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AN INTELLECTUAL JOURNEY: TRANSGRESSIONS OF A NEOCLASSICAL ECONOMIST

by

Emery N. Castle

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Chapter One--The Journey²

AT THE BEGINNING

These pages describe my intellectual journey, begun in 1946, and not yet completed in 2005. Because I believed economics was relevant to the welfare of low-income people—an accurate description of my family—I enrolled in an economics correspondence course from Kansas State University in 1942. Although the course did not impress me, I nevertheless completed one semester at the University prior to entering the Army Air Force during World War II. In January 1946, following my discharge from active duty, I returned to K-State.

An intellectual awakening occurred in the summer of 1946 in a course labeled “Applied Economics,” taught by Professor Edgar Bagley. He developed one or more analytic models for several public policy issues, and then used those models to isolate policy choices for class discussion. Our discussions included monetary and fiscal policy, labor economics, international trade, and agricultural policy. Mastering those analytical models, and then discovering their relevance for public policy, “hooked” me from that time to the present. It was then that I first considered becoming an economist.³

A FLEDGLING ECONOMIST

After I completed work for the baccalaureate in 1948, the K-State Department of Economics and Sociology offered me a graduate assistantship and the opportunity to obtain a masters degree, which I accepted, and found myself taking courses from Bagley again. This time it was economic theory--we read Marshall, Chamberlin, Robinson (Joan) and J. M.Keynes. The Department faculty believed economic theory was important, but it was not employed much in Department research. I used a model from Marshall’s Principles to guide empirical work in my

master's thesis. After the thesis was completed, I published a note in *Land Economics*, my first journal article (Castle, 1950).

While working for the masters, I was assigned to teach a recitation section of the economics principles course, although I had not yet studied graduate economic theory. At that time higher education institutions were still struggling to accommodate the large number of veterans stimulated to attend college by the G. I. Bill. Mostly engineering students attended my recitation section. I told them at our first meeting I had no teaching experience, and if I did not know something I would tell them so, and provide an answer at the next class meeting. They responded most positively. I learned some calculus in the process, as well as much economics. This teaching experience also taught me there is no disgrace in saying: "I don't know." I concluded if such a practice were used more widely, science generally, as well as economics, would benefit greatly.

When the masters work was completed, I became a full time employee with the rank of Instructor. The K-State Department of Economics and Sociology had dual responsibilities. The Department was responsible to the School of Arts and Sciences for instruction in economics and sociology, and to the School of Agriculture for research and instruction in agricultural economics. It became apparent a doctorate would be necessary if I were to advance in the academic world. I liked teaching and economic theory, and thought of myself as an economist specializing in agricultural economics.

At that time Earl Heady, a faculty member at Iowa State University, was acquiring super star status in agricultural economics. He drew on the theory of the firm and statistical analysis to revolutionize teaching and research in farm management and production economics. He attracted outstanding graduate students from throughout the United States, as well as from other countries.

I decided to do my doctorate at Iowa State, under Earl Heady's direction, after considering opportunities at the University of Wisconsin and the University of Chicago.

Iowa State was similar to K-State in that both economics and agricultural economics were in the same department. Thus, throughout my formal education, my primary department was academic home for both economists and agricultural economists.

The graduate program at Iowa State required rigorous work in neo-classical economics. J. R. Hicks and ordinal utility were emphasized, rather than Alfred Marshall and cardinal utility, as was the case at K-State. The "new" welfare economics was developing, and we read the emerging literature in that subject. Given my interest in economic theory, it is not surprising I was curious about economists who preceded Alfred Marshall. I believe the greatest weakness of my graduate work was that it did not provide adequate opportunity to learn how economic doctrine had developed.

I returned to K-State in 1951 as an assistant professor after passing my preliminary examinations at Iowa State and wrote my doctoral dissertation in absentia, a common practice at that time. The dissertation pertained to risk and uncertainty in wheat farming in western Kansas. I was able to obtain experimental data that allowed me to test different strategies for coping with rainfall variation. This research made clear that the practices of farmers, who are practical people, dealt better with the risk and uncertainty realities inherent in Great Plains conditions than did recommendations from the Kansas State University Extension Service (Castle (a), (b), (c)).

When I took a draft of my findings to Iowa State, I expected that Earl Heady would request additional empirical work and extensive re-writing. Instead, he told me I should return to K-State and put my dissertation in final form. I obtained my PhD from Iowa State in August 1952, approximately six and one-half years after I returned to college in 1946 with one semester

of college credit. Following World War II, many academic institutions offered both undergraduate and graduate classes during summer sessions as well as the regular academic year. The G. I. Bill provided financial support for each of the three degrees I obtained.

THE FEDERAL RESERVE BANK OF KANSAS CITY

Shortly after obtaining my doctorate, I accepted a position as agricultural economist at the Federal Reserve Bank of Kansas City. A colleague at K-State, Ray Doll, had gone to the Bank a short time earlier and recommended me. The research department there employed six PhD economists, two in each of the three fields--financial economics, industrial economics, and agricultural economics. It was the responsibility of the research department to provide economic intelligence to the Federal Reserve Bank of Kansas City and member banks in the Tenth Federal Reserve District. We did this by writing numerous articles and briefing papers as well as with speaking engagements.

This was useful experience. I became aware that economics provided a framework for analysis and reasoning that accounting and business management usually did not provide. I also became aware that a considerable gap often existed between the empirical knowledge reflected in the economic literature, and the actual decisions policy makers and business people must make. There were three other young economists in the research department holding doctorates in economics from Harvard University, the University of Minnesota, and the University of Michigan. Finding that I could hold my own with them in economic discussions, led me to better appreciate my graduate education at K-State and Iowa State.

My wife, young daughter, and I enjoyed living in Kansas City with its numerous cultural attractions. In contrast to my academic work, my duties at the Bank were easily fulfilled, and I had time for such activities. I was never socially comfortable at the Bank, however, because I

was exposed to affluence on a scale I had previously only imagined. Rank, title, and status were important to many at the Fed, and were manifested in explicit and subtle ways. I recognized the longer I stayed at the Bank, the more difficult it would be to move to an academic position. My bank experience, beyond one or two years, would not count for much in academia. In addition, the salary differential would become greater with each passing year.⁴

After one full year, with the support of my wife, I decided to test the academic waters. In the summer of 1953 I attended the annual meeting of the American Farm Economic Association in Corvallis. While there I met G. Burton Wood, Head of the Department of Agricultural Economics at Oregon State, where there was a vacancy, and I expressed interest. I also canvassed other Land Grant Institutions in the nation. I was not overwhelmed by institutions seeking my services, but offers from Texas A & M as well as one from Oregon State were forthcoming. I joined the Oregon State University (then College) July 1, 1954 as Assistant Professor, Department of Agricultural Economics, School of Agriculture.

OREGON STATE UNIVERSITY

I fell in love with Oregon and Oregon State immediately. I was given specific responsibilities and freedom in their discharge. It was easy to communicate across academic units, and there were numerous avenues for promotion, tenure, and salary improvement. Although I published regularly, I never felt pressure to do so. Over the years there have been opportunities at more ideally structured, financially lucrative, and academically prestigious universities, nevertheless, I am happy that Oregon State has been my principal academic home for most of my intellectual journey.

The one serious anxiety I felt almost immediately at OSU was the administrative separation of economics from agricultural economics. At that time the Economics Department

was in Lower Division. Lower Division departments were not permitted to have departmental majors and could not offer disciplinary degrees. Their mission was to serve the remainder of the campus through undergraduate instruction. This was not good news in agricultural economics because our graduate students needed to take advanced courses in economics. In addition to these administrative problems and obstacles, for the first time I became aware of differences in traditions and aspirations between agricultural economics and economics. It turned out the administrative separation and cultural differences between the two would constitute the most serious professional issue I would encounter at OSU. I have been a part of several efforts to improve conditions, but the basic problem remains.

The natural resources and the agriculture of Oregon are, of course, greatly different from those in the mid-west. Prior to undertaking longer run projects, I requested work on problems in different parts of Oregon that would not take long to complete. One such problem was in the Christmas Valley in Oregon. A land developer from California had settled there, and acquired options on considerable land, much of it having been abandoned by earlier settlers. The developer had drilled irrigation wells, and advertised that with modern technology it was possible to grow several crops profitably. Both the Soil Conservation Service (SCS), and the Farmer's Home Administration (FHA) were concerned. The SCS because they were getting questions about soils, but believed the length of the growing season and economics were the important issues. The FHA because they were getting requests for loans they did not think could be repaid.

I agreed to take the lead in investigating the feasibility of irrigation in the Christmas Lake area. The SCS assigned a senior economist, Carol Dwyer, to work with me. We completed our work and came to pessimistic conclusions about irrigation farming there, except for extensive

crops such as alfalfa hay. When we had our work in manuscript form, the developer learned of our conclusions, and brought suit against Oregon State to stop publication. Here I was, the first crack out of the box, an assistant professor, without tenure, bringing a lawsuit down upon my institution!!!

This experience taught me how responsible administrators behave under pressure. A.L. Strand, our president, told Dean F. E. Price that he, the president, had another difficult problem at that time, and that Price should handle Christmas Valley. Price then went over our study with me sentence by sentence asking me to defend each step. I could defend every statement save one. The Dean told me to reflect on the one I could not defend. I did so, and changed the statement, although our conclusions were not affected. From that point on, Price never wavered in his support. An Oregon Assistant Attorney General defended Oregon State College in the lawsuit. The plaintiff's motion was denied when the matter went to court, and we thought we were home free. However the developer then persuaded several of his farmer neighbors to go with him to see the Governor. The Governor told them to seek a meeting with Oregon State to explore a possible compromise. At about this time the Oregonian, the state's most prominent daily newspaper, published an editorial that opened with the sentence "An Experiment Station is not a booster club."

The developer and his group did come to Oregon State for a meeting. The Dean presided, they presented their complaints, and I responded. Except for the developer, most of the group seemed to be convinced by my arguments. It was a relief to have this over with, and I learned much from this minor scrimmage (Castle and Dwyer).

At that time public sentiment was very much with resource development. The presumption was that economic development should occur unless there were strong reasons to do

otherwise. In general, federal and state public policies were biased toward development (Kimball and Castle). The burden of proof has now shifted, and “political correctness” lies with environmental protection. An understanding of the political landscape is helpful in anticipating responses to economic studies when they enter the public domain.

Three Significant Responsibilities

Three significant responsibilities were assigned me shortly after I began work at Oregon State:

1. Teach an undergraduate course in farm management and a graduate course in production economics.
2. Develop a research program in water resource economics.
3. Provide leadership for the development of the Department graduate program.

There was an active masters program, and authorization to offer the doctorate. After I arrived, I learned the Department Head and other administrators had ambitions to develop the doctoral program.

Teaching Farm Management. The farm management teaching responsibility (item 1 above) led me to write *Farm Business Management*—a textbook aimed at sophomores and juniors. I invited Manning Becker, a colleague, to share authorship and be responsible for three chapters pertaining to farm records. For several years we both taught sections of the undergraduate farm management course, although shortly after I arrived at OSU, Manning became administratively responsible to the OSU Extension Service with an assignment to develop a farm management extension program. He retained a part time teaching appointment.

The farm management extension program he developed was an unusual one. He decided to emphasize management principles and to illustrate them with realistic examples from parts of

the state other than where the teaching was occurring. A more applied approach, commonly used across the country, tackled actual local problems wherever the class was located often with little explicit reference to economic principles. Manning's belief was that, confronted by realistic but unfamiliar examples, farmers were less likely to question illustrations or examples, and more likely to struggle with underlying management problems and economic principles.

