AN OCEANOGRAPHER'S PERSPECTIVE

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I suppose it's my turn now to add what I can about an oceanographer's perspective on this system. I think perhaps I'd better say how I got into the trade, because I came in sideways and unexpectedly. I entered Scripps as a graduate student in physical oceanography in 1948, my interest in the ocean having been engendered by some naval service. I was a student of Walter Munk's for a while, but he couldn't quite keep up with me, so I got some half-time research support, the usual kind a graduate student gets, from Marine Life Research.

About 1950 or '51 I began to attend these conferences. Well, I felt myself a very sharp young physical oceanographer who was certain to straighten these people out in a very short period of time. I could not understand—as you will, I think, from having heard what these people have said, particularly Jack Baxter—why all of these people, the feds on one side and the loosely called confederates on the other side, were having such terrible battles. I really thought they assembled annually to hear me tell them about the physical oceanography of the California Current. It took me a while to come up to speed on this.

But the format of the conference was a little different in those days. There was a higher proportion of business discussed at that time, although the papers given still dominated the sessions. I would usually give a paper, and being very full of the kind of research that could be done in physical oceanography, I would talk for fifteen minutes or so about geomagnetic electrokinetographs (GEK's), acceleration potential, geopotential anomalies, electromotive forces, and things of that kind. About half-way through my talk, Frances would look at me and say, "Joe, what do you really mean?" . . . I think it might be useful if questions of that nature were asked more frequently in the middle of these presentations. Of course I did have something to say, but I need not have said it quite in those words. By the time I thought about it and expressed it in a more rational way, nobody had any trouble understanding it. It really wasn't all that complicated. In fact, I think some biologists read my papers in physical oceanography in preference to others because at least they can understand what I'm saying, and if they can, they should thank Frances.

Then I would finally get to the end of my spiel, and Roger Revelle would get up and say, "That's not so." And I would say, "Yes, it is!" And we would argue. Here I was, the lowest graduate student on the totem pole at Scripps. I don't mean to imply that Roger was always wrong or always right, but in those days, it didn't matter at all whether Roger was right or wrong; he would win those arguments. There was just no question of it. I would look out into the audience, and there would be Ahlie Ahlstrom and Dick Croker and the like, grinning like apes at my discomfiture, of course. Then as I left the stage, Frances would look at me with her kind, sweet smile and pat me on the back. Our technical director, Bob Miller, whose name has come up before, would take me out and buy me a drink. That was, I guess, one of his job requirements under those circumstances, one of his many functions to keep the place going. Then we would be ready to continue.

What was oceanography in those days, and what were we doing? I suppose the state of physical oceanography in that period—and, in fact, of oceanography in general—could be summed up in the 1942 work of Sverdrup, Johnson, and Fleming because, of course, the war had intervened, and very little had been done in most fields of oceanography during that period. It was Harald Sverdrup himself who started the work in physical oceanography here, and he was one of the major protagonists in the proposals that got CalCOFI started. We began with his ideas and the people that he had trained or had begun to train. The physical oceanographers in those days were Bob Reid and Paul Horrer and Warren Wooster, who had come a little earlier than I had, joined shortly by Feenan Jennings. I don't know what you think of this motley crew, but that's what was assembled out at the Scripps Field Annex at Point Loma in those days.

What was there to work with? Up to that time oceanography had been dominated almost entirely by the work of the Scandinavians and more lately by the Germans, with some from the U.S. The work was either large-scale exploration—the *Dana* expeditions, the *Carnegie,* the *Deutschland,* trying to get at the general circumstances of the ocean—or of localized bays and estuaries. Nansen had done his work on the source waters of the North Atlantic; Brennecke had worked on the source at the other end, in the Weddell Sea; and we knew at least that the major deep water masses of the world ocean were formed mostly in the Atlantic. But the kinds of studies that they were able to make were limited to statements about the general circulation or the mean flow, with little time or money for finer-scale work on variability.

