# Bluefish Science in the Northeast Region: A Case Study

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In May of 1998 the following appeared in the ASMFC Fisheries Focus, the newsletter of the Atlantic States Marine Fisheries Commission:

> The S&S committee recognized the shortcomings of the assessment but concluded that it represents the 'best scientific' characterization of the Atlantic bluefish stock given the currently available data. An important caveat is that the assessment does not consider the possibility that the sharp declines in landings and abundance indices may be due to migration of adult bluefish to offshore areas.

Behind this acceptance of a scientific finding by a government committee lies an attempt by three government agencies, teams of scientists and the representatives of an enormous fishing public to decide how many bluefish were in the sea. This committee identified the measure of the bluefish population that would have legal force, and then offered the caveat to acknowledge that this decision specifically ignored what most informed people thought was the most important single factor involved in counting the bluefish. Perhaps more surprising, this did not result from ignorance, arrogance or incompetence but was a reasonable decision for the committee to come to given a peculiar intersection of the logic of the law and the logic of science.

Along the Atlantic Coast of the United States, bluefish (*Pomatomus saltatrix*) is a very important fish. It is a staple of marine recreational fishing in the New York Bight, the ocean area between Cape Cod in the north and Cape Hatteras in the south. Since 1981 recreational landings have averaged 72 million lbs, with just over half caught from individual boats, 13% caught from party and charter boats, and the remaining 36% caught from shore (MAFMC and ASMFC 1998). Bluefish was more popular in the 1980s than the 1990s when more anglers began to target the resurgent striped bass population. Bluefish has a symbolic importance that goes beyond these numbers, however, because so many children have their first marine fishing experience going after tiny "snapper blues" from the shore. The fishing public in general is very interested in bluefish management.

The idea that citizen participation improves fisheries management is widely accepted (McCay and Jentoft 1996) but participation in any policy setting process become problematic as soon as science is involved. Tensions between the perspectives of scientific experts and citizens have been observed in many aspects of environmental policy (Jasanoff 1990). In fisheries these tensions are given a special twist because, unlike food additives or nuclear power where compared to the scientists the citizens are usually quite ignorant, fishers know a lot about fish. Effective fisheries management must be based on knowledge of the fish stock that meets two related but independent tests: it must be accurate and it must be perceived as accurate by the public. If it fails the first test then the management measures are meaningless because they cannot bring about the changes that managers wish to make. If it fails the second test then management becomes more difficult to enforce. This is particularly true in a recreational fishery like bluefish. All fishing behavior is strongly influenced by community opinion and, as recreational fishers' do not share the commercial incentive to catch more fish to make more money, community opinion about regulations have an even greater importance. At the same time, the surveillance of behavior is more difficult than it is in commercial fisheries where a smaller number of fishers leave and return to particular landing places.

The present case study addresses this central tension between science and citizen participation in fisheries management by documenting one particular scientific debate. It covers a period from the fall of 1996, when one scientific peer review panel approved an assessment of the bluefish stock, to the spring of 1998 when a second such panel approved another assessment. These scientific issues made up only a part of a much larger debate. In the summer of 1998 public comments were solicited on the bluefish management measures being considered. The largest number of such comments were directed at the way the bluefish stock was being allocated between commercial and recreational interests, the second largest at the types of measures being considered, and the third largest at the timing of rebuilding the stock. The comments directed at the quality of the science were only the fourth largest group. The scientific debate is strongly linked to these other aspects of management as people repeat the scientific facts that support their positions in the other debates.

Section I of this presentation is a short description of the research methods used in the study. Then three sections set the stage: Section II outlines the overall structure of the fisheries management system in the Northeast Region, as the area from Maine to North Carolina is referred to in federal fisheries management; Section III describes the system's scientific institutions; and, Section IV gives some brief historical background on bluefish management. The next three section describe the debate that ensued between the fall of 1996 and the spring of 1998. Section V examines four issues related to gathering data: the question of accurately measuring age; the question of the design of the main scientific survey of fish stocks; the question of the amount of effort expended by fishers; and, the question of incorporating fishers' observations. Section VI turns to three prominent explanations of what was happening to the stock: that observed changes resulted from predator - prey interactions; that observed changes resulted from the stock moving offshore; and that observed changes resulted from excessive fishing pressure. Finally, Section VII relates the deliberations of the Bluefish Technical Committee that led to the creation of the stock assessment model and the construction of the legally valid description of the stock.

In the spirit of bluefish science, I will begin by offering a caveat. Case studies are a wonderful method for learning. Their purpose, however, is to be instructive in their detail rather than representative of a class. In this case, the unrepresentativeness that the reader must be careful of is that, because of the extremely uncertain data that scientists have to work with, assessing bluefish stocks is an exceptionally difficult example of a generally difficult problem. Sociologists of science often intentionally trace areas of scientific uncertainty and controversy because these situations reveal more about the social structure of the scientific enterprise than ones where everyone agrees. In this case study, the scientists involved operated in a difficult situation in a highly professional manner. The reader should remember that the professionalism found among fisheries scientists in the Northeast Region usually translates into more solid outcomes than it did here.

## I. RESEARCH METHODS USED IN THE CASE STUDY

The case study presented here is part of a larger study of the tensions between science and public participation in fisheries management. This study includes two other Northeast Region species case studies and two random sample surveys, one of marine fisheries scientists and the other of the

general population of people active in fisheries management in the Northeast Region. Information for the case studies was gathered in a number of different ways. Formal key informant interviews, lasting an average of one and one half hours, were carried out with 24 scientists, 21 fishers, many of whom served on advisory panels, nine activists in, or active observers of, the fisheries management system, and four administrators. Approximately 200 management related documents were reviewed including ten complete transcripts of the Council and/or Commission meetings, of which four related directly to bluefish, though only one of these took place in the case study time period.

We also observed a total of 43 meetings. Of these meetings 38 were official management functions and five were one public and four private meetings of fisheries activists. Those related directly to bluefish were five meetings of the Mid-Atlantic Fisheries Management Council and /or the Atlantic States Marine Fisheries Commission Bluefish Board, one meeting of the Bluefish Monitoring Committee, one meeting of the Bluefish Advisory Panel, two meetings of the Bluefish Technical Committee, and one special meeting called by the chair of the Council to discuss how to overcome difficulties in creating a bluefish management plan. Two of the private meetings of activists dealt with bluefish.

This research was funded mainly by the National Science Foundation with some assistance from New Jersey Sea Grant. All of the research took place under guidelines approved by the Rutgers University Institutional Review Board for human subjects research. All interviews were anonymous and confidential and we have taken care to protect the identities and affiliations of respondents. Special care has been taken to protect the identities of participants in private meetings that we were privileged to attend. We have also, for the most part, not named participants in the open, public meetings although people familiar with the process would often be able infer or find out who is being referred to. The reason for this is that our focus is on the structures and processes where science and participation meet rather than on personalities. The exceptions to this policy are prominent individuals speaking in public fora in cases where it is important to understand the authority behind what is being said. We are greatly indebted to the various participants in the fisheries management process for their courteous and willing cooperation in this work.

## II. THE STRUCTURE OF THE FISHERIES MANAGEMENT SYSTEM

The 1970s were a beginning point for much of today's marine fisheries management. Negotiations around United Nations Convention on the Law of the Sea were accompanied by declarations of 200 mile exclusive economic zones (EEZ) by most countries. In 1976 Congress created the US EEZ, and gave US federal fisheries management much of its present form, with the Fishery Conservation and Management (Magnuson) Act. This act created the eight regional Fisheries Management Councils. These councils are one of the earliest, and still one of the most ambitious, attempts at fisheries co-management, i.e., the sharing of management responsibility between a government and fishers (Jentoft 1989). Representatives of the fishing industry sit on and hold voting rights on the regional councils, which have certain legal powers over the creation of fisheries management plans (FMPs) in federal waters.

The regional council responsible for identifying bluefish management measures in federal waters is the Mid-Atlantic Fisheries Management Council (hereafter the Council). The Council works very closely, indeed regarding bluefish it usually meets around the same table, with the Atlantic States Marine Fisheries Commission (ASMFC or the Commission), which is responsible for bluefish management in state waters. The third major government actor in bluefish management, the National Marine Fisheries Service (NMFS, rather ironically pronounced nymphs) is a federal agency in the Department of Commerce. NMFS implements FMPs in federal waters and must ensure that they meet certain national standards. Most Federal FMPs are created by the regional councils but they only become effective when authorized by the Secretary of Commerce. The creation of bluefish FMPs is done jointly by the Council and the Commission, and both must approve them before they are sent to NMFS for evaluation and eventual acceptance or rejection by the Secretary of Commerce.[1]

The Council is made up of three members from each state from North Carolina to New York. One of these members is the director of the state marine fisheries agency and the other two are "industry representatives," often one from the commercial fishing industry and one from the recreational fishing industry. Two "at-large" seats also exist which can go to any state. In recent years there has been considerable pressure, with limited success, to include environmentalists and fisheries-trained academics on the councils. The current (2000) voting membership of the Mid-Atlantic council consists of seven state directors and twelve appointed members of which three are academics, two are retired fisheries management professionals, and the remaining seven are affiliated with the fishing industry. The word "representative" here should be understood as substantive rather than democratic representation. The selection of council members is done by state governments in a heavily lobbied process, not by the industry itself.

The Commission develops plans and coordinates management by the individual states, which have jurisdiction over "inshore" waters out to three miles. It was created by a compact between the states from Maine to Florida. Each state is represented by a someone from the state agency, i.e., the same state fisheries agency director who sits on the Council or his/her proxy, a state legislator, and an appointee of the governor. Decisions about bluefish management are made by the Bluefish Board, which, like all Commission management boards, consists of the state fisheries directors, one legislative appointee, and one governor's appointee. Management within three miles is legally authorized by state legislatures and carried out by state agencies. However, the overriding need for interstate cooperation, federal legislation which allows the enforcement of Commission decisions on states, and a growing role in channeling federal funds, have given the Commission the central role in inshore marine fisheries management along the Atlantic Seaboard.

US marine fisheries management between 1976 and 1996 was generally not a success. By 1993, 40% of the stocks with known status were overfished (NOAA 1993). The dominant

[1]To maintain state sovereignty, technically only the Council FMP is approved by the Secretary. What the Secretary legally approves on the state side are the Commission's annual management measures derived from the FMP.

explanation given by observers was that the council system, and a history of close NMFS - industry cooperation, has put the 'foxes in charge of the hen house' (e.g., Safina 1994). In October of 1996 a reauthorization and amendment of the Magnuson Act was accomplished through the Magnuson-Stevens Fishery Conservation and Management Act (MSFCMA) and the Sustainable Fisheries Act (SFA).

These acts hoped to address the 'foxes' problem by both strengthening NMFS's powers vis-a-vis the Regional Councils and by more precisely defining the 10 National Standards that all federal FMPs must meet to be accepted by the Secretary of Commerce. The first two National Standards read: (1) "Conservation and management measures shall prevent overfishing while achieving, on a continuing basis, the optimum yield from each fishery for the United States fishing industry"; and (2) "Conservation and management measures shall be based upon the best scientific information available." The acts also contain the following language: "Any fishery management plan shall... specify objective and measurable criteria for identifying when the fishery to which the plan applies is overfished...[and if it is overfished]... contain conservation and management measures to prevent overfishing or end overfishing and rebuild the fishery."

During 1997 NMFS developed a set of guidelines for implementing the language of the new acts. One of the most important guidelines related what it meant to "specify objective and measurable criteria" for overfishing. These guidelines allow an overfishing definition to contain either a maximum rate of fishing mortality (F) or a minimum acceptable stock size (B). In practice, however, for many FMPs including bluefish, both of these components of the overfishing definition are required (MAFMC and ASMFC 1998). This language also makes the creation of an FMP for an overfished stock a legal requirement and this FMP must rebuild the stock to the minimum acceptable stock size (or often to a somewhat higher target value) within ten years. If a Regional Council fails to produce an FMP acceptable to the Secretary of Commerce NMFS now has the authority to impose its own FMP.

