

Plankton Production Biology

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LONG-TERM GOALS

I continue to investigate geographic and seasonal distributions of hydrographic, chemical, and planktonological phenomena in the northern, central, and eastern Arabian Sea, including the continental shelf. Occasionally I comment on general oceanographic or ecological principles. The arranging for publication of translations of monographs by Russian-language authors continues.

OBJECTIVES

1. To comment on the seeming controversy in ecology of “bottom-up” *versus* “top down” regulation of marine and terrestrial communities.
2. To study the ventilation of the upper thermocline in the northernmost Arabian Sea by winter convection.
3. To open windows to the Russian oceanographic literature by arranging for publication of translations of monographs by Russian-language authors.

WORK COMPLETED

1. Following attendance at the workshop mentioned under (2), my arm was twisted to offer a public lecture in the framework of celebrating the 40th anniversary of the foundation of India’s National Institute of Oceanography (NIO) in Goa. I chose to address “Why is much of land green, but most of the open sea is blue?” For a paper published in 2007 (see under Publications, below), the title became “Do we live in a largely top-down regulated world?” The answer is affirmative and is contrary to what most students of phytoplankton and terrestrial plants believe.
2. An AGU Monograph will be the visible outcome of an international workshop on “Sustained Indian Ocean Biogeochemistry and Ecological Research” (SIBER) held at NIO in Goa, 2–6 October 2006. The tentative title is “Biogeochemical and Ecological Research in the Indian Ocean: Progress, Challenges, and Prospects”, and the editors will be J. Wiggert with K.H. Brink, R.R. Hood, S.W.A. Naqvi, and S.L Smith. My contribution (with J.R. Postel), just submitted for review, is entitled, “Winter-Time Convection and Ventilation of the Upper Pycnocline in the Northernmost Arabian Sea”, which describes an analogy to the winter convection and submergence at the subtropical convergences of the normal oceans.

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3. *Work in Progress*): A major issue for publishing books with a small market is to find the subsidies/funds for printing. This hurdle appears to be surmounted for two monographs, which have been translated but not edited, by an active interest of the director of NIO in printing oceanographic works (see under Related Projects. [4]).

RESULTS

1. *Do we live in a largely top-down regulated world or “why is the world green”?*

My paper (Banse, 2007) is based on a review of mostly recent literature, and the undercurrent is my continuing irritation with all too many phytoplankton workers and also terrestrial plant ecologists, who aim at explaining distributions by investigating only the resource-controlled growth rates. Clearly, under sufficiently benign environmental conditions, the terrestrial stage on which the organisms act is mostly green, since the resources (light, water, nutrients) drive primary production (“bottom up”). Resources, i.e., food, similarly drive animal production. However, the numbers or biomass and the types or species of the actors on the stage are largely “top-down” regulated by grazing, which holds likewise for the animals (by predation). Large-scale experiments have now shown that grazers (and often their predators) regulate or determine which terrestrial plant species dominate. Of course, the land remains green in any case. The open sea is nutrient-poor, hence, the bottom-up processes support little biomass, so that the sea is blue but again, the species of phytoplankton and zooplankton are heavily regulated top-down. Comparative studies show that this rule holds also for coral reefs but here, combined with fishing, it may lead to such a reduction of grazing fishes that overgrowth by leafy algae and the resulting shading of the light-dependent corals may lead to destruction of the physical reef structure. The paper briefly treats ecosystems altered by man and states the urgent needs for marine conservation.

