TUNA OCEANOGRAPHY AT SCRIPPS FIFTY YEARS AGO

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A gulf separates the culture of oceanography today from the world of Scripps in mid-20th century, so it may be interesting to record something of what it was like to be an oceanographer in those days.

Today, when hundreds apply for a handful of post-doc positions at Scripps, it must be very difficult to imagine a time when Scripps had to go looking for people to fill vacancies. Now, when almost as many overpriced journals are filled with under-edited, complex papers it must be difficult to imagine a time when we were satisfied with half-a-dozen well-edited journals; it must be difficult to imagine a time when Nature published papers with simple titles like "*Density of benthic communities off West Africa*" – the first paper I wrote in ocean science. Difficult also to imagine a time when one was confident that one knew – more or less personally – everyone else involved in one's field of research, and with whom one corresponded by letter, often hand-written.

I was fortunate enough to be recruited in 1963 to the Scripps Tuna Oceanography Research group (STOR), of the Institute of Marine Resources, and it is the work of that group that I want to recall briefly. But to explain how things worked in the ocean sciences in mid-20th century, I need to go a little further back and to the consequences of the general absence of undergraduate training in ocean science: in the UK, I think there were no formal courses in oceanography, or even in related practical fields like fishery science.

Instead, one graduated as a biologist, a chemist or a physicist and then learned about the ocean on the job; fortunately, for that, we had recourse to the 'Bible' – Sverdrup, Johnson and Fleming's remarkable text, which for sheer breadth of scholarship, has never been surpassed. Biologists had some advantage over the others because there were summer courses to be had at places like the Marine Biological Association in Plymouth, so at least we could learn something at first-hand about marine organisms.

My own case was about as bad as it could get in terms of education for a career in marine sciences: on being released from the armed forces in 1948, along with a deluge of others, I had a very hard time finding a university place of any kind, and had to settle for a bachelors degree in zoology (specialising in entomology} at a minor college of U. London, followed by a doctoral thesis on the ecology and taxonomy of a small group of living-fossil, fresh-water crustaceans. This wasn't a very good grounding for a career as an oceanographer, although it very well suited my interests as a naturalist.

But, once graduated, posts were easy to find in those days and I went directly from thesis work in London to a Colonial Office fisheries research institute in Sierra Leone, where I was asked to study the ecology of the benthos of the continental shelf from the mangroves out to the shelf edge: quite a challenge, but rewarding - although it didn't seem to me to have much to do with improving fishery yields for the local people, which was the mission of our laboratory.

It's hard to understand now how one could handle the data from such work without a laptop or spreadsheets: in writing the it all up, I had to deal with 370 species in 291 genera distributed among 274 grab stations, each species being allotted to a depth zone and a bottom type. That made for a very large table (22 printed pages!), yet I don't remember any serious problems in having my hand-drafted table processed by the typesetters of the eventual monograph! Nor, at that time, did we have access to a mechanical adding-machine or to personal portable typewriters; one wrote in longhand and the group secretary typed it up, so you had to get it right the first time! A handheld Curta rotary calculator and an Olivetti Lettera 22 entered my life later on.

Things started to come together for me some years later, when I found myself running a small federal fishery laboratory in Lagos, Nigeria, in Independence year (1960); here, with three Nigerian colleagues, I was able finally to get at fishery research proper, which had been my aim all along. To support the research on fish biology, I thought we should know something about their environment and – having scrounged some water bottles, reversing thermometers and plankton nets through US AID – I organised a monthly section to the shelf-edge and a daily beach temperature record. We hit the jackpot, and described localised upwelling of water from a deep countercurrent that runs westwards on the upper slope during the monsoon season, with big, red, cold-water *Calanoides* copepods appearing just off the beach. Forty years on, this sub-surface flow seems not to have been formally described and I recall a plaintive note in (was it?) "*Oceanography*" that asked why Argo floats should proceed westwards in the Guinea Current?

