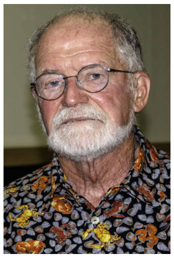

Charlie Miller Oral History Interview, May 12, 2015

Title
"Studying Small Creatures and Large Issues"

Date
May 12, 2015

Location
Burt Hall, Oregon State University.

Summary
In the interview, Miller discusses his upbringing in Minnesota, his undergraduate years at Carleton College, his decision to pursue graduate study in ocean ecology, and the narrowing of his focus on zooplankton while at the Scripps Institution of Oceanography.

After noting a post-doctoral trip that he took to New Zealand, Miller turns his attention to his years at the Marine Science Center at Oregon State University, which is the primary focus of the interview. He describes the circumstances by which he arrived at Oregon State, the techniques used by oceanographers during the time, his uneven first experiences teaching undergraduates, and some early ocean cruises that he took on the center's research vessels. In recounting these ocean cruises, Miller makes specific mention of research on phytoplankton and copepods that resulted in the discovery of a new species of plankton, *Neocalanus flemingeri*.

From there, Miller touches upon issues collaboration and collegiality within the profession. He then details his memories of the earliest conversations on global warming in which he was involved. He likewise shares his thoughts on the ways in which students have changed over time, a rejected proposal that OSU's School of Oceanography relocate to Newport, and changing contours in the relationship shared between Hatfield Marine Science Center and the Corvallis campus. Returning to an overview of his scholarly career, Miller recalls his collaboration with Japanese scientists on the reproductive mechanics of copepodites.

The final segment of the interview is principally devoted to Miller's environmental activism and other activities in retirement. He discusses his opposition to a liquefied natural gas terminal that has been proposed to cross Oregon and shares his perspective on the peril presented by climate change. The session concludes with notes on family, a description of Miller's business editing scientific manuscripts, and an appeal that members of the academy become more involved in social and environmental policy debates.

Interviewee
Charlie Miller

Interviewer
Mike Dicianna

Website

Transcript

Mike Dicianna: Today we have the distinct honor of learning about one of OSU's prominent oceanography professors, Dr. Charles "Charlie" Miller. Today is Tuesday, May 12th, 2015 and we are in OSU's Burt Hall. My name's Mike Dicianna, I'm an oral historian for the OSU Sesquicentennial Oral History Project. And we always like to start with - we're looking to find a life story, and so we like to get a brief biographical sketch of your early days, like where were you born, when, and early childhood.

Charlie Miller: I was born seventy-five years and a few days ago, the 28th of April, so I just passed my seventy-fifth birthday, and in Minneapolis. My father was a physician, so he was present at my birth and he and my mom carried me out of the hospital without giving me a first name, so when I was twenty-one I had a choice. They'd always called me Charles, so everything about me was already established under that name, so I kept it. And Charlie came from baseball buddies in junior high. One day the coach called up and asked for Charlie and my dad said "there's no Charlie here," and hung up. So I informed him that he could use this too, and eventually he did. He was a physician, as I said, and I was raised to be a physician and I studied pre-med at college, which set the college I went to, Carlton College, starting in 1958. It was called a Chem-Zo major, zoology and chemistry, which I eventually converted to biology and chemistry by taking a lot of botany. I was interested particularly in algae and lower level plants.

And it took me five years to get through college because I took a long term off to bicycle through Mexico and walk through Central America and got on down to Peru. And I eventually came back because I got tired of being alone. So when the time came in 1963 to fill out the med school applications, I realized it was really my father's adventure and not mine. And I had studied invertebrate biology, invertebrate zoology at the Pacific Marine Station at his expense, but a long summer course with two guys; Joel Hedgpeth, who became the early director of the Marine Science Center in Newport and Jeff Gonor, who by the time I got here in 1970 was a professor there at the Hatfield Center. What's now the Hatfield Center; it was just the Marine Science Center then.

