June 14, 2016

Chambers, Chris ~ Oral History Interview

Bonnie McCay

Recommended Citation

Chambers, Chris. Interview by Bonnie McCay. *Voices from the Science Centers*. Voices from the Fisheries, NMFS, NOAA. June 14, 2016.

This oral history was produced in 2016 as part of the *Voices from the Science Centers Oral History Initiative* conducted by *Voices from the Fisheries* with funding by the NMFS Office of Science and Technology.

Voices from the Fisheries
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Interview with Chris Chambers by Bonnie McCay
Summary Sheet and Transcript

Interviewee
Chambers, Chris

Interviewer
McCay, Bonnie

Date
June 14, 2016

Place
James J. Howard Marine Sciences Laboratory at Sandy Hook

ID Number
VFF_SH_CC_001

Use Restrictions

Scope and Content Note
Interview includes discussions of: experimental design, Chris Chambers's work in Newfoundland, life cycles of mosquitoes, flounder, cod, capelin, nuclear power plants, individual based models, and contaminants.

Chris Chambers describes his experience with experimenting with different species throughout his career, and how this led to becoming involved with the Northeast Fisheries Science Center.

Indexed Names
Boreman, John
Buckley, Larry
Cooper, Keith
Cross, Jeff
Fahey, Mike
Fisher, Ronald
Guida, Vince
Lawrence, Jeff
Leggett, Bill
Morse, Wally

Olla, Bori
Reed, Bob
Studholme, Anne
Wirgin, Ike

Bonnie McCay: This is Bonnie McCay and I'm here interviewing Chris Chambers at the Sandy Hook Lab, or the J.J. Howard Lab of the Northeast Fisheries Science Center. So, Chris, let's just start with the questions about, well, the background questions, which in a way precedes the first question on the list, which is: how did you get into this kind of work? Let's just start wherever you want to start to kind of explain…

Chris Chambers: Yes sure, I was always interested in biology, as a child. In fact, my earliest interests were in snakes, and turtles, and lizards. I thought I'd be a herpetologist! I proceeded to go to undergraduate school in Kansas at Kansas State, got my Masters degree at Texas Tech, and a Ph.D. at Duke University. For my Masters thesis, I studied salamanders, not snakes, and turtles and lizards, because they became a more reasonable experimental animal than diurnal snakes and lizards, which are fascinating organisms, but lend themselves primarily to observational studies, and I was interested in experimental work. I went to Duke to work with an ecologist there who had previously done quite a bit of work on amphibian ecology, but was diverse, a quantitative ecologist, who worked on plants as well as amphibians. And with him, I began working on a salamander community study of vernal spring ponds in Duke Forest... looking at competition and predation as a mechanism of a structured community.

During my Ph.D. work, I stumbled across, serendipity again, through another graduate student there at the time who was studying the electrophysiology of mosquito hearing. She was recording electronic signatures of the antennae of males and how they find their females. She happened to be working with the aedes aegypti which was lab colony. But at that point, I was very much interested in organisms for which phenotypes and growth and body size are important in ecological interactions. Hence for amphibians, with an egg, larval period that is spent in the aquatic habitats, metamorphosis into a juvenile and adult, have a complex life cycle with ecological and morphological change at that metamorphosis that's very important in the ecology, and the community, and trophic web they live in. Once I saw the ease with which you can maintain mosquito populations in the laboratory, that is, you can go from egg to maturation in seven days and it goes through, of course, the egg and the fur instars, then the larvae, pupae, and the adult stage. Here's a complex life cycle organism with much easier husbandry requirements than even tadpoles and salamanders. For mosquitoes, you can feed them ground up rabbit chow and that was all they needed for their detritivore food niche. Mind you, tadpoles can also be fed fish flakes and rabbit chow, so they are also, retrospectively, very easy to grow in small containers, and therefore in the laboratory you can do fairly large, factorial experiments to study the effects of crowding or parentage on survival, growth and development, age and size at metamorphosis...things I was interested in. And both of those taxa, frogs, and toads, and salamanders, as well as mosquitoes, lend themselves really nice factorial replicated experiments on what are the sources and