I assisted Manning with a great deal of extension teaching across Oregon and I did so, in part to learn more about Oregon agriculture. This involvement with Manning Becker taught me a great deal about extension work in Land Grant Universities. I developed respect for extension workers, and an appreciation for the work they do. I also learned that subject matter knowledge and disciplinary competence can greatly elevate the effectiveness of extension work. My intellectual journey returned me to extension issues later in my career (Castle 1993 (a), (b))⁵.

One lesson was that farmers approached farm management differently than did many farm management books of that time. Most farm management texts were organized much as were economics principle books concerning the theory of the firm. Selection of production techniques and enterprises were treated prior to the acquisition of the factors of production--land, labor, and capital. Farmers viewed their problems in a different way. They first confronted problems of size and scale in the presence of risk and uncertainty. Then they addressed acquisition of the factors of production, and this provided the context for considering production techniques and enterprise selection.

Three guidelines were established for *Farm Business Management*. First, it should be written so that it could be mastered without previous training in economics or accounting. Second, although elementary, the economics should be consistent with state of the art economics pertaining to the theory of the firm. Third, it should reflect the view farmers and farm managers

have of the problems they address, and the decisions they make. There have been two revisions of the book--one with Fred Smith (1972), and one with Gene Nelson (1987). It has been in print approximately four decades, and has been translated into (I believe) six languages. More than 100,000 copies have been sold. Judged by traditional criteria, *Farm Business Management* has been a success. I make no apologies for the elementary nature of the material, but I am uncomfortable with the farm management and agricultural policy implications that stem from neoclassical economics, as it existed at that time, and as it was (and often still is) incorporated in agricultural economics writings. The conventional wisdom was that constant returns to scale existed in farming (an increase in the factors of production would result in a proportionate increase in output). This provided rationale for farms of different size existing side by side in American agriculture. But we knew farmers had become larger and less numerous with the passage of time. We had a responsibility to note this inconsistency and to advance possible explanations. I regret that was not done in *Farm Business Management*. Much agricultural policy literature suffers from similar neglect.

Subject Matter Generalization #1. We now know economists, as well as other scientists, are reluctant to abandon the core principles of their discipline, and we understand better the reasons for this attitude (Hausman; Kuhn). Constant returns to scale have been assumed in much economic modeling, and were thought to be necessary to equilibrium solutions. But that is a technical disciplinary problem and should not dictate how reality is viewed. Economic research during the past three decades has demonstrated that increasing returns, or decreasing costs, may well characterize much that occurs during economic development (Arthur; Buchanan and Yoon). Innovations and “learning by doing” contribute to declining costs. Furthermore, farm firms do

not remain the same over time and this changes the contribution they make to the economy over time. For example, a farmer may purchase inputs that were previously provided on the farm, and may perform more, or fewer, of the marketing functions than in an earlier period. Even though the farmer continues to produce a homogeneous commodity, physically defined, (say wheat), the economic contribution per unit produced will not be the same from one time period to another unless farm-industry relations remain constant.

Agriculture and farming are often used in economic textbooks as examples of firms within an industry having a homogeneous commodity. Yet heterogeneous products abound in agriculture. A few miles from where this is written farmers produce corn for animal feed, as well as for human consumption. Some sell corn for canning, some corn for freezing, and some is marketed directly through farmers' markets. Under such circumstances corn is far from a homogenous commodity even though there may be little difference in the ears of corn affixed to stalks in the field. Heterogeneous products may better explain why farms of differing size exist side by side than does constant returns to scale with an implicitly assumed homogeneous output.

A Water Resources Economics Research Program--The development of a water resource economics research program became a significant part of my intellectual journey. At that time regional committees were financed and sponsored by the Farm Foundation to facilitate and encourage cooperation among Land-Grant University economists on important regional problems. I was appointed to the Western Water Resources Research Committee. Members included S. V. Ciriacy-Wantrup, Maurice Kelso, Mark Regan and other notable members of the profession. Meetings were devoted to discussions of needed research on the policy ramifications of water programs and institutions. These meetings were multi-disciplinary in nature with

political scientists, lawyers, and engineers in attendance. Lawyers and engineers had been the most important professionals in water resources in the West, but it was believed additional expertise could contribute as well (Castle 2000).

The Committee meetings were among the most intellectually stimulating experiences I have encountered. The policy issues pertaining to land involved rules, customs, fragmentation, multiple jurisdictions, and social conflict. Yet much of the teaching and research at Iowa State, based on neo-classical economics, considered land to be no different from any other economic asset. Some production economists referred to land economists as “factor specialists” suggesting they were not connected to mainstream economics, even though Adam Smith and Alfred Marshall had devoted considerable attention to institutional issues. The Water Committee did not get bogged down in such matters. They just took water problems in whatever direction seemed promising.

Two committee members made contributions that have influenced me throughout my career. Wantrup’s book on resource conservation is indeed a classic. His “safe minimum standard” continues to guide public policy, and it was he who first noted that questionnaires could be used to ascertain what values people place on environmental quality. This led to the contingent valuation methodology that has received much attention in recent years. The other person was Mark Regan. He was the United States Department of Agriculture’s continuing member of the federal inter-agency group that created the “Greenbook” (Report of the Interagency Committee). This provided the first framework for applying benefit-cost analysis to water resource development projects. As an employee of a contributing agency to the interagency group, Mark was not at liberty to publish independently on those issues and discussions. Nevertheless he was exceedingly generous with his time and frank in his discussions

with the Water Committee. He understood the technicalities of welfare economics well, and correctly anticipated benefit-cost and water resources issues that dominated the literature for several years. I owe much to Wantrup and Regan (Smith and Castle).

My experience with the Water Resources Committee stimulated me to make contact with economists working with federal water resource agencies in the Pacific Northwest such as the Corp of Engineers, Bureau of Reclamation, and the Soil Conservation Service. I developed an appreciation for the pressures and complexities these people faced in the discharge of assigned responsibilities as they attempted to acquire technical competence, and maintain professional standards.

The OSU water economics research program gave primary attention to the comparability, or lack thereof, of market and extra-market values in economic analysis. Outdoor recreation and water quality were given special attention. Clifford Hildreth, Leo Hurwicz, and Allen Kneese came to OSU at different times to assist with the design of specific research projects. During this period I developed a lifelong friendship and professional association with Charles Warren, a fisheries biologist in the Department of Fisheries and Wildlife at OSU. Long before the notion came to be accepted in resource and environmental economics literature, he convinced me a person may derive satisfaction from knowing something exists in nature, even though not experiencing it directly (“passive use values”).

Charles Warren also called my attention to the political significance of the relative economic value of the sport and commercial fishery in Oregon. He told me of plans the Oregon Game Commission had to survey sport fishers about their expenditures as a way of assigning an economic value to the activity. Warren persuaded me to inquire of the Game Commission if they would finance an economic research study of the salmon-steelhead sport fishery.

At that time several other economists were also working on conceptual models to estimate the economic value of outdoor recreation. Marion Clawson, at Resources for the Future, came into possession of a one-page letter that noted economist Harold Hotelling had written to the National Park Service. In this letter Hotelling stated how travel expenditures for outdoor recreational purposes might be used to estimate a demand function for outdoor recreation. Clawson, using hypothetical data, illustrated such a demand function in a lecture given at the University of Wisconsin (Clawson). When I saw the Hotelling-Clawson model I knew it was superior to my own formulations, and I became interested in using it to guide empirical work.

Following Warren's suggestion, I obtained a contract from the Game Commission to estimate a demand function for the salmon-steelhead sports fishery in Oregon. Game Commission data made it possible to obtain expenditures of salmon and steelhead sport fishers located at different distances from their fisheries. I knew the estimation of functions required by the Hotelling-Clawson model would require econometric skills I did not possess, so I enlisted the help of a long time colleague, William G. "Bill" Brown and Ajmer Singh, an OSU graduate student. This was one of the first attempts to estimate empirically a demand function for outdoor recreation with state of the arts econometric techniques. The research results were published in an Oregon Agricultural Experiment Station publication that has been cited extensively in the professional literature, and attracted the attention of policy analysts. It permitted the estimation of the net economic value of fishing effort as well as salmon-steelhead caught. (Brown, Singh and Castle, 1964, includes a copy of the Hotelling letter to the National Park Service).

Numerous empirical studies of outdoor recreational demand were conducted subsequently at OSU and elsewhere. Contingent valuation has since become relatively more popular in recreational demand estimation than travel cost methodology. Alan Randall, a former

OSU graduate student, has been among the more active resource and environmental economists in developing and applying contingent valuation methodology.

The Water Resource Research Committee experience, my study of welfare economic theory, and work with agency people in the Pacific Northwest provided opportunity to evaluate benefit-cost analysis in both theory and practice. (Castle, Kelso, and Gardner). The amount of empirical data required for benefit-cost analysis is indeed impressive. Benefit-cost results often are sensitive to variations in particular parameters, and skillful analysts soon identify such relationships. Research economists, relying on ex post analysis of actual projects, typically have been critical of ex ante benefit-cost estimates made by development agency economists. Yet few research economists have compared their predictions for resource development project impacts with actual results. Even so, a group of economists, including Nobel Laureates, have signed a petition calling for mandatory benefit-cost tests of environmental regulations, and recommend peer review as a means of insuring the quality of benefit cost analysis (Arrow et al.; Castle 1999).

Charles Warren was instrumental also in helping me initiate research on the economics of environmental quality. He called my attention to a water quality problem in Yaquina Bay, the estuary touching both Toledo and Newport, Oregon. Georgia Pacific, an integrated forest product firm, was then in the process of locating a pulp and paper plant at Toledo, near the east end of Yaquina Bay. The question was how to manage effluent from the plant to preserve the environmental quality of the estuary. A considerable amount of economic and recreational activity would be negatively affected if untreated effluent were discharged into the estuary. With advice from Charles Warren, I prepared a research proposal and submitted it to the federal agencies that were predecessors of the Environmental Protection Agency. The proposal was for

an economic analysis of the estuary with special attention to be given to the economics of environmental quality and outdoor recreation. The proposal was funded and the study was made. I have been told ours was the second grant made by the federal government for such a study, although such investigations are now commonplace.

Herbert Stoevener came to Oregon State as a research associate to manage the study and our families became life-long friends. I served with pride as his escort at the ceremony where he became a Fellow of the American Agricultural Economics Association. Joe Stevens and Adam Sokoloski wrote theses stemming from this work. Howard Horton from the Department of Fisheries and Wildlife participated, as did personnel from the Department of Civil Engineering. This study is important in the intellectual journey described here for several reasons, but especially because of an agreement between Georgia-Pacific and state and local interests regarding the treatment of the effluent. Local interests desired the pulp and paper mill because it promised to employ many people. Yet uncontrolled discharge of the effluent threatened the local tourist industry. The alternative selected was to pipe the effluent overland to the ocean west of Newport. This protected the estuary, and permitted the pulp and paper plant to operate. Even so, it was a relatively expensive activity for Georgia Pacific, a fact that was important in the subsequent policy analysis.

