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Fogarty, Michael ~ Oral History Interview

Joshua Wrigley

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

Interview with Michael Fogarty by Joshua Wrigley
Summary Sheet and Transcript

Interviewee
Fogarty, Michael

Interviewer
Wrigley, Joshua

Date
September 26, 2016

Place
Northeast Fisheries Science Center
Falmouth, Massachusetts

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Use Restrictions

Biographical Note
Michael Fogarty was born in 1951 in Fairbanks, Alaska where his father was stationed in the Army during the Korean War. His parents returned to their native Rhode Island when he was six years old. He developed an interest in marine science which led to him pursing his undergraduate and graduate degrees at the University of Rhode Island. Dr. Fogarty studied marine biology and earned his Ph.D. degree at the URI School of Oceanography. Upon graduation, he began working at the Rhode Island Department of Environment Management with a focus on the lobster and crab fisheries. He began working at the Northeast Fisheries Science Center on an exchange program and became a full time employees in 1980.

He began his career working on stock assessments of summer flounder, Atlantic herring, and American lobster. . He then worked as Chief of the Food Web Dynamics Group.. In addition to his time at the Center, he also taught at the University of Rhode Island and the University of Maryland. Dr. Fogarty provides a rich description of his work. In addition, he gives a detailed description of the complexities of ecosystem-based fisheries management.

Scope and Content Note
Interview contains discussions of: the history of ecosystem-based fisheries management, the impacts of the Magnuson-Stevens Act, intellectual conflicts between scientists and non-scientists

in fisheries councils, issues related to pros and cons of managing via single species, current issues with misreported catches

Indexed Names

Transcript

Joshua Wrigley (JW): This interview is being conducted as part of the Voices from the Science Centers project funded by the Northeast Fisheries Science Center. It's also a part of the Voices from the Fisheries project that is supported by National Marine Fisheries Service Office of Science and Technology. I'm Josh Wrigley, Project Manager of Voices from the Fisheries, and today I'm speaking with Mike Fogarty here at 15 Carlson Lane, where the Social Sciences Branch is located in Falmouth, Massachusetts. The time is about, I guess 1:30, maybe? Around there?

Michael Fogarty (MF): About two, I think.

JW: Maybe closer to two.

MF: Yes, it is two.

JW: Two o'clock. The date is the 26th of September, 2016, so we'll get started here right at the very beginning. Mike, when and where were you born?

MF: I was born November 19th, 1951 in Fairbanks, Alaska.

JW: Fairbanks, Alaska?

MF: Yes. My father was in the service. He was a West Pointer and it was during the Korean War and they graduated his class early because of the war and, of course, sent many of his classmates to Korea, but actually they sent a pretty sizeable contingent to Alaska because there was concern that, for example, Russia may become involved, that the war broadened and escalated and Russia was involved in some of the early instigation of issues in Korea to begin with. Later, of course, the international part of it became dominated by China rather than Russia, but that was the reason for them sending folks up to Alaska. My dad graduated early. My parents got married and they hopped in a car and drove to Fairbanks.

JW: Did you spend most of your childhood then in Fairbanks or did you come back to the Lower 48?

MF: Yes, we moved around a lot. I mean, the station in Alaska lasted three years and then we were sent from there to Fort Benning, Georgia, so quite a change in the climate. I was still so young that, of course, I don't remember the details of that kind of thing, then to Fort Drum and to other places. But after the war, my dad left, after having fulfilled his commitment, left the army. Both my parents were from Providence, Rhode Island, so they came back to Rhode Island and we lived in Providence for a while. Mostly, with one exception, in Connecticut for a brief period of time a couple of years. After the age of six, I was mostly in Rhode Island.

JW: Where did you end up doing your undergraduate education?

MF: At the University of Rhode Island. I became interested in marine biology pretty early on and URI, because of its' graduate School of Oceanography, had a good reputation, so it was natural to want to think about going there just because I'd heard so much about it and the reputation of the school in terms of marine biology.

JW: Had you had any formative experiences with the sea either in Alaska or in, well, I guess Alaska is going back pretty young, but in Rhode Island?

MF: Yes, for Rhode Island, of course, you can't get too far away from the ocean when you're in Rhode Island, so we did a lot of trips to the beach on summer weekends and stuff and I got interested in snorkeling and explore things from that kind of kid-level interest in it, but doing that so I kind of started right there.

JW: During your graduate studies for your doctorate, were you at the University of Rhode Island as well?

MF: I was, yes. I went from my bachelor's degree directly, at URI, into Ph.D. work, yes.

JW: Was that a small transition?

MF: I did take a masters degree as a stepping stone and then yes.

JW: Do an M.A./Ph.D. program?

MF: Yes.

JW: What were you focusing on for your dissertation?

MF: I was working in general…well, in my masters and the way I started out, I was working on sort of basic ecological theory. No connection with fisheries initially, but I wanted to do work that had a field component, a laboratory component, and a modeling component. So I was working on interactions between decapod crustaceans in Narragansett Bay, so, crabs, two species of Cancer crab, and lobster. In the course of that work, so I was essentially looking at competitive interactions between those species and what it might mean for their population structure and dynamics so that the modeling part of it tried to use some classical models of competition theory for that system and I was testing some of the mechanisms both in the field and in the laboratory from that.

It finally dawned on me as I would swim out to my study area and go past fields of lobster traps and everything else that there's another part of the story that I better think about because obviously these populations were being affected by fishing. I'd been reading all the kinds of classical ecological studies, many of which, of course, didn't deal with exploited species at all, and I had never taken any fisheries courses. I had taken both a lot of statistics and ecological modeling courses. So, I started to take fisheries courses. There were two taught by a professor at the graduate School of Oceanography, Saul Saila, who was a very well known and influential fisheries scientist.

JW: Did he work here at the Center?

MF: He's at the graduate School of Oceanography at URI himself, but he trained an awful lot of people that did come here, not only myself but others like Mike Sissenwine and a pretty long list of people that came on at a pretty formative time. People like Mike came in before the 200-mile limit was introduced and the Magnuson-Stevens Act came into force.