2. *Winter-time convection in the northernmost Arabian Sea and the NASHSW*

As partly described in earlier reports, the dry and cold northwest (winter) monsoon blowing over water already of high salinity leads to convection and subduction, forming a salinity maximum with a density of about 25 kg m^{-3} in the northernmost Arabian Sea. (The northern border of the Arabian Sea is between 24 and 25°N .) This salinity maximum in the uppermost permanent pycnocline, first mentioned by Banse in 1968 (*Deep-Sea Res.* 15: 45-79), is now called the Northern Arabian Sea High Salinity Water (NASHSW). Later in the year its core is observed in the north between 50 and 75 m , but farther south between 75 and 125 m depth. It is distinct from the water mass centered around a density of 24 kg m^{-3} that is widely spread in the central Arabian Sea. The NASHSW is relatively well aerated and ventilates the upper pycnocline to the isopycnal of about 25.5 kg m^{-3} , somewhat above the intrusion from the Persian Gulf. The O_2 content between 21 - 22 and 24 - 25°C is higher by 1 - 2 mL L^{-1} than in the open sea to the south. The NASHSW can be traced to 20°N , sometimes even to 18 - 19°N , but owing to the small area of formation, the volume generated and the extent of geographic spreading cannot be large.

IMPACTS

1. By stressing top-down regulation of species composition on land and in the sea, hopefully another lance is not broken in the struggle for a more balanced view of food webs. Also, the call for marine conservation by a plankton worker may come from an unexpected quarter, but is urgent nonetheless.

2. The description of subduction of cooled surface water to the top of the pycnocline in the northernmost Arabian Sea brings this mini-ocean into line with the “normal” open sea with its subtropical convergences.

RELATED PROJECTS

1. Continued interest in the annual upwelling along the Indian southwest coast

Supported by NIO during the stay in India in October 2006 (see [2] under Work Completed), I also visited Kochi (Cochin) with NIO’s Regional Centre and the headquarters of the Union Government’s Central Marine Fisheries Research Institute (CMFRI). Besides much consulting with/advising of junior scientists, I lectured on the temporal stability of the Oxygen Minimum Zone of the open Arabian Sea (OMZ; see Report to ONR for FY 2006) and the history (since 1935) of the knowledge about the upwelling during the SW (summer) monsoon across 10-12 degrees of latitude along the Indian (south-) west coast. The hidden agenda of the visit was to find out more about hydrographic data collected by CMFRI during the last three decades, which in India is a touchy subject. It turned out, however, that the various hydrographic data were retained by the investigators and with their retirement were lost for good.

2. Nutrient budget for the Bay of Bengal

Compared with the Arabian Sea, the phytoplankton (and presumably, animal) production in the bay appears to be low. The principal reason is seen to be the low plant nutrients in the surface layer, due to the limited supply from below owing to the salinity stratification caused by the great excess of river runoff over evaporation (e.g., K.R. Muraleedharan et al., 2007, Prog. Oceanogr. 2: 364-383). In fact, the nutrients are also low in the halocline, the immediate nutrient source. The role of river supply is left open because of dearth of analyses.

I have proposed to Dr. P.N. Vinayachandran, Centre for Atmospheric and Oceanic Sciences, Indian Institute of Science in Bangalore, who has modeled the Arabian Sea and the Bay of Bengal, that a box model of the entire bay regarded as an estuary be considered. The basic idea is that a considerable flux of oceanic deep water is required to maintain the salt balance and, hence, the salinity of the surface layers in the face of runoff plus precipitation minus evaporation. From the simple laboratory mixing rules in quantitative chemistry, I estimate that each volume of fresh water by the time it has reached the middle of the bay (at roughly 32 psu) has been “diluted” by 10-12 volumes of oceanic deep water with, in the bay, very high nutrient concentrations. I am certain that most of the nutrients thus entering the upper layers are not derived from the rivers but from the ocean. (The next task would be, of course, to quantitatively address the ensuing level of phytoplankton production.) After Dr. Vinayachandran had asked for some clarification in July, I have not heard from him.

3. India’s National Institute of Oceanography (NIO)

I continue to stay in contact with NIO and occasionally advise the director, Dr. S.H. Shetye.

