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The Marine Biological Station of San Diego, Its History, Present Conditions, Achievements, and Aims

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Author

Ritter, WM. E.

Publication Date

1912-03-09

THE MARINE BIOLOGICAL STATION OF SAN DIEGO

ITS HISTORY, PRESENT CONDITIONS, ACHIEVEMENTS, AND AIMS

BY

WM. E. RITTER

UNIVERSITY OF CALIFORNIA PUBLICATIONS IN ZOOLOGY

March 9, 1912

ANNOUNCEMENT

While this report has been passing through the press, the transfer of the Station to The Regents of the University of California, as provided by the articles of incorporation (Appendix A), has been effected. The official designation under the new regime will be

THE SCRIPPS INSTITUTION FOR BIOLOGICAL RESEARCH
OF THE
UNIVERSITY OF CALIFORNIA

Although, as indicated by the change of name, an enlargement of activities is contemplated, no immediate alteration of policy or work will take place.

William E. Ritter,
Scientific Director.

La Jolla, California,

February 25, 1912.

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A. OFFICERS

ADMINISTRATIVE

<i>Dr. Fred Baker</i>	President
<i>H. L. Titus</i>	Vice-President
<i>W. C. Crandall</i>	Secretary
<i>Julius Wangenheim</i>	Treasurer
<i>Albert Schoonover</i>	General Counsel
<i>William E. Ritter</i>	Scientific Director
<i>Ellen B. Scripps</i>	Member of the Board

SCIENTIFIC STAFF

William E. Ritter, Ph.D.,
Director (Professor of Zoology, University of California)

Charles A. Kofoed, Ph.D.,
Assistant Director (Professor of Zoology, University of California)

Harry Beal Torrey, Ph.D.,
Librarian (Associate Professor of Zoology, University of California)

Ellis L. Michael, M.S.,
Resident Naturalist

Calvin O. Esterly, Ph.D.,
Zoologist (Professor of Biology, Occidental College, Los Angeles, California)

George F. McEwen, Ph.D.,
Hydrographer (Instructor in Applied Mathematics, University of Illinois)

H. C. Burbridge, A.B.,
Physicist (Assistant in Physics, Stanford University)

Myrtle E. Johnson, M.S.,
Zoologist (Fellow in Zoology, University of California)

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B. INTRODUCTION

This year, 1911, is the twentieth anniversary of the first movements that led to the establishment of the Marine Biological Station of San Diego. It is also the tenth anniversary of the beginning of definite, continuous effort toward that end. The time is fitting, therefore, for a fuller general presentation of the institution's career, structure, accomplishments, and purposes than has yet been written. The account is not merely narrational and descriptive; it is a sort of confession of faith as to the larger meaning of science, of biology in particular. It has been prepared with the desire to make it informing and interesting to those immediately responsible for the life and work of the station, i.e., its patrons, business managers, and investigators; to those who are engaged in similar but wholly independent enterprises; and last but in no wise least, to earnest, inquiring people generally.

Sections B to G will be of interest mostly to the first two groups, in as much as these sections are a detailing of how an institution of research in pure science, starting with almost nothing in a material way and with almost no real friends, may yet grow to a state of considerable size and breadth of outlook. Sections E, G, and H ought to interest a number of persons in no way connected with or concerned about the station as such. Biologists and oceanographers especially should find items in sections F and G that would appeal to them. The concluding section has been put into the story for the reason that the desire to make the Station an instrument of general public enlightenment is embedded very fundamentally in the conceptions upon which the institution rests.

On glancing over the section headings the general reader will, presumably, be inclined to turn to the last two sections and read them first if not exclusively. To read this last part first would be in no wise objectionable; indeed such a course may be commended. But I would strongly urge that the sections "The Station as it is to-day" and "Scientific results already

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achieved" be not passed by entirely. To do so would be to miss much of the real essence of any part. No one can hope to get more than a modicum of the good possible in general knowledge of the results of scientific research, without giving some attention to the methods by which the results are obtained. This consideration has led to the inclusion in the account of an amount of detail about the equipment and workings of the station's boat, the "Alexander Agassiz," that would otherwise probably have been left out.

Another matter of great moment for general, hardly less than for special information, is the vital way in which such enterprises are dependent upon the combined efforts of a number of persons. In any considerable undertaking the dependence of every one upon every other one is so commonplace a fact that the fundamentality of it is likely to be less reflected on than it ought to be. With the two-fold purpose of giving information as to who have been most active in the development of this particular institution, and of encouraging more general recognition of the fact that all such enterprises are in very essence partly individual and partly communal, I have aimed throughout to keep persons rather than inanimate things to the front, and have endeavored not to leave any one unmentioned who has contributed significantly to the results. The larger number of those who fall into this category are referred to under headings dealing with the particular work they have done. But a few individuals have been so intimately connected with many aspects of the station's growth and work that they must be spoken of here.

The operations of the "Agassiz" as set forth under the appropriate headings would have been impossible but for Mr. Ellis L. Michael and Captain W. C. Crandall, they having been responsible for most of the methods actually employed. By this I do not mean that these men and no others are competent for this work, but that the undertaking is such as to demand the combined efforts of several persons; that into their hands fell this important part of the programme; and that they proved equal to the task. Similarly into the general working plans of the station are woven the suggestions, the ideas, and the activities

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of Professors C. A. Kofoid and H. B. Torrey in so intimate a way that surely without them the institution could not have been exactly what it is; indeed could not have been at all without services quite like theirs. To Professor Kofoid's extensive knowledge of laboratory plans, construction, and equipment, to his skill in working out details for such purposes, and to his ability in devising mechanical appliances, are largely due the laboratory building and some of the most important apparatus used on the "Agassiz." Professor Torrey also took an important part in planning the laboratory building and the "Agassiz"; while his special efforts have largely produced the library, not only as it now is but as it will be in its fuller development.

During the spring and summer of 1906 while the Scientific Director was absent from the United States, the conduct of affairs devolved upon Professor Kofoid as Assistant Director, and the period proved to be one of special importance for the station in that, largely through his initiative, three biologists, Professors E. L. Mark of Harvard University, E. B. Wilson of Columbia University, and H. S. Jennings of Johns Hopkins University, were invited to La

Jolla at the expense of Mr. E. W. Scripps, primarily as advisors on certain matters of policy, particularly on the question then being considered, of locating on the land afterward acquired for the station's permanent site.

Moving on to still broader, more general aspects of the institution, it is not too much to say that the transference of the station from its precarious existence at San Pedro to its surer tenure of life at San Diego was a service due more to Dr. Fred Baker than to any other person. And from that day to this his constant thought and care and activity have contributed to making the institution what it is, to an extent that can be only partially specified. As concerns the routine business affairs since the formation of the Marine Biological Association of San Diego in 1904, what has been said of Dr. Baker's service is almost equally true of that of the other officials of the Board of Directors, Mr. Julius Wangenheim as treasurer and chairman of the building committee, Mr. H. L. Titus as vice-president and legal adviser, and Mr. F. W. Kelsey as secretary in the earlier days, and later Mr. W. C. Crandall in the same office. Nor

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would the list of those whose labors have contributed most to the institutional upbuilding and operations of the station be in any wise complete without reference to the vicarious activities, official and scientific, of Mrs. William E. Ritter, wife of the scientific director.

Acknowledgment is also heartily made of the uniform courtesy and in several instances substantial help and encouragement given the station by persons acting in official capacity for the city of San Diego.

Finally, Miss Ellen B. Scripps and Mr. E. W. Scripps, the main financial supporters of the station, remain to be mentioned though only in general terms in this catalogue of acknowledgments. What these two have done in a monetary way is shown under other captions. The fact of fundamental importance to be brought out here is that whatever has been accomplished has been by earnest, thoughtful, sympathetic coöperation between those, on the one side, possessed of technical scientific knowledge and experience but no material resources; and on the other side, those possessed of large material resources but little of the technical knowledge and experience requisite for such an enterprise, the common meeting ground of the two parties being great faith in the efficacy of natural knowledge toward the highest good of mankind. By coöperation I do not mean that one party furnishes merely the money and the other party merely the technical experience. Such a conception is altogether too small for the sort of coöperation that is being practiced here. The truth is that one party furnishes the money primarily, and secondarily solicitude, sympathy and keen intelligence in the general development of the institution, while the other party furnishes technical knowledge and experience primarily, and secondarily thoughtfulness, care, and judgment in the business management.

Thus all along the line there is recognition both by givers and doers that in such an enterprise no one, however much material wealth or personal ability he may have, can accomplish anything alone. Success depends not only on individual but on communal efforts.

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C. THE GENERAL IDEA—ITS INCEPTION AND DEVELOPMENT

The original ideas out of which the station grew though not lacking in positiveness were little differentiated. In 1891 when the scientific director took his place in the newly inaugurated subdepartment of biology in the University of California, the question of marine zoology at once presented itself. Imperfectly as had any of the fields of zoology of western America been cultivated, the least studied of all had been the teeming life of the great ocean on whose margin the University is located. This consideration was of itself a strong incentive to marine investigations. When in addition it was reflected that this field was not only practically virgin but was so set apart and unique in various ways as to warrant the expectation that it would yield richly in both new problems and new light on old problems, there was little hesitation in deciding that the chief effort in research should be in this domain. So it came about that most of the investigations carried on by the department of zoology from that day to this have pertained to the life of the Pacific Ocean.

Exactly what form the effort should take was not clearly indicated either from special familiarity with the most promising problems of marine biology nor from distinctly recognized expediency as to practicable undertakings. One thing was clear: no great headway, in a proper zoological sense, could be made without obtaining a far wider speaking acquaintance with the fauna of the region than any one had yet attained. A large amount of work in systematic zoology would have to be done at the outset.

The reasons that justified turning away from the great Bay of San Francisco, the very dooryard of the University, and finally choosing a location for the station six hundred miles distant, were partly recognized and partly not recognized. It is always instructive to look back upon any development and notice how, with whatever of deliberation it may have been carried

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along, seen and unseen, even unforeseeable factors work together to produce the results actually reached.

The fact about San Francisco Bay that was recognized at once to be severely against it as headquarters for researches on marine biology was the absence from nearly all of its waters of the most characteristic oceanic animals and plants. This absence, which applies to the whole bay excepting the Golden Gate where the water runs with such fury at every change of the tide as to render working in it with small boats extremely difficult and hazardous, is due to the nearly land-locked nature of the bay and the discharge into it of the two large rivers, the Sacramento and the San Joaquin, with their heavy loads of sediment.

The circumstances that were destined to lead to greater and greater emphasis on problems essentially involving the life and physical conditions of the open ocean, which problems would in turn be so greatly favored by the conditions far to the south, were not at all clearly seen at the outset. Beyond the two early determinations that faunal studies must, at least at the beginning, play a large part in whatever might be our undertakings, and that a working place at some suitable point on the seashore proper would be essential, the main alternatives in policy and in method were but imperfectly seen. Assuming such a station assured, should its purpose be exclusively research, exclusively the formal instruction of elementary students, or a combination of the two? Should it aim at being a convenient work-place to which investigators from whatever source might go from time to time as individual needs might prompt, or should definitely correlated researches by the station as such, be aimed at? Should effort be confined to the shore life, or to the free-swimming and floating life, or should all, with no favors to any, be included in the programme? These and various other questions in the light of the meager experience to begin with could be but vaguely asked and more vaguely answered. Special local conditions and experiences gradually indicated the course which it seemed best to steer with reference to most of them.

Important as is the instruction of youth in biology, since provisions for this already existed in the two universities, in

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the several colleges and normal and high schools of California, and for seaside work in the Timothy Hopkins Laboratory at Pacific Grove, it seemed that research ought to have the prior if not exclusive right, for it soon became apparent that with the facilities available or likely to be available any well carried out effort at elementary teaching must seriously interfere with research. Hence, after a few of the earlier summers, all thought of formal classes for beginners was put aside for the time being.

Two main considerations led gradually to the conviction that fairly definitely laid out investigations by the station, by teamwork as one might say, to be prosecuted by those naturalists more or less regularly and permanently connected with the enterprise, would be on the whole both more profitable and more practicable than an effort to develop a laboratory of general rendezvous for investigators of all sorts of interests and from all quarters. The two considerations were, first, the vast scope and possibilities in faunistic researches if only "faunistic" were to be taken in a broad sense, and the impossibility of doing much at such researches without coördinated and continuous effort. The second consideration lay in the remoteness of the Pacific Coast from the main centers of scientific activity of the world. The large cost in both time and money of reaching our shores would be prohibitive for the majority of investigators. The already established seaside laboratories of Europe and the Atlantic Coast of America, offering their splendid facilities, must for years to come be chiefly sought by students. The wisest course seemed to be to make a virtue of our disadvantages as far as possible: to concentrate our small energies and funds upon what it seemed might be done rather well by working in our own way, instead of dissipating them on what appeared highly probable we should not be able to do at all well.

So again by a process of natural selection and elimination the idea gained ground of a biological survey prosecuted as systematically, as continuously, as protractedly and as broadly, as facilities would permit. The natural and inevitable expansiveness of such an undertaking will be seen by any one who has even a meager acquaintance with the phenomena to be investigated and the methods that must be employed. The farther one

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advances in experience and knowledge, the more does he become impressed with the vast scale on which things are done in the ocean and the literally infinite complexity of cause and law there in operation. Except for the gleams of light that early promise more light and the fascination there is in overcoming difficulties, one might welcome a pretext for turning back, once he has gone far enough in such an enterprise to see what is actually on his hands. Mere enthusiasm is too evanescent, and initial personal ambition has too many alternatives to hold one steady on such a course.

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D. HISTORICAL SKETCH

I. PACIFIC GROVE; 1892

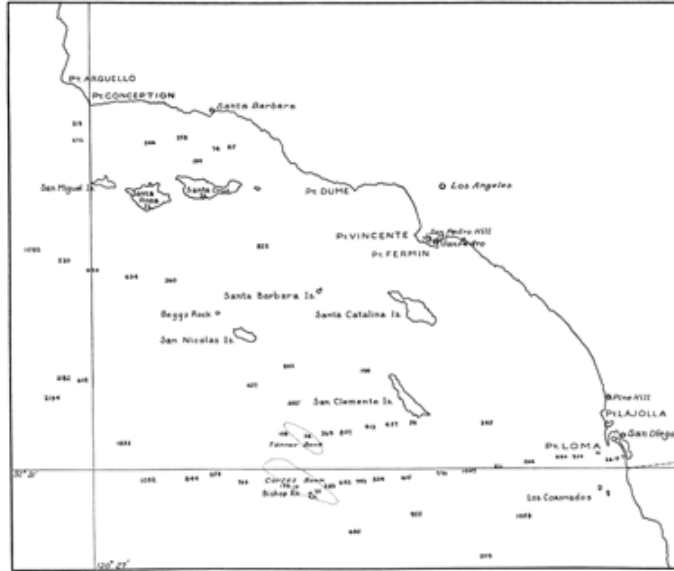
In the spring of 1892 a structure 16 × 24 feet, partly of wood and partly of canvas, and constructed with a view to being taken to pieces and moved about, was built for use as a seaside laboratory at Pacific Grove. The requisite funds, about \$200, were provided by the University, the department of zoology being permitted to expend the amount from its annual budget. This itinerant laboratory was set up for the summer vacation, the site selected being a small cove near the since departed Chinatown. The little apparatus provided, microscopes, glassware, etc., were borrowed from the University. All the water used, salt and fresh, was carried to the laboratory in buckets by hand.

The laboratory party consisted of about a dozen persons, mostly students and teachers but partly recreation seekers.

The first building of the Timothy Hopkins Laboratory belonging to Leland Stanford Junior University was erected and occupied for the first time during the same summer, and alongside that ample, well appointed laboratory our little tent-house made a sorry spectacle. Nothing on record shows any notable discoveries made that year, though a not inconsiderable amount of general collecting was done, both of information and specimens.

II. SANTA CATALINA ISLAND, 1893

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 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.



Map 1.—Showing the Area to be surveyed. Modified from United States Coast and Geodetic Survey Chart. Compare with plate 24, figure 10.

[Full Size]

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From the experiences of this year two important advances were made toward a decision as to what portion of the coast would be best for the location of a permanent laboratory: That San Francisco Bay and the outside places immediately adjacent to it which had been considered as possibilities would better be abandoned; and that some point in southern California south of Santa Barbara, probably San Pedro as it then appeared, presented on the whole more natural attractions to the student of marine organisms than any other portion of the California coast. The counsel of Professor E. B. Wilson of Columbia University, who had been asked by the University of California to come to the Pacific Coast in the interest of the marine work, was greatly influential toward these conclusions. Concerning the natural factors that entered into the final determination of the site problem I speak more in detail a little later.

It is pleasant to recall the lively interest taken by several persons besides those participating in the work itself during these first years. 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. Being a man of uncommon intelligence and breadth of interests, though in no sense a biologist, he was keenly alive to the scientific significance of researches in marine biology and to the possibilities of a marine aquarium as a source of public enlightenment and

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recreation. But for financial difficulties that came on with the widespread business collapse of 1893, and the conclusion to which we on the scientific side were driven that an effective biological station at any point near the Golden Gate would be impracticable, it is highly probable that Mr. Sutro would have become a liberal patron of marine biology and oceanic exploration.

III. EXPEDITIONS, 1894–1900

Owing partly to the fact that the person who had thus far been responsible for directing the seaside work was absent in Europe during the summers of 1894 and 1895, the knock-down laboratory was not erected in those years. A significant collecting and reconnoitering expedition on the coast north of San Francisco was, however, made in 1894. The party constituting the expedition were Mr. S. J. Holmes, a recent graduate from the University and an assistant in zoology; Mr. Frank Bancroft and Mr. E. W. Horn, advanced undergraduate students.

Following up the intimations received by the glance at San Pedro Bay and vicinity while en route to Avalon in 1893, that this was an especially favorable spot for a marine laboratory, a party from the University spent several weeks

there during the summer of 1895. Dr. H. P. Johnson was in charge. A cottage and a tent on Timm's Point in San Pedro Bay constituted the laboratory-dormitory facilities. This season's experience was of special significance in that it greatly strengthened the belief in the natural advantages of this region for a marine station.

Between 1896 and 1901 no organized parties were formed for marine work nor was a seaside laboratory maintained. However, numerous collecting excursions were made from time to time by various members of the department to many points on the coast, ranging from San Diego at the south to the Shumagin Islands, Alaska, at the north. 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.

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The outcome of these rather extensive castings—about was a firm conviction shared by all who had participated in reconnoitering that San Pedro Bay would be by nature a particularly favorable *locus standi* for almost any sort of marine biological activity. So the ground was prepared for a determined effort to create a permanent, well supported seaside station, presumably to be located at San Pedro. This effort took definite shape in the summer of 1901.

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 in 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 in assurance that with the united effort of such varied interest the securing of needed funds and facilities would be possible.

IV. SAN PEDRO, 1901–02.

The form which the ideas of marine biological research and a marine station had taken by this time has been stated in various published utterances by the present writer and his coworkers during the last decade; nevertheless these ideas are not sufficiently familiar either to professional biologists or to the general public to make their restatement superfluous.

The report made to the President of the University of the efforts put forth during the summer of 1901 (Ritter, 1902a, p. 55) contains these words: "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

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California, even though no assurance could be had of being able to continue the work beyond this season." Another publication of about the same date (Ritter, 1902c) has this: "Researches in the life of the sea have been prosecuted widely and with great energy in recent years. We have learned much, very much from them. Perhaps the most important thing we have learned is what to do next. Now it is exactly the thing that should be done next that we here in California are, by the grace of a beneficent Providence, specially commissioned to do. Detailed, comprehensive, continuous and long-continued *observation* and *experiment*—these are the two golden keys that will let us farthest into the mighty arcana of the life of the sea. ... Who that is accustomed to the sea can fail to recognize that an ocean like that off southern California, where icy tempests never rage and where torrid heat never enervates, must be exactly the sort of ocean where observations and experiments of the kind specified could be best carried on? ... The future marine station, particularly the California station, must be planned for physical, chemical, and hydrographic, as well as for strictly biological research. ... The work must go on every hour of the day, and every day of the year."

There being no funds available for putting these large conceptions into execution, and no prospect of securing such beyond the limited amounts that might possibly be contributed by the University and a few well-to-do friends of science and of the University, only the smallest and crudest beginnings could be made. But even a beginning would be something, and confidence in the soundness and ultimate workableness of an idea gives a sort of magnitude and robustness to what is initially small and weak. After having gathered together such sums as we could we went at the undertaking buoyantly, saying: "For the rest, like Elijah of old, we 'stand before the Lord' hungry but full of trust, and therefore expecting the ravens laden with bread and meat to appear at any moment" (Ritter, 1902c).

A grand total of about \$2000 was guaranteed partly by the University, but chiefly by individuals in Los Angeles, for the summer's work in 1901. It was decided to begin work at sea as well as to continue the collection and study of shore life carried

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on more or less interruptedly in previous years. In view of the meagerness of funds the plan was to limit operations on the biological side to dredging and trawling in depths not exceeding one hundred fathoms. On the hydrographic side no more than temperature and density determinations were attempted. The idea of making the investigations

"continuous and long continued" was strongly entrenched in all our minds. It seemed that the particular locations at which collections and observations were made should be carefully determined and recorded. Persons competent by reason of training, knowledge and sympathy to look after the several scientific interests would be basal to success in such an undertaking. On the biological side there was the regular staff of the department of zoology and two graduate students, Miss Alice Robertson and Mr. C. O. Esterly, who while then at the beginning of their careers in biology have remained identified in one capacity and another with the station, and have contributed in no small way to its scientific accomplishments. Professor W. J. Raymond, of the department of physics, had charge of the hydrographic work and made himself doubly useful from his interest in and extensive knowledge of the conchiferous mollusca of the Pacific Coast.

