Science and the Destruction of a Shrimp Fleet

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ABSTRACT Overcapacity is endemic to the tropical shrimp fisheries. This condition is routinely attributed to problems of open access and to the desires of developing countries to maximize export earnings. I argue here, through an analysis of two scientific debates as 'discursive formations,' that shrimp science itself has fundamentally aided and abetted this crisis. Over the last decade, there have been systematic efforts to bolster the received wisdom that (1) shrimp cannot be biologically overexploited (despite documented cases of resource collapse) and (2) there is thus no imperative to regulate fleet capacity in the fishery (despite widely acknowledged examples of economic overfishing). By giving scientific authorization to this received wisdom, I suggest, scientists have endeavored to absolve themselves of the crisis in tropical shrimp fisheries.

Black Thursday: A Prologue

A violent confrontation between two factions of Mexican shrimp fishermen broke out on May 24, 1990 – 'Black Thursday' – in the city of Guaymas, Sonora. The dispute continued through the year. No one died but offices were ransacked, cars and trucks burned, leaders jailed, roads blocked, strikes initiated. A host of government agencies were drawn into the conflict, on the side of one or the other of two competing federations of shrimp cooperatives. In the end – October 8, opening day of the offshore shrimp season in the Gulf of California – only 50 trawlers out of a fleet of 330 were allowed to leave port.

Black Thursday was about overcapacity in the fleet. It punctuated a trajectory of distress in the industry. Over the last decade, catches per vessel have been halved (Miller 1990:87). For lack of adequate earnings, cooperatives have not maintained their boats. Without the means to repay loans for operating costs, owners have been denied further credit. Unable to satisfy the economic expectations of their workers, cooperative leaders have been accused of all varieties of financial and political malfeasance. Fisheries authorities, in turn, have revoked the operating permits of contentious cooperatives.[1]

Overcapacity is an endemic problem in shrimp fisheries, elsewhere in Mexico and throughout the tropical world. It is routinely blamed on open access (Johnson and Libecap 1982), on the unchecked desire of developing countries – enticed by lucrative export markets for shrimp – to subsidize and expand fleets (Platteau 1989), and on the willingness of international aid organizations to underwrite the modernization of fisheries in the developing world (Bailey, Cycom and Morris 1986; Evans 1976; Kurien 1985).

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Submerged in the discourse of crisis is the role of shrimp science itself. Have the practitioners of this science forewarned of the crisis in the world's shrimp fisheries, or have they fostered it? An answer can be found in a close reading of two debates over the last decade.

One concerns spawners and recruits. Are shrimp stocks susceptible to biological collapse from overfishing? The second concerns the utility of surplus production models for shrimp fisheries. Can appropriate levels of fishing effort – fleet capacity – be specified?

Ostensibly, these address separate questions, one, the problem of recruitment overfishing, the other, of economic overfishing. I will suggest, however, that these questions have become inextricably linked in the dominant discourse on shrimp. Through this linkage, I will further argue, shrimp scientists have endeavored to deflect responsibility for the crisis in the fishery.

Scientific Discourse and Systems of Production

What I am suggesting, in a larger frame, is that we give serious attention to the casual claim recently put forward by Pálsson and Durrenberger: 'Biological models are not simply descriptions of nature, they are cultural artefacts, too' (1990:138). Science, they imply, has an epistemological status rather similar to 'folk models,' models which

> are not necessarily authentic but are representations of current social relations of production; they are aspects of the way people represent their relations of production to themselves. The ideas have histories just as relations of production have histories (Pálsson and Durrenberger 1990:138).

Folk models and biological ones are, in short, equally products of social discourse.

While this is by no means a novel contention, most analysts focus their attention on the extratextual features of the discourse. Thus, for example, Pálsson and Durrenberger urge that we 'start to inquire about the place of biologists and other policymakers in the systems of production of which they are parts' (1990:138; cf. also Durrenberger 1988; Smith 1990; McEvoy 1988; Meehan 1984). The point of such inquiry is to unveil the processes by which the 'authentic discourse' of scientists becomes as well an 'authoritative discourse' (Asad 1979:623), informing, in Pálsson and Durrenberger's words, the 'making of the rules under which fishermen must work' (1990:138).[2]

As a complement to this agenda, I suggest that we attend as well to the textual features of science discourse. A useful guide to such analysis may be found in Peter Goodrich's treatment of 'discursive formations,' defined broadly as 'the relation of bodies of knowledge to social practice and structure' (Goodrich 1987:132).[3] Goodrich distinguishes three elements of a discursive formation:

> those of its material basis, or *institutionalisation*, its self-articulation or internal ordering, *intradiscourse*, and its relation to other discourses and discursive formations, its *interdiscourse* (1987:144).

In brief, institutionalization is concerned with the processes through which certain speakers – not any speakers – become authorized to speak. These are the processes with which Pálsson and Durrenberger, following Asad, are most directly concerned. Intradiscourse addresses the

> *self-authorisation* of the discourse itself; characteristically, its delimitation of its objects, its elaboration of the rules and procedures appropriate to recognition or knowledge of such objects, and its privileging of specific terminologies, values and meanings in its systemisation (Goodrich 1987:146).

Interdiscourse, in turn, is concerned with the relations among discrete discursive formations, whereby the semantic materials of one formation, discipline, or 'text' may be 'in large measure predetermined or given by processes external to the text in question' (Goodrich 1987:150). The notion thus challenges 'conceptions of the autonomy or internal development of disciplines and their illusion of self-identity of subject matter, language and meaning' (Goodrich 1987:151).

In short, whereas institutionalization speaks to the question of who may speak, intradiscourse and interdiscourse are concerned with the production of *what* is said. While Pálsson and Durrenberger rightly direct us to the first question, my focus here will be on the equally critical issue of what, in fact, shrimp scientists are saying.

The Stock-Recruitment Relationship

The notion that populations of tropical shrimp (*Penaeus spp.*) can be biologically overfished entered the scientific discourse in the early 1980s. John A. Gulland, the dean of the stock assessment fraternity, offers some reasons for this tardy examination and of the potential for collapse:

> The implicit assumption has been that recruitment is independent of adult stock, *ie*, the average recruitment is the same at all sizes of spawning stock. The scientific justification for this is obscure, though a common reason is that, by making it, the analysis is made much simpler and there has so far been no obvious case of recruitment overfishing among shrimp stocks (Gulland 1984:292-293).

