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"A Sea of Change: JGOFS Accomplishments and the Future of Ocean Biogeochemistry"

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When I was asked to give the last talk at this meeting I began thinking about the kinds of talks that you've heard at this symposium − some of the remarkable achievements of the program and its rich history and scientific legacy. But then the organizing committee told me that what they had in mind was an "epilogue". For those of you who aren't familiar with the theatrical definition of epilogue: the epilogue is a speech given by one of the actors at the end of the play that generally has some lesson or explication of the play − generally something that the playwright thinks you might not have "gotten".

The request for the epilogue came while I was working with our advisory committee on environmental research and education on the development of a ten year plan for interdisciplinary environmental research to be funded by NSF. In this context I began to see a different kind of legacy that you have left − a different kind of lesson learned from your work.

I think that you have changed the culture of science.

There have been large programs before and you've heard ideas about the new large programs that are being planned. But this program has been very special in forging a new approach for large programs and for science.

The principal complaint from those who believe that the work of large groups is not money well spent is that it doesn't lead to the breakthroughs that are led by single investigators and small groups. Basically that it is pedestrian science at a large scale. JGOFS is the best example I know of the foolishness of this view. You changed the culture of the big science experiment and, in the process, changed the culture of science.

First and foremost, JGOFS had a bold vision − you sought to answer one of the biggest questions facing us.

JGOFS has two primary goals:

> 2. To develop a capability to predict on a global scale the response of oceanic biogeochemical processes to anthropogenic perturbations, in particular those related to climate change.

This was an amazingly ambitious goal for the mid−1980s. In order to achieve it we had to accomplish the first goal

> 1. To determine and understand on a global scale the processes controlling the time−varying fluxes of carbon and associated biogenic elements in the ocean and to evaluate the related exchanges with the atmosphere, sea floor and continental boundaries;

JGOFS said that we would understand the processes in the major biogeochemical provinces and the level of basic equations, put them together with a few spatial and temporal surveys of major components of the system and serve up predictions of the response of the ocean to anthropogenic forcing − and that we would do it in a decade. Vaulting ambition.

I think the key to the success of this vaulting ambition was that it took on a clearly focused challenge, but an extraordinarily ambitious one.

The process studies that were done had all the elements of good science − they were hypothesis driven − with multiple working hypotheses.

Together with WOCE, these were the first big programs in which experiment and observation were intimately intertwined with modeling. While those of us with gray hair at this meeting still make jokes about modelers if we are observational scientists, or about data generators if we are modelers, these jokes are on the verge of being lost on the present generation of students. They use models routinely. Experiments in silico are as much a part of their culture as experiments in tube, tanks, or the ocean, both for large scale processes on which we are incapable of experimenting and for small scale processes. The models are the algorithmic embodiment of hypothesis, and they are now part of our culture rather than the domain of a few people with access to enough computational power to do them.

This points to one of the major factors in the evolution of this new culture − the revolutionary capabilities of computing, networking and data architecture. In our case, the culture change has co−evolved with these capabilities, and JGOFS has embraced − and many times led − that co−evolution. I'll come back to this again.

Remember OMNET?

One of the primary reasons that this program was able to be interdisciplinary and international from the beginning was that a small company focused on the ocean science community and provided a painless way of electronic communication to us all. Before MOSAIC, MOZILLA and NETSCAPE, at a time when my colleagues in the university were hard pressed to communicate with anyone via the computer, I had an address book of a thousand oceanographers with whom I could be in touch in a moment. The first JGOFS Steering Committee could communicate effortlessly, exchanging drafts of planning materials, discussions of issues and planning activities for cruises.

When we did go to sea we could continue to exchange information, satellite imagery, advice and data while at sea.

A related legacy was that, together with WOCE, the program saw the need for large scale data management and the development of new data architecture from the beginning. Glenn Flierl, Peter Cornillon, and a group of their colleagues developed new tools for us to use a wide range of oceanographic data easily and seamlessly.

These innovations foreshadowed the call for cyberinfrastructure or e−science that is now a rallying cry for all of science. This program developed its own cyberinfrastructure and in doing so provided tools to all of ocean science and to many other areas of science. The basic data architecture developed by DODS is now the paradigm for most of ocean science, for most of atmosphere and earth science, for the vanguard of ecological science, and has spread into math, physics, engineering and behavioral science because it is the underlying engine for the National Science Foundation Digital Library initiative ...
You did that.

This communication and data revolution also led to a democritization of our science. No one had to be at one of the five or six largest oceanographic institutions to participate. No one had to be the student of one of the dozen or so leaders who met in Woods Hole in 1984 to plan this future. You could be a PI or a student anywhere.

An extraordinary achievement of the program and change in the culture of our science was the interdisciplinarity of the program. Many programs in IGY and IDOE (International Decade of Ocean Exploration) were multidisciplinary. Lots of expertise thrown at problems. But none of those efforts achieve interdisciplinarity. In JGOFS ...
Physical oceanography
Remote sensing
Air sea exchange
Ocean chemistry
Ocean biology
Ocean particulate reactions and dynamics
Sedimentation
Sediment chemistry
Paleoceanography
and modeling,

to name just the obvious fields, were actually brought together in the planning of the program, and were executed together.

I remember the first PI meeting for EqPac in the US and the need to negotiate the sampling strategy to adapt to nearly 70 PIs. More so than any program I know, this program achieved interdisciplinarity.

It foreshadowed modern calls for integrated approaches to the study of complex non−linear environmental systems.

Another innovation was time series observation. JGOFS took the extraordinary step of establishing HOT and BATS, and the even more extradordinary step of continuing them after 10 years. This exploration of processes in the time domain rather than the spatial domain has resulted in some amazing insights − the complementary increase in CO_2 in surface water and the atmosphere, the role of nitrogen fixers in nitrogen cycling in oligotrophic waters.

You studied the ocean at the appropriate spatial scales for the nature of the biogeochemical provinces, but also at the appropriate temporal scales for the dynamic evolving processes. Now everyone wants to do this.

Finally, you took the time for synthesis. The last phase of synthesis and modeling for JGOFS was another extraordinary innovation. I can think of no other program of this scale that took the time for synthesis across disciplines that you have taken. This too has led to a change in the thinking of many large programs. It has also led to a call for more explicit investment in the time to think about results and to combine them with the results of others. We are beginning to see the call for new infrastructure tools − centers and collaboratories − to invest in synthesis as you have done.

So here we stand, having not only made remarkable scientific discoveries, having not only made remarkable progress in being able to predict the effect of anthropogenic change on the oceans, but having changed the way that science is done.

Congratulations to all of you, to all of us.