn. The other books published by the Dunns through Coalbin Press are as follows. Verses by L.C. Dunn titled The Coalbin Press Anthology (1933; 25 copies). Robert, Stephen, Louise, and Leslie Dunn, Other Countries (1942; 40 copies); L.C. and S.P. Dunn, Prose and Verse (1950; 40 copies). The one mentioned above Some Watercolors from Venice (1956; 500 copies).

⁷⁸ Stephen P. Dunn, "The Influence of Ideology on Culture Change: Two Test Cases," diss., Columbia University, 1959.

whose muscles contract in cold water so that he can't sit up or stretch out and is only miserable.

Nonetheless, the three of them, at Louise's instigation, fabricated an ideal solution, the Caracalla Cleanliness, Ltd. While a patron washed his body, they laundered and ironed his clothes. The baths were heated and the floors warmed. They even had a jingle for their hypothetical company, "The baths of Caracalla where the rich and poor will walla."⁷⁹

After returning from Italy and before successfully defending his dissertation in 1959, Stephen underwent one of many surgeries in hopes of improving his physical handicaps. This particular surgery attempted to straighten his right wrist so that he might use crutches, but like so many others it did not work.⁸⁰ Stephen got married on 6 October 1956 to Ethel Deikman after an eleven-year love affair. Ethel Dunn also suffered from cerebral palsy, but she was not as severely impaired as Stephen. She could walk. They recognized their marriage as remarkable because people with handicaps rarely married, much less married someone else with a handicap. They referred to their wedding as the Great October Revolution, in acknowledgment of their personal fortune and mutual love of the Soviet Union.

The couple's professional interests overlapped to a great extent. They jointly published several books, including The Peasants of Central Russia (1967) and

⁷⁹ APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1955-1956, n.d. Hand-written at the top of the letter is 11/15. This would have been 1953.

⁸⁰ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1955, Dunn to Landauer, 9 Apr 1955.

Introduction to Soviet Ethnography (1974). Stephen also wrote many books independently, for example Cultural Processes in the Baltic Area under Soviet Rule (1966) and The Fall and Rise of the Asiatic Mode of Production (1982). Stephen and Ethel Dunn each translated Russian publications into English; for example, for twenty-five years Stephen acted as editor of two journals that translated Russian texts into English, Soviet Anthropology and Archeology and Soviet Sociology. Ethel noted that Stephen's favorite course to teach was Comparative Religions; however, he was rarely offered the chance to teach.⁸¹

It is hard to ascertain to what degree Stephen's ailments influenced Dunn's opinion about racism and bigotry, but there is evidence that Dunn learned from Stephen about various physical and emotional issues facing those who face potential exclusion and bias. Moreover, Stephen's perspective enlightened Dunn in other ways. For example, in 1947 Stephen read "Adjustment of the Physically Disabled," which caused him "disgust and amusement" according to Dunn. Although Stephen disliked it for the most part, he found one statement to ring true. Two girls about his age were discussed in the paper. One was well-adjusted and living a life similar to that of Stephen, whereas the other was "bitter, unhappy, almost vicious." The author noted that the well-adjusted girl was interested in political and social questions and viewed her problems as small in comparison. Stephen said, according to Dunn, "And that goes for more than cripples." Stephen's spirit and motivation inspired Dunn and

⁸¹ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1956, 26 Sep 1956; Ethel Dunn, "Stephen P. Dunn," Anthropology Newsletter 40 (Jun 1999): 48.

Louise, who treasured Stephen because he gave them “a great deal of both mental & physical stimulation & much happiness.”⁸²

Although Dunn wanted to help his son, he had his own research agenda. Blood groups provided Dunn with a concrete investigative tool for conducting biological research on human populations. He planned to analyze the proportions of the A, B, O, and AB blood groups within an isolated community.⁸³ Investigating gene frequencies among a population through the analysis of blood groups had started four decades prior to Dunn’s research and in the 1920s Germans had used the method to study races.⁸⁴ Blood group analysis quickly developed into a popular research tool during the first half of the twentieth century and enough data had been collected by 1954 for A.E. Mourant to publish his book The Distribution of the Human Blood Groups.⁸⁵ As mentioned, in early 1953 Mourant went to the Institute for the Study of Human Variation as a part-time Visiting Professor of Human Genetics.⁸⁶ Mourant

⁸² APS, Dunn Papers, Series I, Box 19, Walter Landauer, 1947 and 1948, Dunn to Landauer, 1 Feb 1947 and 13 Oct 1948.

⁸³ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-“Heredity and Environment,” 1953, 18 pages. This document is a copy of a speech that Dunn gave at New York University’s “Frontiers of Knowledge,” 16 Dec 1952 to 7 Jan 1953. APS, Dunn Papers, Series V, Box 31, Rome Jewish Community – Correspondence, 1953, Dunn to Fajrajzen, 4 Nov 1953. Dunn mentioned doing blood analysis, but did not specify what would be assayed. He does refer to the blood group research in a letter to Frank Tannenbaum at Columbia University. APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1954, Dunn to Tannenbaum, 7 Mar [1954].

⁸⁴ Daniel Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (Massachusetts: Harvard University Press, 1985): 195.

⁸⁵ Wells, 16. Wells stated that Mourant’s book inaugurated the modern era of human genetics.

⁸⁶ RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University Genetics, Resolution RF 57036, 21 Feb 1957.

resided in New York in the months before the Dunns went to Italy, and during his visit Mourant helped Dunn to learn about serology and blood typing.⁸⁷ Mourant also sent laboratory supplies to Dunn in Italy for collecting and analyzing blood samples.⁸⁸ Even though Dunn acquainted himself with the literature and experimental procedures for obtaining and analyzing human blood samples, he questioned whether his field research warranted using Institute funds. Dobzhansky insisted that he must draw on their grant money since that was the point of receiving it.⁸⁹

In preparation for the trip abroad, Dunn also utilized his connections within the genetics community to gather information about isolated populations in Italy. He contacted geneticist Salvador Luria at the University of Illinois. Luria, an Italian Jew, had finished his medical degree at University of Turin in 1935 and relocated to the United States five years later. He went to the Department of Surgery at Columbia University's College of Physicians and Surgeons for his first eighteen months before moving on to other locations.⁹⁰ Luria told Dunn that World War II had increased intermarriage and adversely affected the isolation of these Jewish communities.⁹¹ Dunn contacted several scholars at Italian and Hebrew Universities, who gave him

⁸⁷ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1953, Dunn to Landauer, 19 Jan and 25 Feb 1953.