Gulland's quizzical evaluation of the received wisdom on the stock-recruitment relationship (SRR) was presented at a workshop convened in 1981 at Key West, Florida. Jointly sponsored by the U.N. Food and Agriculture Organization and the United States' National Marine Fisheries Service, the conference assessed the states of several arts – of scientific models, of the actual status of the world's shrimp fisheries, and of management.

The summary statement from the workshop is an extraordinary document. Country by country, Mexico's experience was reproduced. By 1980, fishing effort was excessive; stocks were fully exploited. And there was a call for more scientific advice to correct the ills of the global industry (Anon. 1984:10-11). Indeed, the scientists assembled in Key West rather adroitly absolved themselves of responsibility for this state of affairs. By their own consensus, they were simply dependent agents:

> Despite the growing problems being faced by the managers of shrimp fisheries, the scientists were, in many countries, not well prepared to provide the managers with the advice required. One reason for this has been lack of definition of the ultimate management objectives. Even for a single fishery these may be incompatible, contradictory, and sometimes amazingly vague. Unless the scientist has clear guidance on what the fisheries are being managed for, it is difficult for him to plan his research and frame his advice in an appropriate manner (Anon. 1984:11).

This is a rather fundamental claim proffered up by the scientific fraternity, a strategic interpretation of the relationships among science, polity, and economy. It need not go uncontested. I will suggest, provisionally, an alternative view: much of the scientific work which followed the gathering at Key West has been constituted not by the agendas of local resource managers but by the received wisdom itself. And, I suggest, that wisdom legitimized a consensus on management voiced by Serge Garcia of the Food and Agriculture Organization.

Garcia would shortly expand a paper he had prepared for the Key West workshop. The article, entitled 'The stock-recruitment relationship in shrimps: reality or artefacts and misinterpretations?' (Garcia 1983), offers the following advice on the management of penaeid shrimp fisheries:

> For the want of better information it therefore seems advisable (both for economical and biological purposes) to manage a shrimp fishery by limiting the effort below some threshold level corresponding to an 'acceptable' level of risk. This is of course rather theoretical because this level is difficult to define in practice (except by trial and error, progressively increasing the level of effort allowed). It is likely that with the present fishing conditions, keeping the industrial fishery profitable (without government subsidies), would allow the biological objective (avoiding the critical level of effort) to also be met on average... (Garcia 1983:45).

These two brief texts, one claiming the deference of science to management, the other a rather irresolute example of the advice flowing from the scientist to the manager, frame the skirmish on Black Thursday in Guaymas. And they give meaning to the crisis of overcapacity in the world's shrimp fisheries. The texts themselves are grounded in the debate over SRR.

Exmouth Gulf: Questioning the Received Wisdom

At issue in the SRR debate is the relative importance of environmental perturbations and fishing pressure in determining the size of shrimp stocks from year to year. The initial attack on the traditional assumption came from the Australian biologist J.W. Penn at Key West. His argument was equivocal and, at the time, largely anecdotal. It was to gain substance with the collapse of the tiger prawn (*Penaeus esculentus*) fishery in the Exmouth Gulf, Western Australia, in 1982.

Penn's starting point (Penn 1984) was a typology of 'catchability.' Some species of shrimp are simply more vulnerable than others to fishing gear – because they aggregate in schools, because they are active in daylight hours, because they do not burrow. Penn used this simple notion to question both the economic and the biological foundations of the received wisdom. He suggested that the ease of catching some species belies the assumption that fishing will cease for economic reasons well before the stock is endangered. A corollary followed from this that stocks could indeed be destroyed (Penn 1984:178).

The notion that catchability has implications for the resilience of a stock is intuitively appealing. But Penn's proof was elusive. In the 1981 paper, he marshalled substantial evidence from a variety of tropical penaeid fisheries, showing the expected fluctuations in abundance from year to year – most readily explained by short-term environmental disturbances – but also some disturbing cases of long-term declines in catches. From this admittedly 'circumstantial evidence,' Penn warned that 'control of exploitation ... will become increasingly necessary for conservation as well as economic reasons as technological advances continue to increase effective effort in the established fisheries' (1984:185).

Technology was to blame, in part, for the decline of the tiger prawn in Exmouth. As Penn was positing a collapse because of the vulnerability of tigers to trawls, the Australian government was subsidizing the construction of larger trawlers. And older boats were retrofitted with propeller nozels, increasing speed and trawling power. By the mid 1970s, mechanical shrimp peelers were introduced, allowing efficient processing of small prawns and enticing the fleet to commence fishing earlier in the season. Finally, record prices for the tiger prawn in 1978 and 1979 put further – transitorily fatal – pressure on the stock (Penn and Caputi 1985:166).

The Exmouth fishery, nonetheless, had been one of the most conscientiously managed in the world. Exploitation began in the Gulf shortly after the nearby Shark Bay fishery opened up in 1963. Both fisheries were subjected to limited entry restrictions from the beginning, determined and administered by the Western Australia Department of Fisheries and Wildlife (DFW).

In addition to controlling expansion – from 15 licenses in 1965 to 23 by the end of the 1970s in Exmouth, from 25 to 35 over the same period in Shark Bay – DFW amassed a substantial data base. These circumstances – limited entry and close monitoring of effort, catch, and the environment – afforded John Penn the opportunity to detect and document a case of recruitment overfishing for shrimp. His argument, published with colleague N. Caputi in 1986, relied less on his earlier hypothesis about the susceptibility of certain species to capture and more on the events of 1981 and 1982 in the Exmouth Gulf.

An open bay along the arid western coast, Exmouth Gulf proved to be a propitious natural laboratory for the study of spawning stocks and recruitment. The isolated region has been spared major human disturbances of the littoral environment and the hydrologic parameters of the Gulf have been relatively stable over the period for which time-series data are available. With relative ease, then, Penn and Caputi were able to account for the two historic environmental perturbations in the Gulf – a cyclone in January of 1971, which devastated juvenile shrimp still on their nursery grounds, and a similarly severe storm in February of 1975, which had the opposite effect of improving the survival of adult shrimp in the offshore fishery by increasing the turbidity of the water and thus reducing predation and mortality of the stock (Penn and Caputi 1986:500).