So I applied to Scripps and University of Washington and Stanford. And I had good grades and I was really good at taking multiple choice tests, so the GREs were stellar, and that got me past my D in German and a few other glitches. Glitches were allowed in those days; I'm not sure they are anymore. Anyway, I chose Scripps partly because I wanted to adventure at sea a bit and they had the most ships and the best ships then. And I worked there with a man named John McGowan, he became my professor.

I had applied for marine biology. They had a Marine Biology Program and an Oceanography Program, which included biological oceanography, and I wasn't particularly aware that there was a distinction. I learned right away that there is. The marine biologists work on the biology that happens to show up that will be well-represented in marine organisms, while biological oceanographers are interested in the general ecology of oceans, usually far from shore.

And that's what I got into pretty fast, working with McGowan. I had been assigned to a biochemist who was about the same age then as I am now, and I realized I already knew more biochemistry than he. So I got right out and I got into the Oceanography Program. The problem was they didn't believe that my calculus course, which was called mathematical analysis, was actually calculus, because of the name. But the year that it took to get from marine biology to oceanography was great, because I could learn calculus, which I absolutely had to have to pass the hydrodynamics course, and many other things that oceanographers were expected to do that marine biologists were allowed to ignore, and that's been really good for me.

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And it was a great experience at Scripps in many ways. The professoriate there included not only McGowan but a man named William Fager, who was a brilliant guy, gotten his PhD in physical chemistry at Yale at age twenty-one and had worked on the Manhattan Project and got very upset about how that came out and went to Oxford and became an ecologist. And he was the intellectual driving engine of biological oceanography at Scripps, and I learned a great deal from him. And like all graduate students, I learned as much from my fellow graduate students as from the professors or the books. And they included Bruce Frost, who is my counterpart at University of Washington during all of my career; Peter Wiebe at Woods Hole. We did many things together, cruises and the like, and we got going on big ocean-spanning cruises early on in '64.

MD: Yeah, because in the early 1960s Scripps was the school on the west coast, wasn't it?

CM: No, the UW school was also very good and was founded under Richard Fleming of Sverdrup, Johnson and Fleming. Sverdrup and Johnson were at Scripps. Sverdrup brought the physics, Johnson wrote the ocean ecology and Fleming was an ocean chemist. And in a way, according to Hedgpeth, OSU got started as an oceanographic institution when Burt came here, partly because Fleming missed the timing of an ONR proposal and Burt didn't, so OSU got money that wouldn't otherwise have gone to OSU, and it was just enough to get things going. And the rest of getting it rolling was mostly through AEC contracts to work on isotopic pollution coming down from the Hanford works, down the Columbia and out to sea. And they brought many people on board to do that work, including biologists like Bill Pearcy and Larry Small and Herb Curl, all of my earliest colleagues here.

MD: Yeah. Now what exactly was your doctoral work specifically on?

CM: On marine zooplankton, holoplankton and what we now call mesozooplankton, things a millimeter or so in size or larger, up to krill sizes of a couple centimeters. And almost entirely working with nets. And what I worked on was the implications of vertical migration that had to do with going down, arriving at a layer of water with different current shear than the surface -different current direction than the surface - and coming back up at night. So they would arrive in some different habitat; different in terms of food or fellow swimmers. And what I looked at was the fellow swimmers - what's up here on successive nights? Does it change? And the answer was yeah, it changes progressively, the composition looks different every night. So they have to be kind of catholic in what they can pull off for getting along in the community. At least, I said that.

MD: Yeah. Now the first thing comes to mind is why something as small as zooplankton and not sharks or whales?

CM: Well, this professor who agreed to associate with me and train me was McGowan, and he worked on zooplankton. His early work, his thesis work at Scripps, was on pteropods, small pelagic snails with actual calcium carbonate shells; limacine and a whole group of other pteropods. And he and a coterie of students who worked with Martin Johnson had worked on zoogeography of zooplankton animals across the Pacific, and because they'd done all this sampling and analyzing their groups; Martin on the euphausiids and McGowan on the pteropods and a few others, there was a pretty complete list of who lives in that plankton. Plus there were all kinds of puzzles about the plankton and all kinds of problems with sampling them appropriately to get at changes, in abundance. It's very difficult to sample them in any level of precision. But fortunately with season or location, the numbers change by orders of magnitude, so you do get usable numbers even though your sampling is pretty shabby. Which is still true.