For a laboratory building a little old bath-house on the sand spit separating San Pedro Bay from the sea was rented and reconstructed. An open gasoline launch, the "Elsie," forty feet long, with an engine of 15-horsepower, was hired for the work at sea. A hand-winch at which four men could work simultaneously was provided for hauling the dredge and trawl. A sounding machine and an apparatus for taking sub-surface samples of water were improvised, the funds not permitting the purchase of even the least expensive regularly manufactured articles. Two months' work, from May 15 to August 15, were done. The boat "Elsie" was very active during the whole period. A total of 85 stations were occupied, to many of these numerous visits being made. The chief localities explored were off San Pedro, around Catalina Island, and at San Diego. Although research was avowedly the primary aim, it was determined to offer a few courses of formal instruction for elementary students. Two considerations dictated this. One, the more weighty it must

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be confessed, was the hope that the small fee charged would yield enough to meet the traveling 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 work. The other consideration was the genuine belief that the advantages of so favorable a natural opportunity for instruction ought to be used. The plan was to conduct the school as a part of the summer session of the University, the term of which was six weeks. The teachers were to be paid on the same basis as the University of California men teaching in the summer school at Berkeley. A total of thirteen persons paid the fees, and this number was quite the limit for which there were laboratory accommodations.

Owing to the failure to get even as large a fund for the work during the summer of 1902, exploration at sea was not resumed the second year. The laboratory was operated on the same basis as the preceding year, instruction again being conducted as a part of the regular summer session of the University. Except for the absence of Professor Raymond the investigating personnel of the laboratory was essentially the same as in 1901. Perhaps the most significant effort made during the summer of 1902 was to place the station on a larger, more secure financial basis. Past experience, particularly of the last year, had removed any lingering doubt of the natural attractiveness of the San Pedro region for seaside work. Through the encouragement and active assistance of several prominent citizens of Los Angeles, a plan was devised for securing \$20,000 with which to erect and equip a permanent laboratory building and secure a boat adequate for at least as extensive explorations at sea as had been carried on by the "Elsie." The Los Angeles gentlemen most actively interested and who had contributed considerable sums to the work already done were Mr. J. A. Graves, Mr. H. W. O'Melveny, Mr. Jacob Baruch, and Mr. J. H. Shankland. The plan was to secure the amount, or as near it as possible, among the foremost business men and firms in the city, in sums of \$500, the canvass to be made by a committee of business men each working among his own business acquaintances and associates. The subscriptions were made contingent on nearly the whole

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being raised. Considerable time was devoted to the effort during the summer, not only by some of the gentlemen of the committee but by the writer of this report. The attempt was unsuccessful, the subscriptions having reached only about one-third of the required amount. This experience furnished to the friends of the enterprise 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.

Much attention was given to the question of where, in or near San Pedro harbor, a permanent laboratory should and could be located. The outcome of this was a disquieting recognition of the rapidly growing commercial importance of the harbor and of the probability that a large urban population would be gathered around the little bay in a few years. This realization was disquieting not from any hostility to commercial development, but from a prevision of the inevitable destruction of some of the best collecting grounds in and about the harbor; and of the contamination of the sea-water by sewage and other incidents of industrial activity. These considerations were weighty in determining the next move.

V. CORONADO, 1903-04

As already indicated, the "Elsie" had made a run to San Diego during the summer of 1901 and had done considerable work there. The trip was planned largely with a view to obtaining information concerning the general conditions of that locality from the standpoint of marine research. Report from trustworthy sources, strengthened by cursory observation by the writer in the summer of 1891, was to the effect that San Diego Bay and vicinity presented many natural advantages for such an enterprise as we were occupied with. To leave this region unvisited would be contrary to the original idea of making a reconnaissance of the California coast before settling anywhere permanently. Professor Kofoid, who had charge of the 1901 expedition to San Diego, returned to San Pedro enthusiastic over the biological merits of that more southern locality. Besides he had been interviewed by a citizen, Dr. Fred Baker, who insisted that the San Diego region was probably the proper place against all

others on the California coast for a laboratory. Correspondence was entered into with Dr. Baker, resulting in arrangements whereby a sufficient sum was guaranteed by San Diego citizens to insure the resumption of explorations at sea, not only for the summer of 1903 but for the Christmas holidays following. In addition a laboratory more commodious and well appointed than the one occupied at San Pedro was provided. With the enthusiastic and efficient assistance of Dr. Baker, who from his profession and his interest in conchology was something of a biologist, the removal of the equipment from San Pedro to San Diego was made, laboratory quarters fitted up in the boat-house of the Coronado Hotel Company at Coronado, a small schooner, the "Laura," was rented and outfitted with the meager apparatus in our possession, put in charge of an intelligent Portuguese fisherman, Manuel Cabral, and set to work for six weeks during June and July.

During the first summer at Coronado the business affairs of the laboratory were handled by the Chamber of Commerce of San Diego. This was, however, regarded as an arrangement for immediate exigencies only. Something more specific and workable would have to be devised without delay. Only two courses seemed open. One was to continue to treat the enterprise as part of the department of zoology of the University of California and transact the business through the office of the Board of Regents of the University. The other was to form a local organization of some sort. The difficulties in the way of the first course, arising from the remoteness of San Diego from Berkeley, were sufficiently manifest from past experience. A local organization was really the only alternative. Accordingly the Marine Biological Association of San Diego was created in the fall of 1903 and duly incorporated under the laws of the state soon thereafter. The association consists of a general membership with a board of directors elected annually. The articles of incorporation contain the provision that the station should later be transferred to the Regents of the University of California at the option of the association. (See Appendix A.) The first officers of the association were Mr. H. H. Peters, president; Dr. Fred Baker, vice-president; Mr. H. P. Wood, secretary;

Mr. Julius Wangenheim, treasurer; Professor William E. Ritter, scientific director; and Miss E. B. Scripps and Mr. E. W. Scripps, members of the board of directors. With the exception of the first president and secretary, who soon moved from San Diego, the personnel of the board of directors has remained the same, with Mr. F. W. Kelsey and Mr. W. C. Crandall as secretaries, and Mr. H. L. Titus as vice-president and counsel, Dr. Baker having been president since the second year.

The experiences of 1901 had shown that with such a limited boat equipment bottom and plankton work could not be well combined, and that on the whole collections of the swimming and floating organisms could be made more efficiently and no less profitably from the scientific standpoint. But since the entire realm was practically virgin and it was important that the little we could do should be made to produce as much as possible, it seemed best that plankton studies should mainly occupy us for the time being. It was soon seen, though in a rather shadowy way, how absorbing and indefinitely expansive the problems of this side of marine biology would become. Two summer and two winter periods of telling work were done from Coronado as the base of operations.

The importance of extending the work, particularly the collecting over a larger portion of the year, was coming to be more and more clearly seen. This desideratum was partially met in 1904 by arranging with Mr. Cabral, the fisherman-collector, to run his own fishing-boat, the "St. Joseph," three days a week during the entire summer period and at intervals during the remainder of the year; and by creating the position of Resident Naturalist, i.e., a post at the laboratory to be continuously held by a trained biologist. The first incumbent of the position was Mr. B. M. Davis, who was willing to accept it on the small salary that could be paid with the consideration that half his time might be devoted to his own studies. Mr. Davis was the naturalist for one year only, his incumbency terminating with regret on both sides from his having been called to a position of greater responsibility and compensation.

No formal courses of instruction were offered after the San Pedro period. This aspect of the enterprise was abandoned, not

without regret, partly because of the growing absorption of the instructors in their investigations; partly because of relief from the necessity of depending on students' fees for paying the expenses of the investigators; and partly from the conviction that, all things considered, instruction was of less moment than investigation.

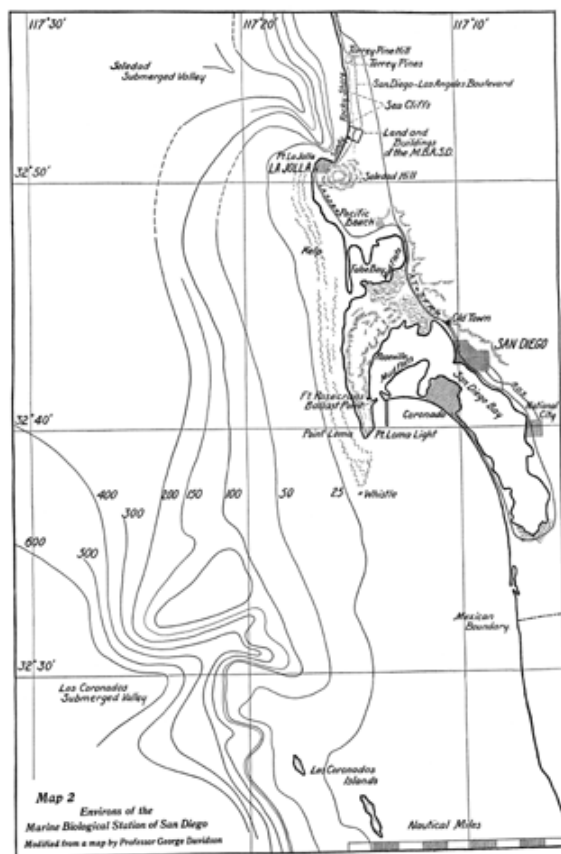
On the side of material support a matter of utmost importance in connection with the removal to San Diego was the keen, intelligent, and financially liberal interest taken in the station from the outset by Miss Ellen B. Scripps and Mr. E. W. Scripps. Although a considerable number of citizens of San 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.

In planning the laboratory at Coronado some of the serious difficulties that had been encountered at San Pedro soon came in sight. The location on the bay side of the long narrow sandspit that separates San Diego from the ocean was admirable in many ways. Boats could be landed and kept with great ease and safety; water for laboratory use could be dipped up and carried into the building so readily as to make a pump almost superfluous; and various species of marine organisms could be collected fresh, vigorous, and in quantity at our very door. But the water of the bay and so the organisms inhabiting it were very different from the water and the organisms of the open ocean; and the safe and easy landing was a good two hours' run with a motor-boat from the ocean. The more the problems of the biology of the ocean proper grew in clearness of definition and in interest, the more serious were seen to be the disadvantages of such a location. Consequently early in the Coronado period the idea

of abandoning the attempt to find a satisfactory inside location and of turning to some point on the open coast where oceanic conditions prevail as near shore as possible began to be considered.

VI. LA JOLLA (VILLAGE), 1905–09

After studying the coast as carefully as possible, La Jolla, a suburb of San Diego situated on a point fifteen miles to the north, was selected as on the whole the most advantageous place. The question may be asked, Would it not have been possible to secure a site within the bay at some point near the entrance, so that the protection afforded by such a location could be had in combination with the requisite of good ocean water and ease of access to the open sea? As a matter of fact a site at Roseville or elsewhere on the lower bay was much talked about, and was favored by some members of the board of directors of the association. As a partial exhibit of the way the decision was reached, I appended a tabulation of advantages and disadvantages of the main claimants that was drawn up during the summer of 1904 (see Appendix E).



[Full Size]

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So it came about that preparations for the summer's work of 1905 were begun by transplanting the station from Coronado to the place in most ways unsurpassed in natural charm by any on the California coast. Here the station was first housed within walls of its own construction and possession. Under the leadership of Dr. Baker a local committee of La Jolla citizens raised by subscription the \$1000 necessary for erecting the laboratory building. Permission was gained from the City Council of San Diego to locate the laboratory on a piece of ground on the water front that had been given to the city for park purposes. The building was ready for occupancy on the arrival of the University party on June 19, 1905. Though the structure was small and simple, it was far superior to any residence the station had before occupied. For the first time the luxury of an abundance of running water, fresh and salt, was enjoyed.

The building was 60 feet long by 24 feet wide. It contained three laboratory rooms of equal size, 12 by 20 feet, on the north side; on the south side a small library room, a still smaller reagent room and a public aquarium-museum 45 feet long and 12 feet wide. In this room aquaria containing as ample a representation of the local marine fauna as our limited facilities could procure and accommodate were kept open to the public; also a fair exhibit of preserved specimens. A collection of mounted specimens of the local sea-weeds, the gift of Mrs. Snyder, a resident of La Jolla, was likewise for use by the public.

The scientific staff during the summer sessions averaged fifteen persons on the payroll and from four to six visiting investigators.

A few of these visitors remained several months at a time. The winter sessions were irregular and brief with a much smaller attendance, as the University vacation was too short to permit many to make the trip to La Jolla. Each summer a series of free lectures on scientific subjects more or less popularized was given. For these not only our own staff but the visiting scientists were pressed into service.

This cheaply constructed laboratory was built in full confidence that it would have to serve only a few years before it would be replaced by a permanent, commodious one. The question consequently of a piece of land that should be the final resting place of the station after its long wanderings was taken up soon after the removal to La Jolla. For a time the little park already occupied seemed to be the most desirable. It was soon found, however, that while the city council was quite ready to grant the association certain privileges, it had no power to do so under the existing laws regarding park lands. This difficulty was overcome by securing from the state legislature an act enabling the city to grant the association the rights sought. By the time these were obtained questions had arisen as to the satisfactoriness of the location. 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.

VII. THE FINAL LOCATION

To Mr. E. W. Scripps belongs the credit of proposing what at first flush seemed an extravagant if not an altogether impracticable solution of the problem. The city of San Diego is a large landowner, its possessions being a heritage from the Mexican regime before California became a part of the United States.

The land is situated in the extreme northern portion of the city, several of the "pueblos" fronting on the ocean. The southern-most of these water-front pueblos is about two miles from the northern confines of the village of La Jolla and another quarter of a mile from the nearest railroad service. Mr. Scripps' plan was to secure this pueblo "No. 1289," consisting of nearly one hundred and seventy acres, as a site for the station. The tract has an ocean front of approximately one-half mile, of which about one-third is available for any buildings, piers, breakwaters and so forth, that the station might need. The remainder of the frontage being a sheer cliff of from fifty to two hundred feet could not be used as a site for station buildings proper, but a considerable area of rocky shore at the foot, including a fine trap-dyke reef, would be a valuable asset as a collecting ground and as a source of rock for building purposes.

In spite of the obvious difficulties that would attach to such a situation because of distance from sources of supply of both domestic and laboratory materials, after much deliberation it was resolved to take the step, provided the land could be secured. The city authorities were found to be well disposed toward the station, as indeed they had been at all times when occasion had arisen to seek aid from them. Willingness was expressed to give the tract to the association could this be done under the law. Since no way of alienating the land was allowed by the city charter other than by selling it at public auction to the highest bidder, such an auction was arranged for by the city council, the understanding being that a bid by the Biological Association at a nominal figure would be acceptable to the city. This course could be legitimately taken because of the right of the municipality to reject all bids. The project having been well discussed before the city council, in the newspapers, and with the dealers in real estate, very generously no bidders other than the association appeared at the auction. Thus this fine tract of land came into possession of the association nominally for \$1000, but with the understanding that Miss Scripps would expend \$10,000 in building a public roadway through this land and other lands belonging to the city. The deed of trust is without restriction, the city officials placing full confidence in the association for the

performance of its duties as a trustee of public property. The transaction was closed in August, 1907.

The tract was made accessible during the same summer by the construction of the boulevard which was a part of what later became the main highway between San Diego and Los Angeles. This piece of road, built by Mr. E. W. Scripps mostly at the expense of Miss E. B. Scripps, as above mentioned, was of far greater significance to the station than the mere furnishing of an easy access from the village of La Jolla. Opening up as it did the whole ocean-side area between La Jolla at the south and Del Mar at the north, and passing through the unique and much favored Torrey Pine Grove, it has been a great factor in bringing to public attention the beauty of the region and the natural productiveness and utilizability of the lands. Further-more, the ascent from sea level to the three hundred-foot elevation of the mesa being through the station's tract, the necessary tortuousness of the road causes it to reach a large portion of the land.

Thus was settled the long and earnestly considered question of just where on the face of the earth the final home of this biological enterprise should be. But the home itself was still far from an accomplished fact, though a long, sure step toward it had been taken some time before when Miss Scripps had notified the board of directors during the fall of 1905 of her decision to place \$50,000 at its disposal, chiefly for building purposes, but also for whatever use the best interests of the station might demand. The main building was to be a memorial to a deceased brother, George H. Scripps. Soon after the settlement of the site question, planning for the first permanent building was turned to afresh.

The difficulties in the way of actually looking after the affairs of the station, incident to the fact that none of the scientific staff upon whom fell the chief responsibility could be at La Jolla more than a few weeks or at best months at a time each year, were becoming more and more apparent to all, but to none more than to Miss Scripps and Mr. Scripps. Nothing had so strongly emphasized the need of the constant presence at the scene of operations of a responsible scientific head as the preparations for

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building. To meet the needs Miss Scripps decided early in 1909 to so endow the station that its affairs could have most of if not the whole time and energy of the scientific director. Accordingly Dr. Ritter took up his residence at La Jolla on June 1, 1909, the arrangement between the Biological Association and the University being that he return to Berkeley each year to give one or more brief, concentrated courses of instruction, the station paying two-thirds and the University one-third of his salary.

A supply of fresh water being an indispensable prerequisite to development of any kind on the new possessions, the city was again appealed to and readily undertook to lay a four-inch pipe from La Jolla to the building site. Construction of the first building began in the early summer of 1909 and was so far completed during the next twelve months that soon after the director's return in June, 1910, from his first year's engagement at the University under the new regime, it could be occupied. As a makeshift settlement of the domestic problem involved in the remoteness of the laboratory from the village, it was decided that the director's family should domicile itself for the present on the second floor of the laboratory building, the first floor furnishing ample space for the scientific work then being done, and the director's wife being equal to the task of transforming the laboratory rooms into quarters not only possible but comfortable for human habitation. So far as the transportation problem was settled, it was settled by an automobile for the director, bicycles for those of the staff who must still live in the village, and two good feet for every one who upon occasion should be choice revert to nature's first solution of the problem of locomotion for man.

VIII. WORK AT SEA

For the sake of consecutiveness in narration nothing has been said about the portion of the station that lives at sea since note was made of the earlier provision for this at the beginning of the San Diego period. During the summer of 1904 Mr. E. W. Scripps had placed his pleasure yacht "Loma" at the service of the station, and in the fall of the same year he gave the vessel

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to the association, accompanying his gift with another of \$1500 toward fitting her with a propelling engine and scientific gear. She was taken to San Francisco, refitted and returned to San Diego in time for the summer's work of 1905. This was the largest, most efficient boat that had thus far been in the service, and the first one owned by the station.

The "Loma" was originally built for a pilot boat and therefore was sturdy and suited to dredging and trawling as well as being seaworthy for considerable distances from shore. Our area for work being outlined roughly by Point Conception on the north and the Santa Catalina, San Clemente, and Coronado islands on the west, stations for repeated observations were established at many points within this area. A more detailed account of these and how the work at sea is carried on will be given in another place. The "Loma" was wrecked near the lighthouse on Point Loma in July, 1906, while attempting to make the run from La Jolla to San Diego Bay between the shore and the kelp beds that skirt the coast. The mishap being due to striking a shore rock and not to a storm, the boat was completely dismantled and nearly everything saved.

The gift of \$50,000 by Miss Scripps being available for equipment at sea as well as on land, planning for a new boat was begun soon after the loss of the "Loma." Although construction of the new craft was commenced in what was supposed to be ample time to assure her readiness for the summer's work of 1907, delays in securing material put off the launching until August 16. Resort was had, consequently, to Captain Cabral and his fishing boat, the "St. Joseph," for sea work that year. The description of the new boat, the "Alexander Agassiz," is furnished by her present master, Captain W. C. Crandall.

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E. FUNDS AND GENERAL RESOURCES

It is not proposed to deal with the finances after the manner of a treasurer's report. Rather the aim is to give an outline of the station's business career in such fashion as to make this tell as much as possible about the progress and policies of the enterprise. Likewise as to general resources, in some cases only estimated values are given and in a few none at all, even though it seems desirable that the articles themselves should be mentioned. It is particularly difficult to treat the library on a strictly financial basis.

The headings under which a presentation having such an end in view may best be made are: (1) Sources from which funds and appliances have come; (2) The main items of expenditure; (3) The present valuation of the station's holdings; (4) The present income.

