Oceanic Projects and Programmes

IGBP and big observational campaigns

B. Huebert

How successful have IGBP projects been at bringing together international groups to do observation programmes that no one nation could manage? International collaboration on big programmes certainly helps bring more people into the enterprise and has unequivocal benefits for scientists in countries that lack the resources to create their own Science Plan, but are the overarching goals achieved? Certainly the development of stationary facilities (Mace Head, Large-scale Biosphere Atmosphere Experiment in Amazonia towers, Cape Verde – see following article by D. Wallace – and other research sites) has facilitated observations by international groups.

Both the IGAC science plan addressing global atmospheric chemistry, and the SOLAS science plan examining the ocean surface and the lower atmosphere, have talked about testing the CLAW Hypothesis (Figure 1). This hypothesis proposes a feedback loop that operates between ocean ecosystems and the Earth's climate, connecting marine biota to DMS fluxes, aerosols and cloud properties. Yet in almost two decades of experiments, we have never managed to get coordinated studies of each of the critical parts funded. In IGAC programmes, the marine biological work always got cut off, leaving just (very good) studies of atmospheric sulphur chemistry. The cross-disciplinary linkages rarely appeal to discipline-oriented reviewers and programme managers.

Projects from both the IGBP and the World Climate Research Programme (WCRP) make plans for observations by groups from many countries. Unfortunately, one can never be sure in advance which of these groups will be funded to participate, so implementation plans can't include strategies that depend on having any one group or platform. Any programme officer in any country, whether well-informed or not, can decline a grant for critical participating groups or platforms. The experiment planners are at the mercy of reviewers, panelists, and programme managers, many of whom have, by choice, not worked in this field recently. Since many agencies conduct their assessments in secrecy from those planners, some judgments will inevitably be poorly informed.

In the US, the National Science Foundation is most likely to support biogeochemical research. Most programme managers take the view that only peer-review can determine funding, to the exclusion of unified planning. Even though an experiment goes through extensive reviews of its science plan to be awarded aircraft or ship time, this is no guarantee that any of its participants will be funded. This makes it difficult to assure that even the most critical observations can be made.

The VOCALS programme (Ocean-Cloud-Atmosphere-Land Study, under the aegis of the Variability of the American Monsoon Systems) is an excellent example. This is a study in the South East Pacific of linkages between ocean heat budgets and mixing, upwelled nutrients, marine biota, aerosols, clouds, and radiative transfer. It could have been the ideal IGBP/WCRP collaborative programme (initiated by CLIVAR – Climate Variability and Predictability), in which all the related programmes (the WCRP working group on surface fluxes, and the IGBP projects: SOLAS studying the surface ocean-lower atmosphere interface, IGAC addressing global atmospheric chemistry, and IMBER that investigates marine biogeochemistry and ecosystems, etc.) could find issues their community could address. So that is what was written into the Science Plan and Implementation Strategy: we will all go to the South East Pacific together, testing portions of the CLAW Hypothesis, studying the factors that control air-sea exchange, and connecting biogenic gas emissions to the particles on which cloud droplets condense.

SOLAS was actively involved in organizing VOCALS: participating in planning meetings; writing plans, brochures, presentations, and platform requests; organizing informational meetings; and pitching participation in VOCALS during

every talk possible. There was real enthusiasm as planners and their constituencies thought of participating in a grander enterprise than any one could do alone. Ancillary groups also took this bait, no doubt in part because the scientific justification had already been written. Anything they do would be enhanced by proximity to the larger VOCALS programme.