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MD: So it was one of those things where you could just kind of evolve into this specialty, and that's been your life.

CM: That happens to most graduate students - the association they manage to set up in graduate school carries on. And I've always found all kinds of interesting problems that have kept me quite busy and satisfied.

MD: Now after you received your PhD in '69, you spent a year down in New Zealand, I understand. Tell me about that experience.

CM: Well I applied for it and probably mostly due to good recommendations from the professors at Scripps I received an NSF post-doc. I could go anywhere I wanted, and I'd been talking during a trip Hugh had made to the states with a man named R. Morrison Cassie who was a statistician who had worked on statistical aspects of zooplankton sampling and the notion that zooplankton in the sea aren't just randomly distributed in the water; they're patchy. And there's all kinds of statistical twists to saying "ah, there's puzzles of abundance here and rarity here and they're organized in a non-random way."

And at that time the digital computer had just reached something like it's modern capabilities. The mainframes, the big computers, were boxes the size of this room we're in, and a university would have one. University of California - San Diego by that time had a room with such a computer in it and it was managed by a crew of acolytes to whom you would hand these boxes of Hollerith cards, punch cards, that the computer could read, and you were incredibly limited compared to what people just use habitually now. Thirty-two bytes, thirty-two hundred bytes was the whole program, and all the data and everything you were going to get out of it. And you basically would turn in your box of cards and you'd get your printout tomorrow with your cards back. So the turnover time was one cycle of computing a day, and if you got up early and stayed up late you could have two, maybe three. But we did all kinds of things like that. That changed what people did in science, and all the way from particle physics through to zooplankton ecology.

And there were many bizarre things. My colleague Peter Wiebe took all the old data about the variability of plankton sample counts that had been published and analyzed by analysis of variance techniques, and all of those had been applied using punch-the-number calculators where you had to write down intermediate values and put them back in later. And we very carefully got punch cards that had the data correct, put it into already then standard programs for analysis of variance, and every old calculation was wrong. Every single one. And that was true of agriculture and everywhere, because nobody could keep track of that many numbers.

MD: Oh yeah, yeah.

CM: All them squares of so on and so on. So many, many things like that. And I used that to compare community composition values between nights and to create models of that, and all that shows up in the thesis, which really wasn't the greatest piece of work in the history of science, but it got me my PhD.

MD: Well you started a long and storied career here at OSU in 1970 as an associate professor. What actually brought you to OSU?

CM: There were three jobs, one at Woods Hole, one at Washington and one here. Frost got the first offer here and turned it down to go to UW, Wiebe went to Woods Hole and I got the leftover, which was OSU, which has been a very good place in many respects. Particularly Woods Hole, but also UW, are ferociously difficult places to get promoted. And they were worse then than they are now. They acquired just a touch of humanity. Not much, but some. Whereas OSU, considering itself second-rate, would promote you and let you carry along if you were doing reasonably good work; teaching well, doing some public service, and didn't look like an idiot. And that lowered the pressure a lot, which is good. I mean it's high enough. The pressure here, as at Woods Hole, probably came from the necessity to raise your salary, as well as research money, out of grants. And that was a painful thing many, many times through my career.

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MD: So the early '70s was a changing tide in the Oceanography school. Lots of changes in science as well as—

CM: Instrumentation.

MD: Yeah. What were those like during what we would consider early days like?