1. Sources and amounts:

(a) University of California—

Money for general expenses, 1892–1901

\$2,000

Publication, 1902 to date	8,000	
	-----	\$10,000
(b) San Pedro period, 1901-02—		
Citizens of Los Angeles	\$1,570	
Students' fees	350	
Mrs. Phoebe A. Hearst	500	
	-----	2,420
(c) Coronado and early La Jolla period, 1903-06—		
Citizens of San Diego and La Jolla (exclusive of Mr. E. W. Scripps and Miss E. B. Scripps)	\$1,750	
	-----	1,750
(d) E. W. Scripps—		
Cash, 1903 to 1911	\$13,000	
Yacht "Loma" (estimated)	2,000	
	-----	15,000
(e) Miss Ellen Browning Scripps, cash, from 1903 to 1911		67,000
(f) Mr. Alexander Agassiz, nets and apparatus, 1905		1,000

Total		\$97,570
—		
166 (g) Gifts to Library—		
—		
United States Government about 300 volumes from National Museum and Smithsonian Institution, Department of Agriculture, Treasury Department, Department of Commerce and Labor.		
American Association for the Advancement of Science, 60 volumes		\$90
Alexander Agassiz, Memoirs of Museum of Comparative Zoology, 15 volumes		150
William E. Ritter, scientific journals, 104 volumes		400
University of California, set of Challenger Expedition Reports, 45 volumes (indefinite loan)		550
Walter Lieber, Encyclopedia Britannica, 9th edition, 28 volumes		90

Total		\$1,190
2. Main items of expenditure:		
(a) Professional service	\$22,000	
(b) Boats, building and repairs	21,300	
(c) Boat labor, crew and scientists	6,000	
(d) Expense of running boat	3,500	
(e) Buildings	25,000	
(f) Publication	8,000	
(g) Land	1,000	
(h) Scientific apparatus (laboratory and boat)	2,500	
(i) Non-professional labor	1,600	
(j) General expense	2,600	
(k) Library (journals, binding and purchases)	1,100	
	-----	\$95,000
3. Present valuation of properties:		
(a) Land (estimated)	\$100,000	
(b) Buildings	24,000	
(c) Boat, "Alexander Agassiz"	10,000	
(d) Scientific apparatus	2,500	
(e) Library	3,000	
	-----	\$139,500
4. Present income:		
Miss E. B. Scripps, 6 per cent interest on \$150,000 legacy	\$9,000	
E. W. Scripps, annual contribution	1,500	
	-----	\$10,500

F. THE STATION AS IT IS TO-DAY

I. LAND

The considerations that most influenced the board of directors to endure the present disadvantages of isolation of the new location were the certainty for all time of ocean water uncontaminated by human habitations, and the

ample elbow-room assured whatever developments might take place. Not much less weighty with some of the board was the belief that at no distant future time the land not actually used by the station would become a rich income-producing endowment. This belief would appear chimerical to a sober-minded person who should contemplate it without taking into account the nature and extent of the demand, as evidenced by events of the last decade and a half, for seaside lands on the coast of southern California. The recent rapid increase in population and wealth that has taken place at various points along the coast, particularly in the vicinity of Los Angeles, can be interpreted in no other way than by assuming that the climatic and other natural conditions of the region are as positively a material and business asset as the prairies of the upper Mississippi Valley and the Great Lakes are an asset of Chicago, and as New York Bay and the Hudson River are an asset of New York City. Once this is seen, it becomes clear that the country tributary to this region in a business sense, that is, available for natural exploitation, is nearly the whole United States. The general fact of the demand for homes in this region because of climatic conditions, taken along with the special ones that the station's property is certainly not surpassed in natural attractiveness by any on the whole coast, and that the developments of the San Diego region are assuring ready accessibility and the many other advantages that appertain to a large concentrated population, constitute the chief basis of the belief.

Looking at the situation in the broadest way, taking account of physical, industrial, social, and intellectual tendencies and possibilities, as well as of the scientific purposes of the station,

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it may be confidently held that every dollar put into the upbuilding of the institution as an instrument for scientific research and general enlightenment may be made to count also as investment for enlarging the income applicable to the scientific work.

II. BUILDINGS

The first permanent building, known as the George H. Scripps Memorial Building (pl. 18, fig. 1), situated sixty feet from the edge of a fifteen-foot sea-cliff, is a plain rectangular two-story structure of reinforced concrete, 26 feet high, 75 feet long and 50 feet wide. Its long axis is perpendicular to the water front and runs east and west. The flat, parapeted roof carries two large iron-framed skylights, one over the corridor, the other over the museum and lecture room. The only other buildings so far erected are a tank-house located about twenty-five feet north of the northwest corner of the laboratory, and a small frame building (20 by 30 feet) which serves in part as a store-room for miscellaneous bulky material, and in part for housing an automobile.

On the ground floor of the laboratory is a corridor 12 feet wide running from the east to the west entrance. Along the north side of this corridor are six rooms (12 by 17 feet each) for investigators. These rooms occupy the entire length of the building. South of the corridor is an aquarium room (17 by 37 feet) occupying the southwest corner; a dark-room (8 by 8 feet) which opens from the east end of the aquarium room; a shop (12 by 8 feet) also opening from the east end of the aquarium room as well as from the corridor and men's toilet; a store-room for reagents and glassware (15 by 16 feet); the janitor's room (9 by 10 feet) occupying the southeast corner, and between this and the east entrance a lavatory for women. Ten feet west of the east entrance concrete stairs ascend from the lower corridor to the second floor leading directly into another corridor (12 by 37 feet), along the north side of which are three investigators' rooms exactly similar to those of the ground floor. At the west end of this corridor is a museum and lecture room (32 by 37 feet), along the north side of which are three more investigators' rooms. South of the corridor and occupying the southeast corner

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of the building is the library (17 by 25 feet); while adjacent to it and opening from the corridor at the head of the stairs is an apparatus and glassware room (12 by 17 feet).

The furniture of the investigators' rooms is simple. It includes a U-shaped table running across the north and half way down the east and west walls, two and a half feet wide and provided in the middle with a chest of five drawers, and with one drawer on each wing. On the east and west walls are a set of book shelves, a large specimen case, a cloak locker, and a "wood stone" table 2 feet 2 inches by 6 feet 3 inches adjacent to a sink, and a two-story aquarium on the south side. These rooms, in common with the rest of the building, are supplied with gas, fresh water, and electric lights. At present they are heated with stoves, although provision is made for gas grates. While thus simply equipped these rooms were planned to yield the greatest convenience and satisfaction as scientific workshops. The common difficulty in controlling, for microscopic purposes, the extreme variations in light intensity is overcome by the very simple device of admitting only north light to the microscope's reflector through three large windows (2 feet 6 inches by 5 feet 8 inches each). Again the usual awkward and troublesome manner of filing and caring for one's working collections is, we believe, satisfactorily overcome by the construction and arrangement of the specimen cases. Similarly the salt-water aquarium is constructed and arranged to allow ease of observation and experimentation as well as illumination from all sides.

The specimen case, constructed of well-seasoned Oregon pine, is 7 feet 3 inches high, 3 feet wide, and 2 feet deep. The insides of its lateral walls are provided with cleats and upon these cleats moveable shelves 1 inch thick are placed, the distance from one shelf to the next varying according to convenience. Upon each shelf rest two trays 15 by 21 inches, in which the bottles of specimens are kept. This arrangement is advantageous from two points of view, (1) bottles of any size may be filed by merely readjusting the distance to the shelf above, and (2) in having two trays instead of one on each shelf, thereby making them lighter and easier to handle. The case is rendered dust proof by a glazed hinged door, which is locked by

two circular window catches, one a third the distance from the top and the other a third the distance from the bottom of the door.

The aquarium (pl. 21, fig. 4), after plans by Professor Kofoid, in each investigator's room consists of two stories. The upper one is rectangular, with inside measurements of 37 inches in length by 17 in width and 13 in depth, and is elevated so that its base is 4 feet 9 inches above the floor. Its walls consist of half-inch plate glass 10 inches high and 35 and 14 inches wide at the sides and ends respectively, which glass is supported in a reinforced concrete frame 2.5 inches thick. The lower aquarium, rectangular in shape and constructed entirely of reinforced concrete 2.5 inches thick, is of the same length and depth as the other aquarium, but about nine inches wider. A wooden shelf resting upon concrete cleats is located intermediate between the two aquaria and the salt-water system is so arranged that small moveable aquaria may be operated thereon. At the front base of the upper aquarium is another smaller shelf, supported upon iron cleats, which may be used for glassware or experimental apparatus whenever desired. It is noteworthy that the plate glass of the upper aquarium is set directly into the concrete walls, thereby eliminating all metal rods and bolts. Another important feature is that the upper aquarium backs against frosted glass windows, thus making it possible to illuminate the aquarium from any desired direction. The salt-water supply pipes are of soft lead with vulcanite cut-offs and delivery cocks. Every main angle is provided with a clean-out plug. The waste aquarium water is carried away by a system of open drains in the cement floor.

The method of circulation within the aquarium is based upon the siphon principle, as follows (pl. 21, fig. 4): From a $1\frac{3}{8}$ -inch soft lead supply pipe (s) the water enters through a vulcanite stopcock into the aquarium inlet (i), which is merely a channel $\frac{7}{8}$ inch in diameter, in the concrete wall. As the water fills the aquarium it also ascends the outlet (o), which like the inlet is a $\frac{7}{8}$ -inch channel in the concrete wall, until it reaches an overflow level (l) about three inches below the aquarium top. This brings the siphon into action, and the water passing through

the descending end of the siphon (d), also a channel in the concrete, enters the lower aquarium. As the lower aquarium fills, the water after passing through a screen fitting in the slots (sl.) ascends an outlet (e) similar to that in the upper aquarium. After reaching an overflow level about two inches below the top of the lower aquarium, the water flows downward through the descending stem of the siphon (f) into the floor gutter (g). This gutter is $3\frac{3}{4}$ inches wide and, beginning in the most easterly investigators' room, runs in a straight line through the other five rooms, and conducts the water down a gentle slope through an exit at the west end of the building into the ocean. The gutter is installed only on the ground floor, the plan for the second story not being completed as yet. This scheme of circulation very simply and effectively solves many difficulties. The water enters and leaves at the base of each aquarium; the pipes when clogged can be readily cleaned through their orifices (m). Overflow is not likely to occur except in case of stoppage of outlets and absence of the investigator from the room. Even if, in an extreme case, water should overflow onto the floor, no damage would be done, for the cement floor is constructed like the deck of a ship, so as to slope toward the gutter. With this system a constant level of water is maintained in the aquaria in such manner as to afford adequate circulation and simplicity in cleaning.

The excellently lighted aquarium room is supplied with two floor tanks used as aquaria for large animals. Each may be partitioned into two, in which case the water enters one compartment by flowing over the partition from the other. The smaller tank is 5 feet 2 inches long, 3 feet 4 inches wide, and 5 feet deep. The larger is 9 feet 3 inches long, 6 feet wide, and 5 feet deep. The bottom in both is 16 inches below the floor and both are concrete, with walls $4\frac{1}{2}$ inches thick. In addition to these floor tanks the aquarium room is provided with four tables for serial aquaria. Each table is supplied with five or more rectangular aquaria 31 inches long and 21 inches wide, arranged in succession according to height, the highest being 15 inches, the next adjacent being 2 inches lower, and so on. They are made of half-inch plate glass supported by galvanized

iron frames, and rest upon either concrete or soft lead bases. Each aquarium is provided with an inlet pipe and bottom outlet. Each may therefore be used as a unit, or by plugging the outlet and siphoning from one aquarium to the next, serial circulation may be maintained. The outlet in either unit or serial arrangement leads into a concrete floor tank beneath the table. This tank extends the entire length of the table, but is constructed so that it may be partitioned into five compartments in which serial circulation may be maintained by allowing the water to flow from one compartment over the partition into the next. The final outlet is similar to that in the investigators' rooms, the water flowing by siphons into gutters and thence out of the building.

The remaining rooms need scarcely more than a passing word. The dark-room is supplied with sink, running fresh water, electric lights, and the usual equipment pertaining thereto, the walls and ceiling being black. The reagent and glassware rooms afford ample space for storage. They are equipped with numerous lockers, some with glass and some with wooden doors so arranged as to yield the most space compatible with convenience. The museum-lecture room is not yet equipped, and the library room is used as such only temporarily until more adequate quarters may be obtained. It is provided with adjustable wall shelves utilizing all the space not taken up by windows, the door and a fireplace.

The tank for the salt-water supply to the laboratory aquaria is a cylindrical structure of concrete reinforced by the so-called "high-rib" steel framework of the Kahn system. It is 16 feet high and 16 feet in diameter, and has a capacity of 20,000 gallons. It is partitioned into halves, either one of which may be used independently of the other. In addition to the usual outlet pipe the tank is provided with another to permit drainage and cleaning. The tank is supported by an octagonal, two-story reinforced concrete building 24 feet high, intended to house the pumping plant when installed.

Great difficulty was experienced in making the tank watertight. The contract was let on the assumption that the concrete walls would be strictly impervious. Whether from faulty design,

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construction, or material is uncertain, but the tank leaked badly at first and no way was found of remedying the trouble except by resort to asphaltum for an inside dressing. This was applied by the membrane method, i.e., by alternating layers of asphaltum and burlap. Leakage was entirely stopped in this way. Whether sea-water in contact with these substances will become noxious to the sensitive organisms of the plankton remains to be determined, but there are good grounds for hoping it will not.

In the light of what we now know, apparently it would have been better had the building of the tank been deferred for a year or two.

III. SCIENTIFIC EQUIPMENT OF THE LABORATORY, AND LABORATORY METHODS

Mention has already been made of the fact that from the beginning the station has depended to a considerable extent on the department of zoology of the University of California for apparatus. This has been particularly true as regards microscopes. Gradually as finances have permitted, this dependence has been and is being overcome. Passing by the instruments, glassware, reagents and so on common to every biological laboratory, reference need be made only to such things as are of interest because of the special work done and methods employed.

On the biological side mention should be made of the importance of the Zeiss-Greenough binocular microscope in identifying, sorting and enumerating the vast numbers of organisms too small for the unaided eye or ordinary simple microscope, and too large for the compound microscope. Reference ought also to be made, though details are impossible, to the various devices employed in counting, measuring, and weighing organisms, and in recording data in the laboratory work. To a considerable extent these have been described in the several technical papers setting forth the researches in which they have been employed. They are spoken of here but without details to impress upon the reader the great but still subordinate importance of these aids to research.

The equipment and methods used in the laboratory work on

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the hydrographic side being much less common, especially in the United States, than are those on the biological side, may be spoken of in somewhat more detail. The apparatus in use comprises:

1. One Ainsworth balance accurate to a tenth of a milligram, primarily for weighing water samples;
2. Three Knudsen burettes and three Knudsen pipettes for chlorine determination by titration;
3. One set of fourteen Aräometers, from R. Küchler, Flmenau, graded readings from 1000–1007 to 1024–1031, for specific gravity determinations;
4. One set of weighing bottles, Guy-Lussac model, for weighing water samples;
5. One Fox gas analysis apparatus for extracting the gases from sea water.

It will be seen by this list of apparatus that the laboratory is prepared to determine the density of the water by three methods. All of these are used more or less, partly for checking one another as to accuracy and partly for facilitating labor. Where thousands of determinations are made, as in this case, rapidity and inexpensiveness are important as well as is reliability. After much comparing the weighing method has come to be most used, it being undoubtedly the most trustworthy. The original cost of the apparatus aside, it is less expensive and almost as rapid as the titration method. The hydrometer method, though most rapid and least expensive of the three, is also least reliable. Reports have come to us from some of the European laboratories of twenty-five determinations an hour by the chlorine method. We have not been able to reach such a speed. The best we have attained is about fifteen per hour, and this rate can be reached in weighing quite as well as in titrating.

The problem of the gaseous content of sea water is undoubtedly very complex both actually and manipulatively. In the opinion of competent chemists the Fox method of extracting and measuring the several gases is probably the most effective so far devised. The apparatus used is however rather complicated. and although Mr. Burbridge, who alone has thus far manipulated

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it, has seemingly made it do as good work as it is capable of doing, exactly how valuable the results will be from the biological standpoint is yet to be seen.

IV. THE "ALEXANDER AGASSIZ," ITS SCIENTIFIC EQUIPMENT AND METHODS OF WORK

1. *The Boat Itself*

The loss of the station's first boat, the "Loma," in 1906, necessitated the building of another. A prime aim in designing the new craft was to make her capable of working in the shallow waters of the bays and close-in-shore areas, as well as in any part of the Pacific Ocean that had been roughly laid off as the "San Diego Region." It is shown in an earlier publication (Ritter, 1905) that this area is a triangle having a shore line boundary of about two

hundred and eighty miles from Point Conception at the north to the limits of California and the United States at the south; a west side boundary in the open ocean of about one hundred and twenty miles; and a south side boundary, also entirely in the ocean, of about two hundred miles. The tract contains approximately twelve thousand square miles and is nearly coextensive with the continental shelf of this part of the coast. The west and south limits, being entirely arbitrary so far as the sea bottom is concerned, have to be extended somewhat in order that two very important features belonging to the shelf, namely Los Coronados Islands and Cortez Bank, may be included. Within the area occur depths of nearly eleven hundred fathoms which must be explored if anything like a comprehensive study of the whole is to be made. This indication of the duties that would devolve upon the boat makes it obvious that she would have to be of considerable capacity, not only for strictly scientific work but also for carrying men and supplies. She would have to be good for cruises of at least a week's duration, and the programme laid out would make it impossible for her wholly to escape heavy seas and rough weather. After much discussion among ourselves and conferring with seamen experienced in work more or less similar to that upon

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which we were engaged, it was decided that the boat should be wide and shallow in proportion to length, should be fitted for sailing as well as for motoring, should have a center-board instead of a deep keel, two driving engines instead of one, and plenty of stern overhang to insure the safety of the collecting lines and nets from the propeller blades. The plans presented by L. Jensen, a San Diego boat builder, were accepted and the contract let to him.

The "Agassiz" (pl. 22, figs. 5 and 6) is 85 feet long over all, is of 26 foot beam, and draws 5 feet of water. She is schooner-rigged, and as originally built was a "ketch," that is, a boat with deck area forward of the mainmast large and unencumbered, the wheel being placed behind the rear mast. Her foremast was at first 65 feet high, carrying a boom and large mainsail, and her mizzen-mast 39 feet, rigged with a boom. She has a spoon bow and a 15-foot overhang. As launched the deck was without superstructures except the two-foot decking of the cabin and engine house, these being separated by a narrow passage way. Below the main deck the space was apportioned as follows: The forecastle contained the galley, the chain locker, and a 110-gallon water tank. Immediately behind the forecastle came the cabin area divided in the middle lengthwise by the center-board box, into a captain's cabin forward on the starboard side, and a stateroom aft; and on the port side the mess cabin and lavatory. Separated from the cabins by a bulkhead is the engine room containing the two propelling engines, the main hoisting engine, and the reeling drum for the dredging cable. Behind the engine room is a lazaretto containing two distillate tanks of 460 and 230 gallons capacity and a 100-gallon gasoline tank.

The "Agassiz" began work in June, 1908, and the first season made it clear that her rigging was too heavy; that the wheel should be forward; that the scientific work should have better accommodations on the after deck; and that the galley was too small. Consequently the following year the mainmast was cut down 15 feet and reduced in diameter; both main and mizzen-sails were made lighter by changing them to the leg-of-mutton pattern; the wheel was placed in a pilot house immediately

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aft the mainmast; a naturalist's house was built on the deck behind the mizzen-mast; and the galley was enlarged by partitioning off a portion of the messroom. These changes greatly improved the vessel not only in seaworthiness but in comfort and in facilities for scientific work. As now arranged the "Agassiz" has sleeping accommodations for nine persons, there being two berths in the forecastle, two "Pullman" berths in the messroom, two berths in the stateroom, one in the captain's room, and two in the engine room.

The twin driving engines are gasoline, 30 horse-power each, and were built by the Western Standard Engine Company of San Francisco. The main hoisting engine is a five horse-power gasoline built by the Union Gas Engine Company of San Francisco. The large reeling drum and its spooling apparatus were designed by Mr. T. W. Ransom of San Francisco, a mechanical engineer, and were built by the Union Gas Engine Company. These three engines and the hoist are in the engine room. Recently a combination hoist and sounding machine run by a three horse-power gasoline engine designed by Mr. Robert Baker, machinist, of San Diego, has been added. This is placed in the naturalist's house on the rear deck. Unfortunately it is not available for work at this writing.

2. Handling the Boat and Heavy Gear

All scientific work requiring engine power is done aft. The sounding wire proper is paid out on the port side and is independent of the collecting gear, so that soundings may be taken while other work is in progress. The collecting cables are attached to the boom of the mizzen-mast and are run out over the stern. While this arrangement has the decided advantage of bringing together the scientific gear and the naturalist's quarters on the rear deck where there is most available space, it is not best for the handling of the vessel. The disadvantage, probably considerable for any type of boat, is specially accentuated with the "Agassiz." From her shallowness and shape of bow, her "foot-hold" on the water, as a land-lubber might say, is insecure. As a result, if not at anchor, she slips around badly when she attempts to lift or pull much else than her own weight,

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especially when, as is the case with the scientific gear, the weight is attached at a point considerably above the water line. These disadvantages are particularly in evidence when work is being done in rough weather or in a heavy sea-way.