Of the four penaeid species in the Gulf, Penn had suggested that the tiger prawn was most vulnerable: *P. esculentus* has adapted the unfortunate strategy of schooling during spawning season (Penn and Caputi 1986:503). But why did the catch, relatively stable up to 1980, drop to half in 1981, then to about a fifth of its former magnitude the following year?

Regression analyses revealed the expected conclusion: the environment (rainfall and hence salinity) of the juveniles *and* the fishing pressure exerted on spawning adults of the previous year explained 94% of the year-to-year variation in catch (Penn and Caputi 1986:500). Moreover, it was the wholesale expansion of effort in the late 1970s – by roughly 50% – that tipped the balance. Penn and Caputi conclude, simply, that the levels of fishing pressure had reduced the equilibrium between spawning stock and recruits to a 'relatively low' point – indeed a point close to a 'total collapse level of effort,' even under normal environmental conditions (1986:502).

Penn and Caputi close a companion paper on management with reasoned advice:

> In practical terms the present study has ... shown that the possibility of recruitment overfishing of penaeid stocks can no longer be disregarded in management terms. As observed by Walters and Ludwig (1981) 'in the long term view it is simply irrelevant to comment that recruitment can often be predicted more accurately from environmental factors than from spawning stock, because it is the spawning stock which can be controlled through management decisions' (Penn and Caputi 1985:173).

In short, Penn and his colleagues questioned the authority of the received wisdom and invoked an alternate text on management. By the end of the decade, however, Walters and Ludwig would no longer provide a necessary piece of the Australians' argument.

The Gulf of Mexico: Sustaining the Received Wisdom

Serge Garcia of the FAO, Penn's protagonist in the SRR debate, had already challenged much of the data upon which the suspicion of recruitment overfishing rested. Garcia detailed his critique most fully in his 1983 paper, which begins with an approving nod to the received wisdom expressed in the management plan for the multi-million dollar shrimp fishery in the Gulf of Mexico. That plan, adopted by the United States' National Marine Fisheries Service in 1981, repeatedly asserted that the three commercially significant shrimp stocks in the Gulf are 'biologically impervious to the effects of intense fishing pressure' (Anon. 1980:74178) due to the classical wisdom: only a small population of these highly fecund spawners is needed to replenish the stock.

Two critical observations followed: (1) the maximum yield 'for a given year is essentially all the shrimp available to harvest, using current technology' (Anon. 1980:74274), and (2) 'no recommendations are made on limiting fishing effort because the resource is not biologically overfished' (Anon. 1980:74192).

The Mississippi River set the course for research underlying the Gulf of Mexico shrimp fishery plan. The river disgorges much of the surface runoff of the continental United States into the Gulf, making that body of water the ecological antithesis of the arid and stable Exmouth Gulf in Western Australia. In the early 1950s, American researchers began a sustained effort to understand the effects of the environment on shrimp in the Gulf. The results may usefully be viewed through the strategic lens of Serge Garcia and his colleague, L. Le Reste, preparers of the definitive 'Life Cycles, Dynamics, Exploitation and Management of Coastal Penaeid Shrimp Stocks,' issued by FAO in 1981.

Within this comprehensive review, Garcia and Le Reste surveyed worldwide studies of interannual variations in abundance. As a preliminary, they distinguish short-term variation as 'noise' and long-term variation as 'signal.' And they suggest, for the latter, that it is 'tempting to look for the cause of this phenomenon in long-term climatic variations, as many authors have done' (Garcia and Le Reste 1981:119). The pivotal text from the Gulf of Mexico addressed the noise; Garcia and his colleague deftly read signals from the same data.

The data set was propitious: catch statistics for whites (*P. setiferus*) and browns (*P. aztecus*) in Louisiana and Texas, rainfall in Texas, river discharge from the Mississippi and Atchafalaya in Louisiana – from 1927 to 1964. Gordon Gunter and Judith Clark Edwards (1969), confirming studies Gunter had been pursuing for 20 years, obtained significant correlations, in the Texas time series, between high catches and heavy rains during the current and preceding two years – for white shrimp, but not the browns. And not in Louisiana. There, they found no significant correlation between river discharge and the catches of either species. Hence the picture of environmental noise – demonstrably affecting production in the Gulf – was complex. As

Garcia and Le Reste observe, 'freshwater outputs seem thus favorable to production in Texas and unfavorable in Louisiana for the same species' (1981:123).

What intrigued Garcia and his colleague, however, was not so much that interannual 'noise' might be comprehended but that the Gunter/Edwards data set simultaneously revealed longer term environmental signals. Reanalyzing the time-series data, the FAO researchers discovered synchronous variation in white shrimp catches off the two Gulf states, despite the fact that the species responded differentially to freshwater outflows. This, they suggested, 'would imply the action of a common factor, probably of climatological origin' (Garcia and Le Reste 1981:123).

Garcia and Le Reste do not attempt to define and evaluate the implicated factor in the Gulf of Mexico. Nor, interestingly, does the reinterpretation of the Gulf material play a significant role in Garcia's subsequent discussions of the stock-recruitment relationship. Thus, the epistemological status of the conclusion – the common climatological factor – may rest within the Garcia/Le Reste text itself. The critical context is found in their 2-page treatment of SRR. They observe, first, that

> There are no shrimp stocks, even those which have been heavily exploited, for which it can be shown with certainty that the recruitment has been affected by the exploitation of adults, except perhaps recently in the gulf between Iran and the Arabian peninsula (Garcia and Le Reste 1981:135).

Then they argue the following:

> For practical purposes ... if there is no clear relationship between the stock and the recruitment within a range of reasonable levels of exploitation, such relationship can be wholly neglected and recruitment can be considered to depend on the environment only. *This conclusion, even if not clearly expressed, underlies all the applications of predictive models based on the relation between the production and the climatological conditions* (Garcia and Le Reste 1981:135; emphasis added).