⁸⁸ APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1954, Telegrams to Arthur Mourant and Philip Levine.

⁸⁹ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #7, 1950-1953, Dobzhansky to Dunn, 22 Oct 1953.

⁹⁰ RAC, RF 1.1 200D, Box 133, Folders 1641 and 1642, Columbia University-Salvatore Luria (Refugee Scholar, Surgery) 1940-41 and 1942.

⁹¹ APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1953, Luria to Dunn, 13 March 1953.

information on the nuclear communities in Italy and aided the project while the Dunns resided in Rome and after they returned to New York.⁹² For example, Livio Livi at the University of Florence had performed similar investigations until 1931 to those that Dunn proposed in 1953. Dunn had read Livi's articles and decided to contact him for suggestions on the research that had been accomplished since 1931. Livi offered to help Dunn when he arrived and suggested that Dunn contact the President of the Jewish community in Rome. Dunn followed Livi's advice and after beginning his research, Dunn updated Livi on his progress.⁹³

⁹² Livio Livi, a Professor of Demography at University of Rome, corresponded with Leslie Dunn prior to the family's departure. Stefano Fajrajzen, Medical Director of Organizzazione Sanitaria Ebraica, helped the Dunn family in Rome as a paid research associate of Columbia University's Institute for the Study of Human Variation (APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1953, Letter to Secretary of Institute for the Study of Human Variation, 22 Nov 1953). Serologist Ruggero Ceppellini and Leslie Dunn co-authored a couple of papers on their joint investigations. Dunn contacted Elizabeth Goldschmidt in the Zoology Department of the Hebrew University in Israel. She told Dunn to contact Professor Gurevitch, Head of Medical School's Department of Clinical Microbiology at Hebrew University and Dr. F. Dreyfuss of the Department of Internal Medicine at Hadassah Hospital in Jerusalem (APS, Dunn Papers, Series V, Box 31, Rome Jewish Community – Correspondence, 1954, E. Goldschmidt to Dunn, 28 Jan 1954).

⁹³ APS, Dunn Papers, Series V, Box 31, Letters between Dunn and Livi from 13 Jan to 9 Feb 1953, and Dunn to Livi, 5 Feb 1954.

After arriving in Italy, it took the Dunns a few months to find a suitable enclave to study, but they eventually worked out all of the details. The Roman Jewish community that they chose resides east of the Tiber River not far from Vatican City. Stephen began his preliminary research on the community's history and social structure shortly after their arrival and according to Dunn was "delighted & working hard"⁹⁶ by interviewing residents and perusing archival documents.⁹⁷ Dunn did not coordinate the details for his biological research until after they chose the Jewish community in Rome. Moreover, he quickly learned that he had to obtain the trust of local community and organizational leaders as well as the cooperation of community members before he could begin the biological study.

Diplomacy and material incentives ultimately gained the Dunns access to the people and information about them. Dunn wrote a letter to Mr. Presidente, the ghetto community's leader, in which he expressed his understanding of the need for confidentiality and promised to conduct his work with discretion.⁹⁸ In addition to obtaining the trust of authorities, he also had to gain the confidence of the community members before they would submit to having blood drawn. Dunn quickly

⁹⁵ Robert C. Fried, Planning the Eternal City: Roman Politics and Planning since World War II (New Haven: Yale University Press, 1973): 42.

⁹⁶ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1953, Dunn to Landauer, 24 Nov 1953.

⁹⁷ APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Correspondence, 1953, Dunn to Steffano Fajrajzen of the Jewish Health Service, 4 Nov 1953.

⁹⁸ APS, Dunn Papers, Series V, Box 31, Roman Jewish Community-Notes, Dunn to Mr. Presidente, n.d.

comprehended how best to get blood samples and retain his welcome in the community:

We're bringing in things needed in the community (milk powder, chocolate, vitamins, drugs) to distribute as incentives or more crudely in exchange for blood. If the rabbi ever finds out that we are getting "missionary rates" on some of these products, he'll throw me out, since there's nothing he fears so much as baptisms, mixed marriages & other 'disruptive forces.' We steer clear of religious leaders as much as possible & work with the doctors.⁹⁹

They also held monthly drawings in which the winning family received food packages valued at 6200 Lire. To gain an understanding about the family size from which Dunn drew blood samples, the prize-winning families in January and February 1954 had eight and eleven members, respectively.¹⁰⁰

Dunn, in one of his more candid appraisals of bartering material things for blood samples, said:

Great success! The first packet arrived this morning & this afternoon an obdurate family rolled up their sleeves. Its hard to say whether it was papa's cigarettes or la signora's & le signorine's "nailons" [nylons] that did the trick but the blood is in the (no, not bag) tubes.¹⁰¹

Material goods successfully enticed the Jewish community's members to participate in the study because they lived in squalor and usually begged for charity. In a 1958 article, Stephen noted that the poor Jews who lived within the ghetto were a distinct group separated from more wealthy Jews living outside, and moreover that outsiders

⁹⁹ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1953, Dunn to Landauer, 24 Nov 1953.

¹⁰⁰ APS, Dunn Papers, Series V, Box 31, Rome Jewish Community-Notes, "A Biological Study of the Comunita Ebraica di Roma," n.d.

¹⁰¹ APS, Dunn Papers, Series I, Box 9, Marvin L. Edwards # 4, 1953-1954, Dunn to Edwards, 13 Mar 1954.

gave charity readily to ghetto residents. Stephen often received money, which he surmised resulted from his dependency on a wheelchair.¹⁰²

Ethical questions abound in reference to the Dunns' methodology of using material incentives in order to gain access to members of the Roman Jewish community; however, their tactics should be viewed within a post-war context. The years following the war's end subjected many people in war-torn countries to hard times, in part because trade lines were not reconnected immediately. Dunn had lots of experience sending books and other publications overseas to academics in the years during and following World War II. In fact, while in Rome, Dunn worked with the non-profit organization C.A.R.E. (Committee for American Remittances to Everywhere), which provided him with coffee, baby food, and high protein food for the people in the Jewish community. A pamphlet about CARE summarized its aim: "CARE is a key to open hearts locked by war, fear, mistrust and want..." Dunn thanked Mrs. Olen for providing CARE with items that were given to the Jewish community. He felt that her donation was helpful not only to the families who received the goods, but also to his attempts to do scientific research:

