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A number of historians of science have been involved in studying the nature of biology at the turn of the century, and the picture that they have developed describes biology during this time as a field struggling to define itself. During the late nineteenth and early twentieth centuries, biologists were trying to legitimize their field by discovering laws and theories that would underpin all of biology. In order to unearth these basic fundamentals, biologists looked to experimentation and shifted their attention to questions concerned with development and heredity. This "core" work did not constrain all of biology, however. There did exist researchers, fields of inquiry, and institutions that pursued biological investigations that did not coincide with the aims of discovering the basic laws and theories of biology. One

such researcher who did not aspire to discovering the laws and theories of biology was William Emerson Ritter.

William Emerson Ritter was largely concerned with making a biological survey of the coast of southern California. He wanted to discover how the marine organisms off this coast were distributed with respect to environmental factors, and he wanted to determine the adaptations they possessed that allowed them to live where they did. In order to achieve these aims, Ritter set out to create a marine station. His attempts culminated in the establishment of The Marine Biological Station of San Diego near the town of La Jolla, California in 1905.

A study of the ideas of William Emerson Ritter as related to the founding and development of this station proves to be very instructive. It not only illustrates that an institution can reflect the aims of a strong personality, but also illustrates that not all researchers or the institutions at which they work must necessarily conform to the aims of the disciplines of which they are a part. The research undertaken at the San Diego Marine Biological Station, under the guidance of William Emerson Ritter, was not directed toward discovering the laws and theories that were the foundation of biology; rather, it was directed toward learning about the marine organisms that inhabited the Pacific off the coast of southern California by discovering and describing the organisms present in the area, their distribution, and the physiological, morphological and/or behavioral adaptations they possessed to allow them to exist where they did.

The Marine Biological Station of San Diego: Its Development as Related to the Ideas and Ideals of William Emerson Ritter (1856-1944)

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INTRODUCTION

A marine station was established near the small village of La Jolla, California in the summer of 1905, but it was not until 1909 that this station was formally given the title of The Marine Biological Station of San Diego. The Marine Biological Station of San Diego endured to become the Scripps Institution for Biological Research in 1912, and finally the Scripps Institution of Oceanography in 1924. There exist very few accounts of the early history of this institution, in spite of its importance today in the field of oceanography.[1] With this in mind, it will be the purpose of this thesis to examine the San Diego Marine Biological Station; particularly, to examine how the investigations undertaken by researchers at the station in its formative years related to the ideas and ideals of William Emerson Ritter (1856-1944), the station's founder

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[1]For an excellent account of the early history of the Scripps Institution of Oceanography, see Helen Raitt and Beatrice Moulton's book, _Scripps Institution of Oceanography: First Fifty Years_ (La Jolla: Ward Ritchie Press, 1967). Their book documents the early efforts to establish the station at La Jolla, California, as well as some of the changes that occurred at the station through the years of William Emerson Ritter's directorship and T. Wayland Vaughan's directorship. Elizabeth Shor has also written a fine account of the history of this institution in _Scripps Institution of Oceanography: Probing the Oceans, 1936 to 1976_ (San Diego: Tofua Press, 1978), although her effort covers a later period.

and first director. This thesis will also attempt to evaluate the relationship between Ritter's aims and the aims of biology at that time. The scope of this project covers a period from about 1890 to 1920.

Ritter's ideas about establishing and developing the San Diego Marine Biological Laboratory were conceived and carried out during an important period in the development of the life sciences in the United States, i.e., 1890 to 1920. Several historians of science have concentrated upon this period in the development of biology, and they have spent much energy trying to define its nature. Among them is Garland Allen, who wrote Life Science in the Twentieth Century in 1975. In this book, Allen focused upon "the growth of biology between 1890 and 1965" in an attempt to understand some of the general characteristics of the development of the field of biology.[2] He concluded that succeeding generations of biologists concentrated upon questions that were fundamentally different than those studied by previous generations.

> It is clear that biologists around 1890, 1920, or 1960 were, in each case, devising fundamentally different kinds of theories, with different notions of what was an acceptable explanation of biological phenomena.[3]

Allen also concluded that biologists of latter generations,

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[2]Garland Allen, Life Science in the Twentieth Century (New York: John Wiley and Sons, Inc., 1975), p. xi.

[3]Ibid.

i.e., after 1880, looked to "hard-core" sciences, like chemistry and physics, for guidance and legitimation. This ultimately led to changes in the way biology was practiced; that is, "biology as it was practiced in the nineteenth-century—in natural history, descriptive and speculative; in physiology, largely mechanistic—was transformed into its twentieth-century mold: experimental, analytically rigorous, and integrative."[4] Essentially, Allen believed that upcoming biologists revolted against the morphological tradition of their predecessors in favor of an experimental approach to biology.

Allen's interpretation met with criticism. His critics insisted that his belief that U. S. biologists revolted against a morphological tradition in biology in favor of an experimental approach to biology was illogical, since it entailed a shift from subject matter (morphology) to methodology (experimentation).[5] In addition, through studies of their own, Allen's critics showed that there was no historical basis for a revolt.[6] Allen's critics did not

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[4] Ibid., p. xv.

[5] Jane Maienschein, Ronald Rainger and Keith Benson, "Introduction: Were American Morphologists in Revolt?" J. Hist. Biol., Spring, 1981, 14: 84.

[6] Ibid.; See also Jane Maienschein, "Shifting Assumptions in American Biology: Embryology, 1890-1910," J. Hist. Biol., Spring, 1981, 14: 89-113, Keith Benson, "Problems of Individual Development: Descriptive Embryological Morphology in America at the Turn of the Century," J. Hist. Biol., Spring, 1981, 14: 115-128, and Ronald Rainger, "The Continuation of the Morphological

deny that fundamental changes in the shape of biology took place in the late nineteenth and early twentieth centuries, but they did deny that those changes were revolutionary in nature. They adopted the position that the changes that occurred were slow, evolutionary, in nature.[7]

In light of his critics' arguments, Allen redefined his position. While not rejecting his belief that the change was revolutionary, Allen did reformulate his interpretation of that change.

> One of the strongest points of agreement that I have with Maienschein, Rainger, and Benson is their objection to my use of the term "morphology," and particularly the phrase "revolt from morphology." They have put their finger on a major source of confusion in my model.[8]

Allen rejected the morphologist-experimentalist dichotomy in favor of a naturalist-experimentalist dichotomy, but continued to favor the interpretation that a sweeping change that led from descriptive and speculative studies to experimental and analytical studies occurred.[9] Allen did not point out when this revolution took place, but he did maintain that it was very real since it did exist "in the

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Tradition: American Paleontology, 1880-1910," J. Hist. Biol., Spring, 1981, 14: 129-158 for a discussion of the subject.

[7]Maienschein, et. al., "Introduction," p. 86.

[8]Garland Allen, "Morphology and Twentieth-Century Biology: A Response," J. Hist. Biol., Spring, 1981, 14: 160.

[9]Ibid., p. 166.

minds of the investigators at that time."[10]

The debate over whether the development of biology in the United States at the turn of the century was revolutionary or evolutionary is not of the utmost importance; rather, it is the stimulation that the debate provided to study the nature of biology during this time period that is of importance. The controversy which Allen's thesis created has led numerous historians to begin an in-depth study of the essence of late nineteenth- and early twentieth-century biology in the United States. As with most disciplines of study, the topic has been approached in different ways. For example, while some historians have focused upon the work carried out at specific institutions, others have studied the development of biology during this period by looking at the work and attitudes of various personalities at these institutions. This multi-faceted approach has been fruitful, and has led to the development of a very interesting picture of the nature of biology in the late nineteenth and early twentieth centuries. The progression of biology at this time was not as simple as Allen envisioned.

Allen concentrated on the period between 1890 and 1965 in his discussion of biology in the United States, but this does not imply that biology only first appeared in the United States in 1890. There was a great deal of activity

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[10] Ibid.

in the field of biology before this period, especially with respect to introducing biology into education. One historian who has done considerable work in this area is Sally Gregory Kohlstedt. She has concentrated upon how institutions, such as museums and libraries, had an impact upon the development of biology, because she views these institutions as having set the stage for the professionalization of biology. It is an area of historical inquiry which Kohlstedt feels has been overlooked by historians of science.

> The regular development of museum resources by colleges during the nineteenth century has only infrequently been noted by historians of science or education, whose attention has been drawn more to the efforts of graduate programs building experimental laboratories at the end of the century. In fact, when noted at all, nineteenth-century college museums are not uncommonly portrayed as obstacles to be overcome by faculty members seeking to find space for research facilities.[11]

Kohlstedt insists that mid-nineteenth century college museums played a valuable role in the institutionalization of biology, and contends that their collections acted as "a fundamental tool for teaching natural history in undergraduate curricula by the 1860s."[12] She does not allege that museums arose to fulfill this role; rather, she

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[11]Sally Gregory Kohlstedt, "Museums on Campus: A Tradition of Inquiry and Teaching," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988), p. 15.

[12]Ibid., p. 16.

proposes that they were slowly integrated into educational practices as the natural sciences embraced a more important place in the curricula of many colleges. Museums and the natural sciences shared a special relationship in the mid-1800s: museum collections served to emphasize the point that the natural sciences were an important facet of college curricula, while the institutionalization of the natural sciences served to give heightened exposure and distinction to museums.[13]

Kohlstedt demonstrates that the expansion of biology in academic circles began in the mid-1800s, and that museums played a valuable role in this development. She illustrates that museum research helped natural scientists of the mid- to late-1800s identify their position in the university and helped to establish biology as a discipline worthy of study at the university level. Her work is informative; however, her study illustrates only one aspect of the development of biology in the United States, and her use of institutions to illuminate that facet is only one way of studying that development.

Keith Benson has also examined museum research, but he has looked at it from the perspective of how it related to research based in the laboratory. He shows that natural history research based around college museums succumbed to biological research centered in the lab.

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[13] Ibid., p. 21.

> However, beginning in the 1860s and continuing through the end of the century, biology moved beyond the museum. First into nature, most notably at Penikese, and its successor marine stations, and then into the university laboratory, biological investigations expanded.[14]

Benson maintains that the shift from museum research to laboratory research occurred because "colleges and universities felt pressure to reexamine the role of science in an academic arena" after the United States Civil War.[15]

> Before the educational reforms in American Universities in the 1870s and 1880s, the usual method of learning natural history involved memorizing a text and reciting the lessons from it on command by the instructor. . . . Such courses never involved the active investigation of nature nor did they include any exposure to research in nature. Certainly laboratory exercises were unheard of. . . .
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> Not only were there flaws in the instructional methods; most schools, especially the smaller colleges and universities, lacked a clear definition of what science encompassed. A professor of natural science might teach "chemistry, physics, astronomy, botany, zoology, mineralogy, geology, physiology, and perhaps Paley's evidence on top of all."[16]

Biologists felt the need to develop "a new approach to the teaching of biology."[17] They also felt the need to expand the role of laboratory research in academic institutions

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[14]Keith R. Benson, "From Museum Research to Laboratory Research: The Transformation of Natural History into Academic Biology," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988), p. 77.

[15]Ibid., p. 57.

[16]Ibid., p. 59.

[17]Ibid., p. 60.

and to increase the amount published concerning research findings. A leader in working toward meeting the goals of improving education and of expanding research was Johns Hopkins University.

> By 1883, the university had constructed a new "Biological Laboratory" . . . hired a physiologist and a morphologist, each charged to conduct a graduate program in biology; established a marine research station; and begun to publish scientific journals containing research papers. All were unparalleled developments in biology in this country and all had significant impacts on the new direction in biology. No longer was natural history the sole goal of the biological community. The "little band of men" charged ahead to promote research ideal in nature, based upon the laboratory model. The thrust was to provide Americans with opportunities in biology that equalled those opportunities in Europe and to ensure that American biology would begin to contribute to science.[18]

Benson has spent a great deal of time investigating the establishment and development of the biological science program at Johns Hopkins, and he insists that the work at Johns Hopkins marks a transitional period between descriptive morphological studies and experimental investigations in biology. He has illustrated this point by examining the work of William Keith Brooks (1848-1908), a faculty member at Johns Hopkins in the late 1800s.

> In considering the continuities between morphology and experimental biology, it is more accurate to depict Brooks as a transitional figure between the descriptive studies and the emergence of another branch of biological investigation, experimentation. While Brooks's own work was decidedly descriptive in character, much of it was

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[18] Ibid., p. 63.

> valuable in raising important questions and in providing techniques and methods for experimentalists. This interpretation concerning the role of Brooks and morphology in the development of American biology presents an alternate view stressing continuities between morphological studies and experimental work, thereby demonstrating that nineteenth-century morphology provided a real framework for biological science in the twentieth century.[19]

An important feature of Brooks' career was that he carried his descriptive work into the university laboratory which, by the 1860s, was quickly becoming the home for biological research. While research was moving into the laboratory at this time, Benson is careful to point out that neither did the new research ideal replace the natural history tradition, which continued to thrive in American museums, nor did experimental work totally eclipse descriptive work.[20] Toby Appel's work on the history of professional societies in the United States also illustrates this point.

> One discovers that, in terms of societies, there was no radical "revolt from morphology." By a gradual transformation, the American Morphological Society incorporated experimental studies in its program and, without serious conflict, eventually recognized the shift by changing its name to the American Society of Zoologists.[21]

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[19]Keith R. Benson, _William Keith Brooks (1848-1908): A Case Study in Morphology and the Development of American Biology_ (Ph.D. dissertation, Oregon State University, 1979), p. 9.

[20]_Ibid._; Benson, "From Museum Research," p. 77.

[21]Toby A. Appel, "Organizing Biology: The American Society of Naturalists and its 'Affiliated Societies,' 1883-1923," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., _The American Development of Biology_ (Philadelphia: University of Pennsylvania Press, 1988), p.

Benson has examined the nature of biology at Johns Hopkins University by looking at the role of William Keith Brooks at this institution and by inspecting the type of research pursued there. His examination of a specific institution and of the work of a particular investigator at that institution illustrates another way in which historians of science have approached studying the nature of the development of biology in the United States at the turn of the century. He is not alone in using this approach. A well-known historian of science who has also used this approach is Jane Maienschein.

Maienschein has done considerable work on the research interests that dominated at the Marine Biological Laboratory (MBL) at Woods Hole in the 1890s. Her work is important, because it illustrates the main concerns of biological researchers of the time. Maienschein, in the preface to Defining Biology: Lectures from the 1890s, asserts that the Biological Lectures presented at the MBL during its formative years, i.e., 1890 to 1900, reflect some of the questions that biologists at this time deemed important.

> The focus of interest of the Biological Lectures shifted from year to year as new discoveries brought new questions, but some themes underpinned discussion throughout the 1890s. Most notably, questions about the significance of heredity and evolution for development, and related questions

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> about the significance of cell cleavage for differentiation of individuals, ran through many of the lectures. Initially, discussion centered on the question, to what extent is the egg cell already organized in its earliest stages? Is there something brought to the egg by heredity, something to some extent predelineated? Or does form and heterogeneity emerge only gradually or epigenetically in the course of time? All of these discussions directly impinge on the more general debate about preformation and epigenesis.[22]

Maienschein also maintains that the biological lectures presented at the MBL during the 1890s provided a sense of community to the researchers undertaking projects there, and that Charles Otis Whitman (1842-1910), the first director of the MBL and the coordinator of the MBL lectures, hoped that this feeling of community would lead to cooperation among MBL researchers and would result in the formation of a cohesive field called biology.[23] Philip J. Pauly shares this view with Maienschein, asserting that particular interests were shared by researchers at the MBL, and that the MBL provided "an environment within which certain problems and concepts gained particular communal importance."[24] Pauly states:

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[22]Jane Maienschein, ed., Defining Biology: Lectures from the 1890s (Cambridge, Mass.: Harvard University Press, 1986), p. 21.

[23]Ibid., p. 17.

[24]Philip J. Pauly, "Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882-1925," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988), p. 134.

> The MBL community's overriding scientific interest in the 1890s was to understand how the different parts of small, isolated cell communities (called marine embryos) functioned in cooperative fashion. More specifically, they sought to determine how such cell communities maintained their organic unity as they matured, that is, as the community became more complex and the activities of individual cells more specialized. The consensus was that while external, "extra-ovate" forces were important modifiers of development, the cell community (like the biological community) was fundamentally autonomous and self-directed. To exaggerate only slightly, scientists at Woods Hole sought to provide an account of life at a resort.[25]

The MBL, under Whitman's direction, was, in Pauly's view, a summer resort where biologists from different specialties could gather to work and exchange ideas and, while in this process, create a unified field called biology.[26]

The research of these two historians has shed light on an interesting aspect of the development of biology. It has shown that while biologists were struggling to make research a valuable part of their science, they were also seeking "to identify a 'core' for biology, if only as an ideal."[27] While the historical investigations of Maienschein and Pauly have illustrated the major research interests of biologists at the turn of the century, it must also be acknowledged that the work carried out at

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[25] Ibid., pp. 135-136.

[26] Ibid., p. 123.

[27] Ronald Rainger, Keith Benson and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988), p. 4.

institutions such as the MBL was not all-inclusive. Benson states:

> A core did exist, then, as identified by those in leading positions in biology. Yet although it provided a coherent focus, this ideal core did not define or constrain all of biology—and this is perhaps the most important conclusion of this collaborative volume. Biology was not, either before or after 1900, so monolithic or unified as the core advocates or popular impressions might suggest. Even the primary proponents of the core did not claim that biology must necessarily include only the core work. Rather the core served as a body of issues and approaches that the participants agreed represented legitimate biology. . . . They sought to identify a core but not to circumscribe biology thereby: biology must consist of more than a core, just as a cell requires more than a nucleus.[28]

An area upon which a few historians of science have converged is ecology, and, like the larger field of biology, it has been studied from different angles. Eugene Cittadino has delved into the reasons why early botanists sought out and developed the field of ecology, and he has examined the research that these botanists hoped to achieve. He maintains that "ecology was first recognized and consciously pursued during the 1890s as a specialization within botany" as botanists began to move "away from description and classification . . . toward studies of process and function."[29] Joel Hagen agrees with Cittadino's summation, and maintains that ecological plant

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[28] Ibid., p. 5.

[29] Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890-1905," Stud. Hist. Biol., 1980, 4: 172, 174.

geographers rejected "taxonomic, historically oriented biogeography" in favor of experimental plant physiology.[30] Hagen and Cittadino have both studied the development of ecology by looking at how descriptive, taxonomic work succumbed to physiological research that sought "to understand the functional relationships between plants and the physical and biotic conditions of their habitats."[31] As to exactly what was underlying this shift will be discussed in the third chapter of this thesis.

Historical studies about the early roots of ecology are not the only types of studies being pursued by historians of science. Joel Hagen recently discussed how Frederic Edward Clements' (1874-1945) theory of succession related to his organismal conception of life. According to Hagen, Clements believed that the individual organism and the plant community shared some common features, and that "whether the ecologist was studying adaptive changes in the individual plant or long-term successional changes in the plant community, the explanation could be reduced to simple stimulus-response relationships between the physical

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[30]Joel B. Hagen, "Ecologists and Taxonomists: Divergent Traditions in Twentieth-Century Plant Geography," J. Hist. Biol., Summer, 1986, 19: 198.

[31]Cittadino, "Ecology and the Professionalization of Botany," p. 192.

environment and the biological organism."[32] In Hagen's opinion, Clements sought to discover ecological laws and theories that would serve to unify the field of ecology.

> Clementsian ecology, however, aimed at being more than physiological rhetoric. The organism and its internal processes provided the subject matter for physiology. By comparing plant communities with individual organisms, Clements attempted to establish a unified physiological theory of ecology. Though very different, the complex community organism and the individual organism could be understood in terms of the same mechanical principles.[33]

Clements' search for a theory that would unify all of ecology was not unlike what scientists were doing in the larger realm of biology.

The work in marine biology appears to represent another area of scientific endeavor that fell outside the central core of biology, but this is not necessarily the case. The study of marine biology and of marine biological institutions is more often than not an area of study pursued for the express purpose of illustrating the aims of general biology in the late nineteenth and early twentieth centuries. The work of Maienschein and Pauly on the MBL illustrates this; however, while their historical investigations seem to be more common, it must be

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[32] Joel B. Hagen, "Organism and Environment: Frederic Clements's Vision of a Unified Physiological Ecology," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., <u>The American Development of Biology</u> (Philadelphia: University of Pennsylvania Press, 1988), p. 275.

[33] <u>Ibid</u>., pp. 274-275.

acknowledged that studies about marine biological research that did not meet the aims of the central core of biology have been done. The work of Carmelo Tomas on ecological investigations carried out by the Stazione Zoologica in Naples illustrates this.[34] In addition, Ralph W. Dexter, in "History of American Marine Biology and Marine Biology Institutions," has briefly described the origins of marine biology in the United States.[35] He has chronologically catalogued when various stations appeared, and, for some stations, has provided a brief discussion about the motivation behind the establishment of that station and/or the focus of the work that appeared there. Studies do exist, but they are few and far between. Little work has actually been done on examining those marine stations that pursued research outside the central core of biology, stations much like the San Diego Marine Biological Station.

As was mentioned at the beginning of this introduction, this thesis will examine how the early research of the San Diego Marine Biological Laboratory related to the ideas of William Emerson Ritter. One of the major claims of this thesis, after all, is that the station's goal of making "a Biological Survey of the waters

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[34]See Carmelo R. Tomas, "Marine Botany and Ecology at Stazione Zoologica," Biol. Bull., 1985, 168: 168-171.

[35]Ralph W. Dexter, "History of American Biology and Marine Biology Institutions. Introduction: Origins of American Marine Biology," Amer. Zool., 1988, 28: 3.

of the Pacific adjacent to the Coast of Southern California," reflected the ideas and ideals of Ritter.[36] This thesis will also examine how Ritter's ideas compared to those of his contemporaries. This is because another claim of this thesis is that Ritter's aims for the San Diego Marine Biological Station did not coincide with the biological endeavors of those of his contemporaries who wanted to discover the laws and theories that they believed would unify biology; consequently, the San Diego Marine Biological Station did not contribute to the "core" work of biology. The validity of these claims will be discussed in the chapters that follow.

