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# Sissenwine, Michael ~ Oral History Interview

Joshua Wrigley



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Voices from the Fisheries
166 Water Street
Woods Hole, MA 02543

# Interview with Michael Sissenwine by Joshua Wrigley
*Summary Sheet and Transcript*

## Interviewee
Sissenwine, Michael

## Interviewer
Wrigley, Joshua

## Date
July 25, 2016

## Place
Northeast Fisheries Science Center
Social Sciences Branch
Falmouth, MA

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## Use Restrictions


## Biographical Note
Michael Sissenwine was born in 1947. He earned his B.S. in Physics and Mathematics at the University of Massachusetts Amherst, and his M.S. in Oceanography at the University of Rhode Island. He began working as a scientist at the Northeast Fisheries Science Center at Woods Hole in 1975, eventually rising to the position of Center Director at the Woods Hole lab and later, Director of Scientific Programs and Chief Science Advisor at NOAA Fisheries. In addition, he held a number of other positions including President of ICES and Distinguished Senior Scientist at UMass Dartmouth School of Marine Science and Technology.

## Scope and Content Note
Interview contains discussion of: University of Rhode Island Oceanography program, development of modern fishery management, disagreements between the Northeast Fisheries Science Center and the fishing industry, International Commission for the Northwest Atlantic[ICNAF], Total Allowable Catch [TAC], 1976 Fishery Conservation and Management Act, forming the Fisheries Management Councils, defining “overfishing,” Northeast Multi-Species Plan, fish mortality, challenges facing scientists, ecosystem-based fishery management and the effect of climate change on fisheries.

In his interview, Michael Sissenwine gives a vivid description of his career studying fisheries and oceanography and working as a scientist at the Northeast Fisheries Science Center. He

details many of the challenges he faced throughout his career, particularly regarding conflicts between NOAA and the New England Fishery Management Council.

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**Transcript –MS_001**

**Josh Wrigley**: When were you born?

**Michael Sissenwine**: 1947. So, and I, I had no interest in fisheries and marine science or whatever, I suppose as a kid I used to go trout fishing the first day of the season on some lake that I lived near, but basically I had no, no particular early interest in any of the stuff that I've been involved in since. I went to the University of Massachusetts at Amherst as a physics major. My degree is a joint degree in physics and mathematics. And it was really an odd set of circumstances that got me to the University of Rhode Island in oceanography. I was actually planning to go to, um, a number of different graduate schools including Cal Tech, I think, was one of them, in math or physics. And it was purely personal reasons that I ended up at URI, related to my, an option for a graduate school that both myself and my first wife could go to, so we ended up at URI.

I'd never even heard of oceanography which is actually not surprising since it, this was 1969 that I started graduate school, and there was not really a discipline of oceanography that anybody knew about. There were like three schools in the country; Woods Hole Oceanographic which I guess I'd heard of, but, the University of Rhode Island, and, I don't know, they have, there were one or two other small schools that existed. I ended up at the University of Rhode Island purely because, my father was a meteorologist doing atmospheric research and he had a colleague who was the President of the University of Rhode Island who was a meteorologist and just, at some point in life when I decided I wasn't going to go to Cal Tech, I was going to get married and stay at the same place as my ex-wife, was my wife at the time was going to be. We started shopping for schools and he said well, contact my friend Werner Baum, was the guy's name, who was the President of the university, and see if he would, see what the options are and the guy said, "oh yeah, we've got this great new disciplinary program in oceanography. It's sort of like meteorology except it's water instead of atmosphere, and so on. " That's how I ended up in it. I started as a physical oceanographer.

**JW:** Do you remember what had been the impetus for URI to start up the oceanography program at that time?

**MS:** Um, it's the Ocean State, I mean, it had, the oceanography program dated back twenty years earlier than that. There was, there were, Charlie and Maria Fish were the founders of the marine lab and they were, they were basically acousticians who studied a lot about sound and the sea and so forth, and probably got some Navy grants for acoustics. You know, most of oceanography, I shouldn't say most because I don't really know, but a lot of the early oceanography related to anti-submarine warfare and stuff like that and sound and the sea.

**JW**: Advanced research?

**MS**: Yeah. They were more biologist; the Woods Hole Oceanographic was specifically founded by the Navy to start anti-submarine, or, submarine related research. About the same

time, I think, these programs developed at URI. And what evolved from that was pretty much a function of the individuals involved. These people the Fishes, Charlie and Maria, who I never met, they were long gone. Probably alive but I never met them when I went there. And the program, the program certainly got a big boost from the Sea Grant Program which was founded by Pell, Pell was a Senator from Rhode Island, so, I mean that, I think that sort of what propelled the University of Rhode Island into being a, an early significant player. Anyway, I ended up there as a physical oceanographer with John Knauss, is the, who's the, you know...

**JW**: Of Fellowship fame?

**MS**: Of Fellowship fame, later became the head of NOAA. And John and I actually became very good friends over the years. And, you know, I, I actually evolved and became a, got involved in applied fisheries and marine biology because there were, it was more funding for students than in physics, which is somewhat surprising but that was the case then, for people who had a mathematics background. So...

**JW**: How many students were in the cohort at the time?

**MS**: Graduate school was probably a hundred students. Hundred, yeah, hundred graduates. It was only graduates. I mean, pretty big, being...

**JW**: And they were all working in fisheries?

**MS**: No, no, this was, they were in chemistry, physics, geology...

**JW:** Oh, so across the board, yeah.

**MS**: Relatively few in fisheries, but because of the one professor, Saul Saila, who was the major professor for a whole bunch of people. I mean, at the time he was about the only professor there that had graduate students that had ever finished degrees. So anyway, he decided to basically go recruit people who were trained as physicists and mathematicians because he thought that's where the future was. And it, and he was right, I mean, basically the future in biology to deal with fisheries or other environmental issues was with mathematics and biological models.

So I, actually first started in population genetics very briefly, for, related to aquaculture and selected breeding but that, that only lasted about one semester before whoever the hell I was working with was a, didn't get tenure. And so I started working directly with Saul Saila and we mostly worked on power plant, nuclear power plant sitting problems. And the issues of the environmental impact of entrainment of eggs and larvae by the cooling water systems these, this was in the year when these were all open circulating systems. And so I started getting involved in population models of what the mortality of these eggs and larvae would do to the populations that were being impacted.

**JW**: What species were you working on mostly, in terms of entrainment mortality?

**MS**: Well, the particular plants that I had some involvement with were mostly entraining the most important species and one of them in the Long Island Sound was winter flounder, the most important species in terms of the national politics was striped bass in the Hudson River,

a whole series of plants where there's huge litigation, years and years of litigation, and--

**JW**: And this was just in the wake of Con Edison's defeat--

**MS:** Well, it wasn't...

**JW**: -- wanted to install a power plant in Storm King Mountain, right, in the mid-1960s, right?

**MS**: Yeah, but then there were, the real issues were, were expansion of Indian Point Nuclear Power Plants...

**JW**: Just down the stream?

**MS**: Yeah, so Storm, Storm King had, yes, I guess they had just lost that, although I think they were still trying to revive it...

**JW**: Maybe it was still in the courts.

**MS**: But it was, it was really the Indian Pointones [power plants] that were the striped bass issue. And so, you know, I started, I got involved with that stuff as a graduate student, mostly, mostly Millstone Point in Connecticut and as I think back on it I really didn't get seriously involved directly in the Indian Point stuff until actually I was hired by NOAA a few years later although I was aware of it and interacted with some people.

So we started, we built some integrated physical, biological models in 19…around 1969, well, I guess, beginning about 1970, over a three-year period. And, you know, they were pretty pioneering in that day. I was working with a, with an ocean engineer who was mostly building the mathematical models of the physics of it, I was doing the biology. And then, but I, all that work was under, was in a consulting role for various power companies and was not easily publishable so I had to do something else for a thesis. So I ended up, sort of in my spare time, building a mathematical model of the, relating the yellowtail flounder fishery to climate variability, the effects of basically temperature variability on growth and on recruitment survival. And that ended up being my thesis, which I completed at URI in 1975.

The way I got to NOAA, interestingly, several years earlier, while I was working on my thesis, somebody from the Galveston fisheries lab, I guess had seen some paper I gave at an American Fisheries Society meeting, he called me up and tried to hire me at the Galveston lab. And I'd never been to Texas in my life, didn't know, I mean, I just sort of thought about it and said "no, I'm not going to Texas." And, and in addition it was, it would've meant having to finish my degree, you know, while working, and I got a lot of good advice saying, no, stay and finish. So I did and just after I finished my thesis, I was offered a position at the University of Hawaii, a faculty position, and also another one at the Southwest Fisheries Science Center and was considering those two when I happened to have looked at some newsletter or something that said, this guy that had contacted me in Galveston had just been transferred to Woods Hole. So I called him up and said, "hey, I finished my thesis now, anything going on at Woods Hole?" Because my family was here.

**JW**: What was his name?

**MS**: Well, Steve Clark, the guy who the, who the conference room is named after. So Steve Clark contacted me to Galveston, I contacted him here. And he said, "I don't know but I'll talk to people" and basically Brad Brown, who was the leading the Population Dynamics stuff at the time, contacted me and, and pretty much hired me as quickly as you could do it, the way the government process works. I didn't necessarily want to come to Woods Hole as my first choice. My wife at the time actually wanted us to go to Hawaii, I preferred La Jolla, but for some...

**JW**: That was where the Southwest Fisheries Science Center--

**MS**: Yeah, yeah.

**JW**: --was located?

**MS**: But she had some, she really didn't want to go to La Jolla so we said, "okay, let's go to Woods Hole, you know, it's nice and all that." I did have, my family was all here too which wasn't necessarily an attraction but it worked out fine. [laughs]

**JW**: I just, I wanted...

**MS**: And that's how I got here in 1975 when I started--

**JW**: Okay.

**MS**: --in Woods Hole.

**JW**: I just realized that I forgot to introduce the interview as well so, if you don't mind, I'll just take a second to read the introduction and we can jump back in here.