Perhaps because we had received help from USAID for oceanographic equipment purchase, we were visited one day, out of the blue, by none other than Wib Chapman from San Diego: Wib had previously been US Secretary of State for Fisheries but was now scouting on behalf of the San Diego Tuna-boat Owners Association. In this capacity he was helping to organise an oceanographic survey (EQUALANT) of the eastern tropical Atlantic and had thought to recruit us and our small research ship *Kiara* for one of the easternmost transequatorial sections. So, quite soon, I found myself unexpectedly doing "real" oceanography with new equipment and with a San Diego tuna skipper (Ted Sorenson) running *Kiara* for us because her usual skipper wasn't certified for deep-sea work; we didn't get much farther south than the equator (our radio failed!), where I was quite puzzled that we couldn't get our gear down without huge wire-angles and didn't understand the significance of the horizon-to-horizon parallel slicks on the glassy surface…pity, I might almost have scooped Townsend Cromwell if I'd been properly trained!

Later, Wib Capman again visited our laboratory in Lagos to discuss our results and, casually one day on the jetty, asked me what I was planning to do next – "You probably won't want to stay here all your life, will you?" he suggested. Then he told me that Benny Schaefer was looking for people to put together a tuna oceanography group at Scripps, and he wondered if I'd be interested? Pretty soon I had a contract as an Assistant Research Biologist in hand: it was as simple as that and was probably one tiny consequence of the interest of the State Department and the US Navy to occupy as much ocean-space as possible, as quickly as possible, to counter the moves in the UN to establish an extended-jurisdiction regime by several coastal states, led by the Latin Americans. Hence also the encouragement of Commerce in a wide-ranging tuna industry and in EQUALANT and, later on, EASTROPAC.

My contract in Lagos being pretty much at an end, anyway, I handed the laboratory over to Eddie Bayagbona (who left soon thereafter for a cushy job with FAO in Rome), packed up my data and, after a short vacation in England in December 1963, headed for San Diego with Françoise, who had been a translator at the French aid mission in Lagos.

Francoise had already spent time at Berkeley, but California was a cultural shock for me, having not previously visited any place remotely resembling La Jolla. Maurice Blackburn met us at the airport and installed us in a small apartment on the shore at Ocean Beach: it didn't exactly help our settling in that a sailor and his girl-friend were killed by a sniper not 100 yards from our door just a few days later…but, anyway, it wasn't long before we were installed in a rented house in Del Mar, right beside the railway.

STOR, at that time, was housed in old Quonset huts on Point Loma, awaiting a move to the new Fishery-Oceanography Center on the cliff above Scripps, a move which happened at the end of 1964 with the dedication of the buildings. Maurice Blackburn, of course, also took me around Scripps, showing me the new lab and some of the ships we were to use, especially HORIZON; the new BCF vessel JORDAN didn't arrive until a couple of years later, but then became our principal platform. The facilities of the main Scripps campus and the informal life there enchanted me from the start and remain my ideal for a scientific research establishment.

I soon learned that I was slated to be the zooplankton expert in a small group which at that time comprised Klaus Wytki (physical oceanography), Carl Lorenzen (phytoplankton), Bill Thomas (marine chemistry) and, of course, Maurice Blackburn for tuna ecology. This was a new departure for me because, apart from my modest efforts off Lagos, my only connection with the subject had been by sharing a big lab with Vernon Bainbridge, our planktologist in Sierra Leone.

I remember very little of our accommodation on Point Loma, except the conference room where we planned our work at sea, which was to be an investigation of the ecology of the extraordinary annual concentrations of yellowfin tuna and other big pelagic organisms off Cape San Lucas at the tip of Baja; that we would be working near the coast of Mexico may just perhaps have been a factor in the choice of this project. Parenthetically, I remember, a couple of years later being briefed by Benny Schaefer before a cruise off Baja, and being given our written operating instructions, in Spanish, to be shown to Mexican coastguards if challenged. Before he handed them to me, he said in his inimitable growl: “I'd better put a couple of dago-dazzlers on these” - and reached for a bunch of rubber stamps on his desk. Things were very different then!