CM: Well the change was happening just as I was finishing graduate school. So in graduate school I got to lead several cruises, and in the initial going in '64, '65, when Bruce Frost and I would go out together, we would be using reversing thermometers on Nansen bottles which hang on a wire. And when you send a trip messenger down the levers snapped and it falls over, which closes the valves and gives you a water sample. You bring that back up and it would be analyzed by electrical technique for salinity levels. And the thermometers would capture by a break in the mercury that happened when you flipped it, of the temperature down there. And when you came back up it had this beautiful little thermometer on it that told you the temperature where you were reading it at, which it did an hour later. And all the corrections went through and you could tell the temperature down there.

You could also tell how deep it was, because one of the thermometers wasn't protected from the water pressure. It had an open bottle around it and the water pressure would squeeze the mercury enough—water is compressible—squeeze the mercury enough to make that temperature higher than the one in the closed container, and that would tell you the depth. And that stuff was all worked out in the '20s and '30s by European oceanographers mostly and European glass blowers who figured out how to make this little sidebar on the side opening on the mercury column that would force it to break when you flipped it.

Well that was a lot of hassle. And then we got electronic Conductivity/Temperature/Depth recorders, so we'd get profiles. And those began to tell us more and more about the vertical structure of the ocean, which has been a major research effort at OSU. Doug Caldwell, that you met across the street, was prominent in that work here. Many, many have been. The more they worked at it, the more refined they made the special intervals of the data, so that pretty soon they were getting data from every successive centimeter down in the water column. And that would tell them things about water column turnovers and eddies, and that's still a major issue in the ocean. We still do not have down the full budget of mixing in the sea, but it's coming. Really good work going on here about that.

MD: Yeah, and so that's basically what OSU became known for, was the open water? Or did we have kind of a—

CM: At this department? Yes.

MD: Yeah, that was more of our speciality.

CM: And around the world. At one point there was no oceanography going on in Oregon. In the early days, they did a profile of coastal trends that go offshore from the lighthouse at Yaquina Head, starting about a mile offshore on out, for every month for years and years. And then all that kind of wound down. Then for a while we had no local oceanography going on, and quite a bit has reemerged lately.

MD: What were some of the classes - you were assigned to teach, naturally, and so what type of classes did you handle?

CM: I was hired as a replacement for a man named Herbert Frolander who had taught here since 1959, and he had moved over to be dean of the Graduate School for a while. And he was still teaching Oceanography 331, which was the basic oceanography course for undergraduates and was often a course for grade school teachers and other people who had to take one science course of some general interest, and they would take that. And I shouldn't say so, but I will, that he was known for being a really easy pushover for grades, so it was a very popular course. And he continued teaching that in the graduate school even after I got here, but I did teach it one quarter. And I got the worst teaching reviews of anybody ever, I think, because there were about forty kids in the class and five of them were engineers and pretty soon I was talking to them and it was impossible to talk to the others. They didn't do algebra, much less calculus, so the engineers were easy to teach about things like geostrophic balance of flow and other processes, and I just gave up on the others. You can't teach actual science to people who can't do math.

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MD: Well that's me, that's why I'm a history major.

CM: Probably not. Anyway, I didn't do that again. But I took over Frolander's zooplankton course, which was a lecture where we talked about the ecology of things in the sea and a lab which was really great, because I taught identification of copepods, euphausiids, ostracods, pteropods, all the main groups of holoplankton, and taught people to dissect so they could look at the parts that have their characters. And that used what I had learned at Scripps, which was how to identify and characterize ecologically and biologically all these animals in the plankton.

MD: Now when you talk cruises you talk way out there in the ocean. Did you ever go out on the ones out in Hatfield, like the *Yaquina* and the *Acona*?