4. Translations of Russian-language monographs

At the first planning workshop for the U.S. JGOFS Arabian Sea Process Study (scheduled for 1994-1995) in February 1991 it again became painfully clear that the “western” scientists are largely un-

aware of the large number of Soviet and Ukrainian cruises after the International Indian Ocean expeditions (1959-1965) because of the language barrier. Between the late 1970s and 1990 alone, there have been at least 20 comprehensive (integrated) expeditions north of about 10°S, many of which focus on large station nets with 55 km spacing of observations (“polygons”); this is not counting the scores of fishery-related cruises that usually also determined temperature, salinity, and oxygen. “Re-invention of the wheel” by U.S. oceanographers, aside from us still largely working along sections only, appeared at least possible.

A U.S.-Ukrainian-Russian workshop was held in Sevastopol (Crimea, Ukraine) in September 1993 to co-ordinate future work, especially during the JGOFS period (Joint Global Oceanographic Flux Study), but very little tangible came of it. Personal contacts established at that occasion, however, led to plans of having Russian-language monographs, many of them treating individual expeditions, translated into English and printed as books. SCOR, and especially, ONR, supported the translating, redrafting of figures, etc., of four monographs, as well as of commissioning one book drafted in English from the start. To make the works accessible also to low-budget libraries and individuals, especially around the Indian Ocean, printing in India was chosen for the five books. Work began in 1997, and in early 1998, a meeting held in Bangalore at the Academy of Sciences by the Academy’s editors, representatives of their press (in Hyderabad), support by the promise of a subsidy from the government’s Department of Ocean Development (later not honored), and myself appeared to seal the deal. The four volumes were roughly translated almost ten years ago, and two of them and the manuscripts of the chapters for the new book were edited a few years later. After delays only in part caused by me, the latter monograph and the two books finally saw the light of the day in 2006, as stated in the FY 2006 report.

Owing to the numerous delays by the press in Hyderabad, the vanishing of the original subsidy, and doubts about finding support for printing, I had decided several years ago not to edit the translations of the two remaining books, although both translations were checked by a Russian-born oceanographer.

L.I. Sazhina (deceased) *Nauplii of dominant pelagic copepods in the World Ocean*. Russian original with 208 small-format pages including 100 plates with about 1400 detailed figures and 2 tables. 1985.

The book presents keys for the six naupliar stages of 85 species, is therefore a unique source, and will not be outdated. The quality of the figures printed on poor paper is not perfect, but adequate (the originals are lost). The publishers are not interested in the copyright issue.

T.S. Petipa (editor, deceased). *Mechanisms of aggregation formation and plankton functioning in ecosystems of the Indian Ocean*. Russian original with 233 normal-sized pages, including 53 tables (now retyped) and 134 figures (now most of them redrawn). The axes of the 199 maps in the appended *Atlas of Bio-Oceanographic Characteristics of the Indian Ocean at Water Mass Boundaries* have been re-labeled and some maps have been redrawn. Unpublished.

The work was intended for publication in 1993 but abandoned as page proofs because of the economic collapse of the Ukraine. The title is somewhat misleading; the book is essentially a thorough treatment of observations on several large grids of stations 55 km apart (“polygons”) occupied during 1980, 1983, and 1987 in north- and south-equatorial divergences, with hydrography and dynamic computations, phytoplankton cell counts, chlorophyll, photosynthesis, micro- and mesozooplankton, their energetics, particulate organic carbon, and includes some modeling. While some of the material might appear somewhat dated, especially so because the last 15 years, or so, of English-language literature are

not considered, the comprehensive treatment as a whole is almost unique. In addition, of course, the data are fixed points in time for a later study of oceanic change.