The first chapter will deal with Ritter's early life and with his involvement in the establishment and development of the San Diego Marine Biological Station. Many of the events that occurred during the formative years of the station had a significant impact upon the program of the station; therefore, information of significance in describing the establishment and the development of the station will be included in this chapter. Chapters two and three will be devoted to illustrating Ritter's ideas. In chapter two, Ritter's organismic philosophy will be examined. Ritter supported a tradition in which

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[36]William Emerson Ritter, "A General Statement of the Ideas and the Present Aims and Status of the Marine Biological Association of San Diego," Univ. Calif. Publ. Zool., April, 1905, 2: iii.

understanding the organism as it existed in nature was of central importance. He maintained that the organism could only be understood by looking at it as a whole and within the context of its environment.[37] To gather information of this sort, he advocated the use of experimentation in conjunction with description.[38] His attitude about how an organism should be studied set the stage for his research program at San Diego. Chapter three will focus upon Ritter's ecological ideas. An inherent part of the San Diego Marine Biological Station's aim to survey the life of the waters of the Pacific was to learn about the organisms present in the Pacific and about how they lived. An important facet of these investigations was that they were ecological. Embryological, physiological, and morphological studies were also carried out at San Diego, but they were carried out to illuminate answers to questions concerned with how specific organisms were adapted to particular environments. Chapter three will move beyond this simple discussion of Ritter's ideas by comparing his ideas to those of the early ecologists and to those of some of his colleagues. A major reason for doing this is to establish that ecology was an important component of the survey Ritter wanted to complete. In the fourth chapter, the actual investigations being pursued at

[37] Ibid., p. viii.

[38] Ibid., p. xvi.

San Diego will be perused to determine if the station was meeting Ritter's aim of completing a biological survey. The conclusion to this thesis will evaluate the extent to which the program at San Diego reflected the ideas of Ritter, and the degree to which Ritter's aims were being met at the station. This chapter will also consider how Ritter's aims compared to the intention of many of his contemporaries of creating a unified field called biology.

CHAPTER 1

WILLIAM EMERSON RITTER AND THE ESTABLISHMENT OF THE MARINE BIOLOGICAL STATION OF SAN DIEGO

William Emerson Ritter died on January 10, 1944. He was eighty-eight years old. Francis Bertody Sumner (1874-?), a scientist at the San Diego Marine Biological Station from 1913 to 1944, commented on how Ritter's age at his death must have surprised many, for Ritter had remained active in his pursuit of scientific knowledge and in his contribution to scientific journals until as late as, and even beyond, 1938. Sumner stated:

> Ritter got a somewhat delayed start in his scientific career, but he more than compensated for this by exceptional productivity at an age when most of us lapse into silence.[1]

Ritter did indeed get a "somewhat delayed start in his scientific career." Born to Horatio Ritter (1822-1896) and Leonora Eason Ritter (1827-1896) on November 19, 1856 in Hampden, Wisconsin, Ritter spent the first twenty years of his life on the family farm in Wisconsin. He was educated at the State Normal School at Oshkosh, and took up public school teaching following his graduation from this institution in 1884.[2] After teaching for a short period of

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[1] Francis B. Sumner, "William Emerson Ritter: Naturalist and Philosopher," Science, April, 1944, 99: 335.

[2] Ibid.; Deborah Day, "Guide to the Papers of William Emerson Ritter (1856-1944) 1893-1942," SIO Reference Series, June, 1982, 82-16: 3.

time, Ritter decided to pursue a higher level of education. He left Wisconsin for Berkeley in 1885, but did not begin to study zoology under Joseph LeConte (1823-1901) until 1886.[3] Following the completion of his Bachelor of Science degree in 1888, Ritter moved to Cambridge, Massachusetts. He received a Master of Arts degree from Harvard in 1891, whereupon he returned to Berkeley as a Lecturer in the Department of Zoology. He completed his doctorate in 1893, while teaching at Berkeley. He continued to teach at Berkeley after attaining his doctorate, and secured the position of Assistant Professor of Zoology in 1893, the position of Associate Professor of Zoology in 1898, and the position of Full Professor of Zoology in 1902.[4]

During the years in which he was completing his formal education and was improving his position at Berkeley, Ritter was also involved in work that culminated in the establishment of the Marine Biological Station of San Diego.

> In the spring of 1892 a structure 16 by 24 feet, partly of wood and partly of canvas, and constructed with a view to being taken to pieces and moved about, was built for use as a seaside

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[3] Eric Mills, "'Useful in Many Capacities.' An Early Career in American Physical Oceanography," for publication, p. 5; Helen Raitt and Beatrice Moulton, _Scripps Institution of Oceanography: First Fifty Years_ (La Jolla: Ward Ritchie Press, 1967), p. 4.

[4] Mills, "'Useful in Many Capacities.' An Early Career," p. 5; J. McKeen Cattell and Jaques Cattell, eds., _American Men of Science_ (New York: The Science Press, 1927), p. 822.

laboratory at Pacific Grove.[5]

The marine laboratory at Pacific Grove was set up to facilitate summer collecting and some limited instruction. The first laboratory party included about a dozen persons, the majority of which were students and teachers. According to Ritter, very little was accomplished during this particular expedition in terms of discoveries, and this in spite of the fact that the collections made rendered a myriad of specimens and a large amount of information.[6] Helen Raitt and Beatrice Moulton, in their book _Scripps Institution of Oceanography: First Fifty Years_, state:

> At the outset there was little to distinguish the University of California's seaside laboratory from those maintained by other institutions, except, perhaps, that it was more migratory and less well-equipped than others. But the nature of the biological studies, and the methods employed in carrying them out, followed the pattern established at such laboratories as that at Woods Hole, Massachusetts, and the Stazione Zoologica in the Bay of Naples. In these and similar institutions scattered throughout the world in those days individual scientists spent a portion of their time, most frequently the summer, pursuing their own biological studies on specimens of marine life.[7]

Raitt and Moulton's conviction that the early work of

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[5]William Emerson Ritter, "The Marine Biological Station of San Diego: Its History, Present Conditions, Achievements, and Aims," _Univ. Calif. Publ. Zool._, March, 1912, _9_: 148.

[6]_Ibid_.

[7]Raitt and Moulton, _Scripps Institution of Oceanography_, p. 5.

Ritter and his followers was largely based upon completing individual investigations on marine organisms may have been true for the laboratory when it was set up at Pacific Grove, but it cannot be applied to the work that occurred when the laboratory was re-constructed on the shore of Avalon Bay, Santa Catalina Island in 1893. During six weeks of that summer, students and teachers from the Zoology Department of the University of California sought a "general familiarity with sea-animals and the conditions under which they [lived] rather than rigorous special researches."[8]

Following these few brief excursions to the field, very little was done by Ritter between the years of 1894 and 1900. The small start at Pacific Grove, followed by several collecting trips around the San Francisco Bay area, had activated Ritter's desire to explore the southern coast of California with the possibility of establishing a permanent field station, so why did he delay in his explorations between these years? The answer can be attributed to the fact that Ritter was not present at the University of California for much of this time. He was in Europe in 1894 and 1895, and he was a member of the Harriman Alaska Expedition of 1899.[9] In spite of his

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[8]Ritter, "The Marine Biological Station of San Diego," pp. 148-149.

[9]Sumner, "William Emerson Ritter," p. 336.

absence from the Berkeley campus, the idea of developing a marine biological station was in Ritter's thoughts. While in Europe, Ritter spent time in Naples and Liverpool, sites of world-renowned marine biological laboratories, and while on the Harriman Alaska Expedition, Ritter made observations of use in relation to developing a field station.

> The observations by the writer while a member of the Harriman Alaska Expedition in the summer of 1899 were of special service toward a general clarification of views regarding points on the Pacific Coast of North America favorable for the location of marine stations concerned with the various aspects of biology and oceanography.[10]

Ritter returned to Berkeley in 1901, and actively re-involved himself in exploring the coastline for a suitable site for a marine station. Ritter was serious about the need for a permanent marine laboratory, and his ideas about what the aims of such a laboratory should be were materializing.

> In view of the importance of the field, and the meagerness of previous investigations in it, it seemed best to plan the summer's work as though it were to be the beginning of a detailed biological survey of the coast of California, even though no assurance could be had of the possibility of continuing the work beyond this season.
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> Such a survey should of necessity comprehend the investigation, not merely of the life of the area, but as well of the physical conditions under which it exists.[11]

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[10] Ritter, "The Marine Biological Station of San Diego," p. 150.

[11] William Emerson Ritter, "A Summer's Dredging on the Coast of Southern California," <u>Science</u>, January, 1902, <u>15</u>: 55.

San Pedro was considered as a possible site from which these aims might be achieved; therefore, in 1901 a temporary laboratory was established with the hopes that "a permanent, well supported seaside station" might soon grace the area.[12] Two thousand dollars was guaranteed to support the work that was to take place. This amount of money, which came from private individuals of the Los Angeles area, was not significant, however, and the summer work had to be carefully planned. It was ultimately decided to collect and study the shore life as had been done at Pacific Grove and Avalon Bay, and to begin some limited work at sea. The work at sea was restricted "on the biological side to dredging and trawling in depths not exceeding one hundred fathoms," and on the hydrographic side to measurements of density and temperature.[13] A small launch, the "Elsie," was rented, to complete these measurements. It served to gather information from eighty-five stations. Research was definitely the focus of the work at San Pedro in 1901, but student instruction was also provided. Ritter cited two reasons for this:

> One, the more weighty it must be confessed, was the hope that the small fee charged would yield enough to meet the travelling expenses of the University instructors, whose meager regular salaries would have to be supplemented in some way to make it possible for them to participate in the

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[12]Ritter, "The Marine Biological Station of San Diego," p. 151.

[13]*Ibid*., p. 153.

> work. The other consideration was the genuine belief that the advantages of so favorable a natural opportunity for instruction ought to be used.[14]

Less than two thousand dollars was gathered to run the station at San Pedro in 1902. The work carried out at sea was discontinued due to the lack of funds; however, the collection of seashore life continued as before, as did summer instruction. In light of declining funds, a decision was made to try "to place the station on a larger, more secure financial basis."[15] Several private individuals of the Los Angeles area devised a plan for securing twenty thousand dollars to be used for building a permanent laboratory and for acquiring a boat that could, at the very least, undertake research similar to that of the previously employed "Elsie." The plan was unsuccessful, with only about one-third of the required amount being collected. Ritter was very philosophical about the enterprise, referring to the experience as "a lesson not to be ignored as to the extreme difficulty of raising a considerable sum of money for such a purpose by such a method."[16] Before the summer of 1903, further problems struck the plan to locate the station in the San Pedro region (see table 1.1, page 36). Harbor improvements

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[14] _Ibid_., pp. 153-154.

[15] _Ibid_., p. 154.

[16] _Ibid_., pp. 154-155.

implemented by the federal government had destroyed not only the building being used as the laboratory, but had also caused the loss of some of the finest collecting grounds in the region. The harbor was becoming increasingly important on a commercial scale, and it was believed by those involved in Ritter's project that this would attract a large urban population within a few years.[17] This made the site extremely unattractive, and the decision was made to move the station from San Pedro to Coronado.

Ritter had explored Coronado in 1901, and he felt that establishing a temporary station in this area after the experience at San Pedro fit in well with his aims of searching for a site at which to establish a permanent station.

> To leave this region unvisited would be contrary to the original idea of making a reconnaissance of the California coast before settling anywhere permanently.[18]

A boat-house owned by the Coronado Hotel Company was outfitted as a laboratory, and a small schooner, the "Laura," was rented and used for six weeks of work in June and July.

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[17]Ibid., p. 155; William Emerson Ritter, "A General Statement of the Ideas and Present Aims and Status of the Marine Biological Association of San Diego," Univ. Calif. Publ. Zool., April, 1905, 2: xiii.

[18]Ritter, "The Marine Biological Station of San Diego," p. 155.

The research at Coronado focused mainly upon plankton, and it was carried out over a large portion of the year. Student instruction was discontinued for three reasons: first, those who had been involved in providing instruction were becoming increasingly absorbed in their investigations; second, the expenses of investigations and of researchers did not have to be supplemented by student fees; and third, instruction was seen as being much less important than research.[19] In spite of the success Ritter and his colleagues encountered at Coronado, there did exist several long-range difficulties with the location; difficulties that resembled those encountered at San Pedro (see table 1.1, page 36). Coronado was rejected as the final site of the station, because of these difficulties; however, the San Diego region was not altogether abandoned.

Fifteen miles to the north of Coronado lay the San Diego suburb of La Jolla. This area was selected as an advantageous place at which to establish a marine station; thus, during the preparations for the summer's work of 1905, the station was transplanted from Coronado to La Jolla. A piece of ground that was to be used for park purposes was granted to the station by the City Council of San Diego. In addition, a local group of La Jolla citizens raised one thousand dollars to be put toward the

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[19] Ibid., pp. 157-158.

construction of a laboratory building.[20] In spite of the eagerness of the La Jolla community to aid in the establishment of the station, questions arose as to the suitability of the area.

> Would there be room for such expansion as the institution might some time undergo? Would complications grow out of the relations between the station, whose primary purpose was scientific research, and the park, whose primary purpose was to serve as a recreation place for the public? Would the purity of the sea-water be more or less interfered with after a while by the sewage and other refuse of the growing population? Events and reflections made these questions more and more pressing as time went on, and the feeling grew that a more commodious, more unhampered site ought to be found if possible.[21]

Edward Wyllis Scripps (1854-1926), one of the major contributors to the station, proposed an alternative location, which was ultimately chosen as the final location for the Marine Biological Station of San Diego. This area, north of the city of La Jolla, was secured for one thousand dollars, and all legal affairs were taken care of by August of 1907. By June 1, 1909, the laboratory was in working order, and Ritter took up the position of scientific director.[22]

The work proceeded well at the station, but "those most involved with the station were . . . anxious to find additional sources of revenue," and eager to make it clear

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[20] *Ibid*., p. 159.

[21] *Ibid*., p. 160.

[22] *Ibid*., pp. 161-162.

to others that the San Diego Station was of value.[23] Among those that the members of the station wanted to impress were the Regents of the University of California; therefore, in the fall of 1911, Ritter and Charles Atwood Kofoid (1865-1947), a researcher who joined the staff at Berkeley in 1900, "advanced to President Wheeler and the regents the idea of transferring the affairs and property of the institution to the university."[24] The response was cautious, but this mood changed after university officials made some investigations into the station. In 1912, the San Diego station became officially affiliated with the University of California, and its name was officially changed from The Marine Biological Station of San Diego to The Scripps Institution for Biological Research of the University of California. An important aspect of this change is that the word "marine" was dropped from the title of the station. This was the result of a decision that the biological investigations carried out at the station not be restricted to the marine environment.

> It is proposed not to restrict biological research at the station to marine organisms as has thus far been done, but to extend it to land plants and animals as well. This would necessitate the creation of experimental culture plots, propagation grounds and houses, animal paddocks and run-ways, herbarium and museum buildings, and various other instrumentalities usual to such

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[23] Raitt and Moulton, Scripps Institution of Oceanography, pp. 61-62.

[24] Ibid., p. 62.

> investigations, all of which require much ground space.[25]

Ritter claimed that "although, as indicated by the change of name, an enlargement of activities is contemplated, no immediate alteration of policy or work will take place."[26] Ritter's claim did not hold true; in fact, the changes in research aims went far beyond even those contemplated in the name change. As the years passed, investigations into the physical and the chemical aspects of the ocean began to take on an increasingly important role at the station, a phenomenon that will be further discussed in chapter four. The recognition that the role of the station was changing as the years passed resulted in yet another name change for the station. On June 28, 1924, The Scripps Institution for Biological Research of the University of California was renamed The Scripps Institution of Oceanography of the University of California.

The establishment and development of the San Diego Marine Biological Station was not the result of the efforts of only one or two persons; rather, it involved a large number of individuals. Some individuals were involved for only a brief period of time.

> It is pleasant to recall the lively interest taken by several persons besides those participating in the work itself during these first years [i.e.,

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[25] Ritter, "The Marine Biological Station of San Diego," p. 246.

[26] Ibid., p. 136.

> Santa Catalina, 1893]. President Martin Kellogg was sympathetic and ready to extend such help from the University, financial and other, as conditions would permit. Professor Joseph LeConte, under whose official headship matters zoological in the University then rested, was ever enthusiastically desirous of seeing a seaside laboratory strongly and permanently established, and to this end never failed to use his influence when occasion offered. Perhaps the most earnest aggressive worker in the cause outside of those professionally entangled was Mr. Arthur Rodgers, an alumnus of the University and for many years one of its most devoted and efficient Regents. Nor would it do to leave unmentioned the great interest taken by Mr. Adolph Sutro of San Francisco in the establishment of a marine laboratory and aquarium in connection with his extensive developments at the famous Sutro Heights just outside the Golden Gate.[27]

Other individuals worked closely with Ritter throughout the origination of the San Diego Marine Biological Laboratory.

> Among the circumstances that contributed largely to the resolution for a more definite and permanent attempt to establish such a station, two only need be mentioned: Dr. C. A. Kofoid, who had come into the department of zoology of the University in 1900, had had much experience in marine and aquatic biology and so was an important addition to the working force available for such studies on the sea and its life as had been occupying us. The other circumstance was the coming to scientific manhood of two university students who had chosen to cast their lots as biological investigators. These men were Dr. F. W. Bancroft and Dr. H. B. Torrey. The strength of these three enthusiastic biologists, added to that previously available, made a total working force that promised much, not only for the main aim, scientific achievement, but also an assurance that with the united effort of such varied interest the securing of needed funds and facilities would be possible.[28]

Moulton and Raitt assert that Kofoid, Frank Watts Bancroft

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[27] Ibid., p. 149.

[28] Ibid., p. 151.

(1871-1923) and Harry Beal Torrey (1873-?) were especially important to Ritter, because they "were thoroughly sympathetic with his views."[29] Another group of individuals who were instrumental in helping Ritter realize his goals were those individuals who financially supported the development of a marine research station, the most important being E. W. Scripps and his half-sister Ellen Browning Scripps (1836-1932).

> On the side of material support a matter of utmost importance in connection with the removal to San Diego was the keen, intelligent, and financially liberal interest taken in the station from the outset by Miss Ellen B. Scripps and Mr. E. W. Scripps. Although a considerable number of citizens of San Diego contributed well during the first two years, these two persons were the chief givers and soon became the exclusive patrons so far as money gifts were concerned.[30]

The early moves to establish the San Diego Marine Biological Station at a permanent location on the southern coast of California have been the focus of this chapter; however, an important question that has come out of this discussion deals with Ritter's motivation behind establishing the station. An examination of the articles Ritter wrote in relation to the San Diego Marine Biological Station provides insight into this problem. From the beginning, Ritter was very specific in terms of defining

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[29]Raitt and Moulton, Scripps Institution of Oceanography, p. 8.

[30]Ritter, "The Marine Biological Station of San Diego," p. 158.

the work to be pursued by investigators at the marine station; that is, he wanted station researchers to carry out "a detailed biological survey of the coast of California."[31] Ritter did not make this statement without already having some thought as to how such a survey would be set into operation.

> Such a survey would of necessity comprehend the investigation, not merely of the life of the area, but as well of the physical conditions under which it exists. It would have to be hydrographic as well as biological.[32]

Ritter's formula for accomplishing such an enterprise was the product of his view of science and how it should be practiced, the topic of the next two chapters.

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[31]Ritter, "A Summer's Dredging," p. 55.

[32]Ibid.

Table 1.1—Advantages and Disadvantages of the Possible Locations Considered for the Location of the San Diego Marine Biological Laboratory

San Pedro

For	Against
1. Accessibility to Ocean	1. Growing Commercial Importance of the Harbour
2. Accessibility to Rich Collecting Grounds	2. Implementations of Harbor Improvements

Coronado*

For	Against
1. Accessibility	1. Difficulty of Getting Good Ocean Water
2. Living Facilities	2. Poor Quality of Bay Water
3. Safety for Small Boats	3. Difficulty of Landing Large Boats in Ocean or Bay
4. Convenience for Mud-flat Collecting	4. Remoteness of Rocky Collecting Grounds
5. Bay Plankton	5. Remoteness from Oceanic Conditions

La Jolla*

1. Accessibility to Oceanic Conditions, Deep Water, Plankton, etc.	1. Unsafe for Boats, Small and Large
2. Good Ocean Water for Aquaria	2. Distance from Mud-flat Collecting
3. Rocky Collecting Grounds	
4. Accessibility	
5. Living Facilities	

*Taken from William Emerson Ritter, "The Marine Biological Station of San Diego: Its History, Present Conditions, Achievements, and Aims," *Univ. Calif. Publ. Zool.*, March, 1912, *9*: Appendix E.

CHAPTER 2

THE ORGANISMIC IDEAS OF WILLIAM EMERSON RITTER

The establishment of the Marine Biological Station of San Diego was described in the first chapter. This description illustrated some of the difficulties encountered in the undertaking and the ways in which those difficulties were resolved. It also illustrated the importance of organizing a group of individuals in order to achieve a specific goal. This enterprise required the expertise of many from a variety of backgrounds; however, one cannot fail to notice the importance of Ritter to this project. Fred Baker (1854-1938), an associate of Ritter's who attended to many of the details of setting up the San Diego Marine Biological Laboratory, made this observation in a tribute to Ritter:

> I believe . . . that few institutions of this character have grown up which more fully express the ideas and ideals of one man than does this one. Rarely assertive, always happily goodnatured, ready at all times to listen to others and to defer to their judgement, nevertheless, he was the dominating force which drove us all to the goal which he had set.[1]

Ritter was instrumental in encouraging the establishment and development of the Marine Biological Station of San Diego. His motivation came from his personal philosophies

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[1]Fred Baker, "Dr. Ritter and the Founding of the Scripps Institution of Oceanography," Bull. Scripps Inst. Oceanogr., January, 1928, 15: 11.

about science and how it should be practiced, and about how a marine station should be organized in order to meet the goals of science. With this in mind, the next two chapters will be devoted to a discussion of just such issues. The present chapter will focus upon what Ritter believed to be the role of science and upon his "organismalist" philosophy. The following chapter will examine Ritter's ecological ideas, and his thoughts about what should be the role of marine biological stations.

Francis B. Sumner believed that Ritter's time was consumed by his interest in exploring the role of science in society and the ways in which science ought to be practiced, and that it was in relation to these explorations that Ritter was chiefly known.