Again the excessive stern overhang, while of undoubted advantage as regards the end in view which gave her this characteristic, namely, the protection of the cables and nets from the propeller blades, is far from advantageous in another way. In even a moderately rough sea when the boat comes to a standstill or to slow headway, as she always must to do her scientific work, the waves strike under the stern with a force that is surprising to one who has not considered the possibilities in such a case, and which becomes a rather serious obstacle to the operations. While admirably adapted to work in smooth and shallow water, at least as far as general design is concerned, she has not the right style of hull for open-sea, deep-water exploration. The truth is, a diversity of requirements, practically unrealizable in any one boat, was attempted in planning the "Agassiz." Experience has proved that while it might be possible to design a satisfactory open-sea boat that would answer rather well for shallow, smooth water, the most desirable craft for work of the last mentioned sort cannot be made that will operate most effectively in the open ocean. Since on the whole our enterprise is more concerned with oceanic problems than with those of landlocked and in-shore waters, we would have done better had we built a boat with greater draught, a keel instead of a centerboard, and a somewhat different form of bow and stern. The increased storage room that would have been secured by the deeper hull would have been an important gain, since as our problems define themselves with the progress of the investigation, it becomes clear that occasional cruises to greater distances from the home port and hence of longer duration, than was seriously contemplated at first, will be necessary.

It is not to be understood that the "Agassiz" is wholly unsuited to her task. A moment's reflection on what she really has accomplished and is all the time accomplishing is sufficient proof that such is not the case. She has two features that may be mentioned as particularly advantageous—her twin propelling

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engines and the slight elevation of her deck above the water. For the frequent turning about required, for overcoming drift, for holding slow headway, and for various other reasons, the two-engine plan is of great importance. Nearness of the deck to the water is advantageous not merely because it facilitates the putting out and hauling in of the heavy gear, the using of hand-nets, the launching of small boats, and so on, but because it makes possible many observations on organisms in the sea that would be precluded by greater elevation. This is of more consequence than at first thought would be supposed—more in fact than one would appreciate who had not had experience in such work on both high and low boats.

3. Scientific Equipment

In addition to the hoisting engines and drums already mentioned, the scientific equipment contains the following:

1. 1200 fathoms of $\frac{3}{8}$ -inch hemp steel cable for dredging and other heavy work.
2. 1080 fathoms of 3 mm. 28-strand galvanized wire for sounding and carrying the serial reversing water bottles.
3. 1200 fathoms piano wire for Thompson sounding machine.
4. One patent Thompson sounding machine, run by a friction wheel on the fly-wheel of the main hoisting engine.
5. One Kofoid horizontal closing net.
6. One small Nansen vertical closing net.
7. One large Nansen vertical closing net.
8. One Peterson vertical closing net.
9. One Kofoid five-gallon water bottle.
10. An ample supply of surface tow-nets of millers' bolting silk, mesh numbers 20, 12, 10, 9, and 000.
11. Numerous dredges and trawls of various sizes.
12. Six combined reversing water bottles and self-registering thermometers, Ekman model.
13. One propeller current-meter, Ekman model.
14. One meter-wheel.

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15. One hundred water-sample bottles for gas analysis, with cases.
16. Three hundred water-sample bottles with patent closure, with cases.
17. One electric lighted photometer for measuring the intensity of daylight.
18. A large supply of glass jars and carboys as containers of biological specimens and water samples.

Items 2, 6, 7, 12, 13, 14, 15, and 16 are the apparatus used in the international researches on the oceanic areas of northern Europe, and were purchased through the Central Laboratory of the International Commission at Christiana. Items 5, 9, 10, 11, and 17 were designed and constructed in San Diego and Berkeley. Items 1, 3, 4, and 18 are commercial articles and purchasable from dealers in such things. Item 8 was made for Mr. Alexander Agassiz, and given by him to the station.

4. Method of Procedure

Experience gained during the earlier years of general exploration of the San Diego region demonstrated that correlations between organisms and their environment are by far too complex to be revealed from data obtained by the usual methods of collecting. Variations in light and in currents, temperature, density, and gas-content of the water, as well as in many other factors, largely determine the vertical and horizontal movements of organisms and likewise their abundance at any particular depth at any particular time and place. Consequently a very careful plan of collecting would be required if the data obtained were to yield the information sought.

In preparing such a plan two fundamentally important considerations are involved. First, the plan must be workable from the standpoint of available equipment, for a plan admirably adapted to a large boat like the "Albatross" for example, would scarcely suit the "Agassiz." Not only must the size of the boat and the number of the crew be considered, but its speed, draft, and in fact its entire design must be taken into account. Again the need of accurate positions while collecting requires that

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locations be selected as far as possible, which can be fixed by accurate sextant observations on points of land. Second, the plan must be made with reference to some particular correlation problem or small group of closely interdependent problems, for it is obvious that all such problems cannot be investigated at the same time. The problems fall into a natural sequence so that those which are selected for investigation at a given time will depend upon work already accomplished.

Suppose we choose variation with respect to season as our problem. Obviously to compare the data obtained from one locality during the summer with those obtained from another locality during the spring, fall or winter, would come short of the best scientific procedure. Were comparison thus made, the variations observed in the kinds and abundance of organisms might be due to differences in locality and not to the effects of season. The first point of importance then, is to confine investigation to the same localities during all the seasons. Again, data obtained during daylight in summer would not reveal seasonal effect with certainty if compared with those obtained during twilight or darkness in the other seasons, because the variations noted might be due entirely to variations in light. Consequently collecting at one season must be done under approximately the same daylight conditions each day for all the other seasons. Similar remarks apply to problems of variation with temperature, currents, density, gas-content, and other hydrographic factors, as well as to a number of meteorological factors such as clouds, fog, rain, etc. However an important difference exists, namely, that while we can confine collecting to definite localities and times of day we cannot control or even foresee, except in a general way, what the hydrographic and meteorological conditions will be. Nevertheless it is necessary to eliminate the vitiating effect of the numerous variable factors if we are to determine the effects of season. This can be accomplished only by repetition of the observations under the same conditions, so far as the means of observing are concerned. To be adequate then, our plan must provide for frequently repeated collecting with the same apparatus, in the same depths, at the same localities, and at the corresponding time of day during all the seasons.

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From the information that had been secured by work done previous to making the plans here described, it seemed best to concentrate still more on the effect of season upon the kinds and abundance of organisms. Three localities were selected on a line extending approximately west of the south end of South Coronado Island. These three localities were taken for the following reasons. First, because of the ease with which sextant observations may be made in almost any kind of weather. Second, because of the bottom topography and its consequent hydrographic and biological interest, especially as affording opportunity for testing Ekman's theory of oceanic currents, and of determining the effect of upwelling water on the organisms. Third, because little time would be consumed in running from station to station, and from station to good anchorage, the farthest station, although in water having a depth of 700 fathoms, being within ten miles of the island.

The locality settled, the question of the time of day when work should be done was next considered. It was certain from observations already made that some organisms accumulate at the surface during morning and evening twilight, and occur in deeper water during daylight and darkness. Work ought to be done consequently during these four parts of the day. The "Agassiz" not being capable of accommodating two crews, work could not proceed more than twelve hours each day. Which, then, should be the period of work, from noon till midnight or from midnight till noon? It was decided to take first the part of the day that includes morning twilight mainly from practical reasons, such as the disinclination of most crews to run for an anchorage during darkness, and the fact that the ocean in this vicinity is usually much smoother from midnight to noon than after noon. Further considerations concerning the time consumed in handling the apparatus made it impracticable to work each station from surface to bottom, so that either deep water or shallow water collecting would have to be left out of this particular programme. Resorting again to experience and data already secured, it seemed best to work mainly in the upper one hundred fathoms.

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An example may now be given of actual work in accordance with these general considerations, which has been for some time and is still being, carried on.

On arriving at the station, which of course is "picked up" by sextant observation, a series of water samples and temperatures is first taken (see description of this operation, p. 189), this series corresponding in general with the depths at which the biological collections are to be made. In the programme here presented the depths are from 75-50, 50-40, 40-30, 30-25, 25-20, 20-15, 15-10, 10-5, and 5-0 fathoms. The biological collections are made and immediately thereafter a second series of water samples and temperatures is taken similar to the first, and

sextant observations taken to make sure of the position at the end of the operations. The "Agassiz" then returns to the station and the observations are repeated. Three complete series on the same station can usually be secured between midnight and noon; one for darkness or very early morning twilight; one for twilight; and one for daylight. Although as previously stated seasonal distribution is made the central problem or point of departure in the programme, the operations yield in addition information as to the abundance and movements of organisms as affected by variations of (1) light, (2) temperature, (3) density of water, (4) topography of the sea bottom, and (5) the gaseous content of the water. One of the three stations having been thus worked, the next day another is occupied and the operations repeated as exactly as possible. The third day the remaining station is investigated. These three stations having been gone over, what shall be done next will depend on circumstances. Three more days may be spent on the same stations, taking them in the same or in a different order; stations in some other locality may be visited for the same kind of work; somewhat different work may be done; or the cruise may have been planned for going once over these three stations and no more.

A copy of the log-book for one day, rearranged for convenience of reading, will be instructive. The day, selected at random, is June 15, 1911.

5. The Log-book for One Day

12:35 Lift anchor South Coronado Island and start for Station 2.

a.m.

1:25 Arrive at Station 2, latitude 32° 22.7' N, longitude 117° 19.2' W.

a.m.

1:30 Surface nets out; surface water-samples taken.

a.m.

1:32 Water-bottle series and temperatures from 75, 50, 7 and 4 fathoms.

a.m.

2:00 Water-bottle series and temperatures from 60, 40, 25, and 15 fathoms.

a.m.

2:10 Nansen net down to 75 fathoms.

a.m.

2:20 Surface nets in; surface water-sample taken.

a.m.

2:23 Nansen net closed at 50 fathoms and hauled up.

a.m.

2:25 Surface nets out; surface water-sample taken.

a.m.

2:25 Nansen net down to 50 fathoms.

a.m.

2:35 Nansen net closed at 40 fathoms and hauled up.

a.m.

2:37 Nansen net down to 40 fathoms.

a.m.

2:43 Nansen net closed at 30 fathoms and hauled up.

a.m.

2:45 Nansen net down to 30 fathoms.

a.m.

2:52 Nansen net closed at 25 fathoms and hauled up.

a.m.

2:55 Nansen net down to 25 fathoms.

a.m.

3:00 Nansen net closed at 20 fathoms and hauled up.

a.m.

3:02 Nansen net down to 20 fathoms.

a.m.

3:05 Nansen net closed at 15 fathoms and hauled up.

a.m.

3:06 Nansen net down to 15 fathoms.

a.m.

3:08 Nansen net closed at 10 fathoms and hauled up.

a.m.

3:09 Nansen net down to 10 fathoms.

a.m.

3:10 Nansen net closed at 5 fathoms and hauled up.

a.m.

3:11 Nansen net down to 5 fathoms.
a.m.
3:12 Nansen net closed at surface and hauled up.
a.m.
3:20 Water-bottle and temperature series from 45, 30, 12, 10 fathoms.
a.m.
3:25 Surface nets in; surface water-samples taken.
a.m.
3:35 Water-bottle series from 25, 20, 9, 5 fathoms.
a.m.
3:55 Water-bottle series from 35, 20, 6, 3 fathoms.
a.m.
4:00 Accurate position determined; lat. 32° 23.6 N; long. 117° 18.9 W.
a.m.
4:10 Under way for Station 2 again.
a.m.
4:20 Arrive at Station 2.
a.m.
4:23 Surface nets out; surface water-samples taken.
a.m.
4:25 Water-bottle series from 75, 50, 7, and 4 fathoms.
a.m.
4:40 Water-bottle series from 60, 50, 25, and 15 fathoms.
a.m.
4:50 Nansen net down to 75 fathoms.
a.m.
5:03 Nansen net closed at 50 fathoms and hauled up.
a.m.
5:05 Nansen net down to 50 fathoms.
a.m.
5:07 Surface nets in; surface water-samples taken.
a.m.
5:10 Nansen net closed at 40 fathoms and hauled up.
a.m.
5:11 Surface nets out; surface water-samples taken.
a.m.
5:15 Nansen net down to 40 fathoms.
a.m.
— 185 Nansen net closed at 30 fathoms and hauled up.
—
5:20
a.m.
5:22 Nansen net down to 30 fathoms.
a.m.
5:26 Nansen net closed at 25 fathoms and hauled up.
a.m.
5:28 Nansen net down to 25 fathoms.
a.m.
5:30 Nansen net closed at 20 fathoms and hauled up.
a.m.
5:33 Nansen net down to 20 fathoms.
a.m.
5:35 Nansen net closed at 15 fathoms and hauled up.
a.m.
5:37 Nansen net down to 15 fathoms.
a.m.
5:39 Nansen net closed at 10 fathoms and hauled up.
a.m.
5:41 Nansen net down to 10 fathoms.
a.m.
5:42 Nansen net closed at 5 fathoms and hauled up.
a.m.
5:44 Nansen net down to 5 fathoms.
a.m.
5:45 Nansen net closed at surface and hauled up.
a.m.
5:50 Water-bottle series from 45, 30, 12, 10 fathoms.

a.m.
6:00 Water-bottle series from 50, 25, 9, 5 fathoms.
a.m.
6:15 Water-bottle series from 35, 20, 6, 3 fathoms.
a.m.
6:25 Surface nets in; surface water-samples taken.
a.m.
6:30 Accurate position determined; lat. 32° 23'5 N; long. 117° 18'5 W.
a.m.
6:30 Under way for Station 2 once more.
a.m.
7:05 Arrive at Station 2 again.
a.m.
7:06 Surface nets out; surface water-sample taken.
a.m.
7:10 Upon attempting to lower water-bottles the signal bells failed to work. Time consumed in putting them in commission, 25 minutes. Fourth water-bottle out of commission.
a.m.
7:35 Water-bottle series from 75, 50, and 7 fathoms.
a.m.
7:55 Water-bottle series from 45, 30, and 4 fathoms.
a.m.
8:00 Nansen net down to 75 fathoms.
a.m.
8:05 Surface nets in; surface water-sample taken.
a.m.
8:10 Nansen net closed at 50 fathoms and hauled up.
a.m.
8:15 Nansen net down to 50 fathoms.
a.m.
8:20 Surface nets out; surface water-sample taken.
a.m.
8:22 Nansen net closed at 40 fathoms and hauled up.
a.m.
8:23 Nansen net down to 40 fathoms.
a.m.
8:27 Nansen net closed at 30 fathoms and hauled up. Cable caught in sheave, 8 minutes consumed in releasing it.
a.m.
8:35 Nansen net down to 30 fathoms.
a.m.
8:40 Nansen net closed at 25 fathoms and hauled up.
a.m.
8:41 Nansen net down to 25 fathoms.
a.m.
8:45 Nansen net closed at 20 fathoms and hauled up.
a.m.
8:46 Nansen net down to 20 fathoms.
a.m.
8:48 Nansen net closed at 15 fathoms and hauled up. Cable caught in sheave again; time lost, 4 minutes.
a.m.
8:52 Nansen net down to 15 fathoms.
a.m.
8:54 Nansen net closed at 10 fathoms and hauled up.
a.m.
8:56 Nansen net down to 10 fathoms.
a.m.
8:58 Nansen net closed at 5 fathoms and hauled up.
a.m.
9:00 Nansen net down to 5 fathoms.
a.m.
9:01 Nansen net closed at surface and hauled up.
a.m.
— 186 Water-bottle series from 25, 10, 3 fathoms.
—
9:05
a.m.
9:12 Water-bottle series from 45, 35, 6 fathoms.
a.m.

9:20 Water-sample series from 20, 9, 5 fathoms.

a.m.

9:25 Surface nets in; surface water-sample taken.

a.m.

9:25 Accurate position determined; lat. 32° 23'.2 N; long. 117° 18'.6 W.

a.m.

Owing to a smooth sea and the fact that a minimum of accidents occurred, the regular plan as given above was completed from an hour to an hour and a half earlier than usual. The remaining time from 9:30 *a.m.* to 11:25 *a.m.* was employed in testing V. K. Ekman's theory of oceanic currents as follows:

9:50 *a.m.* Arrive at latitude 32° 23'.2 N; longitude 117° 17'.4 W.

9:50 *a.m.* Water-bottle series from 55, 35, 15 feet.

9:55 *a.m.* Water-bottle series from 65, 40, 25 feet.

9:55 *a.m.* Surface water-sample taken.

10:15 *a.m.* Arrive at latitude 32° 23'.2 N; longitude 117° 16'.2 W.

10:15 *a.m.* Water-bottle series from 55, 35, 15 feet.

10:20 *a.m.* Water-bottle series from 65, 40, 25 feet.

10:20 *a.m.* Surface water-sample taken.

10:40 *a.m.* Arrive at latitude 32° 23'.2 N; longitude 117° 15'.5 W.

10:40 *a.m.* Water-bottle series from 55, 35, 15 feet.

10:45 *a.m.* Water-bottle series from 65, 40, 25 feet.

10:45 *a.m.* Surface water-sample taken.

11:00 *a.m.* Arrive at latitude 32° 23'.2 N; longitude 117° 14'.9 W.

11:00 *a.m.* Water-bottle series from 55, 35, 15 feet.

11:05 *a.m.* Water-bottle series from 65, 40, 25 feet.

11:05 *a.m.* Surface water-sample taken.

11:20 *a.m.* Arrive at latitude 32° 23'.1 N; longitude 117° 14'.1 W.

11:20 *a.m.* Water-bottle series from 55, 35, 15 feet.

11:25 *a.m.* Water-bottle series from 65, 40, 25 feet.

11:25 *a.m.* Surface water-sample taken.

12:25 *p.m.* Arrive at anchorage on South Coronado Island.

The following record of weather and light conditions were made while the regular collecting was being done:

2:23 to 4:00 *a.m.* Too dark for photometer reading. Ocean smooth, weather foggy and dismal. Barometer remained at 30 throughout entire day. Photometer readings for the day were as follows:

4:28 *a.m.* 13 Hood open.

4:30 *a.m.* 12 Hood open.

4:45 *a.m.* 5 Hood open.

4:58 *a.m.* 39 Hood covered. Fog lifting, partly cloudy.

5:00 *a.m.* 35 Hood covered for remainder of observations.

5:02 *a.m.* 32 5:32 *a.m.* 10.0 6:06 *a.m.* 10.0

5:04 *a.m.* 29 5:34 *a.m.* 15.0 6:08 *a.m.* 9.5

— 187 — 26 5:36 *a.m.* 14.5 6:10 *a.m.* 9.0

5:06 *a.m.*

5:08 *a.m.* 24.5 5:38 *a.m.* 14.5 6:18 *a.m.* 9.0

5:10 *a.m.* 24.5 5:40 *a.m.* 14.5 6:20 *a.m.* 8.5

5:12 *a.m.* 22.5 5:42 *a.m.* 14.0 6:28 *a.m.* 8.5

5:14 *a.m.* 22.5 5:44 *a.m.* 15.5 6:30 *a.m.* 8.0

5:16 *a.m.* 21.0 5:46 *a.m.* 13.5 7:02 *a.m.* 7.5

5:18 *a.m.* 21.5 Floating clouds. 5:48 *a.m.* 15.0 7:12 *a.m.* 6.0

5:20 *a.m.* 21.0 5:50 *a.m.* 13.5 7:18 *a.m.* 6.0

5:22 *a.m.* 19.5 5:52 *a.m.* 12.5 7:25 *a.m.* 6.5

5:24 *a.m.* 21.0 5:54 *a.m.* 12.5 7:50 *a.m.* 6.0

5:26 *a.m.* 18.5 6:00 *a.m.* 11.5 7:58 *a.m.* 6.0

5:28 *a.m.* 17.0 6:02 *a.m.* 10.0 8:10 *a.m.* 5.0

5:30 *a.m.* 17.5 6:04 *a.m.* 10.0

After 8:10 the light was too bright to detect its intensity with the photometer.

6. Handling the Apparatus

(a). The Surface Nets.

When according to the judgment of the scientist in charge the surface nets should be used he calls: "Surface nets out," and two sailors detailed for this service put them over. The large 000 net is fastened to the end of a heavy rope about two hundred feet long, and to its rim is attached an air-tight five-gallon can or carboy to serve as a float. The net is then thrown over the rail at either side of the boat's stern, depending upon the direction of the drift. At a distance of about fifteen feet from the 000 net the next smaller net, mesh no. 9, 10 or 12 as the case may be, is attached to the rope, and about fifteen feet from that one the third net, usually mesh no. 20, is likewise attached. The nets are then allowed to drift until the rope becomes taut, when it is made secure. After the nets have been out long enough the scientist calls "Surface nets in" and the sailors haul them in, detach the two smaller ones and lift the large one on deck. The nets are then washed with surface water to prevent any of the organisms from adhering to the netting and the contents of the bucket are transferred to containers which consist of pint or quart sure-seal fruit jars, or if the catch is unusually large, into pails. About five minutes are consumed in hauling in, detaching and washing the nets, transferring the catch and putting the nets out again.

(b). The Nansen Net.

To operate this net requires the combined labor of an engineer, a man to read the meter, another to

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stand by the bells, and two sailors to handle the net itself. When ready to be lowered the ring from which the net is suspended is fastened to the trip which has previously been made secure to the cable. The man at the bell, usually the captain or scientist, signals the engineer to lower. Just before the net has descended far enough, say seventy-five fathoms, he signals a second time and the man at the bell passes the signal to the engineer who stops the cable. A second signal tells the engineer to raise the cable, the man at the meter gives notice when it has risen far enough, say fifty fathoms, and the man at the bell signals the engineer to stop. The sailors then send the messenger down the cable which strikes the trip and closes the net. A haul has thus been made from 75 to 50 fathoms, and the net is brought to the surface as quickly as possible. When it reaches the surface it is hoisted high enough to avoid the railing, then lowered on the deck and washed. In all hauls made below the surface the water used for washing is filtered through netting of finer mesh than that of the net, thereby preventing contamination by surface organisms. After washing, the catch is transferred to containers as in the case of surface nets, and while one sailor is transferring the catch the other is removing the messenger and adjusting the net for its next haul. The cable may be raised or lowered at any speed but during the interval of each haul the speed is constant, thus insuring a uniform rate of filtration. About one minute elapses between the time the net reaches the deck and its descent for the next haul.