The direct and indirect economic benefits were estimated that stemmed from the recreational use and amenity values of the estuary using “state of the art” techniques, although present day methodology probably would yield greater recreational values. The indirect economic value of the pulp and paper mill to the community, consisting mainly of increased employment, was estimated as well. Georgia Pacific provided information on the cost of effluent disposal, but pulp and paper mill profits were not made available. Clearly, the advantages of the

Toledo location to Georgia-Pacific were greater than the costs of pumping the effluent to the ocean. Otherwise, Georgia Pacific would not have located there.

The following assumptions provided a base for the institutional analysis:

1. Those affected by group economic decisions should have an opportunity to participate in making those decisions.
2. The economic institutions and economic variables for the total economy were assumed fixed or given.
3. A resource management institution is inadmissible if it does not meet the benefit-cost criterion for economy as a whole (benefits must exceed costs).
4. Subject to the above, institutional design should provide for community choice in the distribution of community or geographic area income that arises from inherent comparative advantage.

The received wisdom in resource and environmental economics at that time (and to a considerable extent still) did not extend beyond the maximization of net national income (Kneese). If this were the only objective, untreated effluent could have been dumped into the estuary, because the cost of the effluent disposal method adopted (pumping to the ocean) was greater than the combined estimated direct and indirect economic value of the recreational use of the estuary. Did this mean that the method adopted, (pumping to the ocean), diminished the net national income? The answer clearly is: “No, because pumping to the ocean did not deprive the Nation from having a pulp and paper mill at that location.” Georgia Pacific was willing to pump the effluent to the ocean because of the advantages of this particular location relative to other possible locations. To generalize, the comparative economic advantage of particular areas offers the possibility of different distributions of the income stream that stems from that advantage so

long as the net national product is maximized. In the Yaquina Bay case, possible combinations of economic variables gave rise to six distinct bargaining alternatives without necessarily violating the maximization of the net national income criterion (Stoevener et al.).

The comparative advantage of a particular place or community may be based on natural, human, or social attributes. Incorrect perceptions of community, or place-related, comparative advantage lead to flawed local public policy. Under-estimation may result in the loss of community income, or the sacrifice of environmental values in pursuit of economic development. Over-estimation may result in wasted efforts to attract economic development interests.

The Yaquina Bay study has had considerable impact on my subsequent professional activities. When I served on state boards and commissions I better understood local controversies that arose over natural resource development and management. I became more sensitive to the perils of legal jurisdictions, including states, of sacrificing natural resource quality as a means of competing with other places for resource depleting industries. The study results also were of immediate relevance to rural studies and intermediate decision-making. Yet local comparative advantage is given little attention in resource and environmental economics literature.

The water resources economics research program attracted attention within the OSU academic community. Biological science and civil engineering had long been areas of strength on the campus, and Agriculture and Forestry were leading colleges. In 1960 the joint effort of Robert Alexander, Assistant Director, Oregon Agricultural Experiment Station; Charles Warren, Fisheries and Wildlife; James Kryger, Forestry; Fred Burgess, Civil Engineering; and myself, created an OSU Water Resources Research Institute. Later, a national water resources research program came into existence that established state institutes similar to the existing one at Oregon

State. The published report of the Yaquina Bay study earned awards for outstanding research from both the Western Agricultural Economics Association, and the American Agricultural Economic Association (Stoevener et al.).

Graduate Education in the OSU Department of Agricultural Economics--Leadership of the graduate program in the Department was assigned to me shortly after I came to OSU. My assessment then was that, with effort, we could offer an outstanding MS degree, but that we were not adequately staffed to offer a quality PhD. Federal government research funding at that time provided considerable incentive for academic programs to offer the doctorate, because the doctorate was nearly essential to obtain training grants and research funding. Perhaps for that reason OSU administrators wished to develop the doctorate in agricultural economics. As chair of the Department graduate committee, I was given a voice in faculty appointments, in awarding financial assistance to graduate students, and in developing relations with cooperating departments on campus.

The graduate program leadership responsibility led me to develop guidelines for graduate education in our Department:

An agricultural economist should be a good economist, and graduate students should master economic doctrine at a level appropriate to the degree they seek. Dissertation research should be addressed to problems that require economics as a tool of analysis.

Competence in the neoclassical theoretical core of economics will be expected of all students, but perspective is desirable, as provided by economic doctrine and research methodology. Should students wish to emphasize work outside the neoclassical core, or embark on criticism of the core, they should understand that which they are rejecting or criticizing.

Major professors and graduate committee members are expected to be available for consultation when graduate students conduct their dissertation research. (Faculty availability to graduate students was, and perhaps still is, a major problem with some prestigious graduate programs. I concluded faculty availability was a partial substitute for reputation.)

At that time the graduate faculty of the Department was pluralistic methodologically with diverse education from several universities. No attempt was made to make “all students just alike” when they obtained degrees. In retrospect I believe the faculty were very committed to doing the best they could for graduate students. Several masters students from that time have achieved significant professional recognition. Paul Barkley (now an AAEA Fellow), L. T. “Tim” Wallace, Randolph Barker, LeRoy Rogers and Frank Conklin have obtained the PhD at other institutions, and have established reputations in agricultural economics. Michael Nelson - one of the first PhD students in the OSU program--received an AAEA outstanding thesis award (Nelson and Castle; Nelson, Castle, Brown). Two other early PhD recipients--Harvey Hutchings and Virgil Norton--had noteworthy careers in academia and with the National Marine Fisheries Service.

The graduate program guidelines were not implemented in their entirety immediately, but had influence with the passage of time. The six OSU PhD graduates who have become AAEA Fellows have made distinctly different professional contributions. Contemporary graduate students continue to follow many of the original guidelines.

Resource and Environmental Economics

During the decade of the 1960s environmental concerns became of increasing importance nationally, culminating in Earth Day in 1970 by which time, a substantial cadre of economists, scattered across the country had interest and experience in various aspects of natural resource

policy analysis. Resources For the Future (RFF) came into existence in 1952, and provided outstanding national leadership in natural resource and environmental economics. Not the least of their contributions were numerous publications that met a great need for literature in graduate programs.

At that time the OSU Department of Agricultural Economics was one of the few places where a student could specialize in resource and environmental economics. In the summer of 1969 the Department offered a special summer session in resource and agricultural economics. We brought John Krutilla, RFF, and Chester Baker, University of Illinois, to OSU as faculty. The session was advertised widely and graduate students as well as mature scholars from across the nation attended.

Early in the 1970s the name of the Department was changed to Agricultural and Resource Economics. Outstanding graduate students came to the Department to study before and after Earth Day, supporting the contention that young professionals have the capacity to identify emerging developments within a field of study. As noted above, six graduates from that era became Fellows of the American Agricultural Economics Association (AAEA), the highest honor bestowed by that Association. One, Alan Randall, is the only person to receive an enduring publication award both from the AAEA and the Association of Resource and Environmental Economics. Another, Sandra Batie, was the first woman to be elected president, and the second woman to be honored as a Fellow, AAEA.

Academic Administration and Public Service

As the decade of the 1960s unfolded, it became likely I would have opportunities in academic administration. I enjoyed decision-making and the analysis of difficult problems, and derived great satisfaction from the accomplishments and recognition of my associates. Thus,

academic administration had appeal, but I did not wish to abandon all of my work in economics. I concluded I would be interested in a position as chair or head, because that would provide opportunity to influence both economists and economics.

In 1965 I accepted an invitation from Washington State University to interview for Chair of Agricultural Economics. The evening before my wife, daughter, and I were to leave for the interview, James Jensen, the OSU president, called and asked that I come to his home and, of course, I did so.

Two years earlier the position, Dean of Faculty had been created at OSU. At that time there were only two academic administrators for the entire campus with the rank of Dean or above - the President and the Dean of Administration. The Dean of Faculty was to be the principal academic officer of the University with responsibility for academic personnel, curricular affairs, and faculty governance. The incumbent had been in the position approximately a year. For numerous reasons the Dean of Faculty position, and the incumbent, were not well suited to one another. James Jensen told me the position would be vacant shortly, and that he would like for me to occupy it. This offer came as a complete surprise. I had never thought seriously about surrendering my disciplinary work in economics for academic administration. I much preferred living in Corvallis to Pullman, but administering an academic department was more attractive to me than campus wide administration.

My family and I made the trip to Pullman as planned, and I was offered the position. After I returned to Corvallis, I declined the Pullman offer, and accepted the invitation to become Dean of Faculty. Although I am not prone to depression, I was more discouraged during the month following my decision to become Dean of Faculty than before or since. However, I soon became very busy. The incumbent left several unfinished tasks, and I developed confidence in

my ability to be Dean of Faculty. I developed great respect for the president, James Jensen, and the Dean of Administration, Milosh Popovich. Both became life-long friends.

As my first year as Dean of Faculty came to a close, numerous administrative changes occurred in the College of Agricultural Sciences. The position, Director of the Oregon Agricultural Experiment Station, became vacant, was offered to, and accepted by G. Burton Wood, Head of Agricultural Economics. I then requested permission from James Jensen to apply for the Head of the Agricultural Economics Department. My vacating the Dean of Faculty position would disrupt Jensen's plans for the University, but he knew of the pain I had experienced a year earlier when I became Dean of Faculty. Jim Jensen, a big and generous person, granted my request, and I became Head, Department of Agricultural Economics, Oregon State University on July 1, 1966.

The six years I served as Department Head were the most satisfying of my professional career. I was able to apply much that I had learned as Dean of Faculty. The Department had competent faculty, and I was able to appoint several young additions. I continued to teach and write in my chosen field. There were numerous campus-wide developments that provided opportunities for the Department, and we took advantage of as many as possible. For example, OSU became a Sea Grant University during this period with a significant economics component administered from our Department. These were years of student unrest on U. S. campuses. While this created anxiety for administrators, it also brought excitement associated with student concern for "relevance."

Some of our Department activities were innovative. Shortly after I became Head, I obtained enough "soft money" to hold a Department faculty retreat on the Oregon Coast. We had numerous small group discussions, always chaired by young faculty, usually with assistant

professor rank. Strategic planning, with emphasis on “visioning,” was becoming popular at that time. I was aware of this literature, but suspicious of its misuse. I feared “visioning,” without reference to the realities of resource constraints and new developments, could lead to unrealistic expectations. We developed a kind of master plan at the retreat, but we supplemented it with participatory decision-making and a standing long-range planning committee. This committee was given autonomy to bring new ideas to the administration of the Department, and the Department Head was obligated to call developments with long run implications to the attention of the committee. The result was that priorities were considered guides, but never became dogma. Some of the procedures and practices developed at the retreat remain operative, having survived four decades and five Department Heads.

James Jensen decided to resign as President of OSU and leave campus as the decade of the 1960s neared an end. As noted, these were turbulent times in higher education generally, as well as at OSU. At that time the major academic units were labeled “Schools,” but several wished to become “Colleges.” Oceanography was a Department, but wished to become either a School or a College. The administration of the School of Science was challenging traditional administrative arrangements it had with the Schools of Agricultural Sciences and Forestry. There were numerous other questions pertaining to faculty and student affairs. Jensen was concerned about leaving OSU with problems that would fester while his successor was being chosen and becoming familiar with the place. His solution was an unorthodox one. He appointed a three person University Goals Commission to devote at least one-half of their time for 18 months to study problems OSU faced, and make findings about reasonable aspirations for the university. James Knudsen, Director of the Engineering Experiment Station, Warren Hovland, Chair of

Philosophy and Religion, and I were appointed as the “three wise men” (Castle, Hovland, Knudsen).