> These morphological and taxonomic studies gave place rather early to behavioristic and philosophical ones, and it is probably these last by which Ritter is more largely known. His stature as a biological philosopher can probably be fairly measured only by another philosopher. His writings reveal extensive acquaintance both with the literature of philosophy and of the history of science, such an acquaintance as I believe few living zoologists possess. And they reveal preoccupation with certain philosophical problems throughout much of his life.[2]

Sumner was not alone in his belief that much of Ritter's life was consumed by his preoccupation with philosophical biology. Baker commented on Ritter's "extensive flights into abstruse philosophical biology," and William Wallace

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[2]Francis B. Sumner, "William Emerson Ritter: Naturalist and Philosopher," Science, April, 1944, 99: 336.

Campbell (1862-1938), the president of the University of California at Berkeley from 1922 to 1930, also made reference to Ritter's interest in issues outside the day-to-day practice of biology.[3] Many of Ritter's peers believed that Ritter had a deep interest in philosophical biology. But what exactly were his philosophical concerns? More specifically, what were Ritter's views regarding the role of science in society and regarding how science should be practiced in order to produce the most useful knowledge?

Ritter saw science as being of value in two ways. First, science provided information that promoted progress.

> The communal functions of science on the material side are sufficiently recognized in what is known as Modern Civilization. The incalculable worth of "applied science," commonly so-called, for human life under this type of culture is questioned to only a negligible extent. There is no need of either exposition or apologetic on behalf of this aspect of science.[4]

Second, science had the potential of contributing to "the higher, the spiritual life of man."[5] Ritter was dedicated to the idea that science should contribute to improving humankind.

My belief is that science must justify its right

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[3]Baker, "Dr Ritter," p. 11; William Wallace Campbell, "Acceptance of the Portrait of Dr. Ritter," Bull. Scripps Inst. Oceanogr., January, 1928, 15: 14.

[4]William Emerson Ritter, "The Marine Biological Station of San Diego: Its History, Present Conditions, Achievements, and Aims," Univ. Calif. Publ. Zool., March, 1912, 9: 227.

[5]Ibid.

> to live and flourish, not alone in its ministrations to physical well-being, but also to the higher and highest reaches of man's nature. While I do not for a moment subscribe to the view held by a few, that science is everything, that by-and-by it will supplant religion, philosophy, ethics, art, and the rest, I am fully persuaded that as civilization advances, it must become ever more and more an underpinning and ally of all these.[6]

Ritter was surprised that science was so highly valued as a physical utility, and that "all along the line, educators, publicists, clergymen, politicians, journalists, and, surprisingly, scientific men themselves, [appeared] to take it for granted that the office of science [was] primarily to minister to man's bodily needs, and secondarily to sharpen his wits."[7] Science provided a sphere in which the imagination could be exercised, the result being that "the study of nature, rightly pursued, not only expands and strengthens man's intellect, but enriches his emotional life and contributes to his higher enjoyment."[8] Ritter believed that science, like religion or philosophy, contained doctrines that could help a person improve his or her self, and that could enrich a person's life.

The biological doctrines which Ritter believed could play an important role in contributing to the improvement

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[6]<u>Ibid</u>., pp. 227-228.

[7]<u>Ibid</u>., p. 227.

[8]William Emerson Ritter, "What the Scripps Institution is Trying to Do," <u>Bull</u>. <u>Scripps</u> <u>Inst</u>. <u>Biol</u>. <u>Res</u>. <u>Univ</u>. <u>Calif</u>., 1916, <u>1</u>: 23.

of humankind, both intellectually and spiritually, were doctrines such as "all life from preceding life", evolution, the struggle for survival, "the reign of law", the organism as a whole, etc.[9] To illustrate, Ritter believed that the doctrine of heredity could be of great value to society if only it was understood better by each individual member of society.

> _The laws of heredity_, particularly those discovered by Mendel, have been tested to such an extent as to make them of positive moment to human life. The eugenics idea, started in England by Francis Galton, aims at a practical application of the known principles of inheritance to the good of the human race. In view of the wide theoretic interest that may come from their application to the propagation of man himself, the intelligent, thoughtful members of the community could undoubtedly be far better instructed than they are. Not only the possibilities but the limitations of eugenics as a practical programme ought to be and might be presented in a simple, readable language.[10]

Ritter entertained the idea that the information found in various biological doctrines could contribute to improving a person's intellectual and spiritual nature; however, Ritter felt that much of this information was not readily available to the population at large. He accused the scientific community of publishing information that was virtually inaccessible to the general public.

> Finally, without a doubt, _innumerable bald, unphilosophized facts of living nature that would_

[9] Ritter, "The Marine Biological Station of San Diego," pp. 228-231.

[10] _Ibid._, p. 231.

> <u>entertain and instruct, and consequently keenly interest thousands upon thousands of generally intelligent persons, are buried in the technical language of biological narration and description beyond the possibility of extraction for such purposes except at the hands of biologists themselves.</u>[11]

In light of this short-coming, Ritter proposed various ways in which scientific information might be made accessible to all persons. First, Ritter suggested that the information found in various biological doctrines could be disseminated to the general population by making the language of the biologist understandable to the population at large. Ritter conjectured that biologists were creative enough to write and publish information that could be grasped by laymen. He stated that "the fact that many biologists develop splendidly the talent for graphic art in response to the need of illustrating the organisms and organs with which they deal, is proof positive that the art instinct is not wanting in them."[12] Ritter believed that if biologists were to tap into this "art instinct", then they could write and publish papers that could be understood by interested readers who were not a professional biologists.

Second, Ritter proposed that the ideas in biological science that had the potential of contributing to man's higher intellect and spirit could be disseminated to the general public via formal instruction by a professional

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[11] <u>Ibid</u>.

[12] <u>Ibid</u>., p. 232.

biologist at "the botanical garden, the zoological garden, the natural history museum, the aquarium, the library, [and] the lecture platform."[13] He believed that this biologist should be from a professional institution of research.

> Here comes not only the opportunity but the obligation of those whose vocation is in research institutions. The university teacher may generally be considered to have done his share when in addition to his research work he has instructed his regular classes. Those, on the other hand, whose lots are cast in institutions of research, being relieved of the round of duties incident to the university professorship, would seem to be marked as the ones to use such instruments of general education as are most suitable for reaching the great public outside the schools and colleges.[14]

Ritter wanted to see researchers from institutions provide material to the general public, but he was careful to point out that he did not believe that these researchers should be engaged exclusively in "popular writing or lecturing or arranging collections or the like."[15]

That Ritter wanted to see researchers from institutions present scientific material to the public is related to his conviction that one of the tasks of scientific institutions was to present current scientific beliefs to the public.

My view is that these institutions, _as_

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[13] _Ibid_., p. 233.

[14] _Ibid_., pp. 233-234.

[15] _Ibid_., p. 234.

> _institutions_, ought to hold themselves obliged, from time to time, to give out in a form readily accessible to and comprehensible by the rank and file, the results of their most significant achievements. Indeed, I am willing to go a step farther and say that such institutions might well be held to something of the sort by their boards of administration. I am persuaded that such a course would be, in the long run, not only not obstructive but actually promotive, of the work of investigation itself.[16]

Ritter believed that science was of great value to society as a whole, because it provided technology and the comforts inherent in some of those technologies, and because it was capable of providing information that could improve each individual's spiritual and intellectual capacities. Ritter surmised that the key to providing society with scientific information lay in presenting scientific results in a language that the general population could understand and in providing better education. He saw science as being important in a social way, but how did he feel it should be pursued so that it might furnish the most valuable information? Ritter's support of an "organismalist" concept can answer this question.

There is agreement among historians and philosophers of science that during the late nineteenth and early twentieth centuries biology underwent a change. Biologists began to look for common themes that could be used to unite all of biology, and they embraced a new methodology to

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[16] _Ibid_.

discover those themes. They moved away from descriptive, taxonomic work, and they entered the laboratory to pursue experimental work. This is reflected in the work of Kohlstedt and Benson, whose historical studies have collectively shown that biological research moved out of the museum and into the university laboratory. An important facet of this movement concerns the nature of the experimental work which was utilized. William Coleman in Biology in the Nineteenth Century: Problems of Form, Function and Transformation stated:

> In barest terms experimentation was simply a matter of manipulative procedures. It was but one method, and was called upon to become the preponderant method for biology. Most experimentalists, despite the public glory of their procedure, were not free from metaphysical commitments. In the physiological departments of German universities and institutes, where the means and impulse towards experimental work was uncommonly great, mechanism and materialism were common goods. These usually assumed the form of reductionism, whereby vital processes could be "reduced" to physics and chemistry and definite conceptual content ascribed or implied for these presumably more fundamental sciences.[17]

The experimental work which inhabited the laboratories of biologists in the late nineteenth and early twentieth centuries was mechanistic, and explanations using chemical and physical terms were commonplace. Garland Allen in Life Science stated that "both the methods and subject matter that characterize twentieth-century biology were strongly

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[17]William Coleman, Biology in the Nineteenth Century: Problems of Form, Function and Transformation (New York: John Wiley and Sons, Inc., 1971), p. 13.

influenced by developments in the physical sciences (i.e., physics and chemistry) during the last half of the nineteenth and first part of the twentieth centuries," and claimed that biologists of the late nineteenth and early twentieth centuries "continually looked to physicists and chemists for models of how scientific investigations should be carried out."[18]

An important part of the shift from description to experimentation was the way in which the organism was viewed.

> They saw the organism as a mechanism, a complex machine whose workings man could unravel with the tools of physics and chemistry. Their approach was reductionist in that it sought to take the organism apart and study its parts in isolation: to reduce the whole to its component parts. Their approach was physico-chemical in that the methods of measurement and analysis they employed were borrowed directly from the physics and chemistry laboratories. They were experimental in that they sought to test their hypotheses with living systems in which they studied only one variable at a time.[19]

This mechanistic, or elemental, view of the organism entailed the ideas that any phenomenon was best understood by understanding the individual parts of that phenomenon, and that the whole was nothing more than the sum of the parts.[20] This view was a part of a materialistic view of

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[18]Garland Allen, Life Science in the Twentieth Century (New York: John Wiley and Sons, Inc., 1975), p. xv.

[19]Ibid., p. xvi.

[20]Ibid., p. xxi.

reality which was adopted in the nineteenth century.[21]

Mechanistic materialism was not the only type of materialism which was promoted in the late nineteenth century. Standing in opposition to mechanistic materialism was holistic materialism, or organicism. Organicism upheld the idea that to understand any phenomenon, one had to understand not only the individual parts, but also how those parts interacted.[22] Organismic biology espoused ideals very similar to those advanced by another movement known as vitalism. Morton Beckner has pointed these similarities out.

> They both agree that the methods of the physical sciences are applicable to the study of organisms but insist that these methods cannot tell the whole story; they agree that the "form" of the single whole organism is in some sense a factor in embryological development, animal behavior, reproduction, and physiology; and they both insist on the propriety of a teleological point of view.[23]

While vitalism and organismic biology did display these similarities, there was one fundamental way in which they differed. "Vitalism [was] a biological form of idealism," and embraced the idea that a nonphysical entity controlled

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[21] Ibid., p. xix.

[22] Ibid., pp. xxi-xxii.

[23] Morton Beckner, "Organismic Biology," in Paul Edwards, ed., The Encyclopedia of Philosophy, 5 (New York: Macmillan Company and the Free Press, 1967), p. 549.

the organic activity of an organism.[24] Organicism rejected the existence of this entity; therefore, it fit into the category of materialism.[25] This difference, according to Beckner, was not clearly delineated until the twentieth century.

> The affinity between vitalism and organismic biology is more than an accident. In the history of biology it is difficult to disentangle vitalistic and organismic strands, since both schools are concerned with the same sorts of problems and speak the same sort of language. The distinction between them was drawn clearly only in the twentieth century. Organismic biology may be described as and attempt to achieve the aims of the murky organismic-vitalistic tradition, without appeal to vital entities.[26]

The fact that this major difference between vitalism and organicism was not defined until the twentieth century may explain why, in the late nineteenth century, the antagonist to mechanism was most often noted as being vitalism. Organicism was overlooked, because it was not seen as being fundamentally different from vitalism. In spite of the fact that organicism was not a widely recognized approach to studying the organism, it was still alive and active. Donna Jeanne Haraway, in _Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology_, has discussed the history of development of

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[24]_Ibid._; Allen, _Life Science in the Twentieth Century_, p. xxii.

[25]Beckner, "Organismic Biology," p. 549.

[26]_Ibid._

organicism. In her discussion, Ritter's name appears.

> Several lineages have been proposed for organicism in recent biology. The American zoologist working at the Scripps Institute of California, W. E. Ritter, concentrates on French and American contributions to the perspective (1919). Beckner cites Ritter as the first modern organicist, noting his introduction of the term organismalism to represent the idea that "the organism in its totality is as essential to an explanation of its elements as its elements are to an explanation of the organism."[27]

Beckner, whom Haraway cites, states that "the term 'organismalism' was coined by the zoologist W. E. Ritter in 1919" to describe the theory that understanding the organism in its complete form was as important as understanding the parts of the organism. Information from each type of investigation was of value to the other.[28] Beckner and Haraway both see Ritter as fitting into and even being a leader of the organismic tradition. Whether he was a leader in this tradition or not is not of paramount importance here, but the ideas that he held with respect to this tradition are.

Ritter began writing about organicism as early as 1905. In an article entitled "A General Statement of the Ideas and the Present Aims and Status of the Marine Biological Association of San Diego," Ritter put forth the idea that an organism could only be understood by looking

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[27]Donna Jeanne Haraway, Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology (New Haven: Yale University Press, 1976), p. 34.

[28]Beckner, "Organismic Biology," p. 549.

at it in the context of its environment.

> We are becoming ever more impressed as knowledge advances, with the truth that no segment of the phenomena presented by an animal, morphological or physiological, is fully understood until it is regarded in the light of the entire life career of the animal. We are likewise in position to see as never before what must be done to attain this fullness of knowledge. We must, in the first place, learn by observation all the facts of the life-history of the animal. In the second place we must make use at every point possible of a combination of observation and experimentation for the interpretation of these facts.[29]

Ritter was convinced that the behavior of an isolated part of an organism could not be completely understood until that isolated part was put into the context of the whole organism, and the organism was put into the context of its environment.

Ritter continued to write about organicism, and in 1919 published the two volume work, The Unity of the Organism, or The Organismal Conception of Life. Sumner described it as being Ritter's "outstanding published work in the field of biological philosophy."[30] In these volumes, Ritter not only presented his ideas about organicism, but also criticized the elementalist approach.

> Stated more specifically the task now before us is that of examining closely and systematically the interdependences among the parts of the individual organism. Although these interdependences are

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[29] William Emerson Ritter, "A General Statement of the Ideas and the Present Aims and Status of the Marine Biological Association of San Diego," Univ. Calif. Publ. Zool., April, 1905, 2: xvi.

[30] Sumner, "William Emerson Ritter," p. 336.

> among the most obvious and general of all organic phenomena such an examination of them biology has not yet made systematically. Indeed—and here is one of the most vital things for us to see—a cardinal charge against the elementalist standpoint is that in its very nature it not only does not encourage, it actually stands against such examination. Its opposition to comprehensiveness and systematization is profound and essential.[31]

Ritter concluded that the elementalist, by adopting an interest in studying only individual elements, and by ignoring the idea that those individual parts were interdependent, stood in opposition to broad categories, including the laws of nature.

> The truth is—and it is of great importance since it influences reaches far beyond the confines of scientific technicalities—any scientist, especially any biologist, who is through and through an elementalist, is necessarily a protestant against all law except the law of elements.
>
> The scientific elementalist is inevitably anarchistic toward all the most common, most objective, structures and laws of nature.
>
> His faith is in the laws of the obscure or invisible world and against those of the everywhere visible world. Atoms are more real to his mind than are lands and waters, plants and animals.[32]

Ritter wanted the laws and the concepts that biology postulated to contribute to a person's larger understanding of both himself and the surrounding world. The biologist who failed to uphold the laws of nature consequently failed

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[31]William Emerson Ritter, <u>The Unity of the Organism, or the Organismal Conception of Life</u>, <u>II</u> (Boston: Richard G. Badger, The Gorham Press, 1919), pp. 93-94.

[32]<u>Ibid</u>., pp. 159-160.

to contribute to a person's understanding of his or her self.

Ritter's alternative to elementalism was organicism.

> Adhering to the mode of expression previously used in characterizing the two points of view, the central idea which we shall try to establish may be put as follows: The organism in its totality is as essential to an explanation of it elements as its elements are to an explanation of the organism.[33]

Ritter contended that the study of the organism in its entirety entailed not only examining and defining the actions of all of the parts of the organism, but also involved discovering how those parts interacted. Complete knowledge of an organism came from understanding how independent parts interacted and functioned in relation to the overall organism. Ritter clarified this point in a discussion of the nervous system.

> Perhaps enough illustration and general discussion have been presented to convince the reader not only that "the nervous system functions as a whole" but that this functioning is strictly subservient to the needs of the organism as a whole, whether the normal individual, living normally, or the normal individual, living under special stress, be considered. It is hoped the reader will not have failed, despite the brevity and inadequacy of the presentation, to perceive the fundamental truth that the organism's totality of activities, executed to so large an extent through the agency of the neural mechanism, are in turn subordinate to the needs of the organism as related to its natural environment. . . .
>
> Every specific act of every part of the nervous system is primarily in the interest of

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[33]William Emerson Ritter, _The Unity of the Organism of the Organismal Conception of Life_, _I_ (Boston: Richard G. Badger, The Gorham Press, 1919), p. 24.

some other part and function of the organism than itself.[34]

The preceding description describes one of the fundamental assumptions of organicism, that being organic unity. Organismic biologists agree that "organic systems are so organized that the activities of the whole cannot be understood as the sum of the activities of the parts."[35] Morton Beckner maintains that studies of how the molecules within a honeybee interact can not answer the question of why that bee will sting its drones to death.[36] This type of question can only be understood by looking at the organism in its entirety.

> First, it is impossible to resolve the phenomena of life completely into elementary units; for each individual part and each individual event depends not only on conditions within itself, but also to a greater or lesser extent on the conditions within the whole, or within subordinate units of which it is a part.[37]

The behavior of parts is determined at different levels. An isolated cell behaves in a very specific way, but when that cell is introduced into a tissue, its behavior differs markedly. In turn, when that tissue is put into the organ the behavior of the cell is again significantly modified.

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[34]Ritter, The Unity of the Organism, Vol. II, pp. 183-184.

[35]Beckner, "Organismic Biology," p. 549.

[36]Ibid., p. 550.

[37]Ludwig von Bertalanffy, Problems of Life: An Evaluation of Modern Biological Thought (New York: John Wiley and Sons, Inc., 1952), p. 12.

As Beckner puts it, it is "a general thesis of organismic biology, that the laws which determine the behavior of the parts at a certain level of organization are silent about some aspects of the behavior of the parts at higher levels."[38] Ludwig von Bertalanffy (1901-1972), an Austrian biologist and a strong supporter of organicism, would agree, contending that a blastomere behaves differently when it is isolated from an embryo as opposed to when it is an integral part of that embryo.[39] Bertalanffy discussed this point with reference to the actions of the nervous system.

> The reflexes of an isolated part of the spinal cord are not the same as the performances of these parts in the intact nervous system. Many reflexes can be demonstrated clearly only in the isolated spinal cord, whereas in the intact animal the influence of higher centers and the brain alters them decidedly.[40]

Ultimately, the behavior of an isolated part differs dramatically from the behavior of that part within the whole organism; therefore, a comprehensive understanding of the organism comes from not only understanding the individual parts of that organism, but also from understanding the interactions of those parts in the entire organism.

Ritter's ideas about the organismic concept and its

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[38]Beckner, "Organismic Biology," p. 550.

[39]Bertalanffy, Problems of Life, p. 12.

[40]Ibid.

importance remained in works that followed his book, The Unity of the Organism. He continued to criticize the "many modern biologists who [restricted] the term biology to knowledge gained from experimentation, and who [sought] only to explain all organic phenomena in terms of physics and chemistry" in the book The Natural History of Our Conduct, which he wrote in 1927.[41] In his 1938 book, The California Woodpecker and I: A Study in Comparative Zoology, Ritter stated that to understand the woodpecker, he had to "aim to know the bird not only in its wholeness, but also in all its parts."[42] It is important to understand that Ritter did not reject detailed experimentation on individual parts of an organism; rather, he felt that the information garnered from these experiments had to be used in conjunction with information about how the organism functioned in its entirety in order to gather a complete understanding of the organism.

Ritter, when discussing organismic biology, not only considered some of the aspects of the organismic concept, but also devoted some thought to the question of why it was not more readily accepted by the scientists of his day. His major answer was that most scientists, as

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[41]William Emerson Ritter, The Natural History of Our Conduct (New York: Harcourt, Brace and Company, 1927), p. 6.

[42]William Emerson Ritter, The California Woodpecker and I (Berkeley: University of California Press, 1938), p. 150.

elementalists, saw the organism in its complete form as being something not yet analyzed.

> We may preface a slight expansion of our dogmatic formula by asking the question, "How is it that the principle, embodied in such phrases as the 'Organism as a Whole' so confidently used by eminent investigators, should be so distrusted by most biologists as to give it little influence on biological conceptions?" The proximate reply is that for most biologists the notion is too vague and general to be of high and permanent worth. One statement of this depreciatory estimate is that to take the organism in its entirety is to take it unanalyzed; and this, so such a view holds, is superficial and contrary to the whole purpose and spirit of modern research.[43]

The elementalist viewed biological science as being scientific only if the phenomenon to be studied was broken down into its constituent parts and investigated through experimentation. Ritter stated that "scientific knowledge in biology, as in all other fields, is analytic knowledge; and conversely, analytic knowledge not only is science, but (at least says full-fledged elementalism) is the whole of science."[44] Ritter did not wholly dispute this idea, but did maintain that analytic knowledge was not exclusive.

> Stated positively, while assuming as science always does assume, the validity of analytic knowledge of nature, we shall contend that synthetic knowledge of nature is not only valid also, but that it is as fundamental and essential a part of science as is analytic knowledge.[45]

According to Ritter, the elementalist rejected organicism,

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[43]Ritter, The Unity of the Organism, Vol. I, p. 25.