Two preliminary observations can be made. First, John Penn started with the assumption that shrimp stocks could fail through recruitment overfishing. Then the Exmouth Gulf fishery quickly obliged. Garcia and his colleague, as the highlighted passage above suggests, drive their review and analysis with the opposite assumption, the received wisdom. Second, the strength of this received wisdom into the early 1980s was such, apparently, that Garcia and Le Reste did not feel compelled to fully document their reading of the Gulf of Mexico data. A simple statistical representation of the climatological data sufficed.

Garcia did, however, immediately undertake a challenge of the one apparent case of recruitment overfishing (prior to Exmouth Gulf) cited in his review with Le Reste: the Persian Gulf.

The Kuwait Case

Garcia and co-author Gary Morgan of the Kuwait Institute for Scientific Research begin their brief analysis with the disturbing observation:

> The catches from the shrimp stocks of the Gulf between the Arabian peninsula (the majority of which are *Penaeus semisulcatus*) have been declining since reaching a peak in the late 1960's, resulting in the closing down of most of the fishing companies in the Gulf (Morgan and Garcia 1982:133).

Off Kuwait, stocks improved in the early 1980s, and fishing resumed. Indeed, landings for the 1983/1984 season were at record levels, comparable to the previous peak season of 1966/1967. But the effort expended on this catch had increased four-fold over the relatively constant levels of the 1960s and 1970s, and indices of recruitment had declined steadily. One authority warned in 1985 that this 'long term decline, if continued indefinitely, could lead to zero recruitment (and, therefore, zero landings) in the early 1990's' (Mathews 1985:27).

Morgan and Garcia do not deny the observed trend, but they point suggestively to the environment, not overfishing:

> ...the reason for the recruitment decrease since the beginning of the fishery might have to be found largely in changes in the environment ... (trends such as land reclamation, decreases in ... [river] outflow by damming, etc.) (1982:136).

Morgan and Garcia arrive at this conclusion not through a direct route of measuring the presumed environmental changes, but through a comparative analysis of several stock assessment measures. First, they construct descriptive surplus production models for the Kuwaiti and Saudi Arabian fisheries, models which reveal wide variation in catches for given levels of effort. More importantly, though, the two fisheries display 'a rather different history in terms of effort development' (Morgan and Garcia 1982:136), even though the decreasing recruitment trends track one another. This suggest to them – as was the case in their comparison of Texas and Louisiana fisheries – a common, unmeasured, environmental influence. And they eyeball the fit among unrelated measures of recruitment, all of which show the same 'apparently quasi-linear' and decreasing trend between stock in one year and recruitment in the next. Since some of these measures incorporate the effects of fishing and some do not, the authors suggest that the similarity in trends exonerates fishing as the causal agent in the long-term decline in stocks (Morgan and Garcia 1982:136).

They conclude, finally, that the decline is unlikely to be reversed by limiting or withholding fishing effort: 'only the reversion of the causal environmental factors (provided they are reversible) will allow for a suitable recovery of the fishery' (Morgan and Garcia 1982:137).

By logical argument in the case of Kuwait and by correlation of rainfall and catches for the Gulf of Mexico fisheries, Garcia had thus constructed his defense of the received wisdom. His more comprehensive argument followed shortly upon the brief Persian Gulf study: the apparent manifestations of stock-recruitment relations in shrimp were all statistical artifacts, produced by autocorrelated environmental effects. As he argued confidently in 1983,

> The short-lived nature of shrimp stocks ... introduces a direct linear relationship between the size of the recruitment in year n (R_n) and the size of the subsequent adult stock (S_n in the same year). As a consequence the existence of an environmentally-driven serial correlation in recruitment oscillations would therefore lead to the existence of a serial correlation in stock sizes, and to the production of a spurious positive relationship between S_n and $R_n + 1$ (justifying the word artefact) (Garcia 1983:41).

His position draws on sustained research efforts in the Gulf of Mexico to empirically document the recruitment or replacement function, $S_n = f(R_n)$. In the Gulf, however, this research effort was not undertaken to shore up the received wisdom but rather to facilitate the prediction of commercial stock sizes. The premise is simple: if there is a strong relation between the abundance of postlarval or juvenile shrimp during the estuarine growth phase and the subsequent abundance of adult shrimp offshore, then fishing effort could be adjusted seasonally to the forecasted stock.

Under this premise, Berry and Baxter (1969) sampled postlarval and juvenile abundance in Galveston Bay, Texas, as well as the reported catches of the bait shrimp fishery in lagoonal waters. Similar work undertaken in Louisiana (Barrett and Gillespie 1973) produced equally promising results: juvenile indices of abundance and actual inshore catches were indeed strongly correlated with offshore abundance and could provide sufficient lead time (roughly 3 months) to enable the industry to make appropriate production decisions.

Thus RSR – the demonstrated relationship between recruitment and stock, between juveniles and adults in the same year – would seemingly have utility for Garcia's arguments against SRR. Variations in commercial stock abundance are a direct function of environmental conditions and exploitation in estuaries, and progressive habitat destruction in the estuaries could indeed produce the 'quasi-linear' decline seen in Kuwait. Yet Garcia is surprisingly ambivalent about the value of predictive models generated from recruitment-stock relationships. He observes, for example,

> Most of them have to prove their effectiveness... [W]hen the ability to predict cannot be developed at a reasonable cost the solution consists in evaluating and including uncertainty in the models... (1989:285).

Subtly, here, Garcia is privileging 'signal' over 'noise,' longer environmental trends over interannual variations. Within the specific *intra*discourse over the stock-recruitment relationship, this effort is necessitated by Garcia's 'artifact' argument: the apparent serial correlations of stock sizes from year to year are in fact spurious manifestations of autocorrelated environmental effects. But this emphasis has implications for the *inter*discourse on shrimp as well. Garcia will argue that environmentally-driven trends invalidate the use of surplus production models, models designed to specify maximum sustainable yield and, correspondingly, fleet capacities.

And on the issue of noise – the lingering suspicion that short-term fluctuations in stock sizes may in fact be attributable to overfishing as much as to environmental oscillations – John Penn would himself, by the end of the decade, diffuse the concern.