The Goals Commission experience was a unique and intellectually stimulating activity. The three of us decided at the outset we would not have a chair, and would proceed only with consensus. We examined every facet of the University, and made numerous recommendations pertaining not only to goals and objectives, but operating procedures as well. I learned much about the traditions of different disciplines and fields, and how they might be used to address problems faced by the university and Oregonians. This experience permitted me to view economics in a broader context than I had to that time. There were differences of opinion among the three of us, but I do not believe any of us we were ever suspicious of motives, intentions, or methods of other Commission members. Knowledge I gained from this experience found its way into articles about interdisciplinary work and University governance (Castle, 1970, 1971).

After six years as Head, I believed I had made my principal contribution, although in retrospect, I doubt that should have been a concern. In any event, I became Dean of the OSU Graduate School in 1972.

Near the end of my tenure as Department Head, I was given the responsibility of administering a major grant made to OSU by the Rockefeller Foundation. Roy Young, Vice President for Research and Graduate Studies, interested the Foundation in OSU. At that time, Oregon was a justifiably recognized environmental leader, and there was interest in a systematic view of this experience. The focus of the project was the best use of Oregon’s natural resources during periods of rapid social and economic change. This was a multi-disciplinary undertaking and involved personnel from several schools and departments. The grant permitted OSU to bring several young professionals to campus, including Bruce Shepard in Political Science, Owen

Osborne in Engineering, and James Fitch in Agricultural Economics. Kenneth Godwin, Political Science, and George Carson, History also became involved. Some of the first writing of William Robbins, History Department, about Oregon natural resource history, was made possible by the grant. A significant contribution of the project was the professional development of many people, including those mentioned, as well as numerous graduate students. Several have testified as to what they learned, and later employed, concerning multi-disciplinary research, as well as substantive problems associated with economic development and the environment. Some of the policy briefs written as a result of the project anticipated national policy issues and developments very well, but it is less clear they had significant effect on policy decisions in Oregon.

Numerous public service assignments have come my way in the course of my intellectual journey. Five Oregon Governors asked that I serve on Boards and Commissions, such as the Water Resources Board, the Water Policy Review Board, and the Environmental Quality Commission. At the national level I have served in an advisory capacity to the Agency for International Development, the Department of Agriculture, the Department of Interior, and the National Science Foundation. Administrative assignments, public service work, and multidisciplinary involvement pose intellectual challenges different from those encountered from within a discipline. For example, I developed a research project on inter-basin water transfer as a direct result of service on the Water Resources Board of Oregon. This project provided research experience for two graduate students, and the resulting publication received an outstanding research award from the AAEA (Beattie et al.) These experiences, and the teaching of research methodology over a forty-year period, have permitted a perspective of economics, especially neoclassical economics, than I otherwise would not have had.

RESOURCES FOR THE FUTURE (RFF)

Resources for the Future (RFF) came into existence in 1952, the year I obtained the PhD. Soon after the end of World War II, President Harry Truman appointed a Materials Policy Commission to study and make recommendations regarding the adequacy of natural resources to support of projected economic growth. William Paley, CEO of the Columbia Broadcasting System, was named chair of the Commission. The report issued by the Commission entitled “Resources for Freedom,” carried a recommendation that an independent organization be established to monitor natural resource adequacy on a continuing basis.

The Ford Foundation implemented this recommendation, and in 1952 brought RFF into existence. The first RFF president did not serve long and was succeeded by Joseph Fisher, who served as president until 1973. The Ford Foundation not only brought RFF into existence, but also sustained the organization with a series of four-year grants until 1975. These grants enabled RFF to do multi-year planning and programming. Joe Fisher provided leadership as RFF established its identity and reputation.

Fisher resigned as President in 1973, and the Ford Foundation indicated it wished to re-evaluate its relationship to RFF. There was general agreement that RFF had done outstanding research on a wide range of natural resource and environmental subjects. Many independent observers considered the organization to be the most prestigious national resource and environmental “think tank.” Even so, the Ford Foundation was concerned about the policy relevance of some RFF activities and encouraged a re-evaluation of the RFF program as it sought a successor for Joseph Fisher. Marion Clawson, a long time RFF staff member, was named interim president.

Charles Hitch became president on July 1, 1975. Concurrent with his appointment, the Ford Foundation made another four-year grant to RFF. When his appointment was made public,

Hitch announced he wished for RFF to become larger, more policy relevant, and more multi-disciplinary. At that time there were fewer than 100 RFF employees. When I went to RFF a few months later, the organization already had grown by approximately 20 percent.

Charles Hitch went to RFF with a most distinguished career. He had just retired as President of the University of California State System of Higher Education. He was a former Rhodes scholar, and remained at Oxford as a don for several years after he completed his formal education there. I first encountered his name when, as a graduate student, I read a journal article he co-authored with an economist named Hall. He returned to the United States to head a department in the Rand Corporation that conducted studies on the economics of defense. When John Kennedy became President he appointed Robert McNamara Secretary of Defense, and he brought Charles Hitch into the Defense Department as part of a group that became known as the “Whiz Kids.” The Whiz Kids pioneered the use of cost-effectiveness analysis in Department of Defense decisions. Hitch subsequently went to the University of California educational system as Comptroller. He succeeded Clark Kerr as President of the University of California system, and served eight years in that position.

In late summer 1975, I received a telephone call from Marion Clawson asking if I would be willing to interview for vice president of RFF. I knew most of the senior RFF staff, respected the organization, was aware of the most recent Ford Foundation grant, as well as Hitch’s distinguished career. I accepted the invitation for an interview. An offer was extended, although the interview did not make clear just what my responsibilities would be, or how the senior RFF staff might view my appointment. When those issues were addressed in subsequent negotiations, I accepted, and in December 1975 became Vice President and Senior Fellow, Resources for the Future.

After I went to RFF I soon discovered a serious communication gap existed between Hitch and the RFF staff. He was brought there to make changes, never an easy task, and his administrative style did not encourage information exchange. Joe Fisher had an exceedingly informal style, and little use for administrative structure. Before Hitch, formal program units did not exist. When I arrived, three departments had been established, and an elaborate personnel classification was being implemented. As soon as I became aware of the information exchange problem, I told Charlie Hitch that more conversations between him and the research staff, individually as well as collectively, would be desirable. He replied, “Why? I talk to you every day, and you speak for me. You talk with the staff, and the staff talks with you.” End of conversation.

I enjoyed a good working relationship with Charlie Hitch, and my wife and I became fond of him and his wife. He was never popular with most of the staff. I soon learned he wanted to convey good news to the staff and others, but that I should be the bearer of sad tidings. I did not mind this arrangement, but I have always believed such a policy to be a mistaken one. A leader is most respected when he, or she, announces all developments to those for whom they have responsibility.

Hitch was a better writer than I am. He could make prose “sing”, while I struggle to be anything other than dull!!! His was an excellent intellect, honed by the best education available. We were complements, perhaps because I am able to analyze complicated, “messy” matters, extract essentials, and imagine possible responses. Hitch liked to have “talking papers” before interacting with others about significant issues. I attempted to anticipate emerging issues, identify ramifications, and isolate possible RFF options. Almost always, I would give him my recommendation, but that was kept separate from the analysis I prepared for him. Often he would

take my recommended course of action, and build upon it in an elegant and substantive way. I sensed he was pleased with my work, although he seldom evaluated my performance. Once, almost inaudibly, he muttered something like: “You are as good, or better, than any associate I have ever had.”

Shortly after I went to RFF, I invited S. V. Ciriacy-Wantrup to come to RFF and serve as a member of an advisory group. While there, he and Hitch came together. They shook hands and Hitch said “I do not recall seeing much of you when I was at Berkeley.” Wantrup replied: “That is correct. You were always in that administration building.” Upon hearing this exchange I reflected, but not aloud, “The hermit and the recluse have met.”

Three months after I went to RFF, Marshall Robinson, Vice-President, Ford Foundation, came to RFF with a message for Charlie Hitch and me. He told us McGeorge Bundy, Ford Foundation president, would retire, probably in 1979. He said it was probable that after Bundy retired it was unlikely the Ford Foundation would provide the unrestricted financial support RFF had enjoyed the past 25 years. Robinson proposed a course of action to help RFF establish an endowment, and seek program funds from other sources. He said RFF should become smaller, at least in the short run, and undertake an active fund raising campaign immediately. Hitch told Robinson this made Hitch’s position very different than the understanding he had when he became president. Robinson did not disagree, but said these were the realities. After Robinson left, Hitch told me he had no inkling anything like this was “in the works” when he offered me the position of Vice President. He clearly was concerned about my welfare, and I was touched. I told him I certainly did not hold him responsible, and that realistic people knew we lived in a world with great uncertainties. I sensed he was grateful for my attitude.

From the outset, I knew this development would change fundamentally the leadership needs at RFF. The organization had enjoyed unrestricted funding for 25 years. Although RFF had a competent staff with an excellent reputation, there was little experience among the staff, and no infrastructure within the organization, for seeking funds. There was general agreement our credibility was our most valuable asset, an asset we needed to preserve. This meant RFF must retain its independence in deciding what investigations it would undertake, have freedom to publish in the public domain, and maintain an arm's length relationship with those providing funding. When I was at RFF, staff consulting was minimal and tightly controlled; officers and staff were not permitted to work for, or take fees from, any organization or person that might be affected, positively or negatively, by RFF research. During my tenure at RFF, I dealt with financial supporters in government, industry, and public foundations that attempted, in various ways, to influence RFF research results, or how we would conduct research. I do not believe any were successful.

To his credit, Hitch promptly informed the RFF staff of the message Marshall Robinson had given us. He and I agreed reduction in size was necessary, and I began work with the three division directors to make program adjustments. Endowment fund raising did not go well. We hoped former RFF Directors—William Paley, Laurence Rockefeller, and Robert Anderson—would become endowment sources. Each helped, but not enough to establish a significant endowment. We had some success in obtaining project and program funds, and the Ford Foundation made an additional grant, permitting the establishment of a reserve fund that served as an endowment base.

The challenges I faced at RFF, following the Robinson visit, were altogether different than the experiences I had as an OSU department head and dean. Instead of the excitement

associated with building new programs with young people, I needed to retrench a mature, established staff. Not surprisingly, morale at RFF took a “nose-dive.” I found myself, not only holding hands with young staff, but also reassuring some who had created classic literature in their field of specialization. Bad news travels fast, and the best people at RFF began getting “feelers” from prospective employers.

A plan of action was developed to deal with the situation. The division directors and I estimated the number of core people, or critical mass, we believed necessary for RFF to remain relevant in the public policy arena. Our estimate fell in a 15–20 range and, as I recall, the specific number selected was 17. I then estimated the number of years the core group, together with needed support staff, could be sustained by our reserve fund and realistic “soft money” expectations. This turned out to be eight to ten years. The next step was to identify the seventeen people to be retained. I selected and then conferred with three. The four of us reached consensus about another group of five. We continued in this way until 17 had been selected. These 17 core group members were assured of employment at RFF for eight years if they would remain. After we had made the core group secure, we explained the plan to all employees. Employees not in the core were assisted if they wished to seek other employment. Several did well raising funds in support of their programs, and remained at RFF. Morale improved immediately after these actions were taken. Administrative policies may create, reduce, or affect the incidence of uncertainty. Transparency, or lack thereof, in administration may also affect the amount and kind of uncertainty perceived by employees.