[44]Ibid.

[45]Ibid.

because organicism, in failing to examine individual parts and explain their actions, failed to analyze the organism. This may be true, but this estimation of why elementalists rejected organicism does not tell the entire story.

Elementalists did see organisms in their entirety as being entities not yet analyzed, but only because their ideas about what constituted biology centered upon performing experimental studies and manipulating systems in an effort to discover basic laws and theories. Jaques Loeb (1859-1924), a German-born physiologist who practiced his science in the United States, was a leading proponent of the mechanistic or elemental viewpoint. He believed that the purpose of biology was to "analyze life from a purely physico-chemical viewpoint."[46] Loeb was not interested in discovering facts about organisms and how they existed, but was interested in using "manipulable simple systems as an approach to generalizing biological laws."[47] An example of this can be seen in Loeb's physico-chemical explanation of fertilization.

> I succeeded in showing that the spermatozoön causes the development of the sea-urchin egg in a way similar to that in my method of artificial parthenogenesis; namely, by carrying two substances into the egg, one of which acts like

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[46]Jaques Loeb, The Mechanistic Conception of Life (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1964), p. 3.

[47]Seymour S. Cohen, "Some Struggles of Jaques Loeb, Albert Mathews, and Ernest Just at the Marine Biological Laboratory," Biol. Bull., June, 1985, 168: 127.

> the butyric acid and induces the membrane formation, while the other acts like the treatment with a hypertonic solution and enables the full development of the larvae.[48]

Loeb's interest in manipulating systems went beyond just searching for general laws. Philip J. Pauly insists that Loeb was interested in controlling organisms.

> In the early 1890s Loeb sought to define a biology centered around the control of organisms. Such a goal required a redefinition of the nature of the biologist's work. Experimentation gained significance beyond its ordinary function of providing determinate answers to definite problems within a hypothetico-deductive schema. The activity of experimentation took on value in itself, and experiments became demonstrations of the manipulative powers of biologists. Loeb's work involved a series of pilot projects in biological manipulation, which would indicate the feasibility and desirability of broader efforts in this direction. Furthermore, focus on control of life led to devaluation of a number of traditional biological problems. Loeb sought to avoid such problems as evolution, the nature of life, the causes of biological organization, and the value and limits of explanation of biological phenomena in terms of physicochemical concepts. These "metaphysical concern—especially anxiety about philosophical consistency—were distractions from the central aim of control.[49]

Loeb, like many biologists of his time was struggling to define biology and make it a legitimate science. He attempted to do this by making biology a physical science that had unquestionable laws and theories. To create this physical science, he performed experiments on and

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[48] Loeb, The Mechanistic Conception of Life, p. 14.

[49] Philip J. Pauly, Controlling Life: Jaques Loeb and the Engineering Ideal in Biology (New York: Oxford University Press, 1987), p. 5.

manipulated isolated systems of organisms. He then explained the results using physical and chemical terms. Loeb's notion about what the aims of biological research should be was very much different than that of Ritter. To Loeb, the goal of biological research was to discover the fundamental laws and theories basic to biology. In contrast, Ritter believed that the goal of biological research was to garner an understanding of the organism as it existed in nature.

Ritter believed that in spite of the criticisms that elementalists had for organismic biology, organismic biology was gaining recognition within the biological community.

> As the conception of the organisms as a unified whole forces its way into the biological sciences, the terms organismal or organismic gain ground. The elemental conception, in the sense that parts as we can know them as independent entities are wholly explanatory of their wholes, has proved its inadequacy in every subdivision of biology where basic problems are under investigation. This conception must be supplemented by an hypothesis which recognizes that living bodies are as real and potent in influencing the inorganic elements which they take into themselves, as these elements are in influencing the bodies which they enter. The elemental hypothesis must be supplemented by the organismal hypothesis.[50]

He believed that the time was coming when elementalists and organismicists would not be opposed, but would work together. He felt that "an adequate general concept of a

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[50]William Emerson Ritter, "The Organismal Conception," Univ. Calif. Publ. Zool., 1928, 31: 308.

living being must include the two subconcepts of a very special kind of whole and its very specially interrelated parts."[51] Ritter contended that this union could be achieved by uniting "the older . . . less exact natural history aspects of science into closer more vital cooperation with its newer experimental and more quantitatively exact aspects."[52]

Ritter's view of organismic biology stressed the importance of understanding how the individual parts of and organism functioned in relation to the whole organism. He wanted experimentalists to work with naturalists to achieve this understanding, because the experimentalist had a talent for looking at minute details while the naturalist had a talent for looking at the broader picture of how the organism fit into its environment. Ritter was optimistic that elementalism and organicism might someday be united through the combination of natural history and experimentation; however, his aims were never fully realized because his position on organicism was never fully appreciated. Ritter's organismic ideas were less widely accepted than mechanistic ideas, a fact which acted to alienate Ritter.

> Dr. Ritter had for years been evolving a system of biological philosophy, the central tenet of which was that the actual unit to be reckoned with by

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[51] _Ibid_.

[52] Ritter, _The Unity of the Organism_, Vol II, p. 212.

> the biologist should be the organism itself, rather than its component cells, genes, molecules, or what not. He insisted that the whole is just as essential in accounting for the activity of the parts as the parts are necessary in accounting for the activity of the whole. Ritter's contributions in the field of biological philosophy, though not wholly new, were real ones, and they drew the respectful attention of a number of prominent thinkers in this field. But appreciative readers appear to have formed a small minority, and the launching of the "organismal" view of life created no great stir in the biological world. Unfortunately, Ritter was always disposed, it seems to me, to magnify the differences between his own views and those held by the majority of others. The others have too often seemed to him to be worshipping false gods.[53]

Ritter's ideas may not have been widely accepted by his contemporaries; nonetheless, they did fit in well with the work that he wanted to pursue at the San Diego Marine Biological Station. Ritter was preoccupied with gathering an understanding of how organisms were distributed in and retained their position in their environment, a goal unlike that of his contemporaries, who wanted to develop sound laws and theories that would unify biology. Ritter wanted to achieve this goal by combining the talents of experimentalists and naturalists. It was an approach that he viewed as being successful in the work of ecologists.

> Specifically stated, work of the type long prosecuted by exploring expeditions, botanical and zoological gardens, museums, botanical, zoological and biological societies, and government biological surveys; and that of laboratories in the strict modern sense, the morphological,

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[53]Francis B. Sumner, _The Life History of an American Naturalist_ (Lancaster: The Jaques Cattell Press, 1945), pp. 207-208.

> physiological, and bio-chemical laboratories, must join hands more closely and effectively than they have heretofore to insure continued progress in the organic sciences. Several movements of the day in biology could be mentioned whose meaning, viewed from our standpoint, can hardly be mistaken. Perhaps the most conspicuous of these is that congeries of research activities known as ecology. In spite of frequent depreciative comments about ecology, especially because of its indefiniteness as to both content and delimitation, it has the merit—from our standpoint the very great merit—of facing organic nature as it actually is, that is, of having for its subject matter the modes of life of organisms as nature presents them, and hence of recognizing the laboratory as an agency, but only as one among other agencies, for dealing with its subject. As to method, while ecology recognizes the indispensability of the laboratory and experimentation in the narrow sense, it refuses to let such experimentation usurp the whole of its interest and effort.[54]

Ritter was essentially interested in studies of an ecological nature. This leads to the next chapter, which is devoted an examination of Ritter's ideas about ecology and about the role of marine biological stations.

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[54] Ibid.

CHAPTER 3

THE ECOLOGICAL IDEAS OF WILLIAM EMERSON RITTER

An increasing interest in the historical development of ecology in the United States has been occurring among historians of science. Frank Egerton has reviewed much of the work done to date on "the general history of ecology, terrestrial plant ecology, marine ecology, limnology, population ecology . . . animal ecology," and applied ecology in the United States and Canada.[1] Although there appears to be a vast amount of literature in the area, Joel Hagen maintains that there are still numerous questions to be asked and a number of areas to be explored.

> The development of ecology, the scope and nature of the field, and the relationships between plant and animal ecology during this period are virtually uncharted waters in the history of biology. It is not even clear whether during this period plant and animal ecology ought to be considered parts of a common discipline.[2]

In spite of this perceived gap in studies of the history of ecology, active research is being pursued by professional historians of science. An aspect of the history of ecology which has been diligently explored by historians of science

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[1]Frank N. Egerton, "The History of Ecology: Achievements and Opportunities, Part One," J. Hist. Biol., Summer, 1983, 16: 259.

[2]Joel B. Hagen, "Experimental Taxonomy, 1930-1950: The Impact of Cytology, Ecology, and Genetics on Ideas of Biological Classification," (Ph.D. dissertation, Oregon State University, 1982), notes, pp. 45-46.

is the development of ecology in relation to the discipline of botany. This may be related to the fact that ecology arose as a specialization within botany; thus, the study of ecology from this angle has proved to be the most fruitful. Hagen suggests that ecology is "unique in the degree to which it was shaped by botanical problems and ideas."[3] He points out that those individuals involved in the formative years of ecology were botanists, i.e., Eugenius Warming (1841-1924) of Denmark, Andreas Schimper (1856-1901) of Germany, Alfred Tansley (1871-1951) of Great Britain, Henry Cowles (1869-1939) of the United States and Frederic Clements of the United States, and that early ecological concepts such as community and succession had a distinct botanical flavor.[4] Eugene Cittadino indicates that "the word 'ecology' was often used interchangeably with 'plant ecology' or 'ecological plant geography,'" and that "ecology was first recognized and consciously pursued during the 1890s as a specialization within botany."[5] According to these historians, botany played a significant role in the development of ecology. Although botany may

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[3]Joel B. Hagen, "Organism and Environment: Frederic Clements's Vision of a Unified Physiological Ecology," in Ronald Rainger, Keith Benson and Jane Maienschein, eds., The American Development of Biology (Philadelphia: University of Pennsylvania Press, 1988), p. 6.

[4]Ibid.

[5]Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890-1905," Stud. Hist. Biol., 1980, 4: 172-173.

have had a significant impact on the development of ecology, the question of how other areas of biology impacted upon the development of ecology remains intriguing. The question pertaining to how the development of ecology in United States in the late nineteenth and early twentieth centuries related to the development of the San Diego Marine Biological Station in the early twentieth century is particularly interesting, because of its importance to this thesis. With this in mind, this chapter will be devoted to an examination of Ritter's thoughts on ecology, and of how they compared to the ideas that prevailed among those biologists who referred to themselves as ecologists. In addition, the ecological ideas of other researchers at San Diego will be presented periodically, so that they may be compared to Ritter's ideas.

Ritter wanted researchers at the San Diego Marine Biological Station to carry out "a detailed biological survey of the coast of California."[6] He did not encourage this undertaking without having considered how it would be executed.

> So the future marine station, particularly the California station—must be planned for chemical, physical, and hydrographic as well as for strictly biological research. It must have boats fitted with a great variety of apparatus. For the all-important experimental researches it must have aquaria that will reproduce in miniature and in a

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[6]William Emerson Ritter, "A Summer's Dredging on the Coast of Southern California," Science, January, 1902, 15: 55.

> form easy to control the conditions of nature as far as possible. And over all, of course, there must be men. The work must go on every hour of the day, and every day of the year.[7]

Ellis L. Michael (1881-1920), who accomplished significant work in the field of chaetognath biology while at the San Diego Marine Biological Station, also articulated this idea.

> Necessarily, this programme demands thorough investigation of the conditions under which marine organisms live. Knowledge of the environment is as indispensible to a complete understanding of marine organisms as is that of the organisms themselves. "Conditions of the water as to temperature and currents; mineral, gaseous, and albuminoid content, etc., must be known _at the particular time and place to which the biological studies pertain_." (Ritter, 1905, p. ix.) Chemistry, physics, and hydrography are therefore as indispensable in understanding any marine organism as is morphology, embryology, or physiology. Some biologists, however, hesitate to admit this, not recognizing that their attitude is equivalent to claiming that a marine organism can be completely understood without taking into account its most characteristic quality—its _marineness_ so to speak.[8]

An important component of this survey involved studying the distribution of organisms in relation to physical factors. It was an ambition that resembled the aspirations of early plant ecologists.

During the late nineteenth century, several botanists

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[7] William Emerson Ritter, "Marine Biology and the Marine Laboratory at San Pedro," _Univ. Calif. Chronicle_, 1902, _5_: 226.

[8] Ellis L. Michael, "Dependence of Marine Biology upon Hydrography and Necessity of Quantitative Biological Research," _Univ. Calif. Publ. Zool._, 1916, _15_: ii.

began to promote ecology as a unique area in the science of physiology, claiming that it was the logical outcome of "attempts to understand the functional relationships between plants and the physical and biotic conditions of their habitats."[9] An essential feature of their movement was that of amplifying the role of physical factors in determining the distribution of particular organisms and the structure of distinct biological communities. This differed from the older tradition of taxonomic plant geography, which entailed describing the distribution of a species in an area and explaining that distribution in historical and evolutionary terms.[10] Hagen states:

> Prior to about 1900 biogeography was primarily a descriptive activity closely related to taxonomy. Traditionally, biogeographers focused on describing the distributional patterns of species, genera, and higher taxa. Plant geographers also compiled floras, or lists of regional species. And they provided explanations for the distributional pattern, which after Darwin often meant providing historical analyses of the origin, dispersal, and extinction of species.[11]

Nineteenth-century taxonomic plant geographers patterned their studies after the work of Charles Darwin (1809-

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[9]Cittadino, "Ecology and the Professionalization of Botany," p. 194.

[10]Joel B. Hagen, "Ecologists and Taxonomists: Divergent Traditions in Twentieth-Century Plant Geography," J. Hist. Biol., Summer, 1986, 19: 204.

[11]Ibid., p. 197.

1882).[12] An important consequence of this was that they neglected to consider the role of abiotic factors in determining how plants "originated, spread, and [became] grouped into floras."[13] This is consistent with a view held by William Coleman, who points out that "Darwin moved the emphasis of his argument toward biotic factors and away from abiotic factors in evolutionary change" when he began to discuss the problem of divergence.[14]

> In 1856 Darwin found a solution to perhaps the decisive evolutionary problem—divergence, the production of two or more new species from a preexisting single stock. Adaptation, when effective, tuned an old species to new conditions; divergence assured the production of multiple new species. Divergence was Darwin's answer to the pressing question of evolutionary innovation. What is ecologically noteworthy in this advance is that Darwin's eye and mind now largely abandoned the abiotic environment as an interesting (that is to say, participatory) feature of the evolutionary process.[15]

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[12] Although nineteenth-century taxonomic plant geographers tended to pattern their work after Darwin's work, it should not be concluded that taxonomic plant geography was new, or that Darwin's work served to create this field. Janet Browne in _The Secular Ark: Studies in the History of Biogeography_ (New Haven: Yale University Press, 1983) has pointed out that the history of biogeography dates back to the seventeenth century, when attempts were made to describe the geographic distribution of plants and animals in order to support the notion of Noah's Ark.

[13] Hagen, "Ecologists and Taxonomists," p. 202.

[14] William Coleman, "Evolution into Ecology? The Strategy of Warming's Ecological Plant Geography," _J. Hist. Biol._, Summer, 1986, _19_: 182.

[15] _Ibid._

Darwin's approach to examining divergence and, consequently, the taxonomic plant geographer's approach to examining plant distribution differed from the early ecologist's approach to studying plant distribution. Early ecologists believed that abiotic factors were instrumental in determining why a plant flourished in a particular area, and were responsible for defining the overall structure of a plant community. An interesting result of the early ecologist's obsession with abiotic factors was that they worked outside the definition of ecology coined by Ernst Haeckel (1834-1919) in 1866. Haeckel's definition embraced the idea that biotic factors played a far greater role than abiotic factors in influencing organisms.

> With regard to the character of the conditions of existence, these are for each separate species extremely complex and in most cases are quite insufficiently or even altogether unknown. Where earlier we spoke of the conditions of existence, we considered especially inorganic conditions, factors such as the influence of light, heat, moisture and inorganic nutrients. Far more important than these, however, and exerting a much more powerful influence on the transformation and adaptation of species are organic conditions of existence, that is, the mutual relations of organisms to one another . . . That the interactions between all neighboring organisms are extraordinarily important and that they exert far more influence on the change and adaptation of species than do the inorganic conditions of existence was first emphasized with due clarity by Darwin. Unfortunately, these very complicated relationships between organisms remain for the most part unknown, and this because there exists a huge and interesting as well as important area of future inquiry. Ecology or the theory of the economy of nature, this being a division of physiology and one which our textbooks have totally ignored, when seen from this perspective

> promises to reward us with splendid and surprising discoveries.[16]

Ritter was concerned with discovering and describing the organisms found in the Pacific adjacent to the California coast, and with determining the way in which they were distributed in their environment. Inherent in this interest was a preoccupation with the problem of adaptation.

> Must I look to environment or the constitution of the creatures, or to both for answers? The very fact that I ask the questions almost compels me to look to both. If I knew for a <u>certainty</u> that the full answer lay in either direction alone, I should quite surely know the answer <u>itself</u>, so should be under no necessity of asking the questions. Well then, if my questions are serious and I have gumption enough to seek the answers where obviously they must be sought, it will be necessary to go at the constitution of the animals more searchingly than before, and also at the environment. In other words, I have run with full force into the problem of <u>organic</u> <u>adaptation</u>.[17]

Ritter believed that "ecology . . . in its very essence, [was] to a large extent the problem of adaptation."[18] The early ecologists agreed. Henry Chandler Cowles (1869-1939), a leading ecologist, stated that "if ecology [had] a place at all in the modern biology, certainly one of its

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[16] Ernst Haeckel, <u>Generelle</u> <u>Morphologie</u> <u>der</u> <u>Organismen</u> <u>II</u> (Berlin: Georg Reimer, 1866), p. 236.

[17] William Emerson Ritter, "The Scientific Work of the San Diego Marine Biological Station During the Year 1908," <u>Science</u>, September, 1908, <u>28</u>: 330.

[18] William Emerson Ritter, "The Marine Biological Station of San Diego: Its History, Present Conditions, Achievements, and Aims," <u>Univ</u>. <u>Calif</u>. <u>Publ</u>. <u>Zool</u>., March, 1912, <u>9</u>: 203.

great tasks [was] to unravel the mysteries of adaptation."[19]

Early ecologists attempted "to unravel the mysteries of adaptation" using a physiological approach. They sought to explain plant distribution and abundance on the basis of the physiological capabilities of plants to adjust to certain environments.[20] Hagen points out:

> The new ecological plant geography was to focus on communities rather than on species, on proximate environmental causes rather than on historical explanations, and on physiological experiments rather than upon morphological descriptions.[21]

Ecological plant geographers rejected the descriptive nature of taxonomic plant geography, and reacted to the fact that taxonomic plant geographers used an historical and evolutionary approach to studying adaptation. Plant taxonomists described species and their distribution using the Darwinian idea of adaptation via natural selection.[22] Early ecologists attempted to define communities and determine their structure on a physiological basis.

Thus far, the similarities that existed between the early ecologists and Ritter have been pointed out; however,

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[19]Henry C. Cowles, "The Work in the Year 1903 in Ecology," Science, June, 1904, 19: 880.

[20]James P. Collins, John Beatty and Jane Maienschein, "Introduction: Between Ecology and Evolutionary Biology," J. Hist. Biol., Summer, 1986, 19: 170.

[21]Hagen, "Ecologists and Taxonomists," p. 213.

[22]Collins, et. al., "Introduction," p. 170.

there existed ways in which the ideas of Ritter and the early ecologists diverged. An obvious way in which their ideas differed was in terms of what they deemed as being interesting organisms to study. Ritter was interested in studying marine organisms, while the early ecologists were interested in examining terrestrial plants.

Another way in which the two disagreed was with regard to the specific questions they were concerned with. Early ecologists wanted to understand the physiological basis for plant distribution, so that they might understand community structure.

> An ecological approach to plant distribution supposedly provided a better explanation of community structure. According to the physiological approach, communities of plants in different parts of a country, or even the world, might look quite similar (have a similar physiognomy) if they grew in comparable environments. Although having different evolutionary histories, species of plants in a community would look alike because environments with similar biotic and abiotic attributes would favor plants with comparable physiological capacities.[23]

Ritter, in contrast, showed no interest in understanding community structure; rather, he was intent upon gathering information about the organisms off the coast of southern California and the way in which they were distributed.

> For example, the species representing a given pelagic group having been got well in hand, a natural second step would be the determination of the seasonal distribution of the group, since the study of the collections for the taxonomy would

[23] Ibid., pp. 170-171.

> surely bring together, incidentally, considerable data on this problem. Following close upon the treatment of seasonal distribution would come that of horizontal and vertical distribution, the chorology; and inseparably linked with these would be the problems of food and reproduction; and these again would lead to problems of migration, with their intimate dependence upon temperature and other environmental factors.[24]

The most important way in which Ritter and the early ecologists diverged was with respect to how they approached their research. Ritter wanted researchers at the San Diego station to discover and describe the flora and fauna of the Pacific adjacent to the California coast, and to determine the distribution of those organisms with respect to physical factors. In keeping with this ideal, he sought to combine the talents of specialists from a variety of fields. He contended that specialized studies coordinated toward a final goal were vital to the growth of biology and to the success of his station.

> Even allowing the conception to be right does not the proposal to embody it in an institution of research mean certain failure from the simple fact that it runs counter to the principle of specialization, the principle which has been the king-pin of progress in all recent science? On the face of the matter it looks that way. In truth, though, violence to the principle is neither intended nor done. Quite to the contrary, specialization even more refined and intense than ever, is compelled at some points. The only unusual thing is that the program calls for specialization _in more directions_ than is customary for one and the same institution; and

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[24]William Emerson Ritter, "A General Statement of the Ideas and the Present Aims and Status of the Marine Biological Association of San Diego," _Univ. Calif. Publ. Zool._, April, 1905, _2_: viii.

> that it gives this specialization <u>organic coordination</u> in greater measure than biological research has usually had.[25]

Ritter surmised that the only way to approach and answer the broad questions of a biological survey was by combining specialists from various disciplines.

> The long and short of all this is that it is impossible for me to handle the problems of species, distribution and adaptation . . . with any large measure of success unless I can have the cooperation, not haphazard and incidental, but designed and sure, of specialists in several branches of science.[26]

Michael also recognized the importance of combining specialists from a variety of fields in order to meet the demands of the biological survey.