contributions to phenotypic variation in sizes and development rates. So I began my studies and part of my Ph.D. on the, what's called, the “Tree Hole Breeding Mosquito Community”. So, I looked at three species of mosquito larvae interacting in tree hollows that are rain filled after heavy rain storms, two of which are detritivores, the third is a predator as a mosquito larvae. So you have a predator and two prey: the three species triangle food web which also lended itself to some nice experimental manipulations. Anyway, finished with my Ph.D., I was interested at that point in broadly generic topics like the role of body size or phenotype in ecological interactions. So, I was looking at broad enough taxa to include plant and animal biology, and I applied to various post docs and jobs at the time and one that caught my eye was an ad on the back of Science Magazine which was the only way besides Nature Magazine to find out about jobs at that time, this would be 1983, and there was a job posting for, I think the title on the ad was "Fish Ecology or Related Fields", and my view was “well, fish grow, I mean I don’t know much about fish but I do know they grow”, furthermore, even though I did not read, or was not fully aware of fisheries and fish literature, I realized it was a fairly distinct part of ecology. Different audiences, different funding sources, different clients, if you will, than people who study salamanders and toads and frogs. So, I applied for that position with a gentleman named Bill Leggett at McGill University and went up for an interview, and it worked out and I moved to Montreal in the January of ‘84 I believe it was. That first spring I prepared for our first experimental field season. At that time, Bill was overseeing two projects. One on freshwater systems in the southeastern Quebec and another one in Newfoundland on capelin. He had a couple of grad students and post docs working on this project already and I was stepping into the post doc position. Interestingly, in terms of dynamics in the lab, Bill’s ability to bring on post docs was directly linked to the fact he was the chairman of the department, and the department gave him the funds to hire post docs, and basically continuously, to help his lab maintain their research. So, Bill was not only a great administrator, by the way, but also maintained his role in science, he was with us once a week in the biology department. And just to finish the Bill description, we're still close friends, but he kept advancing, he became the Dean of the Sciences at McGill and became the Vice Principal at McGill, and became the University Principal, the President, they call him Principal in Canada, at Queens University in London, Ontario. So he had a great career as an organizer and an administrator, as well as a scientist, and he's still active in the publishing world. Anyway, so it wasn't until that time in the spring semester of '83 that I had my first contact with the fisheries world, as we were talking about earlier today, at that point, I mentioned a few minutes ago, I was not even familiar with the fisheries literature, I didn't know the name Bill Leggett from John Doe, yet this is an important person in this field.

BM: Ya, you struck gold

CC: I did!...I did not even, I wasn't aware of the different journals as well as their rankings in the fisheries world. CJFAS was just a blur of initials for me at that time, Canadian Journal of Fisheries and Aquatic Science, marine biology MEPS, the Fisheries Bulletin.. those things were journals I never looked at which was really interesting, as I said in our discussion earlier today, I would have considered myself doing mainstream ecology, i.e. ecology, ecological monographs, evolution, those kinds of journals. So this was an interesting transition for me, a

lot of opportunity. At the time, I had finished my Ph.D. I was interested in not just in phenotypic plasticity, but also in quanti genetics, so doing breeding designs to get a handle on the heritable, non heritable parts of phenotypic variation, sizes, ages and sizes of metamorphosis, growth rates survival and so on. One limitation with doing this with mosquitoes, females are not that fecund, they don't have that many eggs, and the mating protocol for mating mosquitoes, of course you want to know who the parents are, so it's a one on one mating by plan. Believe it or not, you decapitate the male mosquito and you put the female ovipositor or her claspers near the male's, and they mate and the eggs are fertilized, and then the female lays eggs on substrate, in this case it was paper towels above a water pool, which mimicked a tree hollow. So the shortcomings of that methodology i.e. limited number of eggs per female, limited number of matings you could get done in one day, sort of constrained the quanti genetic designs, and typically we are looking at half sib designs where ideally one male mates with two females, and you want to get about twenty or more families. That wasn't really practical with mosquitoes, so my mentor at McGill, Bill Leggett, his program spent summers in Newfoundland and they were working on capelin at the time, which is an important forage fish for Atlantic cod, whales, seals, it's the underpinning of the trophic web and that part of the Maritimes of Canada, and it ended up being a great resource for us because capelin like grunion on the U.S. California coast, capelin is an osmerid is a smelt that spawns in summer, third week of June to maybe first week of July, it comes up on the beaches near tidal zone and spawns in six inches deep of water during the wave surge so you go out there with a dip net and a bucket and collect a hundred ripe, running females and ripe, running males and use them as parents and do what you want to do with them. So that was great and, of course, the experience in Newfoundland itself was wonderful.

BM: Where was that by the way?

CC: It was in Conception Bay, a small community called Bryant's Cove which is near Carbonear is the main town nearby but the western side of Conception Bay and the Avalon peninsula of eastern Newfoundland. So we worked on capelin and did some experiments on capelin, the downside about capelin for me was that their life history is such that they spawn and it's very easy to get lots of eggs. The eggs themselves are an interesting focus of study, but larval period duration was not easy. Capelin at that point hadn't been reared in captivity successfully in numbers, and on the downside for me, the capelin larvae take some 6-8 months to reach metamorphosis and like most fish, metamorphosis is a not very easily quickly scored feature, i.e. most systematists and ichthyologists classify a fish as a juvenile, as by definition, once it has ossified all of its' fin rays, so if you wanna score hundreds, if not thousands of larvae or juveniles, yes or no, you don't really have the time to clear and stain or check for ossification of fin rays. So I was shopping around for other species that had more clear metamorphic transitions and one of these species that was available and had some prior literature foundation of studies on them was winter flounder. So flatfishes do have a very dramatic eye migration, and change in ecology and habitat at the time of that movement from pelagic to the benthic stage but usually concurrent with the eye migration across the head and other changes. For us, winter flounder were available in the near shore, if fact they eat a lot of capelin eggs in Newfoundland, spawning fish were readily available, methodology was