The new development is that the director of NIO has offered to have suitable oceanographic (in the wide sense) books printed at no cost to the author(s). I have no doubt about the suitability of Sazhina's book and would be surprised if Petipa's book did not pass muster. The printing quality is far better than that of the Hyderabad press. The only issue for me is which of my long-promised papers to set aside for the editing, i.e., the description of the physical data on upwelling along the southwest coast of India (mostly based on data of 1972-1975), the study of decadal change in the source of the upwelling water as an explanation for the seasonal anoxia now broadly present on the shelf (see FY2006 report, Related Projects), and my own time-series observations of 1958-1960 on two stations off Kochin/Cochin. The latter data were gathered monthly with mostly up-to-date methods and, until two or three years ago, were the only time series of hydrography, nutrients, phytoplankton, and some microzooplankton in the Arabian Sea. Because I managed to be at sea also during the upwelling period of the southwest monsoon, the series is unlikely to be replicated anytime soon, which is one of the reasons for me still not having written it up. The artwork is largely completed.

MEETINGS ATTENDED

1. *International workshop on "Sustained Indian Ocean Biogeochemistry and Ecological Research"*

(SIBER) held at India's National Institute of Oceanography (NIO) in Goa, 2–6 October 2006, the travel supported for the U.S. attendees by NSF. The idea was to consider biogeochemical measurements to be associated with the planned sustained CLIVAR - GOOS (Climate Variability and Predictability, and Global Ocean Observing System) buoys in the Indian Ocean. The high-caliber meeting reviewed current knowledge and outstanding questions but did not come up with a coherent plan for future work. I lectured on "Physical processes in the Oxygen Minimum Zone (OMZ) of the Arabian Sea," noting that unless we understand the physical processes at depth, we cannot make budgets for salt, O₂, or nutrients with useful accuracy.

2. *Annual meeting of the NASA Color Research Team*

The currently supported P.I.s are required to attend the annual meetings while others may participate. Much emphasis during these gatherings is on continuing the updating of calibration of instruments and algorithms, future missions, etc. The 2007 meeting was held from 11–13 April in Seattle. On the schedule was also a discussion of a draft plan for the next decade by NASA's Ocean Biology and Biogeochemistry Program, tentatively entitled, "Earth's Living Ocean: The Unseen World." I lectured on "CO₂ and organic carbon flux," but my pun about our ignorance of the key roles of processes in the "twilight zone" (ca 150-1000 m depth, well below the penetration of color satellites) was lost. Otherwise, I addressed once again the group's fixation with the "bottom-up" mechanism, the belief that underwater light, nutrients, temperature, and photosynthetic efficiency, applied to space-derived chlorophyll, is all that is to know for understanding organic production and, hence, CO₂ flux into and out of the sea, and to depth. I noted once again that the approach only calculates/estimates (the usually considerable) daily phytoplankton growth, but cannot say anything about the reason why next to no population growth is being observed after 24 hrs. most of the time in most of the open sea: Cell division is almost balanced by mortality from grazing that is not observable from space. Mortality is even *in situ* hard to estimate with any useful accuracy, because revisiting of the same population subject to advec-

tion and mixing is required. I am afraid that also this time, the message was lost because of the (false!) mindset of the audience.

3. Second Annual U.S. GLOBEC Pan-Regional Synthesis Workshop

From 25–28 September 2007, a GLOBEC (Global Ocean Ecosystem Dynamics) workshop was being held in Seattle. (GLOBEC was a ten-year field program under ICSU that was co-sponsored by the International Geosphere-Biosphere Programme, SCOR, and IOC. It was directed at understanding the effects of physical processes on marine ecosystems, as well as examining the potential impacts of global change. A five-year period of integrating and modeling began last year). The workshop focused on integrating and modeling data from the U.S. sub-programs in the Southern Ocean, the northeast Pacific, and the northeast coast of the U.S.A. (Georges Bank). I attended, especially because I wished to learn about the views on zooplankton mortality (nothing new emanated). Briefly, I spoke on “Bottom-up Control” *versus* “Top-down Regulation.”

PUBLICATION

Banse, K. (2007) Do we live in a largely top-down regulated world? *Journal of Bioscience* 32: 791-796 [refereed].