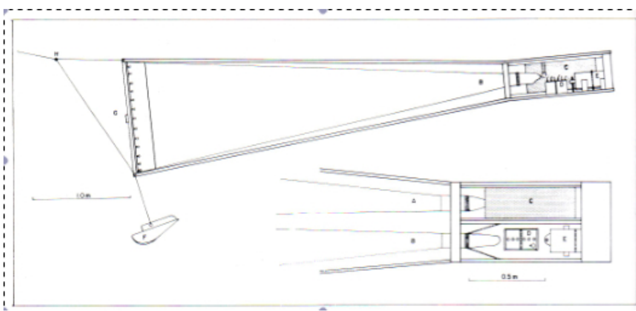
I also remember very clearly a discussion, probably in early 1964, in the Point Loma building, concerning our sampling protocol for the investigation of the environment of yellowfin off Baja. We were discussing how much time we would spend on station and Maurice asked me how long I would need for net sampling: I had no good answer and asked if anybody knew how oceanic zooplankton were arranged, depthwise. “I don't think we know” said Maurice, to which I replied that if that was the case, then it had better be my first priority to find out. I suspect now that if I had asked John McGowan, I'd have got a better answer!

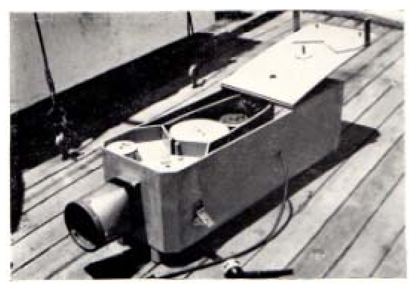
It was clear that we wouldn't have enough station time to use a flight of opening-closing nets routinely, yet I had a notion to try to get as many zooplankton profiles as possible, encouraged by Carl Lorenzen who had interested me in the dynamics of grazing on the observed chlorophyll profile; it was also Carl, incidentally, who made possible the fluorometric measurement of chlorophyll and opened the door to very detailed profiles of phytoplankton biomass and to underway continuous chlorophyll monitoring.

It didn't take me long to decide to attempt to build a zooplankton profiling device based on the Hardy Continuous Plankton Sampler principle of passing a strip of filtering gauze across a water tunnel in a towed sampler, an idea that Maurice encouraged. So, much of my first year at Scripps was taken up with designing such a tool, together with technicians Dougal Reith and Bob Bower. We planned to replace the cod-end of a zooplankton net with a continuous sampler and realised that this would involve very careful design to avoid plankton hanging-up on the net; a rather long, narrow net with a high net aperture/mouth area ratio was required.

Nor was it simple in those days to put electronics and electric motors on submersible equipment and I found myself in correspondence with manufacturers on such matters as the right oil with which to fill a submersible electric motor, and how to monitor temperature, depth, gauze advance in the sampler, and the revolutions of a flowmeter in the mouth of the net. In the end, all data were recorded sequentially as event marks on a Rustrak miniature mechanical recorder. The whole outfit was driven by 6 VDC mercury batteries in a pressure case.

After several sea trials of a proof-of-concept sampler and much subsequent redesign, we settled on what we considered to be the first operational sampler; this was built into a frame that accommodated the sampling net, the continuous sampler, all pressure cases and also an identical net equipped with a simple cod-end to obtain an integrated sample in order to check the sampling efficiency of the main device…





This machine – which I wanted to call a PTD for "plankton-temperature-depth" like STD – had by then come to be referred to an an LHPR (Longhurst-Hardy Plankton Recorder), a name which has stuck to today[1]: here is a modern LHPR, based on the design that we developed later at the NMFS Fishery-Oceanography Center at La Jolla -

[1] *I think that Paul Smith was responsible…*

- that was used by a Spanish group in 2010 to investigate plankton distribution in tidally-generated internal waves through the Strait of Gibraltar. The question they asked was: do the stronger-swimming species manage to maintain their optical depth or do they act as passive tracers?

Between these two versions, several others have been built and used (more or less) successfully: the first commercial versions were built by Benthos Inc. who supplied instruments to laboratories in Hamburg, Kiel, Edinburgh, Miami, College Station Texas, the Allen Hancock Foundation, Bergen, and the NMFS Seattle. These were all constructed in the very early days of underwater electronics and I fear many were abandoned as unreliable in operation.