**MS:** Okay.

**JW**: This is an interview being conducted as part of the Voices from the Science Centers project, funded by the Northeast Fisheries Science Center. It's also part of the Voices from the Fisheries project that's supported by the NMFS Office of Science and Technology. I'm Josh Wrigley and today I'm speaking with Michael Sissenwine at 15 Carlson Lane, which is where the Social Sciences Branch, BNEFSC is located, and the time right now is about 9:20. Dr. Sissenwine has held a number of positions including President of ICES, Director of Scientific Programs, and Chief Science Advisor for the National Marine Fisheries Service, as well as Senior Scientist and is the former Director of the Northeast Fisheries Science Center. So now we can jump back into where we were. Thanks for letting me take that aside there.

**MS**: No, that's fine. So anyway, you got me to 1975 when I arrived here, which I think was August or something like that. So, where do you want to go from here? Are you...

**JW**: Well what was, what was your first position here? What branch were you working?

**MS**: Well, the organization was different, but it was basically the equivalent of what's now the Population Dynamics Branch. We didn't call them branches then, I don't even remember what it was called. Something called RAD, Resource Assessment Division. And so it was a branch that was actually the stock assessment branch, which Vaughn Anthony was the

Director of. I formally worked for Vaughn, although I'd never met him before and he had nothing to do with hiring me. Brad Brown was the Division Chief and I don't remember what that division was called, but it was essentially, ran the stock assessment program and the resource survey program. I mean, those were the two main elements of it. Oh, and the fisheries statistics. So they were the three central issues…elements of giving scientific advice: fishery independent surveys, fishery dependent data, and analysis. So that's where I started.

And this was a really exciting time because it was, uh, it was during the era when ICNAF was in full, you know, full glory. And actually making some progress in managing the fisheries after, after a decade of gross overfishing by what we referred to as the distant water fleets, which were U.S.S.R., Poland, East Germany, those were the biggest ones, but I mean, every country you could think of. But the, oh, Japan was also significant. So anyway, ICNAF had come into its' own and was actually making good progress managing the fisheries and, and really had invented most, a lot of what's now modern fisheries management. Prior to, prior to that ICNAF era, of what I would call invention of modern fisheries management, which really went from around 1970 to 1976, you know, it was a totally different landscape for the way fisheries were managed.

**JW**: Why that periodization?

**MS:** Well, it's just how it came into being. This was also, I mean, it's just when the events occurred, but this was also the period when there was intense debate as to whether the U.S. should extend jurisdiction to 200 miles. It's also pretty critical because it's the period that resulted in huge polarization between the Northeast Fisheries Science Center and the fishing industry. And the reason for that was is that the fishing industry wanted to blame everything on the Russians, even though it was the U.S.S.R., but, you know, all these big boats out there, it was all their fault, they couldn't possibly overfish. And the Science Center went on record as saying "no, the, the regulations were already regulating those foreign fisheries. ICNAF is doing a good job and it's not just them, it's you guys, too, because you're overfishing as well." And carried the government line, the party line that extended jurisdiction is, is the wrong thing to do, 200 mile limit is the wrong thing to do.

**JW**: What year did that statement come out?

**MS**: That debate? I mean it wasn't any single statement, it was essentially a debate that went on from the early 1970s until 1976 when the Fisheries Management Act was passed. It wasn't called the Magnuson Act. Warren Magnuson was credited with drafting it, he probably wasn't the guy who drafted it but it was his, he introduced the legislation. And so it was…it was passed in 1976. It was called the Fishery Conservation and Management Act in 1976, FCMA, 1976. But the debate started, you know, several years earlier. The debate started, probably, predating my arrival in 1975 but it was very intense in 1975…1976. And so the polarization between the industry and the Center was in part because the industry, you know, felt all the problems were foreigners – sort of sounds like the current election – and the Agency's position was, no, not all the problems are foreigners, actually, you know, we actually have good controls and that sort of stuff. There was a perception by the industry, which may have some truth to it, that the Agency scientists were just taking this position because they were having a great time going to meetings all over the world. I mean, that, the headquarters of ICNAF was in Canada, but it was routine to have meetings in Rome and Copenhagen and throughout Europe, and you know, back in a time when people didn't travel the way they do

today. I mean, later in my career, I spent 200 days a year traveling, and, you know, at least 100 of them were international, but in those days it was pretty rare--

**JW**: More rare of a rarity?

**MS:** --it was pretty rare to go to Europe. So there was, there was a lot of glory associated with being involved in this stuff, and I think the industry sensed that and felt that people were having a good time at their expense…which wasn't true in the sense that that wasn't really what was…why it was going on. But it's…in my opinion it's pretty easy to see how it would color everybody's views of things.

**JW**: So going back to ICNAF, what, what were the management controls that they were trying to implement during this time?

**MS**: Well basically, at this…[point, this is when output controls in the form of a Total Allowable Catch (TAC) were implemented. This was one of the early (if not first) applications of a TAC in ocean fisheries.]

**JW**: Prior to the FCMA, that is.

**MS**: Yeah, I mean, basically there wasn't a lot of international fisheries management. There was, there were I believe two other major regulatory organizations that existed prior to, or about the same time or prior to ICNAF. One was IATTC[Inter-American Tropical Tuna Commission], the International Commission for Conservation…IATTC…it's basically the group…the tuna commission that's currently located in La Jolla. It's the Inter-American Tropical Tuna Commission. Ok? So that existed and it was regulating tuna fisheries. And there was the [Pacific]halibut commission, which only had two members.

**JW**: Oh, the International Pacific Halibut Commission?

**MS**: Yeah, and only had two members, the U.S. and Canada. So it was, you know, it was very successful and all that, but it was really so minimal in its activity and its scope and problems, basically, that you know, really didn't know much about it other than it was good science being done. There was a lot of good science. The tuna commission was…I really don't know much about how the regulations went but I think they were mostly effort-based, you know, trying to limit the number of boats and so on. ICNAF was founded in…around 1950. There's a little confusion the dates, 1949 and '51 are mentioned. I think one of them is when the treaty was agreed to and was ratified--

**JW**: Yeah--

**MS:** --a year later--

**JW**: --I've come across both dates.

**MS**: So…and initially, part of the impetus was overfishing of haddock which had occurred [and] was the result of overfishing by U.S. and Canada. It wasn't foreigners. I mean, it was Canada, but it was[n't distant water fleets]..

**JW**: In what areas did it take place?

**MS**: George's Bank, basically. In the Scotian Shelf. And that had occurred twenty years, I think, I think the haddock overfishing, the haddock collapse, one of the first ones was in the 1930s. So it took, you know, like twenty years before the commission actually came into place! But that was the impetus. And it just basically formed, set up mesh regulations which again, going into effect in the 1950s. And then there were some closed areas to protect spawners, spawning haddock that came later. And that's all there was till 1970. And that was about what international fisheries management was.

The Europeans through ICES [International Council for the Exploration of the Sea], which was not regulatory but provided the science for the individual countries to put in place. Again, mesh regulations, based on yield per recruit considerations. So beginning in the late '60s when it was apparent that the foreign fleets, particularly the U.S.S.R., had grossly overfished haddock and it had collapsed by about 1965, '66, '67. Herring was collapsing, mackerel was collapsing, silver hake was down. You know, pretty much…yellowtail flounder was down but that was probably domestic. Pretty much everything was, was potentially being overfished because of the, both domestic and international fleets. So the U.S. and Canada basically led the charge to regulate the mostly Eastern Bloc fleet. And the U.S. fleets, I mean, they were all to be regulated. And a lot of the really exciting science that had gone on in ICNAF – excuse me – in ICES, all shifted to ICNAF. It became the science battleground for regulating fisheries. And so a lot of the big name scientists from Europe were all participating in ICNAF. And essentially, by 1970, the U.S. and Canadian efforts led to TACs, Total Allowable Catches, which was…well, it was, the first time that they had been applied internationally to a mixed stock, you know, multi-species groundfish fishery.

**JW**: Were there any accountability measures that could actually enforce the TAC at that point?

**MS**: No, well I mean all the governments had regulations and were supposed to be enforcing them. I mean, it would be…there are no more today than there were then when you come to international fisheries, other than, well…I'm incorrect in that we've now put in place what are supposedly unbiased observer programs, but, I mean, I'm a U.S. Commissioner to NAFA right now which is the son of ICNAF, and I'm not sure they're any better now than they were then.

So anyway they put in place these quotas, TACs on haddock, cod, and yellowtail flounder, and maybe herring. Those four species were…led the way in about 1970. By 1972, there was a Total Allowable Catch for everything. You know, there were like seven or eight main species and then there was an "others" category that covered everything else. So in a…very, very rapidly the approach to managing fisheries of Total Allowable Catches was put in place and implemented with the national allocations which was a new thing. Each country had an allocation of its share of each of these quotas by area, so [a] very complex allocation scheme.

And by 1976, when it all ended, [ICNAF, that is], there were also pretty sophisticated methods that had been worked out and agreed to for taking account of by-catch, so you'd allocate to each country with an estimate of what their by-catch would be so that none of the stocks would be overfished. Essentially something we haven't really been able to implement domestically ever since. It was also something called an ecosystem quota, which was a total amount that could be taken, which was less than the sum of the individual parts, which was later, within a few years later, copied by, by the North Pacific Council, but for very different

motivations…very different reasons in that case.

**JW**: To what extent was this process of distributing quotas on a nation-by-nation basis a political process?

**MS**: It was an international negotiation, which was obviously political, but you know, it tended to be, as all of these are, driven by past history…but with coastal state preference. So Canada and the U.S. would argue that, you know, they should get all the haddock and all the traditional groundfish because that was their history. And they got some preference in it, in the debate because they were coastal states. France got some preference because it had St. Pierre and Miquelon, which is a French territory off of Newfoundland…and…what's the other…another coastal state up there, Denmark has something up there…I can't remember what it is…Not Greenland, they have something else. Anyway, I believe…or maybe it was Greenland. Greenland borders on the NAFO…ICNAF/NAFO [Northwest Atlantic Fisheries Organization] areas as well. So, yeah, it was political.

The threat of the 200 mile limit gave the U.S. and Canada some leverage. So, you know, the, the Agency position, the Northeast Fisheries Science position was very much that this was all working, it was all exciting, lots of new science was being invented. And unfortunately, the politics and the pressures that had mounted from both the overfishing of haddock here and the rampant overfishing that was going on in the Bering Sea resulted in you know, the political forces saying, "No, we're going to go with this 200 mile limit." That was over the objections of the State Department and the Department of Defense and to some degree the Agency's view was, you know, at a personal level, people working here, was probably, we like going to Copenhagen or Rome, but at a higher level, the Department of State and the Department of Defense were adamant against it because it jeopardized U.S. access to the, you know, go through the Straits of Gibraltar, whatever it is, whatever the various locations that might become threatened by this.