JW: Pre-1980.

MF: Yes. They were already working on problems and some of them in an international context with the International Commission for Northwest Atlantic Fisheries, but ultimately, of course, the whole focus changed once the Magnuson Act came into play and then we were dealing with

a different kind of set of demands. I didn't come to the Center until 1980, so they had already been working on the initial stages of providing management advice for the Magnuson Act.

Much of it, interestingly, had a really broader ecological component than the way things ultimately developed in the sense that while our center scientists prior to the Magnuson Act coming in were working both with colleagues in Europe through the ICES [International Council for the Exploration of the Sea] programs but also again in this ICNAF, International Commission for Northwest Atlantic Fisheries. In that context, the science center staff including folks like Brad Brown and Marv Grosslein, Dick Hennemuth, a host of others, had developed an approach for system-wide management, not just looking at individual species as became the norm later, but really to have a more holistic point of view that said basically there are constraints on the overall production of an ecosystem, marine ecosystem, that sets limits to how much the overall catch can be. They started from that premise, now ultimately in part because of the way Magnuson was written and in large part because of some of the ways it was interpreted legally as meaning it had to deal with stocks, which is debatable in terms of the way the plain language of the law reads, because sometimes they do talk about optimum yield or maximum sustainable yield for stocks, fish stocks. Sometimes they talk about for fisheries and certainly for multi-species fisheries, those are two very different things.

But they were working on problems that, in some ways, are now we're turning back to and trying to modernize and take to the next level in terms of management, because, as you may know, we are having difficulty particularly with the mixed species fisheries in the region, particularly in New England, for getting any kind of sustainable management. Even now after all these years, we still have for the multi-species groundfish management plan 60% of those stocks are classified as over-fished today, so using these classical single species approaches, we haven't found an approach that can really deal with that problem, which is predictable. My interest, because I was coming from certainly not a single species stock assessment point of view from my training, was really receptive to thinking about in the ways that people like Brad and others had been working in this direction I think more holistically.

JW: What questions had they been asking during that time period?

MF: They were essentially saying that because we have interactions among these species related both to predator/prey and competitive interactions or these potential competitive interactions, and that showed up because they were, right from the start of our trawl survey with the late most recent and ongoing series starting in 1963, looking at food habits data.. That was always designed as something that was not targeted only for the needs of single species stock assessment, but to look at ecological factors, so the food habits is one manifestation of that. They were also collecting oceanographic information while at sea and so on, so it was really a broad-based and visionary program in that sense. Whatever species we could get in the trawls, were all enumerated and weighed and measured so it wasn't simply the economically important species, which really put us in good stead now to be able to draw on that source of information.

But they were asking basically a question that revolved around the fact if you have a group of interacting species, how can you manage that complex. If they're interacting then if you try to manage them individually then you're putting up these management actions of individual species

at cross purposes, because if you affect through a management action one species that's interacting with another or another group of species, then it's going to have a strong impact on those other species depending obviously on the strength of the interactions. They're not all strong, but there are quite a number of interactions that we have particularly through predation that can be quite strong interactions.
So, in essence, they recognized from the beginning that you couldn't treat this as if you could deal with each species in isolation. One of the ways they were trying to do this to make it tractable was to look at the entire complex of species and see if it had properties that were the higher ecological level equivalent of a maximum sustainable yield for a system as opposed to a set of individual species involved. That was I think really an important innovative approach.

JW: How did that research focus change then as the legal framework began to shift with the introduction of the FCMA [Fishery Conservation and Management Act] in 1976?

MF: At least in part because of our connection with ICES, there was also the development and ongoing refinement of the single species models, so most of what was going on in ICES at that time was single species approaches and it kind of developed from some of the classical work put forward by scientists at the Lowestoft Laboratory, Ray Beverton and Sidney Holt. Even though in their classic monograph in 1957, they indicated that to treat species separately ultimately is not going to allow us to have a complete picture of what's actually going on. I think that they viewed it as being a tractable intermediary step. The consequence of their work and that of others from different scientists from around the world, but largely in Europe and North America, really started a fair bit of concentration on single species modeling and analysis. It really developed in that way and again, I think probably it's fair to say mostly it's because it's viewed as more tractable., They certainly realized the difficulties if you try to build up very complex models that have all the interactions explicitly recognized that it could become difficult to get the parameters that you need for the models and so on.

But of course as I said, this other approach that had been put into play here kind of sidestepped up that problem altogether by looking at things in an aggregate. Essentially what it's saying is that yes, there are species that are interacting but when you look at the aggregate, those interactions are subsumed in the totals. You're not necessarily having to account for interactions between cod and silver hake or whatever, but it's built into the outcome of the numbers that you're measuring over time, so they kind of, what I think was a clever way of trying to dealing, coping with the complexity of the situation, which obviously could become overwhelming. Even now, one of the main concerns you hear raised about ecosystem based fishery management is that it's too complicated. It's hard enough to do it for individual species and why could you do it for a whole complex of species together? But it misses this critical point that if these species are interacting, you can get actually a more stable and predictable response from a group at a higher level of ecological organization, what are sometimes called guilds or functional groups, than any of the individual parts which are going up and down for a lot of different reasons in terms of changes that affect individual species, but it's quite different for, in an integrative role or fact of having, looking at it more broadly at groups of species that are, because they are interacting, some of these highs and lows get smoothed out essentially when you look at the whole relative to what it is for each of the individual parts. So, one species may go up and cause another to go down, but when you average it all together or bring it all together, you have that kind of a

smoothing effect, so it's easier to deal with in that sense and that's still being true to the nature of the interactions.

JW: What have been the greatest advances in the last couple of decades that have allowed us to deal with that issue of complexity that you just articulated?