known from the literature from visiting labs like the NMFS Northeast Fisheries Science Center in Narragansett, Rhode Island. Jeff Lawrence and {*Al? name unintelligible*}were pioneers in some of these ecological experimental studies on cod, haddock, and winter flounder, but winter flounder was one of their focal species, yellow tail as well. So I visited them on one of my trips down to Durham, I was still doing some research on ponds and breeding amphibians, and so I'd drive up and down on I 95 between Montreal and Durham and Narragansett was not that far off my route so I visited Jeff and Al and Larry Buckley too learned about their experiments with these various coastal marine fishes and how to go about rearing them. First thing I realized both from my work on capelin, trying to rear capelin, and winter flounder is it's far different and substantially more difficult to rear larval marine fish than it is to rear mosquito larvae and tadpoles, no longer rabbit chow, and TetraMin, fish flakes. It is culturing rotifers and in that era culturing rotifers means you have to culture the unicellular marine algae to feed to the rotifers to feed to the larvae before you collect any data and of course you want to make sure you are capable of rearing them in a good condition so you are looking at the experimental treatment, rather than laboratory rearing artifacts. So what that meant to me, was fortunate my mentor, Bill Leggett, was able to support this through grants. Doing experimental work with marine fish became by logistics if not nothing else, a team job. We needed a couple technicians, post docs, a grad student, multiple people working on the same project to culture the algae and culture the rofigers to feed the larvae, to maintain the larval tanks. At that time, we would image analysis just coming into its use in taking images of sizes of larvae, we'd anesthetize them and have a video cassette recorder, a camera connected to a microscope, and that's how we'd get our data, in terms of the effects of environmental variables, temperature, food level and so on, growth and development and size and so forth of those flounder larvae. At that time, in '85 '86, I was also doing some work with faculty at Memorial University of Newfoundland at the Bonne Bay Lab. It was also our getaway from Bryant's Cove which was our field station, if you will, helped to buy DFO support we had a mobile home and a fisherman shed, and that was our lab and that was quite sketchy in terms of the continuity of electricity, we had to plumb our sea water out every spring when we went out to Bryant's Cove, so it was a great experience, but a lot of risks in terms of our systems could fail during a power outing due to a rainstorm or thunderstorm and we'd be sort of stuck for a while so we had to be adaptive. But that work led to, in the fall and summer to additional work on capelin, and winter flounder, cod, other taxa of interested Canadian DFO's mandate, and also very good experimental animals. So, jumping forward, a few years, when I left McGill, I was at that time working on some modeling studies with some collaborators at Oak Ridge National Lab and some folks involved in EPRI, the Electric Power Research Institute, when we were trying to identify what were the cost populations of power generating plants, specifically nuclear power plants, with water cooled towers, what's the cost in terms of egg and larval mortality due to the intakes of this water treatment of water cooling plant towers, on the local intrapopulations, and winter flounder was one of the focal species, so was California halibut on the West Coast, and we were doing what was called in the early days individual based models of our population processes, more so the early life stages, eggs, larvae, young juveniles and trying to get a handle on that. The actual program was called compmac for compensatory mechanism, and what that means, is that the people funding the work and the work was, in my opinion, quite independent of the funding mandate

as well as good science, we're trying to quantify whether or not the reduction in number of eggs and larvae due to power generating practices, actually results in a, if not a net benefit, at least not as a bad a negative as you might think i.e. the surviving fish are growing at a, in population of a lower density due to the culling by the power plant practices which might demonstrate competitive release... i.e. faster growth rate, higher fitness, better survival, perhaps better condition of recruits and maybe that leads higher fecundity, to an early age of maturation. There are a lot of possible benefits from thinning populations, whether it be fish or forest, so that work was done independent of McGill or any institution I was associated with in the last couple years in Canada, in fact I was down in New Brunswick, Canada at St. Andrew's, right over the hill of the DFO lab, the biological station at a facility called Huntsman Marine Science Center, so I was there the last couple years. I was in Canada collaborating with my colleagues at Oak Ridge in California covering the winter flounder on the East Coast and California halibut West Coast, modeling efforts for compmac. At that time, going to various fisheries meetings I recall would be about 1995. I was down at an AFS meeting and, I believe it was Tampa, and saw John Boreman who I had seen at a number of regional flounder, Flat Fish, at fisheries conferences in the Northeast, one in the Winter Flounder biology conference every two years, used to be in Mystic Connecticut. I saw John over the years, and I believe at that time he was still a Cooperative Marine Educational Research person at UMass, but he was affiliated with Northeast Fisheries Science Center for a number of years. John told me about the availability of several jobs at the Sandy Hook Lab that were going to be coming open soon. In fact, they had just hired three senior scientist types of positions at the lab, and wanted to know if I was interested. I was at that time starting to look around, and that was an attractive position for me. I was also interested in, attracted to and being pursued for a job in St. John's Newfoundland at MUN, Memorial University of Newfoundland, another attractive position in Newfoundland in this wonderful location culturally... natural history. The biggest downside about Newfoundland is short summers, and you got to take a plane to get out of there and you're often fogged in and somewhat travel restricted. My partner at the time, who was from Montreal, Canadian, so she knew I was looking at both St. John's, Newfoundland and New Jersey as a possible job that I would want to pursue. Her mom was an American citizen, and she'd spent summers in Vermont on Lake Champlain, so she was Americanized to the degree Canadians can be from Quebec. So it didn't make that much difference to her, in fact, I was more familiar with Newfoundland than she was. She hadn't even gone there until about ten years later.

BM: Well that makes sense, there are very few Canadians have been in Newfoundland.