The other thing that made the Agency and the government against it is there was a gigantic lobbying effort by the tuna industry that didn't want 200-mile limits. Prior to this, the Latin American countries had declared unilaterally 200 mile limits; Peru, Ecuador, and were actually seizing U.S. vessels that went within 200 miles. And the U.S. objected to that, and now had the U.S. ...

**JW**: Was that in the 1950s?

**MS**: Um, I think that began...

**JW**: Maybe post-war...

**MS**: No, no, I think, [in] the immediate post-war era those countries, you know, didn't… weren't going to seize anything. No, it was, it was into the late '60s and into the '70s.

**JW**: Okay.

**MS**: Um, I think Peru and Ecuador declared their 200 mile limits probably, you know, like 1970 or something like that. You know, five, six years earlier. And the U.S., you know, adamantly rejected those claims. And so that's why the FCMA, when it first passed, excluded highly migratory species. It said, these are highly migratory species, they're, and invented

that term as far as I can tell. They, they weren't, you know, they didn't belong to any nation because they, they didn't spawn in your continental shelf or whatever, they didn't, didn't come out of your estuaries. And so that was the, sort of the logic behind it.

That changed when it was clear nobody else was going to take that position but the US. And the U.S. got really fed up with Japanese long lining in the Gulf of Mexico right off shore for things like bluefin tuna and other things. So that, so that change occurred where the Magnuson, or the, I'm not sure what it was called then, but the Act was extended to cover tunas, sometime in the early 1980s, I don't remember exactly when.

**JW**: So before that time there was an exemption whereby a foreign nation could actually apply to fish within the United States...

**MS**: You didn't have to apply, they just steamed in and did it[, for Tuna (HMS)].

**JW**: Okay.

**MS**: They were subject in the Atlantic, they were subject to ICCAT, which is the International Atlantic Tuna…International Commission for the Convention of Atlantic Tuna. So they were subject to ICCAT, the U.S. was a member of ICCAT and so was Japan and everybody else, but whatever the quota, well, until 1982 there were no quotas set by ICCAT. ICCAT just basically said, "You know, you shall limit the number of boats to whatever number of boats you had in 1950 or something like that." So there was, you know, a problem of rampant overfishing of particularly bluefin tuna with U.S. waters.

**JW**: As the United States was moving towards passage of the FCMA in 1976, to what extent was the U.S. Commercial fishing industry behind it or against it?

**MS**: Oh they were a hundred percent behind it, [except for the tuna industry]. They were, I mean, they were lobbying Gerry Studds in this region who was...Warren Magnuson's name is on it but everybody, including Warren Magnuson himself before he passed away, acknowledged that Studds was the powerhouse in the in the House of Representatives, whereas he was the Senator. And there are some people who argue it should have been named the Magnuson-Studds Act. So, yeah, they were…the industry was strongly behind it. New Bedford strongly pushed it and the Alaskan industry strongly pushed it. There wasn't much of a West Coast industry other than in Alaska (actually mostly boats fishing out of Seattle). There wasn't much of an East Coast industry other than New England. Basically, there was a big shrimp fishery in the Gulf of Mexico but it was regulated by the states, there was no federal involvement. There's still very little federal involvement other than Endangered Species Act stuff. West coast, it was, I mean, the sardine fishery had collapsed years earlier, there wasn't really much of a fishery--

**JW**: Right.

**MS**: --in the West Coast. So it was basically Alaska and New England, everything else was invented after the Magnuson Act. Or after the FCMA. So the industry was strongly behind it and used Congress. The industry also didn't want it regulated by the federal government which is why the councils were formed which turned out to be a really pioneering experiment in self-governance or co-management, you could label it either one; it's a bit of both. That was pioneering, there had never been anything like that before. There are probably very few

examples of it in any industry.

**JW**: What was the relationship like in the immediate aftermath of the, of the passage of the Act, between the New England Fishery Management Council and the Northeast Fisheries Science Center?

**MS**: Pretty terrible. It was, it was actually in stark contrast to the West Coast. In the north Pacific the, the agency worked very closely with the North Pacific Fishery Management Council to get the system up and running and basically to throw the foreigners out. In the, on the East Coast, in the Northeast, and the other Councils were almost irrelevant. They don't, they virtually didn't exist at the early days because there were no real fishing activities or issues. So in Alaska, you know, the Regional Director, they weren't called administrators then, was a guy named Bert Larkins who had been the head of the Fisheries Survey Division at the Center, he became the Regional Director and he worked very, very closely with the Council to basically come up with reasons to throw the foreigners out.

Now, one of the things in the first version of this, the FCMA, said is if there's a surplus, more than you can catch, you have to let the foreigners catch it. You have to allow access. And that was, again, something the U.S. international interests wanted. So in part the North Pacific Council came up with the idea of an ecosystem cap of two million tons, even though the quotas would add up to much more than that, because that was a basis under which they could exclude the distant water pollock fleets from catching more.

**JW**: So did their ecosystem cap include everything--

**MS**: Everything.

**JW**: --that would be taken?

**MS**: Yeah, yeah, and I mean, it was, there was no basis for it, it happened to be about what the U.S. might catch and it, you know, it gave, it gave some basis for some, some approach for how you could limit the foreign catch. So, I mean, the head of the Scientific and Statistical Committee [SSC] in the Pacific Northwest was the Science Center Director, so that's how tight the process was there. And actually I think that the head of the Scientific and Statistical Committee has been either the Science Center Director or the head of the Stock Assessment Program ever since in the, in the North Pacific. In this region, the Council, when it was first set up, said they didn't want any stock, anyone from the Center on the Scientific and Statistical Committee.

**JW**: Really?

**MS**: Um, they, at some stage allowed some guy who was an ecologist who wasn't a stock assessment person to be on it because he was a nice guy and didn't really--

**JW**: How did they prefer it to be stocked otherwise?

**MS**: Well, for a number of years they picked the…they mostly picked social scientists from what I could see. A lot of them which were aligned with the industry, they weren't necessarily biometricians…they tended to be cultural anthropologists who were charmed by small-scale fisheries in Maine, [people like] Atkins, [who]discuss[ed] self-regulation of, you know,

whatever, of the lobster fishermen, you know, they'd go out and break somebody's kneecaps if they didn't, if they were putting out too many traps.[1] I never thought that was a very good governance model, but that seemed to be what the SSC thought--

**JW**: It's a little draconian.

**MS**: --in those days. Yeah, yeah. And, but anyway, that was the early SSC. And then they just disbanded it all together and operated for many years without an SSC and essence saying, we don't care about science.

**JW**: During what years was that the case?

**MS**: Probably about 1980 through about '83 or something like that. Then in '83, approximately '83, or maybe it was '84, something like that, they formed a…they formed an SS…Science Committee of three people. One was a guy named Jim Wilson who was…

**JW**: From [the University of] Maine?

**MS**: Yeah. A buddy of Atkinson [Acheson] and, you know, basically didn't believe in fisheries management. Another was Guy Marchesseault who was a Council staffer…who was actually a graduate student with me, and I was the third member. They, and I'm not quite sure why that occurred, but I generally had a pretty good rapport with the Council, compared to anyone else at the Center.

**JW**: Do you remember who the members of the original Council were?

**MS**: Well, one was Leah Smith, who was a social scientist here in Woods Hole. Another one was a woman who's currently the head, I believe she's currently the head of Sea Grant in…at MIT…Barbara Hall?

**JW**: Oh, Madeleine Hall-Arber.

**MS**: Madeleine. Yeah, she may not…I don't know whether she was the first…on the first version, but she was a member back in those days. Jim Wilson was a member, may have been a chair. I don't remember any biologists to speak of. So it was pretty ineffective and useless. And then this group that I was a member of was formed and I guess the agency probably, at some stage, got the backbone to tell the Council they had to have an SSC, because the law says that, you know, says it's supposed to. And so they formed it but they wanted to minimize it and they were probably told they had to have somebody from the agency on it, or something, and I was the least evil of the people they came up with, I guess.

But that turned out actually to be quite significant in terms of where the process has gone since then. And so this was pre-197–…1987. I know that because of some events that occurred in my life in '87. It was sort of '84, '85, '86. And so we decided that we needed to…Guy and I worked together well. Wilson didn't care about any of the biology and we decided we needed to have a definition of "overfishing" if we were going to be continually arguing about what's going on. We needed to have a definition of "overfishing." And at the

[1] Correction: Anthropologist James Acheson teaches at the University of Maine. He is known for his work studying common property resources issues especially within the Maine lobster industry.

time the agency had something called, I believe they were Section 602 Guidelines. And I may have the number wrong, it's 602, it might be 603. The, these guidelines were based on a section in the Act, you know, Section 602, which talked about overfishing and you should prevent overfishing…talking about National Standard One. And actually referred to…no…the guide, the Act didn't…So it talked about overfishing and that the agency will issue some guidelines. So back in the '80s, the agency issued these 602 guidelines. By the way, some of this history, if you look at the report of the National Academy of Scientists National Research Council, approximately 2013, there's a report on rebuilding the nation's fisheries or something like that. It's chaired by Ana Parma and, and Pat Sullivan. And it…the first, the second chapter of that has, has a history of the Magnuson Act that I wrote. And it mentions these 602 guidelines and...

**JW**: I'd be interested in reading it.

**MS**: Well, it's readily available, I mean...

**JW**: I'll, I'll definitely look for it.

**MS**: Yeah. Anyway, so these guidelines came out and they talked about overfishing and they actually said overfishing was…we're not worried about growth overfishing. We're only worried about recruitment overfishing. And in the scientific literature there's a big difference between the two. What Beverton and Holt's classic work was all about was mesh sizes and minimum sizes of fish, largely aimed at addressing growth overfishing. Recruitment overfishing was some vague concept that, at some point, recruitment would collapse if you fished the stock too low…but it was a vague concept. There was no specific definition...

**JW**: Do, was it referring to a specific size of fish, or an age class that--

**MS:** No, it's just referring, the spawning gets too low recruitment will collapse. Didn't define "collapse," didn't define "too low," it's just the obvious reality that if you have no spawners, you have no recruits. But other than everybody agreeing that there is that point at the origin where that's trivially obvious[there will be no recruitment, the recruitment overfishing level]was unspecified. Now there was literature where people said, "Oh yeah, here's where it occurred." In the case of Georges Bank haddock, everybody said, "Oh yeah, it occurred when they got below 50,000 tons. Look what happened. The recruitment collapsed." But it basically was undefined. And it just led to some argument about whether it occurred or not.