However, these people were very sharp. In fact, there is a wonderful paper by Helland-Hansen and

Nansen in 1909. They had gone to sea making traditional hydrographic casts, measuring what they could, and when they analyzed their data, they found not large-scale smooth fields but rather wobbly ones. There would be a station-to-station variation of the depth of various isotherms. Of course, they didn't get to sea very often, and they couldn't repeat their measurements as much as they liked, but they at least thought about them and wrote about the patterns. They didn't know whether the patterns represented mean flows on a smaller scale than people had supposed, or were waves of some kind, huge solitary waves, or were little anticyclonic and cyclonic gyres. They were able to repeat a few of their lines here and there and found that things changed in time as well as in space. Their work was limited by the facilities at hand, but they did say such things as, "Gee, it looks different from time to time. We really should try to augment our work and repeat lines whenever we can, make our observations as close together as possible to avoid aliasing, and whenever possible, stay in one place for a while, repeating the observation to get some feeling for the time variation." That was 1909. Now, that would have been a perfect prospectus for the Atlantic MODE operation if anyone in the MODE group had ever read the paper, but I don't think they had.

That was the background in which we were working. Some notions of the general circulation were at hand in 1948 when the cruises were planned, but we had little feeling for the kind of spacing required to define the field of flow and patterns of the other characteristics, and at what intervals we would have to occupy these stations to follow their time-fluctuations.

Cruise I of MLR and CalCOFI went out in March of 1949, and I was on the *Horizon* (the old *Horizon,* not the *New Horizon*). We also had in that early period such ships as the *Paolina-T* and the *Crest.* May I have a show of hands of people in this group who were ever at sea on the *Paolina-T*? Bravo. Well, I don't have to tell you what she was like. She was a perfectly seaworthy vessel. She would never sink—though we kept hoping! She just didn't care which end was up.

You remember perhaps what our 1949 pattern was like. We had about twelve lines spaced 120 miles apart going offshore from the west coast of North America, and the stations were spaced about forty to sixty miles apart along each line. We found out from the first few cruises that we hadn't done it quite right. We discovered by our own examination, not from anyone else's sage advice, that we would have to tighten this line of stations and spacing in order to get what we were after. I think I'm talking mostly about the physical oceanographic aspects of it, but it certainly proved to be true, and perhaps might have been noticed even earlier, in the egg and larva work. I'm not going to criticize the biologists at this stage; I am sure there are enough of them to do it for each other.

But this was a primitive period of physical oceanography. Out in the California Current we found strange shapes that did not fit any of the concepts we had in mind. There were unexpected loops and whorls in the temperature field on various scales. We calculated that the topography of this sea surface had undulations on a scale that we hadn't anticipated. And what were these?

At that time, someone decided they must be internal waves of a semidiurnal period, and we invited a very distinguished Austrian oceanographer, Albert Defant, to come and work on it. He came for a year or so and tried to make some accounting in terms of the concept of semidiurnal internal waves, but that wasn't the right answer. In fact, when he left, I was assigned to carry out, on the later cruises, the same kind of calculations that he had made for the earlier ones. But it seemed obvious to me that this was wrong, and I was able by that time—believe it or not, with Frances's encouragement and Roger Revelle's education—to approach people in the right way and say that we should not do this sort of smoothing any more.

The program, of course, was not just physical oceanography, but was intended to include biology and chemistry as well. Warren Wooster was a chemical oceanographer in those days before he moved more into the physical end. We did try to measure oxygen, which we did successfully. We were not so successful with nutrients at that time. We tried in the first year or so, in fact, to measure chlorophyll, but our primitive techniques were not good enough. Finally, the chlorophyll program was dropped and only recently reinstituted.

We did, on the basis of, say, the first ten years of data, begin to find some reasonable patterns relating some variables to others. As you recall from reading the proceedings of the 1958 Rancho Santa Fe Symposium *(CalCOFI Reports, Vol. VII),* people such as John Radovich had even then been able to establish relations between some patterns of fish distributions and the sea surface temperature anomalies. There were relations between those two and various kinds of fish that were taken both commercially and by the sports fishermen; that is, there were nonseasonal northward and southward movements of various species that tied in rather well with variations in the physical characteristics of the ocean.

Also, even at that period, the data showed an inverse relation between the temperature and the zooplankton volume. With only seven or eight years of data, the statistics were not as convincing as one would like, but I think both of those conjectures have been borne out by the following twenty years of data, as shown in the recent studies by Bernal and Chelton. So we have accomplished some things and established some relations and patterns, but we haven't learned everything. We certainly have not yet learned enough about the system to make a firm prediction about the success of each year class of the major fisheries.