Convergence

Penn and Garcia contributed to an important volume on the assessment and management of marine invertebrate fisheries (Caddy 1989a). There is, in a conjoint reading of these two statements, surprisingly little concern for SRR. Since Penn most directly challenged the received wisdom on stock and recruitment, it is useful to focus on his contribution to the 1989 collection, prepared with his Australian colleagues (Penn, Hall and Caputi 1989:128-130).

Highlighted in the 1989 paper is not the vulnerability of the tiger prawn to recruitment overfishing but rather its *recoverability*. The argument reproduces an analysis developed in the earlier works, but stripped of the cautions and caveats which had originally accompanied the schematic (Figure 1).

The analysis, based on the Exmouth Gulf data, explicitly seeks an accommodation with Garcia while retaining the documented reality of SRR. The three curves (A,B, and C) in Figure 1 represent initial stock-recruitment relations, modified by varying environmental conditions during the recruitment phase, resulting in a series of equilibrium levels for stock and recruitment. The effort lines, in turn, correspond to recruitment-stock relationships under varying fishing pressure. The intersections of these lines then depict equilibrium stock and recruitment levels under varying exploitation and environmental patterns. Thus, for example, curve B, reflecting average environmental conditions (e.g., for Exmouth, of low January and February rainfall) intersects with the fishing effort line of 60 units – characteristic of the fishery in the late 1970s – at a relatively low equilibrium point.

The analysis is formally identical to one presented by Garcia in his 1983 paper. There, Garcia directed his attention specifically to 'collapse levels of effort,' where the effort/RSR line no longer intersects the SRR/environment line. In the Penn/Caputi illustration, this would occur immediately if additional effort was applied to 'average' (B) conditions. And, indeed, Penn and Caputi note that 'recent levels of effort under normal environmental conditions are also very close to the total collapse level of effort' (1986:502) defined theoretically by Garcia.

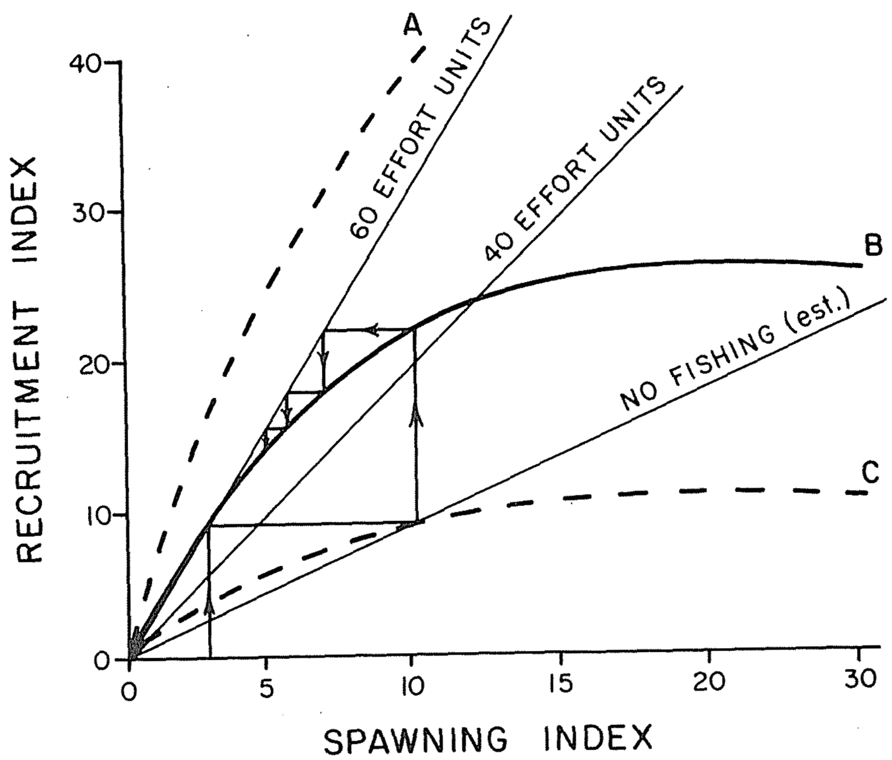

Figure 1. *Exmouth Gulf: Interaction between SRR and RSR for tiger prawns under varying environmental and effort conditions* (Source: adapted from Penn, Hall and Caputi 1989:130).

Let us follow through the Penn/Caputi reasoning. Contrary to Garcia, who with Morgan had rejected the utility of decreasing fishing effort to spur stock recovery, Penn and Caputi suggest that stocks will recover quickly with a curtailment of exploitation (indicated in Figure 1 by the arrowed line).

Their 1986 exposition was tentative, noting the synergistic biases likely if errors had been made in estimating the effective fishing effort and spawning stock and recruitment indices (1986:503). The 1989 reading of this graph is rather more confident. Penn, Hall and Caputi (1989:130-131) observe the following:

> The zero effort line (i.e., no fishing), which has been extrapolated from the data, indicates that the recovery from low stock and recruitment levels could be quite rapid, say 1-2 yr, if fishing ceased... This suggests that with unrestricted fishing effort as vessels left the fishery, a rapid buildup of stocks would occur, thus encouraging fishing effort to increase again. High fishing effort would then lead to a reduction in abundance, followed again by a reduction in effort. This sequence of events would tend to repeat itself and produce cycles in abundance.

This is not the kind of argument Garcia would make: variations in stock abundance, below the 'collapse level of effort,' are – resolutely – driven by the environment and by the predation on juveniles by artisans.[4] If this environment is deteriorating progressively or if the artisanal sector is expanding, little can be done to improve the fishery offshore (Garcia 1983:43).

But lost in Penn's new position is his earlier effort to give authority to Walters and Ludwig on the issue of management. Now, with the 'rapid buildup of stocks' presumed, there is no imperative to control long-term spawning stock size through management. Indeed, Penn's conclusion in 1989 closely mirrors the management edict offered by Garcia shortly after the Key West workshop: the patterns of profit (or loss) in the fishery itself, the short-term entry and exit of vessels, will sustain the stock – or at least allow it to recover from collapse.