CC: Isn't that true?

BM: There are more Americans than Canadians in Newfoundland.

CC: So we decided to move here to New Jersey, she was a biologist at McGill when I first met her, went back to school for teaching credentials and ended up teaching elementary school in New Jersey, and I started this job in '96 at what was called the Sandy Hook Lab, which is the James J. Howard Marine Science Center, and was hired, I believe, in large part

to take advantage of the fairly new seawater lab across from the old army building 74. The sea water lab, I believe was opened in '93, '94, I started in '96, and as an experimentalist, I was, and by the way an experimentalist who had spent my formative years in fisheries experiments in this shed and mobile home in Newfoundland where you can see the dirt, see the earth and the dirt through the floors in the shed. I was really impressed by the facility here in terms of the piped seawater, and the physical plant and structure and what was possible . It was also a fairly, compared to other labs I've visited since, there is a sense it is a large water lab with a large, and for me, very important, empty modifiable rooms. What that means for me is, I could come in and set up experiments on a seasonal or temporary basis and adapt the room to my needs. So when I was first shown around the lab, at the time Jeff Cross was the Division Chief and Anne Studholme was the Branch Chief in my unit, I was a scientist in her group. I recall Anne showing me around the lab during my interview visit and she said: "Well Chris, this is the lab you will be using. And she showed me one of the smaller labs, we had the larger labs in the back hallway that were maybe 40 by 30 feet and maybe a 16 ft high ceiling. Those are the large labs. And across the hall there are smaller labs about less than a quarter of the size. She showed me the smaller size and said "Chris, this would be your lab", and of course, having been around for a while and done experiments for a while, I realized immediately that, this is not enough volume, and I mean to do experiments even though I'm studying baby fish, if you will, the designs of the experiments where they'd be using a one liter or ten liter container maybe even a 100ml container. Once you start multiplying the factors of treatment levels of replication you realize, this room is not big enough for me to do experiments, and so I was also able to take advantage of one of the large labs across the hall, and over the years following, expand down the hall, and now I probably occupy half of the space in the lab just because it lends itself very well to experiments. Prior to my arrival, by the way, as you pointed out, the lab was mostly, if not entirely, field based, with the exception of Bori Olla's work on behavioral observation of the fish in the large tank in both the older and newer building that I was going to start working in. So there were some behavioral works but for the most part the large volumes of space were being used to hold fish in preparation for observing and maybe some tagging studies, things of that nature, but not any early life history studies, and not experimental work. Which is interesting because when I arrived here in '96, my group was called the Life History Recruitment Group, it was the legacy of the icthyoplankton group from years before that was involved in MARMAP the icthyoplankton survey off of the Mid Atlantic Bight here, which was a precursor to GLOBEC and other larger icthyoplankton surveys later. But this wing of my building and the group that I was in, as well as overseeing with the Life History and Recruitment Group were a lot of icthyoplankton systematists so we're talking Mike Fahey, Wally Morse, Peter Bering an egg specialist, John Sebuncu was also involved in that work. And before I came, there were other technical support folks involved in sorting icthyoplankton and quantifying the abundances of eggs, and larvae, and fish found along the Mid Atlantic continental shelf. So, these folks, even though they're very knowledgeable and they're experts in their field of distribution and abundances and taxonomy based on eggs and larvae and early life stages of these fish taxa, they didn't do a process based or experimental based work, which is fine, it's their choice, I understand why they did what they did at that time. I was interested in, as I described already, phenotypic plasticity, growth development, survival, what factors influence variability in

these outcome, is it environment, which environmental factors, is it parentage, is it mother or father, is it heritable, or non heritable. Studies of that nature which are best approached through an experimental methodology with field data as contextual information to provide the rationale and interpretation of the experimental data.

BM: That's what it was like when you got here then.

CC: Yes

BM: So you really initiated the experimental early life history?

CC: Early life history, yeah. Bori Olla and his group had done behavioral work for a number of years. He left before…

BM: Who was that?

CC: His name is Bori Olla.

BM: Bori Olla?

CC: Yeah, he left Sandy Hook before I arrived and moved out to Oregon at the Hatfield Marine Station and NMFS Lab out there. But he was instrumental and mentor to {*Al ?name unintelligble*}Anne Studholme, and others working in the behavioral, what is still called up until next week, the Behavioral Ecology Branch, which is what I am in. I do some behavioral work but not in the tradition of Bori Olla and his group.

BM: But with the reorganization, that will be gone completely?

CC: You know what, in reality, it's been gone long ago. It's the name change that takes so long. I was in the, what's called, the Coastal Ecology Branch, currently the Acting Chief of that is Vince Guida. When I arrived, that was Anne Studholme's branch. Bob Reed was also one of the branch chiefs. That branch has morphed into a more habitat mapping group. Not experimental, but ROVs, videos trawl based, clutch and calibration verification of what they are seeing based on video and other kinds of electronics.

BM: So then the behavior ecology morphed more into your kind of work with the early life stages and experimentation?