Now, does that make us unique in studying the ocean? I don't know. We've done a number of things in the California Current domain, including work with drogues and direct measurements of flow with GEK's. We've studied such things as inertial rotation and upwelling; I won't try to list all of the things the biologists have done in this program.

But in general, what have the CalCOFI meetings achieved? What has been discussed or, to put it another way, what has not been discussed at these meetings in the thirty years that I have been attending them? I remember that, at the time, the earlier conferences seemed to be dominated by minutely detailed presentations of successions of year classes of various species beginning from before the time of the Flood. After all, I suppose Noah was the first oceanographer. (I mean Noah the biblical character, not the governmental body. But I think the record shows that after only one major cruise he left the field and took up growing grapes.) But as a newcomer to the field, I found some of those presentations as dreadful a bore as some of my discussions of the GEK must have seemed to Dick Croker and to Ahlie and others.

After all, I expected that the physical oceanographers would have the whole problem solved within a year or two. (I was very young.) But after a while it began to appear that the problem was a great deal more complicated, difficult, and in fact much more interesting than I had imagined. The sum of all of those charts on long, long sheets of butcher paper (audio-visual aids were rather primitive at that time) began to take some kind of effect.

The term *ecology* was not used so frequently or so lightly in those days, but is that what we were up to, even then, whether we recognized it or not? The topics discussed covered everything—winds, currents, temperatures, eggs and larvae, salinities, nutrients, chlorophyll, oxygen, food webs, internal waves, diurnal migration, inshore and offshore populations, eddies, spawning seasons, upwelling, narrow- and wide-ranging species, larval survival, catch per unit effort, competition among species, chlorophyll maximum, and light penetration. I know I've left out a number of items, but we can't go on all day with this.

How many times have you heard someone in this field suggest that there should be a meeting involving people in all of the disciplines appropriate to fisheries, or to ocean circulation, or to the nutrient distributions, or to ocean productivity? Anyone who has attended CalCOFI meetings for any length of time and listened carefully would have learned that no one of these problems can be attacked separately. We must take bits of information from all of these fields and put them together. Each contributes to the understanding of the others. They are simply different aspects of the same problem.

At CalCOFI we have for years maintained a breadth of view and assembled a range of disciplines and interests that is outstanding among the meetings I attend. Indeed, so many scientific meetings are becoming more and more narrowly focused. This may be proper in some cases, of course, but we should at least see that some meetings are broad enough to encourage people from wide ranges of disciplines to attend and exchange information and ideas.

At last year's meeting, we had an excellent, concise presentation on the state of the fisheries, with some historical review. Later John Hunter stated, among other things, that the anchovy population off southern California eats about thirty million tons of copepods each year. Now, it's not the first time I had heard that, or analogous information, but somehow the context in which his facts were put really got to me. All of a sudden, I realized that what we have done in CalCOFI's history is to assemble the world's greatest and most nearly complete set of background information bearing upon an ecosystem of this scale. Of course we've not done enough. The word *ecology* has been used much too lightly by many people, and we are not yet ready to say that we understand this ecosystem—that is, that of the waters off the Californias—but are we not in a much better position than most such groups?

This may be what the creators of this program had in mind, or they might have, more than thirty years ago, expected easier and narrower solutions. I don't know, but in any case, I think they have built very well. It is our turn now to continue putting these many pieces of information together to try to understand the system. I think the burden is on us for two reasons. One is that we owe it to the people who created and supported the program. The other is that we may already be closer to understanding our system than any other group is for its own particular part of the ocean. It would be dreadful if we fell behind.

I would like to mention a little more about the people who have spoken today. As I said, in her presentation Frances did not talk very much about the science she did. She made very little reference to her science, though she has done a great deal. Instead, she and the others have talked mostly about the beginnings of this field, the means by which these programs were created, funds obtained, equipment arranged for, labors of Hercules performed, problems faced at each stage.

Now, I and many of you have made no such contributions as yet. We've been simply riding along on their shoulders. They've made it possible for us to do what we have done and to continue doing what we are doing. They know this. Do you? If you do, do you make the mistake of thinking that all of their efforts were spent in management and none in science? If so, look at the record and think again.