But I'm getting ahead of myself. The first published data from the prototype equipment was obtained in 1965 in the upwelling region to the west of Baja where we obtained 13 detailed profiles (60 sampling depths) to 400 metres through the oxygen minimum layer (minimum around 0.20 ml/L) and found, to our surprise, dense and depth-restricted layers of *Calanus* (<400 ind/m^{-3}) by day within the oxygen-deficient layer. These, and other diel migrants, must undergo a daily variation in their oxygen environment of 0.2-5.0 ml/L – a very surprising finding that seemed to justify all the design effort of the previous couple of years.

While the development of the LHPR was in hand, I also participated in STOR Tuna Oceanography cruises TO64-1 and TO64-2 off Baja California: my role was to understand the ecology of the pelagic galatheid crab *Pleuroncodes planipes* that was thought by Maurice Blackburn and others to have a major role in aggregating yellowfin tuna in this area. The immediate question was its role in transferring the high density of diatoms in the green, upwelled water into tuna food. It was immediately obvious by observation (and by examination of its mouthparts) that it was actively filtering by an alternate left-right sweeping motion of its highly setose second maxillipeds while sinking in the green upwelled water, to tail-flip back to the surface again to repeat the process.

Carl Lorenzen, Bill Thomas and I also performed grazing experiments at sea to quantify potential ingestion rates of C-14 labelled diatom cultures and found clearing rates of 1-3 l/hr which – by comparison with rates obtained for herbivorous copepods and euphausids - showed clearly that *Pleuroncodes* was the dominant grazing organism in the upwelling plumes. It thus proves to be a tropical counterpart to the related lobster krill (*Munida gregaria*) of the Southern Ocean. Large concentrations of these crabs occur preferentially in upwelling plumes off Baja and were already known to attract and be fed on by several tuna species; later investigations by STOR showed that sparser populations farther offshore have their origin in larvae advected from pelagic reproduction which we detected in the upwelling regions: a return path to the coastal benthic population was postulated in the Pacific Intermediate Layer countercurrent.

Such was my own contribution to the work described in almost 100 papers produced from STOR research in the Baja region, as a result of which Maurice Blackburn was able to write in one of his terminal annual reports that the yellowfin tuna was now perhaps the best-understood large pelagic fish anywhere. And, I think, it is fair to say that the Baja offshore environment was one of the best understood dynamic regions anywhere.

The STOR group itself was a very good example of a research set-up that I believe is highly effective: the collaboration of a group of individuals having a wide range of interests and skills in physics, chemistry and biology in the solving of complex ecological problems. Much later in my career, I was fortunate enough to be involved in another such, the Biological-oceanography Division at Bedford Institute of Oceanography, led by Trevor Platt.

At STOR, we covered physical oceanography (Klaus Wyrtki), nutrient chemistry (Bill Thomas), phytoplankton ecology/physiology (Carl Lorenzen), zooplankton/nekton ecology (myself), and tuna ecology (Maurice Blackburn); our list of publications reminds me that this early group was expanded later on and that many people not formally within STOR participated in our work (e.g. Dick Eppley, Bob Holmes, Gunnar Roden, Chuck Jerde, and others). We undertook not only observational work at sea, but also experimental, and it was Bill and Carl's incubations of phytoplankton cultures, both *in situ* and shipboard, that gave critical support to the evolving understanding that – of the classical nutrients – nitrate is the most commonly limiting. Prior to those years, emphasis had been on phosphate. Klaus Wyrtki's heat budget studies also broke new ground.

The people with whom I personally interacted most closely at Scripps were the planktologists in MLR – especially John McGowan, Ed Brinton and Angeles Alvarino. With Lauren Haury I had some differences, because he built another LHPR but with a filtering net ill-adapted to purpose, and then stated in print that the LHPR concept was a crock; he must have changed his mind, however, because I was delighted to find (when preparing this text) a photo in the 1984 SIO Annual Report, of Lauren launching an LHPR aboard *New Horizon* that looked remarkably like one of my later versions of that instrument!