The main actors in bluefish decision making are the state directors who dominate the actual voting, particularly on the Bluefish Board, and the Northeast Regional Director (RD) of NMFS. This is the person whose evaluation of the FMP will most likely determine its acceptance or rejection by the Secretary of Commerce. All of these actors (with the exception of the Virginia state director) can call upon his or her agency's own staff of fisheries scientists. The Council and Commission also have fisheries scientists on their staffs, although their role on the Commission is mainly to coordinate activities among state scientists.

Although there are women who play prominent roles, a very large majority of the participants in fisheries management in the Northeast Region are white men between the ages of 30 and 60. Where commercial interest are prominent the number of women involved increases somewhat as commercial fishers' wives have traditionally played an important role in advocating for the industry. Few women seem to be involved in public roles in the recreational industry and women are heavily underrepresented in fisheries science. In our survey of US fisheries scientists we found only 18% were women.

## III. THE SCIENTIFIC INSTITUTIONS

Figure One is a schematic representation of the human ecology of the scientific institutions involved in bluefish management. Above the black line is the picture of the natural world that these institutions construct, below the black line is the social context in which the institutions operate. The upper left hand box represents the unknowable actually condition of the bluefish stock. The upper right hand box represents the legal condition of the bluefish stock that will be used as a basis for official decisions about bluefish management. Between these two are placed the major scientific controversies which had to be resolved if a link was to be built between the actual and legal conditions of the stock. Rather than 'resolved,' it would be more accurate to say that answers, surrounded as they were by multiple disclaimers, had to be given.

FIGURE ONE ABOUT HERE

The bottom half of Figure One depicts the major fisheries management groups and how they relate. On the right hand side is the Federal system made up of NMFS and the Councils. The arrow from NMFS to the legal condition of the bluefish stock box represents the Secretary of Commerce's final acceptance of the FMP, which amounts to a finding that it meets National Standard Two: "Conservation and management measures shall be based upon the best scientific information available." Arrows between boxes represent influence and /or authority. It is notable that the main entry points to the process from the fishing public are through the state level administrators or through the industry representatives on the Council, who are appointed at the state level. The arrows from the agencies to the fishing public represent the combined influence on fishing behavior of the perceived legitimacy of fisheries management, and the available surveillance and enforcement powers. The arrow going up the far left side represents the fact that the fishing behavior of the public is the only link from all of this back to the actual condition of the bluefish stock.

The most important institution represented on Figure One is the circular dashed line named the science boundary. Scientific "boundary work," i.e., negotiations over what is and is not science, has been recognized as least since Jasanoff (1990) as the key process involved in using science to give legitimacy to a policy. In bluefish management, as in other areas, this boundary line involves both a discourse about the meaning of "scientific" and a classification of people as being scientists or nonscientists. These roles were usually but not always based on expertise. For example, at least one industry representative on the council is a fisherman with extensive all-but-dissertation training in fisheries science. A Council staff scientist told me that they do not think of statements he made as being any different than they did those of other fishers on the council. This person's sometimes theatrical attempts to challenge official assessment science are routinely frustrated. While status within the scientific community depends on the respect of peers, becoming a "scientist," someone who can speak with authority about science, also often depends on being hired to play that role.

The states and the Commission, the Council and NMFS all have scientific staffs. Representatives of these staffs come together regularly in various fora, come to know each other and each others' work very well, and form a concrete scientific community, in the sense used by Barnes et

al. (1996). They share a culture that includes a sense of shared responsibility of fisheries management, understandings of leadership, and criteria for evaluation of scientific work.

State and federal scientists, however, have both expressed a perception of themselves as being in somewhat different camps. Part of this is that statistical stock assessment expertise is more concentrated among federal scientists. Both types of scientists also described differences in how they relate to the industry. They agree about the substance of these differences but with different interpretations: the federal scientists see the state scientists as overly influenced by the industry, in some cases to the point of scandal, while the state scientists see the federal scientists as uncomprehending of industry knowledge or concerns, in some cases to the point of arrogance. These perceptions should not be overblown as federal and state scientists often work smoothly together. Scientists working for the Council stand on a middle ground between the state scientists and the NMFS scientists who are based at the Northeast Fisheries Science Center (NEFSC) at Woods Hole, Massachusetts. Council scientists are federal scientists with a stock assessment expertise but they work for an agency that is more directly exposed to industry and state concerns.

Meetings of the ASMFC Bluefish Technical Committee (BTC) are the most important forum where these scientists interacted in this particular case study. Being nominally the Commission's committee did not really define this forum. The interactions in these meetings were between individuals from various agencies who fell within the science boundary and who, or more accurately whose bosses, were concerned about bluefish. Federal scientists from both Woods Hole and the Council were present at the meetings and assumed important roles, while official members of the committee who were nonscientists attended but remained quiet. The science boundary divided the BTC between, in the language of the scientists themselves, "real stock assessment scientists," and "regular biologists." The difference is expertise with neither biology nor bluefish, but with statistical modeling.

In Figure One the peer review process is placed on the intersection between the science boundary and the stock assessment model. Peer review in fisheries management takes several forms and is an evolving process that receives a lot of self-conscious concern within this scientific community (Anderson 1997). Stock Assessment Workshops (SAW) are open meetings that take place at Woods Hole. At these meetings federal, state and other scientists pull together the available data on a stock and perform the actual stock assessment. (The degree to which the BTC performed the bluefish assessment in this case was unusual.) The SAW's assessment is then peer reviewed by a Stock Assessment Review Committee (SARC), which is also an open meeting, that includes a broader group of fisheries scientists. The findings of the SARC are then presented in Public Review Workshops, which are basically informational and the one observed here did not involve any modifications of the material. NMFS and other fisheries administrators view basing an FMP on the findings of a SARC as the strongest foundation for certifying that the "best scientific information available" has been used.

The peer review process is perceived, by both the fishing public and the scientists, as the most important institution for drawing the science boundary around information. It is peer review that sets the stamp of "real" science. One of the most common concerns expressed by fisheries science administrators, both in interviews and in public, was the need to strengthen this process, which,

importantly, most often meant finding ways to bring in more scientists with fresher perspectives. These administrators see the evolution of the fisheries scientists into a tighter community and culture as containing a potential threat to their scientific credibility. A wide range of outsiders participating in peer review, in turn, provides external endorsements of the scientific nature of the results. The tension that must be held is that peer review also implies expertise in the species and, more important, methods being used.

## IV. SOME BACKGROUND ON BLUEFISH MANAGEMENT

Bluefish is one of two species, the other being American lobster (*Homerus americanus*), that have been particularly central in defining federal v. state roles in management (Hoover 1986). The reason is that a large portion of both of these fish are caught on both sides of the three mile state jurisdiction line. The initial impetus for managing bluefish was not a concern for the condition of the stock, which was abundant at the time, it was a concern by recreational fishers that a developing foreign market would mean an expanded commercial fishery (Hoover 1986). The first attempt to create a Bluefish Fisheries Management Plan (FMP) was rejected by the Secretary of Commerce in 1984. Several reasons were given and one critical one was the problem of jurisdiction; the plan dealt only with commercial fishing in federal waters and, hence, could not effectively address overfishing (Hoover 1986, MAFMC and ASMFC 1998). As a result, the Council joined with the Commission to create the joint bluefish FMP that was eventually adopted in 1990.

The bluefish FMP was born through an allocation dispute between commercial and recreational fishing interests and it has continued to be controversial. Organizations representing New York and New Jersey party and charter boats, i.e. small businesses that take anglers fishing, sued because they claimed that restrictions on the number of fish that could be taken (the bag limit) had been approved without adequate examination of the disproportionate impact these measures had on anglers from a lower social and economic status. Party boats take out large numbers (50+) of anglers who pay on an individual basis. They claimed that many of their customers, especially around New York, are poorer people who need to bring home fish to eat to justify the cost of the trip. This lawsuit was unsuccessful, but more than one observer interviewed for this research credited it with forcing the process to consider socioeconomic information more seriously.

The process of creating Amendment One of the bluefish FMP began when the Council adopted an initial scoping document in December of 1994 and ended in the fall of 1998 with the adoption of the Amendment. The scoping document was a response to an assessment of the condition of the bluefish stock that had been produced by the 18$^{th}$ Stock Assessment Workshop (SAW). SAW 18 had found: that the total biomass of the bluefish able to reproduce, the spawning stock biomass or SSB, had decreased 74% between 1982 and 1993; that "recruitment," the entry of new fish into the fishable stock had been generally low; and, that the fishing mortality rate (F) was at about twice the F at maximum sustainable yield (Fmsy), part of the designated definition of overfishing (NEFSC 1994).

The first two years of Amendment One process, 1995 and 1996, took place under the rules of the old, 1976 Magnuson Act. NMFS pushed hard for severe restrictions on the recreational bluefish

catch, the most onerous being cutting the bag limit by up to two thirds from 10 fish to three. Negative public reaction was intense and the Amendment failed to pass the Council. Under the old system, NMFS could not force the issue by threatening to impose its own FMP. In 1996 NMFS tried to reduce the federal role in Bluefish management by eliminating the Council's role. This was supposedly done as part of an effort to improve Federal government efficiency, but it was widely seen as an attempt to sidestep the recreational fishing industry's influence on the Council. Whatever the motives, the recreational industry was successful in using Congressional pressure to thwart the move.

This 1995-1996, pre-Magnuson-Stevens Act history is important for understanding the 1997-1998, post-Magnuson-Stevens Act events that are the subject of this case study. For one thing, by 1997 management administrators were beginning to feel embarrassed about the time it was taking to produce Amendment One. Several recreational leaders also expressed, both in interviews and at meetings, their perception that the NMFS leadership was still smarting from their earlier failure to restrict bluefish fishing and that now they intended to use bluefish as the test case for their new powers. While there is no direct evidence that this was ever the case, this perception colored the recreational industry's approach to the issues.

In the fall of 1996, the 23rd SARC (NEFSC 1997a) touched off the second round in the creation of Amendment One with a new bluefish stock assessment. They found bluefish to be "over exploited," a term that now triggered a legal requirement for a plan to reduce fishing effort. This finding contradicted a previous finding by the BTC that the bluefish stock was "fully exploited." This disagreement did not have its source in the slightly different input data the SARC reviewed. The real difference was that the SARC had decided that the benchmark F that the Council and Commission had been recommending in the current version of Amendment One was not appropriate. In other words, they moved the stock from fully to over exploited because they disagreed about the appropriate definition of over exploited, not because of a substantial disagreement about what the data they had was telling them about the amount of fishing pressure. The reason for the disagreement was that the SARC saw the decline of bluefish as being so precipitous that, while the definition that was in Amendment One may be appropriate to maintain the long term health of the stock, it was not stringent enough to ensure the recovery of the stock from its current low level. The SARC recommended that a level of F be implemented that was so close to zero that the statistical methods used to measure F could not distinguish it from zero. Implementing this F could conceivably shut down the fishery.

This stock assessment was greeted with widespread disbelief and anger. This was partly because changing the stock from fully to overexploited, especially with the legal ramifications of the change, was seen as high-handed. But there was also a strong sentiment that the SARC's decision was simply wrong and it had recommended drastic measures based on very shaky evidence. A Council administrator remarked at one meeting that he had never heard so many people, even people with no particular interests to defend, raising so many questions about a stock assessment as were being raised about SARC 23's assessment of bluefish.

While there were many issues and problems with the bluefish assessment one disagreement stood out. Many people involved in the bluefish fishery, including some scientists, believed that this

precipitous decline was actually a statistical illusion. They believed the numbers actually reflected, rather than a decline, a movement offshore of the bluefish stock - the bluefish were not declining in numbers, at least not seriously, they had moved away from where they had been caught in the past and the methods of catching them, both those of fishers and scientists doing surveys, had not followed them. All of these disagreements described below relate in some way to this central question of whether the bluefish had declined in number or moved offshore.