And so to a large degree the Act was…the National Standard One guideline was unenforceable, because it said thou shalt not overfish. Overfishing shall not occur. But there was no scientific basis to define it. And so largely the reason that overfishing had occurred in New England over and over again for year after year was because nobody could say whether it was occurring or not. I mean, scientists said it was occurring, and somebody else said, no it's not. [redacted]

**JW**: So what were the...

**MS**: Nice person. But anyway, what happened is when this group was formed, Guy and I got together and said, "Hey, if we're going to, if we're going to do something useful, let's, let's define overfishing so we can stop this argument about whether it's occurring or not, and we'll

have, we need a quantitative criteria."

So, there was stuff that had been published recently by John Shepherd looking at different models for stock recruitment curves and he had talked about one of the parameters being set by the slope of the stock recruitment curve at the origin, which is now referred to as the "h" parameter. And in ICES, because of John Shepherd and John Pope, they were talking about fishing mortalities that were too high which were based on, if you take data on stock on one axis and recruitment on the other, and you take each individual point and you put it down there, they have a slope from the point to the origin for each point. And so if you take those slopes and you put them in order, you know, lowest slope to the highest, and you, what, what, they were talking about as the recruit, as fishing mortality is too high, is the fishing mortality associated with the slope at the ninetieth percentile, which would mean that 90% of the time if you fished at that level, each year class or each generation, would fail to replace itself.

Okay, the concept was pretty simple. If your population is going to be sustained, you know, humans we need two children per couple, we actually need about 2.2 because there's infant mortality and so on, but, that's the concept. So we started…Pope and Shepherd started talking about fishing [mortality]in that context. What does fishing do in terms of the mortality it's causing that relates to whether a population can replace itself or not. And they talked about overfishing is when there's a 90% chance you can't replace yourself. Under fishing, when there's a 90% chance that you can, and they talked about something called…what they actually labeled as FMED, when there's a 50/50 chance.

I then happened to be involved in some consulting in Peru in 1984, I do remember that specifically, where I started using these same models and approaches on some Peruvian stock, Pacific whiting, I think it was, and so I came back here and Guy Marchesseault and I started working on, well, let's define "recruitment overfishing" for New England stocks. And, you know, how about your recruitment, you're overfishing in terms of recruitment if your fishing mortality results in less than a 50% probability that each generation replaces itself. If it's less than a 50% probability it means eventually the stock's going extinct. That's got to be overfishing.

So we went on the road and we actually convinced pretty much everybody in the fishing industry that that's how to define overfishing. That when you…if you…if generations can't replace each other, I mean, how can you argue with that? So everybody agreed. We, I specifically did it without giving any numbers; I just sold the concept. I don't know whether Guy knew what the numbers were, I knew what the numbers were. So we got the Council to agree, and we got a new management plan that the concept was overfishing occurs if successive generations, on average, not each one but on average, can replace themselves with their reproduction. And that…that then became the underpinning of one of the groundfish amendments.

And then we started saying, okay, let's implement it, what does this mean? And we started doing the analysis in terms of what's called spawning per recruit, so we actually went to historical data for the three most important stocks in the region: cod, haddock, yellowtail flounder. I also looked at red fish, as I recall, and said what is the spawning per recruit? I mean, how much can you reduce spawning per recruit from the un-fished level, how hard to can you fish before you violate this criteria that on average year classes have to replace themselves, based on historical data. And we looked at his historical data and we calculated that…we described this as something called F replacement, the level at which the stock will

be replaced. This is, this is described in a paper that's Sissenwine and Shepherd…1987 I believe. Anyway we found that it was 30% for haddock and 20% for cod and yellowtail, turned out to be the level. You could actually reduce the reproductive output of a year class from 100% down to 20 or 30% and the...

**JW**: And still have them be viable?

**MS**: And you'd still have a viable population, on average. It wouldn't grow, on average, but it wouldn't go down. And obviously if you wanted it to grow, you'd fish lighter and you'd have more reproductive output. So, all right that's fine, didn't mean anything to anybody. And then we started saying, what does this mean in terms of, at this stage the only thing that was being regulated were meshes. What did it mean in terms of meshes? And did the calculations and said you've got to increase the mesh size for cod to seven inches. The mesh size at that time was about five…five and a quarter. It's now up to six and a half, so it's not far from that [seven]. And so the industry went ballistic and said, "No, we can't do this, this is horrible." And they, it actually got to the point where since they'd already agreed that this is the definition, the agency now had the basis to say, "Okay, you're overfishing. If the Council doesn't act, the Secretary will implement an emergency plan." So we sort of gamed the system to put the Council in a position where they actually, we thought, had to act. And that was, that was the whole strategy of it, to get an agreement on something that was operational and then take it from there. So...

**JW**: What kind of consensus existed in the scientific community for the new ideas about fishing mortality coming from you and from Pope and Shepherd?

**MS**: Well...

**JW**: Was there a hot debate?

**MS**: Well, it, the stuff that was being done at ICES, I mean, I criticized it in a paper that I wrote with Shepherd and Shepherd bought into the criticism and that was, the criticism was that they were focusing on this ninetieth percentile as being a threshold and that was way…that was outrageous, that would result in stocks collapsing. So that criticism was accepted in peer reviewed literature, that Shepherd co-authored. So it was accepted.

Guy Marchesseault and I also wrote a paper that was published in a Sea Grant symposium result that also laid all of this out. Teri Frady, by the way, happened to be the editor of that Sea Grant volume back in…God…1980-something. She was working for Sea Grant then before she was hired here. Sea Grant Alaska. Anyway, so yeah, it was pretty much accepted as, "This is reasonable."

And very quickly the idea of spawning biomass per recruit as a basis for overfishing definition spread and was used throughout the Southeast for a number of years…was used… was spread to most of the stocks here. Now it's gone up and down in its use for a variety of reasons, some of which are right, some of which aren't, but...And there was also work being published by Bill Clark from the Halibut Commission, who also did some modeling work that described spawning biomass per recruit as a basis for defining overfishing.

Now the difference between what Bill did and what I did, was Bill used it to estimate levels that were associated with FMSY, whereas we were, at this stage, FMSY wasn't a

consideration, it was where's recruitment overfishing occurring. So most of this stuff I came up with said recruitment overfishing occurs when spawning biomass per recruit is reduced to 20 to 30%. Bill's stuff said FMSY occurs with less fishing, which is correct. You can only reduce it to 40% or maybe 50%. And so what's happened now is essentially that 40 or 50% has become the reference level because the law in 1996 changed to make the standard not recruitment overfishing, which is a more intense fishing level, but FMSY.

So the evolution is, you know, the stuff that we did for groundfish here is fully consistent and part of this evolution to where we are now where spawning biomass per recruit is, is, um, routinely part of the definition of overfishing in terms of FMSY. So, yeah, it was pretty widely accepted. It just wasn't accepted by the, it was accepted by the industry in concept, because I didn't get any argument. It just wasn't accepted when they found out what it meant.

So anyway, the result of this was that the regional administrator, at the time, or Regional Director, as he was known then, was only acting, was a guy named Dick Schaefer, who was the Acting Director. He had been acting for several years, I don't know why they were unable to fill the job. Can't even remember who was in it before him. Oh, Allen Peterson had been in it. Allen Peterson had come down here to be the Science Director.

**JW**: Where had he been previously?

**MS**: He was the Regional Director.

**JW**: Okay.

**MS**: And before that he was the Director of the State of Massachusetts Fisheries. So, Peterson was here, Schaefer was there, and I worked directly with Schaefer to essentially, working with people in Headquarters say, "Hey, we now, we now can finally use the Act and make the New England Council do what it needs to do, because we've got an agreed definition of overfishing." And so we started drafting a fishery management plan. We basically did draft a fishery management plan.

**JW**: Was this the Northeast Multi-Species Plan?

**MS**: Well, it was to be the Northeast Multi-Species Plan, but it essentially put in place a…you know, a six and a half inch mesh, or something like that, and some other things but mostly that. And we thought, you know, this will, this will finally turn the…turn things around. And Congress interfered. Kennedy, Studds, Kerry, I'm not absolutely sure whether it was Studds or Barney Frank or whichever one was there then…one of the two. Pretty much, everyone in the New England delegation, but I remember specifically Kerry and Kennedy. And there were others, probably Gregg from New Hampshire, Snowe from Maine…[they] wrote to the Secretary of Commerce basically threatening that if the Secretary put this plan in place the agency would suffer. A veiled threat against the budget. And the Secretary of Commerce caved in and said, "No, we'll accept the Council's multi-species plan." I guess it was the multi-species plan. And so that got accepted in probably about 1978.[2]

I had left on sabbatical. I'd taken leave to go on a…what was called a senior post-doctoral sabbatical to New Zealand at that point in time, in '88. So I wasn't fully engaged in what was

[2]Narrator Correction: The NE Multi-Species FMP was implemented in 1986.

happening then. But anyway, they put in place this plan. But one of the things we did that was extremely important in the end, is the plan did have the definition of overfishing. And it set up something called the Technical Monitoring Group, the TMG, that said annually, the Council argued that what it was going to do was going to work even though all the scientific advice said you're five-and-a-half-inch mesh won't work. So they said, "Oh, we're going to put in diamond mesh. It's different."[3] The studies showed that that might help with flatfish but it wouldn't help with cod or something, I can't remember the exact details. So all the science said it won't work but they said, but Senator Kennedy and Senator Kerry said it'll work, so the agency accepted it.

But the agency insisted that the definitions were in the plan, because how could you argue with the definitions, and there'd be a Technical Monitoring Group to report whether the plan was working. And that Technical Monitoring Group essentially became the concept behind the SAFE [document] and the report on Congress on the status of stocks and all that. So it went in place and so from about 1988 onward, there was a report that came out and said, "Here's what you have to…the plan isn't working and here's what you have to do to fix it." And people gave advice every year on here's what you had to do to fix it. At that stage the plan only had closed areas and mesh sizes. Might have had effort. I guess it had effort, too.