(c). The Kofoid Net.

This is operated in almost the same way as the Nansen, although its great weight makes the handling more difficult, and impossible on the "Agassiz" in rough weather. Being constructed to make horizontal instead of vertical hauls, two messengers are used, the first one to open and the second to close the jaws. An interval of fifteen minutes or more usually elapses between the sending down of the two messengers. Owing to its weight the net tends to act as a sea anchor. This is overcome by steaming ahead with one engine while the net is making the catch. For a full description of this net see Kofoid, 1911c.

(d). The Ekman Reversing Water Bottles.

If it is desired to take water-samples and temperature in say 75, 50, 45, and 10

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fathoms, the terminal bottle is attached to the end of the cable and lowered to 25 fathoms. The second bottle is then clamped to the cable and lowered to a depth of fifteen fathoms, when the third is attached and lowered to a depth of 35 fathoms, after which the fourth is attached and lowered to 10 fathoms. This brings the terminal bottle into 75, the second into 50, the third into 45, and the fourth into 10 fathoms as desired. After waiting about a minute to insure the setting of the thermometers, a messenger is sent down which reverses the first bottle, thereby freeing a second messenger which reverses the second bottle, and so on. After all the bottles have been reversed (which can be detected by feeling the cable), the cable is elevated until the first bottle reaches the deck. The bottle is then detached from the cable and handed over to the scientist who notes the temperature and places the instrument in a specially made rack which allows the water to run out into a container. By this means as soon as the water has been removed from the terminal bottle everything is in readiness for a second series. In case gas samples are taken the water must be removed more carefully, so as to preclude the possibility of admixture with air. The gas analysis sample being secured the bottle is then placed in the rack as before. There is no necessity to hurry in removing the water from these bottles, for the first one to arrive on deck is the last one needed in the next series. The cable is operated in exactly the same way as when working with the Nansen and Kofoid nets.

(e). The Other Apparatus.

The Kofoid water bottle, the trawl and dredges are operated in much the same manner as the instruments already mentioned.

(f). Methods of Recording.

Even though the best apparatus, the best methods of operating, and the most expert crew in the world were employed, the data would be worthless for the main purpose of the investigations unless properly recorded. A double-entry plan of recording is used on the "Agassiz." After the hauls and water-samples are transferred to containers, the recorder labels each one. For this purpose he is provided with haul and water-sample books consisting of tags about one and one-half inches square, made of the best linen paper. Printed along the left margin of each tag are the following items:

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Haul No.	Water-Sample No.
Apparatus	Haul No.
Date	Date
Position	Position
Depth	Depth
Bottom	
Water Sample No.	Temperature
Temperature	
Haul-Book Tag	Water-Sample Book Tag

The recorder enters on each tag the information called for, using a soft lead pencil except in the case of haul numbers, which being in sequence irrespective of the nature of the haul, are written in India ink previous to sailing. However, to provide against loss of tags, a few tag-books are carried in duplicate in which the haul number is not designated. When a tag is properly filled out it is placed inside the jar containing the haul, and formaline (about 10 per cent) is added, or when practicable the hauls are carried to the laboratory without killing. In addition to the tags a log-book is kept in which is also entered the same data for each haul and water sample, together with the details of operating the apparatus, accidents, weather conditions, and in fact anything that might have the remotest bearing on the results. Upon completion of the day's work the recorder checks the entries in his book against those the captain has kept. As soon as possible after the material has reached the laboratory the data for each haul and water sample are typewritten in triplicate upon accession sheets. Each accession sheet for hauls is 17 by 14 inches in size and ruled into columns having the following headings: haul number, character of haul, apparatus used, date, time of day, station, position, depth of haul, nature of bottom, water-sample number, temperature (during haul, at surface, in air), remarks. The accession sheet for water-samples is of the same size and bears the following headings: water sample number, date, time of day, haul number, position, depth, temperature, and many other headings for entries of density, chlorine content, salinity, etc.

Experience has taught that the recorder should not attempt anything else than his particular task when thus engaged. Hauls

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and water-samples come in so fast at times that it is exceedingly easy to make mistakes in reading and recording. Any attempt to hurry means error, and should the recorder fail to enter a single catch the entire day's work would probably be vitiated, because of the difficulty of determining where the error occurred. There is no one thing upon which success of the work depends more than on trustworthy recording.

5. The Library

Although a library is so vital an adjunct of any institution of scientific research, that of the Marine Biological Station at La Jolla is unfortunately but meagerly developed. The main reliance thus far has been on the library of the University of California. The supply of books proper consists of something less than five hundred bound volumes, but these are supplemented by a much larger number of pamphlets and reprints, and by the considerable library of the director. The cards of the Concilium Bibliographicum are provided, and the pamphlets are arranged in accordance with the system employed by that institution. The subscription list of journals contains only fifteen of the great number that would be requisite to make a really adequate working library for such an enterprise. No department of the station is in sorer need of enlargement than this.

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G. SCIENTIFIC RESULTS ALREADY ACHIEVED

I. BIOLOGICAL

1. *What Has Been Done Toward a "Speaking Acquaintance" with the Fauna*

A "Biological Survey of the Waters of the Pacific adjacent to the Coast of Southern California"^[1]

See for example Appendix C, the By-laws of the Association, and "A general statement of the ideas and present aims and status of the Marine Biological Association of San Diego" (Ritter, 1905a).

having been adopted as a general statement of the station's scientific program, the first thing was to describe and record as many as possible of the kinds of organisms inhabiting the region.

So far the groups of organisms listed in table 1 have been dealt with to some extent. To make this table as truthful and at the same time as useful as possible, no little puzzling has been done on the question of what should and what should not be included. As it stands it aims to contain all the species of the "San Diego Region" to the study of which our work from the beginning has contributed. It shows (a) the groups of the organisms that have been studied and the results published; (b) the number of new species described in each group; (c) the total number of species so far found in the area; and (d) the persons doing the work.

Table I

a	b	c	d
Group	New species	Total no. species	Investigator
Unicellular Organisms—			
Peridinia	37	195	Kofoid
Ciliates	7	7	Kofoid
Coelenterata—			
Hydromedusae	32	57	Torrey
Actinaria	0	1	Torrey
Ctenophora	1	3	Torrey
Alcyonaria	8	23	Nutting
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Asteroides—			
Serpent Stars	5	20	McClendon
Flat-Worms—			
Endoparasites	1	2	Watson
Chaetognatha	0	10	Michael
Nemertea	10	37	Coe
Bryozoa	21	70	Robertson
Mollusca—			
Cephalopods	0	4	Berry
Naked Opisthobranchs	7	0	Cockerell
Shell-bearing Gastropods	4	0	Raymond
Shell-bearing Gastropods	6	21	Dall and Bartsch
Crustacea—			
Copepoda	43	113	Esterly
Ostracoda	6	17	Juday
Cladocera	0	1	Juday
Amphipoda	16	0	Holmes
Stalk-eyed	14	?	Holmes
Enteropneusta	3	3	Ritter and Davis
Tunicata	13	25	Ritter
Fishes	3	253	Starks, Davis and Mann
	---	---	
Total	328	862	

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 farther than the mere characterizing, arranging, and cataloguing of 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 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?

Such studies as those by Michael on the structural features and movements of the chaetognaths, by Kofoid on the dinoflagellates, and by Ritter and Johnson on the asexual propagation of the salps, to say nothing of studies by

many other biologists on many other groups, make it certain that researches must be carried on in full light of the probability of a negative answer to the question.

2. What Has Been Done Toward a Deeper Knowledge of the Organisms

While it is not possible to present in tabular form, as was done under the preceding head, what has been accomplished here, the more significant results may be briefly stated.

(a). Abundance and Mode of Life

Mr. Michael's work on the chaetognatha or "bristle-jawed" worms, representing as it does the most advanced point yet reached on the ecological side of the station programme, is noticed first.

The mere statement (see table) that ten species of the group

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occur in the area, would give a very imperfect picture of the population as it actually exists. Thus of the nearly 79,000 specimens collected during the five-year period, 1904–1909, covered by his report, about 51,600 were of one species, *Sagitta bipunctata*. Over 10,000 of the remaining 17,400 belonged to one other species, *S. enflata*, and nearly half of the balance to another, *S. serratodentata*. One species, *S. draco*, was represented by a single specimen.

Thus it appears that much 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.

So far as the evidence goes, it indicates that none of these species lives chiefly on the surface of the sea; that all make bi-daily excursions, more or less extensive, up and down; that each has, within rather wide limits, its own most favored stratum or "center of migration" as Mr. Michaels terms it; and that this center and the movements from it depend upon several factors. Thus *S. bipunctata* occurs most abundantly on the surface twice each day, namely within an hour after sunrise, and within an hour after sunset. From fifteen to twenty fathoms seems to be the most favorable depth for this species. The evidence tends to show that its movements are influenced by light, by temperature, and probably by density of the water. Since these three are the only environmental factors considered in the investigation, it is not known whether others are operative. Contrary to what might have been expected, the data do not reveal correlation between abundance and different seasons of the year.

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.

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From the results so far obtained it appears that as regards up-and-down migration, these animals behave quite differently from the chaetognaths. For example, *Calanus finmarchicus*, one of the most abundant species, comes to the surface in greatest numbers between 7 and 8 p.m. (during June and July) and begins to descend about midnight, appearing to be nearly gone by four o'clock in the morning. No second return to the surface occurs the same day. Dr. Esterly points out that this confirms the conclusions reached by several other observers that some other factor or factors than light must come into play in determining the movements. What these are remains to be ascertained. The animals descend to 200 fathoms in great numbers, but do not seem to go much deeper. As would be expected, it is during the day that they are found in the extreme depths.

Another species, *Eucalanus elongatus*, "does not exhibit any well-marked diurnal migration," and though very abundant at from 200 to 250 fathoms is never abundant on the surface. No cause has been definitely discovered for the failure of this species to make the customary daily excursions, but Dr. Esterly calls attention to the fact that the animals are particularly transparent, more so perhaps than those of any other species, and suggests that this may partially explain their peculiarity of habit.

The larvae of the enteropneusts or "intestine-gilled" worms are the only other animals upon which anything has been published from this standpoint. Ritter and Davis (1904) point out that one species, *Tornaria ritteri*, almost never occurs on the surface, but at certain times and certain localities occurs in considerable abundance at depths from 25 to 100 fathoms. This conclusion was based on work done before closing-nets were used, and was not founded on a very great amount of data statistically treated. It nevertheless seems reliable. Although a quantitative study of the vertical distribution of the animals in relation to daylight was not made, laboratory experiments were performed to determine whether or not they react to light. The results were negative; and no explanation other than the structural and functional constitution of the larvae were found for the phenomenon of a pelagic period in the life of the species. Interesting relations were made out between stage of development, bulk, specific gravity, and power of swimming of the larvae.

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But the programme of the station specifically in hand, that of studying the occurrence and ecology of the pelagic fauna and flora, by no means comprehends all that has been accomplished in the way of answering those larger questions of the living world that are particularly approachable through the life of this locality.

(b). Morphological and Physiological Studies

Torrey and Martin (1906) describe an instance of sex differences among hydroids that amounts almost to secondary sexual characters even so far down in the animal scale as this. The highly modified branches, called corbulae, which carry the sex elements in the Plumularian genus *Aglaophenia* are quite distinctly different in the species examined.

Dr. Watson's extensive study (Watson, 1911) of the fish parasite *Gyrocotyle*, though carried on largely in the zoological department of the University of California, Berkeley, was based on material much of which was collected at the station, and some of the work was done here. The investigation was undertaken primarily with the hope of settling the debated question of which end of this animal should be regarded as the head. Much of the creature's structure, both gross and microscopic, was examined and the movements of the living animal were studied to a considerable extent in their bearing on the main question. The conclusion is that the end bearing the acetabulum or adhesive cup is the true anterior end. This is based partly on the fact that the animal crawls, so far as it crawls at all, with this end foremost; and partly on morphological evidence, particularly in connection with the nervous system. Another point, seemingly not specially contemplated at the beginning of the study, turned out to be one of the most significant of all, namely, that of the bearing of the conclusions above indicated on the question of which is the anterior or "head" end of the tapeworm. Cogent reasons are brought forward in support of the view that the attached end, or scolex of this latter parasite, which is commonly called the head, is really the tail end, the "head" having been lost. A rather interesting detail is the fact that there is "no trace in any tissue of the body of an epithelial layer of cells."

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Bancroft and Esterly (1903) reëxamined the reversal in direction of heart-beat in the ascidian *Ciona intestinalis*. Their work is a good illustration of the seeming inexhaustibleness as a subject of research of almost any biological phenomenon. Despite the large amount of study that has been bestowed upon this matter, a striking fact, hitherto unobserved, was brought to light: "Not only does the direction of the contractions remain fixed while a part of the heart is connected with only one of its ends, but in some way a change is effected in the heart tissue so that the direction of the contractions still remains fixed after the part has been isolated from the end which was instrumental in producing the fixation."

The "set" of a beat, as it might be called, in either one direction or the other, the authors speak of as physiological polarization. Comparison of the phenomenon with the "habit" acquirement by the arms of the star-fish observed by Jennings, and to be mentioned farther on, is instructive.

(c). Reproduction and Development

Kofoed (1908a) has shown that in one group of unicellular organisms (*Ceratium*) the cellulose shell of the organisms is normally shed and renewed from time to time; that the three outgrowths of horns characteristic of the genus exhibit, in some instances at least, alternating periods of growth and rest; and that self-amputation followed by regeneration of the horns occurs. In these unicellular organisms, therefore, phenomena of growth and development occur that are quite comparable with such phenomena in many higher organisms. This similarity seems to be exhibited again in the clinging together for a time (how long is not known) of the individuals which arise from one another by division. A detailed study of the members of these series, both qualitatively and quantitatively, might throw light on the important general question of the relation among the individuals of a lineal generation of cells. But the author did not have sufficient material of this particular sort to enable him to attack the question.

Dr. Torrey's work on the hydroids and anemones has touched a considerable range of developmental phenomena. From studies

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partly descriptive but chiefly experimental (Torrey, 1905a) he concludes that the facts "appear to give strong support to the view that the stem [of the hydroid *Clytia bakeri*] instead of retaining unmodified its regenerative capacity, actually loses with age its ability to produce structures which formerly characterized it; and that this is owing to a modification of conditions *within* the organism, which govern its behavior without being necessarily a part of it." And further that "the resemblance of the phenomena of colonial differentiation in *C. bakeri* to the phenomena of senescence is so strong as to suggest a similar interpretation for both."

Of his series of studies upon the fine large hydroid *Corymorpha palma*, Dr. Torrey has devoted one (1910c) to the much investigated phenomena of heteromorphosis and of polarity. Complete reversal of polarity of pieces cut from the stem was observed, and a considerable measure of control was effected over the phenomena, both as to occurrence itself and as to the rate of growth. For example it was shown that by inserting the distal end of a section of the column into a capped glass tube, a hydranth promptly develops at the free or proximal end of the piece, but not at the end in the tube; whereas a similar piece not enclosed in glass develops distal but not proximal hydranths. The author believes exclusion of oxygen by the glass tube explains the difference in results.

Torrey and Mery (1904) have considerably enlarged our information about asexual propagation in the anemones, three rather distinct modes of fission having been observed in the same species. In the species studied another instance of heteromorphosis is recorded for the group, of which until now comparatively little has been known.

Number II of Torrey's studies on *Corymorpha* (1907a) is devoted to the embryonic development of the animal. This species appears to be peculiar in its genus in that the egg-carrying medusae do not become detached from the hydroid. Egg-laying occurred in May, June, July, August, September, December, and January, no observations having been made in the remaining months of the year. From this standpoint as well as from several others, the animal is well fitted for biological

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studies. Extruded eggs undergo marked amoeboid movements before fertilization. The larvae have no free-swimming stage, and this fact, together with the absence of free medusae, would seem to leave the species with little ability to scatter about widely. The later development is described somewhat more fully than the earlier. The great plasticity of the organism from beginning to end of its developmental career is specially noticed, particular attention being given to the extent to which the differentiation both of larger morphological features and of cellular details is more or less dependent upon activities of the organism and external conditions. Attention is called to the close resemblance between the tentacles and the hold-fasts, or frustules, abundantly present on the lower part of the stem in the early stages of development. The attractiveness of this species for biological study, while strong from the accessibility and size of the animals, is made doubly so by the number of further inquiries suggested by this initial research.

An important contribution to the reproductive habits and embryonic development of the enteropneusta was made by Dr. B. M. Davis (1908). A fuller account of egg-laying is given for the species studied than has been recorded for any other of the group. Although not sufficient material was secured to make possible anything like a complete description of the earliest stages of development, enough was observed to impress the author with the resemblance of the embryo in the stages studied to the corresponding stages of *Amphioxus*. This species is the second enteropneust known in which no true tornaria stage occurs. By far the most important contribution made by Dr. Davis to the development of the group was the conclusion reached, on very strong evidence, that the middle and posterior body-cavities arise entirely differently from the way in which they are currently supposed to arise. He found them to originate by backward growth of and constriction from the first, or anterior cavity, and not by separate outpocketings from the enteron. While there is not sufficient ground in all the germane phenomena for attaching as much theoretical importance to the particular question of fact here involved, as was done some years ago, the matter is nevertheless one of real interest and it

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is to be hoped that this investigation may be followed up in the not distant future.

In the study of enteropneust larvae by Ritter and Davis, already noticed, three quite distinct stages in the larval development were recognized and correlated with different habit-of-life periods of the larva. This class of studies, that of tracing in detail the relation between structural features in pelagic larvae and modes of life in nature at different periods, has yielded instructive results wherever pursued and so promises well for future work.

The four papers, Ritter and Bailey (1908), Ritter (1909b), Johnson (1910), and Ritter and Johnson (1911), are concerned with a common developmental problem notwithstanding the wide diversity in title and special topics. The problem may be stated thus: To what extent do the members of the repetitive growth series so obvious and widely prevalent in organic beings fall into groups of such sort that each member is a function, in the mathematical sense, of the position it occupies in the series to which it belongs? Stated in another way the problem is: How far are the periodic phenomena so common in organisms expressible in terms of systematic quantitative difference for different parts of the observed periods? It will be noticed that the problem involves not only one of fact as regards organic phenomena, but also for its solution, one of method, that of systematic quantitative determination within the confines of individual organisms.

The second paper mentioned (Ritter, 1909b) contains an account of the initial attack upon the problem. The results, so far as they pertain to this problem, are summarized in the following quotations: "An approximately exhaustive study of one of the animals finds it to contain a manifolding of similar parts to an extent that is surprising in view of the slight prevalence of such parts recognized by cursory observation. Comparison of many individuals of various sizes and ages ... reveals the fact that to a large extent, the measure of which is not known, these manifolded parts have arisen (a) as lineally genetic series ... from a few initial ancestral organs; while others have arisen (b) as repeated productions from common original substrata, or menstrea. Attention to these repetitive series makes

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it quite certain that many of them ... not only are subject to definite schemes as to positional arrangement and time of origin, but also as to mass relations. Otherwise stated, the repetitive series which obviously constitute so large a part of the whole animal are to a great extent disposed in mathematically treatable order." Although the systematically graded quantitative relation among repeated parts was definitely recognized in this study, no attempt was made to measure the quantities.

The first effort toward dealing with the problem quantitatively was made by Miss Johnson (1910). Her most immediate results were that "the salpa chain presents an obvious periodicity," the so-called "blocks" of zooids making the periods; and that the zooids at the ends of the block are much smaller than the intermediate ones, those of maximum size being nearer the distal end in the younger blocks, and nearer the proximal end in the older

blocks. Attention was called to the resemblance of the size-scheme of zooids in the blocks to the well-known phenomenon of "grand period of growth" exhibited by many plants.

Ritter and Johnson (1911) found that in the wheel-shaped groups characterizing the chain of another species of salpa, the size gradation of the zooids is more generally distributed in the groups than in the species previously studied, the graphs made from plotting the measurements approaching more closely the normal probability curve. In this species, though the size-scheme is very clearly revealed by quantitative study, the differences among the zooids are so small relatively to the whole animal as to be unrecognizable to the eye. Consideration of such environmental and physiological influences as might conceivably bring about the observed gradation among the zooids led to negative conclusions, leaving no other recognizable explanation of the phenomenon than that growth itself proceeds in that way.

The paper by Ritter and Bailey (1908) records a preliminary attack on the same general problem in quite a different and more fundamental quarter, but a quarter where the difficulties of manipulation are much greater; that is, in the realm of cell division as this manifests itself in the developing egg. The method of quantitative determination was that of weighing. So far as the problem itself is concerned, the meager results were

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of doubtful value, but the manipulative experience was of decided value as suggestive of future effort in this same direction.

(d). The Adaptations of Organisms

This problem has been touched in several researches besides those occupied expressly with ecology which, in its very essence, is to a large extent the problem of adaptation.