## V. DATA ISSUES

Uncertainty in fisheries science can arise in several ways. In some cases, American Lobster (*Homerus americanus*) is a good example, there may be a great deal of disagreement among scientists about the biology of the fish. In the case of bluefish, however, most of the uncertainty came from the quality of the data that the scientists had to evaluate. As will be clear below, problems with data quickly become more than just technical issues. Like all sources of uncertainty they can be quickly taken up in the overall management debate. Three data gathering problems in particular haunted the bluefish science: problems with aging, problems with the survey used for fisheries independent data, and problems with measuring fishing effort.

### Aging.

The problem of aging bluefish is one that received relatively little attention among nonscientists concerned with bluefish, but which the scientists considered very important. Time is the basic variable in the theory of population dynamics on which these models are based and time is reflected in the aging of the fish. Knowing the age of individual fish is critical because the more sophisticated ways of analyzing fish populations are based on tracing the history of "year classes, " which are cohorts of fish the same age. Fish born in 1991 begin as age 0 fish, then this age class becomes age 1 fish in 1992 (setting aside in this explanation when in the year they were actually born), age 2 fish in 1993 and so on. The models preferred by fisheries scientists trace these year classes through time. They look at the number of fish from a particular year class that have been caught each year. They estimate how many of them should have died from natural causes. Then they can specify, based on their previous knowledge of fish population dynamics, an equation that gives estimations of various "parameters," such as how many fish from that year class are in the ocean and how heavily they are being fished. When they have information on all the year classes for enough years they can calculate these equations simultaneously. When they do this, the information from the different years and different year classes work together to make estimates of the parameters for the whole stock more accurate. The year classes approach gives them different points of comparison within the single fish stock they are studying.

It takes some time to figure out how old a fish on a lab bench was when it died. It is done by looking at the fish's scales or its otoliths (hard formations found in fishes' inner ears) and counting lines like you would age a tree trunk. Because this process takes so long, and measuring a fish's length takes seconds, samples of fish are used to make "age-length keys" which give the probabilities of a fish being a particular age if it is a particular length. This way many more fish can have their age estimated from their length.

SARC 23 (NEFSC 1997a) used age-length keys for bluefish that came from the North Carolina Division of Marine Fisheries' analysis of fish caught in their winter commercial fishery. The SARC expressed concern about these keys being appropriate for recreational and commercial fisheries up an down the coast, but concluded that they were the best available approach. It did not raise any questions about the internal validity of the NC age-length keys. In February of 1997 when SARC 23 was presented to the Council in the form of a Public Review Workshop of SAW 23 only one person, a biologist and a state fisheries director asked about the aging issue. His focus was also on the applicability of the NC commercial data to other fisheries.

In August 1997 the Mid-Atlantic Fisheries Management Council received a letter from the South Atlantic Fisheries Management Council. Their Scientific and Statistical Committee (SSC) was concerned not about the overuse of the NC winter commercial aging data, but about the fact that this data had not been used for 1995; information from recreational catches (which of course are mainly in the summer) had been used instead. This concern stemmed from the fact that the fish caught in the winter commercial fishery were, on average, twice as big as the recreational fish. They also raised a new issue. The NC data was based on scales and not on otoliths, which their SSC feared might lead to inaccuracies of as much as three years.

The North Carolina Division of Marine Fisheries addressed this later concern with a study of bluefish aging techniques. They attempted to gather bluefish scales and otolith samples from around the coast, but only Northern states provided the samples. They then did a study of the reliability of three aging techniques, scales, whole otoliths, and otoliths divided into sections. They reported their findings at the BTC meeting in February 1998, a meeting at which Woods Hole was not represented. Whole otoliths are not reliable for aging fish beyond age three. While there is a lot of variability between otoliths and scales there is not a "tremendous difference" until age six. After age six aging a bluefish is a "crap shoot" and it is likely that many ages in the past had been assigned by guess. "Scales were extraordinarily difficult to read.. [even] when they got a good [otolith] section, which was rare in a larger, older fish, it was very very difficult to determine [age]." The equipment they were using to section the otoliths was state of the art and worked fine for every other species. The problem is the internal structure of the otoliths in bluefish. The reporter said that he had called around to other scientists who used different equipment and found that they had not had success with bluefish either. The upshot of the report was that he recommended that when analyzing the bluefish stock, fish over age six should be lumped together into a "six plus" age category. The use of such plus groups in these models is standard practice, however the question of the age at which the plus group will be set is an important one.

In spite of the gravity of this information, people at the February meeting simply asked a couple of informational questions and moved on. At the March 1998 BTC meeting, however, Woods Hole was represented by a federal scientist deeply involved in assessing bluefish. Almost immediately, as the minutes for the February meeting were being read for acceptance, he began to object in a sarcastic tone to the "unsubstantiated rumors that you can't age fish " based on the NC report that he had not yet seen in writing. "These minutes say that you can't age bluefish beyond 6+," he said "beyond these

minutes I don't know anything to back that up." In a tense moment in the meeting, the state scientists reacted defensively that nothing in the minutes should be contrued as an endorsement of the NC presentation. The federal scientist insisted that this be made clear because he "does not want to get blindsided by this stuff." He is very concerned because this issue of six year and older fish is critical to the assessment and they must have an aging workshop to address the issue.

Later in the meeting he had a chance to make an extended presentation of why he was so concerned. VPAs, he began, (a stock model of the type described in the beginning of this section) are done through back calculation from the oldest year class to the next oldest and so on to the youngest. At each step you must allow for natural mortality, i.e. fish that die for other reasons than fishing. He then presented two bluefish models. One was based on nine + as the oldest year class, which is how they did the assessment at SAW 23, and the other on six +, the implicit recommendation of the North Carolina report. The nine+ gave an initial stock size in 1982 of 379,000 tones and a 1996 size of 158,000 tones. The six+ model had a different result. Now with the six+ model "you have to kill them off quicker to get to age 0 .....so what I have done by one simple manipulation of the input data, based on thoughts you might have on how you can reliably age these fish, I have decreased the stock size by half in 1982 and by a factor of four in 1996."

To understand the implications of what this scientist said, it is important to remember that the new law required that a minimum stock size be established for bluefish, and that management measures must rebuild to that level, whatever it is, in ten years.

**Survey Catchability.**

Two basic types of catch data are involved in marine stock assessment, information about the catches of fishers and "fisheries independent" data that comes from surveys by management agencies. Fisheries independent data is a critical source of information because the same gear is used in the same place year after year, effort, i.e. the amount of time that the gear spends in the water, can be accurately measured, and the hauls are placed across the ocean according to a deliberate, mathematically designed plan. SARC 23 (NFSC 1997a) examined nine sources of fisheries independent data. Eight of these were surveys done by state agencies in their state's waters, the ninth, and by far the most geographically comprehensive, was the Northeast Fisheries Science Center's fall survey.

The NEFSC does two fisheries surveys every year over a large patch of ocean between Cape Hatteras, NC and Canada. The most important commercial fish species in the Northeast Region are groundfish that spend their relatively stationary lives on the bottom of the ocean. Because of this, the NEFSC surveys are done by pulling a trawl net along the bottom. Bluefish are a migratory, pelagic species that spend most of their lives swimming quickly through the water well above the bottom. Bluefish are not easily caught by bottom trawl gear and this fact has led members of the fishing public to argue that the survey data on bluefish systematically underestimates the bluefish stock. The catches of bluefish in the spring survey are so sporadic that it is not even considered in evaluating the stock. Data from the fall survey, however, is used in evaluating the stock.

The survey issue is not a simple one. Scientists point out that as the poor catchability of bluefish in the trawl nets is not in and of itself reason to dismiss the data. As long as the same gear is always used the same way the results are valid, and usable if the variance in the catch is not too high. Of course, too high is a judgement call and an important one in the bluefish assessment. This argument also assumes that the bluefish are distributed in the water column in such a way that the fraction of the population exposed to the gear remains constant.

In relation to bluefish, much of the fishing public perceives these surveys as completely inadequate and many of them do so for sophisticated reasons. All of the interested public, for example, seem to be aware of the problem of the bluefish being too high in the water column to be caught. In July of 1997, leaders from recreational and commercial bluefish interests from one important bluefish state, including three Council members from the "industry representatives" category, met with scientists from their state to discuss how to respond to SARC 23. The content of the meeting was a review of the scientific arguments involved and a marshalling of counter arguments. Many were offered. One raised the point that the bluefish swim faster than the survey trawls move through the water. A lawyer suggested that the surveys assume greater constistency in fish behavior than was warranted. Another criticism voiced by a council member was that the survey is done backwards in terms of the movement of bluefish, going north to south while the bluefish were moving south to north.

Some of these issues were echoed in the two meetings of the ASMFC Bluefish Technical Committee (BTC) observed for this case study. These scientists also criticized the survey on the basis of its being on the bottom and too slow to catch bluefish. As the federal scientist put it: "First of all, our trawl gear can't catch big bluefish unless it runs into them when we are hauling back because the damn things can out swim a trawl that is only this far [holds hand out] off the bottom and 2) out on George's Bank or out on most of the shelf the trawl is down here and the bluefish are some 50 fathoms up there." His conclusion: "we need a bluefish survey, that is what we need."

The BTC scientists were faced with several statistical problems resulting from the survey design. The fall survey is divided into geographical areas and an important distinction is between the inshore areas and offshore areas. Inshore, the survey catches on the order of fifty times as many fish as in the offshore areas. This huge difference comes almost entirely from the number of young, age 0 and 1 fish, that the inshore survey catches. In the offshore survey the ages in the catch are fairly evenly spread. Scientists on the BTC were very concerned about the quality of the survey data and particularly the quality of the offshore survey which caught few fish and had many hauls that caught no bluefish at all and many others with only one. With the majority of hauls showing either 0 and 1 fish the assumption it becomes very difficult to arrive a statistically valid conclusions.

## Effort Measurement

Perhaps the most basic, common sense indicator of the size of a fish stock is how easy it is to catch a fish - the easier they are to catch the more fish there must be. Fisheries science calls this catch per unit effort (CPUE) and it is found by dividing the catch by some measure of fishing effort.

From the perspective of who raises the issue and how often, the question of effort measurement is like a mirror image of the question of aging. Members of the bluefish fishing public are very concerned about how fishing effort is handled in assessments, the issue was raised in nearly every interview with fishers in which bluefish were discussed. Many see a failure to understand changes in effort as the underlying reason for the perceived inaccuracy of the assessments. Sitting in the recreational fishing communities they have seen the degree to which anglers have shifted from catching bluefish in the 1980s to catching striped bass in the 1990s. They believe that this drop in effort is a major reason for the drop in catch and do not believe that this is adequately considered in the scientists' models.

Most of the scientists acknowledge some problems with effort measurement but feel that the models they are using handle it adequately. They see other issues as more pressing. The model that SARC 23 uses is based primarily on the relative number of fish caught in the various year classes but a indicator of recreational CPUE is used based on the Marine Recreational Fishery Statistics Survey (MRFSS). This survey includes both intercepts of anglers returning from fishing and a general population survey of coastal states that gathers information about catches. SARC 23 (NFSC 1997a) acknowledges that the intercept survey does not measure the lengths of an adequate sample of fish. As their measure of effort, SARC 23 uses the number of "trips that caught bluefish plus trips in which bluefish was the target species and in which some fish (of any species) were caught" (NSFC 1997 p 156).

At the Public Review Workshop for SARC 23 before the Council in February of 1997 the following exchange took place.

> **State scientist**: The last issue I have deals with effort. There are no tables here dealing with fishing effort particularly on the recreational side and I'm just curious if you considered trying to use that or use boat trips as some surrogate for fishing effort.
>
> **Federal scientist**: Actually it doesn't show up in this one but in the last assessment we went through what I would characterize as extensive discussions on effort. Looking at various ways to appropriately use the recreational data and the commercial data, we looked at that in a really hard way, there was no real modification done at this time so that's why. ....Actually I think we're using an index from the recreational survey in our tuning of the VPA, but what I said before is true, we didn't do anything new there, we had done those extensive analyses in the last go round so we are sort of using that up to the present.
>
> **Council member:** Let me see if I understand what you are saying. In response to the question I asked earlier, you pointed out that it was something new that happened since the last SAW that helped you fine tune this assessment. Are you now saying that you did not take into consideration in that same time period anything that changed in the recreational fishing performance? No, but we all know that that has changed significantly with respect to the targeting of bluefish and the lack thereof.