So the group met, people like Ralph Mayo who was a key Center scientist who sort of disappeared. I'm not sure what he's up to now, but he was a really good guy, important guy in the process where on the Technical Monitoring Group. Eric Thunberg was involved. Steve Correia who's a state guy who was recently given an award by the Council for his…all his efforts. I think he may have chaired it. They all met and they came up with good, sound scientific advice that said, "Fishing mortality is still too high, spawning biomass per recruit is too low, you're violating the overfishing requirement. And here's what you have to do to fix it." And they said cut fishing effort by 50%, expand these areas, so we started to get into these debates about whether the areas were…this was occurring…yeah, after I'd come back from New Zealand I guess. I wasn't in the stock assessment group when I'd come back from New Zealand, I was involved in [more ecosystem work. I became the Director of the Fishery Ecosystem Division and spent a lot of time helping to launch GLOBEC].

Oh, this is just as an aside, I guess part of the reason I was selected to be part of this SSC that didn't really exist, I mean it was just the two of us working together, Guy and I, was I wasn't part of the stock assessment group. And about 1985 I had been…we reorganized based on an organizational proposal that I made to the Director. He asked me to do it. And I had intended to be, expected to be, the Director of the, what was to be all the stock assessment stuff, what is currently READ. And I think that's what we named it then. But instead there were multiple candidates, multiple people that the Director thought could lead READ and there was another division that the only person who was a viable candidate to lead it wouldn't move to Woods Hole and he wanted everybody in Woods Hole. So he convinced me that I should switch and become the head of the Ecology Division and stop being involved in stock assessments. And since I was an oceanographer by training, that's fine. Which was a very good professional move on my part, I mean, it gave me tremendous experience in a new realm and credibility in a lot of things.

But anyway, because of that, I was attractive to the Council system because they had steadfastly said, "We don't want any stock assessment people." And even though I was, had

[3] Narrator Correction: The Council proposed the use of square mesh instead of the traditional diamond pattern.

done stock assessments for years and so forth, they accepted me, which is, guys like Mike Fogarty get that sort of acceptance as well because they're…even though he's…I hired him to do stock assessments, they have a different label.

So we got this Technical Monitoring Group going. It kept reporting on how the stocks were overfished. The Council would constantly say, "Oh, well, we'll just close a different area. You know, we'll have more closed areas to stop the overfishing." And they'd pick areas that nobody fished. And as a result of that, we decided we had to develop an objective way of evaluating areas. And that became something called the Two Box Model, which was a box inside and a box outside that closed areas.

**JW**: When was this, about?

**MS**: Well this must have been after I got back from New Zealand, so it was '88, '89, '90, through the end of '90. And it continued, after I became the Science Director, it continued into, into '90…I came back in '96 so it continued, except going on this TMG with the…I think probably the Two Box Model finally came into its own after I was already the Science Director, because I vaguely recall sitting in my office as Director, not my office as whatever the hell job I had, I can't even remember where I was, with staff designing the concept of the Two Box Model with Ralph and the other key people were the social scientists; Eric Thunberg and John[Walden]…

**MS**: It's a nice model that said if you can't fish here, where will you fish in order to maximize your profit? Pretty common sense thing. And so they'd keep proposing areas that there was no effort in, and we'd keeps saying, "No, that won't work but if you take this one, we'll take account of where the effort will go and it's going to go to places where they're going to catch less."You know, it was essentially set up to create inefficiency because that's the way the Council set it up, or wanted to deal with things.

And so we kept getting into these debates based on this Two Box Model which John and Eric were primarily the architects of, as I recall it at least. Pretty sophisticated work, in those days. And each year we'd say, "No, that won't work, this will." And the Council would say, "But we think it will." And they'd go forward and continue overfishing and by about 1992 or so, that led to the Conservation Law Foundation suing. And they were able to sue and win the suit because there's a definition of overfishing and there was a Technical Monitoring Group that said you're overfishing. So that process that started with this funky SSC of me and Guy and Jim actually culminated with a basis for this lawsuit and so the Conservation Law Foundation sued and they won. That the Secretary was in violation of the law by allowing overfishing. And I believe they were given like two years, the settlement said, "Okay, the Council's got two years to fix this." That was probably about 1992.

[By] 1994, the Council had not fixed it. It was still…I was in Washington at that time as the Senior Scientist of the agency. But Eric and John and Ralph Mayo and Steve Cadrin

were…not Steve Cadrin…Steve Correia, were still doing their thing in this Technical Monitoring Group and reporting each year. And they…you know, kept reporting that overfishing is occurring.

So the Conservation Law Foundation went back to the judge and said, they haven't lived up to this consent decree, or whatever it was…some sort of legal instrument. And we want the judge to…the courts to take over the fisheries. Something like that. And so suddenly people started scurrying around. We have to do something. And the result was that a lot of people put their heads together in a hurry including me in Washington, some political appointees in Washington, [and]Steve Murawski[in Woods Hole], based on stuff that was coming out of the Technical Monitoring Group, and proposed the three large closed areas on Georges Bank. In New England, the Georges Bank area, the Nantucket Lightship and, I don't remember where the other one was. Because they were so large that…and they were prime fishing grounds, and they were analyzed, I guess, by this modeling method although I wasn't here so I'm not sure, as the response to the court requirement. It said this, this will really do something. Which it did, I mean it had immediate impact on reducing overfishing in those offshore areas.

**JW**: What had been the closed areas prior to the Georges Bank areas that were--

**MS**: Um, one of those areas--

**JW**: --identified then.

**MS**: One of those areas had been closed seasonally to protect haddock since the 1950s. And it may…you know, it was first one month, it may have been expanded to two or three months. There were no year-round closed areas. So that happened in '94. And then I came back to the Center in '96 and this Technical Monitoring Group was in full bloom with the Two Box Model. I guess the Two Box Model really came into being in…probably around that time…it probably didn't exist prior to me going to Washington.

So with that, year after year we proposed increasing the closed areas even further because they still were overfishing and…you know eventually that wasn't sufficient. And by approximately 2002…2003, there was another lawsuit, or an expanded one that not only was Conservation Law Foundation but it was Oceana, a whole bunch of others. And that suit again found overfishing was occurring, again based on the definitions that we'd agreed in about 1985. And the results of the Technical Monitoring Group, the whole apparatus. And I was in…I think I was still technically the Northeast Fisheries Science Center but I was also working directly for the agency head, developing the structure that resulted in the program with the…well, the research office of the agency, which I later became the director of. But at that stage I'm not even sure it formally existed.

But we spent, Bill Hogarth and myself and, whoever the Regional Administrator was then. I can't even remember who it was. Well, at times it was Andy Rosenberg…sometimes it might have been Pat Kurkul for part of it. We basically spent, you know, lots of time in District Court in Washington, D.C. negotiating and arguing over all these things before, in approximately 2003, we came to a…some sort of an agreement that set up…resulted in us having to put in place regulations with target TACs and realistic closed areas and effort reductions to end overfishing within some short period of time. That required all of the stock assessments to be redone in a short period of time which resulted in something called GARM

1, which I believe was in about 2004. By then I had left the Center. I was in Washington. But I remember, you know, basically instructing John Boreman who had become the Center Director, [and] was my deputy, in how to organize GARM 1 with Steve Murawski...

**JW:** What did GARM stand for?

**MS**: Groundfish Assessment Review...and Monitoring, or something like that. I mean there's a series of GARM 1, 2, and 3 that are all on the websites and all that.

**JW**: Yup, right, right.

**MS**: But they were invented to basically replace the SARC structure because we had to do twenty assessments at once. And, you know, they, they got done and this was probably about 2004 or '05, when new…the plan put in place, effort cuts that really counted, really started to happen. And from that point on the…virtually all scientific advice to stop overfishing has been accepted.

**JW**: And yet was overfishing still occurring at that point?

**MS**: Yeah, it was occurring at that point, and it's occurred since. But it isn't[because scientific advice was ignored in recent years.]

**JW**: And I guess from what you're saying that it had also been occurring from the very point when you defined "overfishing."

**MS**: Oh, yeah, it had been occurring, it had been occurring since [the] 1960's, probably, [even earlier].

**JW**: So there's never been a point when it has not been under the condition of overfishing based on the definition that was implemented.

**MS**: For some stocks. I mean, haddock, is no longer overfished based on those sort of things but some of the stocks have been overfished continuously since they invented the wheel almost. You know, that's exaggerated obviously. But, from about 19…2004 or [200]5 on, the Council has accepted scientific advice in the vast majority of cases, to end overfishing. It hasn't happened, because of other things, in part some of the advice has been wrong. It's been too optimistic. This whole issue of the retrospective patterns, that you've probably heard about. But you can't blame the Council from that point on [mid 2000s], in my opinion. They've accepted the advice. That's also about when I retired…which had nothing to do with any of this story. I just decided to do other things. So that's how we got to where we are in ground fish. It's...

**JW**: Can you say anything about the retrospective patterns, that you just mentioned?

**MS**: Well, they are a phenomena that's been known to occur in stock assessments throughout my experience professionally. That is…that systematically you either overestimate or underestimate the stock. And that's been observed in fisheries around the world for a long time. Historically, I thought that the reason behind it was that people were cautious in either diagnosing that a stock is collapsing because they're cautious…because people shout at them when they say it's collapsing. I mean, it's human nature. Or they're cautious to say it's…you

know…it's recovering very rapidly because they're afraid if they say it's recovering very rapidly, the fishing industry will want to fish it immediately and if they're wrong, they'll overfish.

So I've always thought that there's a degree of cautiousness or inertia that doesn't deal well with change when it's very rapid in either direction. And I think that's probably true. I think that probably does happen. But this is more systematic. It's widespread in the stock assessments and it seems to be always in the same direction, the stock assessments are too optimistic, the stocks turn out to be worse off. There are…there's been a lot of modeling and analysis that's been done that indicates some reasons that would occur. It occurs if catch is underreported which is possible. It occurs if natural mortality changes which is possible. Those sorts of things.

But by and large, all those have been investigated and various analyses have been done to try and correct for the problems, and none of those…none of those corrections turn out to be permanent solutions. In any one year, you can correct…I mean, it's sort of like fitting a model with one parameter, one data point. You can always get it right in any one year. But they never seem to work the following year. So the problem exists and it hasn't been solved. Right now it's jeopardizing the entire credibility of the stock assessments in this region. It's really, it's a very serious problem in terms of credibility, and it has adversely affected the management. There's no question about that. A number of stocks, you know, continue to be overfished because the advice that was followed turned out to be too optimistic. I mean I have some of my own personal theories as to why it occurs and they're really quite technical and all that. And so far nobody's come up with a reason.