Considering the question of adaptiveness of the specific characters of the flagellate infusorian *Tripsolema*, Kofoid (1906c) reaches the conclusion that "while it is not probable that all of the species are all equally well adapted to survive, it is evident that they have been and are *sufficiently* well adapted to maintain themselves." In this study, as the quotation indicates, the author makes use of the important principle that the question of adaptation receives much more illumination by treating it from the standpoint of the relative adaptiveness of several species belonging to a single group, than by giving attention to one species alone. Reference will be made to this principle in connection with another study.

In his work on *Ceratum*, already noticed, Kofoid (1908a) goes extensively into the question of the adaptiveness of the phenomena primarily dealt with (the shedding of the test and self-amputation and regrowth of the horns) to various environmental conditions. He brings forward good reasons for the conclusion that the phenomena mentioned "assist in the adjustment of the specific surface and possibly also of the specific gravity to changing conditions of flotation, especially as effected by temperature."

Dr. Torrey studied the habits and movements of a species of anemone in detail (1904b) and recognized some of the activities to be clearly adaptive, while others "appear to have no adaptive value whatever." For example certain movements of the tentacles are very definitely in the interest of securing and using food, while others seem to have no purpose either in this way or any other.

In dealing with our common shore anemone, *Bunodactis xanthogrammica*, this author (1906a), though not concerned directly with the problem of adaptation, points out that the same

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species lives along the whole western coast of North America from Panama at the south to Unalaska at the north. This is certainly a remarkable case of the adaptation of the same species to a great variety of environmental conditions.

Although the extensive study of Jennings (1907) falls properly under the heading of animal behavior, noticed later, the question of adaptiveness of this behavior to natural conditions under which the animal lives, received considerable attention. The results bearing on this may be summarized by saying that of the large number of reactions and activities of the starfish studied, every one is probably to some extent advantageous to the individual animal. At least it is not suggested that any activity studied is without significance for some particular aspect of the creature's existence.

(e). The Doctrine of Natural Selection

Kofoid (1906c) and Ritter (1909b) have dealt specifically with this subject to some extent.

Studying the species-characters of the dinoflagellate already referred to, Kofoid concludes that "in assigning natural selection as the cause of the species characters in *Tripsolema* we are at once confronted by the difficulty of finding any evidence of the *differential* survival value of any of the characters in question." "It is," he says, "impossible to establish the fact of any advantage accruing to one of these species over its nearest allies by reason of its structural distinctions and difficult to find any satisfactory basis for a logical inference or conclusion to that effect."

Ritter carried the idea of coordinating the differences between two closely related species with the differences between their environments well toward a quantitative determination, with the result summarized in the following: "So far as the present inquiry has gone, the attempt to find a causal relation, or a necessary correlation, between the character differentials of the two species (*Halocynthia johnsoni* and *H. haustor*) and their environmental

differentials, has produced negative results. In other words, the results do not enable us to affirm anything more definite about the adaptation of *H. johnsoni* to its environment

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than that in a general way it is so adapted; that is, that it is sufficiently adapted to enable the individual animals to live and maintain their specific identity in a considerable range of environmental conditions."

(f). Animal Behavior

This term has come to have a very definite meaning in recent biology and as such to stand for one of the most important subjects of investigation. It has been dealt with by Dr. Torrey in several studies (Torrey, 1904b, 1905b, and 1906a), and by Dr. H. S. Jennings (1907).

Torrey's work on *Corymorpha* (1905b) brought out, among other things, the interesting result that the pronounced geotropic movements of the animal appear to depend not on the muscles of the body, although these are well developed, but on the axial cells which are not at all muscular. The action of these cells is, it seems, due to their changing turgidity. If this interpretation be right, the author says, "*Corymorpha* stands alone among the metazoa in possessing a tropic mechanism distinct from the body musculature." Such a conclusion naturally leads to reflection on the similarity of these movements of the animal to the negative geotropism of plant seedlings. "I know of no animal," Torrey says, "which more closely approximates the plant in structure and tropic response. If the behavior of the one be explicable on the basis of direct reactions to stimuli, of the reflex type, I do not see how the behavior of the other can be excluded from a similar interpretation."

Study of the tentacular and ciliary movements in the anemone *Sagartia* (1904b) under various chemical and mechanical stimulations extended our knowledge of these phenomena in several directions. For instance the question that has been raised as to whether reversal in direction of ciliary action could be induced by mechanical as it can by chemical stimulation was definitely answered in the affirmative. Special emphasis was laid on the fact that polyps act more or less definitely and vigorously in various food-taking operations, depending on how hungry they are.

In all of Jennings's long series of studies on animal behavior

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it is doubtful if any single one contains a greater number of important observations or more interesting reflections than the one here noticed. Reading carefully through the whole one hundred and thirty pages which constitute the paper, with a view to summarizing the most significant facts, I am so impressed with the difficulty of making such a summary as to be impelled to quote the author's own words: "The foregoing account," he says, "is intended as a storehouse of objective facts, for reference when information concerning the behavior of the starfish is required in order that there may be no farther excuse for theories that leave out of account the facts. It is therefore not practicable to make a summary that will really represent the results set forth in the paper. To form an idea of the matters treated, reference should be made to the table of contents." But Jennings has given an appraisal of his work as follows: "Perhaps the most important thing developed in the paper is the demonstration of the variability, modifiability, unity and adaptiveness in the main features of the behavior of the starfish. The movements are shown to depend on the varying physiological conditions of the animal, and the numerous factors which demonstrably modify the physiological condition, and therefore the behavior, are set forth in detail. Habit formation is demonstrated and discussed in full." From this enumeration I select the item of *unity* for a little further remark, not however, with the implication that it is foremost in importance among them all, but because it is less usually dwelt upon by most investigators than are the other items. "It is clear," says Jennings, "that the behavior under the righting impulse tends toward the accomplishment of a general turning of the starfish as a whole, and that given parts sacrifice their own more direct turning, or even reverse it, in the interest of the general result. The behavior shows what can be hardly characterized otherwise [than?] as a general 'plan,' each part doing what will assist (often very indirectly) to bring about the result." The fact that this unity is in some instances incomplete; that occasionally "the action is discordant," is emphasized as important in "forming a theory of the matter." *The unity of action is, it seems, a development.* In connection with the extremely interesting results on

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habit formation, worthy of special notice, is the fact that "the behavior of young specimens is more readily modifiable than that of old ones."

Jennings also makes clear what, in general outline, is his position as to the deeper meaning of the facts observed. "Conversation with investigators," he says, "leads me to believe that a large proportion of them would welcome a distinctly 'vital' explanation as readily as any other if they could see that it helped them in understanding and controlling the activities of organisms. But such a view as that of Driesch merely transfers the problems to the Entelechy, where they are less attackable than before." On the other hand, "Investigators may hold with Driesch, as the present writer does, that most of the simple chemical and physical explanations that have recently been given are superficial and quite inadequate to account for the regulatory activities of organisms." We are not, he thinks, obliged to enroll under either flag, but "can hold, in preference to either of these views, that our present analysis is incomplete, and that there will be something for investigators to work out in these fields during the next ten thousand years or so."

Several researches carried on at the station have been the basis, wholly or partly, of memoirs that cannot be claimed as part of the station's output, since nothing more was contributed to the work than granting to the

investigators the privilege of occupying the laboratory and using some of its appliances.

Under this head come papers by Mr. W. C. Adler-Mereschkowsky of Russia, on sessile diatoms; by Miss Sarah P. Monks of Los Angeles, on variation and the self-amputation and regrowth of the arms of the starfish *Linkia columbiae*; by Professor W. R. Coe of Yale University, on the nemertians of the region; by Professor T. D. A. Cockerell of Colorado College, on the opisthobranch molluscs; by Professor T. H. Morgan of Columbia University, on the problem of self-fertilization in the ascidian *Cione intestinalis*; by Dr. A. J. Carlson of the University of Chicago, on the electrical stimulation of the heart in several marine invertebrates; two reports by Professor C. C. Nutting of Iowa University, on alcyonarian polyps for the United States Bureau of Fisheries; one or more papers by Mr. M. B.

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Nichols of the University of California, on the calcareous seaweeds of the coast of southern California; four by Dr. C. M. Child of the University of Chicago, on "form regulation" in the sand anemone *Harenactis attenuatis*; and studies well advanced on endoparasites of fishes by Mr. W. E. Allen of the University of Illinois; on the circulatory system of annelids by Dr. H. R. Linville of Jamaica, N. Y.; on the so-called pyloric gland of ascidians by Dr. H. S. Colton of the University of Pennsylvania; on the problem of accessory chromosomes in chaetognaths by Dr. Nettie M. Stevens of Bryn Mawr; and on the endostyle of ascidians and amphioxus by Dr. David Marine of Western Reserve University, Medical Department.

Since these investigations cannot be held to represent activities of the station as these are now carried on, no resume of the results is given here. I cannot, however, refrain from mentioning Child's observations and reflections on "polarity" and correlation, particularly in connection with the development of what he calls "rings" from pieces cut out of the body of the anemone studied; nor from calling attention to Morgan's proposal to make use of the repugnance to self-fertilization manifested by the ascidian under observation, for getting at the chemical basis of individuality. The brief paper by Miss Monks must also have something more than a mere mention. The remarkable variability of the starfish studied, and the unquestioned ability of its amputated arms to produce a complete animal even though the plane of severance is some distance from the disc, are facts of unusual interest and strongly invite further examination.

II. HYDROGRAPHIC

Although as was stated in another connection, study of the water was from the outset counted as an essential part of the enterprise, little has yet been published of the very considerable amount of data collected.

The preliminary paper by Dr. G. F. McEwen (1910) contains a brief statement of the methods used, a very general summary of water temperatures and densities observed up to 1909, and by way of generalization, a short discussion of the probable significance of colder in-shore surface water along this coast.

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The particular thing brought out in this discussion is the applicability of V. W. Ekman's theory of oceanic circulation to the facts observed.

While this is all that has been published on hydrography as such, the extent and importance of the hydrographic work as an adjunct to the biological investigations are indicated by the use made of this data in the papers of Michael and Esterly.

The earlier data collected but not yet published should be referred to. 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. From 1903 to 1906 considerable work was done in this way, particularly on the San Diego Bay and in-shore water by Mr. W. T. Skilling, teacher of physics and chemistry of the State Normal School at San Diego. Mr. Skilling also began work on the chemistry of the water.

Under this head reference should be made to the modeled topographic map (pl. 24, fig. 10) of the continental shelf and adjacent land area of Southern California constructed in 1906 by Professor R. S. Holway of the Department of Geography, University of California. This was constructed partly on the basis of soundings taken by the station's boats, though more from the work of the United States Coast and Geodetic Survey and other departments of the national government. This map is very useful in several ways for the operations at sea.

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H. THE FUTURE

I. THE STATION'S PROGRAMME PROPER

Turning from the past and present to the future, it is desirable to remind ourselves of the general idea upon which our whole enterprise immediately rests, namely, that of a "Biological Survey of the Waters of the Pacific adjacent to the Coast of Southern California." An earlier statement (Ritter, 1905a) will furnish a useful introduction to what is to follow. "While there is no reason for attempting a rigorously laid out attack on the numerous problems, a natural sequence within certain limits will establish an order; and where practical administrative conditions conveniently adapt themselves to such sequence this order will be followed. For example, the species representing a given pelagic group having been gotten well in hand, a natural second step would be the determination of the seasonal distribution of the group. ... 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. And here, completeness of knowledge being ever the watchword, the demand would arise for applying experimental and statistical methods in the effort to get at the deeper significance of the facts observed, and generalizations reached from the observational investigations. The chain of questions hanging one to another is endless and, of course, completeness of knowledge in a literal sense is an unattainable ideal."

This quotation is made partly to emphasize the general view which has from the beginning guided the station's work and development; and partly to show that when the statement was written six years ago even a rough outline of the course then proposed was only approximately that which has actually been followed. For instance, experience has proved that such a severance of problems of vertical migration from those of vertical

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distribution as was suggested, is impracticable. Other discrepancies between the programme indicated and that actually carried out will be seen by anyone who studies Michael's work, particularly. With this reference to what the past teaches concerning both the value and the limitations of laying out work for the future, we may pass to a consideration, in barest outline only, of what now appears the next thing to be done.

II. NECESSITY OF CLOSER CO-OPERATION BETWEEN FIELD WORK AND LABORATORY WORK

Looking at the matter from the standpoint of both the present stage of the scientific work and the actual conditions of the station as a means of prosecuting this work still further, two things are quite clear. These are the desirability of continuing the collection of data at sea in much the same way as this has been in progress during the last three years especially, though increased in amount of work done and in refinement of method; and of taking up in earnest laboratory experimentation to supplement the field operations. The sharpness of definition which these two things have taken on with experience, brings up so concretely the much discussed question of the relative merits of the experimental as against the observational and descriptive method of research in biology, that I cannot forbear some remarks on the subject.

The view to which one is irresistibly led in 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 the more important or more promising in the begetting of offspring among the higher animals. If, for example, it is desirable to know how many kinds of fishes there are in the sea, there is no way of finding out except to go a-fishing and keep at it until no more fish can be caught; or if the question is raised whether or not the deepest depths of the ocean are inhabited by living beings, it can be answered in no

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other way than by devising and using some means of capturing organisms in these depths if they occur there. On the other hand, if it is desirable to know whether eggs that develop naturally in sea-water will develop at all, or in some modified way, in water that contains only one of the minerals found in normal sea-water, there is one possibility and only one of finding out, and that is to place undeveloped eggs in the particular kind of water about which the question is asked and see if they develop.

One method leads to knowledge of one sort, another to knowledge of another sort, generally speaking. Apparently the question of the greater importance of one method as against another could arise only as a sequel to a judgment 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 is 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.

As a matter of fact the sharp distinction frequently made between the experimental and the descriptive methods in biology has less scientific validity and less practical utility than the distinction between field, or out-in-nature methods and laboratory methods. Observation is surely essential in laboratory investigations no less than in field investigations. And no one can give an intelligible account of what he has accomplished either in laboratory or field, without description of some sort. Experiment is likewise resorted to almost if not quite always in work done with sufficient care and intelligence to meet the requirements of modern biology, in field studies no less than in laboratory studies. The testing of different kinds of closing nets, for instance, in the effort to find at what depth a particular species of pelagic organism occurs in greatest abundance, is as certainly an experiment as is the testing in a laboratory of the effect of light of different intensities on the same species. But there comes to view a distinction between out-in-nature

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experimenting and observing, and laboratory experimenting and observing that is far-reaching, not only as to application but as to scientific conceptions in the largest sense.

Much stress has recently been put upon the element of control in research. By some biologists this is held to be almost if not quite the end and aim of such research, because according to the view of these persons, control not only constitutes the essence of our understanding of living beings, but also because the highest level of utilitarian

interest in organisms is reached in this way. There can be no doubt about the importance of control, and it stands forth in particularly large and bold outline when seen from the vantage ground of research of such scope as that in marine biology, where it presents itself under two very distinct aspects. First in the field work there is the complex and exceedingly difficult matter of controlling, that is, restraining and determining, everything concerned in the investigation *except the organisms themselves*. The object is to find what organisms there are in existence, exactly where they are, and what they are doing under the conditions imposed upon them by nature alone. Control over them is what is *not* wanted even were such a thing possible, since knowledge of their mode of life *in nature* is exactly what is sought. By judicious and long-continued experimenting with the means of collecting and observing we find that very extensive information and understanding can be obtained in this way. At the same time the farther we go on this track the more numerous and the more clearly defined become problems not to be reached by these means—problems whose solutions, so far as solutions are possible, must be reached through a shift from controlling the *means* of observation to controlling the *objects* of observation, i.e., the organisms themselves. Such control can generally be exercised far more effectively and advantageously in the laboratory than anywhere else. As illustrative of the binary method demanded for handling problems of marine biology, attention may be called to Esterly's work on the vertical distribution of the copepod *Eucalanus elongatus*. So far the field results indicate an absence of a regular up-and-down migration of this species. Assuming these results to be correct, the question naturally

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arises, Why the absence of such movement when other related species perform daily excursions? Noticing the unusual transparency of the animals, Dr. Esterly makes the suggestion that their relative invisibility affords them a protection not enjoyed by less transparent species and so does away with a need for migration which the others have. How is such a hypothesis to be tested? Obviously in no other way than by laboratory experiments. Again, Miss Johnson and myself have learned a number of interesting things about the asexual propagation of certain species of salpa by studies for the most part on preserved specimens. But these studies have brought to light a number of questions which, so far as we can see, cannot be answered without keeping living, growing animals under observations for considerable periods of time. But such observation is impossible without the best of aquarium facilities—and the aquarium is only one part of a laboratory.

The mutually supplementary relation between field work and laboratory work in such an enterprise is so obvious as to make dwelling upon it seem superfluous. Actual conditions and practices, and to some extent views, do nevertheless justify insistence upon the point. For one thing the great cost and difficulty of bringing together, duly balanced, the two kinds of work are serious obstacles in many cases to the realization of the ideal. An obstacle still more unfortunate in some ways is the well-defined notion rather widely held, that field studies are of quite subordinate importance. I believe, however, that an open-minded review of past and contemporaneous biology will convince anyone of the danger that lurks in overconfidence in any *single method* of research. Possibly there are nooks of science somewhere in which one method is enough; but if so they have escaped my notice. The admonitions of experience as to exactly where monomethodic research tends, are useful. Field observation alone unquestionably encourages illy supported and more or less sentimentally colored generalization. Unsupplemented description, whether of organisms as wholes or of parts of organisms, produces results that savor more of the collector and cataloguer than of the whole-hearted student of animate nature. The laboratory too singly confided in has still greater danger

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because a danger more pervasive and subtle. There can be no question that laboratory biology may have much the stamp of museum anthropology, of library sociology, of scholastic philosophy, and of cloister theology. We must undoubtedly take many, probably most biological problems, into our laboratories for study. But the idea of learning biology proper in a laboratory or a museum is as preposterous as the idea of learning navigation from a toy ship on a mill pond. Valuable as may be the "selected types" method of elementary instruction in biology when used with discretion, its possibility for evil when allowed to gain a full mastery over independent thinking is enormous. Recognition of the direful tendencies of the method was forced upon me some years ago by reading in an exhortatory tract written by an enthusiastic teacher for his classes in zoology this well-turned, assuring epigram: "When you have dissected a fish you have dissected the whole animal kingdom." The mischievousness of such teaching would, I suppose, be admitted by most biologists to-day so far as concerns gross structure. The real magnitude of the evil is appreciated only when one sees clearly that the epigram would almost certainly be just as false if made with reference to minute structure, to physiological or psychological activity, or to chemical composition. The living world is illimitably vast, complex, and changing, and cannot be forced into a few ossific formulations by all methods of work combined, much less by some one or a few methods.

From what has been said, experimentation, particularly laboratory experimentation, would seem to play a part supplementary to observation-in-nature in only the restricted sense of assisting observation toward the solution of problems raised by the latter and found to be unmanageable by it. In addition to this important role experiment has another more independent and still higher, namely that of discovering attributes of organisms that could not have been suspected to belong to them by inspecting the organisms in their natural environment alone; or otherwise expressed, attributes that could be revealed only by bringing the organisms into relations and conditions to which they have never before been subject. For example, the fact that an animal has the attribute of behaving in a particular manner

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when subjected to a chemical environment radically different from anything either it or any of its ancestors were ever subjected to, is of the greatest significance to philosophical biology, but could be discovered by no other means than experiment. This sort of discovery is not restricted to laboratory experimentation though undoubtedly some of the most startling, most definite results have been and will continue to be reached by this method. The transference of a plant from a desert interior to a seaside is obviously much the same kind of experiment as that of the transference of an aquatic plant from water holding one chemical substance in solution to water holding another substance.

By whatever method either the form or the behavior of an organism be modified through experiment, a *sine qua non* to sound interpretation of the new form or behavior is extensive and exact comparison with the form or behavior of the same organism under natural conditions; and such comparison is impossible without extensive and exact description of the organisms both as they occur in nature and under the new conditions. So we are forced back even from a consideration of this higher use of experimentation to the demand for the most extensive and the most exact studies possible of organisms not only as they have developed and lived their lives in nature, but *while* they are developing and living their lives in nature.

These general and special considerations as to method taken together with a consideration of the problems now in hand and of the resources of the station, indicate very definitely not only what ought to be done in the near future but also what may be aimed at with prospects of a good measure of success.

As to field work the foremost desideratum is that it should be *made more continuous*. This means practically that the "Agassiz" should be kept in constant commission though not necessarily at sea all the time. The main point is not so much to increase the volume and scope of the operations, though this would result, as to complete what is already in progress. Great gaps exist in the data so far secured, and consequently most of the conclusions as to daily, yearly, and vertical distribution and

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movements of the organisms are more or less tentative. In other words, the laws governing in these matters are only partly made out. On the laboratory side the experimental work urgently calls for the completion of the salt-water circulatory system and the building of a wharf as the two most important items. The details that would be involved in these extensions need not be entered into here. It is enough to say that increase to the extent indicated, with no curtailment of what is being done, would necessitate an increase of yearly expenditure of not less than \$5000 over what is now available.

Reference should be made to the involvement in the question of future policy and development of two kinds of work which though in a sense only subsidiary to biology, are yet very important. They are research in hydrography and oceanography, and the application of mathematics to biological problems.