From 1976 through 1978 RFF struggled to adapt to its new environment. The plan, described in the previous paragraph, was designed to get the organization through a rough period, but was not necessarily sustainable. Charles Hitch and Marshall Robinson devised a

different approach, and proposed a merger with the Brookings Institution. The RFF reserve fund, together with a \$7 million Ford Foundation grant, was to be given to Brookings, - if they would accept RFF as a distinct division within their organization. Brookings reacted positively to the proposal. The next step for Hitch and Robinson was to obtain approval of the merger from the RFF Board and staff.

Early in 1978, Charlie Hitch told Gilbert White—Chair of the RFF Board of Directors and internationally known geographer—and me about the merger plan just prior to an RFF Board meeting. Gilbert and I were not favorably disposed toward the merger. Neither was the RFF Board or most of the staff. I was frank with Charlie Hitch about my concerns, and this difference of opinion affected our relationship for the next several months. Despite my concerns, I worked closely with the Brookings Institution during the summer of 1978 on program and administrative details of the possible merger. As the plan developed it provided I would lead the new Brookings division with the title of Vice President, the only vice president Brookings would have. In mid-summer 1978, Hitch told the RFF Board of his intention to retire the following year. I was named President Designate to become president when Hitch retired. The RFF Board wanted to provide as much stability as possible during those trying times.

The RFF Board asked the Ford Foundation what it would do for RFF if we were to cut all apron strings. We hoped they would grant us the \$7 million they proposed to give Brookings as a dowry if we merged. Instead, Ford said the \$7 million could become a matching grant if we could obtain a match within 90 days. This, of course, was unrealistic. Nevertheless, by August 1978 Gilbert White had obtained a \$2.5 million commitment from a major foundation to be used as match, although initially he was not at liberty to reveal the source of the commitment. Gilbert and I conferred about a strategy for obtaining permission from McGeorge Bundy for more than

90 days to meet the match. We knew Bundy, Marshall Robinson, and Charles Hitch would be at a meeting in Aspen, Colorado in late August. The problem was that Gilbert could not attend that meeting because of an international commitment. He said I would need to go in his place and request more time to meet the match. I protested, saying: “Gilbert, do you expect me to go to a meeting with Bundy, whom I have never met, that also includes Robinson and Hitch, who favor a merger, to ask for an extension of time to meet a match they all hope will fail?” Gilbert replied: “Emery, if you expect to be president of RFF, you had better start acting like it.”

The meeting with Bundy was in his cabin at Aspen in late afternoon. It included Hans Landsberg, an RFF Senior Fellow, as well as those named above. I was asked by Marshall Robinson to brief McGeorge Bundy concerning merger plans with Brookings. When that discussion was completed, I asked if we could discuss an independent RFF. There was a moment of silence, but Bundy agreed, then said we should go to dinner soon. Bundy had discussed an independent RFF with RFF supporters, but, of course, he did not know about the \$2 and 1/2 million pledge Gilbert had obtained. I told him of that grant, and requested RFF be given a year, rather than 90 days, to meet the match. I followed this with a short statement about the kind of program I envisioned for an independent RFF. The latter was Gilbert’s suggestion. Bundy wanted to know where Gilbert had obtained the \$2.5 million pledge. I told Bundy I did not know. However, I gave him a telephone number where Gilbert could be reached. I had deduced correctly from where it was to come, and I suspect Bundy did as well. Bundy said the date for meeting the match would be extended, but that October 1, 1979 was the absolute, final, “drop dead” date.

In addition to the \$2.5 million pledge Gilbert obtained from the Andrew W. Mellon Foundation, John Krutilla was instrumental in arranging a \$1.5 million grant from the C. S. Mott

Foundation in October 1978. On September 30, 1979, one day before the “drop dead” date, the John D. and Catherine T. McArthur Foundation responded positively to a written proposal I had prepared for a \$1 million grant. That grant of \$1 million, with other pledges, permitted RFF to meet the \$7 million match.

I became President July 1, 1979, and remained as President of RFF until March 1986. By that time, RFF had met all of its pledged obligations to Ford, had developed real estate with an office building in urban Washington D. C., and had assets of approximately \$30 million. It was a fully independent, endowed organization. My time at RFF was spent differently than I anticipated when I went there. The Marshall Robinson visit soon after I went there changed my responsibilities as I saw them. I concluded then that my first responsibility was to ensure the survival of the organization. Marion Clawson said two people had been essential to RFF’s survival--Gilbert White and me. If that is correct, my primary objective was achieved.

Subject Matter Generalization #2. Did the RFF experience affect my intellectual journey? Yes, indeed. Numerous decisions were made in the presence of great uncertainty when my future, or RFF’s, was “on the line.” My tools as an economist were useful in every situation I faced, but were seldom applied in a doctrinaire way. The building and real estate RFF acquired provides an example. After our successful fund raising campaign, I suggested to Ted Hand, RFF Treasurer, we should consider a real estate investment. Ted took my suggestion seriously, and became well informed about urban real estate. He located a building with undeveloped land for sale in walking distance of the White House, owned by the Truckers Association, and only a short distance from RFF offices. The Truckers Association wished to sell because they wanted to build a different headquarters near the beltway.

Ted and I analyzed the possible purchase independently, and each came to the conclusion it would be a good RFF investment, as it has since proven to be. Ted relied mainly on analysis of most probable outcomes. I was more risk averse, and concerned myself with the risk the real estate investment might have for the maintenance of our endowment. The RFF research staff, composed mainly of PhD economists, was divided in its opinion. Those who opposed the purchase argued that markets were efficient, and if the building and undeveloped land were a profitable investment, urban real estate interests already would have made the purchase. I knew this doctrinaire view was invalid because at that time money was tight, interest rates were high, and every developer Ted and I had talked to had been attracted to us because we had a strong balance sheet, and represented a source of capital they wanted to access.

The successful application of economic doctrine requires attention be given to particular circumstances, or conditions, that may invalidate a deduction from assumptions that reflect the essence of a general reality, but neglect the particular reality of concern. In this example, stable, competitive urban real estate prices depended upon an implicitly assumed stable market for mortgage loan funds. If the supply of mortgage loan funds cause interest rates to rise to an unprecedented high level, a perfect real estate market assumption may give rise to an hypothesis that real estate, and real estate mortgages, provide attractive investment opportunities. The decision to purchase clearly has been a highly profitable one, due in no small part to the superb management provided by Ted Hand after the building with undeveloped land was acquired.

While I was at RFF, Nobel Laureates Kenneth Arrow, Tjalling Koopmans and T. W. Schultz came to assist with various projects. I was impressed by the satisfaction these acclaimed economists derived from application of theoretical concepts to resource and environmental

policy. The tension I had experienced in academic institutions between theoretical and applied economics was nonexistent in my encounters with these Nobel Laureates. They knew we had them there because we valued their imagination and the power they commanded in using theoretical constructs to help solve problems. And they came because they experienced satisfaction from application of economic theory.⁶

My RFF experience greatly enhanced my life, and I am indeed grateful for it. It permitted me to view and participate in the Washington scene, meet members of the Eastern establishment, and become acquainted with prestigious economists whose writings I had long known. I never caught Potomac Fever, but if I had gone to Washington earlier in life I might have become infected.

THE RETURN TO OSU

The University Graduate Faculty of Economics

When my wife and I made the decision to go to Washington D. C., we agreed we would remain there approximately ten years. Neither of us, I believe, ever seriously doubted we would return to the Pacific Northwest, and we were away almost exactly one decade. But our reason for returning was not the one we anticipated. More than a year before we returned, my wife began to suffer loss of cognitive ability. Her physicians did not provide an explanation, but I concluded we should get to a location where we would remain for some time, perhaps for the remainder of our lives. A part time position at Oregon State drew us to Corvallis.

At that time three OSU departments were providing graduate education in economics. My former department, Agricultural and Resource Economics, located in the College of Agricultural Sciences, was one. The Department of Economics in the College of Liberal Arts was another. The third was the Department of Forest Resources in the College of Forestry.

Before I returned it became apparent to OSU administrators that maintaining three independent graduate economics programs was not a viable option. The three respective academic Deans, with the support of the Graduate Dean, appointed a faculty group to make recommendations. By fortunate coincidence, all of the three academic deans were economists--Bill Wilkins, Liberal Arts; Carl Stoltenberg, Forestry; and Ludwig Eisgruber, Agricultural Sciences.

The principal recommendation of the faculty committee (later known as the Vars Committee), was that a core of graduate economics courses should be made available to economics graduate students in the three departments. They also recommended a part time person be appointed to administer the core program, specifically to insure the core program courses be taught “regularly and well.” The position was offered to me, and I accepted subject to two conditions. One was that I be responsible administratively to the Dean of the Graduate School; the other was that my office not be located in any of the three departments. Lyle Calvin, Dean of the Graduate School, and a friend of long standing, agreed to have me report to him, and Russell Youmans, Director of the Western Regional Rural Development Center, another friend, provided an office. I returned to the OSU payroll in March 1986.

We worked with the core courses that had been recommended by the Vars committee and designed a curriculum to be required of all master and PhD economics students in the three Departments. I arranged for this curriculum to be reviewed by Edwin Mills, Northwestern University and formerly with Princeton and John Hopkins; George Tolley, University of Chicago; Irma Adelman, University of California-Berkley; and Vernon Ruttan, University of Minnesota. I then discussed by telephone what we were trying to accomplish with Nobel Laureate Kenneth Arrow. The consensus view was that a program with an applied emphasis, but with a firm theoretical base as we were proposing, would serve a useful purpose in the

economics profession. The reviewers also emphasized the importance of avoiding the temptation to try to emulate programs of the more prestigious economics departments in the Nation.

Soon after I returned to OSU, I obtained approval of a recommendation that the graduate economics core program be named the University Graduate Faculty of Economics (UGFE), to consist of faculty holding graduate faculty standing in the participating departments. As Chair of the UGFE, I arranged for the Economics Department and the Department of Agricultural and Resource Economics to be housed in the same building. Another accomplishment was obtaining permission from the State Board of Higher Education for the Department of Economics to offer graduate degrees. I had great support for this from the graduate dean, the academic deans, the provost, and the president. Prior to that the Department of Economics' involvement in graduate education consisted mainly of offering economics courses to students majoring either in Forest Resources or Agricultural and Resource Economics.

Having administrative responsibility for academic personnel located in three departments with each department located in a different college was not a simple undertaking. I instituted procedures to keep all unnecessary paper from coming across my desk. I also arranged to participate in decisions that affected my assigned responsibility. Actions and activities that would diminish the visibility or recognition of the departments I was coordinating were avoided. I was chair of a ten-person policy committee--three from each department and me--that established guidelines for the UGFE.

The UGFE is still in existence, an indication that it has addressed a need. There has always been a consensus that economic graduate students should master a rigorous core of neoclassical economics. At the outset, courses were required in history of economic thought, research methodology, and comparative economics to provide perspective. These requirements

have not been maintained because of diminished resources and retirements. As this is written, students have requested perspective type material be made available in seminars. Problems also stem from traditional differences between students in economics on the one hand, and agricultural, resource, environmental and forestry economics students on the other.

Rural Studies

After deciding to leave RFF, the question of what I might do for the remainder of my professional life arose. The UGFE work would be important, but it would not be a full time responsibility. I did not wish to devote all of my efforts to resource and environmental economics, but I did want to work on important social problems where possibly I could make a difference. Rural studies satisfied these criteria, and, after I returned to OSU, I began to explore ways to develop a program of work. This would not happen automatically in a University because rural problems do not fit neatly into disciplinary ‘pigeon-holes’, although OSU had two attractions in this respect. One was the OSU tradition of multidisciplinary activities; the other was Bruce Weber. Bruce was the only OSU economist with a rural assignment, and he had great credibility with other economists, as well as with those who used his research results. My intellectual journey provided me the great, joyous windfall of being a close associate of Bruce Weber for nearly two decades.