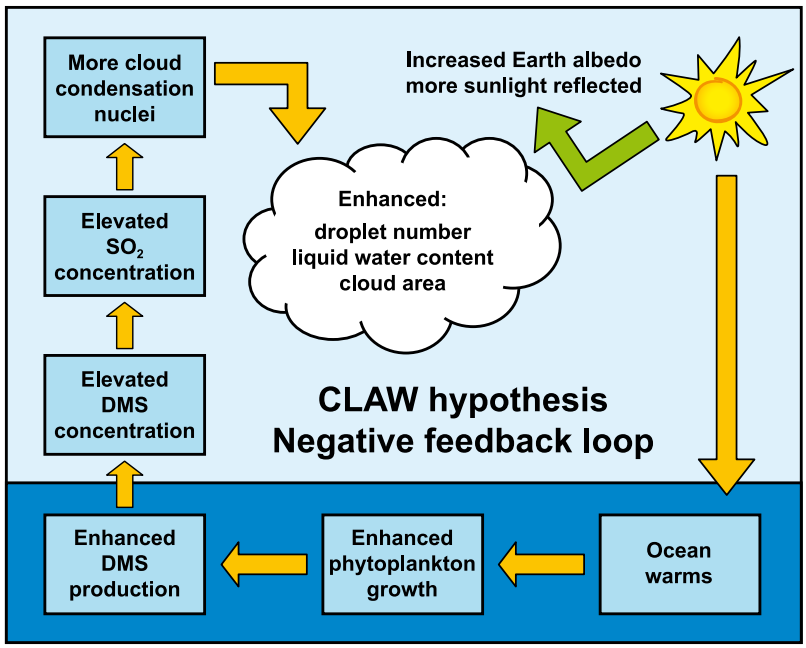
In a big programme, each platform has its niche: the oceanographic questions of nutrient upwelling, mixing, eddies, and heat transport in VOCALS would be best studied using a continuously-moving ship, doing butterfly patterns and pulling a SeaSoar that could profile throughout the mixed layer. Atmospheric measurements, cloud radars, and gas fluxes would be best measured from a (nearly) stationary ship, pointed into the wind so that flow distortions are minimized. Airmass mixing, *in situ* particles, photochemical budgets, and entrainment of free-tropospheric air would be best studied from long-range aircraft. The implementation strategy described the role of each, but had to be worded so that the loss of any one group or platform would not jeopardize funding for the folks who *were* able to get resources to do their part. Everything and everyone had to appear expendable.

Happily, NOAA made the *R/V Ronald H. Brown* available for VOCALS. However, a second ship could not be funded, so the Brown had to split its time between the surveying and stationary strategies. The good news is that one set of biological productivity observations was made from the *Ronald H. Brown*, alongside eddy correlation DMS flux measurements: some SOLAS-inspired biology and flux measurements survived the funding process. Unfortunately, neither oxidants, NO, nor OH was measured, so the programme could not observationally be able to constrain the photochemical link between the measured DMS fluxes and the growth of aerosols that control the clouds. Furthermore, the cloud-oriented C-130 flight profiles could not support budget studies of sulphur gases and aerosols, so the SOLAS observations and the aerosol/cloud studies evolved into *coincident-but-independent experiments*.

VOCALS Rex went into the field in the fall of 2008 as planned. Some very successful SOLAS-inspired experiments were conducted. Many valuable insights are emerging, especially with regard to the relationships between ocean dynamics, biology, and dimethyl sulphide chemistry on sub-kilometer scales. CLAW will have to wait again. The IGBP-type interdisciplinary objectives that got many of us excited about VOCALS could not be achieved. Again.

This same problem plagues many IGBP programmes: without some agency that is willing to agree in advance to fund a coherent set of observations and platforms, certain essential measurements will inevitably come up against an oppositional reviewer or other obstacle, and not be funded. Many valuable studies will be done, but the discipline-connecting goals that motivate these large programmes often are not. This problem may be too much for the International Group of Funding Agencies for Global Environmental Change (IGFA) to resolve. We have the scientific interest and the observational tools to test the CLAW hypothesis, for instance, but we still lack the institutional ones.

This is a challenge IGBP must address if we are ever to test our most significant conceptual models against observations on the necessary scales.



A schematic diagram of the CLAW hypothesis of Robert Charlson, James Lovelock, Meinrat Andreae and Stephen Warren (1986), *Nature*, 32:655-66
(Reproduced with permission)

Barry Huebert
Former SOLAS SSC Member
Department of Oceanography
University of Hawaii
Honolulu, HI 96822 USA
huebert@hawaii.edu