MF: A lot of it builds on developments that come not only from ecology or only from fishery science, but I mean a lot of the information on how one copes with complexity comes from a lot of different sources including some very important ones on the human side of the story in terms of how organizational structures work. Nobel Laureate Herbert Simon, for example, really paved the way for thinking about complexity in these systems and how one deals with it, whether it is a human system or an organizational structure, for example. Many of the same principles apply, obviously with different twists to them, but other kinds of systems, including natural ecological systems. They basically rely on concepts of hierarchical structures of complex systems. If you understand the nature of that structure, that hierarchical structure, you can take advantage of that to circumvent some of the problems that come about than if you tried to, again in this case, deal with the individual parts but instead of treating them separate, then try to find out all the exact nature of the interactions among them and all the mathematical specifications of how they might interact, you kind of sidestep that. Again, for some of the reasons we were talking about before. So, that theory had been developed by Herbert Simon. His contributions back in the late '50s and into the '60s, so it had been a longstanding issue of interest. It ultimately became a well-developed concept of complexity and complexity theory attributable to a pretty broad spectrum of different types of systems.

JW: When you came here in 1980, how did you wind up securing your first position with the Fisheries Service? And what was the first position?

MF: When I graduated, I worked for a while for the Rhode Island Department of Environmental Management and I was doing research that was both field and modeling again.
JW: Still on crabs?

MF: Some of it was. Some on lobster. I had done a lot of work on lobster particularly. Crabs at the time were, there was a fishery for them but it was far less important, of course, than lobster. I had been going and taking on other responsibilities in terms of some of the modeling for other species as well and in one case particularly for summer flounder. I became a member of a scientific committee that had Center representation on it and Mike Sissenwine was one of the lead players in terms of the supervision of the center scientists on this summer flounder committee. I was contributing to the modeling work, so he invited me to come to the Center on something called the Intergovernmental Personnel Act, which is an exchange program that's meant to go a couple of years at the most; probably two years at the most. I came in and was in the stock assessment group working initially on summer flounder, but for the center instead of for the state on this exchange program. They invited me to stay in a full-time position and so I did that. I've been here since then, so now 36 years, except for three years that I spent down at the University of Maryland teaching in the late 1990s. Yes, it really came about from my interaction. Mike had also graduated from URI, so I knew him; not well at the time, actually, but --

JW: Had he been a couple years ahead?

MF: Yes, he was, but he was one of Dr. Saila's students and a real shining light for us, and very quickly assumed high-level positions here at the Center. Yes, again, it was kind of these accidents of history. I mean, I think probably most of the people you've talked to already told you that they probably ended up in places that they wouldn't have really thought about that that's where they'd end up, just because life just takes you in different directions.

JW: Very true.

MF: As I said, I was in stock assessment and did that for 12 years, but there was a pretty vibrant Ecosystem Program led by Marv Grosslein, someone whom I deeply respected, and after 12 years and Marv had moved up in the Center, I took over his position leading the Food-Web Dynamics Program. Again, we were both doing fieldwork with trying to understand the nature of the interactions among these species, but also modeling it. So I kind of came again full circle to some of the things I had been doing in graduate school. Ever since then I've been more involved in the ecosystem side than on the fish population dynamic side.

JW: Who else was in your cohort when you arrived?

MF: At that time, I'm trying to think. I was the only one at that time that came into the Population Dynamics Group when I first arrived. People were coming in and kind of a sequence after that, so it wasn't as though it were a large cohort, but --

JW: Probably a better way to phrase it would have been who were the other people within stock assessment at that time?

MF: There are some people who are still there. Loretta O'Brien was there. A couple of others that have since retired. Most of them have retired, actually, now, but at the time it was people like, well, Wendy Gabriel was in the group at that time or a little bit later. She came from the University of Massachusetts. Bill Overholtz was one of my colleagues. Some of the more senior level scientists were people like Emory Anderson, who later became the Secretary General at ICES. Steve Clark, for whom our conference room is named in Woods Hole. I mentioned Brad Brown. Vaughn Anthony with whom I worked for a number of years on herring assessments.

JW: He had come down from Boothbay?

MF: He came from Boothbay, yes. Gordon Waring at the time before he went into protected species research was in the assessment world and, in fact, he convinced me to take over the herring assessment. But anyways. As I said a lot of the folks have either retired or moved on from when I came in, but there are a few. Then later others and some other URI grads including Mark Terciero and of course, Anne, my wife, Anne Richards, who you've already spoken to. She came in 1986, I think it was, to the Center.

JW: And you continued working in ecosystems?

MF: Yes. I got involved more in teaching also. I've always found the Center to be a good place in that way in terms of trying both to contribute to the broader scientific environment. In that case, it started with Professor Saila had retired and they weren't able to immediately get a replacement. So for three years, I taught his course, going back and forth from Woods Hole to URI. That was a good experience. When you're trying to teach something to someone else, of course, you learn that well, there's so many things I took for granted and I can't take them for granted, because I have to explain them to everyone. When I say things, I mean things like assumptions and approaches and so on.

JW: Were there any challenges in having to constantly move back and forth between the academy and then the environment of more sort of policy-oriented science?

MF: Not particularly I don't think. I mean I never felt that there were either any impediments to working in that academic environment or any disadvantages. I think people were pretty supportive of that kind of thing. Of course, it is a very different culture in terms of when you're working at a university than in a federal laboratory particularly, Because many academic scientists have to be independent entrepreneurs in a lot of ways to get grants to do this, and usually they may be working on grants that fund them and their students, or maybe it's some of their immediate colleagues either at their institution or at others, but it's not generally a big teamwork thing, whereas here, of course, it's the exact opposite. It can't work unless there's a good teamwork environment and because of the spatial and temporal scales that we're having to examine questions on. Obviously no one or even a small group of people could do it, naturally because you need to be doing it continuously to understand the changes over time. It requires having that right kind of infrastructure. So, it's different in that sense. I don't think I've ever really encountered some of the snobbism that sometimes comes about with "pure" versus applied science. Or, at least if people felt that way, they never usually said it to my face. [laughs] But, yes, I mean I think that if you're doing interesting work it also can appeal to your colleagues even if they're in a purely academic setting.

JW: In terms of the history of ecosystem-based fisheries management, in 1980 what was the state of thinking at that time? I mean what were people talking about EBFM [ecosystem based fishery management] in those terms? What was the scientific kind of conversation that was taking place then?