We are trying to find out what the factors are that have held such a community together for 2000 years and what have been the results of continued marriages between relatives, since nearly all marriages are contracted within the community. We take samples for analysis of blood, urine, and saliva, and as incentives to cooperate in our study we have given food, vitamins [sic] and medicines. This is where your

¹⁰² Stephen P. Dunn, "An Outsider Visits the Roman Ghetto: The Past Lives On," Commentary 25 (1958): 130-40, 134-35.

packages did double duty both as relief and as a contribution to scientific research. They have been received with gratitude.¹⁰³

Interpretation and Substantiation of Field Work

Through his anthropological and sociological analysis, Stephen not only found that the ghetto community relied on charity, but also that the historical circumstances surrounding the community members' isolation had caused them to develop a close kinship and solid identity. Most residents living in the ghetto in the mid-twentieth century did not want to leave the ghetto's imaginary confines, which had a long legacy. Jews had lived in Rome for over 2,000 years since their arrival to the area in the 2nd century BCE, and were segregated from Catholics after laws were passed during the height of the Roman Empire. Walls were erected in the mid-16th century in response to the influx of Jews ousted from Spain. These walls physically isolated the Jewish community from Christians for over 400 years, until they were partially torn down by Napoleon Bonaparte's troops in 1803. Laws enforcing Jewish separation

¹⁰³ APS, Dunn Papers, Series I, Box 3, C.A.R.E. (Committee for American Remittances to Everywhere), 1953-54. The correspondence starts in Nov 1953 with a letter from Francis X. Mayers, Chief of Mission, CARE and ends with a receipt for Dunn dated 4 Feb 1954. Dunn sent Mrs. Olen the people's names to whom her CARE packages were given. Dunn said three families wanted to thank her themselves, but since they could only write in Italian he sent their messages of appreciation along with their names and addresses. Dunn also told Mrs. Olen about the study that he and others were doing and about the people in the community. The term "care package" was originally CARE package, referring to the Cooperative for American Relief Everywhere, a post-World War II organization providing relief, according to the Online Etymology Dictionary (<http://www.etymonline.com/> accessed 2 Dec 2006). These appear to be two distinct organizations sharing the same acronym, CARE.

from Christians were eradicated in 1870 and afterwards some ghetto residents stayed in the neighborhood, choosing to live as their ancestors had lived.¹⁰⁴

The Jewish community remained proud of its ancestry and religion, and furthermore based its identity on those two factors: ancestry and religion. Yet, Stephen found that the community's older inhabitants knew little about Judaism and that their religion actually meant very little to them. Children of the community, however, started attending a separate school in 1938 as a result of anti-Semitic laws imposed on Italian Jews. When the Dunns lived in Rome this school thrived, and through it children from the Jewish community learned about their religion and culture and came to understand the religious holidays that their families continued to celebrate. Thus, the Dunns witnessed a shift occurring among members of the Jewish community of Rome: the younger generation was embracing their religion, although most of their parents knew little about its rituals and their meanings.¹⁰⁵

Louise's demographic studies ascertained that the Jewish community of Rome consisted of about 4,000 people. In the periphery around the ghetto community resided an additional 8,000 people whose relatives had been members of the original Jewish ghetto community. The Dunns concentrated on the 4,000 people in the nuclear community and collected biological samples (blood, urine, and saliva) from about

¹⁰⁴ Stephen P. Dunn, "An Outsider Visits the Roman Ghetto: The Past Lives On," *Commentary* 25 (1958): 130-40.

¹⁰⁵ Dunn, "An Outsider," 130-40. "These people," wrote Stephen, "now form a tightly knit, economically specialized and depressed, highly tradition-minded group." Dunn and Dunn, "Jewish Community," 118-28.

650 of them.¹⁰⁶ An assessment of surnames demonstrated that twelve last names were common to fifty-percent of the ghetto residents. Roman denizens recognized these surnames as typical of the Jewish ghetto community. The Dunns compared these twelve names to rosters of those deported during the Holocaust, and found a similar frequency of common surnames among the 2,000 plus people listed on the German rosters. Dunn noted that few returned after deportation, which greatly affected the community and left many widows and orphans.¹⁰⁷ “Conditions in the ghetto are particularly bad,” explained Dunn, “for the Germans deported and killed 2000 people from this community, most of them parents, and the community as a whole had to look after the dependents of these.”¹⁰⁸

In terms of the enclave’s marital habits, endogamy was evident within the nuclear community based on interviews and health records. Dunn’s biological results provided him with evidence of intermarriages within the ghetto’s community because it supplied solid proof of the enclave’s uniqueness in comparison to surrounding populations. With the help of others, Dunn found two significant differences in gene frequency which separated the Jewish community from the larger population.¹⁰⁹

¹⁰⁶ APS, Dunn Papers, Series V, Box 32, Rome Jewish Community-“The Ancient Jewish Community of Rome,” manuscripts, n.d. (with S.P. Dunn): 5-6 of 16. The most recent citation on this manuscript is 1967.

¹⁰⁷ APS, Dunn Papers, Series V, Box 32, Rome Jewish Community-“A Genetical Study of a Jewish Community, the Old Ghetto in Rome,” n.d: 13-15 of 27.

¹⁰⁸ APS, Dunn Papers, Series I, Box 3, C.A.R.E. (Committee for American Remittances to Everywhere), 1953-1954, Dunn to Mrs. Olen, who provided items through CARE for the Jewish community, no date.

¹⁰⁹ Ruggero Ceppellini and his staff at a laboratory in Milan helped Dunn to test the blood samples (Dunn and Dunn, 1957, 124).

There was a high frequency of blood type B within the Jewish community (at 26.1%) when compared with two non-Jewish population samples taken from Romans (11.3%) and Central Italians (10.6%). Although European Jews tended to have a higher frequency of blood type B, the Jewish community of Rome had the highest frequency known to date. They also found over four-percent of r' (Cde), an allele of the Rh system. The norm among Italians was under one-percent. Dunn noted that these two unique gene frequencies, blood group B and the r' (Cde) allele, could not be attributed to adaptive processes based solely on their evidence; however, he postulated that gene flow (i.e. migration), fluctuations in sampling, and natural selection could account for their results.¹¹⁰

Biological results were only part of the equation for Dunn. The information gathered during the Dunns' trip to Italy allowed them to construct a multidimensional view of the Jewish ghetto community in Rome.¹¹¹ Their data, however, did not provide conclusive results. Stephen defended his dissertation in 1956, but his committee told him he needed more evidence. Dunn acknowledged that his son's

¹¹⁰ APS, Dunn Papers, Series V, Box 32, , Rome Jewish Community-"A Genetical Study of a Jewish Community, the Old Ghetto in Rome," n.d.: 16-20 of 27, and Rome Jewish Community-"The Ancient Jewish Community of Rome," manuscripts, n.d. (with S.P. Dunn): 7-9 of 16. Dunn remarked that the Jewish population demonstrated no observable hereditary diseases and that they had not tested for any either.