> During the past fifteen years the Scripps Institution and its forerunner the San Diego Marine Biological Association, have been making a biological and hydrographical survey of the waters adjacent to the coast of Southern California. Intensive rather than extensive research in marine biology is the leading idea of this survey, and, although this involves the acquisition of detailed information concerning particular marine organisms, knowledge of the biology of the sea is the ever-present ideal. How is this ideal to be approached? Certainly not by isolated investigations prosecuted under the Institution's auspices by biologists sojourning in Southern California, nor by investigations made for the purpose of advancing any general biological theory. <u>Continuous and co-ordinative research of several highly trained specialists is the first essential</u>. Under certain conditions the Institution is not only glad but eager to give visiting naturalists opportunity for prosecuting their own researches, and special phases of general biology are studied from time to time. It

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[25] Ritter, "The Scientific Work," p. 329.

[26] *Ibid*., p. 331.

> is realized, however, that the ideal sought can be approached only by a programme of research which involves highly specialized and intimately co-ordinated investigations on particular and restricted problems concerning the structure, development, function, behavior, etc, of particular species of marine organisms. But even specialized investigations can be included in the Institution's marine programme only when subordinated to the larger problem of understanding the sum total of the phenomena of marine plants and animals.[27]

Michael did not accept the idea that the San Diego Marine Biological Station should exist as a facility where researchers could gather periodically to pursue individual investigations, nor did he feel that the research pursued should be done to advance general biological principles. He wanted the station to pursue a specialized research program that was concerned with a long-term study of the biology off the coast of southern California.

Charles Atwood Kofoid agreed with Ritter and Michael that the goals of the San Diego Marine Biological Laboratory could be achieved only by co-ordinating the research efforts of individual researchers at the station.

> As a counterbalance and complement to the principle of uncorrelated individual specialization which has thus far animated much of the work done in most of the marine stations (excepting, of course, the promising achievements in this direction of the International Commission for the Investigation of the Sea) we find in the program of the San Diego Station an effort to develope the principle of organic coordination in

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[27]Michael, "Dependence of Marine Biology upon Hydrography," pp. i-ii.

specialization in marine research.[28]

Kofoid was optimistic that stations like San Diego, which sought to combine the talents of researchers from various fields, were coming into existence. He stated that

> a new type of station is now in the process of evolution, one, moreover, which is no longer merely a biological station, but rather a station equipped for the solution of biological problems with the aid of all pertinent sciences. The causal analysis of the problems surrounding a living organism in its environment calls for exact and thorough knowledge of both the animal and its environing factors, and necessitates the aid of chemical, physical, hydrographical, and meteorological research in close correlation with the biological and subordinate to it. The biological station of the future is thus coming to be a marine or fresh-water observatory with a broader base and wider scope of action.[29]

Like Ritter, Michael and Kofoid, the early ecologists wanted to specialize; however, they were not interested in uniting specialists from a variety of fields. They wanted to create a specialized field that utilized a physiological approach to studying organisms. Ritter was critical of the specialized research that the ecologists aspired to. He accused researchers at various marine stations of being too specialized and of using organisms only to study very particular problems. The consequence of this was that oftentimes the researcher paid little or no attention to

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[28]Charles Atwood Kofoid, "The San Diego Marine Station," Internat. Revue ges. Hydrobiol. Hydrog., 1908, 1: 864.

[29]Charles Atwood Kofoid, "The Biological Stations of Europe," Bull. U. S. Bureau of Education, 1910, 440: 2.

the organism itself.

> The latter makes use of animals and plants that live in the sea in general biological researches. That these organisms happen to be marine is an incident merely. The investigator turns away from them without hesitation when others, from whatever source, come to hand that suit his purpose better. Further, the user of marine organisms in such investigations is quite indifferent to everything concerning them that does not bear upon his particular problem. He puts aside the marine animal after it has served his purpose without having even noticed, perhaps, the major part of its traits and qualities and the questions concerning it.[30]

Michael also discussed the general biologist's attitude towards the organism. After complimenting researchers at the Marine Biological Laboratory (MBL) at Woods Hole with having completed wonderful work that promoted American biology, Michael criticized them for overlooking the intrinsic value of marine organisms. He maintained that they used marine organisms only to shed light on general principles, and that "the [investigators] with such aims [made] no attempt to understand marine organisms as such."[31] Michael further supported this argument by using examples from the Naples Zoological Station.

> Perhaps the difference in point of view will be more clearly revealed by considering the aims and work of the Naples Zoological Station, the world's most renowned marine laboratory. Here the lines of investigation are distinctly twofold. Like the

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[30]Ritter, "A General Statement," p. ii.

[31]Michael, "Dependence of Marine Biology upon Hydrography," p. v.

> policy of the Woods Hole Laboratory, that of the Naples Station facilitates research in the widest sense, and no effort is made by the Station to influence the lines of investigation of those who occupy the "research tables." The investigator is permitted to pursue any sort of research he chooses. . . . Interest is centered in the general theoretical bearing of the structures and functions investigated, and not in the organisms to which they pertain. In so far as the Naples Station has occupied itself with studies of this type it has been devoted to researches in general biology which only incidentally contribute to marine biology.[32]

Michael saw research at other stations, in particular, at the MBL and at Naples as being different from investigations undertaken by researchers at the San Diego Marine Biological Laboratory. The research at San Diego was directed toward making a biological survey of the waters off the coast of southern California: discovering how an organism fit into its environment was the focus of the survey. As for stations like Woods Hole and Naples, the organism was used as a tool to discover and support general theories.

Ritter's criticism of specialization relates, of course, to his criticism of elementalism. Ritter believed that specialized studies, while supporting general biological ideas were not gathering any information about the organism as it existed in its environment. Ritter's organismic concept entailed achieving just this. The specialized studies of researchers were not directed toward

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[32] _Ibid_., pp. v-vii.

this aim, but were directed toward the elementalist's aim of understanding how isolated parts functioned.

Ritter and the early ecologists believed that the answers to the questions they were asking came out of specialized studies centered in physiology and morphology; however, they disagreed about the nature of those specialized studies. Associated with this disagreement was the way in which Ritter and the early ecologists valued description and experimentation.

Cittadino proposes that the interest by American botanists in ecological problems in the 1890s reflected their desire to legitimize their field. Botanists, in an attempt to legitimize their field, sought to abandon studies that involved description and classification in favor of studies that were dedicated to discovering process and function. They turned to ecology, which appeared to be more experimental and less descriptive.[33] Hagen states:

> At the turn of the century a group of botanists self-consciously defined a new area of botanical research. These ecologists defined their new discipline in opposition to what they believed was a moribund, nineteenth century natural history tradition.[34]

Cittadino and Hagen agree that a shift occurred from description to experimentation; however, they do not agree about the cause of that change. As previously mentioned,

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[33]Cittadino, "Ecology and the Professionalization of Botany," p. 174.

[34]Hagen, "Ecologists and Taxonomists," p. 213.

Cittadino maintains that the movement from description to experimentation was the result of a need botanists felt for legitimizing their field. Hagen, in contrast, feels that by reintroducing the idea that abiotic factors accounted for a plant's distribution, ecologists automatically moved away from description toward experimentation.[35] In either case, ecologists viewed description as being insignificant, and this in spite of the fact that it continued to play a valuable part in their work. Ecologists not only described the plant they were working with and the area from which that plant was taken, but also described the distinct units of vegetation that they called communities. The early ecologists relied on description, but failed to acknowledge its contribution to their research. Ritter did not fall into the same trap. He admitted the importance of combining methodologies, asserting that it led to a fuller understanding of the problem under investigation. Ritter never discussed the idea that some methodologies were less scientific than others.

While maintaining that experimentation was essential to biological inquiry, Ritter simultaneously asserted that description was also imperative. This was especially true in relation to the work that he wanted to carry out at the San Diego Marine Biological Station.

The view to which one is irresistibly led in

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[35] Ibid., p. 198.

> carrying forward an enterprise like ours is that both field observation and laboratory experimentation are wholly indispensable, since each furnishes ways of entrance into the problems presented that the other cannot possibly furnish. There is no more ground for holding either the one or the other as _the_ method, as being the more important or more promising, than there is for holding either the father or the mother to be more promising in the begetting of offspring among the higher animals.[36]

Ritter contended that all methods of investigation were of equal importance in discovering facts, and that all methodologies that could be used should be used if they applied to the question at hand.

> One method leads to knowledge of one sort, another to knowledge of another sort, generally speaking. Apparently the question of greater importance of one method as against another could arise only as a sequel to a judgement already reached that one kind of knowledge is more important than another. If the object of biological research is held to be "to know, to understand organic things" (Ritter, 1908), if a particular biological undertaking has the end in view of getting as much knowledge as possible about the organisms in a restricted area of the earth, there can be no partiality shown for one method over another. Each and every known method will be invoked as far as practicable and prized without stint for the particular thing it can do.[37]

He believed in a "danger that lurks in overconfidence in any _single_ _method_ of research," and posited that the only way to gain knowledge about a particular problem was by combining several methods of investigation.[38]

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[36] Ritter, "The Marine Biological Station of San Diego," p. 211.

[37] _Ibid_., p. 212.

[38] _Ibid_., p. 214.

Michael also advanced these ideas. Michael, like Ritter, saw experimentation as being vital to the marine biologist, but he did not see it as wielding power that could usurp field observation.

> Laboratory experiment and field observation must go hand in hand. The former cannot, except by inference, ascertain the manner in which a species is related to its environmental complex. The latter cannot, except by inference, ascertain the nature of response involved in correlations observed between marine organisms (or any other kind of organisms) and their environments. . . . Assuredly, both sorts of investigation are required in order to approach, even remotely, complete knowledge of the behavior of any species.[39]

Michael believed that both lines of investigation were needed to expose an accurate picture of an organism as it existed in nature. No one line of investigation could alone provide answers to questions deemed important. As an example, the experimentalist could not apply conclusions drawn from his experiments to organisms in nature. For one thing, experiments performed in the laboratory "reveal[ed] only what transpire[d] in a laboratory and [were] necessarily incapable of revealing what occur[red] in nature". And for another thing, the complex environment from which an organism was taken could not be duplicated within the laboratory.

> Certain stimuli occur in nature which are necessarily absent in the laboratory, and others are probably introduced in the laboratory which do

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[39]Michael, "Dependence of Marine Biology upon Hydrography," p. xv.

> not occur in nature. This is particularly true with respect to the ocean. How, for instance, could the stimuli associated with depth, distance from the coast, velocity of current, or wave action be duplicated? Moreover, the nature of an animal's response depends upon the duration as well as upon the intensity of any particular stimulus, and upon other preceding and attendant stimuli. This fact makes it obvious that laboratory experiments can offer no reliable evidence concerning an animal's behavior in nature.[40]

Michael's criticisms of the shortcomings of experimentation are logical, especially in light of his argument about the difficulty of duplicating the natural environment in a laboratory setting; however, his statement that experiments provided no reliable evidence about how an organism might behave in its natural setting is not entirely valid. Experimentation is indeed capable of providing insights into the natural world, even though those insights may not be reflections of the exact workings of things.

In addition to the problem of either neglecting or duplicating environmental factors in experimental researches, Michael maintained that there was a problem associated with applying the results of studies on a few individuals to larger populations of organisms.

> Even if the natural environment could be duplicated, another insuperable difficulty confronts the laboratory experimentalist. He is compelled to restrict his experiments to a few individuals; but he always applies his conclusions to races, varieties, species, or some other similar group of organisms. How can he be sure that the behavior of the individuals selected is

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[40]Ibid.

typical of that of the group as a whole?[41] Michael contended that the behavior of a few could not be inferred to be the typical behavior of a larger group, unless the researcher knew the complete environmental details of the area from which the individuals under experimental observation were taken, and knew how the group of organisms from which the individuals were taken was distributed with respect to environmental variations.[42] Regardless of these shortcomings of relying upon experimentation alone, it can be seen that Michael applied them only to marine biological investigations in which the scientist wanted to understand the behavior of an organism.

Ritter asked questions that were ecological, in that they were concerned with how organisms were distributed within and adapted to their environments; however, he differed from ecologists in the way in which he approached answering these questions. As to defining where Ritter's ecological ideas came from, they came from a combination of the early movement in ecology and of what Ritter perceived to be a lack of marine biological studies in the field of marine biology.

Ritter was familiar with the movement of ecology, and, in fact, praised ecology as being a field which was imposing a change upon the way in which biology was being

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[41]<u>Ibid</u>., p. xiii.

[42]<u>Ibid</u>., p. xii.

viewed.

> The primers of science teach that biology is the science of Living Matter, thereby giving the minds even of school children a bias away from the patent, out-of-door facts of the living world, toward those elements of organic beings which can be seen only in the laboratory and by the aid of magnifying glasses. Not animals and plants themselves, but their germ cells, and chemical compounds, and minute activities have absorbed well nigh all the interest of a whole generation of students. But a change is impending. Indeed such manifestations as that of the new subdivision of biology called ecology, now bodied forth in this country by the Ecological Society of America; and the Departments of Botanical Research and of Marine Biology of the Carnegie Institution of Washington, the first represented on our program today by Dr. McDougal, are, rightly interpreted, evidence that the change is already well under way. These and other facts that could be mentioned indicate a growing recognition that though immeasurably rich be the rewards of delving into the minutiae of organic structure and function, a complete interpretation of life can never be reached by travelling in this direction alone.[43]

Ritter spoke passionately about the rise of ecology, but as to whether or not he interacted with any of the leading botanical ecologists of his time cannot be ascertained from his published articles. The majority of Ritter's comments concerning ecology were reserved for various researchers at the San Diego Marine Biological Station, and the projects they were carrying out. Ritter often used the term ecology in relation to the work of E. L. Michael. Ritter praised Michael's work as being "the most advanced point yet

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[43]William Emerson Ritter, "What the Scripps Institution is Trying to Do," _Bull_. _Scripps_ _Inst_. _Biol_. _Res_. _Univ_. _Calif_., December, 1916, _1_: 22.

reached on the ecological side of the station programme."[44] Upon Michael's death in 1920, Ritter wrote about Michael's contribution to ecological studies at San Diego and to ecology in general.

> At the time of publication of this and other papers by Michael on the movements of chaetognaths as influenced by physical conditions of the sea, ecology was less clearly defined and less widely cultivated as such than it has since become; and its basal conceptions had hardly been applied at all to marine life, even though that life had been so long and so fruitfully investigated ecologically—as we may now say. My impression now is that had Michael's plankton studies identified themselves earlier and more closely with the ecological development in this country, they would have attracted more attention than they so far have. So confident was Mr. Michael that the "plankton program" of the Scripps Institution, in which he himself was playing a major part, was developing ideas and methods of first rate importance, that his disappointment at the slight attention which his papers received was rather keen. Hence it was that the recent evidences of interest in his work shown by the officers and members of the Ecological Society were doubly gratifying to him.[45]

Ritter was convinced that Michael was a leader in marine ecology, and stated that Michael's "going [left] a breach in the staff of the institution and in its program of marine ecology that [would] not be easy to fill."[46] The extent to which Michael was involved in studies of an ecological nature will be examined in detail in the

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[44]Ritter, "The Marine Biological Station of San Diego," p. 194.

[45]William Emerson Ritter, "Ellis L. Michael and his Scientific Work," Ecology, 1921, 2: 70-71.

[46]Ibid., p. 70.

following chapter.

Ritter was familiar with the field of ecology; however, a more important factor which influenced his ecological way of thinking was his frustration with the lack of investigations that sought to understand organisms as they existed in nature. Ritter was extremely critical of the way in which marine science was generally being practiced, and these feelings were apparent in his early articles. He published an article in 1902 which detailed the dredging work carried out on the coast of southern California in that same year. The article contained not only Ritter's ideas about how work along the Pacific coast of North America might be approached, but also his conviction that investigations concerned with discovering the life of the area, as well as the physical conditions under which it existed, were inadequate.[47] His impression that studies pertaining to the life of the oceans were scant was further articulated in 1905.

> The meagerness of knowledge, not only of the fauna and flora, but also of the oceanography of the eastern part of the North Pacific can hardly be realized except by the few specialists whose studies have led them into immediate contact with it. . . . Our information about the most general facts concerning the currents, for instance, is wholly inadequate to constitute a foundation for investigations on distribution of organisms. And as to zoology, there are whole groups of prime importance for any of the wider questions of marine biology, like the dinoflagellata, the radiolaria, and the chaetognatha, about which

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[47]Ritter, "A Summer's Dredging," p. 55.

> there is hardly a recorded observation. Even the better studied groups, like the fishes, the mollusks, and the crustaceans, when ecologically regarded have been hardly more than glanced at.[48]

Ritter contended that, in general, marine science was failing to discover aspects about the types of organisms found in an area, how they lived, how they were distributed, etc. While he did acknowledge that particular geographical regions "like the Mediterranean, the North and Baltic Seas, the environs of the British Islands, and to a less extent, the North American half of the Atlantic" had been studied, he also pointed out that within these regions only small areas were truly well known.

> Let one go to the Bay of Naples, for instance, perhaps the best cultivated locality, and make an inquiry about the ecology of the most familiar species found there, and see how far from satisfactory an answer can be obtained.[49]

Ritter's criticism of marine stations, both of the way in which they pursued their research and of the actual research they pursued did not apply to all stations. Michael recognized this fact, maintaining that there were several stations working toward research similar to that of the San Diego Marine Biological Station.

> With few noteworthy exceptions, among the foremost of which stands the Port Erin Marine Biological Station, it is not the marine biological stations but enterprises such as the Monaco Institute of Oceanography, the great oceanographic expeditions, the fisheries laboratories like the United States

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[48]Ritter, " A General Statement," p. xiv.

[49]*Ibid*., p. xv.

> Fisheries Stations at Woods Hole, Massachusetts, and at Beaufort, North Carolina, and the International Commission for the Investigation of the Sea, that have contributed most to the science of marine biology.[50]

Michael was careful to point out that while these stations did pursue marine biological investigations similar to that of San Diego, some of them, particularly the Port Erin Station and the International Commission for the Scientific Investigation of the Sea, were primarily interested in pursuing marine studies for the purpose of contributing to economic biology.[51]

As was indicated in the preceding quote Michael saw the Port Erin Station as being a leader among those stations that contributed to marine biological research. He believed that "the Scripps Institution . . . followed, in general, the lead of the Port Erin Station, but [differed] perhaps most conspicuously . . . in that its primary interest [was] in pure rather than economic biology."[52] Michael may have been correct in his assessment of the similarity of the Marine Biological Station of San Diego to the Port Erin Station, but he overemphasized the idea that the station was involved in pursuing research for purely economic reasons. Although

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[50]Michael, "Dependence of Marine Biology upon Hydrography," p. viii.

[51]*Ibid*.

[52]*Ibid*., p. xx.

the Port Erin Station did open in 1892 to serve the fisheries, "its affiliation with the fisheries interests [was] not such as to absorb it funds and the time of its staff, the Piel laboratory and the fisheries laboratory at Liverpool serving these interests."[53] The station was free to pursue other things such as intensive studies of the plankton of the region. In addition, the area, seen as being largely varied, was an interesting area of study for any researcher interested in marine biology.

> The Port Erin station by reason of the purity of the water and richness and great variety of the fauna offers unusual attractions to anyone wishing to do experimental or observational work on living animals, or to carry on developmental or cultural studies.[54]

If Michael had wanted to discuss a station which pursued research similar to that of the San Diego Marine Biological Laboratory, but for purely economic reasons, he should have looked to the Laboratory of the Marine Biological Association at Plymouth, in Plymouth, England. This station's opening in 1888 was seen by Gilbert C. Bourne (1861-1933), a biologist from Oxford, as marking "an epoch in English zoological science, just as the opening of the Stazione Zoologica at Naples . . . marked an epoch in German science."[55] Although Bourne perceived the Marine

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[53]Kofoid, "The Biological Stations of Europe," p. 176.

[54]Ibid., p. 184.

[55]Gilbert C. Bourne, "The Opening of the Marine Biological Laboratory at Plymouth," Nature, June 28, 38: 198.

Biological Laboratory at Plymouth and the Naples Zoological station as having had tremendous impact upon science in their respective countries, he did maintain that this is where the similarities ended.

> Such an institution as that at Plymouth challenges comparison with Dr. Dohrn's famous zoological station at Naples. But there is a remarkable difference between them. The Naples Station was founded for purely scientific objects: it does not profess to undertake investigations for the benefit of economic interests.[56]

The Marine Biological Laboratory at Plymouth epitomized the ability of a station to successfully combine economic, scientific, and popular interests. Bourne wrote:

> The Laboratory at Plymouth, which is now ready for work, is remarkable as being the first institution in this country designed purely for scientific research which has been originated and firmly established by the efforts of scientific men appealing to the generosity and confidence of wealthy individuals and corporations who desire the progress of knowledge for practical ends and the general good of the community.[57]

The researchers at Plymouth under the direction of Dr. E. J. Allen had "for their object the study of the seasonal changes which take place in the physical and biological conditions prevailing over the entire area covered by the international programme, though more particularly directed to a study of the waters entering the North Sea from different directions."[58] These studies were designed to

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[56] Ibid., p. 200.

[57] Ibid., p. 198.

[58] Kofoid, "The Biological Stations of Europe," p. 148.

achieve very specific goals.

> They are designed to determine (1) the origin, history, and physical and biological characters of the water found in each locality at different seasons of the year and at corresponding seasons in different years, changes which must necessarily have a profound influence upon the distribution and abundance of the fish life in the sea: and (2) the variations which take place in the floating and swimming organisms (plankton) which constitute the fundamental food supply of the sea.[59]

Becoming familiar with the plankton species of the area and with how the distribution of those species changed as environmental conditions, i.e., temperature and salinity, changed was the focus of the research at Plymouth. It resembled research done at San Diego; however, the investigations pursued at Plymouth were done to provide information for fisheries management.

> The Marine Biological Association receives an annual grant from the Treasury, on the express understanding that it shall conduct researches upon questions relating to the life-history and habits of food-fishes. It must not be supposed that this work is not scientific because it has a practical object in view. Science is not only the art of thinking correctly, but of observing and recording correctly, and correct observations and records of the life-history of our food fishes are just what are wanted at the present time.[60]

The San Diego Marine Biological Station did not pursue investigations for economic reasons.