**Federal Scientist:** I just answered your question. No, we have taken into account the additional information, the additional years, the rec index that we used again from these extensive analyses we did in the last assessment. We used that again, and added the new information to it.

**Council member:** Do you see a dramatic downturn in the participation in bluefish fishing recreationally? Any downturn? Do the lower catches, in fact, addressing [the state scientist's] point, do the lower catches in any way reflect less participation?

**Federal Scientist**: That's hard to say. We've looked at the effort data. I mean, I think probably, yes, that's true.... yes the recreational catch has declined. It's possibly due to less participation, we haven't quantified that to a great degree.

**Council member:** Okay, explain something to me, to the extent then that you would use lower catches to help drive some of the conclusions about SSB, how then can you use that number if it isn't in some way fine tuned with effort? It suggests to me from what you are saying, that the effort isn't as important in the calculation as the use of the number itself.

**Federal scientist:** I'm sorry if we got cross wired here. We're using an effort index to tune and we've incorporated the new information that is available up to the present through 1995 which is the terminal year of this assessment.

At first the Council member thought that the scientist was saying that they had not updated the effort data itself, when he had meant that they had not changed the way that data was modeled. This is perhaps because the Council member was focused on the question of whether or not the change in effort was being considered. The federal scientist's need to double check the answer, implies that he was not aware before this interchange the degree to which the fishing public was concerned that changes in effort were being ignored. The opaque language he choose to use in his last remark suggests that he still may not have sensed the degree of concern.

In addition to the fear that changes in effort are being ignored, members of the fishing public have often expressed three other criticisms of the use of the MRFSS to measure effort. The first is that the general telephone survey does not cover Pennsylvania, and a great many of the customers on party and charter boats come from the Philadelphia area. The second is that the intercept survey work is done only during the day and a lot of bluefish fishing is done at night. They also believe, and I have heard a scientist express this belief as well, that the CPUE is higher at night than it is during the day. These things are seen to be correlated as night anglers are often from Philadelphia.

The third criticism is that the growing number of anglers who catch and release their bluefish rather than keeping them is not considered. As one sports fishing organization put it the "methodology becomes even less dependable when you consider the recreational community has, in most recent years, been releasing the majority of its catch. This brings into question the use of recreational "landings" and recreational "catch" in the assessment. It almost appears the two are interchangeable in places when in actuality, the figures are different by orders of magnitude" (JCAA 1998). This criticism does not accurately apply to either the SARC 23 assessment (NFSC 1997a) nor the stock assessment finally adopted as an appendix to Amendment One at the end of the case study period (Gibson and Lazar 1998). Both of these explicitly considered the release of fish and used figures that reflected the increasing trend toward catch and release.

Recreational effort measurement was addressed by both the SARC and the BTC as a serious technical issue, especially once larger issues of model and raw data selection were becoming clarified. In the end, the BTC choose to use a different definition of effort than the one in SARC 23. Rather than "trips that caught bluefish plus trips in which bluefish was the target species and in which some fish (of any species) were caught" (NSFC 1997:156) they chose to use "fishing trips from Maine to Florida where bluefish were indicated as the primary species" (Gibson and Lazar 1998:3). The type of model they chose to use relies even more heavily on scaling input data by effort than does the one used by the SARC. The BTC considered other definitions of effort, such as any fishing trip in areas where catching bluefish was possible. In the end they tried several different definitions and found that changing these effort definitions did not have a "critical impact" on the biological parameter outcomes.

**Fishers' Observations**

Atlantic Seaboard fishers believe that they can make a real contribution to the pool of information that managers can use in making decisions about bluefish. In interviews they pointed to their knowledge about how different combinations of changes effect the bluefish stock, and particularly tracing the movements of the fish. One example they offered were summers when the water was hot and there were no sand eels around, when this happened the bluefish were less abundant in New Jersey but lobstermen in the Gulf of Maine were telling them that bluefish were chewing the lobster pots to get to bait. Examples they gave of their knowledge of bluefish involved both what they were seeing and what they were hearing from different kinds of fishers around the coast. The observations they found important involved of the behavior of both bluefish and specific species that the fishers link with bluefish, i.e., striped bass, menhaden, and sand eels. These behaviors were most often seen as driven by environmental changes, particularly water temperature. They would patch what they were seeing and hearing together into a single picture of the stock's movement. What emerged from these discussions were not simply "anecdotal data," as these observations are often described, but "anecdotal hypotheses" about what is happening to the stock and what the important driver of these changes might be. None of the fisheries scientists interviewed, nor the fisheries scientist who accompanied us to some of these discussions with industry members, found these not yet systematically tested hypotheses, unreasonable. For what was happening in the mid-1990s, the hypotheses took various forms but all suggested that the bluefish had moved offshore.

Fisheries scientists that we spoke with, to a person, agree in principal that using fishers' observations to improve stock assessment would be a good thing. Beyond that there is little agreement about what it would actually mean to use such data. Many of them say that they rely on information from the fishing industry. NMFS has said, for example, in an official response to public comments on the summer flounder plan that "NMFS must rely on the industry and other sources to procure accurate catch information associated with mesh size." A federal scientist associated with bluefish management said in an interview that he "challenges" the captains to give him data that he can use, he would have it gladly. These statements indicate a willingness to make use of the data, but are empty in the sense that the real problems of gathering and analyzing the data are not addressed.

The problem, especially for NMFS, is one of scale. It includes both the logistical issue of processing detailed information from across the breadth of the Northeast Region, and the conceptual problem of translating local observations into meaningful information at a larger scale. It is very difficult to develop ways to use fishers' observations systematically to aid stock assessments. There is just so much of it. They are working on developing ways to record fishers' experiences and this has not proved easy. An attempt to use log book information to aid the bluefish assessment from party and charter boats broke down mainly because the amount of information was so great that they did not have the people to do data entry in order to use the data in a timely fashion. This led to anger and resentment by the people who took the time to provide the data.

This interpretation is born out by some of the experiences at the state level, where more opportunities exists for working with fishers to address local research problems and to work together in joint research efforts that involved more than just using fishers' ad hoc observations. A scientist who works for a very small state said that you can't do fisheries science without industry participation, the industry are the best source of the best data. They have managed to maintain good relations to the point where they are often invited by fishers who want to show them something. Examples of the many research questions that states have addressed along with fishers include: looking at the effects of types and sizes of fishing gear on the fish, such as the effects on the fish of passing through nets with different mesh sizes; measuring actual discard rates; and a study of hook and line mortality, critical for understanding the effects of catch and release fishing, and how this related to the handling of the fish and the hooks. These sorts of research are important for refining stock assessments, but they do not address the central question of counting fish over large areas.

Both fishers and scientists learn in these small scale interactions. The scientist mentioned in the last paragraph says that many fishers have a hard time understanding that the distribution of fish of different ages in a stock is as important as its overall size. They are not comfortable with random designs, the criticism that "scientists don't tow where the fish are" is often repeated, and not just in this scientist's state. In another state a scientist has been working for three or four years on a state management board for an inshore species. The board is made up mostly of fishers. He reports that they began with a good intuitive knowledge of the ecology of the fish and that there has been a quick learning process. The fishers are now very comfortable with using scientific reasoning in management and building mechanisms for generating research-based information into the management measures.

For the larger scale problem of an overall stock assessment the use of fishers' observations is a charged issue. At the special bluefish meeting, in response to a council member's raising the question of the degree to which fishers' observation did not jive with the SARC 23 assessment, the Regional Director (RD) of NMFS began by pointing out that anecdotal information is very difficult to use because fishers' observations in one place often don't agree with observations in other places. Then he said the following:

> Anecdotal is not a perjorative description, neither is analytical, although people are very happy to throw rocks at the analysis and are offended if people say 'that is anecdotal.' That seems to me to be silly. Nobody ignores anecdotal information.....Anecdotal information is used in the way that you can use anecdotal information, the same with analytical information. There is nothing perjorative about it.... I have

> never felt that it is not used in the assessment. It is used in the analysis when people are examining tuning indices and trying to explain why certain things occur in diagnostics. That is exactly what they used. It is used extensively.

Two things are interesting in the RD's statement. The first is the defensiveness, he wanted people to understand that this is not some bias he or anyone else at NMFS has against the knowledge of fishers. The second is the assertion that fishers' observations are used as background information in putting together an assessment model. In this study, the BTC's had exactly the kind of discussion that the RD was talking about. The analysis of this discussion, reported below, shows that the RD's assertion pointed at the heart of this particular assessment process and was actually much more complicated statement than it appears to be on the surface. The RD's statement does reflect how NMFS handles questions of fishers' observations. The presenters at the SARC 23 Public Review Workshop, and the SARC 23 document itself (NFSC 1997a), both respond to the questions of offshore migration and predator prey interactions with "you're right and we looked at that question and additional work is needed."

## VI. COMPETING EXPLANATIONS OF THE STOCK CONDITION

During the case study period, three major "explanations" of what the data were showing about the stock were most commonly drawn upon in the debates over bluefish management. These three explanations were environmental factors, offshore displacement, and fishing pressure. No one claimed that the explanation they advocated was the only valid one, but there were clear differences in emphasis and in implications for management decisions. The predator-prey explanation did not empirically contradict either of the other explanations. Furthermore, both the predator-prey and fishing pressure explanations reflect different basic ideas about how fisheries management should be done. The offshore displacement explanation, on the other hand, was entirely empirical, and not an expression of a different approach to management. It did, however, directly challenge the overfishing explanation, because the more that the evidence of a decline in bluefish could be accounted for by offshore displacement, the weaker became the support for the overfishing explanation.

### Environmental Factors.

The issue of environmental factors such as habitat damage and predator - prey interactions played a relatively minor role in management debate (in this case study) compared to the offshore displacement and fishing pressure explanations. This explanation ties into a broader debate in resource management that contrasts "ecosystem management" with "single species" management (Grumbine 1994). The perceived need to manage fisheries as part of a broader ecosystem is intuitively appealing, especially to fishers. It resonates with their common argument for focusing more on non-fishing related causes of declines in fish stocks. As discussed above, the idea of an "ecosystem approach" fits more comfortably with the kind of information their own observations generate about what is happening in the ocean than single species, statistical approaches (Wilson and Dickie 1995). Industry critics in popular articles and at management meetings sometimes linked the advocacy of an ecosystem approach with criticism of stock assessment as "math problems" and fisheries management as based on statistics rather than biology.

In interviews, several academic fisheries scientists were proponents of the ecosystem approach and no scientist denied it theoretical validity. There was skepticism about its practical relevance for management. It was also pointed out that the degree to which a fish species is tightly linked to other species varies, so the implications considering predator-prey interactions in management will depend on the species being managed.

In April of 1996 the Subcommittee on Fisheries, Wildlife, and Oceans of the House of Representatives Resources Committee held a hearing on the decline in bluefish. At that hearing the NMFS representative attributed the decline in the stock to overfishing while other witnesses, including the Council, the Commission, and angler groups emphasized environmental factors (NOAA 1996), particularly a decline in bluefish prey species (JCAA 1996). Later the same year NMFS produced a very short paper relating environmental variables to bluefish abundance (Terceiro 1996), which was later incorporated as a SAW 23 working paper. The paper found that many environmental variables do indeed correlate with bluefish abundance. The paper does not purport to test any theories about which such relationships might be the most consequential, after reporting the correlations it merely points out that many of them may be spurious and that models will have to be developed. The main purpose of the paper seems to be to demonstrate the complexity of the question.

During the Public Review Workshop for SARC 23 the following statement was made by a fisher who sits on the Council. It is one he repeated at the special bluefish meeting and it is representative of a commonly heard criticisms of management.

> We're protecting all of the predators, fluke, striped bass are recovered, we're protecting the weakfish, we're protecting all of the predators, how about everything else? We're trying to protect the butterfish, the squid, everything in the ocean, but there has got to be a natural balance somewhere.

A few minutes later another council member...