¹¹¹ RAC, RF 1.1 200D, Box 132, Folder 1631, Columbia University Genetics, Resolution 57036, 21 Feb 1957. Although the "Roman Campaign of '53-'55" did not produce solid scientific evidence, Dunn felt that he had demonstrated the importance of cross-disciplinary research on human populations. Work conducted through the Institute convinced Warren Weaver and other members of the Rockefeller Foundation to continue their support of the Institute for the Study of Human Variation until 1960. Columbia University was semi-supportive and marginally cooperative. The Institute closed in 1958 for various reasons, including Dunn's impending retirement.

dissertation was “thin” and thought the experience would be good for Stephen.¹¹²

Stephen investigated a second group, re-wrote his dissertation as a comparison of two cultures, and successfully defended three years later, in 1959. Stephen analyzed a historical case for his second culture, Mexican Indians and the Conquest of Mexico. This event occurred between 1516 and 1650, and therefore, Stephen read historical documents to collect his data. His field work and conclusions on the Roman Jewish community filled almost two-times as many pages as the Mexican Indian survey.¹¹³

L.C. Dunn recognized that his scientific data also presented inconclusive results. In 1961, he called himself an amateur when discussing his work in human genetics for readers of American Naturalist, and he pointed out that his investigations had started some interesting new avenues for research, but had not provided definite answers. As discussed below, Dunn had added data on an African-American community to his studies by 1961. The Jewish and African communities were biologically unique compared to the populations surrounding them; yet, Dunn’s biological data only demonstrated that “Jews marry Jews, and Africans, Africans.” More cross-disciplinary investigations had to be undertaken before further conclusions could be drawn, he wrote in 1961.¹¹⁴ Work along the lines mentioned by Dunn would be attempted, but not by him. He wrote the American Naturalist article

¹¹² APS, Dunn Papers, Series I, Walter Landauer, 1956, Dunn to Landauer, 7 Apr 1956.

¹¹³ Stephen P. Dunn, “The Influence of Ideology on Culture Change: Two Test Cases,” diss., Columbia University, 1959.

¹¹⁴ L.C. Dunn, “Big and Little Populations: An Amateur’s Excursion,” The American Naturalist (May-Jun 1961): 129-36, 136.

one year before he retired and a few years after the Institute for the Study of Human Variation had ceased to exist. Although he continued to perform experiments after retiring, he focused on mice and not human beings.

As mentioned, Dunn collaborated with members of the Institute on the study of another isolated community, African-Americans living in South Carolina. William S. Pollitzer, a student with the Institute for the Study of Human Variation, wrote his dissertation on a population living in Charleston, and another study assessed a nearby group living on James Island. Both surveys analyzed the genetic variations in hemoglobin and blood group antigens among these two populations. In addition, Pollitzer performed some physical anthropological assessments, and his investigation dealt with a more integrated group of African-Americans than that found on James Island.¹¹⁵ Dunn and others helped Pollitzer with his assessments of the Charleston population. Dunn considered these studies comparable to those on the Jewish community in Rome.¹¹⁶

Even though this project formed the basis of Pollitzer's dissertation, it appears that Dunn enrolled Pollitzer rather than Pollitzer suggesting the idea to Dunn. During the fall of 1954, Dunn was in South Carolina giving four talks on "Heredity and the Community" when some African-American physicians presented him with the idea of

¹¹⁵ APS, Dunn Papers, Series V, Box 31, James Island Community and James Island, South Carolina-Genetics Material. Dunn wrote a one-page, hand-written, and undated summary of Pollitzer's work and the subsequent James Island survey.

¹¹⁶ William S. Pollitzer, R. M. Menegaz-Bock, Ruggero Ceppellini, and L.C. Dunn, "Blood Factors and Morphology of the Negroes of James Island, Charleston, S.C.," American Journal of Physical Anthropology 22 (1967): 393-98.

conducting research on a Charleston community. Together, they decided to proceed with the project, eventually incorporating researchers from Howard University and the South Carolina Medical College. Dunn returned to South Carolina to tour areas pertinent to his Negro population studies, collecting blood samples. During this trip he spent Easter with a local reverend who was “in charge of church, school, and state of the Legreeville Community on James Island.”¹¹⁷

The Institute’s researchers analyzed the South Carolinian communities’ bodily fluids, especially blood. To their surprise, investigators not only found an abnormal hemoglobin known for its presence in African-Americans (sickle cell hemoglobin), but they also found one never before detected in an African population¹¹⁸ Traveling south again, Dunn went to South Carolina and Georgia, accepting an invitation to give a lecture in Macon because it coincided with his trip to Charleston. While on the road, Dunn started planning the next phase of research, which involved following up on these two forms of abnormal hemoglobin.¹¹⁹ Both were known forms of abnormal hemoglobin resulting in the diseases, sickle cell anemia and thalassemia. Thalassemia was considered an ailment plaguing people of Mediterranean descent, but eventually it was shown to be prevalent among people from a variety of geographical locales and

¹¹⁷ APS, Dunn Papers, Series I, Box 20, Walter Landauer, Dunn to Landauer, 30 Oct 1954, 17 Feb 1955, and 9 Apr 1955. Pollitzer noted his interest in the South Carolina studies because of having grown up in Charleston and stated that he started his dissertation in the mid-1950s. He is not more specific about how he decided to study the Gullah of South Carolina and Georgia (Pollitzer, xvii-xviii).

¹¹⁸ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to Krout, 20 Jan 1956.

¹¹⁹ APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1956, Dunn to Landauer, 7 Apr 1956

racés.¹²⁰ Finding abnormal hemoglobin in the Africa-American populations prompted Dunn to submit a grant application to the United States Public Health Service, even though Dunn recognized that hemolytic anemias were peripheral to the original research plan.