As to hydrography, investigations not only at this station but at several European stations have gone far enough to make it positive that as a supplement to biology the study of environment (for such hydrography as thus treated really is) must not only be kept up but must be perfected and extended if anything like sound biological advance is to be made in the directions thus far followed. There can be no hesitancy about this. The question is, Are not some of the oceanographic problems so interesting and important in themselves, that is, independent of their relation to biology, as to justify according them an independent place in the station's aims and programme? There is certainly some temptation, not to say tendency to do this. My present view is, however, that it ought not to be done *except there be special and additional funds* provided for the purpose. My belief is that although oceanographic problems are of undoubted importance, and although pursuit of them on their own merits would frequently be quite different from what it would be with oceanography held strictly subordinate to biology, important hydrographic results may still be reached without the least departure from or impairment of the original intent.

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III. THE INDISPENSABILITY OF MATHEMATICS FOR THE SOLUTION OF SUCH BIOLOGICAL PROBLEMS AS THE STATION IS ENGAGED UPON.

The question of the place of mathematics in the station's future work, although quite different methodologically from that just considered, is administratively much the same. It is obvious from a cursory examination of our latest publications that there is hardly a phase of the biological research constituting the main programme that does not even now demand constant resort to quantitative treatment involving considerable proficiency in mathematics. If the investigations continue in the course marked out, this demand will surely become more insistent. Two main and rather widely separated lines of mathematical dealing are clearly entered upon. One includes the array of problems involving the correlation between the organisms numerically treated, and environmental factors mensuratively treated. Success depends and will more and more depend on the resourcefulness and skill with which data can be segregated, coefficients of correlation computed, and so on.

The other demand upon mathematics grows out of the nature of the individual organism and depends on the fact of rhythm or periodicity, so familiar and so deep-seated in apparently all organic beings. Age in almost if not quite all organisms, man as well as the rest, with which we are concerned in practical life, is of such obviously great importance that its wide neglect by biology itself except in the most general way is truly remarkable. It would appear that science must before long take due heed of the great extent to which a given organism's morphology and physiological capacity at a given time in its life are mathematical functions of the life-career taken as a whole. Once this is duly recognized, quantitative valuation of a particular structure or activity at each particular age, relative to the value of the same part or function at various other ages extending over as great a part of the whole

and the astronomical observatory. And seemingly the labor of selecting and managing the proper material for study, of measuring, weighing and recording, segregating and computing data, will be greater than that involved in chemical and astronomical research in much the ratio that biology is greater in complexity than chemistry and astronomy.

The important administrative question arises, How is all this labor to be done? Undoubtedly much of it will be rather formal and routine and will not demand the highest ability in either biology or mathematics. At the same time, certain it is that the best results biologically would be missed without the constant maintenance, and that on a high plane, of the biological standpoint. It seems as though the demand for mathematical cleverness may be greater than the biologist is likely to possess. Sooner or later it will probably be found necessary for the somewhat mathematical biologist and the somewhat biological mathematician to join forces avowedly and regularly. Dr. Karl Pearson is furnishing a splendid example of how useful to biology the biologically inclined mathematician may be, and at the same time how disastrous it would be to the science to leave the quantitative aspects of it to workers whose chief training was mathematical rather than biological. On the other hand, among biologists, Dr. Raymond Pearl^[2]

Pearl more than anyone else among the considerable number of biologists who are now applying mathematics to biological problems with true insight and effectiveness, seems to me to deserve being mentioned because of his general discussions, especially that entitled "Biometric ideas and methods in biology, their significance and limitations" (*Scientia*, **10**, 101–119). So far as I have read, nothing approaching this paper in general grasp of the subject has appeared.

particularly is showing that mathematics called to the service of biology and kept strictly in its place as an assistant, is not only enormously important, but for many of the deepest problems absolutely indispensable.

Probably these same organizational and administrative difficulties have been and are being felt by all the sciences that have passed from the descriptive and qualitative to the exact and quantitative stage; and probably, too, they will seem somewhat less formidable to biology as they are more closely approached than they do seen at a distance—just as appears to have been the case with the older sciences.

IV. RESEARCHES NOT YET UNDERTAKEN BUT ESPECIALLY INVITING BECAUSE OF NATURAL ADVANTAGES

So far the consideration of future work has had in view the restricted programme of the station which is being actually carried out. However a general forecast such as that now occupying us would be too narrow if reference were not made to scientific desirabilities and possibilities lying beyond the present programme.

1. The migration of water birds and other phenomena of their life.

Cursory observation of the water birds of the locality suggests that various problems of migration, feeding and breeding, might be taken up to excellent advantage. An ornithologist accompanying the "Agassiz" on her cruises could undoubtedly gain much information with little increase of expense beyond the salary and outfitting of the observer.

2. The life of pelagic fishes.

The pelagic fishes offer innumerable problems that fall within the station's aims, and while under existing conditions these cannot be touched they should be kept constantly in view. Undoubtedly a combination of field and laboratory studies would be necessary for handling many of the problems, and keeping them in mind is likely to result in an attack upon them sooner or later.

3. Animals that live on the sea-bottom.

Although expediency has led to the relinquishment for the present of work on the bottom-dwelling organisms, it would be a great misfortune to lose sight of the fact that researches in this vast field are quite as fundamentally part of the station's purpose as are investigations on the free-moving organisms. Nor should it be the design to hold aloof from this until the problems of pelagic life have all been taken up. The two domains touch each other at so many points and so intimately that it will probably be better to turn to certain questions of bottom life before many years, even though this can be done only by curtailing work on pelagic problems.

4. The ultra-minute organisms of the sea.

Probably the most important field still untouched by the station, though particularly

For the introduction of this term (Greek *nannos*, dwarf) see H. Lohmann (Internat. Revue d. ges. Hydrobiol. u. Hydrographie, 4, 1–38). It may be doubted whether an appropriate name has been chosen for it, but Lohmann more perhaps than any other single investigator has made it clear that a great realm of biology exists here, the exploration of which has so far extended along its very margins only.

or dwarf plankton, in familiar terms the floating organisms of ultramicro size. Under this head would come, at least as I am now using it, the bacteriology of the sea in the large sense, as well as the treatment of any other organisms or stages in the lifecycle of organisms that are so minute as to escape ordinary methods of capture and observation.

One of the main questions that arises in this domain is, If there be such a thing as a smallest species of organism in the sea, what are its characteristics, especially those of size, mode of nourishment, and of resistance to the destructive tendencies of its environment? This question, or rather series of questions, is not only legitimate from the standpoint of observational science, but is one the answering of which marine biology is now advanced to the position for attacking. And there are several easily recognizable points of attack: What may be found in sea-water by the extreme concentration of its floating particles through subjecting it to the centrifuge? What is there in the way of organic beings in the meshes of the finest filtering media through which water will pass, after large quantities of it have been filtered? What is there in the digestive tracts of animals which though themselves minute, live on others vastly more minute? What do many small pelagic animals feed upon, the digestive organs of which have so far furnished little or no evidence of food having been taken? What causes the patches of "slick" or "greasy" water noticed by everybody familiar with the sea? What of the putrefactive bacteria of the carcasses of marine animals? Are living organisms of any kind taken ashore in the spray that is blown inland, often to considerable distances, on nearly all seacoasts almost all the time? If such transportations do occur, what is the fate of the organisms transported?

These are all questions not only of great interest in themselves but of still greater interest because of the illimitable vistas

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they open to the scientific imagination. For instance, what better experimental approach to the panspermia hypothesis advocated by Lord Kelvin, Professor Arrhenius and others, can be suggested than is here presented? If organic "germs"—better minute organisms—are carried about through all space, starting from and lodging upon the great bodies of the universe, here, there, everywhere, and all the time, what more promising place to hunt for those that may land on and pass from our earth, than the vast expanses of the sea which for eons have surely been both the germinating and the conserving beds of myriads upon myriads of organic beings?

Only a few years ago the notion of such universal dissemination of organisms could have had no standing outside the realm of poetic fancy. Now, however, that it is backed up by weighty scientific observation and deduction, and can be given a place in a soberly laid out programme of scientific investigation, one can hardly avoid stopping for a moment to ask, What after all is the difference between poetic imagination and scientific imagination? Is it not chiefly that the first runs on with a minimum of conscious reference to past objective experience and, rightly enough, neither asks nor cares much about future testing by the same sort of experience; while the latter demands a basis of considerable well-attested observations to start with, and looks forward to much rigorous testing by that same means? The interesting thing about this view of imagination is that according to it we are not dealing with two wholly distinct imaginations, but rather with one imagination used in two radically different ways. Science finds itself in a favorable position to realize the truth of this view when on rare occasions of which the present would seem to be one, it moves well to the front in some of its largest provinces. Incidentally it is wholesome to be led to see definitely that poetry and science, though so far asunder in their parts most remote from each other, really interblend in their nearest parts. Poetry has its truth primarily in man's imaginative and subjective nature and only secondarily in his objectively experiential nature; while science has its truth primarily in his objectively experiential and rational existence, and only secondarily in his imaginative and subjective existence.

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V. THE QUESTION OF MAKING THE STATION AVAILABLE TO VISITING INVESTIGATORS

This is of great importance. Even though the idea upon which the institution immediately rests does not require us to consider it, yet the larger aims of biology will not allow it to go unheeded. From a strictly business standpoint the course we have followed and are following is nothing less than absurd. Unquestionably the work done by Child on the growth-capacity of the anemone *Harenactis*, and by Jennings on the activity capacities of the starfish *Asterias*, is not surpassed in scientific value by any yet done at the station. Yet when comparison is made between the cost of these investigations dealing with creatures than can be picked up on the shore with almost no effort, and others of approximately equal scope carried out as part of the station's regular oceanic-survey programme, the discrepancy against the programme work is seen to be so great as to make one fairly gasp when looking at the case from the business point of view. And the gasping is not likely to be alleviated by the reflection that in all probability, had the original aim been to promote work on the basis on which Child's and particularly Jennings' was done, the total output might have been increased in nearly the proportion that the cost of the programme work bears to the cost of independent work, the expenditure remaining what it has actually been. There is one way of justifying such a financially absurd situation that most men (men of science at any rate) would readily accept as sufficient, namely, by insisting that the valuation of knowledge of nature in terms of money is only one way, and that the least important, of estimating its worth. But this justification does not fully reach the present case. How justify the expenditure of large sums on a particular kind of scientific research when even less expenditure on

another rather closely related kind, of no less intrinsic importance, will produce much larger returns? Justification in this case must be found by considerations that lie wholly within the realms of knowledge values; that is, that disregard money as a measuring stick. The justification comes easy and ample, according to my view, in the proposition that the widest generalizations

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which any science is capable of reaching never can be reached until the whole range of phenomena touched by such generalizations has been examined. I have expressed essentially the same conception but in a special application of it, in another place (1908) as follows: "No phenomenon essential to the life-career of any organism can be pronounced as fully explained so long as any other phenomenon likewise essential to that same life-career is entirely unknown or entirely ignored."

Brought down to an expression that fits the case in hand, this would say that the breadth and depth of biological philosophy which all biologists confidently believe possible, can never be obtained without the expansion of observational and experimental research to include, along with expansion in many other directions, just such knowledge of marine organisms as we are here obtaining and trying to obtain. In other words the practical, the business question is not, "In what field can we get the largest, quickest returns on the money invested?" but "What field is open to us and tillable by us that has been least cultivated and is least likely for various reasons to be cultivated by other instrumentalities?" Right or wrong, the course to which we are committed is that of turning such resources as we have to the supplementing of work already well in hand by other similar undertakings in various parts of the world. There are at least a half-dozen other marine biological stations in the United States, to say nothing of the much greater number in Europe, quite as well located and some of them at least much better appointed than is this station for the prosecution of such researches as the two above specified. But no other one in our country is in position, all things considered, to enable a biologist to carry out such researches as those, for instance, by Mr. Michael and Dr. Esterly.

But—and here is the important practical point—does the policy to which the station is committed rigidly exclude promotion, especially through the direct expenditure of money, of work of this independent, more generally provided-for sort? By no means. The truth is, within limits rather readily determinable in actual administration, such research may be furthered to a very considerable extent to the advantage of all interests

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concerned, particularly those of the station, without in the least hampering the programme work.

VI. THE STATION'S ATTITUDE TOWARD INDUSTRIAL PROBLEMS CONNECTED WITH MARINE ORGANISMS

I cannot, perhaps, better state what I conceive to be the wisest course for the station in this behalf, than I have already done elsewhere (Ritter, 1911). Speaking of the coöperation of the station with the State Game and Fish Commission in studying the lobster problem, and with the Bureau of Soils of the national government in a survey of the kelp beds of this southern coast, both entered upon during the past summer, the following words were used: "These industrial undertakings are at present aside from the main aims of the station. This however is in no wise due to lack of sympathy on the part of the chief patrons and officials of the Biological Association with such undertakings, but entirely to the circumstance that under the present limitations of income it seems wisest to make research the primary object. Consequently whenever, as in these cases, it happens that equipment and experience can be made to serve industrial ends without considerable interference with research, the management is more than glad thus to extend the station's usefulness."

It is wiser for us now to make research primary and loaves-and-fishes problems secondary for the simple and very practical reason that the community—the state, the nation—to which this institution belongs, is in far greater need of intellectual, spiritual sustenance than it is of loaves and fishes. The time may come when this will not be true, when physical needs will be more pressing with our people than spiritual needs. But it is surely not so at present. Should that time come it would probably be wise to reverse the order of emphasis, to make industrial aims primary and research secondary. But it is greatly to be hoped that still further support and development of institutions of applied science, already so admirable among us, will do their full share toward putting off indefinitely the day when actual want of the means of physical well-being shall be so urgent as to hamper seriously the procurement of the wherewithal for spiritual well-being.

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I. THE DUTIES TO THE PUBLIC OF RESEARCH INSTITUTIONS IN PURE SCIENCE

The reader will have recognized that although the station has up to the present devoted itself almost exclusively to research, an undoubted tendency has manifested itself to depart from this straight and narrow way. Elementary instruction was given to young people several summers; an aquarium and museum, open to the public free of charge, were maintained a number of years; from time to time popular lectures and demonstrations have been given by the investigators connected with the laboratory; recently relations have been entered into with the California State Game and Fish Commission and with the United States Bureau of Soils for the investigation of industrial problems pertaining to the sea; and in various less obvious ways efforts have been made to be of service outside the realm of pure science.

It seems desirable to place on record more fully than has hitherto been done the ideas held by the present scientific director touching the duties to the public of institutions for research in science generally and of this station particularly.

As a point of departure for what is to be said we take the assertion that science "for its own sake" as frequently understood is a false and unrealizable ideal. Science "for its own sake," art "for its own sake," wealth or anything else "for its own sake," if held without fundamental qualification, bears the germs of its own degradation if not of its death. Science can no more live "to itself alone" than can a human being. The fallacy prevalent here is in reasoning that because science and because art each has an exalted *intrinsic* nature and worth, it therefore has a nature and worth *quite apart* from its relation to other things and to men. Somehow it seems difficult to grasp the truth that the worth of science is in deepest essence *partly intrinsic* or resident and *partly extrinsic* and relative. However, that its essential worth is thus two-fold becomes obvious upon reflection.

On the one hand science has a nature of its very own. It is

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not anything else whatever. It is not religion, it is not philosophy, it is not art of any kind, it is not mathematics, it is not commerce. At the same time, equally true is it that science never has existed nor can it be conceived wholly apart from the world of *other* interests. For instance, science simply could not be without objects of nature to operate on, and appliances such as instruments and chemicals and literature to work with. And more interesting still from the standpoint of method, verification and confirmation (almost always by more than one worker) are entirely essential to science. Science is as certainly communal as it is individual.

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.

Not so with science in its relation to the higher, the spiritual life of man. Looked at from this standpoint it is truly surprising that the value attached to science should be so largely that of physical utility. To be sure, there is a rather general recognition that science, or certain aspects of it, is valuable for mental discipline, especially of the powers of observation. It is allowed, too, that science has an important function in delivering men from superstition. Beyond this little is claimed for science as a contributor to the higher needs and life of humanity. All along the line, educators, publicists, clergymen, politicians, journalists, and, surprisingly, scientific men themselves, appear to take it for granted that the office of science is primarily to minister to man's bodily needs, and secondarily to sharpen his wits. If anything beyond this comes from it, so current opinion holds, this is wholly incidental and secondary.

My belief is that science must justify its right 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

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as civilization advances, it must become ever more and more an underpinning and ally of all these.

The distinction between an institution of applied science and one of pure science might be stated thus: The former is one the primary aim of which is to use certain more or less well-established truths and principles of science to the answering of man's needs and desires in certain well-defined directions. For example, the Bureau of soils of the United States Department of Agriculture is for the purpose of applying chemistry, physics, and geology to the end of increasing the productivity of the land of the United States. The Liverpool School of Tropical Medicine is for the "perfection of physicians in tropical hygiene" and for "investigations in tropical diseases." An institution of pure science, on the other hand, should be one the primary aim of which is to extend the bounds of man's knowledge of nature in a specified field, *and* to show something of the significance of the new knowledge for the higher life of mankind. To be more definite, an institution of research in biology or in astronomy could justify its existence, in a democratic country like ours, only by making considerable additions to knowledge and then by showing, in language comprehensible to the generally but non-technically educated members of the community, something of the meaning of this knowledge for human beings in both the physical and the spiritual aspects of their natures.^[4]

The soundness of this view is dependent upon the soundness of two assumptions which cannot be argued here but which may be briefly stated: 1. The person of average natural endowment and education in the United States is capable of understanding the most essential things in any scientific discovery that has ever been made or is likely to be made for many years to come. 2. It does "matter" enormously not only to the individuals but to the nation as a whole, whether or not those who are capable of this much understanding have an opportunity to get it.

I now mention certain biological discoveries and generalizations which have, as I believe, very great importance to civilized men but which are by no means as widely known as they ought to be and might be, and which can become thus known *only through the efforts of professional biologists*.

The significance of *omne vivum ex vivo* (*all life from preceding life*) not only for philosophic biology but for the attitude of thoughtful people generally toward the problems of practical

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living, should be more clearly and firmly grasped than it has been. That the dictum is solely an expression of the summed-up results of technical science and practical experience; that so far it has not encountered the crucial

"one exception" and hence ranks with gravitation as one of the best established of nature's laws; and that its unescapable implication is that the succession of living beings in nature was without beginning, that is to say, has come from an infinite past, are matters readily susceptible of popular presentation and may be counted on greatly to interest many people, were the subject to be presented by the biologist who himself had fully grasped the problems and clearly seen their significance for human life and conduct.

The generalization, based on an enormous range of observations, that *all organic beings, including humans, are subject in all aspects of their natures, to the principle of evolution*, needs to be and may be far more widely and firmly implanted in popular intelligence than it is; and its bearings on general ideas of progress, social and other, and on popular estimates of perfection and imperfection, are very important.

That biology has been forced through its own advances, to recognize that the *struggle-survival doctrine*, upon which she earlier staked so much as the cause of evolution, *is really of very subordinate importance in this way*, needs to be set forth to the general public far more emphatically and convincingly than it has been. Undoubtedly this strictly biological doctrine has been used to justify much cruel, destructive practice particularly in the industrial world, and now that biology herself has found the doctrine to be so largely erroneous, it would seem the bounden duty of biology to rectify as far as may be the harm that has been done.

The conception of "*the reign of law*"^[5]

Were I pressed to say which of the "biological discoveries and generalizations" here mentioned is uppermost in the interest and effort of the present programme of the San Diego Station, I should almost certainly select this one of disseminating knowledge concerning the reign of law in the organic world. To learn more than we know about the laws that prevail in the wealth of life of the great oceans seems to me an object of the greatest importance for the general higher welfare of mankind. So slight is our knowledge in this domain relative to what future generations will possess, that all of us, professional biologists and the generally informed alike, look out upon the expanse of the sea with an impression concerning its inhabitants (so far as we think of these at all) that is very vague and therefore meaningless and uninteresting. We conceive this vast wealth of life *en masse* as one may say; that is, our knowledge and thoughts about it are undifferentiated and chaotic. We take for granted in a hazy kind of fashion that some sort of order prevails. Such knowledge has little power for good, either practical or theoretical. Knowledge as to what this order is must be explicit before it can be very significant and interesting. It is just this sort of definite information that the San Diego Station is striving after.

in the organic world

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ought to be much more widely and concretely established than it is in the public mind. Under stress of the necessity of dethroning notions of supernaturalism from living nature, biologists have up to now been so occupied with explaining phenomena in terms of *natural causation* that the *orderliness* of organic phenomena has had to take a back seat both in research and in speculation.

The well-established truth that apparently all organic beings have in nearly if not quite all their parts and functions *capacities far beyond those needed for ordinary life*, frequently far beyond what are ever used excepting under very unusual circumstances, is of great significance for a general theory of life. But being so comparatively recent a discovery, and standing in sharp contradiction to the widely prevalent views about the "economy of nature," and to the utilitarianism of the Darwinian theory of natural selection, it has as yet found little place in either the learned or the popular theories of life. The general enlightenment needed on this matter might come partly from teachers, secular and religious, partly from psychologists, but most basally from biologists.

The conception of "*the organism as a whole*" that has been forcing itself into biology, particularly from the side of embryology, is destined to have a far-reaching, elevating influence on general beliefs, attitudes and practices. There is no likelihood that the idea will be brought into the full light of day in any other way than at the hands of biologists. Poets and poetical humanists in all ages have had much to say about "the whole man," but the idea appears never to have germinated to the extent of greatly influencing the every-day lives of ordinary mortals. Biologists must be the original culturists here as they have been in so many other realms of things germinal.