I was impressed by the fact that the MLR planktologists took the identity of their material very seriously and that each had contributed critical taxonomic and biogeographic research to our knowledge of the Pacific fauna. The taxonomic review of chaetognaths undertaken by Angeles Alvarino is a masterpiece and an essential underpinning of research on their ecology. I could not have interpreted my zooplankton profiles correctly had their papers not been at my elbow: how else to understood that a large species of copepod (*Eucalanus bungii*), routinely encountered deep in equatorial regions, was a North Pacific expatriate bound for a lonely destiny? The further south my profiles, the deeper and less dense was the layer of this species.

In 1965 and 1966 we also had to begin to plan our involvement in the EASTROPAC multi-ship surveys of the region from 20N-20S and from the coast out to 126E – a very large piece of ocean – that was scheduled to start in 1967. Apart from involvement with routine, standard observations I planned to get as many detailed plankton profiles to 500m, at either midday or midnight, as could be managed and to cover as much of the region as possible. This brought me into daily contact with Warren Wooster, who was coordinating the programme and who became a life-long friend. He taught me a great deal about the nut and bolts of oceanographic programmes at sea, and about the oceanography of the eastern Pacific.

In early 1967, the first EASTOPAC surveys were undertaken and the STOR group manned the new Bureau of Commercial Fisheries ship *David Starr Jordan* on sections running south from Baja down to 20°S; this was my first experience of working at sea on a modern, highly-seaworthy ship – and it was a revelation of what could be achieved. There were many glitches with a slightly improved version of the LHPR equipment used off Baja, all because we were trying to use early electronics to do new things underwater for which they were not designed. In the end, on four cruises, we got 83 good profiles from 500m, all in the western part of the EASTROPAC region.

These sufficed (somewhat later on) for the derivation of a first-order description of the constraints on the vertical distribution of zooplankton in permanently-stratified seas, which I took to the ideal case, from which others (such as regions having seasonal spring blooms) could be treated as derivative. It was clear that the pattern was simple and logical: "*A layer of abundant epiplankton contains a subsuface maximum that tends to coincide with the bottom of the mixed layer and with the depth of maximum carbon fixation by plant cells, but lies above the chlorophyll maximum. Zooplankton abundance declines sharply downwards across the pycnocline, forming a discontinuity between the epiplankton and the low-biomass plankton below*".[2] It was possible to describe the spatial distributions of all the main species and groups of zooplankton in response to light and seasonally-changing nutrient conditions; it was also, for me, the start of a research interest that would continue to the end of my career.

But, also in 1967, my life changed again unexpectedly (and radically). By then, together with Inter-American Tropical Tuna Commission, we were well established in the Fishery-Oceanography Center which was otherwise mainly inhabited by Bureau of Commercial Fisheries groups, directed by Ahlie Ahlstrom who reported to the BCF Regional Director in Long Beach, Gerry Howard.

One day I got a strange phone call from Gerry, suggesting we meet in the bar at the San Diego airport that very afternoon where, after a couple of beers, I found that I had a new job as Center Director in succession to Ahlie, who wanted to go back to his larval fish: any discussion of the daunting problems of recruiting a foreign national to the federal service came later. I know that Charlie Hill, who looked after administration for the new Center, was told not to worry too much about formal procedures, but to help me get what I needed in my new job without unnecessary fuss: he certainly insulated effectively me from such formal paper-work as existed in those days and let me get on with what I thought it was important for me to do. Nor did Ahlie ever once look over my shoulder, which he might easily have been forgiven for doing: he gave me a valuable lesson in how to leave a job gracefully that I needed much later on.

