Intergovernmental Oceanographic Commission

THE POTSDAM LECTURE

FORECASTING OCEAN SCIENCE? PROS AND CONS

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Presented
4 October 1999
Potsdam
Germany

A Contribution to the IOC/SCOR/SCOPE Assessment Workshop
on The State of Marine Science
and its Contribution to Sustainable Development

United Nations Educational, Scientific and Cultural Organization

For bibliographic purposes this document should be cited as follows:

The Potsdam Lecture, 1999. *Forecasting Ocean Science? Pros and Cons*, E. Seibold; IOC Technical Series 61, UNESCO 2002 (English only)

Published in 2002
by the United Nations Educational,
Scientific and Cultural Organization
7, place de Fontenoy, 75007 Paris

Printed in France

SC-2002/WS-50

PREFACE

The IOC/SCOR/SCOPE[1] Assessment on The State of Marine Science and its Contribution to Sustainable Development was the third of its kind. The first IOC/SCOR assessment of marine science took place in Ponza, Italy, in 1969. Its results were published as "Global Ocean Research" (SCOR, 1969). A follow-up IOC/SCOR study of expected major trends in ocean research up to the year 2000 was held in Villefranche, France, in April 1982. Its results were published as "Ocean Science for the Year 2000" (IOC, 1984).

As part of the third assessment, this time co-sponsored in addition by the Scientific Committee on the Problems of the Environment (SCOPE), a number of distinguished representatives of the marine scientific and 'user' communities were invited to attend a Workshop in Potsdam (October 4-7, 1999).

Because Professor Eugen Seibold had been personally involved in both of the previous ocean science forecasting exercises, in fact chairing the second one, he was invited to provide the participants at the Potsdam Workshop with a Keynote Address representing his personal view on the process of forecasting ocean science, and the conditions necessary for success. What follows is essentially the text of his address. It contains useful lessons for considering change.

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Scientific Committee on Oceanic Research (SCOR)
Scientific Committee on Problems of the Environment (SCOPE)

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Eugen Seibold, born in May 1918, is Honorary Professor of Geology and Palaeontology at the Geological Institute, Freiburg University, Freiburg im Breisgau, Germany. Educated at Eberhard-Karls-Universität Tübingen, Germany (1938-1939), and at Rheinische Friedrich-Wilhelms-Universität Bonn, Germany, he obtained his Doctor rer. nat. from the Eberhard-Karls-Universität Tübingen, Germany (1948). He was Assistant Professor of Universität Fridericiana Karlsruhe from 1952 to 1954; Associate Professor of Eberhard-Karls-Universität Tübingen from 1954 to 1958; Full Professor of Christian-Albrechts Universität Kiel from 1958 to 1980; and President of Deutsche Forschungsgemeinschaft, Bonn from 1980 to 1985. He is Past-President of the European Science Foundation (1985-90), and the International Union of Geological Sciences (1980-84). He is Member of the Balzan-Prize Committee, Milan; and Member of Several Academies in and out of Germany. He is one of the world's experts in marine geology and has published more than 160 articles in scientific journals and several books. He holds Honorary Doctorates from the University of East Anglia, and the University of Paris VI. For his outstanding contributions, he was awarded the Albert I Medal by Monaco, and received the Blue Planet Prize (Tokyo, 1994).

FORECASTING OCEAN SCIENCE? PROS AND CONS

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INTRODUCTION

When I was asked to give a lecture about my experiences in forecasting ocean sciences, I hesitated because I remembered a Chinese saying: *"Experience is a comb which nature gives us when we are bald"*. But perhaps I shall be able to offer you such a comb, seeing that so many of you in this active audience are embellished with wonderful hair.

Nearly 20 years ago, in 1981, the IOC Executive Council asked SCOR and other organizations to bring together a group of active oceanographers. They should propose plans for the future and evaluate the outcome of a similar experiment from 1969 that took place in Ponza and was published as "Global ocean research" (SCOR, 1969). The new group produced "Ocean Science for the Year 2000" (IOC, 1984). As in Ponza, Warren Wooster did the work of writing; therefore, in Villefranche I could comfortably relax as chairman.