MF: To go back to this issue that I mentioned earlier about scientists here advocating of this more holistic approach. Of course, it certainly was not called ecosystem-based management or ecosystem-based fishery management, although that term, ecosystem --

JW: What was the language used back then?

MF: Well, the quite particular one for this approach that I'm talking about was probably most often referred to something like total biomass management, so you're talking about the biomass of all species. Underpinning this, of course, is a lot of ideas, not only of these aggregates that I was talking about but the fact that it was all controlled by energy flow. So, the amount of energy that's coming in at the base of the food web has a very important effect and sets the stage for

what the fishery production is going to be under a given set of environmental conditions. Yes, those ideas were certainly being developed and explored and it was understood from the start that these species are not in isolation; they're interacting for these biological interactions that we talked about but they're also many of them are being caught together. So, to think that you could set a separate target level for species A when it's being caught with species B and saying you think you have a different target level that you could control really isn't realistic. Fishers have some degree of control in terms of targeting different species, but particularly for a trawl fishery, it's really not feasible. So this idea of saying the maximum sustainable yield for cod is going to come at an F of .26 and that for haddock is going to come at .18 or whatever the numbers are. You can't really achieve that because you're catching them together so they have differential vulnerability to the fishing gear and that changes over time as the fishing practices change. So, at the end of the day, it's not a reasonable foundation to proceed from., I think that part of it was pretty well understood. The term ecosystem-based management had been used as early as 1969, as far as I can tell; probably before then. This total biomass approach, though, that I mentioned first really was brought forward as a concern in this area in a paper in 1959 by a scientist from the Virginia Institute of Marine Science, who basically --

JW: Who was that?

MF: His name was J. Laurence McHugh. I'd been talking about total biomass management, but he called for management *en masse* to basically say that because of these various issues I've just been talking about, because they're caught together and they interact, it doesn't make sense to think about managing them separately. That environment and those messages were out there pretty clearly for when the New England Fishery Management Council set up shop, starting really effectively in 1977, as they began to become operational. Initially, they really had just management plans for three species: cod, haddock, and yellowtail. A lot of the quota numbers, they were manage under quotas, but it was from a lineage that came from, those numbers came from this total biomass approach that had been taken, which was much broader than those three species, but they decided and the Center was providing information on the assessments for those three species to start off.

Almost right from the start, the New England Council formed a fishery management task force where they grappled with these issues. In 1980, they had a set of publications that kind of gave some preliminary outlines of what they felt, this management task force, felt was reasonable for management. It was quite explicit that single species, MSY [maximum sustainable yield management], should not be used. Among the members of that first, well, there were quite a few members, but there was initial overview document that was put together by the task force and the leaders of that, the authors, were people like Dick Hennemuth, who at the time was the Deputy Director here. Brian Rothschild, who you know had a long career both with the federal government in National Marine Fisheries Service, but also in academia at the University of Maryland and later at the School of Marine Science and Technology [University of Massachusetts Dartmouth]. Lee Anderson was an economist who was recently the Deputy Director for the Mid-Atlantic Council. So, people that have been very active since then and involved in different aspects of this, but yes, they were quite clear that this idea of treating it as if you could deal with each of these species in isolation was not a tenable premise. Eventually, ultimately, of course, that's the way things did develop, that since that time despite this warning,

they progressively added species to the mix that the New England Council was dealing with, but again, always single species assessments.

JW: So, the Council at first, when they received this paper, was supportive of the ideas that it contained of taking a total biomass approach?

MF: I think so, although I have to be honest and say that I was not part of the meetings and dialogue at that time, but there is a book by the first Director of the New England Council, Spencer Appolonio, and Jake Dykstra who was a prominent fishing industry representative. Jake is from Point Judith, founder of the Point Judith Co-op and actually one of the drivers behind getting the U.S. to adopt the 200-mile limit to get the foreign fleets off our coast. In 2008, they published a book about the first decade of experience of the New England Council. They were very clear about the problem and also their views that from the start, it couldn't work doing it by the single species approach.

So, there are some histories like that and there's others by social scientists including Margaret Dewar who pointed out some of the difficulties that were faced trying to grapple with this management problem and coming in, too, for the really obviously, at that time, all the councils were taking baby steps in terms of how a federal program of fishery management was going to work, but it was particularly acute here in New England because so much damage had been done by the distant water fleets. They came, showed up off our shores starting in 1961 with fishing power that was so immensely greater than anything our domestic fleet had, that they just went in and sequentially depleted quite a number of stocks so that within four or five years people here at the Center were estimating that they had removed almost half of the biomass of at least of the species that were vulnerable to those types of fishing gears. So it was a pretty massive intervention by the foreign fleets. Finally, the impetus for the 200-mile limit and also the Magnuson Act was this idea of getting the foreign fleets off our coast. Because then, at that point, we had a 12-mile international border so they could come in almost everywhere and they had a very strong impact on places like Georges Bank and that kind of thing.

In many ways we're still grappling with the aftermath of the effects of that huge impact. But it certainly colored the issues that the New England Council had to deal with because they were dealing with now a system that been pretty badly overfished because the levels of removals definitely were not sustainable.

JW: What are some of the biggest drivers now, I guess, in pushing us back toward a more holistic approach reminiscent of what you were describing, it sort of characterized the early days of fisheries management here?

MF: I would argue that it's this accumulating evidence that the single species approach hasn't been working that well and it's particularly evident, I think, in New England for some of the reasons I was mentioning where you've got for that one management plan that covers 20 stocks, 60% of them are overfished still as of today. I think it's that recognition. The recognition that we're dealing with the other part, and we haven't spoken about this, but it's also obviously been appreciated that environmental change and of course the main concern we now have with climate change in particular, long-term low frequency change, is increasingly evident as a driver in the

system and now many of those single species assessments didn't deal with that part, so the natural side of it in terms of physical forcing was not being taken into account, nor were the interactions among the species. I think there's a recognition that what we had been doing, I mean we're supposed to provide optimum yield and there's no way you could really think that you're getting anything like an optimum yield when you're ignoring all these factors. You're trying to say what you think the optimum yield from each individual species is, but because you're leaving out big parts of the equation, what you actually get could be wildly different than what you'd really like to achieve. So, I think that's a big part of it, but the big impediment is still this belief that it's too complicated to do, which brings us back to what I was talking about before about not falling into the trap of saying we're going to have to have these immensely complicated models that we have to populate with parameters, but instead to take advantage of emergent properties of ecosystems that are more stable and predictable and then your task is to make sure that that stability and resilience is maintained. If you do that, then you can, the rest in one sense can take care of itself. That's the angle that our group is trying to deal with and trying to explore and say can you actually make that work? Where are the potential danger points where you could go wrong? Could you exacerbate problems?