Abetted, then, by Penn's willingness to model a variable environment and his logical capitulation on the issue of management, Garcia restricts his 1989 discussion of the stock-recruitment relationship to two paragraphs. Although acknowledging that the SRR question 'remains open and is certainly worth more attention than it has received in the past,' (1989:286), he rather glibly dismisses the Exmouth Gulf collapse:

> It is of course obvious that at some high level of effort problems of recruitment can be encountered (Garcia 1989:286).

John Caddy of the FAO, prefacing these contributions, closes a decade of discussion on a Garcian note:

> The debate between the relative effects of excessive effort and of environmental factors such as rainfall on shrimp still continues, with the balance perhaps now tipping toward environment as opposed to parent stock size as the main controlling factor for penaeid shrimps except under conditions of extreme stock depletion (1989b:6).

Thus the debate had collapsed. Garcia was convinced that the trouble in Exmouth Gulf was due to radically excessive effort. Penn acquiesced, but was confident that it didn't mean much to the resilient tiger prawn. With the help of his Australian colleague, then, Garcia had largely succeeded in reauthorizing the received wisdom: recruitment overfishing is, for Garcia, unlikely, and for Penn, irrelevant.

This convergence in turn shaped – 'preconstructed' (Goodich 1987:150) – a second discourse, the question of economic overfishing.

Surplus Production

'Surplus production,' simply defined as 'an increase in biomass in excess of losses to natural mortality' (Sissenwine 1978:23), is typically modeled as a dome-shaped (Schaefer) curve (Figure 2a). The shape of the curve is deter-

mined by three growth components: recruitment to the stock, individual growth of fish within the population, and natural mortality, including predation. In virgin, unexploited stocks, 'a point will be reached where recruitment and individual growth are just balanced by natural mortality and stock growth will cease (Anderson 1977:24).'

At that point (P_{max} in Fig. 2a), the unexploited stock size will be largest and there will be no 'surplus production.' Sissenwine reviews the curve's implications:

> Under exploitation, the stock adjusts to a new equilibrium level, at which point yield equals surplus production. Clearly, the population is unable to cope with sustained yields in excess of MSY. Here, MSY is defined as the peak value of the surplus production curve. Surplus production equals MSY for a population size of P_{msy}. If the population is larger than P_{msy}, then a constant catch rate of MSY will result in a downward adjustment of population size to a new equilibrium at P_{msy}. If the catch rate continuously exceeds MSY or exceeds surplus production that is smaller than P_{msy}, then the population will eventually decline to economic extinction (abundance level at which it is no longer profitable to fish) (1978:23).

In theory, then, it is possible to specify a level of fishing effort, F (hence F_{msy}), required to capture MSY, a level of effort (typically to the left of MSY, less than F_{msy}) at which profits in the fishery are maximized (MEY), and, finally, for stock which display significant interannual variation, a long-term, maximum average yield (MAY).

For shrimp, however, the appropriateness of the domed-shaped sustainable yield curve of typical production models has been questioned. Shrimp have only a year-long lifespan, individuals reach their maximum growth rapidly, are captured before natural mortality becomes a significant factor in total biomass and, under the conventional wisdom, the size of the spawning stock surviving through the exploitation phase bears little relation to subsequent stock sizes. Thus shrimp are presumed not to exhibit the growth parameters shaping standard surplus production models (Gulland 1984:293). Rather, it is assumed that, for a single cohort, a flat-topped curve is more appropriate, one without a descending right-hand limb (Figure 2b). And, given the presumed environmental influences affecting annual stock sizes, year-by-year production models will replicate one another only if the driving environmental variables are serially correlated.

A critical implication is contained in this line of reasoning. If recruitment overfishing *cannot* occur, economic overfishing *will* occur. Poffenberger, with the U.S. National Marine Fisheries Service, argued this proposition at the Key West workshop. First, he observes that

> Once effort has increased to a level which results in yields in the 'flat' portion of the curve then relatively large increases in effort would result in only modest increases in yield. The important point is that the sustainable yield would not decrease with increases in effort. Therefore, this model assumes that within the flat-topped part of the curve penaeid shrimp

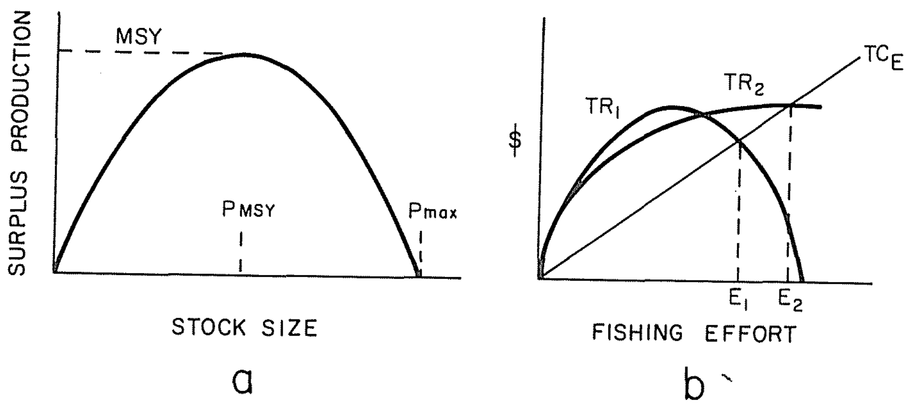

Figure 2. *(A) Surplus Production Model (Source: adapted from Sissenwine 1978:23); (B) Domed-shaped (TR_1) and flat-topped (TR_2) total revenue curves* (Source: adapted from Poffenberger 1984:303).

> stocks are capable of maintaining sustainable yields for a wide range of fishing effort (1984:300-301).

Impervious to biological overfishing – given this assumption that no stock-recruitment relationship exists – shrimp stocks simultaneously invite economic overfishing. This again follows logically from the shape of the flat-topped growth curve. In open access fisheries, regardless of the population dynamics of the stock, fishing capacity will move toward the bionomic equilibrium – the point where total revenue and total cost curves intersect. This point exceeds MEY, the maximum difference between total cost and revenue curves, and thus represents an inefficient use of capital and/or labor: 'economic overfishing.'