CC: Right, and there are still people in the branch who do and have done the last ten years or so, field work on again a recruitment processes, I think is the best way to capture what we've all been doing for the last decade or decade and a half. Whether it be in the field or the laboratory. And some of my work too, by the way, most of my field work has been inshore, very inshore. Meaning up the Hudson River and the Hudson River estuary. We started with the surveys of the Hudson River, trawl surveys, and you know this has been done by various

folks in the past, more extensively by the utility company contractors who have been monitoring the Hudson River for probably forty years. But our initiative initially was to get on the river and see what's there, be opportunistic in terms of which species may be abundant enough or caught easily enough, perhaps near spawning condition, to be possibly used in an experimental context in the laboratory. At that time, we came across Atlantic tomcod which is a non commercial species. These again are cod fish, so we argue that our results are relevant to other cod fish, haddock, Atlantic cod, etc. but the nice thing about the atlantic tomcod is, it spawns in shore, and indeed up river you can catch it by box traps in December and January with our own efforts or through one of the contractors for the utility companies and these fish are small bodied, reaching no more than 10-12 inches in length. They have relatively large egg sizes, and that's important for people like me because if you do an experiments on animals one of the challenges I alluded to earlier is the culturing of prey for these fish is not trivial. Our typical protocol back in the Newfoundland days was culture the algae to feed the rotifers to feed the larvae. Well, that's changed somewhat in two ways. For tomcod, we skip the whole rotifer stage because the eggs are large enough to produce larvae, that at first feeding, you can feed brine shrimp too which is much easier to hatch up in numbers than rotifers are to rear, so tomcod are small bodied, meaning that I can do more experimenting even with adults in the lab because they don't require such a large footprint, than say an Atlantic cod would. They mature in a year which is also good, because we can look at full life cycle questions, they have large eggs, they can be fed brine shrimp. They are like I said, gadid, cod, so we argue that our results are more relevant to cod fish than say if we're working with striped bass or blue fish or mummichog...silverside eels that aren't as close relationship to other cod fish. And furthermore, the Hudson River Foundation has been one of our primary sources of funding. We received probably three or four grants over the years from them, specifically to work on tomcod and other grants for other purposes. But they've been a substantial source of funding and the center, I've gotten mixed messages from the center, we can also argue that tomcod is an important forage fish. In April and May, it's the only fish in the river in the lower estuary, it's about 5-7cm in length and is the primary fish for piscivorous fish such a young of the year blue fish and one year old striped bass so it's important in the trophic web of the Hudson River estuary. Anyway it's proved to be a wonderful ecological and ecotoxicological model. With which we evaluate the effects of various impacts on marine resources and specifically in this case contaminants to dioxin like compounds, PCBs, aerochlorids, and other contaminants that have been released inadvertently or by plan in the Hudson River estuary over the last six decades.

BM: So you've done a lot of, you're doing a lot of work now on contaminants.

CC: Still working on contaminants, using both tomcod as a model right now, we've done that for a number of years in different contexts, and we're also looking at the effects of contaminants on Atlantic and short nosed sturgeon, so two endangered species. Following pretty much a similar protocol, exposing the animals as embryos to different doses of different PCB congeners, dioxin, looking at the sub lethal, as well as lethal responses. And this year, and last year, and next year we're looking at our general experimental categorization right now, is the effect of environmental co-stressors on key early life history

traits, so we're looking at the separate and combined effects of contaminants and again multiple different contaminants, as well as climatic change, higher temperatures in the summer, maybe warmer winters. In some of these studies overlaying the effects of hypoxia or low dissolved oxygen, and trying to get a handle on how these co-stressors may be additive or multiplicative or counter additive in their interaction with each other. So that overall umbrella of the effect of co-stressors or environmental stressors on early life stages covers much of what I'm currently doing including, by the way, even though it is a different line of funding and a different audience, are all of our ocean acidification efforts, because high CO2 is another stressor in the environment and we're addressing that as well with a variety of species including winter flounder, fluke or summer flounder, and silverside tom cod, mummichog in the estuaries.

BM: For co-stressors, you need to have much larger samples? How do you manage that?

CC: Well, larger team, more volume, more collaborators. This goes back to what I said earlier about my time at McGill. When I started working at marine fisheries, it became obvious early on how, unlike in grad school, where you actually could do an experiment by yourself as a single grad student, no way in terms of marine estuarine ecology, you have to rely on team effort to produce the whole trophic web they're using to feed to the algae and rotifers to the fish larvae. Here, with the evaluation of co-stressors and through collaborators, I say my primary mechanism for expanding both our insights, inferences, and the actual size of the team is working with collaborators who bring different skills to the table. My primary collaborator on the sturgeon and the tomcod work, for instances, is Ike Wirgin at NYU Environmental Medicine Institute in Tuxedo, New York. He is the person who brings the gene expression component to the work. Other collaborators at CUNY-Queen's College look at the effects of these contaminants on cardiac heart development as well as other organ development. Keith Cooper did some work with us at Rutger's on the histopathology effects of these stressors including CO2 in ocean acidification kinds of studies. So everything is pretty clear from anyone who does science for a living. You can't be an expert at everything so you go through collaboration. And as you mentioned today earlier, Bonnie, the importance of interdisciplinary work, either by the individual or making that feasible, in my case, through expanding the team to include broader a scope because your team members each have their own personal scope and that makes the larger team more effective.

BM: How is that rewarded by the agency? Is that an issue?