> One of the things that we've discussed in the few years that I've been on Council, and in other meetings prior to being on Council, was a greater understanding of the migratory patterns and the relationships to ocean conditions and water temperatures and bait [fishers often refer to prey species as "bait"] availability for this species. Has there been any additional work done on that, because it could be a significant contributing factor to the presence in our waters.

To whom one of the NMFS scientists responded..

> We did look at a paper that suggested that perhaps there has been some movement of larger fish offshore... It's weak evidence at this point...The other paper we looked at was one where we had a correlation matrix with about 25 environmental and biotic variables reviewed that paper [note: this is the paper mentioned above] and, of course, the problem with a correlation analysis is that you don't know which ones are spurious, particularly when you have 25 variables. So, the recommendation as a subcommittee and in the SARC level was, in fact, to do more work in that area and we gave some specific recommendations about how to analyze that data and how to look at it. ... Some factors that you would think would be highly influential had the wrong sign....Well, you've got to go much beyond that in a scientific sense to really prove cause and effect. Right now, it's extremely weak evidence, if any at all, in terms of the correlations between bluefish abundance and biotic and abiotic factors. So, that's about where we are at this point

The weakness of the evidence, of course, is not weakness in the sense that it fails to show that predator - prey interactions are important. The problem is that there are so many possible paths to

account for the correlations in the paper, a model has not been constructed of how these interactions are operating.

Another industry representative at the same meeting...

> ..that seems to be the approach sometimes, that we want an ocean full of bluefish and we want an ocean full of striped bass and those things may simply not happen. The problem is, in the process, what [The NMFS scientists] said is correct, the fishing pressure is the only thing that anybody can do anything about, but that makes the fishing industry the lowest common denominator in that attempt to maximize simultaneously or multiple variables. I disagree with the idea that fishing is the only thing that you can do anything about because certainly one of the greatest benefits to the bluefish resource might very well be to reestablish a greater commercial fishery on striped bass.

This statement shows another important aspect of the political complexities of considering species interactions, because the appropriate extent of the commercial fishery on striped bass is a loaded question from the perspective of allocating the stock between the commercial and recreational sectors. A mirror of this issue is raised by many people concerning bluefish in the recreational community, they argue that a cause of the decline of bluefish is commercial fishing on prey species, particularly menhaden. The reduction or elimination of the menhaden fishery has been a priority of several recreational fishing groups.

As a result of both the manifest unclarity around the predator-prey interactions and the Congressional interest in the subject a special appropriation $800,000 was made to study interactions between bluefish, striped bass, and their prey species. Part of this money was used by NMFS to study striped bass and the rest was sent to Rutgers University to administer the research on bluefish. This resulted in workshops and a $350,000 being made available to scientists to submit proposals to study some aspect of the general problem. There was opposition to this use of the funds from people who wanted it to be used more directly to improve the assessment. The program is still going on and has funded a number of studies of poorly understood aspects of juvenile bluefish life history, habitat, and interactions with striped bass and prey species. In 2000 a specific call for stock assessment research was added to the request for proposals.

**Displacement**

Among fishers the idea that the bluefish had moved offshore was close to a consensus. I never heard a fisher deny it. Longliners who targeted swordfish told me that bluefish in the 1990s were stealing their bait much more than in the past. Others told me that they had heard the same thing from tilefish and wreckfish fishers who fish in deep water canyons. This was the typical nature of the information. One fisher's observation was reinforced by that of another until a picture of the position of the resource was built up that was entirely coherent, in the sense of being an internally consistent explanation, but not systematic. Thus a consensus emerged based on a great deal of information, but in a way in which information challenging that consensus could easily have been dismissed as not fitting the "common sense."

A few typical statements. The first, from February 1997, is from a recreational fishing activist who served as chair of an advisory panel for another species and later became a member of the Commission.

> I talk to offshore guys who see huge schools of large bluefish 60-100 miles offshore. The assessments we are getting are based on looking in the same places they always looked. These assessments say we are in deep trouble. I think we are but not to the degree that they are stating.

A recent (2000) communication from a recreational fisher and council member, referring to the case study years.

> One trip I was on saw our lures and baits get harassed by a large, well spread out school of five pound bluefish over 120 miles offshore! These encounters with bluefish offshore, while not unheard of were certainly not common in prior years. On one fishing trip, we came upon a massive school of bluefish at the tip of the Hudson Canyon, 76 miles offshore. The school stretched for miles and was right on the surface.

At the bluefish industry leaders strategy meeting in July of 1997:

> Council member: The 1996 year class was weak according to NMFS. Surveys are done in estuaries while the bluefish are offshore. Offshore the bluefish are giving fishermen a hard time but they are not seen inshore. Another participant: They are not inshore down south.... Commercial Bluefish fisher: We are seeing half pound fish 10 miles offshore. Council Member: cold water runs down the coast along the beach to 12-13 miles off and bluefish don't like this.

The inshore water temperature theory was repeated by several fishers in different contexts. Water temperatures figures frequently in fisher's observations. One commercial "blue water" fisher, meaning someone who works far offshore, told me that he basically sees the ocean as a patchwork of different temperatures. The importance of temperature is also reflected in the business of the Council member, the same person who supplied the quote above about Hudson Canyon, who said the following at the Bluefish Monitoring Committee meeting in August 1997:

> To add a little anecdotal information onto the record...one of the businesses I am involved in provides satellite temperature chart service to recreational fishermen from New England through North Carolina. As part of that service people that receive our charts phone in fishing reports for their trips. They call them in to any one of six reporters who work exclusively for us. And that information is provided back in weekly reports to our customers up and down the coast. For the week of July 9th to July 15th I went back and read through the reports that were broken down by (50,000 reports) inside the 50 fathom curve areas and also by the canyons. What I found didn't surprise me but it might shed some light on the offshore distribution in these fish. I broke these down into canyon areas, and remember, keep in mind these are people who are not fishing for bluefish. They are fishing for yellowfin tuna...[tape is turned over]... There were bluefish harassing and believe me some of the reports said they could not get away from them. Bluefin tuna fishermen off Montauk, off Shinecock Inlet 45 miles, [the outer tip of Long Island] the sea buoy to the dip off Mariches [?] Inlet about 50 miles, the Texas Tower and Triple Wrecks area that's 50 -60 mile off NJ. The Slough Area, Little Italy, those areas are 16-28 miles off New Jersey, the Chicken Canyon, the Hambone and Sausage Lumps off of Delaware, the Fingers off Indian River, and 28 Mile Hill off of the Delaware-Maryland area. Those are all in one week period. That is a lot of bluefish covering a lot of area offshore.

In spite of the number and coherence of the fishers' observations, some scientists did not find these arguments convincing. The following quote is a response to an exaggerated version of the displacement hypothesis. An academic fisheries scientist, and a recognized expert in bluefish, at the

Congressional Hearing in April 1997 responding to the testimony of a recreational fisher who suggested that some East Coast bluefish had even gone to the Azores :

> They're fairly mobile, but those occurring on the east coast undergo more or less regular migrations up and down the east coast. The likelihood that bluefish from the east coast shore showing up in the Azores is not very likely in my estimation. They have regular migrations. The timing of those migrations are very, very, variable. They're often temperature-dependent, but there are other things going on that we don't quite understand, so that I'd say on the east coast we know with some reasonable confidence what the migrations of the adults are like

The quote demonstrates considerable confidence in scientific understanding of the migration of bluefish. The displacement hypothesis, however, does not depend on the bluefish having moved very far offshore. Three quarters of the recreational bluefish landings come from state waters within three miles of shore (MAFMC and ASMFC 1998).

The response from one federal scientist when displacement was raised at the Public Review Workshop for SARC 23 was a follows:

> Actually in the subcommittee we did address some additional work that has been done in both of those areas [predator-prey and displacement]. We did look at a paper that suggested that perhaps there has been some movement of larger fish offshore, so that, in some way, addresses the availability thing you are talking about. It's weak evidence at this point, but we looked at it.

The paper he referred to was written by one state scientist who was convinced that the displacement hypothesis had merit. The paper (Crecco 1996) is cited in SARC 23 as "there is some evidence in the pattern of commercial landings and effort that adult bluefish have been displaced further offshore in recent years"(NFSC 1997a:161). That document did not use the word "weak" to describe the evidence. The paper argued that there had been a gradual shift, beginning in 1988 in commercial bluefish catches from state to federal waters, suggesting a shift in the stock. In addition the offshore strata in the NMFS survey between 1980 and 1995 did not show a decline in adult bluefish while the inshore strata did. In a memo to the BTC in October of 1997 this scientist characterized this evidence as "strongly" supporting the displacement hypothesis that itself had a "strong possibility" of being true.

The federal scientist at the Public Review Workshop referred again to the displacement hypothesis a little while later in answer to another question about the "dome shaped partial recruitment vector," a statistical pattern showing that there were fewer older bluefish in the data than should be expected.

> In this case, since we have this funny fishing pattern, which we would characterize as a dome shape where the older ages aren't recruited to the extent that the age ones are... The reason for using this pattern is because it has repeatedly shown up in the analysis that the Committee has done, it's not the usual one and in fact, most of the time you have a hard time justifying using this kind of relationship, but in this particular case we reviewed the evidence, both in terms of the analysis that you normally use to look at the fishing pattern. Also, as I mentioned before there is some albeit weak evidence that larger fish are moving offshore and they may not be available, that would be a plausible mechanism why you would have a dome shaped curve.

So while this scientist continues to characterizes the evidence for the displacement hypotheses as weak, it is the only explanation he offers for why the "funny fishing pattern" keeps showing up in the data so often that they feel they have to go ahead and use it in the assessment. In an interview, another Federal

scientist explained that the dome shaped pattern of age and vulnerability to fishing found in bluefish contrasts with the more common pattern in which the older a fish is the more vulnerable it is to being caught. For bluefish year one fish are the most vulnerable to being caught. His explanation for the dome shape was partly the fact that they are targeted by gill nets and hooks, and partly because the larger fish are further offshore. He believes the Connecticut survey produces the best data because the larger fish go into Long Island Sound in the summer. Fishing gear was the reason for the dome shape offered by SARC 23.

**Fishing Pressure**

The third explanation given for the stock condition was that it was being overfished. SARC 23 concluded that "fishing mortality has exceeded the current biological reference point of Fmsy = 0.20 since 1991, and SSB in 1995 was at an historic low....Fishing mortality should be reduced to 0.1 (8% exploitation) or below to halt the decline in SSB"(NFSC 1997b:24). The evidence presented was an "integrated catch analysis" (ICA), a year-class based model of the type described above in the section on aging. They used nine "true age" classes, ages zero through eight and a nine+ class in which were placed all fish aged nine and older. The age classes were based on length to age keys that were, in turn, based on the North Carolina commercial fishery. The data used in the model came from the North Carolina commercial fishery, which catches about a third of the commercial landings, MRFSS survey measures of recreational catch and fishing effort, and six fisheries independent data sources. These later sources were five surveys done by state fisheries agencies and the inshore strata of the NEFSC Fall survey. Data from three other state surveys were tried and discarded as "not useful." Data from the offshore strata of the NEFSC were not evaluated for inclusion because they were not yet ready. Some tagging data from an environmental group was also reviewed and considered too preliminary to be included.

Critical reactions to these findings mainly took the form of highlighting the problems with the data discussed above. The varied reactions of other scientists to the model also focused on the data problems, particularly the aging issue. In the face of both serious questions about the data coming from the scientific community, and the widespread belief within all sectors of the fishing community that the bluefish had not declined but had moved offshore, agreement was reached that the BTC should try to create an alternative to the SARC 23 findings.

During this year and a half period between SARC 23 and the creation of alternative findings by the BTC, NMFS put up a sprightly defense of the SARC findings and the need to take the management actions that they called for. Early on, NMFS cast the disagreements with the stock assessment as coming from "the sentiments" of people who were not willing to face reality. At the council meeting that followed the Public Review Workshop in February 1997 for SARC 23 the following exchange occurred:

> Council Member (an academic): I believe that when this and these additional analyses go out to public hearing, there will be tremendous discussion, tremendous public concern on what's occurring here, and perhaps once that public comment is completed, this Council may wish to take very careful note of that comment and reconsider some of the actions that it's taking.