Figure 2b illustrates the comparative difficulties with flat-topped fisheries. The level of effort (E_2) associated with total revenues (TR_2) for asymptotic stocks, such as shrimp, exceeds the corresponding effort (E_1) for parabolic stocks. Thus, as Poffenberger deduces,

> it can be argued that for an uncontrolled penaeid fishery there is an inherent tendency towards more fishery participation (effort) than for fisheries targeting longer-lived species (1984:303).

In theory, again, the regulatory imperatives of this model are obvious. At once impervious to recruitment overfishing and prone to economic overfishing, penaeid stocks should be managed for efficiency – by limiting effort at the point of MEY. But how robust is the model itself? Let us examine, again, the Gulf of Mexico.

Surplus Production in the Gulf of Mexico

The Gulf of Mexico shrimp fishery has been subjected to the most sophisticated surplus production modeling in the world, and yet Garcia and Le Reste chide the industry as 'an excellent example of inefficiently managed fisheries' (1981:167). The Gulf's management plan, while devoting some discussion to the ills of economic overfishing, echoed Garcia in rejecting the utility of surplus production models for specifying maximum economic yield (Anon. 1980).

To be sure, the production models developed for the Gulf did reveal substantial confusion. Trends in effort and catch differed across the statistical reporting areas in the Gulf, differed by species, and were confounded by environmental oscillations. Several tentative conclusions were offered by the architects of the models: (1) stocks in the Gulf have been subjected to significant increases in effort since 1960; (2) catch per unit of effort has declined, indicating, according to Rothschild and Brunenmeister (1984:160), that 'stocks ... are decreasing in abundance with the progression in time;' and (3) several of the stocks appear to be 'at the transition point' between an asymptotic and a parabolic production function, suggesting the need for 'careful monitoring of the status of the stocks' (Rothschild and Brunenmeister 1984:160). But these authors admit to their own uncertainties:

> ...from a statistical viewpoint it is difficult to judge the descriptiveness of the asymptotic and parabolic forms of the production model with regard to the dynamics and status of Gulf of Mexico shrimp. If the asymptotic form holds, then the population is in good condition, but if the parabolic form holds then the considered population is on the verge of collapse (1984:170-171).

The problem arose when Brunenmeister tried to determine the appropriate 'm,' the exponential parameter in the nonlinear production model which gives shape to the fitted curve. For brown shrimp, Brunenmiester found the best fit ($R^2 = 0.572$) with m at 0.31, close to the m characteristic of a flat-topped curve. But nearly identical fits were obtained with m at 1.0 and 2.0, representative of parabolic curves (Brunenmeister 1984:203). Similar indiscrimination occurred for production models of white and pink shrimp although, in general, the estimated MSY with m close to 0.00 exceeded those derived from parabolic functions (Brunenmeister 1984:203-207).

Ironically, the surplus production modeling utilized in the Management Plan found the best fit with $m = 3$, a model which, the plan dutifully observes,

> is usually associated with species which are very susceptible to recruitment overfishing. Penaeid shrimp are very resistant to this type of overfishing (Anon. 1980:74278).

For heuristic purposes, the management plan adopted instead a GSP model with $m = 2.0$, still a parabolic function, and estimated a MSY for the entire

Gulf at 85 million pounds of browns, 38 M lbs. of whites, and 14 M lbs. of pinks (Anon. 1980:74280).

However, what follows directly upon the MSY discussion in the Management Plan's narrative was not, as we might anticipate, an analysis of MEY and a specification of appropriate fleet capacity. Rather, there was a cursory review of catch estimates for the inshore sectors of the industry (with an implicit suggestion that unrecorded catches from these sectors may be responsible for generating the improbable parabolic shape of the production function [Anon. 1980:74280]), followed by an attempt to estimate 'maximum probable yield' – biomass of the fishery at oscillation peaks. Treatment of MEY, contained elsewhere in the 200-page document, was ephemeral.

The Council at once acknowledged the problem of overcapitalization in the fishery and rejected corrective measures. Again, the received wisdom that shrimp manifest no stock-recruitment relationship impregnates the Plan's discussion of economics: 'No recommendations are made on limiting fishing effort because the resource is not biologically overfished' (Anon. 1980:74192). The parabola, used to generate MSY, was here replaced by the flat-topped curve, and the economic rationale suggested above by Poffenberger (and Garcia and Penn) was thus invoked. The Gulf fishery, the Plan suggested, had approached an open access equilibrium, with vessels 'generating just enough revenue to cover total costs over a long period of time, and entering or exiting the fishery in the short run with prevailing economic conditions' (Anon. 1980:74292). But that equilibrium point exceeds the level of effort needed to harvest the maximum economic yield (Anon. 1980:74242).

The result, of course, is a reduction in earnings per vessel. As the Plan observed, however, other objectives would be satisfied. Employment, inshore and offshore, is enhanced (Anon. 1980:74292); capacity is retained in the fishery to fully exploit peak stock abundances; and there are savings to be had by not having to enforce effort limitations (Anon. 1980:74292). In short, the Management Plan aimed toward its primary goal: '...to attain the greatest overall benefit to the nation with particular reference to food production and recreational opportunities on the basis of maximum sustainable yield as modified by relevant economic, social or ecological factors' (Anon. 1980:74192).

There are critics of the Gulf shrimp plan, and there are culprits other than the Management Council contributing to the overcapitalization of the fishery. Maril, for example, remarks on the dramatic rise in new-vessel costs in the 1970s, the result, he suggests, of investment capital 'gone looking for someplace to hide' during an inflationary decade (1983:146). The effects have been to saddle owners with high interest payments, to retard exit from the fishery in poor years, and to substantially increase fishing effort in the Gulf. And, again, there is noise: the peaks and the troughs. As Maril reports,

> Even if the shrimpers are out doing their best, they have 'bad' years. Bad years play havoc with the shrimper's income. In a bad year he will usually go into debt, and two bad years in a row can do him considerable harm (1983:148).

But to return specifically to the text at hand, the Gulf Council's management plan, some summary observations can be made. The structure of the argument appears to be as follows. (1) Since recruitment overfishing is assumed to be impossible, fleet capacity need not be regulated to prevent resource collapse. (2) Because of the strength of this received wisdom, the empirically-generated parabolic forms of surplus production models – implying the possibility for recruitment overfishing – must be discounted. (3) Nevertheless, because a parabolic function can be fitted to the historic catch and effort data for the fishery, MSY can be specified. (4) Existing capacity exceeds $f_m$sy but this excess ought not be reduced through entry limits and may, indeed, be beneficial for fully exploiting periodically large stocks.