The Monaco Institute of Oceanography was another Station which Michael saw as pursuing research similar to

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[59] Ibid.

[60] Bourne, "The Opening of the Marine Biological Laboratory," p. 200.

that of the San Diego Station. The Monaco Institute of Oceanography was built and endowed by S. A. S. Albert I^{er}, Prince of Monaco.[61] This institute consisted of the Oceanographical Institute in Paris and the Oceanographical Museum in Monaco, each under the same administrative council and advisory committee, but each run by separate and independent staffs.[62]

The purpose of the museum gradually changed between the year when it first opened, i.e., 1901, and the year when it formally opened, i.e., 1910. Although "originally planned . . . to hold the rapidly accumulating collections made by the Prince in his numerous cruises in the _Hirondelle_, and later in the _Princess Alice I_, and _Princess Alice II_," the museum eventually expanded to hold a "general collection of all marine life and an exhibition of the results of oceanographic research and the methods and apparatus employed in its prosecution."[63] This expansion also made provision "for laboratory researches by competent investigators in the fields included in the scope of the museum."[64] The Oceanographical Museum provided an opportunity for individual researchers to follow their own

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[61]Margaret Deacon, _Scientists and the Sea, 1650-1900, A Study of Marine Science_ (London: Academic Press, 1971), p. 382.

[62]Kofoid, "The Biological Stations of Europe," p. 37.

[63]_Ibid_., p. 38.

[64]_Ibid_.

personal lines of investigation.

> The station is open without charge to all competent investigators in biological sciences and oceanography. Application should be made in advance to the director, stating full details as to the line of investigation to be pursued, the time of arrival and departure, and a full list of apparatus and chemicals needed. The institution does not provide microscopes but furnishes free a microtome and all other necessaries for laboratory work. Investigators have the use of an ample supply of aquaria, may accompany the <u>Eider</u> on its collecting trips, have access to the library and collections, and, by special arrangement, have the use of museum material for research. There are no restrictions as to choice of subject for investigation or place and manner of publication. A half dozen furnished chambers are provided in the building for the use of visiting investigators. It is expected that a circular of information will be prepared for applicants, with full details regarding the conditions under which the laboratories may be used. The excellent facilities so freely offered at Monaco have been promptly utilized by a constantly increasing number of scientists. In 1908 more than twelve visiting investigators carried on researches in the laboratories of the museum, coming mainly from Germany, Switzerland, Russia, and Italy, and giving thus a distinctly international and cosmopolitan aspect to the clientele of the museum.[65]

That individual lines of research were pursued at Monaco illustrates a dissimilarity to the goals of the San Diego Marine Biological Laboratory; however, this type of research was not the only type of research that occurred at Monaco. The staff at Monaco were involved in their own program which dealt "with the local environmental conditions, with the plankton and hydrographical data, and

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[65] <u>Ibid</u>., p. 39.

with the distribution of the local fauna"[66] That investigations of this type were pursued is most likely due to the nature of funding at Monaco. The Monaco station, being supported by the Prince of Monaco, did not have to follow a direction dictated by economics, nor did it have to adopt the research interests of individual investigators who pursued research at the station; however, because the station was privately funded, the research pursued often reflected the interests of Prince Albert.

> He began his oceanographic work in 1885 in a schooner, the _Hirondelle_, but afterwards graduated to progressively more powerful steamers, the _Princess Alice I_, _Princess Alice II_, and _Hirondelle II_. His first work was to map the surface currents of the North Atlantic which he traced by releasing floats and plotting the course of those that were found and returned. In the larger ships he was able to work more extensively on the conditions and fauna of the deep sea. On one occasion, in 1895, he became involved in a whale hunt off the Azores and grew to be fascinated by the enormous animals and the almost unknown giant squid on which they fed.[67]

The Monaco station provided a place where individual researchers could pursue their own lines of investigation; however, their work was not the focus of the station. Because individual researchers using the facility did not actually contribute to its support, the station and its staff were not obliged to participate in their research. This freed the staff to pursue other investigations, which

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[66] _Ibid_., p. 46.

[67] Deacon, _Scientists and the Sea_, p. 382.

were quite often a reflection of the interests of Prince Albert of Monaco. Many of these investigations were centered upon gathering an acquaintance with the flora and fauna of the area and the environmental conditions under which those organisms lived. These goals were not unlike those of the San Diego Marine Station.

Another organization which Michael saw as contributing a great deal to the field of marine biology, albeit for economic reasons, was the United States Fish Commission at Woods Hole, Massachusetts. The Fish Commission was established in 1871 under the auspices of "ascertaining whether any . . . diminution in the number of the food-fishes of the coast and the lakes of the United States [had] taken place; and if so, to what causes the same [was] due; and also whether and what protective, prohibitory, or precautionary measures should be adopted."[68] Spencer Fullerton Baird (1823-1887) played a key role in the development of the United States Fish Commission, and served as its first director.

Baird had official and unofficial reasons for wanting to develop the organization. Officially, he wanted to develop the commission in order to determine the condition

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[68]Susan Schlee, The Edge of an Unfamiliar World: A History of Oceanography (New York: E. P. Dutton and Company, Inc., 1973), p. 67: quote from "Joint Resolution for the Protection and Preservation of the Food Fishes of the Coast of the United States," in Report on the Condition of the Sea Fisheries in 1871 and 1872, p. xi.

of the fisheries off the coasts of Rhode Island and Massachusetts.[69] Unofficially, Baird wanted to establish a fish commission so that he might "initiate a sustained ecological study of North American waters."[70]

> Europeans, Baird knew, had already begun to explore their coastal waters and were finding a fascinating variety of marine plants and animals. But in the United States only the Coast Survey's short cruises with Louis Agassiz and his pupils and the Navy's occasional multipurposed expeditions had produced any marine zoological collections, and these were at best haphazard. Baird wanted to create a permanent bureau which could provide marine biologists with the support and equipment they needed to systematically study the sea over a long period of time.[71]

When Baird was officially appointed United States Fish Commissioner in 1871, he immediately began seeking out assistants to help him realize the official aims of the commission. Each assistant chosen was given the task of defining and researching some problem that related to the fisheries. For example, Addison E. Verrill (1839-1936), a natural history professor from Yale, was given the job of discovering if "a decline in mollusk beds was responsible for the diminution of coastal fisheries."[72] The investigations of the first summer were run out of a vacant Light House Board building in Woods Hole, and with boats

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[69]Dean C. Allard, Jr., Spencer Fullerton Baird and the U. S. Fish Commission (New York: Arno Press, 1978), p. 76.

[70]Schlee, The Edge of an Unfamiliar World, pp. 67-68.

[71]Ibid.

[72]Allard, Spencer Fullerton Baird, p. 87.

borrowed from the Revenue Cutter Service. As the years passed, Baird moved the station from Maine to Connecticut in hopes of examining many different environments and of finding a suitable, permanent location for the fish commission. In 1881, the permanent location was determined to be Woods Hole.[73]

Upon establishing the permanent residence of the commission, Baird began expanding the commission's operations. He looked toward private funding, for he felt that the government, which had set up the commission, would be opposed to funding the station if it sought to pursue more basic scientific investigations. He was able to secure funds from private sources, and Congress "impressed by so tangible an expression of interest in marine zoology . . . responded with appropriations totalling $117,000 for a laboratory."[74]

Upon completion of the U. S. Fish Commission's new quarters came changes in the way in which studies were conducted. No longer was the commission strictly confined to studies related to the fisheries.

> Each summer, while the Albatross cruised in the waters off Hatteras or moved north to dredge and trawl off Cape Cod, a band of scientists and their graduate students came from universities throughout the East to occupy the Commission's new laboratory and dormitory at Woods Hole. There they spent the summer collecting shallow-water

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[73]Schlee, The Edge of an Unfamiliar World, p. 69.

[74]Ibid., p. 70.

> animals, studying the life cycles of fish or the diseases and parasites that affected them, and examining the embryological development of sea urchins or of other simple animals which was often easier than attempting the same study on a land animal. This was the sustained ecological study of the North America waters which Baird had hoped to see. The results of the Woods Hole Studies, some of which were a help to fishermen, were published in the Commission's reports, and the specimens themselves were either sent to join the National Museum's valuable collection or were kept by the scientists in part payment for their work at the laboratory.[75]

Schlee's description of the work at the Fish Commission sounds like a description of the work carried out at the San Diego Marine Biological Station; however, Schlee's description still hints at the fact that much of the work going on at the fish commission was done to support general biological principles. The statement, "examining the embryological development of sea urchins or of other simple animals which was often easier than attempting the same study on a land animal," illustrates the idea that many of the researchers at the fish commission were there to carry out studies in which the organism served only as a tool to support larger theories.

The International Council for the Investigation of the Sea was another organization which Michael saw as attempting investigations similar to that of the San Diego Marine Biological Station. This council was borne out of a series of conferences held in Stockholm, Sweden in June of

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[75] Ibid., p. 73.

1899 and in Christiania, Sweden in May of 1901. It was at the second conference in Christiania that the proposals from the Stockholm conference "were supplemented and put into their final form."[76]

> It was thereby shown that the research work might best be divided into two main divisions, of which the one had in view the physical conditions of the sea, the other the biological—more especially with regard to the animals most useful as human food. Naturally, it was seen from the beginning that the study of the physical conditions, of the chemical nature of the ocean waters, of the currents, etc. was of the greatest importance for the investigations of the problems connected with life, that on the other hand, the study of the floating organisms had particular worth for the solution of hydrographical problems, and consequently that a sharp line should never be drawn between these two main divisions, yet nevertheless, were it only from practical considerations, one was obliged to think of this division of labour on account of the wide extent of the sphere of work.[77]

The International Council pursued investigations similar to those of the San Diego Station; however, much like the Plymouth Station, this association pursued these investigations for purely economic reasons.

Another station which Michael believed pursued research similar to that of the San Diego Marine Biological Station, but which he did not mention in his 1915 article was the Naples Zoological Station. Founded in 1870 by

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[76] "Bureau of the International Council for the Study of the Sea. Report of Administration for the First Year: 22nd July 1902—21st July 1903." Int. Coun. Stud. Sea. Rapports, July 1902—July 1903, 1, p. II.

[77] Ibid.

Anton Dohrn (1840-1909), a student of Haeckel, the Naples station was the culmination of the "growing interest in exploring life at sea, Dohrn's own marine experiences, [Dohrn's] championing of Darwinism, and [Dohrn's] need to prove himself."[78] Although most of the researchers at Naples used sea organisms as tools in their developmental studies, some of the work completed concerned coming to understand more about the marine organisms themselves. It was a fact, after all, that one of the forces behind the development of the Naples Station was that of learning more about the life inhabiting the Gulf of Naples. Carmelo R. Tomas states:

> Studies in marine botany in the Gulf of Naples predate the founding of the Stazione Zoologica by nearly fifty years. The studies of Delle Chiaje (1823) and Costa (1838) describe species of the rich flora found in the waters of the kingdom of Naples. This flora and equally abundant and varied fauna were factors influencing Anton Dohrn in establishing the Naples Institute. Within four years of the opening of the Stazione Zoologica, marine botany research was begun in earnest.[79]

The research carried out at Naples centered upon algae, and focused upon determining the species inhabiting the area and upon defining their distribution.

> The earliest visitors (1873-1900) were almost exclusively German researchers encouraged by Anton Dohrn to visit and work at his station. Among these, J. Reinke, P. Falkenberg, G. Berthold, R.

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[78] Christiane Groeben, "Anton Dohrn—The Statesman of Darwinism," Biol. Bull., June, 1985, 168: 9.

[79] Carmelo R. Tomas, "Marine Botany and Ecology at Stazione Zoologica," Biol. Bull., June, 1985, 168: 168.

> Valiante, and C. Sauvageau were the first to extensively study the benthic algae of the Gulf. Armed with modern elements of taxonomy and physiology, these early workers (Reinke, 1878a,b; Falkenberg, 1879, 1901; Berthold, 1882a,b; Valiante, 1883; Sauvageau, 1892) established vital species lists as well as distribution in the Naples area. In addition, their observations on gametes of brown algae, cellular composition including ions, chromoplasts, vacuoles, and associated membranes further added to the general knowledge of algae.[80]

Michael acknowledged that this line of work was being pursued at Naples.

> But the Naples Station is also engaged in a second enterprise having for its object, as manifested in its magnificent monographic series Fauna and Flora, exhaustive knowledge of the fauna and flora of the Gulf of Naples and the Mediterranean Sea. These monographs, it is true, relate mainly, though not entirely, to the structure of the organisms but, in so far as they are taxonomic, they constitute the initial step toward understanding the organisms of the Gulf and of the Mediterranean.[81]

The Naples research was similar to that of the San Diego Marine Biological Laboratory, albeit, on a much smaller scale and at a much later time. By this time, i.e., 1915, researchers at San Diego were expanding their investigations to include learning more about how various organisms were adapted to their environments. Michael saw this as being the next logical step to be taken at Naples.

> The natural second step must be ecological, i.e., determination of how the various species are related to their environmental complexes. Then

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[80]Ibid.

[81]Michael, "Dependence of Marine Biology upon Hydrography," p. vii.

> would follow the more intensive studies of structure, function, and behavior required to understand how and why the organisms maintain these relations. . . . But however limited the actual achievements may be, the station is engaged in a programme of research having for its object complete understanding of the structure, function, and behavior of the organism by virtue of which they are adapted to the marine environments of the Gulf of Naples and the Mediterranean Sea. When this type of investigation shall have been carried beyond the preliminary taxonomic stage, the Naples Station will be engaged <u>strictly</u> in researches in marine biology.[82]

The Naples Zoological Station had begun to identify and describe marine species in the Bay of Naples; however, Michael believed that the Naples Station had to begin taking part in ecological, physiological, morphological and behavioral studies that examined how the species of the area both fit into and maintained their position in the environment.

The Naples Station did pursue research the was somewhat ecological, but this research was of lesser concern than research directed toward developing and supporting general biological theories. From its inception, the Naples Station was defined as a research station under private control. Because of this, it had distinct advantages.

> The Naples zoological station is a private institution, the property of its director, and is unique accordingly in its origin, support, and administration. The only restrictions upon the powers of the director are those under which the site in the public park was granted to the

[82] <u>Ibid</u>.

> station, insuring the use of the building solely for scientific purposes. The station is not officially attached to any other institution, educational, political, or economic, and has thus escaped the evils of bureaucratic control, and having a strong executive, it has not needed such supervision to insure its success. An annual report to the German minister of foreign affairs by the director is the only external obligation of the station.[83]

Individual researchers at the station were able to define their own investigations, since there were no educational, political or economic pressures upon them. This was also true of the departments that existed at the station. The department of zoology carried out work in systematics, morphology and embryology from a very early time. The department of comparative physiology completed work that had "profoundly important relations to human and comparative medicine."[84]

The Naples Zoological station was primarily concerned with studies in general biology, as opposed to studies in marine biology. I believe that Ritter was dismayed by this fact. More generally, he was disappointed that many stations claimed to be marine biological laboratories when, in fact, the researchers there did not pursue questions pertaining to marine biology. Ritter made a "distinction between _marine biology_, and _general biology prosecuted by_

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[83]Kofoid, "The Biological Stations of Europe," p. 13.

[84]_Ibid_., p. 12.

researches on marine organisms."[85] He articulated this point in more detail.

> The former has for its aim, in the large, the getting of as comprehensive an understanding as possible of the life of the sea. It, of course, presents itself under a great variety of secondary questions; but the sum total of the phenomena of marine plants and animals will never be lost sight of as its real aim. The latter makes use of animals and plants that live in the sea in general biological researches.[86]

E. L . Michael agreed with Ritter, and pointed out that in spite of the fact that there did exist marine stations that pursued investigations that were attune to marine biology, the majority of marine stations were dedicated to researches in general biology.

> The great majority of marine biological stations are devoted to general rather than marine biology and, of course, collect their material when, where, and how it may be best obtained without regard to the problems of marine biology. They rarely trouble themselves with questions concerning the seasonal, vertical, horizontal, or topographical distribution of any species of marine plant or animal. They frankly make no attempt to determine how and why variations in the distribution of organisms are correlated with fluctuations in light, temperature, salinity, gas-content, and other elements of their environments, or of how any species is ecologically related to any other species.[87]

Several stations existed to provide researchers with a locale where they could actively investigate their own

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[85]Ritter, "A General Statement," p. ii.

[86]Ibid.

[87]Michael, "Dependence of Marine Biology upon Hydrography," p. vii.

lines of research, and simultaneously exchange ideas and cooperate with other researchers in the hopes of constructing a cohesive science of biology. The MBL was just such a place.

> The sense of community was important at the MBL and for the emergence of productive lines of research in American biology generally. Researchers gathered in Woods Hole each summer and found a group of people with related concerns, thus allowing them to move beyond the research isolation which most felt at their home institutions. The lecture series reflects the shared interests by addressing overlapping problems: of epigenesis and preformation, of the significance of past evolution, of heredity, of fertilization, of cleavage, of the importance of physiological processes or of environment for directing development. Moving from relatively descriptive cytological work to some manipulative experimental studies in the 1890's, the MBL community sought to understand what happens in development and how differentiation and organization arise to become established.[88]

Specialized, independent researches with cooperation among researchers was the impetus behind the MBL, just as at the San Diego station; however, the individual investigations taken up by researchers at the MBL were significantly different than those pursued by researchers at the San Diego Marine Biological Laboratory, because the questions being asked were significantly different. Jane Maienschein maintains that the direction in which the research was heading is reflected in a series of biological lectures presented at the MBL throughout the 1890s.

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[88]Jane Maienschein, ed., Defining Biology: Lectures from the 1890s (Cambridge, Mass.: Harvard University Press, 1986), pp. xi-xii.

> The focus of interest of the Biological Lectures shifted from year to year as new discoveries brought new questions, but some themes underpinned discussion throughout the 1890's. Most notably, questions about the significance of heredity and evolution for development, and related questions about the significance of cell cleavage for differentiation of individuals, ran through many of the lectures. Initially, discussion centered on the question, to what extent is the egg cell already organized in its earliest stages? Is there something brought to the egg by heredity, something to some extent predelineated? Or does form and heterogeneity emerge only gradually or epigenetically in the course of time? All of the discussions directly impinge on the more general debates about preformation and epigenesis.[89]

Questions concerning development dominated the early work at the MBL. Charles Otis Whitman and William Keith Brooks both concentrated on developmental questions.

> By 1890, many MBL researchers had focused on the question of how the egg becomes fertilized and begins development. Specifically, a number of American researchers began to ask whether development follows a pattern which is predominantly inherited or which is acquired and hence emerges only gradually; that is, whether preformation or epigenesis predominates. In particular, Whitman focused on the question: to what extent does the egg cell already experience organization?[90]

These were not questions that researchers at San Diego were concerned with. The researchers at Ritter's station were intent upon determining the distribution of organisms and the factors, both abiotic and biotic, which accounted for that distribution. They did not concentrate upon studies of development and heredity, unless those studies were of

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[89] Ibid., p. 21.

[90] Ibid., p. 31.

some value in explaining the distribution of the organism in question. The investigators at Wood's Hole pursued research that answered questions pertaining to general biology, and the organism was used only as a vehicle for understanding patterns of development. Monroy and Groeben wrote:

> The university offered excellent teaching facilities, but here [at the MBL] were two things a university could not provide. One was a place where people could work under conditions of complete freedom, meaning that no demands should be placed on them. The second was the marine material which was proving increasingly to offer unique experimental opportunities.[91]

In spite of the fact that the organisms used most often for the research were marine, the researchers at Wood's Hole, like the researchers at Naples, were not interested in solving all of the problems of marine biology.

Ritter wanted to carry out a program of research that was somewhat ecological in nature. His desire to pursue studies of this sort were driven to lesser degree by an interest in highlighting the field of ecology than by an interest in pursuing studies that had not yet been pursued in the field of marine biology. An examination of the actual extent to which Ritter and the researchers at the San Diego Marine Biological Station were successful in their studies will be made in the next chapter.

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[91]Alberto Monroy and Christiane Groeben, "The 'New' Embryology at the Zoological Station and at the Marine Biological Laboratory," Biol. Bull., June, 1985, 168: 42.

CHAPTER 4

THE INVESTIGATIONS OF RESEARCHERS AT THE MARINE BIOLOGICAL STATION OF SAN DIEGO IN ITS EARLY YEARS

In the previous chapter, Ritter's ideas on ecology and their relation to his research goals were compared to the ideas and goals of early ecologists in order to assess their similarities and dissimilarities. It was shown that many of Ritter's goals were similar to those of the early ecologists, goals such as investigating how organisms were distributed in and adapted to their environments. In spite of these similarities, there did exist ways in which Ritter's ideas and goals differed from those of the early ecologists. Early ecologists leaned toward experimentation and specialization, while Ritter tended toward a combination of experimentation and description and toward general studies that combined the efforts of various specialists. It was also determined in the previous chapter that Ritter's ideas were influenced to a greater degree by his contention that the marine sciences were failing to carry out marine research than by his desire to support the ecological movement.

Ritter's ideas are interesting, but their importance can only be ascertained by examining if they were supported by researchers at the station. Research at the station, in its early years, included not only biological studies, but also hydrographic studies. The focus of these investigations will be examined in this chapter to determine if in fact they complemented Ritter's ecological ideas.

Many individuals were involved in research activities that led up to and beyond the establishment of the San Diego Marine Biological Station. As early as 1892, when the marine station was situated at Pacific Grove, there were a number of people involved in active investigations that centered upon collecting organisms for identification. These activities, which continued into 1893, resulted in the accumulation of a great deal of information.

> A summer's work at Pacific Grove, supplemented by numerous collecting and observation trips to various points on the coast both south and north of the Golden Gate, having given us a glimpse of biological conditions on this portion of the seashore, a desire to see more of the southern coast was aroused. Accordingly for the summer of 1893 the piecemeal laboratory found itself re-erected on the shore of Avalon Bay, Santa Catalina Island. The biologically inclined portion of the company consisted chiefly of the undergraduate students from the University, and general familiarity with sea-animals and the conditions under which they live rather than rigorous special researches was the scientific fruitage of the summer's undertaking.[1]

Even at this early time, Ritter was preoccupied with studying organisms in their natural environment. It was an aim that was not held exclusively by Ritter.