Numerous economists have attempted to develop rural economics with varying degrees of success. The American Economic Association classification system for fields of study gives rural economics parallel standing with urban and regional economics. Nevertheless the scholarly contributions related to rural economics falls far short of those in urban and regional economics. Rural places occupy more than 95 percent of the land area of the United States, and rural America is home to more than one fifth of the population. Such facts point to the possible

economic and social importance of rural places but, standing alone, do not make a compelling case that special attention should be given to rural economics.

Social pathologies exist in rural places and, at the extremes, those pathologies are comparable to those found in inner cities, with the suburbs faring better. It has been argued land rents are lower in sparsely populated places than elsewhere, and disadvantaged people may live there because of lower the living costs. Although land rents per unit of land may be high in the inner cities, high population densities (crowding) lower living costs per person. Another related consideration pertains to improvements in labor productivity in traditional rural economic activities. If less total labor is needed by such industries, excess labor must either migrate to places of greater economic opportunity, or accept lower economic return for their services. If migration does not occur, low income, poverty, hunger and other social pathologies may result.

Such considerations may not be sufficient to motivate economists, or a university economics department, to give special attention to rural people and places. To obtain such a commitment, it is necessary to demonstrate that rural problems are not addressed adequately by traditional regional or urban economic analysis. Therefore the principal intellectual justification for special economic study of rural people and places is the relatively low human population densities, geographic distance, and the spatial variability in the countryside that affect the performance of both market and non-market institutions. Relatively low population densities, distance, and variability in natural space are not given explicit attention in typical urban or regional economic studies. The performance of rural markets, non-market institutions, and collective policies and actions therefore qualify as subjects for special investigation. Of course rural, urban, regional, national, and international interdependencies establish the framework and the environment for such investigations.

Rural economics, then, is concerned with economic problems unique to rural places, typically not addressed by urban or regional economic research. Central place theory, an important paradigm in urban and regional economics, neglects rural complexities unless a rural perspective is specifically introduced (Hite). OSU colleagues Wu and Plantinga demonstrate that the introduction of environmental amenities into the central place model give rise to conclusions that have both rural and urban place implications.

Rural places have long been attractive to those who choose to “opt out” of, or reduce their dependence upon, the mainstream economy. Economists typically are less interested in those who do not respond as though led by an invisible hand, than they are in those who are active in markets. The study of rural places provides an opportunity to learn more about those who “opt out”, or who are made worse off by economic reorganization. (Norman and Castle).

Bruce Weber and I have come to view rural communities as stemming from intersections of geographic space with political jurisdictions. This view means sociology (communities), political science (legal jurisdictions) and geography and regional science (space and distance) are involved. The choices stemming from community or regional comparative advantage is the stuff of economic analysis and directs attention to collective action (Stevens) and intermediate decision-making at the community level. Intermediate decisions are collective decisions between local individuals and firms and those at national or international levels. Both traditional economic theory and the theory of collective choice require modification if applied to intermediate level decision-making.

There are similarities between the academic status of rural economics at present, and resource economics when I began work there. The reaction of many economists at that time to the idea of using consumers’ surplus to estimate a demand function for outdoor recreation was

not unlike what I now hear about approaches in rural economics. “Fuzzy,” “not rigorous,” “no evidence to support” are terms applied to resource economics then, and rural economics currently. This is an inevitable reaction to concepts that transgress boundaries of established disciplines (Castle, 1998, 2002, 2003).

The National Rural Studies Committee (NRSC)

During the summer of 1986, I was invited by the W. K. Kellogg Foundation to participate in a symposium on the problems of rural America. Most of the other participants were rural operatives holding a variety of positions in government and non-governmental organizations. Several of these people spoke of the lack of a framework for thinking about, classifying, and generally helping them make sense of the myriad of rural problems they encountered in their work. As a result of that symposium, I submitted a proposal to the Foundation for a grant to establish a National Rural Studies Committee. The proposal was to assemble a multi-disciplinary group of scholars to study rural America on a regional basis with the objective of making recommendations to assist institutions of higher education to better serve rural America. The proposal also requested funds to provide young academics an opportunity to take leave from their regular responsibilities, and, under the direction of Bruce Weber, work on rural problems. The assumption was that such an experience would lead them to consider rural studies as a career. The proposal recognized rural studies curricula usually do not exist in American higher education at either the undergraduate or graduate level.

The proposal was funded with two four-year grants and one extension. Distinguished academics from a number of disciplines agreed to serve, and five regional meetings were held. A number of activities were undertaken to address needs that emerged from the regional meetings. Gene Summers, a rural sociologist at the University of Wisconsin was a committee member, and

also was President of the Rural Sociological Society (RSS). As a result of an NRSC meeting in Mississippi, he brought into existence through RSS a Rural Poverty Task Force. This task force undertook a number of action programs in various regions, as well as nationally (Summers). In addition to the Poverty Task Force, the NRSC held a summer school for rural community college and four-year college faculty. A book of readings was prepared (Castle, 1995), and various teaching aids were disseminated. Rural studies cases were used in a public policy course at Princeton University.

In places visited by the NRSC, the principal involvement of higher education in rural America was by the community colleges and through the Land Grant University Extension Services. Those working at the community level had a paucity of reliable literature they could access. This was in contrast to the abundant literature and scholarship available to urban community workers. Further, it was found that higher education provided few opportunities for graduate degrees that emphasized rural issues in course work and research. Sustainable extension programs require both an influx of educated personnel and a continuous flow of information.

When the NRSC went out of existence in 1997, its impact on several institutions was documented, including the University of Illinois, Cornell University and the University of Wisconsin. These effects included the formation of institutes and centers, creation of courses, establishment of curricula, and the attraction of young scholars into the field. (Castle, 1997). Upon completion of the NRSC work, all members and center associates were asked to evaluate their NRSC experience. Several claimed, and then documented, that the experience was “career changing”.

After the NRSC work ended, I was asked by President Paul Risser to design a rural studies program for Oregon State University. A small grant was made available from the

Provost's office to make this possible. The Rural Studies Program that was designed and implemented was based directly on NRSC recommendations made in its final report to the W. K. Kellogg Foundation. I served as Director of the Program for two years, when Bruce Weber succeeded me. Bruce's leadership resulted in the Rural Studies Program receiving a major grant in 2004 from the Provost's office. The grant is to be used to establish one of six multidisciplinary centers of excellence on campus. The Rural Community Sustainability Center will provide support for research, extended education and graduate education.

The NRSC experience confirmed for me a phenomenon Russell Youmans had long maintained, that some agricultural interests have long worked to keep rural issues from having a place on the national public policy agenda. Such interests fear that consideration of rural issues will divert attention from the many agricultural policies and programs the agricultural industry now enjoys. One tactic is to grant the importance of "rural" issues, and then work to subsume possible remedial activities within existing agricultural programs. Treating "agriculture" and "rural" as synonymous may stem from ignorance, or may be a strategic attempt to continue support for traditional agricultural programs.

Economic Research Methodology

Since 1966 I have had the good fortune to teach a course in economic research methodology from time to time. This experience has been an important part of the intellectual journey described here. A few comments on that experience will serve to conclude Chapter I and introduce Chapter II.

As an undergraduate at K-State, I took courses that introduced me to great philosophers and I became aware of the philosophy of science. When doing graduate work at Iowa State I took a course in research methodology from Earl Heady. At that time (1950) most scientists I knew

appeared to believe that they were practicing logical positivism. Heady told us that economists should get their hypothesis for “testing” from economic theory. I could understand why economists would empirically estimate production functions or demand functions, but I did not understand how this “tested” economic theory. When I went to Oregon State in 1954, there were opportunities to teach research methodology, but I was not sufficiently confident of my knowledge to accept those invitations.

Economists were not writing a great deal about methodology at that time, but I read most of what appeared in the economic literature. I learned of the writings of Karl Popper and recognized he addressed some of my concerns about logical positivism. After I returned to the Department in 1966, I offered my first course in economic research methodology and included in the course an identification of problems that young professionals often face, along with acceptable responses in academic environments and public policy arenas. I drew on my experiences as Dean of Faculty and my public service work in doing so. I had difficulty obtaining literature suitable for the graduate students who took the course.⁷

The second edition of Thomas Kuhn’s *Structure of Scientific Revolutions* appeared in 1970, and was added to the reading list. I taught the course twice after Kuhn’s second edition appeared before I departed for RFF. To my surprise, I did not discover significant interest in economic research methodology at RFF, although Michael Toman and Talbot Page were notable exceptions. While I was at RFF I read and pondered both Caldwell’s *Beyond Positivism*, as well as McClosky’s “Rhetoric of Economics” *Journal of Economic Literature* article.

When I returned to OSU in 1986 Gene Nelson, then Head of Agricultural and Resource Economics, invited me to offer the research methodology course again. I did so, but by this time there was an abundance of literature. Two new journals, *Economics and Philosophy*, and *The*

Journal of Economic Methodology were available. Fresh insights were provided by, but not limited to, N. de Marchi, D. Hands, K. Hoover, U. Maki and A. Randall. No doubt I learned more during this period than did most of my students. This literature in the philosophy of science not only improved my understanding of economic research methodology, it also provided a basis for many of my misgivings about neoclassical economics. This was especially true of Hausman's *The Inexact and Separate Science of Economics*. Chapter II is entitled "Transgressions" and is concerned with methodological issues pertaining to neoclassical economics.

As noted in endnote two, an intellectual journey necessarily requires that special attention be given to particular subject matter and conceptual issues. For the journey described here this is the use of neoclassical economics by an applied economist concerned with agricultural economics, resource and environmental economics, and rural economics. Clearly neoclassical economics is a robust theory, having survived for more than a century. My major concerns have stemmed from three problem types: a) how can economists get different answers from neoclassical models just by changing an assumption or assumptions? Did not Popper say a scientific theory would forbid, not facilitate? b) Why is it that application of neoclassical economics to a new field, (say) resource economics, requires extensive justification and elaboration, even though the basic building blocks of the theory clearly are faith propositions with little empirical justification? c) How valid are policy prescriptions derived from neoclassical economic models, when some real world conditions and circumstances are neglected?

The teaching of research methodology required that I read a literature that has permitted me to formulate tentative answers to such questions. Chapter II presents my current thinking on these subjects. This viewpoint has, in reality, been developed over a professional lifetime of applied economic study, rather than in the context of a particular time, place or situation. Except

for the remarks of this section, Chapter I stands on its own. Most readers would not know that my journey was incomplete if it ended here. But I would.

CHAPTER TWO--TRANSGRESSIONS

INTELLECTUAL BOUNDARIES--THEIR ORIGIN AND FUNCTION

Neoclassical economics is a distinct body of thought if judged by its method. This view rejects the notion that all sciences are the same methodologically. Economics, especially neoclassical economics, probably has been afflicted by “physics envy” from time to time but Mill knew better. Economics is not physics, and neoclassical economics is most certainly not physics. The assumptions underlying neoclassical economics play a different role than assumptions employed in physics because economic phenomena are different than physical phenomena. Among other roles, assumptions in economics define the domain, or establish the boundaries of the subject matter. One of the principal purposes of this chapter is to establish that a literal reading of the assumptions will lead one to establish inappropriate boundaries for neoclassical economic theory. Subjects that are embraced by the assumptions may be inappropriate even as relevant and important subjects are excluded. Transgressions are violations of boundaries. This chapter identifies neoclassical boundaries the applied economist should respect, and those that may be challenged legitimately.