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The hypothesis that all phenomena of organic beings, including those pertaining to the very highest aspects of human nature, *are correlated with chemico-physical phenomena*, though not yet rigorously demonstrated in most of the subtler psychic and aesthetic provinces, is securely established over so wide a range of life phenomena and has thus far so well withstood rigorous efforts of disproof, that without doubt it has already greatly influenced general thought and attitude toward the deep problems of human life, and will more and more influence them. In a matter so vital, and one about which general intelligence is bound to be so widely astir for such information as can be had, it is of the greatest moment that information from the best sources should be readily available.

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 attached to these laws, and to the possible good 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 simple, readable language.

That *imperium in imperio* of human concerns, *the problem of the relation between the sexes* is calling almost frantically to the biologist for help at certain points where it is coming to see dimly that he alone can help. A few

investigators are doing splendid things in this way, though what has been done is but as molecule to mountain relative to what remains undone.

Finally, without a doubt, *innumerable bald, unphilosophized facts of living nature that would 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.*

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And undoubtedly many, perhaps not all, professional biologists are abundantly endowed by nature with the ability to do this extracting and preparing for general consumption. Acquiring the knack to do it is dependent first and foremost on being convinced that it ought to be done. 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; and there is every reason to believe that this instinct would come out as literary skill here and there, as well as in the form of skill in delineation, were the need felt as keenly in the one case as in the other.

Assuming the contention to be sound that biological knowledge ought to be more widely disseminated than it is, and that so far as concerns the capabilities and desires of people such dissemination is possible, the familiar question arises, "What are you going to do about it?" "The schools!" Nine out of ten, I suppose, of those who would assent to my contention would turn automatically in this direction.

To forestall doubt about my just appraisal of the schools, the college, the university, in educating the young, I refer to an article ("Feeling in the interpretation of nature," *Pop. Sci. Mo.*, 79, 1911) in which I have taken the ground that these instruments ought to and could, do vastly more than they do toward making the people appreciative of and intelligent toward nature. Here I would insist that no matter how efficiently and broadly the tasks of institutional instruction might be performed, they would still have to be extensively supplemented before the real saving power of knowledge could be realized. This supplementing would have to be done in two places particularly: In the home for young children before school age is reached; and for grown-ups after the school period is passed.

Our eyes must be opened in some way to the fact that education taken in the full sweep of its meaning, is too life-and-death a matter for us as a nation to be left to the formalities of the schoolroom, the university lecture hall and the laboratory, even though these be excellent beyond the possibility of improvement. This truth is being forced upon us at a few points. As one

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instance, it is becoming clear that wider instruction on sex matters is imperative, and that parents and the home primarily, and the school secondarily, must be looked to for the broader, better knowledge. Again, the simply incalculable power of the press and the speaker's platform for educating and influencing the voting part of the population are recognized and resorted to upon occasion.

I may now state my views summarily: Biological science, as now developed, contains numerous facts and generalizations of very great moment to the higher intellectual and spiritual life of the people generally. The essence of all these can be stated in language readily comprehensible to persons of average intelligence and education. Most if not all these facts and generalizations are of such nature as to make their strongest appeal to the majority of people only from their bearings on problems of personal experience, so that in the nature of the case they can be of living interest and significance to such persons only after the period of formal schooling is past and the business of actual living is on. Instruction concerning them must, consequently, be given by other means than the school. Some of the most important instrumentalities for such instruction are the botanical and zoological garden, the natural history museum, the aquarium, the library, the lecture platform, and in some ways most important of all, the public press.

And now for the culminating point: In the main the instruction given through all these instrumentalities must be by *professional biologists*. It will never be done well, that is, in a manner at the same time vivacious, convincing, and dependable, by persons who have merely "read up" on biology with nothing but an elementary training to start from. Only persons constantly occupied with the first-hand gathering of data, with the making and testing of hypotheses, and with the submitting of results and conclusions to fellow-workers for criticism and verification, can do the safest teaching in these ways.

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

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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. The press, as already said, is probably the most available and powerful of all such instrumentalities.

I would not be understood to mean that every person regularly employed by institutions of research in non-industrial science should be held responsible for a certain amount of popular writing or lecturing or arranging of collections or the like. Such an idea put into practice would undoubtedly carry disaster in its train not alone to the institutions but to the cause designed to be promoted. My view is that these institutions, *as 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.

It is true something in this way is being done by some, possibly all, of the research foundations of the country. But in very few if any, so far as I can judge, is the doing accepted as a weighty obligation and as a set policy. So it happens that what is done is an exceedingly small fraction of what ought to be and might be done.

Under its present management, the Marine Biological Station of San Diego holds duties in this direction to be as incumbent upon it as are those of making discoveries about the Pacific Ocean and the things that live in it.

Submitted December 28, 1911.

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*. Although most of Miss Monk's work was done independently of the station, this final paper of hers is included for the convenience of whoever in future shall pursue further this important study.

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A. ARTICLES OF INCORPORATION OF THE MARINE BIOLOGICAL ASSOCIATION OF SAN DIEGO

Know all men by these presents, that we, the undersigned, a majority being citizens and residents of the State of California, have this day voluntarily associated ourselves together for the purpose of forming a corporation under the laws of the State of California, and we hereby certify:

1. That the name of said corporation is the "*Marine, Biological Association of San Diego*."
2. That the purposes for which said corporation is formed are:
 - a. To succeed to all the rights, property, and privileges held by the Association designated the "*Marine Biological Association of San Diego*."
 - b. To carry on a biological and hydrographic survey of the waters of the Pacific Ocean, adjacent to the coast of Southern California; to build and maintain a public aquarium and museum; and to prosecute such other kindred undertakings as the Board of Directors of said corporation may, from time to time, deem it wise to enter upon.
 - c. To transfer the whole or any part of the properties, rights, and privileges of said corporation to the Regents of the University of California, the same to become a department of the said University, coordinate with its already existing departments, and to affiliate with said University.
 - d. To acquire by purchase, gift, or otherwise, and to hold and dispose of all kinds of properties, both real and personal.
3. That the place where the principal business of said corporation is to be transacted is in the City of San Diego, County of San Diego, State of California.
4. That the time for which said corporation is to exist is fifty years from and after the date of its incorporation.
5. That the number of directors of said corporation shall be seven, and the names and residences of the directors, who are hereby appointed and elected for the first year to serve until their successors are elected and qualified, are as follows, to wit:

Names	Residences
Homer H. Peters	San Diego, California.

Names	Residences
Ellen B. Scripps	San Diego, California.
W. E. Ritter	Berkeley, California.
Julius Wangenheim	San Diego, California.
Fred Baker	San Diego, California.
E. W. Scripps	West Chester, Ohio.
Jas. MacMullen	San Diego, California.

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6. That the above named directors were duly elected at a meeting of the members of said Association, duly convened and held in the room of the Chamber of Commerce of San Diego, County of San Diego, State of California, on the 3rd day of May, 1904, for the purpose of forming this corporation; and of electing directors to take charge of, and the management of its property and affairs, and to form this corporation; that a majority of the members of said Association were then and there present and voted at said meeting and election for the above-named directors; and as a result of said election, the above-named persons were unanimously elected directors of said corporation.
7. That this corporation is not formed for the purpose of pecuniary profit; that there are no shares, no capital stock, and no subscription to stock or shares by any person.

In witness whereof, we have hereunto set our hands and seals this 5th day of May, 1904.

Ellen B. Scripps
E. W. Scripps
Julius Wangenheim
W. E. Ritter
Jas. MacMullen
Fred Baker
Homer H. Peters

B. BY-LAWS OF MARINE BIOLOGICAL ASSOCIATION OF SAN DIEGO, A CORPORATION

I. [No Heading in Originals]

The name and purpose of this corporation are those set out in its Articles of Incorporation.

II. [No Heading in Originals]

The officers of this corporation shall be a President, Vice-President, Scientific Director, Secretary and Treasurer.

III. [No Heading in Originals]

The officers of this corporation shall be elected at its regular annual meeting, which shall be held on the second Tuesday of July of each year, at ten o'clock *a.m.*; and it shall not be necessary to give any notice of such annual meeting.

The officers shall hold office for one year and until their successors shall have been duly elected.

IV. [No Heading in Originals]

The Board of Directors shall have power to fill vacancies in the offices, arising from any cause. It shall be their duty to carry out the purpose of this corporation. They shall have full control of all funds of the corporation, and the management of the institution. They shall

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make a full report of all moneys received and disbursed and of the affairs of the institution at each annual meeting. They shall have power to elect new members of the corporation, a majority of the directors being requisite for such election.

The President and Vice-President, or any three members of the Board shall have the power to call a special meeting of the Board, or of the corporation at any time, by depositing in the United States Postoffice at San Diego, a notice addressed to each member of the Board or of the corporation, postage thereon being prepaid, two days before said meeting; and special meetings of the members of the corporation may be called on the written request of one-half of the members of the Association, and like notice of said meeting shall be given.

V. [No Heading in Originals]

The Board of Directors may, at any time, fix a limit to the number of members of the corporation, provided, however, that this limit shall never be less than twenty-five (25).

VI. [No Heading in Originals]

These by-laws may be amended by two-thirds vote at any regular or special meeting of the members of the corporation, notice of the proposed amendment having been mailed to each member of the corporation at least fifteen (15) days before the meeting at which a vote on said amendment is to be taken, or in the manner provided by the general laws of the State of California.

The foregoing by-laws are hereby adopted and approved this 23rd day of May, 1904.

C. RESOLUTION RELATING TO THE PURPOSES OF THE MARINE BIOLOGICAL ASSOCIATION

At the annual meeting of the Marine Biological Association of San Diego held July 20, 1907, the following resolutions, presented in substance by Mr. E. W. Scripps, were unanimously adopted:

Whereas this Association desires to acquire pueblo lot 1298 containing about 160 acres, now owned by the City of San Diego, and

Whereas some explanation may be due to the City Council and public of San Diego as to the Association's need for so large a tract of naked land,

Therefore be it resolved, That this Association set forth its plans and intentions with reference to the future development of the Biological Station as follows:

1. It is declared that said land would be owned by and for the exclusive use of the Biological Association, until such time as the ownership and management of this as of all the belongings of the Association shall pass to the Board of Regents of the University of California in

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accordance with a provision in the articles of incorporation of the Association.

2. It is proposed to erect for the scientific work of the station a commodious, imposing structure, one section of which will be built at once at a cost of about twenty thousand dollars. This will give some idea of the nature of the structure when finally completed, and its need for adequate setting.
3. The next step will be the erection of cottages for the accommodation of the various attaches, including a number of University of California professors.
4. The erection of other buildings for the housing and accommodation of visiting naturalists and scientists who will engage in the work here at various seasons.
5. 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 investigations, all of which require much ground space.

Briefly, it is the intent of these resolutions to inform the public that while at present the Association has not sufficient funds to enable it to carry out all its designs, it is its purpose to create on this foundation an institution of biological research the bounds for the expansion of which shall have no limits except those of biological science itself, and such as may be set by limitation of means and facilities.

D. LIST OF INVESTIGATORS WHO HAVE WORKED AT THE STATION

Ackert, J. E.	University of Illinois.
Allen, Dr. Bennet M.	University of Wisconsin.
Allen, W. E.	University of Nebraska.
Bailey, S. E.	University of California.
Bancroft, F. W.	University of California.
Billinghurst, B. D.	Prescott, Arizona (Sup't of Public Schools).
Bovard, J. F.	University of Oregon.
Burbridge, W. C.	Stanford University.
Carlson, A. J.	Stanford University.
Child, Dr. C. M.	University of Chicago.
Cockerell, Dr. T. D. A.	Las Vegas, New Mexico.
Coe, Dr. Wesley R.	Yale University.
Colton, Dr. Harold S.	University of Pennsylvania.
Congdon, Edna	University of California.
Cort, W. W.	University of Illinois.
Crandall, W. C.	San Diego State Normal School.

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Crocker, Gulielma R.	University of California.
Davis, Dr. B. M.	Los Angeles State Normal School.
Davis, D. W.	University of California.
Esterly, Dr. C. O.	Occidental College.
Evans, Herbert M.	University of California.
Foote, Ethelwyn	Pasadena, California.

Gutberlet, J. E.	University of Illinois.
Henderson, Margaret	University of California.
Hindle, Dr. Edward	School of Tropical Medicine, Liverpool, Eng.
Hubbard, Marian	Wellesley College.
Jennings, Dr. H. S.	Johns Hopkins University.
Johnson, Dr. H. P.	University of California.
Johnson, Myrtle	University of California.
Johnson, Riley O.	Chico State Normal School.
Juday, Dr. Chancey	University of Wisconsin.
Kleeberger, Frank	University of California.
Kofoid, Dr. C. A.	University of California.
Linville, Dr. Henry R.	Jamaica High School, New York.
Long, Dr. J. A.	University of California.
Mark, Dr. E. L.	Harvard University.
Marine, Dr. David	Western Reserve University (Medical Dep't).
Martin, Ann	University of California.
McClendon, Dr. J. F.	Randolph Macon College, Virginia.
McEwen, Dr. G. F.	Stanford University.
Mereschkowsky, Dr. K. S.	University of Kasan, Russia.
Michael, Ellis L.	San Diego Marine Biological Station.
Miller, L. H.	Los Angeles State Normal School.
Monks, Sarah P.	Los Angeles State Normal School.
Moore, Fred	University of Iowa.
Morgan, Dr. T. H.	Columbia University.
Morris, B. L.	Stanford University.
Nichols, Maurice B.	University of California.
Nutting, Dr. C. C.	University of Iowa.
Oldroyd, Mrs. T. S.	Long Beach, California.
Paden, Agnes	University of California.
Powers, Dr. J. H.	University of Nebraska.
Raymond, Professor W. J.	University of California.
Reese, Dr. A. M.	University of West Virginia.
Rommel, A. J.	University of California.
Richardson, Grace	Oceanside, California.
Rigden, E. Josephine	University of California.
Ritter, W. E.	San Diego Marine Biological Station and University of California.
Robertson, Dr. Alice	Wellesley College.
Shelford, Dr. V. E.	University of Chicago.
Starks, Dr. E. C.	Stanford University.
Stevens, Dr. Nettie M.	Bryn Mawr College.
Stokes, Susan	Orange, California.
Streedain, A. B.	University of California.
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Stringer, Miss Caroline	Omaha High School.
Torrey, Dr. H. B.	University of California.
Toy, Susie M.	Oceanside, California.
Turrentine, R. J.	Mt. Vernon, Missouri.
Van Orden, Dorothea	University of California.
Von Geldern, C. E.	University of California.
Ward, Dr. Henry B.	University of Illinois.
Watson, Dr. Edna Earl	University of California.
Wells, Clarence W.	University of California.
Williams, Dr. R. D.	University of California.
Williamson, Mrs. Burton	Los Angeles, California.
Wilson, Dr. E. B.	Columbia University.
Young, Helen	University of California.

E. ADVANTAGES AND DISADVANTAGES OF THE POSSIBLE LOCATIONS CONSIDERED BEFORE THE DECISION WAS REACHED IN FAVOR OF LA JOLLA

For

Coronado

Against

Coronado

For

1. Accessibility.
2. Living facilities.
3. Safety of small boats.
4. Convenience for mud-flat collecting.
5. Bay plankton.

Against

1. Difficulty of getting good ocean water.
2. Poor quality of bay water.
3. Difficulty of landing large boats in ocean or bay.
4. Remoteness of rocky collecting grounds.
5. Remoteness from oceanic conditions.

Roseville

1. Safety and convenience for all kinds of boats.
2. Convenience to rocky collecting grounds.
3. Convenience to mud-flat collecting grounds.

1. Inaccessibility.
2. Living facilities.
3. Remoteness from ocean water.

La Jolla

1. Accessibility to oceanic conditions, deep water, plankton, etc.
2. Good ocean water for aquaria.
3. Rocky collecting grounds.
4. Accessibility.
5. Living facilities.

1. Unsafety of boats, small and large.
2. Distance from mud-flat collecting.



Fig. 1. Marine Biological Station at La Jolla, viewed from the south.

[Full Size]

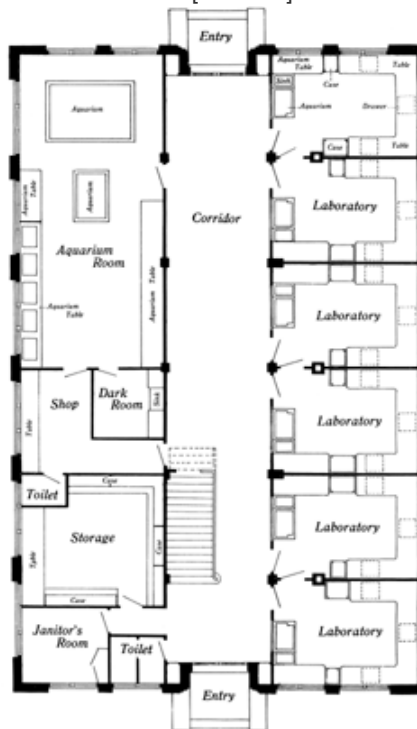


Fig. 2. Ground floor plan of Laboratory.

[Full Size]



Fig. 3. Second floor plan.
[Full Size]

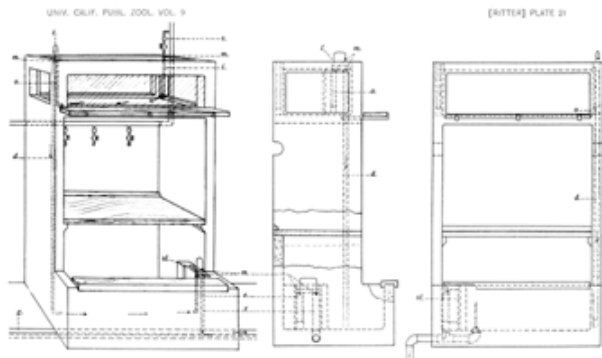


Fig. 4. Diagrams of private research aquarium. *A*, front. *B*, end. *C*, back.
[Full Size]

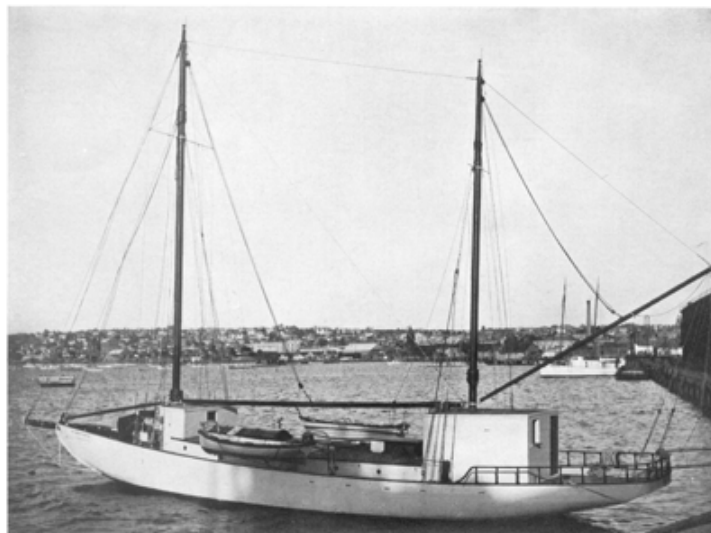


Fig. 5. The "Alexander Agassiz" as she is now.
[Full Size]



Fig. 6. Deck view of the "Alexander Agassiz" before she was remodeled.
[Full Size]



Fig. 7. View of one corner of the Library.
[Full Size]



Fig. 8. View of one corner of Reagent and Collection Room showing arrangement of general working plankton collections.
[Full Size]



Fig. 9. Marine Biological Station viewed from the hillside to the northeast, showing La Jolla Point.
[Full Size]

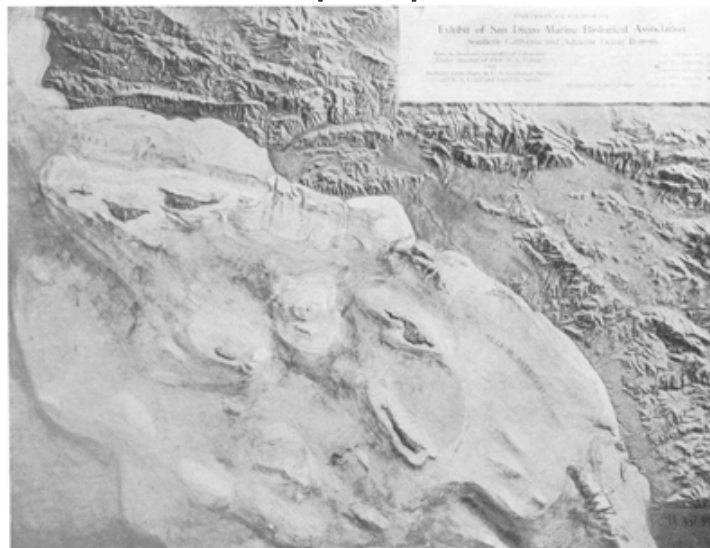


Fig. 10. Topographic map from Point Conception to Mexico, showing coast, islands and sea-bottom. Compare with Outline Map 1.
[Full Size]