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[1]William Emerson Ritter, "The Marine Biological Station of San Diego: Its History, Present Conditions, Achievements, and Aims," Univ. Calif. Publ. Zool., March, 1912, 9: 148-149.

Numerous publications came out of the station in its early years (see Table 4.1, page 137). The oldest article from 1893 was published by Ritter in the Proceedings from the California Academy of Natural Sciences. It was largely a description of various species of Pacific Coast Tunicates.

> While summer work in biology of the University of California was being carried on at Pacific Grove, during the month of July, 1892, my special attention was given to the Tunicates of that locality.
>
> A large collection was made by myself and students, and the paper here presented is a portion of the results of the study begun on the living animals at the seaside, and continued on preserved material brought back to Berkeley.[2]

Other articles from this time period, i.e., 1893-1900, were much the same. Descriptions of various species of west coast crustaceans were provided by Samuel Jackson Holmes (1868-?) in 1895, and Ritter provided descriptive information on ascidians in papers published in 1896 and 1897. Papers following this format carried over into the twentieth century.

The bulk of the research carried out at the station at the turn of the century was concerned with making collections, identifying and describing the organisms in those collections, and cataloging the region from which various organisms were taken. The articles resulting from

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[2] William Emerson Ritter, "Tunicata of the Pacific Coast of North America. I.—*Perophora annectens*, N. sp." Proc. Calif. Acad. Nat. Sci., 1893, 4: 37.

this work contained taxonomic information. Most of the material made little mention of adaptation, distribution, etc. In spite of this, Ritter maintained that research of this type was in keeping with the aims of the station.

> In accordance with the general plan of the series of faunistic papers of which the present is one, the object has been kept constantly in view not merely of describing the new pelagic tunicates occurring in the area and of reporting the presence of such already familiar ones as have thus far been found; but of furnishing a ground work, as well designed and securely constructed as possible, for future investigations into the general biology of this group of animals.[3]

This classification research was, by far, the main thrust of the investigations in the station's early years, but it was defended by Ritter as being an integral part of the higher aims of the station. In addition, it was defended by Ritter as providing information that was inherently interesting.

> While the chief value attached to the describing and recording of species lies in their being the first steps toward a deeper knowledge of the organisms, there is a firm conviction in the minds of most of those who have participated in the work that a genuine and high intrinsic value pertains to such knowledge. The difference between the attitude of civilized and savage man with reference to nature consists to a considerable degree in the difference between a comprehensive and accurate knowledge of what actually exists in nature, and a restricted and at many points inaccurate knowledge. The starting place, consciously or unconsciously, of all knowledge of nature is description. So that were the enterprise to go no further than the mere characterization, arranging, and cataloguing of

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[3]William Emerson Ritter, "The Pelagic Tunicata of the San Diego Region," Univ. Calif. Publ. Zool., 1905, 2: 51.

> the kinds of organisms, it would still be justified. When, however, there is an understanding that the primary object is to lay the foundation for a superstructure of still more significant knowledge, the task is pursued with added zest, and special emphasis on this view relative to "systematic" zoology and botany is justified by the rapidly growing body of evidence to the effect that no matter what biological problems are made the subject of investigation, whether in the morphological, the physiological, or the developmental aspects of the organic beings, these problems essentially involve the question of kinds. That is, it looks as though we are being driven to recognize that all qualities whatever, be they anatomical or physiological, will if studied closely enough furnish taxonomic characters. To state the matter from a different angle, it appears that no biological generalization is fully stated until it is stated in reference to particular kinds of organisms. For example, a vast range of living beings possess the property of response to light; but do any two kinds, or even individuals, respond in quite the same way?[4]

In addition to keeping with the aims of the station and to furnishing intrinsically interesting information, Ritter saw these "faunistic papers" as providing a starting point for work done by researchers such as Harry Beal Torrey, Calvin Olin Esterly (1879-?) and Ellis L. Michael.

Torrey joined the staff of the Marine Biological Association of San Diego in 1901, and was described by Ritter as a member who played an integral part in the development of the station.[5] Torrey's first article, "The Hydroida of the Pacific Coast of North America with

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[4]Ritter, "The Marine Biological Station of San Diego," pp. 193-194.

[5]*Ibid.*, p. 151.

Especial Reference to the Species in the Collections of the University of California," was largely taxonomic. It contained descriptions of numerous hydroid species and a key for identifying west coast hydra. Torrey described the Pacific North American species with the notion that this activity would lead to an understanding of the presence and the patterns of distribution of various hydroid species of southern California.

> The scope of this paper has broadened since its preparation was begun. It was intended at first to embody the species of hydroids collected off the southern coast of California during the summer of 1901 by the University of California. To obtain a proper view of these species, however, especially from the standpoint of their distribution, not only was it found necessary to consider all the known species of the western coast of North America, but all the previous collections of the University were overhauled and a number of new species brought to light.[6]

Torrey's paper differed in one significant way from other papers published by researchers from the San Diego station at the same time, in that it encompassed a discussion of how the hydroids of the southern California coast were distributed. In examining the distribution of these organisms, Torrey discussed important physical environmental factors.

> It is important to discover the relative variability of species, their plasticity or adaptability, what characters are affected

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[6]Harry Beal Torrey, "The Hydroida of the Pacific Coast of North America with Especial Reference to the Species in the Collection of the University of California," Univ. Calif. Publ. Zool., 1902, 1: 3.

> directly by the conditions of the environment, what are more stable or not affected at all. So I have tried to bring into this paper as much pertinent ecological material as possible. Most of the western species have been described from preserved specimens, and in many cases there are no records of environmental features, such as depth, temperature, character of the bottom, etc. The depth and temperature, whenever known, are given in the table of distribution.[7]

Torrey was concerned with many of the same issues that Ritter was concerned with; in particular, Torrey was interested in gathering an understanding of the adaptability of a species to its environment. Torrey and Ritter were not alone in wanting to achieve this goal, for Michael and Esterly held similar views.

Michael contributed the article, "Classification and Vertical Distribution of the Chaetognaths of the San Diego Region: Including Redescriptions of Some Doubtful Species of the Group," in 1911. While mainly concerned with providing keys to and systematic descriptions of species, the paper also included a section on the distribution of chaetognaths in relation to environmental factors such as day and night, temperature, salinity, season, and locality. The research that contributed to this article merged taxonomic work with hydrographic work, and, as Michael stated, this type of research had the potential of providing fruitful results.

> While most expeditions have scattered their observations over a large territory, the Marine

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[7] *Ibid.*, p. 5.

> Biological Station of San Diego has confined its collecting to one locality. . . . However, our explorations have taught us much regarding methods of collection and apparatus necessary for an adequate quantitative study of plankton distribution. We are convinced that direction and velocity of currents, temperature and salinity of water, winds, clouds, fog, rain, light, and darkness all affect the distribution of plankton even within a very small area. The influence of all these conditions must be known to solve any problem concerning the quantitative distribution of plankton. . . . With this apparatus we hope to discover relations in the horizontal and vertical movements of plankton which will be valuable not only to students of planktology, but to commercial fisheries as well. By strict attention to one locality we have obtained data containing significant facts not present in the results of most expeditions.[8]

Ritter saw Michael's work as being very important to the station, and discussed it extensively when considering the work done at the station toward gathering a "deeper knowledge of the organisms" of the area.[9] As mentioned in chapter three, Ritter praised Michael for doing important research as far as ecological studies were concerned. Ritter contended that many of Michael's results were analogous to results from studies on terrestrial plants and animals, and he discussed how Michael's research on the make-up of chaetognaths had led Michael to see that the population was largely dominated by one species. This was

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[8]Ellis L. Michael, "Classification and Vertical Distribution of the Chaetognatha of the San Diego Region: Including Redescriptions of some Doubtful Species of the Group," Univ. Calif. Publ. Zool., 1911, 8: 24-25.

[9]Ritter, "The Marine Biological Station of San Diego," p. 194.

a feature often seen in terrestrial environments.

> Thus it appears that much of the same rule prevails respecting abundance of different kinds in this group of oceanic organisms as that with which we are familiar in many groups of land plants and animals. By far the larger part of all the grass in almost any naturally grass-grown region will be of one species, and so with forested regions. Most of the trees belong to one or to a very few species, though several species may be represented in comparatively small numbers.[10]

Another researcher at San Diego who published articles that focused upon taxonomy and distribution, and whom Ritter saw as having done significant work in the realm of ecology, as far as the station was concerned, was Dr. Esterly.

> The group in which the next best headway has been made in finding how abundant the different kinds are, and how and where the creatures pass their time, is the copepods or "oar-footed" crustaceans. Dr. Esterly has published two preliminary papers on this phase of his work, and has a third, much more extensive, nearly ready for the press.[11]

A paper dealing with plankton distribution came from Esterly in 1912, and another appeared in 1914. Esterly's work was concerned with the distribution of both copepods (1912) and ctenophores (1914), and his research relied heavily upon the cooperation between individuals familiar with the taxonomy of various organisms and individuals familiar with hydrography. In 1914 Esterly acknowledged the work of hydrographers when explaining how his research

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[10] _Ibid._, p. 195.

[11] _Ibid._

had progressed.

> This is one of a series of papers, from the Scripps Institution for Biological Research, that deals with the behavior of plankton organism as indicated by field data. Three publications have already appeared (Michael, 1911, and Esterly, 1912, and Esterly, 1914b) concerning the Chaetognatha, Copepoda, and Ctenophora. The facts upon which any conclusions may be based are obtained by first counting the individuals of different species in each successful surface and sub-surface haul. The hauls are then arranged according to months, time of day, temperature or salinity, and, since the number of animals taken in each set of hauls is known, it is possible to get some idea of the behavior of the organisms in nature assuming that relative abundance with respect to various factors is a reliable criterion.[12]

The group of papers by Torrey, Michael and Esterly stand as examples of relatively advanced work going on at the San Diego station in the late nineteenth and early twentieth centuries. Ritter hoped that further work, beginning at the level at which Torrey, Esterly and Michael were working, would lead to an understanding of organisms as they existed in nature. In addition, this research was important in terms of constructing a solid foundation upon which increasingly more advanced studies could be built; particularly, advanced studies tied up in experimentation. Ritter wrote:

> Now that considerable headway has been made in investigating the distribution and movements of pelagic animals as they occur in nature, the desirability of subjecting the same group to

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[12]Calvin Olin Esterly, "The Vertical Distribution and Movements of the Schizopoda of the San Diego Region," Univ. Calif. Publ. Zool., 1914, 13: 123.

> laboratory experimentation is more obvious than ever.[13]

The preceding statement leads to a particularly interesting question about the extent to which experimentation was pursued at San Diego in its early years. Experimentation did exist at the San Diego Station prior to this 1912 announcement by Ritter; in fact, physiological and morphological studies had been proceeding at the station since the early 1900s. A 1903 publication by Frank W. Bancroft and Esterly presented a physiological study on the heart of a sea squirt, or ascidian. Entitled, "A Case of Physiological Polarization in the Ascidian Heart," the paper was concerned with the reversal of the heart-beat direction in the ascidian *Ciona intestinalis*. Another article, "Embryology and Embryonic Fission in the Genus *Crisia*," by Alice Robertson (1859-1922) was much the same, but was committed to the study of embryonic fission in the bryozoan, *Crisia*.

> The processes of embryonic fission in the Cyclostomata were first made known a few years ago by Dr. Sidney F. Harmer. That investigator found that this unique process of reproduction of the embryo occurs in several somewhat distantly related genera of the subclass, viz., in *Crisia*, in *Lichenophora*, and *Tubulipora*. The facts disclosed were so interesting and remarkable, that further study of the phenomena was deemed desirable both for the corroboration of the results, and for the purpose of completing more of

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[13] Ritter, "The Marine Biological Station of San Diego," p. 198.

the details.[14]

It appears that Robertson's study was done strictly for the purpose of learning more embryology; however, this is not necessarily the case. Recall from chapter three that Ritter wanted to unite experimentation and field observation so that he might be better equipped to approach the problems of "species, distribution, and adaptation."[15] Robertson's research provided information that could be applied to understanding why organisms were able to live as they did. It furnished information that played an integral part in gaining a fuller understanding of the "life history" of an organism.

Experimentation existed at the San Diego Marine Biological Station throughout its early years; however, many of the early experimentalists did not possess as much familiarity with the species with which they were working, at least insofar as how that species was distributed in the area, as some of the later experimenters who had pursued monumental descriptive studies during the formative years of the station. Among those researchers who possessed a considerable understanding of the distribution of the organisms with which they were working when they began

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[14]Alice Robertson, "Embryology and Embryonic Fission in the Genus Crisia," Univ. Calif. Publ. Zool., 1903: 1: 115.

[15]William Emerson Ritter, "The Scientific Work of the San Diego Marine Biological Station During the Year 1908," Science, September, 1908, 28: 331.

their experimental studies was H. B. Torrey. Torrey completed excellent work in reproduction and development after completing his article on the classification and distribution of Pacific coast hydroids. He alluded to this research in the article, "The Hydroida of the Pacific Coast of North America."

> If there is one thing more than another which the preparation of this paper has brought emphatically to my attention, it is the great necessity for long-continued observation on the growth and development of hydroids under natural and artificial conditions.[16]

Torrey did a great deal of work on the organism Corymorpha. His first article, "The behavior of Corymorpha" dealt with the responses of this organism to mechanical, thermal and chemical stimuli, and to gravity.[17] His second article dealt with the development of C. palma from an egg. Torrey followed the goals he had outlined in "The Hydroida of the Pacific Coast of North America," and studied the growth and development of Pacific Coast hydroids by studying various hydroid species and by pursuing detailed studies on one particular species. Torrey studied the development of Corymorpha, with the goal being that of learning more about the adaptations of this organism. This appears to fit in with Ritter's ideas, but what about other researchers like Michael and Esterly? Did they, like Torrey, apply

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[16]Torrey, "The Hydroida of the Pacific Coast," p. 4.

[17]Harry Beal Torrey, "The Behavior of Corymorpha," Univ. Calif. Publ. Zool., 1905, 2: 333.

themselves to experimentation after they had completed their taxonomic works? In the cases of both Michael and Esterly, the answer appears to be yes.

Michael continued to update his classification on the chaetognaths, and, in 1913, added one new species, _Sagitta californica_.

> After publishing the results of a long and critical study of the chaetognatha of the San Diego region involving the individual identification of nearly 80,000 specimens, a few net-hauls made in October, 1911, unexpectedly yielded what the former prolonged search failed to reveal—a new species.[18]

Following this article, Michael began to work on the behavior of _Salpa democratica_. This project originated out of his collections.

> In working over the plankton collections made under the auspices of the Scripps Institution certain peculiarities in the occurrence of _Salpa democratica_, the smallest of the Salpae, led to intensive study of its distribution within the San Diego region. Although the study is not yet complete, the relations revealed between fluctuations in surface temperature and variations in surface distribution proved so striking and so significant, especially as regards the validity of the prevailing plankton concept, as to make it advisable to publish at once the results concerning this aspect of the problem.[19]

Michael undertook in-depth studies to determine the

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[18] Ellis L. Michael, "_Sagitta californica_, N. sp., from the San Diego Region: Including Remarks on its Variation and Distribution," _Univ_. _Calif_. _Publ_. _Zool_., 1913, _11_: 89.

[19] Ellis L, Michael, "Differentials in Behavior of the Two Generations of _Salpa democratica_ Relative to the Temperature of the Sea," _Univ_. _Calif_. _Publ_. _Zool_., 1918, _18_: 240.

distribution of this organism, eventually turning to morphological studies to get to the root of the problem. An interesting detail to note is that Michael, like Ritter, saw the importance of combining different methods of investigation.

> The way in which the morphological complexities in the life cycle are reflected in the distributional data make it necessary to describe in some detail the successive stages in this cycle. Moreover, these morphological implications of the distributional data afford indisputable evidence of the fundamental interdependence of morphological and ecological research; they demonstrate the necessity, if we are ever rightly to interpret any biological phenomenon, of conducting our investigations not only in a rigorous and critical manner, but also from the comprehensive natural history point of view so characteristic of Darwin and his immediate followers—that point of view which recognizes in all details of structure, function, behavior, and variation the unifying fact of individual and species adaptation, and which therefore holds all lines of research indispensible and no fact of nature negligible.[20]

Esterly, after completing his taxonomic studies, did a great deal of work on the behavior of various planktonic organisms. In 1919, he put out the paper "Reactions of Various Plankton Animals with Reference to their Diurnal Migrations."

> This paper reports the results of a year of study on the behavior of some marine plankton organisms. The chief aim of the work was to ascertain the factors that determine the diurnal migration of such forms. It was outside my purpose to attempt to learn how sensitive the animals under experiment might be toward changes in surrounding factors such as light or temperature, for example.

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[20] *Ibid.*

> It was not in the plan, furthermore, to consider questions connected with the interpretation of behavior as applied to such matters as the mechanism of orientation. The end desired was to learn how the direction of movement is affected by various external conditions.[21]

Esterly, unlike Torrey and Michael, did not approach his behavioral studies from either a morphological or a physiological standpoint; rather, he simulated environmental factors in the lab so that he might be able to study reactions directly. To Esterly "the actual experimental facts were sought rather than the laws or principles underlying them."[22] Esterly noted the pros and cons of this type of experimental research.

> Now if natural surroundings are to be simulated in experiments conducted for the purpose already mentioned, it is a comparatively simple matter so to arrange conditions that if the animals move downward they meet with lower temperature, higher salinity, and decreased light intensity, or with the reverse of these conditions if they ascend. It is needless to point out that there are some oceanic conditions that can not be reproduced in the laboratory, such as great depth of water, tidal currents, or distance from shore. But it is easy to provide a temperature gradient with cold water at the bottom of a column. If a light is placed at the top of the column the intensity will be greatest at the surface and progressively less below the surface. Or it may be arranged that cold water of higher salinity shall be found at the lower part of a cylinder where the light intensity is measurably less than toward the top. On the other hand, experimental conditions can be secured in the laboratory which would not be met by the organisms of the sea. An example of this

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[21]Calvin Olin Esterly, "Reactions of Various Plankton Animals with Reference to their Diurnal Migrations," Univ. Calif. Publ. Zool., 1919, 19: 2.

[22]Ibid.

> is lighting a vertical container from below. It is permissible, of course, to introduce unnatural conditions but only for the purposes of interpreting activities observed under the more natural conditions. On the whole, however, the experimental conditions were patterned after the general oceanic environment as regards light, temperature, and salinity.[23]

Researchers, such as Torrey, Michael and Esterly, who had spent time collecting and identifying many of the species of organisms in the southern California region, did progress to experimental work. Torrey continued his work on hydroids, studying the behavior and the development of _Corymorpha_. For the most part his studies were of a physiological nature. Michael pursued experimental studies on chaetognaths, looking at their behavior from a morphological standpoint. And Esterly moved on to experimental studies that sought to understand how external factors affected the behavior of planktonic organisms.

An important point that should be mentioned at this time is the fact that the majority of the work at San Diego focused upon plankton. Studies carried out on other organisms such as general fish species, starfish, cephalopods, etc. did exist, but these studies and the subsequent publications from these studies were sparse in comparison to the work centering upon planktonic organisms. The work concentrated upon plankton, and the pattern invariably followed was that of collecting and identifying

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[23] _Ibid_., p. 3.

species and then moving on to experimental manipulation of those species.

Of all of the work carried out at the San Diego Marine Biological Station, there did exist one field of research that was invaluable to all of the other research. That field of research was hydrography.

> The idea of a "Biological Survey of the Waters of the Pacific adjacent to the Coast of Southern California," set forth in the articles of incorporation of the Marine Biological Association of San Diego as the main reason for the existence of that organization, has from the outset included, as an integral part of its program, hydrographic as well as biological investigations.[24]

From its inception, a major goal of Ritter's biological survey was to determine the abiotic conditions under which an organism existed; however, investigations aimed at gathering such information were not "taken up in earnest until the summer of 1908."[25] It was at this time that George F. McEwen (1882-?), a young physicist from Stanford was asked to join the permanent staff of the San Diego Marine Biological Station. McEwen was a wonderful asset to the station, and was very clear as to what the hydrographic questions of the station should be.

> It may be profitable to state briefly, in concluding this preliminary report, what the general hydrographic problems before the Station

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[24] George F. McEwen, "Preliminary Report on the Hydrographic Work Carried on by the Marine Biological Station at San Diego," Univ. Calif. Publ. Zool., 1910, 6: 189.

[25] Ibid., p. 190.

> are: (1) In its exclusively hydrographic aspect the problem cannot be better worded than we find it in the Second Report of the North Sea Investigations, 1904 to 1905: "We want to know the physical conditions under which marine life exists, and we should like to know the conditions at all parts and all depths of the sea at all seasons of the year, and from one year to another. We want to learn the general, or average, hydrographical conditions of temperature, density, and current at each place and at each depth, and then to learn the changes or fluctuations that these conditions undergo during the year or during longer periods." (2) Seen from the standpoint of specific biological investigations, the hydrographic problem may be stated thus: Assuming a large body of knowledge answering the demands and desires indicated under (1) to have been secured, the problem then becomes: What particular conditions, agreements with, or deviations from, the average conditions, already found, prevail at the particular time and place at which any particular biological observations are made?
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> Methodologically considered it will be noted that investigations designed to answer the needs indicated under (1) might be strictly, i.e., exclusively hydrographic, while those designed to answer the needs indicated under (2) would be always primarily biological, i.e., the hydrographic observations would always be made concomitantly with and subordinate to the biological observations.[26]

The focus of the hydrographic research was to determine "the typical, average, and extreme physical conditions in temperature, density, salinity, and current in each 'section,' at all depths, at all hours of the day, during each month, and during each of a series of years."[27] Although this work was far from being complete when this

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[26]*Ibid*., p. 202.

[27]Ellis L. Michael, "Dependence of Marine Biology upon Hydrography and Necessity of Quantitative Biological Research," *Univ. Calif. Publ. Zool.*, June, 1916, *15*: ii.

statement was made in 1916, work had been done. "During the summer of 1901 a series of temperature and density determinations were made by Professor W. J. Raymond of the Physics Department, University of California."[28] This slow beginning was corrected in 1908, following the addition of McEwen to the staff.