> NMFS Regional Director: This has come up a number of times now about the concerns that the public will have ...Clearly, it will be controversial, but sometimes the need to do such things is controversial, the issue is what is needed to try to rebuild this stock. So, I think that regardless of the public outcry, sentiment and so on, it is important to be clear that that is what is apparently needed.

At the industry strategy meeting in July of 1997 a Council member, active in the recreational community, reported that he thought that the RD was softening his stance on SARC 23 because of the amount of anecdotal information, from all over the coast, that supported the displacement hypothesis. But even in October of 1997, when the Council administrator made his statement at the special bluefish meeting that he had never heard so many raising so many questions about a stock assessment, the response of the RD was "we need to establish credibility but I also strongly feel that there needs to be a management program not predicated on whether you like an assessment result or not." Nevertheless, at that meeting it became clear that NMFS did not reject a new bluefish assessment in principle, but the SAW / SARC schedule was full and there were no more resources that NMFS could put into bluefish for Amendment One to be finished in any reasonable time period. By November 1998, NMFS had agreed that the Commission's bluefish technical committee (BTC) should reevaluate SARC 23 and create a new assessment on which to base Amendment One.

While many in the recreational fishing community believed that NMFS' firmness stemmed from their smarting over a perceived defeat in the 1995-1996 period, another explanation can be found in prevalent ideas within NMFS' about how fisheries management should be done. The ideas included a precautionary approach to management decisions, an exclusive focus, in a practical if not a discursive sense, on controlling fishing pressure, and a fairly mechanical idea of how management decisions should be reached based on a strongly articulated science boundary, with NMFS being responsible for "technical" issues and the Council being responsible for "management." Within the bluefish debate, these ideas, appropriately, were mainly articulated by the Regional Director, but statements in meetings and interviews by other NMFS employees indicate that many of them shared these basic notions of what NMFS should be about.

Federal resource management agencies in general are mired in a struggle over how closely they should be linked with and sympathetic to resource industries. In many cases the industries are able to bring political pressure to bare on the agencies. This is a struggle in which scientists have often been on the front line of those who want the agencies to be conservative resource guardians (Wilkinson 1998). NMFS also has a history of working very closely with the fishing industry and failing to make the decisions needed to protect fish stocks (McEvoy 1986). NMFS today is seeing a generational change in which many people have enthusiastically embraced the FAO Code of Conduct for Responsible Fisheries. That code contains what is known as the precautionary principle: "States should apply the precautionary approach widely to conservation, management and exploitation of living aquatic resources in order to protect them and preserve the aquatic environment. The absence of adequate scientific information should not be used as a reason for postponing or failing to take conservation and management measures" (FAO 1995: 7.5.1). The Code has emerged as something of a credo in the fisheries science community internationally. In our survey of US fisheries scientists, 80% of all scientists,

and 80% of NMFS scientists agreed with the statement "It is critical that fisheries management be risk-averse and choose lower fishing pressure when stock condition is uncertain."

NMFS' application of the precautionary approach is at the heart of their vision of management and fishing pressure is what NMFS is best empowered to control in a precautionary way. While they are happy to admit that other environmental factors may be important this admission is an intellectual assent with few practical implications. The new laws do emphasize the importance of habitat, and require FMPs to identify areas of essential habitat for their species. All NMFS is able to do with this information is to write letters to other agencies asking them to consider the protection of this habitat. Beyond this, NMFS scientists also tend to conceptualize fisheries management as a technical problem and fishing pressure plays a key role in this conceptualization as discussed below. The combination of these things means that NMFS sees reductions in fishing pressure, in response to and to the degree indicated by a stock assessment model based on precautionary assumptions, as what fisheries management is all about.

That the NMFS perspective tends to see fisheries management as essentially control fishing pressure is clear from several statements. During the initial presentation of SARC 23 at the Public Review Workshop, for example the NMFS scientists said "So, again, I repeat that the focus of the SARC was more on, how do you get out of the current dilemma of say doing things that are within man's control, and the only thing you can do is really to lower catches. "

Other participants are less comfortable with this assumption, as in this quote from an interview with an industry representative:

> On bluefish there is a very very fundamental disagreement that we have on the issue of stock condition, number one and number two the impact of recreational fishery on stock size. Biologists are starting to acknowledge that there is less impact now, but they are saying 'oh, maybe there's not but we still have to protect them and this is the only way we can do it, we can't deal with environmental factors, we can deal with you so we are going to screw you guys.' I think it is destructive and I don't think it is going to have a great deal of impact biologically but it will have a great deal of impact from the standpoint of socio-economic impact. It will destroy more human beings and small businesses who cannot survive this and who are not the cause of the problem.

The conflicting interests in fisheries management means that there can be no final, objective criteria that determines where the burden of proof lies. The issue, in the final analysis, is about the distribution of gains and losses from assuming or avoiding the risks of overexploitation. Those who are going to lose business now from a cutback, and who may or may not be the ones who enjoy its potential future benefits, are going to be much less sanguine about defining as irrelevant other causes of stock decline that cannot be responded to with changes in fishing pressure. While nearly everyone gives lip service to the precautionary principle, many people resist, and not always unreasonably, its stark demarcation of what the null hypothesis should be. As a council member put it at the special bluefish meeting

> [With all due respect for the precautionary principle] I think you will have a very hard time convincing people that it is wise to take actions that will put people out of business today because if we get squared away two years from now that we really didn't need to do that in the first place well now you can tell them to go back in business. Once they are gone they're gone.

In other words, lip service aside, there is real resistance to the idea that fisheries management should be a fairly mechanical process based on reducing fishing pressure in response to models with built in precaution. In this context the following image, a quote from the RD at the special bluefish meeting, is interesting.

> Basically what the SFA [Sustainable Fisheries Act] guidelines say, nobody can see this because it is too small probably, but you know a simple control that any engineer would draw for running a machine. At the bottom you say where the stock is and at the top you say how you are going to actually control the fishing mortality rate. And irrespective of where you are on that graph you should be able to know what to do. And right now you don't have a clue....This is what should be in the management plan irrespective of whether people believe the current assessment. There are always going to be people who don't believe the assessment, because they have a local view of the fishery.."

This "engineer's machine" model is an interpretation he is offering of the SFA guidelines, i.e., NMFS' interpretation of how the act should be practically implemented." This remark expressed an attitude and personal commentary rather than a policy, but nearly everyone at the meeting, including the Council member quoted above, picked up on this idea and agreed in principle with this idea of "contingency planning," as it came to be referred to. The only real disagreement was from an Council scientist who argued that the set of tools currently pointed to in the FMP met the basic idea of the contingency planning, while the RD wanted the "top" of the engineer's drawing to contain specific actions rather than simply tools and options.

Identifying those actions is the work of the Council, not NMFS, because that is a "management" rather than a "technical" decision. Nontechnical aspects do exist within the engineer's machine because everyone recognizes that fisheries management is finally about making political decisions. The most basic political decision, however, the application of the precautionary principle, is built in to the model that tells the machine "where the stock is," because that decision is too important to be left to politics.

NMFS has rejected the failed model of the federal agency working in cahoots with the industry. Given the huge scale of the issues involved in federal fisheries management, the alternative model they have is that of the guardian of the resource telling the industry, to quote a NMFS official's off-the-record statement to a Council member in May of 1997, "Bluefish is overfished, deal with it."

The problem with applying this model to bluefish was the input to the engineer's machine. The statement 'bluefish is overfished,' at least to the degree that SARC 23 claimed, was not enough. New law or not, it seemed, the drastic cuts implied by the SARC's findings were not going to happen when so many people, scientists and lay people, did not believe the findings valid. By November 1997, in hopes of unblocking Amendment One, the small group of scientists that made up the Commission's Bluefish Technical Committee had been asked to review the SARC 23 assessment.

## VII. THE DELIBERATIONS OF THE BLUEFISH TECHNICAL COMMITTEE

Two full-day meetings of the BTC took place in the Winter of 1998. The most active participants in these meetings was a smaller group of scientists who had specialized expertise in statistical stock assessment models. These "real stock assessment scientists" included a scientist from the Council, one from the Commission, and three state scientists also seemed very comfortable discussing the statistics. For the ease of presentation I will refer to these three as Peter, John and Tom. The Woods Hole scientist who joined the second meeting is also a well respected statistical modeler. All of the other scientists were mainly quiet during the meetings, except for the Chair who is a non-statistical biologist. Of course, some of the quieter ones could have been stock assessment experts and one certainly was. In addition to these meetings there were several telephone calls, particularly among the subcommittee responsible for doing the actual stock assessment. These calls were not observed, of course, so the material for this description comes from observing the two meetings and an examination of both input and output documents from the meetings.

Two such documents were pulled together in preparation for the first meeting in February. One of them was an Stock Assessment Update of the same analysis that SARC 23 had used, a "virtual population analysis based on an integrated catch-at-age analysis "(VPA-ICA), using data that had become available in the mean time. Most of the discussion in the first meeting focused on this document. The second document was the short memo by Peter, that laid out the arguments for the displacement hypothesis.

The BTC sat around a large table in a hotel meeting room, the wall was lined with and outer ring of chairs for observers. Almost everyone who actively participated was a scientist on a state staff. Also present were the Council scientist and Commission scientist mentioned and the Commission scientist had prepared the Stock Assessment Update. Two scientists from the US Fish and Wildlife Service and one from New Jersey Sea Grant were also present. No NMFS scientist sat at the table nor spoke, but three did observe.

The tone of the meeting reflected both a sense of urgency, almost to the point of distress, that an answer was needed soon, and unclarity about what exactly they should be doing. The Chair kept returning to the urgency of the task and the bureaucratic boundaries defining it. At least initially, he did not feel that their job was to do a stock assessment, that was the job of a SAW and a SARC. His goal for the meeting was that they would point out the weaknesses in the data and recommend ways to improve it. The Council scientist, on the other hand, wanted them to go further than this. He believed that it was the assessment that had caused Amendment One to stall and that the BTC was the group that could get it on track again. He wanted them to consider returning to yield-per-recruit based definitions of overfishing that had been in Amendment One before SARC 23 changed the definition to Fmsy. A lawyerly interchange ensued when the Chair suggested that Fmsy was a legal requirement but the Council scientists responded that the law says "you don't have to use it if you can't calculate it."

The meeting began with the North Carolina report that contained the information about the inaccuracy of bluefish aging. While this report received little reaction when presented the aging

problems came up a number of times in the discussion of the VPA-ICA. In the official minutes aging figured importantly in the reasons for finally rejecting the ICA approach.

The bulk of the discussion centered on the now updated VPA-ICA. As the Commission scientist who presented it put it, referring to the information from the NEFSC fall survey, the "bottom line is that inshore and offshore data have different signals." Including the offshore strata, where far fewer but older fish are caught, made the stock appear stable or at least to be declining less steeply. (A result one would expect if the displacement hypothesis were valid.) The offshore data did not fit the model well, so he did not include it in his final analysis. Peter pointed out that one cannot be sure what "fitting" means. Data "fitting" a model figures prominently in all of the BTC's discussions. As a concept its meaning is straight forward. Any statistical model is based on assumptions about how pieces of data should relate to one another. The assumptions include theoretical assumptions based, in this case, on biology. For example, all of these models assume a set rate of natural mortality, i.e., that an age one bluefish has the same chance of dying from a non-fishing cause as an age seven bluefish. The assumptions also include statistical assumptions, e.g., that a sampling error will have a normal distribution. Certain indicators and tests of the 'sensitivity' of the model to small changes can help ascertain the degree of fit and the relative importance of different assumptions, but give less information about why a particular set of data does not fit. The fit of one set of data often depends on what other data is included.

At this point in the meeting many of the quieter scientists spoke up. Even without the offshore data, several suggested that the bottom trawl data did not make sense. One pointed out that usually you can follow large and small year classes through a time series of this kind of data, in this case you could not. Another pointed out that the model indicated that fishing mortality went up after management measures were implemented with the Bluefish FMP. The anomaly of the dome shaped pattern that reflected the fewer than expected fish in the middle year classes got the most attention. As the scientist presenting the model put it "I don't understand where the age three, four, and five fish go." Both the commercial and recreational landings showed the same problem. Several possible reasons for these missing fish were suggested around the table: subtle interaction in sampling errors were possible; the lack of data on night fishing; geographical differences were apparent in the MRFSS data; different gears were being used; or perhaps when fish get older they move somewhere where they not available to the fishery.