Although our primary purpose is to study normal variations in human populations, we feel it incumbent on us to follow up new leads by first steps which may pave the way for medical studies by others later on. But in each case we follow only those which seem of scientific importance for the understanding of the hereditary constitution of human populations.¹²¹

Although Dunn published few articles on the South Carolinian Africans, he performed extensive analysis on the data and learned a fair amount about them. They shared social and cultural traits, including language, with inhabitants of the West Coast of Africa. Pollitzer's physical anthropology, blood group, and hemoglobin type analyses confirmed that the Gullah people living in the area near Charleston had a similar biological constitution to those residing on Africa's western coast. Moreover, Pollitzer showed that the Gullah Africans living in South Carolina had remained relatively separate from the Europeans that they lived among. Pollitzer based his studies on a random population selection, which differed from the tactic taken with the James Island study. Those associated with the James Island study focused on

¹²⁰ D.J. Weatherall, "Toward an Understanding of the Molecular Biology of Some Common Inherited Anemias: The Story of Thalassemia," Blood, Pure and Eloquent: A Study of Discovery, of People, and of Ideas, ed. Maxwell M. Wintrobe (New York: McGraw-Hill Book Company, 1980): 370-414.

¹²¹ UACL, Central Files, Box 382, Folder 5, L.C. Dunn, 1954-1957, Dunn to Krout, Vice-President of University, 18 Dec 1955.

variation by assessing gene frequencies with the ultimate goal of comparing this population to other African populations in the United States.¹²²

The James Island community had between 140 and 150 intermarrying families, of which Dunn and his co-workers collected complete hemoglobin data on forty-five of these families, a total of 280 people, and gathered incomplete samples on some of the remaining families. The blood samples were subjected to electrophoretic analysis, which showed the presence of sickle cell trait in 16% of the samples collected. Two other abnormalities, hemoglobin C and thalassemia hemoglobin, were each present in 2-3% of the samples. In two of the families, they found a new phenotypic difference of the A₂ hemoglobin variety.¹²³ Hemoglobin A is the designation given to the most common type of normal adult hemoglobin which consists of having two alpha and two beta chains. A₂ refers to the less common normal adult hemoglobin in which the person has two alpha and two delta chains. The existence of A₂ was first realized in 1955, but the difference between hemoglobin A and A₂ was not clarified until after 1960.¹²⁴ Dobzhansky responded enthusiastically

¹²² APS, Dunn Papers, Series V, Box 31, James Island, South Carolina-Genetics Material, Dunn's handwritten notes, 1 page, n.d.. Dunn refers to "we" but I do not know exactly who helped him with the James Island research.

¹²³ APS, Dunn Papers, Series I, Box 7, L.C. Dunn-"Abnormal Hemoglobins, Thalassemia, and Blood Groups in a South Carolina Negro Population" n.d., Abstract, 1 page.

¹²⁴ Weatherall, 382-83. Weatherall attributes the finding of hemoglobin A₂ to H.G. Kunkel and G. Wallenius of the Rockefeller Institute, and confirmation of it in 1959 to Ruggero Ceppellini, the researcher who Dunn worked with in Italy and who visited the Institute for the Study of Human Variation for eight months sometime between the autumns of 1954 and 1956. Weatherall cited: H.G. Kunkel and G. Wallenius,

upon hearing about the new abnormality found: "...no matter what it is, a new blood gene allele is not found every day, and this will put the Institute on the map. It is very good news."¹²⁵

Dunn published on the Roman Jews more than on the James Island Africans, which can most likely be explained by one or both of the following reasons. First, the Institute for the Study of Human Variation was nearing its end around the time that Dunn was processing the James Island information. Shortly after the Institute closed, Dunn submitted the blood data collected under its auspices on the Roman Jews and African-Americans to the University Microfilm Bureau in Ann Arbor, Michigan.¹²⁶ Second, Dunn continued to publish articles and give lectures on the Roman Jews into the 1960s in connection with his son, which most likely reflects Dunn's concern for Stephen's career.

In summary, Nazi and eugenic laws drove Dunn to broach questions about biological races and racial prejudices. He used science to speak about these societal issues plaguing not only Americans, but everyone affected by the discriminatory propaganda circulating widely during the first-half of the twentieth century. He made claims about human genetics and race in several publications before attempting a scientific investigation on human beings. Inspired by his theories, Dunn started the

"New Hemoglobin in Normal Adult Blood," *Science* 122 (1955): 288; R. Ceppellini, L'emoglobin normale lenta A₂," *Acta Genet Med Gemellol* (Roma) 8 (1952): 47-68.

¹²⁵ APS, Dunn Papers, Series I, Box 6, Theodosius Dobzhansky-Dunn Correspondence #8, 1954-1955, Dobzhansky to Dun, 13 Dec 1955.

¹²⁶ APS, Dunn Papers, Series I, Box 24, William S. Pollitzer, 1960-1962, Dunn to Pollitzer, 24 Jun 1960; APS, Dunn Papers, Series I, Box 24, Leon Poliakov, 1961, Dunn to Poliakov, 24 Oct 1961.

Institute for the Study of Human Variation and conducted population genetics studies on two small isolated communities in which he used blood groups to analyze human evolution.¹²⁷ The combination of information gathered by Dunn, his son, social anthropologist Stephen P. Dunn, and his wife, Louise P. Dunn, provided him with a multidisciplinary perspective on the Jewish community in Rome. Thus, Dunn responded to discriminatory laws by attacking racial prejudices, and decided to start an interdisciplinary Institute to study human populations. This led him to go into the field and collect data on human beings in an attempt to study human evolution, and his efforts produced fruitful, yet preliminary investigations into the evolutionary history of human beings.

Before going into the field, Dunn had proposed in his UNESCO pamphlet, Race and Biology, that geographical and cultural isolation act on human populations and produce races.¹²⁸ After the family completed their analyses of the Jewish ghetto community, Dunn had his biological data, Louise's demographic survey, and Stephen's social, economic, and historical information.¹²⁹ Dunn's conclusion supported his previous statement about factors that separate populations. Thus, his

¹²⁷ Elazar Barkan states that "While [Lionel] Penrose did original research in human biology, Dunn did not." I have demonstrated the error within this statement. In terms of Dunn's work in human biology, Barkan cites only Dunn's publications on Hawaiians, which Dunn completed in the 1920s. Elazar Barkan, The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars (Cambridge: Cambridge University Press, 1992): 268.

¹²⁸ L.C. Dunn, Race and Biology (Paris: UNESCO, 1951): 25-26.