JW: You mean you might cross the threshold of some sort?

MF: Yes., It's pretty clear that if I said well, we're going to set up these functional groups. and Let's say we're going to define them by their role in the ecosystem and that also reflects which species are caught together. So, in that case, large cod and large spiny dogfish would both be classified in the piscivore guild because they can be caught together and they're both top-level predators when they're larger sizes, but in this case, the price per pound and value is so radically different for those two species that in general if we just said okay, you can take as much piscivore biomass up to this point as you want but not put any other constraints in place. And the most valuable ones like cod would get, to the extent that they can direct the fishing effort, would really be hammered. Dogfish they'll catch, but actually because of some of their characteristics, they have a much better chance of surviving if they're thrown over the side. They don't have a gas bladder and all these other things and much thicker and heavier integument to protect them.

JW: Trauma of being brought onboard.

MF: Yes. Not that there's no damage done. So, you get an imbalanced system which I think in practice is what actually happened the old way, having it single species but ignoring these different things. Part of what we're trying to do is also to make sure that we find ways to not only protect the individual species so none of them can get into a position where the ecosystem structure is imperiled, but also to think about what are the different kinds of incentive or disincentive structures you can have put in place as part of a management. For example, you might need to put in extra protection for species that are more valuable because they're more likely to be targeted otherwise. That kind of thing.

JW: Would various forage fish species, I guess, fall into that sort of category?

MF: Yes, but in a different way. That's a really interesting question, because they do have direct value in the fisheries, although their price per pound for herring and mackerel and others is much

lower than for many of these piscivores. But because they're so important in the food web, including for the piscivore species but also for others including some whales and birds and so on, that there's an issue that comes into play in terms of whether there's an intrinsic value that does translate to a dollar value of keeping a good healthy stock of them in the water instead of extracting them. That kind of thing.

But it raises an important issue too that we've run into when we were doing the modeling that it makes a big difference if the objective of the council is to have a biomass or weight centered objective or dollar value objective in terms of both how the different ways that that would involve fishing effort being directed in different areas of emphasis for different fleet sectors and so on. It makes a hell of a difference in terms of what those objectives are and almost invariably the goals that are set by the Fishery Management Council don't explicitly take into account economic value directly, they take into account biomass or weight-centered metrics.
Here you're surrounded by economists and social scientists who could tell you that well, if you do it that way, then you're missing a big part of the story because you're leaving out all the incentives that the human part of the equation is driven by. We're also trying to rectify that by bringing the social and economic dimensions to the problem in at the ground floor in terms of the ecosystem-based fishery management because we really want to do more than just pay lip service to this idea that humans are an integral part of the ecosystem, which is a variant on that phrase you'll see in almost any definition of ecosystem-based fishery management.

JW: Is there a lot of debate right now as to the extent to which humans should be considered a part of a natural system? Is that sort of a larger conversation going on?

MF: I think it's understood that if we don't think about it in those terms, not only will we do a disservice to the ecosystem, but also to the human communities that are dependent on these resources. I do think it's fair to say that sometimes it gets honored more in the breach than the observance in the sense of actually bringing it directly into direct consideration in a management plan. For example, a lot of the economists here might be involved in doing a cost benefit analysis after a set of regulations had been put in place. So then you've already set your objective and it's to extract a certain amount of yield and so on and then they're looking at the cost-benefit ratio, but if the decision had brought the economic and social part of it in before the strategy was actually settled on, then you see you'd have a very different type of a system and different kind of a result because you would have gotten it in on the ground floor instead of after the fact, which is in essence I would argue is what actually happens for the most part.

So, we're working with Geret DePiper and others here to bring in some of the ideas, one of which came from Steve Edwards, who is one of our economists many years ago and unfortunately who passed away from leukemia at a young age, but he had been thinking about these stocks of fishes as the equivalent of dealing with the portfolio if you're a financial manager. So, now you've got a bunch of stocks, fish stocks, as opposed to fiducial stocks, and how do you actually take advantage of the stabilizing mechanisms? Of course, as you know, what you're trying to do when you have a mutual fund is to manage a bunch of different stocks, but do it in a way where the risk is kept under control by certain choices of which companies you have in your portfolio. So, you don't want them all going in the same direction because if you do, you might do well for a while, but if they're all marching in the same direction then they

could all get into deep trouble together. Whereas if you have a balanced portfolio of species, I keep saying species, but that way we're --

JW: I get your meaning.

MF: Then if you deliberately pay attention to that part of it, the stabilizing effect overall that I've been talking about before can be framed in terms of portfolios of fish and maintaining a robust portfolio becomes an objective. We're trying to build those ideas, so they build on economic theory and financial theory, but they also can be framed not only in terms of biomass but in terms of dollars. We try to do it both ways.

JW: Since you've been here from the very early 1980s, what are some of the greatest organizational shifts or changes that you've seen at the center? Just in terms of administration and the way science is conducted and things like that?

MF: Of course, there have been a number of reorganizations along the way, different division structures, and as you know, we're just going through one now that is combining two previous divisions. I think an important and, to me, concerning change that's occurred is that there are many ways in which the breadth of the science that we were doing when I first came here in terms of fundamental ecology and other aspects have been diminished. This is something that's shown up in other parts of the world in fisheries agencies, but it becomes the thing where everybody begins to think that well, it's the mathematics that rules instead of understanding the biology and the ecology and the sociology and the economics. For many years, I was a beneficiary of that because I was playing that game and well, I won't say I was playing it in that sense, but I was working in an area that was a beneficiary of that kind of view. As it went along and I saw other parts wither and die that I don't think should have because one thing I know for sure is that the mathematics shouldn't take precedence over the fundamental scientific understanding. I think that's a real problem because there are whole segments of the Center that have gone away entirely since I've been here that I think to our great detriment. For example, at one time there was an extremely strong program in terms of fish disease and epidemiology. So, understanding how those forces and factors, whether they're caused by natural pathogens or there was a lot of work being done on the effects of pollution causing for example different types of cancers, tumor genesis in fish.