In Ponza we had worked day and night in a lonely hotel, and Warren had asked the 27 participants not to bring any literature with them. Excellent experts, we were told, should know important issues by heart. And following UNESCO's definition at that time that everybody is an expert outside his own country, we were certainly experts. In Villefranche we had asked some of the participants half a year before the meeting to produce several background papers. These were distributed and discussed by the whole group. Finally, the draft report was discussed during the Joint Oceanographic Assembly, in Halifax, in August 1982.

Most of us, both in Ponza and in Villefranche, were scientists; therefore my experience and my examples, often taken from the geosciences, are biased.

In 1982 I felt proud to state that the geological group at Ponza in 1969 had been very successful in its forecasting. Half of the 11 programmes that had been proposed to address important questions had more or less solved the problems they had posed, at least in principle. The other half needed much more work. What explains this success? The new global tectonics concept had originated in the early 1960s, and as from 1968 geoscientists had been able to use the state-of-the-art drill-ship, *Glomar Challenger*, to test the concept. The geoscientists had a global, guiding idea, a timely instrument, effective organization, and good scientists on the ship. And they were able to define their questions well.

A physical oceanographer at the Villefranche meeting felt that this 50% success did not mean real progress in science. After a dozen years one would have expected to see more advances into new fields. Active science should be pushed on by surprise, by unpredicted problems and results. In Ponza, he said, nobody had mentioned eddies and fronts; but they had now become focal problems. On the other hand, most of the issues discussed in Ponza were no longer of interest. What can we learn from this discussion?

I think that we can learn from both the geologist and the physical oceanographer. They touched on two important aspects: forecasting and planning on the one hand, and unpredictable new approaches or results on the other hand. Planning is a necessity, even if it is sometimes easier to predict tsunamis in the Pacific, or storm surges in the North Sea, than to forecast science and technology developments. But both types of forecasting can be handled successfully, at least in part. The latest triumph in a very complicated scenario – the correct prediction of El Niño – encourages us to look ahead.

Every scientist looks ahead, selecting possibilities for future work, problems to be solved, methods to use, regions to visit, colleagues to work with, and funding agencies to contact. The scientist must plan for the near future, and sometimes for his or her whole career. Of course, he cannot plan the creative ideas he is hoping to have in the future. But he may be able to select conditions that favour his chances to get such ideas. Nevertheless, an active scientist normally hates the term "research planning", seeing it as restricting his freedom to do what he likes. But deep down, he knows that his success depends upon the exchange of ideas, on co-operation within his own discipline and with other disciplines, and last but not least, on adequate instrumentation and funding.

FORECASTING WITH NEW CONCEPTS

Planning can be successful if you can follow a strategy or a new concept. According to "Ocean Science for the Year 2000": *"…even a very fruitful idea today by a prominent man of science* (only man, you see, the text was written 17 years ago) *may be insufficient to generate a predictable process of scientific development in the next few decades. For such an idea to take root there should be present a ripe and fertile scientific climate."*

In the early 1960s the concept of 'Plate Tectonics' became ripe because a lot of relevant data had been collected during and after World War II. People simply had to ask, *"why are there mid-ocean ridges with so many earthquakes?"* and other, similar questions. **To sum up: first of all, critical gaps in our knowledge have to be assessed. This is the prerequisite for planning.** Today, the 'Global Ocean Conveyor Belt' could well act as a similar driving concept for the future.

A second condition is to look at present and foreseeable developments in instrumentation. In the 'Plate Tectonics' case, the drill-ship was already underway – and as marine scientists we should never forget how much we depend on engineers and ships' crews. Perhaps most of the major advances in ocean sciences in recent decades have been associated with the successful development of new types of instrumentation, as well as means and methods of observation.

A third condition for the success of a new, important initiative in marine research, is the response of a specialized

field to demands from other disciplines, from economics, or from society. For example, a new branch of oceanography – 'Palaeoceanography' – developed out of the results of the Deep Sea Drilling Project. Physical oceanographers should be encouraged to participate in this new branch of marine science, even though physicists may not be comfortable with proxy variables like 20°C +/- 1°C for palaeo-temperature, or 35°/oo +/- 1-2°/oo for palaeo-salinity, preferring exact figures to feed their models.

Co-operation between different branches of science is becoming increasingly urgent to solve our major problems. This seems to me to be the most difficult, indeed almost overwhelming, challenge facing oceanography for the next decade. Everyone talks about co-operation – but who really does it?

To take one example, planktonic organisms are responsible for more than half of the biological production of our planet, and they influence our global climate. What an important issue! With the help of parallel computers, we should be able to tackle the mathematical simulation of the complex planktonic ecosystem, and so learn a great deal about both biology and climate.