¹²⁹ Stephen P. Dunn, "The Influence of Ideology on Culture Change: Two Test Cases," diss., Columbia University, 1959; APS, Dunn Papers, Series I, Box 20, Walter Landauer, 1956, Dunn to Landauer, 7 Apr 1956.

family's researches affirmed his views, and in 1959 he re-stated that "Isolating factors in human populations may be geographical or social." He also defined the aspects that create barriers: "Differences in language, religion, or custom may act as social impediments to mating." Then he added:

In some societies race prejudice – a social acquirement – may be an isolating factor, as in the southern United States. The Roman Jewish community has been a social isolate formed under the influence of a variety of historical factors both religious and economical.¹³⁰

As noted in his statement, Dunn linked the situations facing the Jewish community living in Rome and the African-American community of James Island.¹³¹

Dunn had a specific agenda in choosing to investigate two communities that suffered lengthy periods of isolation and discrimination. His study of the African-American community living in the southern United States addressed racial prejudices, segregation and the inferior treatment of African-Americans. His study of the Roman community focused on a Jewish group that had been persecuted in a fascist country during World War II. Dunn gathered biological and cultural data that would allow him to counter biases held over from Nazism and that had long been affecting African-Americans. Yet, although he used his science to investigate two specific social situations and the cultural problem of discrimination that deeply distressed him, Dunn did not publish much about racism in the years after his return from Italy. Instead he kept to scientific matters when discussing human beings for the most part, and addressed discrimination in general rather than specific cases of racism.

¹³⁰ Dunn, 1973, 113-14.

¹³¹ Dunn, 1973, 121; APS, Dunn Papers, Series V, Box 31, James Island Community.

Culminating Efforts: Publications and Legacy

Dunn acted as a co-author on scientific papers submitted to scientific journals, but for his own purposes, he chose to write his genetic results in a language accessible to non-technical readers.¹³² This was true for his final book Heredity and Evolution in Human Populations, which was largely based on his knowledge of human genetics and understanding of serology and provides a summary of his research accomplished through the Institute for the Study of Human Variation. In it, Dunn presented a geneticist's view of evolution and human races by largely keeping his discussion to scientific topics.¹³³ The book was published as part of the Harvard Books in Biology Series that aimed to educate laymen.¹³⁴ Dunn had plenty of experience writing about scientific topics for a general population by the time that he undertook this monograph.

In his book, Dunn summarized the new approach that geneticists offered to the study of evolution as the difference between assessing genotypes and phenotypes. Geneticists and anthropologists examined genotypes, which deal with an individual's genetic constitution, when they performed surveys of blood groups. Blood groups allowed investigators to learn about gene frequencies, which was the best approach for understanding human evolution. Older methods analyzed phenotypes, which are

¹³² Some examples include: American Journal of Physical Anthropology and Scientific American. His book, Heredity and Evolution in Human Populations, was also written for a general audience.

¹³³ Dunn, Heredity and Evolution in Human Populations, 1959.

¹³⁴ Garret Hardin, "Heredity and Evolution in Human Populations," Science 129 (24 Apr 1959): 1123-24.

the physical features that are based on a person's genetic makeup. Descriptions of a person's traits and summaries of a race's typical features depended on criteria such as head shape, arm length, hair color, and skin color. These are phenotypes, according to Dunn, and phenotypes are affected by environmental factors to a greater degree than genotypes.¹³⁵

After explaining to his reader the benefits of blood group antigen research for learning about human evolution and variation, Dunn discussed the research that had been performed on isolated human populations. He discussed six communities in total, five of which were analyzed by members of the Institute for the Study of Human Variation. The five studies associated with the Institute were Dunn's Jewish Romans, Sanghvi's Indian castes, Pollitzer's Charleston Gullah tribe, the James Island African-Americans, and Dr. I. Lester Firschein's Black Caribs of British Honduras. Bentley Glass of Johns Hopkins University conducted the other study on Dunkers, a group descended from German Baptists and living in Pennsylvania.¹³⁶ Ultimately, these studies reinforced Dunn's view that social and cultural factors implemented by choice or by force have kept human communities segregated for centuries. These social and geographical barriers succeeded at creating biological variation among the world's population. Even as civilization and urbanization

¹³⁵ Dunn, 1973, 92-96.

¹³⁶ Dunn, 1973, 109-22. The chapter is titled, "Isolated Populations and Small Communities" (106-31). Dunkers is the name given to German-American Baptists living in Pennsylvania who baptize through a triple immersion. They were originally called Tunkers and Tumblers, which meant "to dip," and at some point the name was inadvertently altered (Oxford English Dictionary).

decreased the number of geographical barriers, social isolation continued to impede integration. Therefore, human beings were subject to evolutionary forces that did not act on other living organisms.¹³⁷ Considering that isolated groups were his main focus, he also discussed the effects of consanguineous (cousin) marriages on a community as dependent on the population size and original genetic constitution. The Institute's studies on Jewish Romans, Indian castes, and Dunkers failed to find deleterious effects from consanguineous marriages. Evidence demonstrated that the homozygous content of a population's gene pool did not increase. Dunn concluded that the only true differences among human beings are those that people display through superficial means, such as dress and habits, and those that are evident after biological analyses. He further stated that it was erroneous to validate the common assumption that an isolated community is a group of idiots or degenerates. The biological data allowed only preliminary conclusions, but for Dunn it showed that small populations with a few thousand members could reproduce without detriment and therefore were essentially no different from their surrounding populations.¹³⁸

Reviewers liked the book and found few flaws with it. Human geneticist Bentley Glass commented on Dunn's prose of "perfect lucidity in handling the subject," and his scientific competence to produce "a comprehensive, balanced view of all its [the book's] main aspects." Moreover, Glass hailed Dunn for his ethical stance because Dunn showed "evidence of a social conscience well-disciplined by

¹³⁷ Dunn and Dunn, 118-24; Dunn, 1973, 113-14.

¹³⁸ Dunn, 1973, 122-27.

respect for scientific evidence.” It should be reiterated that Dunn drew on Glass’s research to bolster his points and even extensively quoted Glass.¹³⁹ Over fifteen years after Dunn wrote Heredity and Evolution in Human Populations, it was “still looked upon as an indispensable source of scientific data on race.”¹⁴⁰

Conclusion

Ultimately, through his field research on human populations Dunn wished to understand how cultural and social factors contributed to biological disparities. He, thus, asked: How can observable differences be analyzed through that which is not seen?¹⁴¹ In other words, he hoped to come to terms with the relationship between genotype and phenotype by analyzing human beings at both macro- and micro-levels. Ultimately, Dunn asked “what came first?” Do cultural factors, such as religion and language, cause biological differences? Or did the biological differences cause the creation of unique cultures?¹⁴²

Even though Dunn did not obtain an answer to his causal question, he gained other insights through his field research. His investigations substantiated his opinion that information once thought to be truisms needed re-evaluation in light of new and updated methodologies. Previously, evolution could not be experimentally tested and

¹³⁹ Bentley Glass, “Heredity and Evolution in Human Populations,” The Quarterly Review of Biology 34 (Sep 1959): 239-40; Dunn, 1973, 130-31

¹⁴⁰ Smythe, 77.