It didn't take me long to understand that the Center lacked the usual mandate for a fisheries laboratory: we had no direct responsibility for the management of any fishery, or even for providing quota advice to a management structure. So we could use our resources on basic fishery science, particularly in continuing the CalCOFI investigations. I can recall no pressure not to go on with basic research into the causes of fluctuations of California current sardine and anchovies, or on operational predictive techniques in the tuna fisheries. Both of these were sufficiently difficult and innovative (and being done sufficiently well) that I quickly realised that the new Center was going to attract attention internationally, and would become a respectable member of the Scripps family of laboratories. It goes without saying that it didn't take long to realise that the physical plant matched the research opportunities in an exceptional way. One of its delights of the Center were the clifftop balconies, and offices with windows looking out to sea: fortunately, air-handling plants had not yet become fashionable, or profitable to install.

Although I quickly became involved in all the issues of the day (porpoise mortality by tuna seines, the Santa Barbara oil spill, DDT and dying pelicans, CalCOFI and so on) I remember most warmly the discussions that led to John Hunter and Rueben Lasker's use of the seawater hall to spawn anchovies at will and to the experiments underlying Lasker's

[2] *Longhurst, (1976) Deep-Sea Research 23, 729-754*.

critical period hypothesis of larval fish survival, and thus experimental confirmation of Hjort’s match-mismatch mechanism.

Running through all this was the theme of EASTROPAC, for which I came to be the coordinator when Warren Wooster quit for reasons I can't now recall. The task of seeing that the data were properly archived and the Atlas office run properly was not fun and it was only Cuthbert Love's doggedness that got the Atlas finally into press despite all the expected and a host of unexpected problems (like the Latins didn’t appreciate that their volume was the last one to appear, a mistake we wouldn’t make today, I think).

However curious it may seem now, I was able to continue my personal research on plankton profiles while running a major research laboratory. I managed to keep the LHPR deployed on the remainder of EASTROPAC and began the work of analysing the results obtained. We also redesigned and built a new version of the LHPR during this period that was the forerunner of modern versions like the one illustrated above: design possibilities for underwater electronics were very rapidly evolving and the system finally became quite reliable.

But in my front office things were changing fast in ways I didn’t like, and which didn’t seem calculated to do our work any good. The most visible change was the incorporation of BCF within a new agency - NOAA. We were also at the beginning of modern planning and programming in Washington, and I remember reading with hilarity the federal manual on Robert MacNamara's PPBS (Programme Planning and Budgeting System) for the Department of Defence, but I should rather have realised that the days of amateur science administrators like myself were numbered. The weight and insensitivity of the paper-work that flowed from the federal embrace of PPBS was a major factor in my quitting in 1972, and taking a post in Plymouth.

However, (as I wrote elsewhere) it really does you no good to run for cover when the heavens are falling: I found the equally objectionable 'Rothschild Principle' awaiting me in Britain, and finally 'Sector Management' did for me as director-general of BIO in Canada: there, I had the option of going to Ottawa to organise it, or of going back to the bench in the Biological Oceanography Division. It didn't take me long to decide...and I was soon happily occupied working with the final version of the LHPR aboard HUDSON (“*the most capable oceanographic ship in the western world*”, according to Konstantin Federov about 1985) in the Canadian Arctic, in the North Atlantic and back into the eastern tropical Pacific for the BIOSTAT experiment: a multi-disciplinary comparison of vertical processes on the dynamic Costa Rica Dome and in a stable thermocline area.

There is one sour note to all this, however, because the STOR results – like much of what was published prior to, say, 1995 – has essentially evaporated from view: a recent satellite study of frontogenesis in the North Pacific noted unusually crowded fronts off SW Baja, making it a unique region in Pacific oceanography, and commented that although this was known to be an area rich in large pelagic species – whales, sharks, tuna and turtles – it would be interesting to know what attracted them! The entire literature from the STOR investigations had been missed! Then, my pained response “*The answer must be red crabs, of course!*” was itself the sole reference to red crabs in a later paper I received for review, this time on why turtles aggregate off southern Baja: again, the earlier, pre-electronic papers were all unread! Nor did the editor of the journal insist that the author at least consult the primary literature…

As an afterthought, I should perhaps mention that Francoise and I now run a gallery of contemporary art in Cajarc, a small town in the Lot valley of SW France, where we established ourselves after I retired from BIO in 1995; one of the clients of our gallery, some years back, was a very surprised physical oceanographer from – of all places – Scripps!

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