> It has been impossible, thus far, for the Institution to conduct chemical investigations on the environment of marine organisms, but much time has been devoted to physical and hydrographical research. Since 1908, when intensive hydrographic research was begun, more than four thousand observations of salinity and density have been made and nearly five thousand surface and subsurface temperatures have been taken within one hundred miles of the coast and between Point Conception (34° 30′N) on the north and Los Coronados (32° 10′N) on the south. Within this area two hundred and sixty arbitrarily delimited rectangular "sections," each five minutes on a side, have been investigated, more than five hundred hydrographic observations having been made in some.[29]

As time passed at the station, increasing amounts of hydrographic data was gathered, but there was still much more to be done. The station still needed a chemist and a chemical laboratory in 1908, in order to complete chemical investigations of the sea; however, funds were not available.[30] In addition, a need was being felt for publishing the data that was being gathered.

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[28]Ritter, "The Marine Biological of San Diego," p. 209.

[29]Michael, "Dependence of Marine Biology upon Hydrography," p. ii.

[30]Ritter, "The Scientific Work," p. 333.

Although hydrography was seen as being an integral part of the San Diego Marine Biological Station's research program, very little of it was being published. This was a problem which did not go unnoticed by researchers at the station. Michael stated:

> During the past few years the data relative to the oceanic exploration of the Scripps Institution have been in almost continuous demand. Hydrographers have desired our temperature and salinity records in order to complete their researches. Investigators publishing faunistic papers based on material dredged by this institution have repeatedly requested information concerning the season, locality, depth, nature of the bottom, and other conditions under which the specimens were obtained. Finally, the plankton records have been urgently called for, particularly by those who, desiring to make quantitative ecological investigations, could not proceed at all without them. It soon became evident, therefore, that all our hydrographic, plankton, and dredging records would have to be published if we were adequately to meet these demands.[31]

The problem which Michael addressed was rectified in 1915 with the publication of the hydrographic records of the San Diego station through the years of 1901 to 1915.

> The biological survey of the waters of the Pacific adjacent to the coast of Southern California, which was begun by the Marine Biological Association of San Diego in 1901, has been carried on by its successor, the Scripps Institution for Biological Research of the University of California, since 1912. The aim has been to obtain as comprehensive an understanding as possible of the life of the sea, and consequently

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[31]Ellis L. Michael and George F. McEwen, "Hydrographic, Plankton, and Dredging Records of the Scripps Institution for Biological Research of the University of California, 1901-1912," Univ. Calif. Publ. Zool., 1916, 15: 3.

> considerable time and energy have been devoted to the investigation of the hydrographic conditions under which marine organisms live. The hydrographic data thus accumulated, together with all the other field data and a full discussion of the methods of collecting, laboratory analysis, accuracy of results, etc. have been published in detail.[32]

An important characteristic of the hydrographic work was that as time passed it began to outgrow the aims which Ritter had set for the station. In spite of the fact that the original place of the hydrographic work in the station's program was "to be subordinate to the investigations on plankton, serving only to provide specific information on the environment of the organisms being studied," researchers involved in this aspect of the station's program soon became aware that "there were exciting discoveries to be made regarding the physics and chemistry of the ocean and that the various aspects of these fields were worthy of study apart from their relationship to the plankton and other forms of ocean life."[33] In spite of this discovery, the hydrographic researchers did not immediately abandon their role in executing a successful survey.

In addition to helping E. L. Michael prepare the

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[32]George F. McEwen, "Summary and Interpretation of the Hydrographic Observations made by The Scripps Institution for Biological Research of the University of California 1908 to 1915," Univ. Calif. Publ. Zool., 1916, 15: 256.

[33]Helen Raitt and Beatrice Moulton, Scripps Institution of Oceanography: First Fifty Years (La Jolla: Ward Ritchie Press, 1967), pp. 83-84.

> raw field data for publication and work out a statistical method for application to plankton, McEwen prepared hundreds of charts and graphs based on the thousands of observations made since the station's establishment. This concentrated and usable information on temperatures, salinity, currents, and other physical and chemical phenomena was extremely valuable to the other researchers attempting to reach conclusions about the relation of ocean life to its environment. In 1916, McEwen and Michael devised a combined plankton net, water bottle and thermometer which was of great improvement over the collecting apparatus which had been used formerly. . . . McEwen developed a way to measure the closing depth, and on the whole the new apparatus was much more sure and simpler to use.[34]

McEwen and his successors continued to apply their work to the survey, but they also spent time involved in work outside of the general aims of the survey. McEwen became very interested in oceanic circulation and how it related to seasonal weather forecasting. He had extensive plans for executing such research.

> Such an investigation would require the execution of a comprehensive programme of oceanic and meteorological research, on a scale demanding generous government support. A practical plan of procedure would consist of two parts: (1) such regular observations as it is practicable to make from the ships of our new mercantile marine; and (2) continuous observations made from lightships, lighthouse stations, islands, and one or more properly equipped vessels cruising regularly in certain selected regions where continuous observations in the open ocean are especially desirable.[35]

"Erik G. Moberg, then a candidate for doctor's degree, began important investigations on the chemistry of the

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[34] _Ibid_., p. 84.

[35] _Ibid_., p. 18.

sea."[36] Moberg's publication "I. Observations on the Effect of Tidal Changes on Physical and Chemical Conditions of Sea Water in the San Diego Region" illustrates what some of his interests were; however, his research was still directed toward the aims of the station at this point.

> A series of hydrographic observations and plankton catches taken at the entrance to Mission Bay, in November, 1922, gave interesting information concerning the effect of tidal changes upon oceanographic conditions in that locality. Since the topographic features of Mission Bay are rather unique, similar studies were undertaken at the Scripps Institution pier in order to determine whether tidal movements along the open coast produce effects analogous to those produced in the bay. A knowledge of the changes in physical, chemical, and biological conditions accompanying changes in tide is of especial importance since much of the material used by the Scripps Institution in oceanographic studies is obtained from points near shore.[37]

These research efforts do not reflect Moberg's main interests. As Moberg proceeded in his studies, he began working on determining the "physical and chemical factors which determine the hydrogen ion concentration [of sea water] and control its variation."[38] Moberg's reasons for pursuing such studies did fit in with the aims of the San Diego station, but they also moved beyond these aims.

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[36]Raitt and Moulton, <u>Scripps Institution of Oceanography</u>, p. 85.

[37]Erik G. Moberg, <u>Bull</u>. <u>Scripps Inst</u>. <u>Oceanogr</u>. <u>Tech</u>. <u>Ser</u>., March, 1927, <u>1</u>: 1.

[38]Erik G. Moberg, David M. Greenberg, Roger Revelle and Esther C. Allen, "The Buffer Mechanism of Sea Water," <u>Bull</u>. <u>Scripps Inst</u>. <u>Oceanogr</u>. <u>Tech</u>. <u>Ser</u>., 1934, <u>3</u>: 231.

> As pointed out by Legendre (1925), information concerning the equilibrium relationships between the pH and the salts of carbonic acid is of far-reaching importance for studies of the life-processes and the vitality, migrations, and distribution of marine organisms. Geologically, the components of the buffer mechanism of the sea are significant factors in the formation and diagenesis of marine sediments and, in particular, of marine limestones.[39]

Moberg was interested in the chemical features of sea water, and he pursued these investigations not only to support the station's biological survey, but also to explain interesting questions about the physics, chemistry and geology of the ocean.[40]

All of the work directed outside of the aims of the station culminated in the rapid advance of "knowledge of the physics and chemistry of the ocean," and in the recognition "by Ritter and other members of the staff and administration" that the hydrographic work was becoming an important force of its own at the station.[41] That the work advanced quickly and that it was received so positively was

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[39] _Ibid_.

[40] His analysis of the buffering of sea water also included studies on the boron content and the calcium content of sea water. See Erik. G. Moberg, "The Boron Content of Sea Water," _Science_, 1933, _77_: 510, Haldane Gee, David M. Greenberg and Erik Moberg, "Calcium Equilibrium in Sea Water. II. Sealed Bottles Shaken at Constant Temperature," _Bull_. _Scripps_ _Inst_. _Oceanogr_. _Tech_. _Ser_., 1932, _3_: 158-164, and Haldane Gee and Erik G. Moberg, "Calcium Equilibrium in Sea Water. III. Empirical Variation in Gas Phase," _Bull_. _Scripps_ _Inst_. _Oceanogr_. _Tech_. _Ser_., 1932, _3_: 105-173.

[41] _Ibid_.

no doubt related to the dedication of the hydrographers to their work.

> Last Saturday night Dr. McEwen and Mr. Moberg and Mr. Woodward made a series of hourly collections of water samples at the Mission Bay Bridge. They remained through most of Sunday so as to get a variety of conditions of the tides. The intention was to see how the bay affected the temperature, saltiness and other conditions of the water which flows back and forth under the bridge. Late in the night someone who thought their equipment of bottles looked suspicious evidently informed the prohibition officer, as someone came and asked them a number of questions and looked at the contents of their bottles.[42]

The increasing importance of hydrographic work was not the only way in which the aims of the station were being bypassed. Individual researches that had very little if anything to do with the program were being undertaken. An excellent example of just such a project which had nothing whatsoever to do with Ritter's aims was the work undertaken by Francis B. Sumner. Moulton and Raitt describe it as "one of the most unusual research projects ever conducted" at the San Diego laboratory.[43]

Sumner joined the San Diego team in 1913, and was engaged in research that focused upon "heredity and environmental influence in the genus of mice <u>Peromyscus</u>."[44] Sumner's first application to do such research at the

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[42]Raitt and Moulton cite this news article in their book, <u>Scripps Institution of Oceanography</u>, p. 86.

[43]<u>Ibid</u>.

[44]<u>Ibid</u>.

station was rejected, but when he applied again on February 1, 1913, he was accepted. Sumner stated that "my proposition came at a favorable psychological moment, and met, from the outset with sympathetic reception from [Ritter]."[45] Raitt and Moulton have elsewhere clarified what Sumner meant.

> The proposal came at an opportune time, as the state legislature's appropriation, part of which had been designated for an addition to the research staff, was soon assured. Also, less than a year before, when explaining the institution's change in name in his annual report, Ritter expressed the feeling of himself and the board that the institution "ought to have the utmost freedom as to the particular provinces of the vast domain of biology that it should cultivate at different periods of its existence."[46]

Ritter's biological survey did not completely disappear, but it did come to play a smaller role in the program of the Marine Biological Station of San Diego as time passed. Part of this change in the importance of the survey can be attributed to that fact that as research continued, new questions presented themselves for exploration. The increasing interest in answering hydrographic questions that fell outside the aims of the station stands as an example of this. The shift can also be attributed to the change in the policies of the station.

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[45]Francis B. Sumner, The Life History of an American Naturalist (Lancaster: The Jaques Cattell Press, 1945), p. 198.

[46]Raitt and Moulton, Scripps Institution of Oceanography, pp. 86-87.

There can be little doubt that the opening of the station to general biological researches contributed to the erosion of the aims of completing a biological survey.

The early research at San Diego followed a definite pattern. It focused upon collecting and identifying various species in different phyla, and then subjecting particular well-known and/or abundant species to experimental studies in order to learn more about that species. This mode of research seems to have fit in well with Ritter's goals for the San Diego station, which involved gathering a general familiarity with the life off the coast of California. Ritter praised those researchers who had identified so many species, and had then pursued problems involving the identification of the structures, the functions, or the behaviors that allowed species to exist where they did. As time passed and as work progressed, however, things changed. Although the aims of completing a biological survey were never lost sight of, research at the station did begin to look more like that of other research stations. Investigators became increasingly concerned with their own investigations, and as Sumner's work illustrates, oftentimes the investigations undertaken had little to do with marine biology.

Table 4.1—Scientific Papers Written by Investigators Associated with The Marine Biological Station of San Diego.*

BAILEY, S. E., see Ritter and Bailey.

BANCROFT, F. W., and ESTERLY, C. O.
1903. A case of physiological polarization of the ascidian heart. _Univ. Calif. Publ. Zool._, 1, 105-114.

BARTSCH, P.
1907. New marine mollusks from the west coast of America. _Proc. U. S. Nat. Mus._, 33, 177-183.
1911a. The recent and fossil mollusks of the genus _Cerithiopsis_ from the west coast of America. _Ibid._, 40, 327-367, 6 pls.
1911b. The recent and fossil mollusks of the genus _Bittium_ from the west coast of America. _Ibid._, 40, 383-414, 8 pls.

BARTSCH, P., see Dall and Bartsch.

BERRY, S. S.
1911. Notes on some cephalopods in the collection of the University of California. _Univ. Calif. Publ. Zool._, 8, 301-310, 2 pls.

CHILD, C. M.
1906. The relation between regulation and fission in Planaria. _Biol. Bull._, 11, 113-123, 19 figs. in text.
1908. Regulation of _Harenactis attenuata_ in altered environment. _Biol. Bull._, 16, 1-17, 16 figs. in text.
1909a. Experimental control of certain regulatory processes in _Harenactis attenuata_. _Biol. Bull._, 16, 47-53, 6 figs. in text.
1909b. Factors of form regulation in _Harenactis attenuata_. I. Wound reaction and restitution in general and the regional factors in oral restitution. _Journ. Exp. Zool._, 6, 471-506, 24 figs. in text.
1909c. Factors of form regulation in _Harenactis attenuata_. II. Aboral restitution, heteromorphis, and polarity. _Ibid._, 7, 65-96, 12 figs. in text.
1909d. Factors of form regulation in _Harenactis attenuata_. III. Regulation in "Rings." _Ibid._, 7, 353-395, 31 figs. in text.

Table 4.1 (Continued)

COCKERELL, T. D. A.

1901a. Three new nudibranchs from California. Jour. Malacology, 8, 85-87.

1901b. Four new Tethys from California. The Nautilus, 15, 90.

1902. Three new species of Chromodoris. Ibid., 16, 19-21.

DALL, W. H., and BARTSCH, P.

1907. The Pyramidellid mollusks of the Oregonian faunal area. Proc. U. S. Nat. Mus., 33, 491-534, 5 pls.

1909. A monograph of West American Pyramidellid mollusks. Bull. U. S. Nat. Mus., 68, 258 pp., 30 pls.

DAVIS, B. M.

1908. The early life-history of Dolichoglossus pusillus Ritter. Univ. Calif. Publ. Zool., 4, 187-226, 5 pls.

DAVIS, B. M., see Ritter and Davis

ESTERLY, C. O.

1905. The pelagic copepoda of the San Diego region. Univ. Calif. Publ. Zool., 2, 113-233, 62 figs. in text.

1906a. Some observations on the nervous system of copepoda. Ibid., 3, 1-12, 2 pls.

1906b. Additions to the copepoda fauna of the San Diego region. Ibid., 3, 53-92, 6 pls.

1911a. Third report in the copepoda of the San Diego region. Ibid., 6, 313-352, 7 pls.

1911b. Diurnal migrations of Calanus finmarchicus in the San Diego region during 1909. Internat. Revue ges. Hydrobiol. Hydrog., 4, 140-151.

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CONCLUSION

In the introduction to this thesis, it was contended that the Marine Biological Station of San Diego pursued a research program that reflected the ideas of the station's founder and first director, William Emerson Ritter. It was also contended that this research program placed the station outside of the central core of biology. In an effort to verify these claims, a series of arguments were presented, each one illustrating either an aspect of Ritter's personal philosophy of science or the research undertaken at the San Diego Marine Biological Station. This chapter, as the concluding chapter, will be devoted to two things. First, it will be devoted to an evaluation of whether or not researchers at the station were sympathetic to Ritter's aims. Second, this chapter will be devoted to an examination of how the research at Ritter's station compared to the core work of biology which was directed at creating a unified field called biology.

Many participated in the establishment and development of the San Diego Marine Biological Laboratory by providing either financial or moral support; however, the formation of the station was primarily the result of Ritter's desire to see a marine biological station established on the Pacific coast. Following its formation, the station set out to make "a Biological Survey of the waters of the Pacific adjacent to the Coast of Southern California."

Ritter was interested in discovering how organisms were distributed in the environment with respect to physical factors, and in identifying what adaptations they possessed to allow them to exist where they did. These were questions similar to those held by the early ecologists; however, Ritter sought answers to these questions for fundamentally different reasons and in a fundamentally different way than the early ecologists. Ritter was not alone with his beliefs. Some of the investigators with whom he worked at the San Diego Marine Biological Station held many of the same ideas. Dr. C. O. Esterly, Dr. E. L. Michael and Dr. H. B. Torrey, for example, were concerned with describing the marine organisms off the San Diego coast and with determining their distribution with respect to abiotic factors. Their research captured the aims of Ritter's survey.

Researchers at the San Diego station not only met Ritter's aims by pursuing investigations that contributed to the biological survey of San Diego marine life, but also met his goals by combining their work. Ritter wanted to combine various specialists and diverse biological methodologies in order to learn more about the distribution and adaptations of organisms. The conjunction of the work of biologists and hydrographers met this goal. The hydrographic work was invaluable to researchers at the San Diego station, and, in spite of the fact that it took on an importance of its own as the years passed at the station, the hydrographic research was initially pursued for biological purposes.

Researchers at the San Diego station followed Ritter's aims, but was the station wholly successful in meeting these aims over a long period of time? In its initial years, i.e., 1905-1915, the program at San Diego was successful in pursuing research that met the aims of Ritter. Researchers were very much involved in collecting and identifying organisms. Some hydrographic work was being pursued at this time, so that information about the distribution of organisms as related to physical factors might be gained. The work was progressing as Ritter envisioned, but as time passed this changed. This can be attributed to two factors: first, to the addition of George F. McEwen to the staff in 1908; and second, to the transfer of the station to University of California at Berkeley in 1912.

The addition of George F. McEwen to the staff was of paramount importance. Not only did the station begin to pursue hydrographic investigation with more fervor than previously had been done, but McEwen, as a researcher very dedicated to his work, soon recognized the value of pursuing investigations outside the immediate aims of providing hydrographic data for biological researchers. He became immersed in research centered upon ocean circulation and how it related to weather. McEwen was not the only one to see the importance of other lines of research. For example, Erik. G. Moberg became interested in discovering more about the chemistry of the ocean. Ritter's goal of completing a biological survey was never lost sight of, but as time passed oceanographic investigations into the chemistry and the physics of the sea took on an importance of their own.

The increased effort in the field of hydrography was not the only thing that had an effect upon the program at San Diego. The change in the status of the station in 1912, from a station privately owned and run to a department of the University of California had a definite impact upon the station's program. When this change occurred, the name of the San Diego station was changed from The Marine Biological Laboratory of San Diego to The Scripps Institution for Biological Research of the University of California. That the word "marine" was dropped from the station's title reflects the fact that the station was opening itself up to a multitude of research project possibilities. No longer was the station to be an isolated institution where only one program of research was pursued. It was decided that the station should expand its research efforts beyond the field of marine biology. One of the more unusual projects taken up at the station was Francis B. Sumner's investigations on the genus of mice

Peromyscus. Research investigations pursued at the station did not need to fit into the station's program when this change occurred, because the station no longer existed as an institution where investigators gathered to work towards a common project. The station became a place where various researchers could go to quietly pursue their own unique studies.

In light of the fact that the main aim of Ritter's station was to make a biological survey of the waters of the coast of California and that the station was achieving its aims, at least in the first few years of its existence, it can be contended with a great deal of certainty that the San Diego Marine Biological Station did not fit in with the larger biological aims of creating a unified field called biology. Recall from the introduction that many biologists, especially those that worked at the MBL, were interested in defining fundamental laws and theories that would serve as a solid foundation upon which all other biological interests could rest. They were largely mechanistic, and treated the organism as a machine to be broken down and analyzed. Phenomena exhibited by these individual parts were to be explained in chemical and physical terms. This tradition was popular among Ritter's peers, for many scientists believed that the methods of investigation harbored in this tradition would be useful for discovering the laws and theories that underscored the science of biology. Ritter disliked this approach, at least insofar as to how it related to this own aims. Ritter supported a tradition which held that studies of the organism in its entirety and as it existed in nature were required in order to acquire complete knowledge about that organism. He maintained that explaining the phenomena of an organism by breaking it down into its constituent parts may have been adequate to discover laws and theories, but it was not adequate if one really wanted to understand the organism as it existed in nature. The real focus of Ritter's research and the work at San Diego was to gather an understanding of how the organism existed in nature. It was an aim that was not compatible with biological research directed toward creating or supporting basic biological laws and principles.

This story of the San Diego Marine Biological Station is of value in terms of illustrating that institutions can reflect the aims of certain persons who are involved with them, and that not all of the investigations of individual researchers or of larger institutions must conform to the aims of the disciplines of which they are a part. Although this thesis may illustrate these things, this story is far from being complete. This thesis has brought to light a host of questions to be pursued. For example, it was shown that the focus of the station changed as the station grew older. This change was attributed to the fact that hydrographers became increasingly concerned with pursuing questions of their own, and that there was a shift in the controlling force behind the station, i.e., the station was turned over to the Regents of the University of California by a small organization of San Diego citizens. This discussion has a great deal of room for expansion. For example, the relationship between the University of California and the Station could be examined to ascertain if the university played a major role in determining the aims of the station. An investigation into T. Wayland Vaughan, who replaced Ritter as director at the station in 1924, could be made to see how he affected the aims of the station. An examination could also be made concerning what happened to Ritter's survey following his retirement from the directorship of the institution in 1924.

Another question that this thesis brings to light concerns how the San Diego Marine Biological Station fit in with other stations. This was only briefly examined in this thesis, but could be expanded greatly. Questions pertaining to how research compared between stations, the amount of communication between stations, whether the San Diego station was greatly influenced by any of these stations are all questions whose answers would be very informative. In addition to this, it would also be of value to expand the discussion of how Ritter's aims compared to those of his contemporaries. This might allow one to say more about the nature of biological science in the late nineteenth and early twentieth centuries. As was mentioned in the introduction, a number of historians have been involved in defining the nature of biology at the turn of the century, and I think that an expanded discussion of how Ritter's station fit into biology at this time would be very valuable in terms of helping to define this period.

All of these questions, of course, are for other research projects, as they lie outside the scope of this thesis; however, they would be interesting to pursue, because answers to them would provide more information about an institution that was developed during an important time in the development of the life sciences.

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