JW: Things like mycobacteriosis in stripers?

MF: Yes, among them. We had some world leaders in that field, including Carl Sinderman, who at one time was the director of the Sandy Hook Lab. There were a number of people here who were involved. That whole dimension of our work withered and died as individuals who were the leaders in those fields reached retirement age, but we weren't able to get replacements and there were different priorities that were established, some of which meant that you're saying "well, basically, that we don't do science, we produce products for managers. If you don't have that commitment to the fundamental underlying science, we can't provide good products.
So, in this example I've just given, one of the problems with have with single species stock assessments is that it usually treats natural mortality as if it were a constant and yet we know that there are these epizootics that occur that can decimate and have done. Many species, the striped

bass story that you mentioned that became so prominent and important in the Chesapeake or there have been epizootics of herring that have decimated herring populations and so on and so forth. And another cause of changes in natural mortality are changes in predation rates and so on, so there are important parts of the story that have been dropped out so the assessments make these simplifying assumptions, but in that case, the consequences that, I mean, our assessment scientists are not estimating fishing mortality, they're estimating total mortality. That's the signal that you can get from changes in the age composition or size composition. Then they subtract off a term for natural mortality, which is usually an assumed value and the rest is assumed to be fishing. So, you could be really badly wrong about what you say the fishing mortality rate is if that term you're treating is a constant and subtracting off the total is wrong.

Another example is we had a very vibrant underwater research program, world leaders in many respects in terms of the use of submersibles and also --

JW: Benthic research?

MF: Yes. Not exclusively, but yes in large measure. They had underwater habitats that were being set out on places like Stellwagen Bank and so on, so there was a big effort in turning direct observation at sea, as you said, largely with bottom communities, but this team did a lot of pioneering work in the offshore canyons at the edge of the continental shelf often in terms of understanding those as ecological systems and why they were so important centers of abundance, for example, for so many species. That kind of thing. So, a lot of that is gone and I think my perception has been it's because there's this idea that we're just providing a service to managers by giving them stock assessments instead of also investing in the science.

So, when we've had discussions over the last couple years about priorities, the talk is almost always of doing things like cutting the science or cutting the monitoring programs and so on, and increasing this output in terms of product, but like I said, you can't have good product unless you have good science. So, I think it's a fundamental problem to be thinking in those terms. You have to have an investment in the fundamental science. I think another thing that happens is that because the pressure's become so strong, political pressures and elsewhere, that innovative thinking becomes squashed because now you're pretty much worrying about covering your backside and doing things that are the same old way of doing things, but there might be some perception that you're less likely to get sued or something if you do it. So, I think it's very different, I think, in terms of the atmosphere than when I first came here, in my opinion in that way in terms of the breadth of the actual science and investment in fundamental science.

JW: [brief pause] I'm trying to think of what we could cover. Did the experience of teaching in any way afford more latitude of inquiry in terms of your research and everything than working for the government or how did that sort of compare when you were down at the University of Maryland and URI?

MF: I honestly do think, Josh, when I first came, that there was active encouragement for thinking about things in a different way. I mean, you had a job to do. So alright, Fogarty, you're going to do this assessment this year or these two assessments or whatever it is. There was always the encouragement to be involved more broadly. For example, to be involved in scientific

committees that opened up different avenues. I mean pretty early on, by the early '90s I was involved in a fair bit of climate type research and particularly ultimately through this U.S. GLOBEC program, which was the Global Ocean Ecosystem Dynamics program that was funded by NOAA and NSF [National Science Foundation] to understand the impacts of climate change on marine ecosystems and ultimately I became the chairperson of that committee for five years. It's a national program that had, well, actually some international dimensions, but we had a Georges Bank program, we had a California current program, Gulf of Alaska program, and then also one in the Southern Ocean.

JW: What issues were you pursuing with GLOBEC?

MF: It was as science program, so we were trying to understand the underlying forcing factors in how climate might affect these different systems and we tried to do it in a comparative sort of way, so they were selected for different characteristics that we could contrast. Now, unfortunately, because of the expense and everything, they were anything like controlled and replicated kinds of comparisons, but because NOAA was a full partner in funding it, it was understood from the beginning that the idea was to have results that would be relevant to management. I mention that as an example. Later we had another program that I was also involved with and on the steering committee for it called CAMEO, so the Comparative Analysis of Marine Ecosystem Organization that was, again, jointly funded by NOAA and NSF.

Those are two examples of things that I've experienced that gave encouragement to exploring new things and to try to move in new directions and that kind of thing and not simply deal with producing, getting on a treadmill and producing one stock assessment after another. Now, in part because of the political demands and difficulties that I think, unfortunately, our stock assessment scientists who are very good have been notable as easily to get off that treadmill and part of it is because for different reasons I think but now when you get a stock assessment or read a stock assessment, well, the Gulf of Maine cod stock assessment, the last one, I think was, if I'm remembering this right, probably 600 pages long with all the tables and figures and everything. When I was doing stock assessments, they probably would never be more than 100 pages long. Just the sheer magnitude of pulling together all this information in a document that way, no matter how you slice it, it's time consuming. I mean you can automate it, but I think they're in a trap and part of it is I think, that again to be inoculated against lawsuits, having to provide more and more detailed information that's not scientific information necessarily, but detailed information about underlying data and so on and so forth.

JW: Do you think there are any issues with accessibility when considering documents that are that long and so expansive in their coverage of various supporting materials and considerations and stuff?