The Gulf of Mexico's management plan, in short, closely replicates Serge Garcia's logic.

Garcia's Logic

Garcia is resigned to the fact that surplus production models are likely to persist in shrimp stock assessment. They are, as he notes, mathematically simple models with small data requirements, useful perhaps as empirical descriptions of the trajectories of fisheries (1988:238-239). Yet he rejects their validity as specifiers of MSY and $f_m$sy – because of the confounding effects of environmental signal (Garcia 1985:147). Without accounting for such signal, MSY will be misconstrued:

> The trajectory of a developing fishery across a catch-effort-environment three-dimensional space will be wrongly taken as a traditional production curve on the catch-effort plane, while in fact basic properties of the model will have been lost. In particular, if few data points are available and environmental changes are autocorrelated, the apparent MSY may be grossly in error and the curve may not predict correctly the catches obtainable with substantial changes in effort (Garcia 1988:238).

Apparently, what is implied in the last clause above is this: in such cases as developing fisheries, where effort by definition is expanding and the data points are likely to be few, catch/effort models may grossly *under*estimate sustainable yields. And, once again, he introduces his irreversibility argument: if observed downward trends are due in large measure to autocorrelated environmental effects, then

> the possibility of reversing the observed evolution of the fishery stock along the same trajectory by reducing the amount of effort is often not feasible (Garcia 1985:147).

Garcia thus invokes the environment to at once defend the received wisdom on the lack of a stock-recruitment relation and to question the utility of surplus production models. There is a temporal imperative in this logic which, I suggest, in large measure accounts for the crisis – in Guaymas and throughout the world's tropical shrimp fisheries. In short, Garcia privileges environmental signal over noise.

The bias surfaced first in the scientist's reanalysis of data from the Gulf of Mexico. Recall that these data were first analyzed by researchers for the purpose of identifying short-term environmental fluctuations – noise – as a means to forecast catches. Garcia and his colleagues addressed the data set to the question of signal, for the purpose of demonstrating the independence of long-term oscillations in stock abundance from short-term environmental fluctuations. By itself, the demonstration did not preclude the search for noise, and hence the possibility of developing short-term predictive models of seasonal abundance and adjusting seasonal fishing effort. But it did allow Garcia to invoke the notion of 'autocorrelation' repeatedly through the decade to refute claims for a stock-recruitment relationship and, as we have just seen, to question the use of surplus production models.

Once these arguments have been drawn, Garcia's management recommendations follow logically. Recall the advice: without fear of biological collapse, fishing effort can be allowed to increase so long as the industry remained 'profitable.' In turn, this would ensure that the biological objective – avoiding collapse – would be met *on average*. Here, John Penn and his colleagues fill the gap: if there is an unexpected collapse, the stocks may recover quickly.

Guaymas Revisited

The workshop in Key West in 1981 was a watershed. The received wisdom on SRR first came under close scrutiny and stimulated much activity in the scientific fraternity through the decade. But the operation of the wisdom had already worked itself out. Shrimp fleets throughout the tropical world approached or occasionally exceeded collapse levels of effort. Frequently – as in the case of Mexico (Edwards 1978:146) – these expansions themselves were encouraged and subsidized by FAO and other aid agencies. They occurred in developed countries – Australia and the United States – and developing ones alike. The scientific discourse of the 1980s was largely a post-mortem, disingenuously validating the received wisdom.

Ultimately, the truth of this authoritative discourse turns out to be a great deal less important than what it authorizes. Simply put, it shapes the events which occured on Black Thursday in Guaymas. Neither the loan schedules of boat owners nor the nutritional demands of the shrimpers and their dependents are indexed to indices of recruitment. Pressures on the fishermen do not oscillate with environmental signals and noise. But these are the ones who must finally contend with the received wisdom. In Guaymas, Garcia's project of 'keeping the industrial fishery profitable' may occur, as captains and cooperatives watch their boats rot at the docks.

Notes

1. Information on the current events in Guaymas has been obtained from Sonoran newspapers,

primarily *El Imparcial*, Hermosillo, Sonora, Mexico, and from discussions with officials of the Secretaría de Pesca and its research unit, the Centro Regional de Investigacion Pesquera, in Guaymas. Details of the conflict are reviewed in Vasquez-Leon, McGuire and Cederstrom (1991). Background on the Pacific Coast shrimp fisheries may be found in McGoodwin (1980; 1987); McGuire (1983); Magallon-Barajas (1987); and Chávez and Lluch (1971). Hoyos (1991) briefly reviews the recent deterioration in yields in the Gulf of California and suggests that shrimpers themselves are attributing the decline to fishing during the spawning season, *i.e.*, the reality of a stock-recruitment relationship.

2. A similar agenda has been set for the 'juridical field' by Pierre Bourdieu (1987). Translator Richard Terdiman summarizes: 'Bourdieu traces in detail the social and particularly the linguistic strategies by which the inhabitants of the legal universe pursue this effort to impose their internal norms on broader realms and to establish the legitimacy of interpretations favorable to the self-conception of the field, to the ratification of its values, and to the internal consistency and outward extension of its prerogatives and practices (1987:809).'

3. Goodrich's scheme, which he applies to legal discourse, draws primarily upon Mikhail Bakhtin's *The Dialogic Imagination* (1981) and Michel Pêcheux' *Language, Semantics and Ideology* (1982).

4. In an FAO *Technical Paper*, Willmann and Garcia construct a bioeconomic simulation model of the sequential (inshore, small-boat sector; offshore, industrial sector) shrimp fishery in Surinam. They offer the following advice: '...an expansion of artisanal shrimp in coastal swamps will imply a reduction in income, employment and foreign exchange earnings to the Surinamese economy. A promotion of artisanal fisheries could be justified on account of re-distributing income and employment to rural areas, and in respect to greater supply of protein to rural consumers. In regard to both these objectives, alternative ways and means may, however, be more appropriate considering the great loss in foreign exchange earnings resulting from such a policy (1985:42).'

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