Economists from Mill to Friedman have known that simplifying, unrealistic assumptions are necessary for the construction of classical and neoclassical economic theory. It is helpful to recall that assumptions may be unrealistic for different reasons. They may be unrealistic because they do not capture the totality of reality, even if, for certain purposes, they capture its essence. Assumptions may also be unrealistic by relying on “as if” phrases that are derived from analogy.

A neoclassical economist may assume people behave “as if” they maximize utility. The assumption does not maintain that people go through all the mental processes required to maximize utility. Instead the theory asserts that actual behavior corresponds to what would occur, if they actually did so. Assumptions may also be unrealistic because they are false in every respect regarding the phenomena of concern. Unrealistic and false assumptions are of no relevance to this discussion and need not detain us.

The title of a book by Hausman, *The Inexact and Separate Science of Economics*, identifies two distinguishing characteristics of neoclassical economics--inexact and separate. The implications of an “inexact” and “separate” science are discussed next, followed by an argument that “incomplete” should be added to “inexact” and “separate” as characteristics of neoclassical economics. A discussion follows of how applied economics is affected by the separateness, inexactness, and incompleteness of economic theory.

The Separate Science of Economics, (Hausman (p.90-91), lists four components of economics as a separate science: 1) Economics is defined in terms of causal factors with which it is concerned--basically the pursuit of wealth. He cites Mill to the effect that every other human passion or motivation becomes an abstraction, except those which are perpetually antagonizing principles such as aversion to labor, or enjoyment of indulgencies. 2) These causal factors serve to define the domain of economics. 3) The “laws” of the predominating causal factors are reasonably well known. 4) Neoclassical economic theory provides a unified, complete, but inexact account of its domain.

Two important consequences stem from the separateness of economics. First, economists often have a tenuous relationship with other social theorists also concerned with economic phenomena (p. 263). Second, economists, unable to refute challenges from other disciplines

concerning assumed economic causal factors, have consistently elected to maintain a separate discipline (Chapters 12, 13).

Hausman's view of economics has several important implications for applied economics. Important trade-offs exist between theoretical completeness on the one hand, and applicability and relevance on the other. Furthermore, some of the most celebrated work in economics has been associated with the relaxation of standard assumptions. Such relaxation often has been associated with a loss of generality, even as it allows improved relevance for certain other purposes. Chamberlin's monopolistic competition is a case in point. Choices in favor of generality at the sacrifice of relevance may be a result of assumptions that bring inexactness into the system.

The Separate Science of Economics

Yet there is value in a separate science of economics apart from the ego satisfaction of economists. Economic doctrine provides a frame of reference for viewing particular economic phenomena, as well as other academic disciplines. When applied economists believe that the separateness of their discipline prevents important issues from being considered, they may wish to work jointly with other disciplines. Maintenance of the separateness of economics does not necessarily preclude useful multidisciplinary research. Economic research results may be magnified when viewed through the lenses of other disciplines. It is my judgment the most useful multidisciplinary research comes from those who are at the forefront of their respective disciplines. Indeed, the promise of inter or multi-disciplinary research is based on the premise that each of the relevant disciplines will bring its best theories and concepts to the table.

Economics as an Inexact Science

As noted, Hausman states neoclassical economic theory provides a unified, complete, but inexact account of its domain. Suppose there exists a theoretical system with four assumptions: 1) consumers pursue goods and services and this is their primary motivation, 2) producers pursue wealth and this is their primary motivation, 3) all products are homogeneous, and 4) producers and consumers have access to the same information. Suppose further that none of these assumptions is in absolute accord with reality for two possible reasons. One reason is that an assumption may apply to all items in a class, but not to the extent specified. For example, with the first and second assumptions, grant that all consumers pursue goods and services, and all producers pursue wealth, but also grant this may not be the primary motivation of all producers and consumers - clearly a source of inexactness.

The Incomplete Nature of Economics

A lack of preciseness may arise from another source. An assumption may not be in accord with every item in a class. Consider the third assumption. Perhaps 90 percent of the producers of a given commodity sell in a market with a single price for their output (a homogeneous good), but 10 percent sell in a market that differentiates on the basis of quality (a heterogeneous good). Conclusions deduced from the above assumptions will be based on an incomplete assumed view of reality. In one case all items in a class were described, but in an inexact way. In this case all items in a class were described accurately, but all items were not described (incompleteness).⁸ Should it be said: neoclassical economic theory provides a unified, but an inexact and incomplete account of its domain?

APPLIED ECONOMICS AND A SEPARATE, INEXACT, AND INCOMPLETE THEORY

The above has profound implications in applied economics. To a certain extent applied economists play “catch up” with, and are dependent upon, mainstream economics. Applied economists gain credibility when they are perceived as competent in their subject matter field. And many simply want to use the best tools available for the problem at hand. At any given time they rely on mainstream literature and personnel coming from leading graduate programs in economics. Scientists generally, in this respect economists are not exceptions, rely heavily on consensus for the validation of findings in a discipline. Over time this conservatism may increase the reliability of scientific findings, but for any given period, fads and fashion may predominate.

In the following material, examples illustrate the above with particular relevance to agricultural, resource and environmental economics. In the not distant past, mainstream economics was preoccupied with general equilibrium theory. Before that there was a preoccupation with welfare solutions and social optimality. This applied economist is encouraged that these preoccupations no longer prevail to the same extent they did in an earlier period. Yet some current and emerging applied literature has not yet “caught up,” and fails to recognize the separate, inexact, and incomplete nature of our theoretical framework.

Increasing Returns

Increasing returns have been an important part of economic theory since the time of Adam Smith. In the relatively recent past constant returns to scale received the greatest emphasis in a great deal of economic analysis. To be sure, constant return are convenient in much quantitative research and in general equilibrium solutions and both are of continuing importance in mainstream economics. General equilibrium places a premium on assumptions that are kind to general solutions, and increasing returns are, at a minimum, a nuisance in such efforts. In 1928, Allyn Young stood this issue on its head and raised question about the general relevance of

equilibrium in view of the empirical importance of increasing returns. (On this point see Kaldor especially). In the 1980s the importance of increasing returns was noted again, especially by Romer, but also by Arthur and Krugman, giving rise to what is sometimes called the “New Growth Theory” (an unfortunate description). These parallel developments do not constitute a theory, but direct attention to the significance of increasing returns. Buchanan and Yoon provide a perspective of this recent literature. The following passage from Arthur (p ix) is of interest:

“In March 1987 I went to my old University, Berkeley, to have lunch with two of its most respected economists. ‘What was I working on?’ Increasing returns. ‘Well, we know that increasing returns don’t exist,’ said one. ‘Besides, if they do,’ said the other, ‘we couldn’t allow them. Otherwise every two-bit industry in the country would be looking for a handout.’”

In 1994 Buchanan wrote:

“By the mid-1970s, the aridness and emptiness of highly formalized general equilibrium analysis came to be widely recognized, even by those economists who had participated in the theoretical advances” (p. 9). Also on (p 3): “_____generalized increasing returns provide the analytical foundations for research programs in international trade, in macroeconomics, in theories of endogenous growth, and in the economics of ethics.”

Increasing returns are well understood by agricultural economists engaged in international trade studies, but are neglected in agricultural policies and rural studies. This is surprising because increasing returns in agriculture have permitted peasants in many parts of the globe to escape subsistence living, and enabled urban population densities. The reasons for this neglect probably are related to the long-standing practice in classical and neoclassical economics of treating technological change as an exogenous variable.

Bruce Weber and I once organized a workshop on increasing returns for economists interested in rural issues, but with scattered exceptions, increasing returns have not become the basis for on-going rural research programs. Increasing returns are a principal reason comparative advantage changes between and among regions. Further, increasing returns in traditional rural industries such as farming and timber production add to redundant rural labor supplies. Graduate students who wish to understand the causes of income differentials among regions should become thoroughly familiar with this body of theory.

Some theorists who have written about increasing returns in economic growth, have assumed increasing returns do not apply to agriculture (Kaldor, Buchanan). Agricultural economists working in agricultural policy appear to have been imprisoned by short run, static models, usually with assumptions of constant returns to scale. The recent industrialization of parts of American agriculture has resulted in some agricultural economists calling attention to the “new” agriculture. Yet the seeds of these developments can be found in Adam Smith’s description of the division of labor and the extent of the market (Castle 1998 (b)). If future agricultural developments are to be anticipated, I believe increasing returns, and the extent of the market will need to be combined with heterogeneous product analysis. It may not be in the cards for agricultural economists to influence greatly future agricultural policy in the United States. Nevertheless it would be unfortunate if the lessons that can be learned from the more economically advanced societies do not become available to less economically mature places.

Heterogeneous Products

It is my impression that agricultural economists are increasingly accommodating heterogeneous goods in their investigations. If so, this is to be applauded. Heterogeneous goods are those with imperfectly correlated attributes that often can be neither predicted nor controlled.

Grading or sorting may be undertaken to standardize quality. Markets with reach, or extent, create powerful incentives for uniformity in production, permitting specialization and economies of scale. The desire for diversity in consumption, however, provides incentive for separation of attributes, with recombination consistent with various market requirements. Market forces that drive uniformity and separation in production also must be meshed with the need to accommodate desires for differentiated consumption goods (Castle, 1972).

Heterogeneity is central to many problems in resource and environmental economics where resource attributes are fixed, but not necessarily highly correlated among units. This becomes especially relevant with natural capital. Because of heterogeneity, aggregation of different capital categories often is not possible except by applying a common denominator to the services they provide. This becomes especially troublesome in dealing with intergenerational issues because evaluation of resource services begs the underlying question of values over time.

A comparative analysis of market and non-market heterogeneous goods would be both interesting and valuable. In particular, induced innovations are likely to be quite different for market than for non-market goods, and this is of relevance to the availability of each over time. Incentives that make induced innovation possible in the production of non-market goods is a neglected subject in resource economics.

As Hausman notes, the domain of economic theory is defined by causal factors provided for by assumptions underlying neoclassical economic theory. The resulting domain of economic theory may not include people, or circumstances, of economic significance because of the incomplete nature of causal assumptions. Two such circumstances are described below. Intermediate decision units such as state and local government are not necessary for the operation of the neoclassical general equilibrium system. Firms and individuals at the micro level

are quite sufficient. Macro economic policy clearly requires some type of economic decision-making at that level. Yet intermediate decision-making is fundamental if there is to be coherence in community economics, a subject of much current discussion. Furthermore, actual intermediate decision-making is of considerable economic importance.

A second subject pertains to people and situations that are systematically excluded from the domain of economics because of the basic assumptions employed. Both require they be “found” by researchers; they will not be identified by neoclassical economic theory.

Intermediate Decision-making

The economic importance of intermediate decision-making arises from considerations of community welfare. These are decisions intermediate between firms and individuals at the micro level, and the national government at the macro level. Collective choice theory has obvious relevance, and should be exploited in this context (Olson; Stevens). Numerous complications arise when intermediate decision-making realities are introduced. For example, inconsistencies in choice indicators are encountered. A national benefit-cost criterion is not likely to be high on the list of local priorities. Given our system of government, the comparative advantage of the various levels of government in the performance of particular functions is of obvious relevance. This is a subject to which economics would appear to have much to contribute even though it has not done so to this time.