Here I would like to stress an additional, fourth, condition for success, which has become increasingly important in recent decades, namely the relationship between marine science and society. In UN Resolution 2172/XXI of 1966, only the "*need for greater knowledge of the ocean and its resources and the acquisition of this knowledge through international co-operation*" was mentioned. In later years more detailed tasks were discussed, but only one of them included "*the peaceful use of the ocean*" (unpublished IOC Secretariat recommendations, February 1969). The Ponza Report (SCOR, 1969) briefly treated some of the social issues involving marine science. But the Villefranche Report (IOC, 1984) paid much more attention to this relationship, with full chapters on *"Marine Science and Society"* and *"Ocean Uses and Research"* for example. To highlight the point, the report states: *"There are significant human problems in all countries that can be mitigated to some extent with the results of marine research. The ocean is already a source of extractable resources, living and non-living, and additional resources remain to be developed. The ocean provides a medium for shipping, communication and national defense. It modulates the climate, receives the waste of domestic, agricultural and industrial activity, and welcomes those seeking recreation."*

With our growing world population and the exploding costs of many branches of science and technology, social issues are receiving more and more attention – not least the question of what to do about society's increasing, and in some cases dangerous, waste products. Excluding the fact that we oceanographers generally have an emotional relationship with the sea, we have to recognize that the land on which people live covers only one third of the globe, with the rest being covered by the sea; the pressure to use the oceans to solve human problems will increase with time.

FORECASTING BY EXTRAPOLATION

Unfortunately, the example of the global tectonics concept was an exception, where all possible positive factors worked together. Usually, forecasting science and technology can only be based on extrapolation from existing trends. Some trends, of course, must continue because they reflect important ongoing challenges, like mapping the seafloor, and monitoring ocean currents, productivity, or pollution near estuaries. Even so, there is a need to identify some key topics for future investigation, as was done in 1969 and 1982. For instance, one might expect to see increasing emphasis in the future on certain areas, like biodiversity, to answer fundamental questions such as: *"how stable are ecosystems and how fast can they respond to environmental change?"*

Even more important is the use of continuously developed new instrumentation and methods to increase reliability and accuracy, or to reduce costs. Since the Villefranche meeting we have seen breathtaking progress thanks to the use of satellites as platforms for remote sensing of the ocean surface. In Ponza, there were only short discussions on this topic. However, less than a decade later, in summer 1978, the first oceanographic surveillance satellite, SEASAT, began to provide data about wave heights, wind direction, currents, water temperature, icebergs and coastal water characteristics. Today, satellites hold fascinating promise for all branches of oceanography. We are even able to use them to penetrate kilometres of water to map seabed morphology. In the future it might be possible to characterize bottom or intermediate water masses by use of gravitational data. Now, it is no longer measurement that is such a problem, but how to store and digest the almost overwhelming, permanent flow of data.

The solution to many problems calls for long periods of observation, and thus planning, for instance the investigation of the transport of heat and salt in the ocean, or its storage of CO_2.

FAILURES IN FORECASTING

Most of the proposals in physical oceanography made in Ponza became uninteresting to marine scientists after a while. In addition, some important developments were not forseen. In the earth sciences, the historical evolution of continental margins was neglected. The investigation of oceanic palaeoenvironments was also inadequately treated, although this was corrected at the Villefranche meeting. But even then nobody could imagine that so many new methods would arise, allowing a time resolution for studies of climatic variability of less than a few decades, even down to annual resolution in sediment cores of some areas. The new time resolution opens up exciting new insights into climatic variations, especially during the last 120,000 years or so, and at present, most importantly, even during the past 10,000 years. What are the indications for the growing influence of human activities? To better understand these changes between cold and warm phases of different amplitudes, and especially how rapidly they may have occurred sometimes, gives us a chance to examine the prospects for future climate change. What a change of approaches, and what a wealth of new results during the past few years!

Some of the activities proposed through the Ponza initiative were planned, but scientists lost interest in them for economic or political reasons. For example, up to the 1970s, broad areas of the deep-sea floor were defined where ore-grade manganese nodules occur. Then the price of copper or nickel dropped substantially, and the strategic importance of cobalt was reduced. As a result, Ponza recommendations on manganese nodules lost their importance, and many maps and chemical analyses lie stored in archives awaiting the attention of historians or the arrival of more economically attractive conditions. Similar predictions about the potential for exploitation of near-shore mineral and sand deposits or offshore phosphorites were equally too optimistic.