The upshot was that with these three, four and five year old age classes showing fewer fish than theoretically expected the VPA-ICA model could not be estimated. The model was designed to assume that older fish are more vulnerable to the fishery than younger and if the data deviated too far from this assumption the model would "force" it back into line, giving meaningless results.

Given these problems, the Commission scientist had reevaluated the work done at SAW 18. In his opinion, the scientists at SAW 18 had gone too far in trying to fit this data to a model of this type.

> the stock recruitment data does not allow me to fit a good stock recruitment relationship. I went through this exercise to get the values of Fmsy and went back to reconstruct what was done in the SAW 18 and it was exactly like pulling a number out of the air...the numbers that went into the Sheppard model parameters [at

SAW 18] were numbers that were estimated, not calculated, not fitted, to anything. So I would say that I think a stock recruit from this set of numbers don't have any meaning for calculating any reference point.

In the end the committee decided that it could not recommend using this type of model to give an updated estimate of Fmsy, and this problem with the stock recruitment relationship was recorded in the minutes as the reason they made this decision.

Beyond these statistical problems with the population measurements, the scientists, Peter and Tom in particular, were also concerned that the model did not seem to realistically reflect what was known about the biological growth and reproduction patterns of bluefish. Bluefish mature quickly and are quite fecund, i.e., they produce a lot of eggs. These characteristics should make them less susceptible to overfishing pressure. Peter pointed out that the model made bluefish look similar to long-lived species that mature more slowly and are susceptible to damage at Fs as low .15. These results were showing full scale overfishing, implying that the stock can't take much fishing pressure. Tom called the model's estimate of Fmsy "inexplicable." He speculated that there must be some sort of scaling problem because the results were not just marginally odd, they were orders of magnitude below what you would expect based on the life history arguments. It is "baffling," echoed Peter, "it is like there is something else out there just eating them."

The implications for management from SARC 23 were troubling. The F of .06 which the SARC recommended was, as one scientist put it, "beyond our resolving power, " by which he meant that .06 is so close to zero that the statistical models they would have to use to measure the fishing mortality rate (F) could not reliably pick up the difference between the two. "We have to do what is reasonable, this would mean closing the fishery. That just does not make sense!" John lamented the fact that they are now so involved in quantitative management when 20 years ago they used to manage fisheries without it. They could still give advice based on their overall judgement, but now the law requires particular numbers.

Given these results, the discussion turned to what had to be done next. The chair insisted that they had to have at least a preliminary answer by the end of the day. The Council scientist pointed out that the SARC 23 is at the present time the "best available data," and the chair agreed that if there is no answer then SARC 23 is the answer. Tom would not accept this, SARC 23 was "just as bad." "How do you choose?' asked Peter "is it just statistics" that you can rely on for an answer? The Council scientist said that he has watched SARCs reject VPAs better than this.

They decided unanimously on the basis of the discussion to reject the VPA from SARC 23. The chair was very concerned this be a truly unanimous decision, asking that everyone make sure that they were certain they agreed. Both humor and signs of nervousness showed some tension. They were not comfortable challenging so directly the findings of another group of scientists. One scientist remarked that when NMFS finds out about this their reaction will be to hold up a cross to ward off evil (he demonstrated rather than described this reaction). Nevertheless, they are agreed that the advice that came out of the SARC "does not make any sense."

Tom suggested that they examine the possibility of using a production model. A production model examines the production of an entire stock of fish and does not break the fish into age classes. Thus both the aging error issue and the lower than expected numbers of age three, four and five fish will not violate the model's assumptions. These models also yield estimates of a standing stock biomass (SSB) at MSY and Fmsy. Hence they meet the legal requirements. They are also fairly easy to do, he would be able to do it in a day. They agree that he should do this for the next meeting. They will also look more closely at how to weight the various sources of data and meet again in a month.

Between the two meetings two documents were prepared. One was the production model. The specific model they chose was the ASPIC biomass dynamic model. The other document, prepared by Peter and one other state scientist, was a new assessment that used several different models using the inshore and offshore NEFSC surveys as their indicators of abundance. They created indicators of relative fishing mortality by age that they used to bring the missing age three, four and five fish into balance and fit the statistical model. The result was an assessment using nonstandard approaches that they felt reflected the bluefish reality better than the standard ones did.

The March 1998 meeting of the Bluefish Technical Committee took place around a rectangular table. Most of the people were the same as the February meeting. At the head of the table sat the chair and Council scientist. At the bottom of the table, at first alone as no one took the seats near him until later in the meeting, sat a scientist from Woods Hole who had been very active in bluefish issues.

The meeting began with a reading of the minutes of the February meeting that was not the usual quick formality. Peter immediately began to object. The minutes did not emphasize enough the objections that they had had to the ICA model used by SARC 23. He listed the issues he did not think sufficiently emphasized. One was how the model showed fishing mortality (F) rising in the years after management measures were introduced. Another was the various ways in which the model did not reflect the biology of the fish. A third was the fact that the model found an Fmsy that was much lower than the assumed natural mortality rate. "We could not think of any other FMP where the overfishing definition was 40% below the natural mortality rate." He also thought more emphasis should be given to the SARC's management recommendations requiring a severe reduction in the fishery.

The scientist from Woods Hole was not pleased. It is true, he argued, that the SARC 23 estimates may look implausible. SAW 23 had made a consensus to use parameters that emphasized the recent indicators of low recruitment. SAW 23 had had a lot of concern with whether or not this was an appropriate model, and they went along with it because it seemed reasonable, given the condition of the stock, to use a conservative estimate for near term management advice. While Peter's points are reasonable, he continued, it should also be pointed out this none of these criticisms are new. It should be stated in a way that does not sound like this group is saying that the SARC made an error and picked the wrong reference point because they did something stupid. "That is the way it sounds when I read the minutes 'without objection the thing was rejected.'"

He also picked up on a statement that said that there was a suspicion that more midsize bluefish are landed than are reported, "you can have a gut feeling but obviously there's no data or it would be in

here." This caused a defensive reaction from the chair, who described this as "anecdotal information," clearly meaning this as justification rather than criticism. He was supported by the Council scientist. The tension, particularly that between the head and foot of the table, continued through the Wood's Hole scientist's objections to the seeming unequivocal acceptance of the North Carolina aging report discussed above in the section on aging. Finally a recently ex-Commission scientist, now working for a state, who had been responsible for organizing and coordinating the committee, broke the tension somewhat with "Am I glad I am not writing notes for this committee anymore, you people are brutal."

Then both the scene and the mood shifted. The Woods Hole scientist was invited to the front of the room to give a presentation. The chair suggested that it was important that because NMFS had not been present in February that they should listen now. The tension dissipated as the Woods Hole scientist took on the role of lecturer with an attentive and responsive audience. His comments on aging and survey catchability are reported above in their respective sections. He also compared the implications of the two models prepared for the meeting with the ICA model. He emphasizes that in the ASPIC model, there is a problem with the level of the biomass estimates in absolute terms because it hinges strongly on the assumption that the bluefish stock is fully available to the fishery.

Then Tom presented the ASPIC model. This model does not rely on aging, only on the number of fish caught, but it is very important that the fishery-dependent data be accurately scaled by effort. The model uses the MRFSS recreational data, the NEFSC inshore trawl, and a recreational catch-per-unit-effort index which they calculated from the MRFSS. They were not able to use the NEFSC offshore trawl because it showed a trend in a different direction than the other data and the model cannot accept that. Nor could the model accept the CT survey data. The model assumes that the NEFSC inshore survey is an accurate reflection of the bluefish biomass available to the fishery. If there has been a redistribution from inshore to offshore then the model is generating a higher F than it should. The model gave an Fmsy of .40 and found that F in 1997 had been .51, so the management advice that would proceed from the model would not be nearly as drastic as that from SARC 23. However, he argued, these results are more conservative than Peter's and "I don't know if we can spin it that way but we should keep this in the back of our mind." The Woods Hole scientist reemphasized that the key difference between the ASPIC and models that use age data is the increased importance of the assumption that the whole biomass is available to the fishery.

Then Peter presented his assessment. He had felt it necessary to develop his own approach because he believed that the stock had moved offshore and that an assessment needed to be developed that reflected that. He melded several accepted approaches together but not in a standard cookbook fashion. His model found that the stock was not overfished. He reiterated a number of arguments for the displacement hypothesis. First, trends in landings of striped bass are almost an inverse of those of bluefish indicating that just as the striped bass were coming back in the early 1990s bluefish were falling. Second, many people that he talks to have seen a lot of bluefish offshore. Third, the bluefish stock is not being compressed into a few age classes as is usually the case when a species is overfished. Fourth, if the stock had been overfished in an earlier period this should be reflected in lower average weights and it is not. Fifth, his model predicted an Fmsy that was consistent with Tom's model.

Altogether, he felt that this made a strong case that the population was not overfished. Tom was very critical of Peter's model, mainly he said, because all the other models were saying that the stock is collapsing. Peter's results could also reflect a single strong year class. Peter responded that a strong year class would cast further doubt on the hypothesis of poor recruitment. The Commission scientist argued that the compression of the stock into a few age classes could be seen in data from New Jersey south. Tom objected that merging in and offshore data was based on an ad hoc assumption. In his model they had tested the inshore index and gotten rubbish. Peter responded that they always make ad hoc assumptions.

The chair opened the discussion up by asking people which of these conflicting assumptions they felt comfortable with. Toms argued that the use of the offshore index was the important difference between his model and Peter's. He did not trust the offshore data because it bounced around without trend, while the inshore numbers simply declined. The offshore data, he had decided, had no information in it. The chair responded to this.

> "This is my personal perspective, clearly some members of the community out there that we are going to have to take this out to feel that there is some component of this fishery that has moved offshore. Now if we can't account for that in any assessment that we can do that's fine. But my question is the index that we get from that inshore stuff is what it is. If it has so many problems with it that we absolutely can't use it, because of the way we have done it or whatever....Um, I know that I certainly feel that the issue of unavailable fish is not mythical as [the Woods Hole scientist].

The Woods Hole scientists had used the word "mythical" earlier in reference to the stock of bluefish that the displacement hypothesis claimed was offshore. He had not, however, meant that word in the sense of nonexistent. He responded

> Well, I don't think so either.... I catch them on the 100 fathom line too."

Yet, Tom said, while "the inshore could have plausibility, the thing that I grapple with is why bluefish would be pushed offshore by stripers." Peter responded that he was arguing that the rise in striped bass has accelerated an existing displacement of the bluefish stock.

The Woods Hole scientist then offered a summary analysis, at this point he was animated, clearly enjoying the intellectual puzzle aspect of the discussion. He argued that at the bottom of all the headaches was that the catchability is so much higher for the two youngest age groups. "We're stuck." The best CPUE data is fishery dependent so it is not independent of quantity. He suggest that they had the same ideas but were using different methods to get to at. The reason the offshore did not fit in the ASPIC model was hauls that had zero or one bluefish, this made the data statistically intractable. The offshore displacement of the bluefish showed up in the ICA model in both the missing three to five year olds and the fairly low Fs. He and Peter were using the same, age-based data in a little different way to get results that disagreed with the ASPIC because the ASPIC model assumes that the stock is fully available when it is displaced offshore.

"This is why I said earlier that this biomass dynamic model is probably OK in relative terms...what we have to be careful about is not getting ourselves wedded to any absolute terms of biomass," because the ASPIC model is so dependent on the assumption of full availability. He

recommended that the best thing to say would be that the inshore part of the stock is currently being overfished but not the offshore. This is what the ASPIC model, after all, showed because what it was characterizing was the part of the stock that was fully available to the fishery. If they couched it in those terms "you get the same amount of fishing mortality reduction, in relative terms, needed to attain your goal without putting your neck out on the chopping block of absolute numbers... That is the kind of advice I am comfortable with." This approach was out of bounds, however, as the Council scientist explained, the national standards required them to give an absolute number.