¹⁴¹ APS, Dunn Papers, Series V, Box 32, Rome Jewish Community-“The Ancient Jewish Community of Rome,” manuscripts, n.d. (with S.P. Dunn): 3 of 16.

¹⁴² APS, Dunn Papers, Series V, Box 32, Rome Jewish Community – “Man in the City” (Symposium Lecture), 1968: 1-2 of 3.

concepts in human genetics had to be derived from animal and plant genetics. The research available in 1959 when he published Heredity and Evolution in Human Populations convinced Dunn that scientific investigations of human beings and evolution were fully possible, and that more work needed to be done. Easy and final answers were not obtainable, but headway would continue to be achieved and more information collected. Over his lifetime, Dunn had learned a valuable lesson, one that he shared with his readers. Genetics was not the exact science that its proponents claimed it to be when he started his studies in the 1910s. Rather genetics was an explorative science with a promising future.¹⁴³

¹⁴³ See Dunn's Prefaces to the 1959 and 1973 editions of his book Heredity and Evolution in Human Populations. Both are in the 1973 edition (v, vii-viii).

Conclusion

Science was an integral part of Dunn's life, and those in his scientific community were his friends. He assumed that every scientist, especially geneticist, whom he met, would become his friend, and therefore their relationship would be more than simply professional. "I've been so lucky in scientific friendships," Dunn told Walter Landauer in 1927, "that I've come to expect that every scientist is ipso facto a potential friend, and may share more than his professional ideas with me."¹ As he was nearing retirement over thirty years later, Dunn still felt the same way. He told D.F. Jones:

One of the most satisfying features of a life spent in scientific work is that our colleagues so often become our friends; and that their contributions to building up the science to which we are devoted become in this way a part both of the intellectual and the personal life of all of us.²

Indeed, Dunn fostered personal relationships with his academic colleagues, and he made these people his friends. He viewed science as a way of life, and it permeated every aspect of his livelihood.

Dunn's outlook on the meaning of science and scientists in his life guided his actions, and as a result he built networks of colleagues and created a scientific

¹ APS, Dunn Papers, Series I, Box 18, Walter Landauer, Jul-Nov 1927, Dunn to Landauer, 15 Aug 1927.

² APS, Dunn Papers, Series I, Box 17, Donald F. Jones, 1960, Dunn to Jones, 3 May 1960.

community around himself. Originally, I had hoped to delve deeper into the complexities of these communities and networks in this study, but biographical material is scant for a fair number of these people without additional archival research. Analyzing Dunn's activities is a large first step toward a more ambitious project on members of his community and these networks. Two projects that can build from this one are examinations of American geneticists as a scientific community and the use of scientific networks for political ends.

What I have been able to do is ascertain who were some of the more influential and pivotal people in Dunn's scientific and political life. They were geneticists: Theodosius Dobzhansky, Walter Landauer, Hermann J. Muller, Milislav Demerec, Curt Stern, D.F. Jones, and Henry A. Wallace.³ They were anthropologists: Franz Boas, Ruth Benedict, Otto Klineberg, and of course his son, Stephen P. Dunn.⁴ They specialized in other biological sciences: biophysicist Selig Hecht and neurophysiologist Harry Grundfest. They were preeminent scholars from other scientific disciplines: astronomer Harlow Shapley, physicist George B. Pegram, and sociologist Robert S. Lynd. This is not an exhaustive list, but from it some generalizations can be made about Dunn's network, which reflect on his activism.

³ Henry A. Wallace was also Secretary of Agriculture, Vice-President (1941-1945), and Secretary of Commerce. Robert Cook could easily be added to this list; however, he was an antagonist in Dunn's life, who nonetheless pushed Dunn into action (Journal of Heredity) and caused Dunn to clarify his ideas (Heredity, Race and Society).

⁴ All four were associated with Columbia University's Anthropology Department. Otto Klineberg was also trained in psychology.

Several had ties to the Soviet Union,⁵ and to Germany.⁶ Except for Muller, they resided primarily in the northeastern United States during the years surrounding the Second World War.⁷ Moreover, some were local to New York City,⁸ whereas many worked at Columbia University.⁹

In addition to the topics covered in my dissertation, Dunn contributed fundamentally to mouse genetics. This is an important aspect of his life and one that has not been adequately explored above. In an expansion of this present study, I intend to explain Dunn's scientific legacy in mammalian genetics of which his intellectual descents speak so highly. Three authors published a two-volume book for the International Committee on Standardized Genetic Nomenclature for Mice and dedicated it to Dunn, Hans Grüneberg, and George D. Snell, "whose pioneering efforts produced the first rules for mouse genetic nomenclature and set an example for the habits of consultation and co-operation among mouse geneticists that still

⁵ Theodosius Dobzhansky and Harry Grundfest were born in Russia. H.J. Muller lived for many years in the Soviet Union. Stephen Dunn studied the Soviet Union's culture and society.

⁶ Franz Boas, Walter Landauer, and Curt Stern spent many years in Germany before moving to the United States. Milislav Demerec grew up in Austria-Hungary, coming to the United States for graduate studies in 1919.

⁷ Harlow Shapley was at Harvard University. Walter Landauer and D.F. Jones spent their careers at Agricultural Experiment Stations in Connecticut in Storrs and New Haven, respectively. Henry A. Wallace worked in Washington D.C.

⁸ Milislav Demerec was at Cold Spring Harbor and Curt Stern at University of Rochester.

⁹ Franz Boas, Ruth Benedict, Otto Klineberg, Selig Hecht, Harry Grundfest, George B. Pegram, and Robert S. Lynd were employed at Columbia University and its College of Physicians and Surgeons.

endure.”¹⁰ In his textbook Mouse Genetics: Concepts and Applications, Lee M. Silver recognizes Dunn’s laboratory methods as the source for his ability to develop “a feeling for the organism” while he was a postdoctoral fellow in Dorothea Bennett’s laboratory at the Sloan-Kettering Institute.