CC: It's rewarded certainly by the agency, like in standard science and grant proposals, i.e. whether it be recognized as buzzwords through what interdisciplinary work had been involved in, who are your collaborators, who are your clients as well. Who is your work serving. And somewhat different emphasis in those areas in a federal position as opposed to a university position, but still you can't not recognize these trends in science, the importance of interdisciplinary work, importance of data, management, data management plans, the importance of outreach and education, whether you're applying to NSF, which I'm not allowed to do as a federal employee, same thing for the NIH, or NOAA opportunities that I

am eligible to apply for as a PI, the other ones I can be on as a collaborator or a co PI perhaps. We all see similar kinds of RFPs these days, you know, who are your clients, where your results might be used, by absolute statute - what's the outreach education component of your work? So it's very similar across all sectors of science be it private, I don't know, public universities, private universities and government agencies.

BM: When you're doing collaborations on who gets credit for what, that's something that's fairly easy to handle for a professional?

CC: It is with my professional colleagues. I'm currently drafting a document drawing from a variety of sources, by the way, Ecological Society of America, AFS, other journals as well as societies in terms of how is co authorship determined. And the biggest challenge for me right now is how to give my support team a recognition. I'm talking about biological technicians as much as anyone else, not so much students or post docs, they have a different role and they're self driven, but certainly everyone who works here with me at least, I believe wants acknowledgement and credit, and I'm working on a description on how best to have that as an objective or at least a starting point for those discussions. I still characterize myself, despite the fact of having worked for the federal government for 20 years, very much as an academician in my mindset. I approach projects like a grad student, or a post doc would meaning trying to do the best science I can. When I look at my work at the Huntsman Marine Science Center, in St. Andrews, like I said earlier just over the hill from DFO, I could set my watch by when the DFO folks were going home at 4:30 when, if I went over to their lab between 10 and 10:15 in the morning, I couldn't find anyone because they're all on their coffee breaks in their own little coffee break offices, they knew where to go, I couldn't find them. And I thought that was a bit odd honestly, I mean having been nothing but academia up until that point. I still carry those kinds of, what I considered to be, good habits about how to get science done, even though I work for the federal government. I'm sure there are a lot of other people I'm talking about who feel like I do and I do suggest, by the way, to my colleagues, in as best I can and most sensitive but still truthful but as honest as I can, a lot of the folks who were here when I arrived, by the way, only knew Sandy Hook Lab. They grew up in the area, started with a summer co op job that morphed into a federal term position, and a permanent job, and they spent their whole careers here. I don't come out of that model, I come out of a grad school model, where at least for the first half or third of my career, if you can call grad school a career, you learn to expect, you move on i.e. undergrad school, master in my case before Ph.D., post doc, research scientist, and that moving had a lot of benefits. The diversity of experience amongst that list of benefits. So folks, internally now who I would characterize perhaps still in the previous model of how one progresses in the federal system, they still hope that that promotion ladder applies to them across all, what we call, if you will, grades in our system right? And that's a question by the way we'll deal with in our discussion groups, and that is, if you want to, what's the best strategy for advancing yourself in regards to positions if you feel like you're capped locally. Well, for my experience, you can move up for a while internally and then you'll reach some barrier, it might not be due to your capability, it could be due to strategic planning by your managers. It would make sense to apply laterally and up elsewhere because new opportunities arise and you can ratchet your

way up the pay scale, promotion scale. And you can't do that very easily internally. In fact, when I described this to my friends and folks here who were technicians, who would like to be promoted to a fishery biologist or research fishery biologist, they just have to realize who they're competing against. They're competing against Ph.D.s who are looking for jobs, and they're competing against people who have experience in project leads. You know, these things don't come about for the most part, there are exceptions, but don't come about internally if you haven't had that kind of experience, and it's hard for local people to hear, because they don't want to move, they like the area.

BM: They're really entrenched in the civil service model--

CC: They are.

BM: --and that's different...

CC: It is different, and that's probably one of the areas where there's still a tension between how to get things done according to rules and regulations versus interest in making sure the work gets done well. And as efficiently and as quickly as possible.

BM: How are we doing with my questions? Pretty well?

CC: Where are we on that list... well, I've answered secured the position... where were you living at the time, what had you been doing in research and work prior to joining, what was the research focused on in the branch when I joined, the state of the science at the time...dominant paradigms of government approaches in your field...I looked over your list , Bonnie, and I would say my research priorities, like we were talking about this morning, in terms of work history and retrospectively, all of us can trace our path back to where we came from, but couldn't project forward to where we were going to be, if we were 20 years old again, but much of my science is still under the umbrella of, the importance of individual variability, the importance of variance, and one of my most meaningful experiences in terms of research is recognizing the interlocking components of mathematics and statistics, evolution and experimental design. They're all based on ideas, at least to my knowledge, largely advanced and if not initiated by Ronald Fisher, in terms of fisherian fitness and of course the f-test is named after Ronald Fisher! And agricultural experimental design fundamentals follow him as well. And the interconnectedness is laid on the foundation of understanding variance, variability, the pattern sources and consequences of that variability. So I've been doing that kind of work throughout my whole career, whether it be experimental work or modeling populations, it's about understanding individual variability and its' role in understanding population processes. So, earlier today you mentioned the changes brought about by Magnuson-Stephens and even more recent emphasis on ecosystem based management. The world is complex enough and we're all limited enough in our own scope in time, no one can do everything, right? I'm happy to do population modeling at a larger level, ecosystem analysis, or projections, or even reviews, and that's all of interest to me. But I can't do that and experiment, so I have to divide up my time somewhat. Regardless of how