**JW**: I meant to ask you before, since I conducted an interview recently with Emory Anderson...

**MS**: He was one of the key NAFO, ICNAF players.

**JW**: Yeah. And we had sort of briefly touched on natural mortality and how at a certain point in time, it was fixed at 0.2, quite often.

**MS**: Yup, yup.

**JW**: And I was wondering if you might be able to comment on the evolution of how, of how that number has changed or, if you'd want to say anything about natural mortality in relation to fishing mortality.

**MS**: Well, the key point about it is it didn't matter much until now. So, I mean, Emory probably recalled the, the cartoon that John Pope developed.

**JW**: Yup, yup.

**MS**: Created.

**JW**: Very funny.

**MS**: Yeah. That's actually, I've borrowed that from John with his permission, published it somewhere. It is published. [laughs] Um, but, John was a funny guy. I also published

something called the *Scientific Fig Leaf*, which was a quite creative idea about why certain types of models always fit, as a result of some statistical artifact that showed you could, that data could be random and the model would fit.

**JW**: Interesting.

**MS**: So he was a pretty astute person. Anyway, so natural mortality…I mean there were lots of methods developed over the years that related natural mortality basically to life expectancy. And they're pretty common sense things. And it just turns out for lots of temperate bottom fish, which are mostly what are being fished around here and in northern Europe, you know, the ICES world where all of this developed. 0.2 was a pretty good number that corresponded to things that lived about fifteen years. So that's where it came from. But Pope creatively said you know, that's true, but it really can't, you know…[and he] drew this cartoon. Basically, it's a, it's an okay number for cod, haddock, flounders, sole and all these things that are common in northern temperate waters that live fifteen years. I went off and started working in New Zealand on deep water species and found out I was working on orange roughy. They live a hundred and fifty years. Didn't work very well for them. So, you know, we had to develop other methods and so on.

But that's essentially how it came about and I would say that most of the determinations of natural mortality today, even today, are primarily simple rules that are related to lifespan. There are more complicated formulas. Daniel Pauly created one that takes account of temperature. But the temperature's almost always the same in these areas and he still comes up with 0.2 and John Horning has one that has something of, uses something out of growth parameters, but basically they're all pretty much the same simple methods based on lifespan or growth rate related stuff that relates, that seems to correlate with lifespan. The…there are very few direct determinations. I mean you pretty much the only way you're going to get a direct determination is a tagging study and there isn't really much feasibility for tagging studies that can estimate population size for large ocean populations. They work great in lakes.

It never really mattered because the methods that were being used basically converge to pretty reasonable estimates of population size. When the total mortality on the population cumulative[ly] got to be about 1.0to 2.0 units and the amount of the natural mortality for all the stocks that we were dealing with was generally half to one, anywhere from 20% to 50% of the fishing mortality, so if the fishing mortality and the natural mortality…if the fishing mortality is large compared to the natural mortality, whether natural mortality is 0.2 or 0.25,[it] doesn't make much difference compared to a fishing mortality of 0.6 or 0.8 or 1.0 in some of these things…So, in the world of lots of overfishing, natural mortality estimates, are a minor part of the problem. And that's the way I grew up in this stuff and the way Emory grew up in it, and John Pope.

What's happened now is we've…we're managing for FMSY. We were managing for recruitment overfishing before. We manage for FMSY. FMSY is…generally turns out to be about the same as natural mortality. So we've gone from a world where it didn't matter much, to it matters as much as fishing mortality. Then you start to make matters even more difficult for the science because sometimes we're managing to rebuild stocks and we [might]have to be at half FMSY, or twice, or half M…so now M is the dominant thing. So...

**JW:** So how...

**MS:** But we haven't gotten any better at estimating it. But it's become more and more important.

**JW:** How did that differ when you were managing for recruitment instead?

**MS:** Because recruitment overfishing occurs at a higher fishing mortality--

**JW**: Okay.

**MS**: --than FMSY. So when we were, when we were considering when does recruitment overfishing occur, it was usually twice M, two times M. Could be more than that. And we were generally trying to reduce from four times M to two times M. Okay? But now we're trying to reduce from, you know, a little bit over M to a little bit under M, and it becomes a much bigger part of the equation.

**JW**: Yeah.

**MS:** That the error in it becomes much more significant. So we've made the science harder. When you think about it, I mean the, the sort of underlying concept of this is that we're trying to estimate the virtual population, which is basically the sum of all the catches. And that's the minimum population size, right? It had to be at least that many fish because we caught them. And then we adjust it for those that we lost to natural mortality. So if we caught, you know, a thousand fish, and natural mortality is low compared to fishing mortality, then the adjustment might be instead of the population being a thousand which is the minimum, it had to be bigger than that, maybe it's 1,200. But if natural mortality and fishing mortality are the same, then no, it's 2,000. And getting natural mortality wrong by 10% is a lot bigger problem. And that you know, it's pretty simple minded. One of the problems that exists in all of this science is that I don't think it's enough, it's not as much the science as the managers that sort of expect scientists to perform miracles. They've never taken a course in numerical analysis and understand how errors propagate.

**JW**: I, as an historian, I stay with the humanities. [laughs]

**MS**: I mean it, but it is a really actually quite a fundamental issue is that a lot of what the scientists here are trying to do is to please a conceptual framework and a legal framework that's been developed totally ignorant of the limitations of data and the way errors propagate in complex systems where you're trying to estimate things through methods of numerical analysis. So it just, I mean, the simple concept that ending overfishing makes the science more difficult is something that I can guarantee you nobody that wrote a law to end overfishing ever thought about. Now I'm not saying they should say, "Oh, we're going to overfish to make the science easy." [laughs] That would be wrong. But recognize that it was easier, and it, it's probably going to be more expensive to get it as precise if you're not overfishing.

**JW:** So in what ways has the, I guess, scientific research sort of co-evolved with the FCMA as it's gone through its different iterations and reauthorizations, you know, in the last couple of decades?

**MS**: Well, the…first there's the issue of what's research and what's operational, you know,

how much is just turning the crank to feed the management beast that requires feeding. And I think that fundamentally what's happened is that research has taken a back seat to feeding the beast, to just updating stock assessments and collecting the data to implement national standard guidelines on [things such as] habitat [and] on bycatch reduction and so forth…We don't understand these things, much better. I mean, we don't understand habitat any better than we did before we had rules requiring essential fish habitat. We have a hell of a lot more data on describing habitat. We have no more, no significant increase in knowledge about why habitat's important or if it's important, so. [By the way, EFH is one key aspect or requirement of the law that ups the need for research, that is not included in a National Standard. It probably should have been, because this would have prompted guidelines to be written.]

We have a hell of a lot of data on by-catch. We haven't done much, we, we've done a little research on, a little gear research to reduce by-catch but we haven't done really very much to analyze how important it is in terms of population dynamics, overfishing, all those sort of things. It's just a lot more data. We have a lot more surveys that are responding to specific issues of setting quotas and so on, but, um, historically this region was pioneering in implementing surveys from the perspective of ecosystem surveys and that stuff has been reduced. Scientists spend much less time, or have much less time, to actually think about dealing with, you know, long-term scientific problems, or even, even improving their models to be more complete and comprehensive because they're so busy just going to the next stock assessment meeting to update things. But I think, I think there's been a very substantial loss in terms of the ability to be creative and think about things.

In the early days in the ICNAF days, in the early days of, of fisheries management and, you know, years, the years in Europe when they were inventing all this stuff to, the, the people that were involved in stock assessments would get real busy for, they'd have what they call the annual cycle. They knew that, you know, in, in the ICNAF case, the major stock assessment meetings occurred in June and then the Commission met and made its decisions in September. And there were some working groups that began meeting in April, so some people would be involved in these working groups in April and then they'd go to the stock assessment meetings in June and they probably, you know, the typical person, didn't go to the Commission meeting. So their work was sort of, three months real busy doing these things and maybe they had some prep time or something, but three, four months, whatever. The, the leadership would go to, maybe they went to the June meeting and then they went to the Commission meeting, so they were sort of, all their energy was taken up by this stuff for three months, four months.

But for the most part, the scientists that were involved in fishery management processes had a good deal of time between this annual, you know, when the lulls in the annual cycle occurred, to do creative things. To think about things to improve their methods, to write papers and so on. That doesn't exist anymore. It's continuous. There's, you know, there, there's one occurrence after another. You do a stock assessment and then you go to 50 PDT meetings. Fifty's exaggerating, but lots of PDT meetings in between. And sometimes somebody doesn't like your stock assessment so you have to do another one. And so on and so forth. So that the, so that there's a greater separation between the scientists involved in the fishery management process and the rest of the Science Center that's doing research because the people involved in the stock assessments are doing them full time. And they weren't before. They, because they're not working as closely with the people doing ecological research or whatever, they're less likely to incorporate it into their stock assessments and the ecological research is less likely to be relevant because they're not talking to anybody, you

know. So it's a real challenge to in fact have, have a program, and this spreads to the monitoring side too because they're all separated. And it, they won't...

**JW**: You mean at-sea monitoring?

**MS:** Yeah. At-sea monitoring, surveys, all of it. These, there was a much greater ability to integrate these things and to form a community of scientists that talked to each other and worked together. When the stock assessment people weren't working full time, you know, continuously at it, when they had their annual cycle, somebody came to me and, this is years ago, and, in June right before the NAFO meeting, or ICNAF meeting, and said I want to talk to you about this idea I have to study recruitment variability and how warm core rings off the Gulf Stream entrain fish. They told me that in June or May or something. I said, come back in October. And then they come back in October we might work on it for a couple months, that doesn't happen now because in October something else is happening.

**JW**: Yeah.

**MS**: So, so there's, there's more isolation, I think, of the stock assessment people. More frustration, burnout, less satisfaction, a real problem. When in fact, historically, those, those people within this Center were the most active people publishing. With all, with many of the good ideas, if you look, if you look at what was produced at the Center in the, in the '80s, you see that huge amounts of it are coming out of the people who are doing the stock assessments. In actual published literature. You know, me, Steve Murawski, Mike Fogarty, who are all involved in stock assessment stuff. And many, many others. That's become more and more difficult. I mean it still happens, with some of them, but it's, it's more difficult.