An important challenge involves an appraisal of community assets such as natural, man-made, human and social capital. Heterogeneity exists within as well as between these categories. Of these, social capital is the least understood and the most controversial. Even so, there is accumulating evidence that the “social capacity” of a community, group, or nation for collective action contributes significantly to overall productivity. Whether such considerations are grouped

under a catch- all such as institutions, or a different heading, is less important than improving economists' abilities to treat this category in a comparable way to other categories of capital. Some notable economists object to the use of social capital to describe such assets, although rigorous basis for this objection remains to be established (Durlag, Sobel, Woolcock).

Concern can be expressed about the theoretical base of the "new institutional economics" even though some useful and interesting results have been obtained. The use of economic efficiency models to explain the rise of new institutions appears questionable, unless an explanation can also be advanced for the persistence of institutions that violate economic efficiency criteria. This may be an arena where the separateness of economics will need to be abandoned. Many studies that attempt to measure changes in economic productivity over time indicate that results are improved when an institutional variable is included (Arrow et al.). But this variable is typically less completely specified than are those that emerge more directly from economic theory.

Excluded People and Groups

There is an explicit assumption in much economic analysis that people or places that have not fared well in the past will do better only by becoming incorporated into the prevailing economic system. Yet persistent poverty and enduring income differences among areas within the same economy calls such an assumption into question. As was demonstrated earlier, particular assumptions may result in the systematic exclusion of individuals or groups from a system of economic thought. Two issues come to the fore. One is whether economists have a responsibility to at least identify and possibly consider exclusions. The other is whether economists have the capacity to explain reality, even if those excluded are identified. Possible exclusions include: 1) those excluded because of the inexact and incomplete nature of

neoclassical economic theory (see above), and 2) those made worse off by economic reorganizations when compensation is not possible, or possible but not paid.

Paul Samuelson has penned the following: “Marie Antoinette said: ‘Let them eat cake.’ But history records no transfer of sugar and flour to her peasant subjects. Even the sage Dr. Greenspan sometimes sounds Antoine-ish. The economists of the 1930s—Hicks, Learner, Kaldor, Scitovsky and others, to say nothing of earlier writings by J. S. Mill, Edgeworth, Pareto, and Viner perpetrate something of a shell game in ethical debates about the conflict between efficiency and equality” (p144).

In the cited article Samuelson demonstrates that differences in comparative advantage do not always insure that trade will benefit all. (Numerous writers have questioned the policy relevance of traditional welfare theory. For example see Sen, Buchanan, Roth, and my former student Bromley.)

An important part of rural economics pertains to a possible incompleteness in economic models. Incompleteness is denied if it is believed that returns to factors of production across geographic areas always will be equalized by market adjustments within a reasonable time period. If the assumptions underlying economic models specify all individuals are equally willing and able to make market adjustments, the inexactness and incompleteness may need to be considered. More remote rural places may become repositories of activities and people that in some sense reflect market adjustment failures. Non-market institutions may be required for understanding and possible social adjustment.

Failure to understand that part of society may be excluded from an economic investigation may account, in part, for the hostility that often exists between economists and non-economists. Non-economists may intuitively understand an economic model is inexact and

incomplete, but will be unable to identify with precision why this is the case. In reply, economists may focus on the inadequacy of the criticism, rather than admit the inadequacy of their model. The result may be a great deal of “talking past” one another.

SUMMARY COMMENTS

To transgress is to violate a boundary. The basic assumptions underlying neoclassical economics have created boundaries with consequences that may not have been intended. If the consequences of these boundaries, intended or unintended, are understood, better use of the theoretical system will surely follow.

A strong case can be made that many of the major theoretical contributions in economics have resulted from the careful modification of one or more of the basic assumptions underlying the theory. Those who are successful in doing this are admitted to the elite, and become a part of the consensus that distinguishes the disciplinary acceptable from the unacceptable. Applied workers face a different challenge. They may have neither the competence nor interest to improve the basic theoretical structure of the discipline. Even so, they may discover real world situations to which the theory, with proper modifications, can be applied. Under such circumstances the applied worker faces a dilemma. Should they venture into the unknown, risking disapproval from the community of scholars, or should they move to more tractable problems and wait for theory to be developed?

The experience described in this manuscript suggests there is no ready substitute for an in-depth understanding of the methodological fundamentals of one’s field. The above paragraphs on the characteristics of neoclassical economics may seem to some as an attempt to discredit neoclassical economics. I deny any such motivation. All theoretical systems are partial. They become powerful only if the limits of applicability are known. Only with such knowledge can the

applied economist use the neoclassical framework with power and confidence. I regret deeply that I did not have the insights of Hausman and other methodologists earlier in my career.

With respect to my own experience, the economics of environmental quality could not have been investigated in an economy concerned only with homogeneous goods (an inexact assumption). Further, persistent low income places could not have been understood if economies consisted exclusively of people motivated primarily by wealth acquisition (an incomplete assumption). And the featureless plain of central place theory, arising from the separate science of economics, is incapable of reflecting the intricacies of local government and local comparative advantage, the essence of intermediate decision-making. In all these cases it was appropriate to challenge customary boundaries of the discipline. Yet, if interpersonal utility comparisons are to be avoided in resource allocation (a reorganization is okay if winners can compensate losers), then this boundary should be respected for income distribution issues as well (losers, in fact, must be compensated - see Samuelson above).

To Conclude —for Now

The intellectual journey described earlier is not yet complete even though, given my age, more has been observed than remains to be discovered. From experience I know that my views will change as information and knowledge are acquired. Certain comments about neoclassical economics that have been made may appear to be overly critical of a system of thought that has survived more than a century.

I view the matter differently. As noted, all theoretical systems are partial views of reality. Theoretical systems will not gain power from exaggerated claims of their scope of application. It is encouraging that mainstream economics appears to be placing less emphasis on sweeping generalizations and general prescriptions relative to appropriate analysis of particular problems.

It is appropriate for applied economists to consider transgressing boundaries that have been established for purposes other than those that motivate them.

As noted earlier, I came to Oregon State University mid-year 1954. Except for the decade spent at RFF, Oregon State has been my intellectual home since then. I have been assigned an office in Ballard Hall, near the one I occupied as an assistant professor in 1954. Geographically speaking, I have come full circle, even though I am not the same person intellectually or philosophically.⁹

The Department of Agricultural and Resource Economics has accommodated itself very well indeed to a different external environment than existed from 1954 to 1975 when I went to Resources for the Future. It is a joy to observe the professionalism and competence with which the Department conducts its affairs. If I had had the present Department Head, William G. “Bill” Boggess, as a role model, I could have been a better Head when I held that position.

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ENDNOTES

- ¹ Paul Barkley deserves credit for whatever coherence this manuscript may have. I am also grateful for the thoughtful review of Chapter two by Daniel Hausman. Responsibility for inadequacies rest with the author.
- ² The reader will be justified in asking at the outset: “How is an intellectual journey different than just a plain old journey?” This question deserves a serious answer. The usual journey typically is defined by two variables--time and place. A description of such a journey will permit a reader to have an impression of the time-place context of each passage. “Intellectual journey”, as used here, adds an additional variable to the time-place context. An additional variable, for want of better term, I label “subject matter.” In this case the subject matter is the application of neoclassical economics. Had it not been for this body of theory the journey described would not have been taken. Further, I have spent much of my professional career trying to decide just what I really think of neo-classical economics. In three places in the manuscript, I neglected time-place considerations and focused on subject matter issues. To some readers these passages will seem like digressions; to me they are the “third dimension” that is appropriate and relevant in a description of an intellectual journey. In chapter one, two such passages are labeled “subject matter generalizations” and the headings for these sections are italicized. Almost all of Chapter II pertains to “subject matter” with little reference to time or place. Considerable subject matter discussion occurs elsewhere in the manuscript but in a time-place context. Chapter II presents a view of neoclassical economic research methodology as of 2005. As used there “transgressions” means “violation of a boundary.” The argument is that applied economists must constantly challenge the boundaries of their discipline if they are to be relevant, and contribute to an evolutionary economics.
- ³ Years later, my intellectual journey required a presidential address be given to the American Agricultural Economics Association. Professor Bagley honored me by providing an introduction. It was appropriate I ask him to do so, because his classes were responsible for my becoming an economist.

- ⁴ While I was at the Kansas City Fed, Harry Truman returned to Independence, Missouri after serving as President of the United States. He took an office in our building while arranging for more permanent quarters. He took lunch with us in the officers' dining room, and I then saw him from time to time. This was exciting indeed, and I became something of a student of his life. From that study I learned a great deal, including distinguishing between the importance of a position one may occupy, and the importance of the person occupying the position.
- ⁵ In 1993, OSU President John Byrne asked me to recommend where the OSU Extension Service should be placed in the organizational structure of the University. Prior to doing so, I interviewed approximately 90 people from within the University, as well as users of Extension services. In my report I recommended that extended education become a total University responsibility, that the Dean and Director of Extension report to the Provost, and that academic Deans administer off campus education parallel to the administration of research and resident education. This was a major, in not revolutionary, recommendation for OSU. In a demonstration of great courage, President Byrne announced a cluster of decisions accepting my recommendations and assigning responsibility for implementation to Provost Roy Arnold. If difficulty of assignment were to be evaluated, I believe I had, by far, the easiest task of the three.
- ⁶ I served as Chair of the RFF Publications Committee for most of the ten years I was there. In that capacity I made final decisions on manuscripts that were published or not published. It was necessary I maintain familiarity with literature drawn upon by the authors, especially neoclassical economics, the dominant paradigm employed. I co-edited two RFF books as a part of those responsibilities (Castle and Skillings; Castle and Price).
- ⁷ I believe Karl Popper and agricultural economists have had a confused relationship. According to de Marchi, there is little evidence of direct interaction between Popper and economists on methodological issues. Popper's emphasis on falsification may have had

intuitive appeal to economists during a period when econometrics was coming to the fore. An impression may have existed that placing hypotheses derived from economic theory in juxtaposition with numerical historical data constituted a falsifiable “test” in economics. Responsibility for any such impression must rest with economists, not Popper. He believed in “risky conjectures” capable of refutation. Had economists really believed in following Popper, they would have been more interested in ex ante predictions of economic events, followed by ex post investigations. Such ex post efforts would permit account to be taken of variables that were provided for in ex ante predictions and those that were not. Such methods would meet some of the requirements of an evolutionary science. This is not a plea for a rehabilitation of Karl Popper in economic research methodology; rather it draws attention to the economic methodological implications of one of Popper’s insights.

- ⁸ My reading of part of the voluminous literature on the role of assumptions in economics leads me to believe an important omission exists. Suppose four assumptions are each believed to capture 95 percent of the essence of the phenomena to which they individually pertain. Assume further the four assumptions combine to form a theoretical system that defines the domain of the theoretical system itself. What is the expectation that the essence of a deduction from such a system will pertain to the domain? Is it .8145? This is, of course, a trivial example, but it is of relevance with a theoretical system that is inexact and incomplete as a consequence of its assumptions. Paul Barkley believes this becomes a variation of the theory of the second best if carried to its logical conclusion. I accept his word for this, because I have not come to grips with Paul’s argument.
- ⁹ I suspect pluralism and pragmatism would best describe my orientation as an administrator and public servant (Castle, E. N. 1996a and b).