advanced ecosystem modeling becomes, it still has to be connected, the thicker the strands of connection, the better to what's going on at the individual population species level. I do think that single species modeling of fish stocks for assessments and quotas is a mistake, when you ignore the context in which each species live, be it the food web or the community they interact with or the fact that fishermen for general trawl fisheries can't select, well, they just want to get cod. You get what's in your net, you might drop your net where you think a cod school is but you pull up whatever is not able to avoid your net. But fundamentally, and this goes back to Fisher actually, the unit of selection is the individual. Evolution happens and in a population context of differential survival and fitness of individuals. So, again I don't resist ecosystem approaches, I just realized they're part of a more holistic picture that includes individuals, populations, communities, and species, and evolutionary principles. It's all dynamic.

BM: Do you feel pressured to make your work more directly focused on either ecosystem management or more generally particular issues in fisheries management?

CC: No. In fact, sometimes, we've had these conversations internally, and when we were first planning the EFH, Essential Fish Habitat documents for the Magnuson-Stephens Re-Authorization Act, I remember sitting downstairs with John Boreman and Tom was here at that time and maybe it was Jeff Cross, it was during the transition in the division, and identifying the four tiers of EFH, what do we need to know about species and what don't we know about species, and ideally you want to know everything but we're not there with anything, so you have to identify what's realistic. We know anything about these life stages presence, absence for instance. Do we comment on the quality of the habitat they live in, and how that habitat through a level might influence the processes that involve in determining population size, and how we can integrate all this together. Even at that time, I would be concerned about internally, scientists from, whether it be for internal proposal calls or external collaborative calls, to let's not wed ourselves too tightly to habitat because that might simply be, you know, last a decade to a fifteen year trend and we don't want to be known just as the habitat lab just because what happens when habitat is no longer a hot topic on the research front. So, I used broader words than that. Ecology and ecosystem, ecosystem is a good one by the way because it captures all the components of it. So I guess my view on that is that, I think for anyone who does science, especially those who are trained in ecology, it is very easy to justify the work that we do in those broader contexts. I had no problem just for my work with regards to habitat but I don't think habitat had priority over broader principles. It was part of the ecosystem if you will.

BM: What about…I don't have the questions. There was a question about the data you used, it doesn't sound like your data has changed over time --

CC: No, it doesn't.

BM:-- the kinds of data. Is there something else about the data that changed?

CC: Yes, I will tell you, because I did my Masters before my Ph.D, I covered a lot of ecological and evolutionary courses in my masters degree. I was fortunate enough by accident or someone else's insight to take statistics as an undergraduate. So by the time I, and I took a year of biometry biostats as a masters student. So by the time I got to Duke, three quarters of my classes were in the math/stats department. So, you know, I came out with, again my Ph.D. mentor very much encouraged us to know more than he did, and he was also very good with quantitative methods and statistics. So, I came out of Duke with a solid ground of statistics, and that point it was frame computers, just beyond the card reader world , I can't remember. To take your stack of cards over to the computer department, and at McGill, one change that was obvious, compared to Duke, at Duke we could run our SAS or SAS programs at no cost, at Duke they cost money. So that led me to a PC based SAS like product called CISSTAT. Anyway, so my point is, I was already quite comfortable doing all the quantitative methods I needed for my work. I arrive at the Sandy Hook Lab and at that point, they had on staff, two or at least one statistician and another guy who saw himself as a statistician and other people who seemed to be more than willing to take their data to these statisticians to let them analyze it which really was a surprise to me, and.. In my education you basically you did your own stats, you hoped to do the best that you can, you learn about it, you're sort of self-taught courses but you feel confident with it. So I'm very much not the older NMFS model in terms of turn to person X and ask them to figure out your data. You do experiments, you gotta be this way, you should know what statistical tests and analysis you're gonna do before you even start the experiment, 'cause it's wedded to the actual design! So, that hasn't changed for me at all. My observations are that, yes there are certain methodologies and statistics that have changed over the years, stats versus other stats I was exposed to, to some degree self programming, are freeware community based analyses as opposed to software like CISSTATS, you can still buy it, but it might costs $1,000 dollars a copy as opposed to free ware. Things of that nature. But as one of the students said today in our discussion group, one important attribute of an individual in this area, is you need to recognize where you need to go and recognize what skill sets are important, these particular women who were saying this are the two who are working on GIS and clearly GIS is a tool that is important in a lot of types of studies these days and is important to have that exposure. For me, still to this day, statistical competency is kind of critical to most anything I do, so that's sort of what I continue to believe in and continue to practice.

BM: Do all of your, the people who come to work with you, are they prepared or is that something you require?

CC: Well, depends, certainly my technicians aren't. They're often bachelor's degree folks who haven't had that exposure. Grad students are to some degree. As you can appreciate, a lot of times, one strength or weakness is fairly tightly correlated with their major professors strength or weakness, and sometimes that's the opposite by the way. If your majoy professor was weak in topic X, you'd make sure you were strong… but usually it's a positive correlation. So it depends, yes, and so I can't generalize to everyone who works with me, but for the most part, they learn while they're here and what needs to be done.