**JW**: So what is, what is this sort of, I guess, atomization then of, of research here, sort of mean for the implementation of ecosystem based fisheries management moving forward then?

**MS**: Well, I mean, the people who are pushing ecosystem-based fisheries management traditionally were part of the stock assessment unit. They were Steve Murawski, me, Mike Fogarty, you know, several others. And there were also people who weren't part of the unit, I mean there was always been Ken Sherman doing his thing, but, you know, we worked very closely together. In fact, when I left the Pop-Dy Group to go lead the Ecology Group it was actually because Sherman wouldn't come to Woods Hole and Peterson was the director at the time, felt that I was, I had a good rapport with Sherman so we worked together then and, and I took over the division that he didn't want at Woods Hole. So there was, there was a very good working relationship that, between these groups. John Hare seems to do pretty well with that. He's, he's managed to be in very good stead with the Pop-Dy people as far as I know. Tom Noji does pretty damn well at it, too, but I, I think there's just less, less connectivity and less, less cross pollenization than there was before.

There's also a... there's a resistance throughout the system to actually be bold about an ecosystem approach. And it's all sorts of things could change, and those changes are going to be, are scary in terms of the details of what needs to be implemented. Organizationally, they'd change things. The way the management is structured, they'd change things. The way the science is done changes things. The way we define overfishing changes. So there's, throughout the system, it's not just about science, there's, there's a, an inertia or fear of the unknown because it's unknown.

When…the idea of an ecosystem approach has evolved tremendously in decades. A lot of the…a lot of the early thinking was, was about trophic webs and so you have, in the 1970s the development of ecosystem models that are trophic web based. There was a bunch of stuff done in the North Pacific by a guy named Tivo Lavasto who was an Estonian immigrant, became a U.S. citizen I assume, who was at the Northwest and Alaska Fishery Science Center, who developed some models called bulk biomass[models], where in essence he said, we can estimate how many fish there are by, what, by what's consumed by the marine mammals in the Bering Sea. It was actually pretty logical stuff[that's an ecosystem version of VPA.]

**JW**: How did he plan to gather that data?

**MS**: Oh, do lots of estimates of how many marine mammals there were, we knew what they ate so we, we can model what the consumption was by the marine mammals and that gave us, it's really actually conceptually the same as virtual population analysis. But he was doing that work and trying to relate that in other data that he was gathering. He started to look at predator prey relations to develop models that would be ecosystem based to say let's catch this instead of that.

In the North Sea and in the Baltic, a guy named Eric Ursin, who's a Dane, was developing multi-species models, the Anderson and Ursin model is the well-known one published in the 1970s. They were very complex models that took account of all the food web dynamics. Here, we used to have Eric Ursin come and work with us all the time. We had Tivo and his people, [other Europeans like Niels Daan, and[Alaska] people working with us, including Pat Livingston who was, is, who became the head of the Stock Assessment Group there but was one of the young scientists in those days. We had a lot of cross pollenization work, working on ecosystem models. I put together a food web model of Georges Bank and it was published in about '82 or '84, something like that. So we, we had all these food web models but we generally took the view that they strategically might tell us what the balance between species are, but they weren't good enough to actually decide on what the quotas would be on a year to year basis. They didn't have predictive capability. And I think that's, may still be true, I don't know because we haven't tried.

**JW**: Yeah, I was going to say, is that permanently elusive?

**MS:** Well, it may be. I think it very well may be. But one of the big differences was that in those days we, we would often say, well, an ecosystem approach is what we should do. But we know what direction we need to move these stocks; they're all grossly overfished and if we had a ecosystem model that worked, maybe the target would be 100,000 tons instead of 150,000 or whatever the number was. But we're down to 10,000. We know what direction to go. We don't have to get it precisely right. And the ecosystem considerations aren't going to change that we have to rebuild these stocks. So that was one thought.

The other, the other important thought was, we're only managing a handful of them and we may pretend we're managing several, but in reality, we really only care about four of five stocks out of...

**JW**: The most economically valuable.

**MS**: Yeah. The ones that, the ones that have garnered a lot of attention, that the lawsuits are over, whatever. You know, haddock, cod, yellowtail, a few others. We're not…don't really care about ocean pout, we don't care about [skates], witch flounder…we don't care about window pane flounder. So, I mean, if, if you're not really serious about all those others, then there's, you can't actually be serious about an ecosystem approach. If you're only really focusing on these[few] and you don't care what the spill offs are to the others realistically, then it, it really isn't going to be an ecosystem approach anyway.

So now what's happened, we now have reduced fishing mortality substantially throughout the country. I mean even where, it's unclear how much it's been reduced here. We know we've cut fishing effort by a factor of two, four, whatever; hugely. It's got to have gone down. I mean, it just has to, regardless of what the numbers say.

**JW**: Sheer number of vessels on the water.

**MS**: Yeah, yeah. And but we're also trying to manage everything. I mean, virtually every species is subject to management and it's not just that we set a quota on it, we might close a fishery for scallops because of window pane flounder and so on and so forth. So we can no longer say, well, we don't, we understand an ecosystem approach is needed, but we know what direction to go and we're not really that interested in all these other things, we're really only interested in those. That's no longer true. We're interested in everything under the law. And we don't know what direction to go. I mean, we're, you know, some things we do but lots of them we don't, when you're dealing with all of them. So, so I think it's really does say, it's time to get serious, it's, you can't just, you can't just say yeah, it's a good idea but later.

We also, we also have a problem that we, we try to make, well the ecosystem approach, or ecosystem based fishery management, can be interpreted many ways and within it there are interest groups that are using it for their cause within it. So there's the social science side, which I won't even get into, but basically yeah, humans are part of it; sustainable communities, all that sort of stuff. So that's one, one part of it. Second part of it is habitat; what is fishing doing to the habitat. Third part of it is pollution; what is pollution doing to the fisheries. Fourth part of it is climate; what is climate doing to the fisheries. And the fifth part of it are trophic interactions; what's fishing for this doing to that. Those are all elements of an ecosystem approach. And depending on who's talking about it, they're probably only interested in one of them, or maybe two. And so you get all this, people talking past each other about what they're doing and how well we're doing and so on. I won't comment how we're doing on the social side and I'm not sure how you could measure it even if I could, did comment on it. The pollution side, um, I mean it's, it would be hard to envision that we need more regulations; they're there, now it's whether, you know, they're enforced, whether ships illegally dump and all that. That's another issue, but, but essentially the policy framework to deal with ocean pollution is pretty damn complete. I mean, it isn't good, I mean I don't think, I don't think it requires a lot of science, it's just that somebody needs to decide what industry to regulate. The habitat side, what is fishing doing to habitat, I think there's virtually no research. I mean I think there's a huge amount of, of work that's been done to identify different types of habitat, but I think we have miserably failed to conduct research that links habitat to fish productivity. And I think it's doable research in some cases, but...

**JW**: What do you think needs to happen there in order to gain a more comprehensive sense of what habitat means and what its importance is?

**MS**: Do research. I mean, but it's doable research. We tried to start it here a number of years ago but nobody wants to fund it because frankly, I think, the essential fish habitat theme in fisheries management, is not about habitat essential to fisheries, it's about preserving habitat. And people who, who push that theme don't necessarily want to know that it doesn't matter whether the habitat looks like this or it looks like that, the fish don't care. In some cases; in some cases they do. So right now our central fish habitat program is a program that says, protect habitat. Pretty much anything that fishing can change we don't want to change it. Don't change habitat, that's what our program is, not optimize habitat for fish production, okay? And I think the people in the habitat programs that are responsible for this want not to change habitat because...

**JW**: That preservation is the best course.

**MS**: Up until, yes, don't change habitat. So we haven't done any research on it to speak of, I mean other than identifying it, we haven't studied its functional value which is doable. I think you could lay out very good experiments to do. And the…I don't think anybody really cares [or] wants to know the…but in any case, we have a legal framework that's handled that. It's not, what more can be done in terms of the, in terms of implementing an ecosystem approach to protect habitat. We have, every plan has a habitat protection element that's been, you know, is completely vetted as you can think of. The, the climate element is an important one that we need to do a lot more on. So right now we talk about climate a lot. We… forever, I mean, you know, there've been papers published for fifty years on how recruitment goes up and down with the climate and various things. But we, the Magnuson Act itself is totally ignorant and very…basically doesn't mention that MSY changes with environment, is a, is a moving target. We've only, only recently talked about that as an issue within the National Standard One guidelines, you know, they acknowledge that. I don't even think those guidelines have been approved yet, and it doesn't say what to do about it, it just acknowledges that this is something to be considered. So policy wise, the agency has to come to grips with putting in place some framework by which you can make objective criteria based decisions as to when the reference points in fisheries management should be changed because the climate is changed. That's probably the easy part. I mean, I think scientists could figure out schemes for that. The hard part is we'll probably have to figure out how to change all the allocation schemes.

**JW**: As species move.

**MS**: As species move. And that's politically a bombshell. So no, I mean, I think we could come up with good scientific advice about what's going to move, when they're going to move, how you know they moved, you know, how you know,--

**JW**: Right, right.

**MS**: --it's not just a, not just a one-time phenomena, but it's a lasting change.

**JW**: Yup.

**MS**: And, but, and you could get, you know, social scientists, people help in decision making and complex social systems or whatever to contribute to a process of how do you renegotiate all these things.

**JW**: Now is the difficulty...

**MS**: But at a political level, nobody wants to acknowledge the problem.

**JW**: Yeah. And is the difficulty there partially due also to the political nature of climate change discussions and the, the validity of climate change--

**MS**: I don't think so.

**JW**: .--or is that not so much a factor in?

**MS**: I don't think it's much of a factor. Because, I mean, the fishermen acknowledge it, they're, they know it, they're, they don't necessarily know whether it's because of carbon emissions or whatever, they may, who knows what they believe on that, but they know the fish have moved.

**JW**: Yup.
**MS**: I mean, there's no question about that. Nobody's really disputing that. The policy makers, or the Fishery Management Councils themselves and Congress, I mean, what do you do when somebody says, "Well, black sea bass need to be managed by the New England Council, not the Mid Atlantic." The negotiated allocations of summer flounder that we negotiated based on historical catches which were mostly from North Carolina to New Jersey have to be shifted to New York through Massachusetts. You got to give up some fish and say, they're here now. I mean, that's, that's a political bombshell.