MF: Yes, to me it's an invitation to get lost in the weeds and miss the big picture. That's what I think. You're so wrapped up if you're producing one of those documents or even, god help you, you have to read them, and you're looking at all this endless detail and yet the childlike questions that should be asked are not asked. Like the childlike question of why do you think the natural mortality rate is constant or whatever it is? Or why do you think you can treat this species as if

had no connection with that species? Those don't get asked. That's where the thing is lost, because now you're just getting more and more detail like this that --

JW: Has there been any sort of evolution with how natural mortality is calculated? My interview with Emory a while back, he was showing me this funny cartoon that I forget who had drawn it --

MF: John Pope's?

JW: Yes. It was point two.

MF: Well, yes, to be fair, there have been and there is an ongoing effort in terms of folks from Pop-Dy [Population Dynamics] to understand the changes in natural mortality and it's had a somewhat checkered outcome. There was the herring assessment before last did have estimates of predation mortality that were derived independently from the assessment and they weren't used directly in the final assessment. But they were used to inform what was called the ramp-up effect of natural mortality that kind of mimicked the main features of the way it seemed to be increasing from the actual consumption estimates and it seemed to correct a problem in the assessment. You probably heard talk about this stuff with the retrospective problems in the assessments--

JW: Retrospective patterns?

MF: Yes, retrospective patterns. Where essentially each time you redo an assessment you can get a very different answer. So, basically, right now they're going up to the most recent year that they can and then they make an estimate, but then they can repeat that analysis and say, okay, what if you left off the most recent point but did it from the one previous and so on? Very often it reveals this big spread in the estimates if you do that, which means something is going wrong somewhere, because most of these methods are most sensitive to the information right at the end of the time series. The very part you need to know the best, have the best information, but many of the analytical methods are most sensitive to the assumptions that are made going in to setting what you're going to say the most recent conditions are for the stock size, for example.
This is a consequence of what's called an underdetermined set of equations, so you don't have the full information content you'd need to estimate all the parameters, including, say, letting natural mortality vary by itself in the classical single species assessments. There are multi-species analogues that actually do try to take care of that very question in terms of the natural mortality rate varying by taking into account not only what's removed by fishing, but what's removed by predators of different types. So, you need the food habits data, but it's much more demanding in terms of the data to do it. But like I said, with these underdetermined sets of equations, there's just not enough input information to allow you to estimate these other things. So, if you add in then additional suites of information including how much consumption of species X and species Y there is, but you can then have a way of approaching it from a different angle.

JW: If that extra data, then, become incorporated in, is that something that would eventually diminish the prevalence of retrospective patterns?

MF: That's one possible and obvious one that could. But there's some other things that can cause these retrospective problems including, for example, misreported catches or misreported, well, either the landings or the discards are both, which as you know, is something we're grappling with now because of everything's been brought to the fore with the Carlos Rafael case. So, there are a number of different ways that are understood where the problem could come about and it is a very legitimate concern about some of the data quality issues for the catch and discard or the landings and discard. But other things can change as well, including environmental conditions that could affect the growth rates and so on. Very often the stock assessment scientists say to us we're measuring the weights at age directly so if they change due to environmental conditions, we're taking them into account. As far as that statement goes, it's absolutely correct. However, what it leaves unsaid and sometimes the inference is drawn that, well, we're actually doing it because of that we're actually doing ecosystem management. But the flaw in the thinking is that what doesn't happen next is if you have those changes coming in that you're documenting, the management strategy doesn't change. They still apply the same reference points. So, in that instance, let's say for the sake of argument you had a decline in the weights at age, which we have seen quite a number of species, well, the consequences in how you would deal with it differ very sharply in terms of whether you had that change as being caused because you had a very high year class and there was density-dependent growth or whether there was decline in productivity in the system that caused the decline in the weights at age.

JW: In that scenario you'd have fish that have matured without growing at the same rate as they would normally?

MF: Yes.

JW: OK. Just want to make sure I'm understanding that.

MF: But it makes a big difference whether which of those two scenarios I just mentioned really are causing that, because for example, on one hand if it's the environment and a change in productivity, from my point of view, the natural thing to do is to say well, we've got to be more conservative in our management because we're not in the constant productivity regime, which is basically that underlying thinking of most of these stock assessments. But on the other hand if it's changed, and this is true right now, for example for haddock and some other species that are high biomass where the weights at age are much lower, then how you respond to that might be very different. I mean, basically you might say well, there's a rationale for us to increase the fishing mortality rate under these conditions for haddock because we'll ameliorate some of the problem because what's really coming up because of competition between, competition for haddock, individuals for food, and so on, so it's a diametrically different approach. So, I guess what I'm really trying to say is unless it actually takes it to that next level and makes a decision about how you're going to deal with what you think are safe and appropriate levels of removals, then you're certainly not doing ecosystem management. It is true that you are accounting for change --

JW: But it's not responsive?

MF: It's not responsive, right, and it could lead you in a very bad direction, because you could go along and say well, I'm still going to take this same level of fishing mortality, but I know that the stock is lower because of this condition factor thing, but you might need to really lower that fishing mortality rate, too. That's almost never done. I think sometimes there are these glib responses that are given, and that's one I hear a lot, that basically we're doing ecosystem management because we're taking that kind of thing into account. There's an element of truth in it, of course, because you are accounting for a change in productivity, but unless you follow the path all the way through to see what the implications for what you should do for management, then you're not really doing ecosystem management, in my view anyway.

JW: How do you foresee ecosystem-based management, ecosystem-based fisheries management developing in the next couple of years and what that might look like? I guess both in government and then also within the academy as well.

MF: I think it is, broadly it's going to be continued to be recognized as a change that we need to make, so there'll be that kind of support. I think that, well, the reply that I'm about to give is tempered by this recognition that people were saying many of the same things that I've been saying just now, 40 years ago.