BM: Has high speed computing made any difference?

CC: No.

BM: Not relevant…

CC: You need a computer, even though, again my generation, my first stats class, this is pre computer, we used Monroe calculators, calculating sums of squares and the actual statistical test, which was great in terms of knowing what the components are of these analyses --

BM: Sure, you needed to work it out yourself.

CC: --Yeah. Now, it's too much push a button and get an answer and you don't really know what it means kind of thing. That's a problem.

BM: So, in terms of those last questions then I think the questions, the advent of statistical models and mathematical models. I'm not sure what that meant, what it would mean in your case, and certainly it's relevant to stock assessments. I think that is mostly what is directed at.

CC: Okay, let me take a different angle on that. When I was in Canada, a lot of my friends did work at DFO and they were either stock assessment scientists or they were research scientists. Here in this center, all the stock assessment people, I believe, are at Wood's Hole. The other labs do research of one form or another, field or experimental or aquaculture, genetics which is really great and, I mean, people ask me about my job, and one, among other things that I like about it so much, is that it's diverse and my managers trust me enough to realize, you know what, Chris probably knows better than I do what the next topic of study to be is. I hope they feel that way. So, that's good. The challenge I think that a lot of bench or lab or field scientists feel regarding understanding nature and in this case, the fish populations in nature and the ecosystems they live in, is how to transfer that information to assessments, and historically the challenge has been, and I understand why it's a challenge, the stock assessment folks, they cover their range of skills as well. They're smart, driven, or less so. But they have a tradition of using certain models. And it's something you alluded to in your presentation today which is really important, that is, we know we can do better, how do we get there quickly? And sometimes there's resistance to incorporating newness, if you will, or a change in oldness, a change in the habit. Because this is like, Oh, we have been using the same models, like we have been using the same gear, right? You've been using this trawl or this ship for 30 years, we don't wanna change anything. We like the coverage, the data, the quality, the repeatability. Similarly with assessments, if you're in a single stock paradigm, you are probably very comfortable and very good at it if you've been doing it for a couple decades, and by the way the IBMs I used to work on, and I am also working on some for ocean acidification right now, these individual based models. They are sort of, in a way, the opposite of stock assessment models. Their intention is to bring in explicitly, interindividual variability in fish. Assessment models try to collapse things down to averages or maybe a step above logistic equations, i.e. a step above the number of individuals at a current time

with some function of past numbers at that past time. They might be age structured, there may be some details on age dependent survival, age dependent fecundity, so on, but they don't bring, usually, historically, any reasonable characterization of the environment into those models. The thermal environment, the seasonal changes, prey abundance, community structure, density dependence, these are principles that have been acknowledged and used when, usually, most of the time, when it's available logistically feasible for half a century or more. So why aren't these in these models? And this goes back to another topic we talked about this morning, that is, fisheries science has its' own history and legacy that split off from ecological science back in the late 1800s and it's quite interesting that there's some occasional reconnection, I wouldn't say it's getting better and better in terms of borrowing ideas from other ecological disciplines to see well they've probably already thought of this in plant ecology like how to set up a transect for measuring variance and spatial structure. When I look at that literature, the chances of us being exactly the same forefront, in terms of our advancement, is 0. So maybe behind us, and maybe ahead of us, let's look go to the ones that are ahead of us and see what they did and transfer what's transferrable and makes sense to fisheries ecology. So {*name* at *place name unintelligible}* back in the sixties was doing fantastic plant ecology work based on the same methods as some of the same folks who are now doing GIS and video based surveys or trawl surveys are now realizing, oh yeah, structure exists in nature --

BM: Spatial structure

CC: --yes yet and there is patchiness and you have to find, in order to optimize your sampling scale, you have to know the scale of variance.

BM: That's interesting. This is great. I would love to talk about this forever but I'm going to have to stop.

CC: That's good, I am supposed to go out seining with one of students for some silversides.

BM: Well, that's important

CC: That's important (laughter), yes.

BM: This is great, this is really interesting.

CC: Well, good

BM: And I really appreciated like this last discussion. That was really important.

CC: Right, right. Did you design these questions, Bonnie, or how did these come about?

BM: I did not design these questions. Um, Patricia Pinto daSilva I guess did this together with some other colleagues.

CC: Right

BM: But, you know, we talked about them. We interacted a lot about them and I thought these were good questions.

CC: They are good questions

BM: You know, they certainly get to the heart of the matter.

CC: Yes, they do. And I think that, we have done some surveys on public knowledge of ocean acidification at one of our open houses a couple of years ago. One of my high school students and I were doing it for her senior project and sometimes you almost need to do a survey in order figure out to find a better way to phrase the question, right. So I am sure through interviews, from our discussion, what new key word --

BM: I would change this a lot if I was developing it over again. Because certainly…but that is a general principle of survey work, you should do pilots. You get the sense of what the right language is, how do you get into it, and so on. And that is what you would normally do in. In this case, we are barreling through because we only have a short period of time since the money will expire, they want to pay the transcribers before the money runs out.

CC: Yes, ok, fantastic