Another failure, probably because it was politically naive, was the Ponza proposal to establish a global network of river-mouth monitoring, where it was hoped to include both developed and developing countries.

IMPOSSIBLE FORECASTING

So far, I have been making some remarks about forecasting with continuation and extrapolation of existing trends, and about some failures. But the real breakthroughs in science and technology are unexpected and can never be predicted or planned. This is the real dilemma for science managers responsible for funding. Recognizing this, the Villefranche Report stated *"The vitality and creativity of our science will be demonstrated in the years ahead by the degree to which we have erred"* (IOC, 1984). What can we do to increase our errors, hoping that we will be surprised by many new discoveries and their consequences?

In 1977 the first deep-sea vent, where hot water enters deep ocean water, was discovered at the Galapagos Ridge. Around it was a new fauna of mussels, worms, and other organisms living by feeding on bacteria that derive their sustenance from chemosynthesis in the dark abyss. At Villefranche, in 1982, we discussed this remarkable discovery and similar ones made in following years. But we had only vague ideas about the global importance of these vents for the chemical budget of the oceans, or for the overwhelming role of bacteria in general, which now seem to form a "Deep Biosphere" extending hundreds of metres beneath the seabed. We did not know about the association of many hot vents in the oceans with massive metalliferous sulphide precipitates containing more than 3% of copper and high concentrations of zinc, silver and gold that would be economically very interesting if found on land. The so-called cold seeps, associated with similar astonishing ecosystems, were completely unknown to the workshop in Villefranche.

I am convinced that during the next decade the ice-like “clathrates” (methane hydrates), recovered first in 1981 off Guatemala, will bring us similar surprises even on a global scale.

THE ROLE OF THE SCIENTIFIC CLIMATE

What can we do to respond properly to new discoveries and their often-surprising consequences? We have to try everything to help to create a "ripe and fertile scientific climate" extending all over the globe and through all parts of society. This is of course a trivial and rather vague statement, of the kind that can be dissected and found useless. The Norwegian saying that *"necessities that everyone agrees about are never taken seriously"* is certainly very true.

To create such a climate we must begin by encouraging scientific and technological curiosity in both students and the general public. To improve public awareness of science means continual education. This should be one of our primary responsibilities as scientists. Curiosity and excitement, free of the profit motive, should be the driving forces for basic research, which in turn forms the basis for possible application. Of course, application of research interacts with and results in feedbacks to fundamental science. On research cruises one can enjoy this fertile scientific climate and how it often gets warmer after the first results arrive, or after hot discussions of different interpretations.

A scientific climate extending across the globe? During the Joint Oceanographic Assembly in Halifax in 1982, I had the opportunity to present the results of the Villefranche meeting. I stated: *"In all similar oceanographic documents written during the last decades, we find paragraphs dealing with the partnership between industrialized and developing countries. Sometimes these chapters are* *only found in an appendix, or were read as merely 'duty exercise'. In some countries this partnership was improved or established, not by abstract declarations but by common activities, even when they had begun in a very modest way".* I would now add that one of the obvious places for building such a partnership is in coastal areas, with their manifold problems both in science and in the management of industry, biological resources, and tourism. Many developing countries are situated in the tropics, with unique scientific features like coral reefs, mangroves, or tropical storms. What a lot of opportunities there are to cooperate! In Halifax, I continued: *"In some other countries, and with some scientists, too, we could not overcome barriers of different kinds. This is a very deplorable fact. Let us hope that the next generation of politicians, economists and scientists will be more successful in proving, with more actions, that the ocean is the common heritage of mankind and science is the common heritage of mankind, too."*

Back to the creative scientists we critically depend upon. Of course he or she is intelligent enough to see that planning is necessary. But we should also have in mind the French saying "C'est seulement un plan!" – in other words be flexible enough to change when new exciting findings demand it. In order to discuss and define these changes we have to tolerate very special characters. Often for outsiders, and sometimes even for insiders, passionate scientists seem to behave like fools. This reminds me of a saying by some Welsh sailors: *"Three things are untameable: fools, women and the salty seas."* Three cheers to this salty sea, with all its chances, dangers and problems we have to solve!

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