JW: [laughs]

MF: So, what I've been saying isn't anything new. We are trying to find a new way to deal with it and to try to bring it into actual management, but I mean there's tremendous institutional inertia. But the challenge, I think, that everyone sees is that okay, how do you actually make it operational? One pathway we're trying here in New England is to take advantage of this hierarchical structures that I mentioned earlier in terms of system productivity and functional group productivity and that kind of thing. And actually to try to assemble it in a way that does give advice that can actually be used directly in management. For example, to say you can under these sets of environmental conditions and right now you could take so much haddock and so much cod, but not trying to do it in a way that says we are modeling all the parts that say it should be this much for cod and haddock, but rather to say we want to make sure that we keep the balance correct so that there's no structural problems that emerge in the ecosystem and we also want to do it in a way that we set constraints that this individual species are not driven below threshold values that would be chosen by the council and managers and stuff. But, we're not saying we can control them all individually. I mean, you can treat it as an optimization problem that says, okay, you're not going to exceed this overall cap level and you're not to let the parts go below a certain level. One of the things we're doing is to just say then well, given that, what removals could you take that would take that would meet those constraints, but also kind of minimize the variance overall in terms of what the fisherman sees in terms of catches so it gives them some stability in terms of their planning horizons and that kind of thing.

But, yeah, it doesn't pretend to say yeah, I can tell you to the second decimal point as we say now in stock assessment of what the fishing mortality on haddock should be. We are saying how much we think you could take out of the whole and how much you could take out of those upper level parts like piscivores and bentavores and whatever, but then the rest of it involves a strong dose of humility in saying well, you can't really control exactly how much the individual parts

are but we want to put it in a way that can provide enough of either the incentives or disincentives to protect the parts but realize they're going to be going up and down over time.

Personally, I think the wild card and I know it's the climate change and they're going to have to change anyhow because we're dealing with the same system anyways, because we've got species that are moving and pretty soon these catch allocations that people have, they're not going to be able to take.

JW: They'll be irrelevant in some areas?

MF: I think. I don't know how long it'll take, but it'll force a reappraisal. But it also keys into this and other related ideas that most of the theory of fishing is based on these concepts of equilibrium and I don't think we should assume anything about equilibrium.

JW: You mean the environment will assume a state of equilibrium?

MF: Yes. Because things are changing and they're being buffeted by so many forces. I mean, of course, there's rich literature that basically says that ecosystems are never truly in equilibrium to begin with and they're certainly not in anything like a deterministic equilibrium, which is most of the theory of fishing is based on deterministic equilibrium. This idea of MSY, for example, and saying it's going to ignore all the variability and there's just a single value cleanly defined that is going to give you an optimum yield and if you stray from that you take remedial action. For example, if you over-harvest but take remedial action, it's going to go right back to where you said it was and that's not the way the world works.

JW: At what point do you think MSY will become a term of the past? That we'll have an intellectual paradigm shift that recognizes the limitations of that notion?

MF: Well, as long as it's enshrined in law and it's mentioned specifically in Magnuson, it has to be dealt with. So, until it gets taken out, there'll always be an impetus for dealing with it. However, having said that --

JW: Because it seems like there's a shift in thinking, you know?

MF: Yes. I think probably --

JW. : Even though it's still codified.

MF: Yes. I mean, in the '70s there were a bunch of papers that came out just as the Magnuson Act was coming out, including one by Mike Sissenwine that basically said that you should never think about managing these species as if they were single species MSYs, but unfortunately, the Council has been, because they're not scientists, have been led to believe that there is such a thing. When in fact, the only way you could think about that being true for an individual species, is if you actually condition it on all these other changes that are occurring, not only with the other species, but with the environment.

I think to me that's a big thing, but I, again, a moment ago I said having said that, well, now I'm going to say, drop the other shoe, we actually have a lot of species that we manage where we don't have a real estimate of MSY. About 40% of the stock assessments that are done here at the Center are based, these index-based assessments that are really picking up on trajectories either from the survey or from the catch or both in different ways. And they're making a determination of the status of the species relative to the historical past, essentially. I mean, that's the way it works. But there are no models that are being fit that say here's an MSY level for this species. They'll say, let's say for the sake of argument, they might say that the MSY is the long-term catch or something like that. It's perfectly legal to do that, but it's not really any kind of an analytical estimate of MSY.

So, some of what I was just talking about in particular if you threw away the equilibrium concepts would operate in a world that basically didn't try to say you can estimate an MSY and that's it a real thing or anything, but we're still going to be able to give you a number because here's the decision rule that you're going to use. If it's legal for what we're doing now, then it has to be legal for this example that I just gave, you know? I guess what I'm trying to say is in reality there's more flexibility than might be implied by the law, but you still get people that defend MSY and I don't really know why, because it doesn't make any scientific sense to think that there's going to be an MSY for cod alone and an MSY for herring. What you do to cod is going to affect herring and actually in that case vice versa, not only because the herring are food for cod, but herring can prey on the eggs of cod. So, to think that you're going to set these reference points --

JW: Complex relationships.

MF: Yes. But to think that you're just going to set a reference point in isolation can't work, right? I don't know why it hasn't crumbled already, actually. [laughs] You know? Because it's been, like I said, it's been known, pointed out, for a long time. Anyways, you just have to keep tilting at the windmills and find your points of View.

JW: Any parting thoughts or anything? Or anything that we didn't cover?

MF: I think--

JW: If you do think of anything, we could always do a follow-up interview to cover it. [laughs]

MF: No, I think we've covered a lot of ground. I do think that change is afoot. I think that the burden of proof is definitely being put on those who would change rather than maintain the status quo, but that's okay. I mean, that's understandable. I think that here we're fortunate to be in a very data rich environment, so if you can do it anywhere, you should be able to do it here, make the change. But as I said, for a lot of good reasons and concern about change, it's a human nature kind of a thing in general, or at least understandable, that I don't honestly know what will happen. Like I said, when I look back and read these papers 40 years ago that said that it's a problem and I'm sitting here well, what am I --

JW: You think we'd be in a far different place?

MF: Yes, you would think.. Arguably the results haven't been that good using the existing approach for mixed species fisheries. if you still have, so many over-fished groundfish species. Probably the number for all the species that we manage is still around 25% overfished. I would argue that if you were in a business or in the military and you had a failure rate of something like 25% that it would force a change, you know, I think, this is pointing to something that's more systemic as a problem.

JW: Thanks for stopping by and talking.

MF: Thank you. A good time, yes.