Dorothea had a rule (handed down from her mentors, L.C. Dunn and Salome Waelsch) that each scientist in the lab, herself included, had to spend at least two mornings each week in the “mouse room” examining animals, recording litters, and setting up new matings. Mouse room time was meant to serve two purposes. The first was to maintain and track a large breeding colony with hundreds of genetic variants in various experimental crosses. The second purpose was to provide each student and postdoc passing through the lab with an intimate look at the creature that is the mouse.¹¹

Today, Silver bridges science and politics as a professor of Molecular Biology and Public Policy in the Woodrow Wilson School of Public and International Affairs at Princeton University.

What can be said about L.C. Dunn from the above examination of his socio-political activism on behalf of scientists and other academics? He was diplomatic, helpful, efficient, trustworthy, genial, and genuine. Even from the early stages of his

¹⁰ Mary F. Lyon, Sohaila Rastan, and S.D.M. Brown, eds., Genetic Variants and Strains of the Laboratory Mouse, 3rd edition, Vol. 1 (Oxford: Oxford University Press, 1996): v. They also dedicated it to Earl and Margaret Green who did the database work.

¹¹ Salome Waelsch and Dunn were colleagues for many years at Columbia University, conducting research together for about twenty years, from the mid-1930s to mid-1950s. Silver rightly attributes the phrase, “a feeling for the organism,” to corn geneticist Barbara McClintock and recognizes Evelyn Fox Keller for highlighting this point in her biography of McClintock. Lee M. Silver, Mouse Genetics: Concepts and Applications (New York: Oxford University Press, 1995): xi-xiii; Evelyn Fox Keller, A Feeling for the Organism: The Life and Work of Barbara McClintock (San Francisco: W.H. Freeman, 1983).

professional career, Dunn rarely, if ever, doubted his leadership abilities and throughout his life he took on many administrative positions. Not only did he act quickly and tactfully, but he also knew how to get results. When a problem arose that Dunn believed needed attention, he went directly to the chief executive officer. For example, he wrote to the President of the Carnegie Institute of Washington when asked for feedback about the Eugenics Record Office, cabled the Soviet Ambassador in Washington D.C. when the Seventh International Congress of Genetics was cancelled, and contacted Senator Harley M. Kilgore to give him feedback about his legislation for governmental funding of science.

In his activist endeavors, Dunn gained proficiency in the subject, developed strong opinions about it, and felt passionately about its outcome. Hence, he was a concerned citizen and informed resource. This also helps to explain his shift over time from acting at the private level to taking a public stance. He typically started on committees performing behind-the-scenes activities and later ventured into the public sphere. This can be seen in his response to fascism with the Emergency Committee in Aid of Displaced Foreign Scholars and Faculty Fellowship Fund, and then his participation with the American Committee for Democracy and Intellectual Freedom, especially through his efforts in aid of Spain. As a consequence of World War II, he gathered information and drafted legislation before publishing his opinions in Science and speaking at Congressional hearings. He also took a similar tactic with eugenics and scientific racism. Before the war, Dunn was cautionary but did not take a strong anti-eugenic stance in his published statements, whereas his committee work

attempted to destabilize eugenics. In the years following the war he published extensively on the benefits of human variation in an attempt to eradicate racism, especially scientific racism. Furthermore, he sought to reach readers from a variety of backgrounds, and therefore produced a variety of publications ranging from technical to pictorial. He approached politics in a systematic manner, much like his research methodology.

Dunn had faults, too. He was naïve when it came to matters pertaining to the Soviet Union. Dobzhansky described him as “idealistic to a fault,”¹² an attribute that created some very disheartening and trying situations for Dunn during the Cold War years because of his earlier campaigns against fascism and in behalf of Soviet scientists. Dobzhansky recalled that

Violence, cruelty, and inhumanity were repellent to Dunn, regardless of whether they came from the Right or the Left. The attacks on his truly selfless and idealistic activities were an unexpected and therefore more bitter disappointment. He almost entirely withdrew from socio-political activities into himself and his science.¹³

Contributing to Dunn’s disillusionment was his optimism and faith in humanity, as well as his tendency to develop grandiose ideas that he was usually forced to abridge. His hopes for an institute of genetics and later the Institute for the Study of Human Variation at Columbia University caused him difficulties and disappointments over several years. Additionally, he occasionally was indignant and pompous, as was the

¹² Theodosius Dobzhansky, “Leslie Clarence Dunn (1893-1974).” Yearbook American Philosophical Society (1974): 150-56, 155.

¹³ Theodosius Dobzhansky, “Leslie Clarence Dunn, November 2, 1893-March 19, 1974.” Biographical Memoirs of the National Academy of Sciences 49 (1978): 79-104, 87.

case with his interactions with members of the Rockefeller Foundation at times and his description of the “stiffnecked” members of Columbia’s University Committee on National Defense.

One of his most damaging faults, however, was his inability to say no. Even when he felt the need to curtail his activities, he could not always follow through with his intentions, as was true of his attempt to resign from the Emergency Committee in Aid of Displaced Foreign Scholars. He overworked himself to the point of exhaustion and illness. When Persis Miller heard that Dunn had suffered a heart attack in June of 1940, she wrote to him saying, “You are able to do so much valuable work that everyone counts on you to do more than is possible for any one man.”¹⁴ She was right. Others expected a lot of Dunn, but he also demanded a lot from himself. After he recovered from this condition, Dunn did not take it easy, but rather increased his responsibilities until the end of the war. He finally reduced his tasks by leaving New York on a sabbatical leave in 1946, and he needed a little over one-year to recuperate. Then he returned to New York and to his old ways.

Dunn had an eventful and rewarding life, full of pains and pleasures and defined by periods of disillusion and purpose. He also had a long life, dying on 19 March 1974 at the age of eighty. Dobzhansky wrote obituaries of Dunn for the National Academy of Sciences and American Philosophical Society, one of which pleased Dunn’s wife immensely. Louise congratulated Dobzhansky on fittingly

¹⁴ APS, Dunn Papers, Series I, Box 3, British Scientists-plans for evacuation, 1940, Persis Miller to Dunn, 30 June 1940.

summarizing Dunn's finest attributes into one sentence, declaring that "No truer statement could be made about Dunn." Indeed, Dobzhansky succinctly characterized him: "Leslie Clarence Dunn was as admirable as a human being as he was eminent as a scientist."¹⁵

¹⁵ APS, Dobzhansky Papers, B: D65, Louise Dunn, Louise to Dodick (Dobzhansky), 29 Aug 1974; Theodosius Dobzhansky, "Leslie Clarence Dunn (1893-1974)," Yearbook of the American Philosophical Society (1974): 150-56.

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