Peter added, getting a laugh, that "I don't think that my analysis will pass as well as the ASPIC, I just think it is the right answer." Tom pointed out that his was the more sophisticated approach and would stand up to examination. " I think our approach is more rigorous analytically, it is a more accepted model, the residual diagnostics can be examined, that sort of thing." The Commission scientist also pointed out that the ASPIC was more conservative, to which the Woods Hole scientist responded that one could call it conservative or one could call it biased low.

Peter recommended that they try to postpone for a couple of weeks but the chair said that they didn't have that option. The Woods Hole scientist had suggested that if the offshore data were converted into weight rather than the number of fish that it would become more statistically tractable, whether tractable enough to fit the models could not be known ahead of time. This process, however, would take someone around two weeks and no one had the time to do this. Tom suggested that the could bring all three of the models before the Council Science and Statistical Committee that must make the final decision. This is in fact what they did. According to the summary minutes, the Science and Statistical Committee decided, as Tom and Peter had both predicted, that the ASPIC model "provided a good assessment of the stock, given the uncertainties about aging and other biological characteristics."

## CONCLUSION

For understanding what the case study tells us about the tension between citizen participation and science in fisheries management the most essential issue is how the fishers' observations, the "anecdotal" data, was treated. In this instance the RD's claim that it was always used as background for tuning and explaining models was inaccurate, but it was inaccurate in a much more complex way than if it had simply been ignored. The scientists did consider the fishers' observations that the bluefish stock had moved offshore, they considered them extensively, and most of the scientists believed that these observations reflected reality. As one scientist said "I catch them at the 100 fathom line, too." They were also aware of industry concerns about the accurate measurement of effort and scientists' concerns about accurate aging. In the end they chose a model of the stock that relied on good effort measurement and did not rely on aging. It also depended on one assumption more than any other assumption: that the bluefish stock had not moved offshore.

The consideration of the observations of the fishing community was not only without influence on the official outcome, it was haphazard. The public was involved in the assessment of the bluefish stock in only a purely formal way. Council members, fishers and other participants were allowed to raise questions but the simple response "you're right and we considered that" was sufficient to silence

these concerns, until they came through the back door of political pressure of various types outside the science boundary.

What happened with fisher - scientist interaction in this case went well beyond the issues that the literature on the subject has highlighted. Several of the standard explanations for breakdowns in such communications do not fit. It was not primarily an issue of scientific and lay cultures seeing the problems differently and not understanding one another (Smith 1990, Wilson and Dickie 1995), although some of that was seen around the issues of aging and effort. For the most part the fishers and scientists understood each other well. The scientists were fully cognizant of what fishers were saying and the lay people had a fairly sophisticated grasp of the issues of data collection, if less so with the actual models. Nor was it a question of interaction processes where lay people were intimidated by poorly designed interaction mechanisms and/or their insights trivialized (Kaminstein 1996), although improvements could certainly be made in how public hearings and advisory panels are organized and designed (Wilson and McCay 1998).

What channeled these interactions in an unproductive way went deeper than either of these two explanations and affected both communications between fishers and scientists and among scientists. The legal and bureaucratic system distorted these communications by predetermining what could be meaningfully said, not simply by defining the objectives of the meetings and discussions, but by controlling the form of the arguments to such an extent that their effectiveness was detached from their truth content. The scientists and other participants were well aware of these distortions and responded to them with irony and humor because they have no more effective responses.

The science boundary was in part drawn by the legal constraints on what facts were allowed to be effectively defined as scientific. The relevance of every scientific argument, meaning arguments about the actual situation in the natural world, was always already determined by legal and bureaucratic imperatives. This turned the meaning of 'scientific argument' into an argument about the best way to get the estimates of predefined model parameters past peer review. Everything else is 'considered,' but finally external to the problem at hand. Yet, this was not a result of bias or incompetence, but of good intentions. The system of limiting the possible decisions was, of course, set up intentionally in the Magnuson-Stevens Act to provide necessary mechanisms of external accountability to the fisheries management system.

Does this mean that the scientists shopped around for an assessment that would fit some management agenda? No, except in the narrower sense that the scientists had a general commitment to being conservative. These people were motivated by a desire to be good scientists in the eyes of their peers and to do their jobs well. This meant making as fair an assessment of the bluefish stock as possible given the legal and technical constraints of those jobs, and their committement to being conservative. The assessment had to describe the entire stock using certain parameters that would stand up to a peer review. Their best judgement was that a substantial portion of the stock had moved offshore. All of their models and the opinion of the broader fishing community, the ASPIC included when evaluated in the light of the full availability of the stock, pointed to the same conclusion. There had been displacement and there was an inshore portion of the stock that was being heavily fished and an

offshore portion that was not. Yet they could not legally offer this "relative abundance" answer with which they felt the most comfortable.

Peer review, that which decides what is the "best available scientific information" is much more than just scientists deciding that another scientist's work is as close to the truth about nature as possible. Peer review relies on evaluating well known, proven techniques that the peer review panel feels competent to judge. Peer review panels have to be given something that they are able to evaluate in a timely fashion. These panels must focus mainly on the internal consistency and quality of the model before them, because they are not there to redo the assessment and cannot evaluate all possible options the assessment scientists might have not known or had discarded.

Peter's analysis might have more accurately reflected the state of the bluefish stock than Tom's, but both of them knew that on these other criteria Tom's was much stronger. That there was a serious problem with aging, the strongest data argument for Tom's model, had been established by professional scientists in controlled circumstances. The offshore displacement of the stock, the strongest argument for Peter's model, had not been. Peer review is about drawing the science boundary around a particular document, it is based on who did what and how, and whether these people and techniques are within the science boundary. Peer review is based only secondarily on how true something is. In spite of these flaws, peer review is the best, even the only way to establish that something is "scientific," and entitled to all the legal and discursive legitimacy that goes with that status.

The most serious questions this case study raises are about the use of science as a mechanism for implementing the precautionary principle. That the law and NMFS are committed to this principle is all to the good. Three troubling things emerged. The most serious is affect of the legal mandates about the forms of the definitions of overfishing and rebuilding schedules, that were put in place to ensure that the management system could be held accountable for being conservative. At least in this case they constrained the scientific discussion to the point to warping it, forcing the scientists to submit results that did not reflect their best overall judgement about the condition of the stock. The second is that both the legal mandates and the need for a science boundary have led to an overly strong exclusion of the fishers from the scientific discussions. More satisfactory responses need to be made than "your right and we examined that" and the scientists need to have the flexibility to make them. The third is that the commitment of the scientists to the precautionary principle led them to openly incorporate this essentially political decision into technical models. This is a misplacement of the science boundary that, if used to often, will undermine their credibility in the long run.

Many fisheries science programs are emerging that bring fishers in as full participants while maintaining the integrity of science (Wilson 1999). None of these programs, however, provide many clues as to how to address this problem on the geographical scale that NMFS faces. All we can do here is continue to experiment. The case study does suggest, however, that alternative ways to hold managers accountable need to be developed beyond stringently predefined overfishing definitions. To the extent these definitions undermine the ability of scientists and others to use their best judgements they will undermine management as a whole, including the goal of conservative protection of fish stocks. In this same spirit, it suggests that other purely mechanistic attempts at rationalizing and automating

fisheries management decision making, such as the engineer's machine idea of management, may threaten to constrain discussions of both science and management beyond the point where they still able to use people's opinions about what it true as an important part of coming to a decision.

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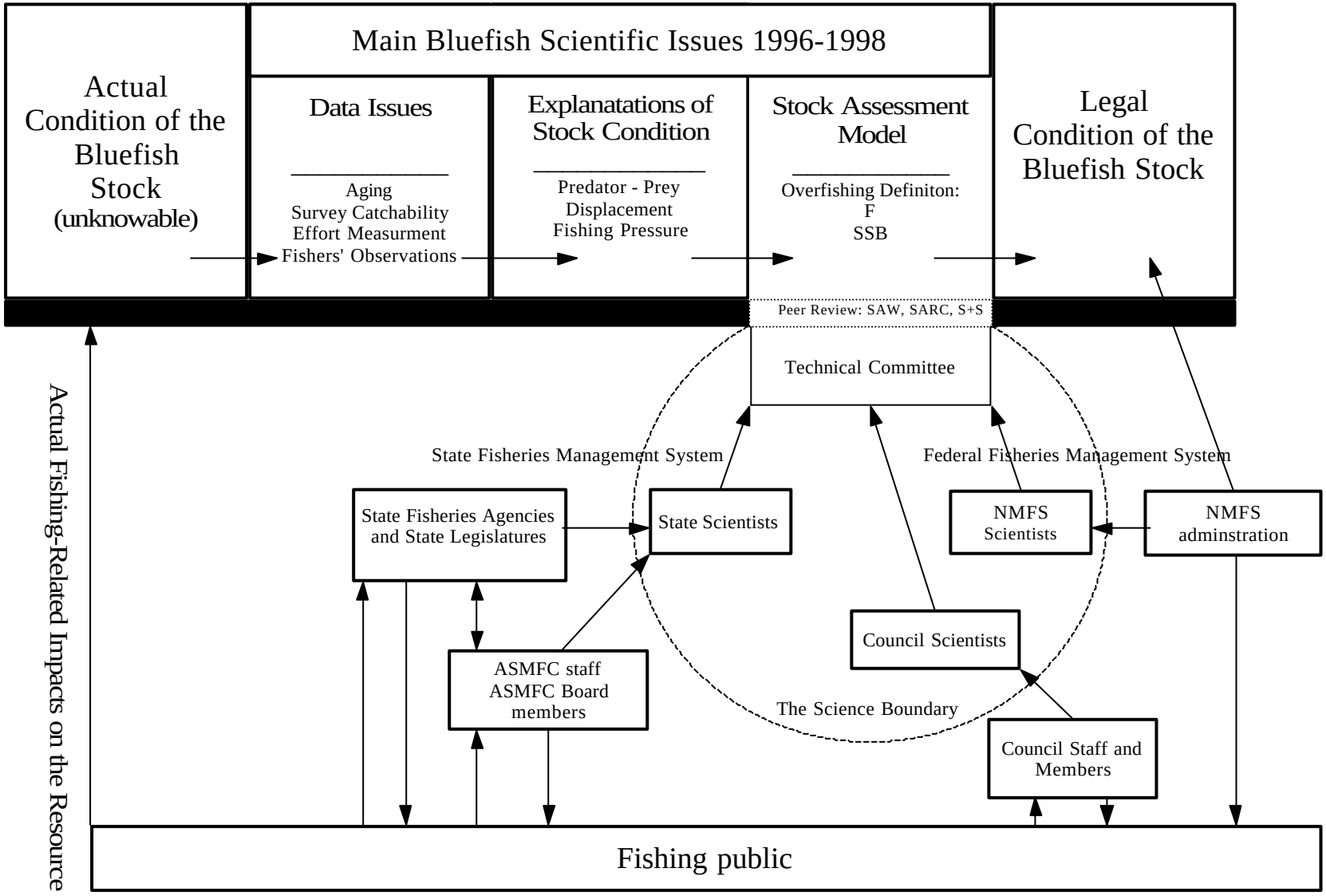
Main Bluefish Scientific Issues 1996-1998
Actual Condition of the Bluefish Stock (unknowable)
Data Issues
Aging
Survey Catchability
Effort Measurment
Fishers' Observations
Explanatations of Stock Condition
Predator - Prey
Displacement
Fishing Pressure
Stock Assessment Model
Overfishing Definiton:
F
SSB
Legal Condition of the Bluefish Stock
Peer Review: SAW, SARC, S+S
Technical Committee
State Fisheries Management System
Federal Fisheries Management System
State Fisheries Agencies and State Legislatures
State Scientists
NMFS Scientists
NMFS adminstration
Council Scientists
ASMFC staff
ASMFC Board members
The Science Boundary
Council Staff and Members
Actual Fishing-Related Impacts on the Resource
Fishing public