**JW**: And that's been an ongoing political battle between New York and New Jersey...

**MS**: That one, that one specifically is one of the loudest, but they're all, I mean, they're, there've got to be hundreds of those. I mean every day there's another one. So, so that's a big chunk of the ecosystem approach that people don't want to deal with. Again, it's not science-bound. So I think, I think that there's a huge part of the ecosystem problem that's not science limited and is gone about as far as the science side can go given what people need to start to address the problems. I mean, we could do a lot better to develop the framework for how you change biological reference points when climate changes, but I guess if you've got, you know, team of ten people from around the country and gave them, gave them a couple of months, they'd develop a pretty nice description of how to do that and a process and all of that.

**JW**: Do you think for meaningful change to take place there has to be some unified definition of ecosystem based fisheries management, since it seems like we're talking about a lot of disparate parts that all operate on their own basis?

**MS**: No, I think we should…I don't think the definition matters anymore. Definitions are just…I mean I've written several of them. Other people have written several of them. I don't think they matter anymore. They're all basically the same. I think it does matter to recognize that there are relatively separate parts. I mean this issue of habitat doesn't interact very much with the climate issue. The pollution doesn't interact very much with the habitat conservation, the trophic web stuff does interact with climate a bit and does interact with all of them, but, but, you know, there's so much to be done in each one that, that in some ways, the rubric of an ecosystem based approach has probably outlived its usefulness, we ought to just say,

here's what we're going to do about climate, here's what we're going to do about pollution, here's what we're going to do about habitat, here's what we're doing about social justice and so on. All of which, you know, there's stuff going on, lots of good stuff going on and the one that we actually don't know what to do about it are trophic interactions. And that's because we don't know whether the outcome of fishing this [gestures with hands], in terms of this, is predictable or not. We know they interact. We know obviously if you eliminate all the prey, they're not going to eat that prey. But what we don't know is whether they'll eat something else.

**JW**: Yeah.

**MS**: And whether it matters or not. So, so we do on the science side, we need to do much more on the predictive end of it.

**JW**: And that's an issue that the Mid Atlantic Council is now considering, right, with their unmanaged forage fish omnibus amendment.

**MS**: Yeah, and so is the New England Council with its ecosystem based fishery management plan. But after you've done that, the agency needs to recognize that the implication is that MSY for all of these stocks that we have biological reference points that are all defined in terms of MSY…for each individual stock, are no longer exist[ent] or [valid on their own]. They're not meaningful if they're trophic interactions. The MSY of cod, obviously, depends on how much, on what it eats. The MSY of herring obviously depends on what eats it. So the idea that you can rebuild herring to MSY that was estimated without any consideration of its' predators and vice versa is just ridiculous. There's an, there's an, there's effectively an MSY for the production of fish for Georges Bank and it's probably at its maximum related to solar, incident solar radiation and upwelling, an ecological process. And the species mix that's associated with it is probably a lot of small, fast growing fish with not many cod, or certainly not big cod, you know, and so on.

So you've got to come to grips with, okay, what does MSY mean in an ecosystem? It doesn't, it doesn't mean a bunch of individual MSY estimates for individual stocks which we can rebuild them all to at once. And scientists have been saying that for fifty years, probably. But the law and the policies, largely ignore that. Again, that's somewhat addressed in the, in the revisions of the National Standard One guidelines which are, again, I, had been out for review, I've commented on them, I don't know whether they've been agreed to or not. I mean, as of a month ago, they hadn't been. They may have been now. But all it does is it says that you may estimate MSY for a group of stocks but it doesn't say what does that mean in terms of the, do you, are you still required to define "rebuilding" on a stock by stock basis, which is the way the Act implements it, is implemented now.

So that, that's a hugely challenging area and, to me, this, this idea of a evolutionary or step wise approach to an ecosystem, to ecosystem based management which is what many people would advocate, is really just saying we're going to continue to do what we're doing, which is we're going to protect habitat because people want to protect habitat, and we're going to, if we know that climate has changed such that you just can't rebuild a stock we'll, we'll have a political debate about that and we may change the reference points, which we did on southern New England yellowtail flounder that we flipped back because different people were in the meetings so the answer changed. And, you know, we'll pay lip service to trophic interactions because that requires not only more science, but a huge policy change.

**JW**: It was one other thing that I wanted to ask you about here. Sort of, in going, I guess to the, more of a fundamental question about fisheries science, and it comes from a quote that I saw from Ray Beverton where he had, I guess, mentioned the supremacy of mathematics in overtaking biology within fishery science and how he saw that potentially as a threat. What, what are your thoughts there, in terms of the duality between--

**MS**: Oh, I...

**JW**: --biology and math when it comes to fishery science and how it, how it moves forward?

**MS**: Yeah. I, I think it's, I think it's become an increasing problem because of this isolation of the people doing the math from the people collecting data and involved in fisheries.

**JW**: The integration issue that you mentioned earlier.

**MS**: Yeah, yeah. I mean it was less of a problem when, when the people who were doing the stock assessments also would go out on fishing boats now and then, which they used to. And I'm sure there's still a few of them that do, because there's some, there's some people who really, you know, work hard at interacting and want to do that sort of stuff. So I guess some that do. But I'm sure there's some that never do. The people that went out and did stock assessments would always spend a couple weeks a year on the research vessels; they have less time to do that, I'm sure many of them would like to but they probably don't have time to do it. And they don't spend as much time talking to the biologist that actually rolls up their sleeves and cuts up fish and looks at their gonads and that sort of thing. They just don't do as much of that because they're, they're so bogged down. So I, I don't see it as much the threat of going from being a biologist to a mathematician, I see it as going from being a scientist to a full-time operations analyst and, you know, this, um, overlap between those two switches.

But it's not the mathematics. Mathematics to me is just thinking, you know, logical thinking that you can precisely articulate in some way, which is, which is always to be desired. But, but just logically thinking about some operational aspect of how we come up with a number that satisfies a legal framework or a policy framework rather than how we actually understand the truth, or how the system works, that I think, is the threat. And it has become very much, you know, it's not whether it's right or wrong, it's whether it's legal or not.

You know, you have a number of actors in this system. You have scientists, you have managers, you have fishers that are regulated, and you have all sorts of incentives being created for all of them to make decisions to have good science to obey the law. And other incentives to break the law, or less incentive to have good science but to feed the machine, or less incentive to have, make good decisions but make safe decisions where you don't get sued or criticized. I think the whole system needs to spend a lot more time thinking about how all these things interact and how the, what sort of incentives, positives and negatives, we create for the actors in it, and how those incentives ultimately play out in its performance. And I worry that, you know, we often create systems that create bad incentives, not good incentives. So.

**JW**: Human nature, maybe.

**MS:** Well it is human nature, but think about the human nature part of it when you make,

when you, when you decide on a, on a policy or a framework. I mean if you can enforce every rule then there's, then the incentive is a stick. But if you can't, I mean if it's, if the stick costs more than the fishery is worth, then it's not good public policy to create the incentive through the stick. And the, and it's pretty clear that, that you can't afford to enforce everything by having an observer on every boat with enforcement capability. And you also need to think about what that does then to, to the incentive to provide the data that the science side needs that's accurate, rather than biased.

**JW**: Yup.

**MS**: There are all sorts of these interactions that tend to get ignored at this stage.

**JW**: Well, we've, I think we've covered a lot of ground here, we've gone over the evolution of the Fishery Conservation and Management Act, organization of science in a mission oriented agency and ecosystem based fisheries management.

**MS**: Yup.

**JW**: So.

**MS**: Well, okay.

**JW**: Pretty much covered everything. Is there any, any other parting shots or last thoughts or anything that you want to=-

**MS:** Uh...

**JW**: --get down or anything like that?

**MS**: Nothing, I mean I...

**JW:** Anything career related going back again, or...

**MS**: Well, I mean, one thing that I find interesting right now, I mean, the Science Center is searching for a new Director. You know, I made a decision probably three or four years into my career when I was here in Woods Hole, that I thought the, the best career opportunity, best career situation or outcome that I could have and that most of the people that I was working with, was as the Director of one of these Science Centers. Because, you know, it was prestigious, you had flexibility to do big science, you know, you could go to a university and go get your little grants and have a few students and do whatever you did, but didn't have a research vessel. You never have the ability to actually influence policy in a big way, so that if, if you actually wanted to do big science that involved large-scale ecosystems, programs and exert leadership and be, be policy relevant, but not be at a total bureaucrat, actually be able to be part of the science with ideas and so on and publish a paper here and there. It was hard to envision a better situation than being the head of the Northeast Fisheries Science Center, or the North Pacific, whatever it was. Anything higher than that position, higher than that became too bureaucratic and too political and so on, it was just the right level.

And, you know, I was very happy to have achieved that. I'm not sure that I'm happy about it going beyond it at various times of my career because those jobs were not as much fun. But I

really get the sense now from not only here, but around the country that it's really regrettable that that's not the case anymore. The flexibility to actually use your good judgment to lead a balanced program that thinks about serving society right now and also investing in the future and mentoring people, is just gone because of the controls on the budget, the, the pressures, the political pressures, the workload, the deterioration of the research vessel capability, even though one of the things I was very involved in was getting the money to build the ships that we have today, but the way they're being operated, pretty much remotely from the Center, has really deteriorated the value of that asset in my opinion. So it's, it's, also the, just as a minor thing, the salary structure has changed so that there's relatively little financial incentive to want to be a Center Director anymore, I mean, there's just, just the way it's, throughout government, the salary structure has gotten too compressed. But it's really, it really is, I think, unfortunate that the, the nature of the science leadership is, in my opinion, really suffer, suffering tremendously because of a whole bunch of things that make the people who could probably be really great in those jobs just say, why would I want this? That's, that's really unfortunate. Because it used to be the most attractive job there was, as far as I'm concerned for, you know, for people who are interested in policy role and big science and so on.

**JW**: Okay.

**MS**: It's just too bad.

**JW**: Well, thanks very much for sharing your thoughts today.

**MS**: Okay.

**JW:** And for talking with me.

**MS**: Let me, as, on another subject...

**JW**: I'm going to shut the recording off here.

**MS**: Okay.

**JW**: Unless you want to get in one last question.

**MS**: I don't care one way or another.

**JW**: Okay.