CM: Right, I used the original *Yaquina* for a trip to Ocean Station Papa at 50°N, 145°W in the lower part of the Gulf of Alaska. And by that time already - this was '73, '74 - Bruce Frost and I and one of his older colleagues named Karl Banse were talking about the bizarre ecological system in the Gulf of Alaska in the north Atlantic, much of the northern north Atlantic and the coastal zones of the Pacific and the Atlantic. You have a Spring bloom in which mixing has stopped by solar warming of the upper layers as you get a stratified layer that still—that now has nutrients in it because of the mixing that had preceded. And those blooms would be, first, a lot of diatoms and a lot of dinoflagellates. At the time we didn't know much about microscopic sized or micron-sized phytoplankton. And the Gulf of Alaska had persistent levels that had been shown since World War II by measurements by the Canadian oceanographers on the weather ships that were stationed at Station Pete, that there was always plenty of nitrate, plenty of phosphate. There was no excuse for not having phytoplankton blooms there unless something else was going on. And we decided to find out what that was.

And our initial hypothesis was that the large copepods that belonged to the genus neocalanus - about four or five millimeter up to eight millimeter sizes that dominate the spring, late winter, spring, summertime in the Gulf - were eating all the phytoplankton as fast as they could grow. They could keep up. And there were a number of reasons for thinking that. One was that they store up a lot of lipid during their growth phase and then their oldest larvae descend to a thousand to two thousand meters depth and hang out there in a resting phase. And they mature at depth and they reproduce at depth, making big oily eggs which float up. And so they could pump eggs into the surface to hatch and those would be enough to jumpstart, to grow as the phytoplankton increased.

So that was the initial hypothesis. And we began by getting time on the weather ship itself to sample every week. We couldn't do it more often, but every week we would sample all the way down to two thousand meters. And I had three people; Hal Batchelder, who's one of those guys that wrote that biography of me, and Martha Clemens and Richard Connelly. They went out for forty-nine day cruises on the weather ships. They were very long cruises. Martha did four, Hal did three, I think Richard did three. And that gave us an entire year and three months of data from which they and I reconstructed. When they weren't at sea they were counting the animals, reconstructed the life history of those animals; *Neocalanus cristatus plumchrus*. And later on we discovered that one of them was different, was actually *Neocalanus flemingeri*. And the timing of everything looked very good for the hypothesis. So then we got funding to do some experimental cruises in spring and fall where we went out and measured their grazing rates and we measured the phytoplankton growth rates.

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We had all kinds of colleagues from different places. It was a work project called SUPER; Subarctic Pacific Ecosystem Research. And oops, there wasn't nearly enough grazing to account for the growth rates. And it turned out that just at that time, all across oceanography, people were realizing the importance of protozoans, both protozoans and very small phytoplankton. Now phytoplankton are five microns or less, and what we learned was that virtually all the phytoplankton up there are very small. One of the people we asked to participate in this project was John Martin, who was an expert on measuring trace minerals, and Frost and I thought that probably one of the alternate hypothesis we ought to include for testing was that there were trace metal limitations of the growth of the larger cells. Small cells have relatively more surface area compared to their volume, or biomass, than big cells. So the little cells might be able to get enough trace metals while the big ones couldn't, and trace metal, particularly copper and zinc, were showing up all over as potential candidates for such limitation.

And it had been suggested that iron was it, and I invited Martin to participate and he wrote me a letter back saying no, he was busy. So what happened? After one year of SUPER studies of the interaction of the copepods and the phytoplankton he comes out with a paper from his own cruise with a different program called VERTEX showing that if you added iron to very carefully gathered cultures which were not contaminated with iron from the ship or the sampling or anything else, well you'd get a grow out of big cells. And that's the iron limitation hypothesis. And we got no credit, he got it all. So it doesn't matter what you—who gets the credit; it's great to learn something.

So that's been very important. And I realized that the mechanism was that the small cells that could use the iron and that was there as a part from what he added, or the ones that protozoans could eat, and the protozoans were the food of the copepods. And we later showed that by showing that mostly copepods had very small amounts of chlorophyll in their guts, yet they were growing and everything was normal about them. Much of that work was done by a UW grad named Mike Dagg, who was at the time working in Louisiana but came and joined us on these cruises.

So that was the kind of biggest hit in my career, and one of the observations that we made was that there were two kinds of *Neocalanus plumchrus*; there were red-orange ones and there were bright red ones. And once I saw that with them live, I started looking at them and went back to the samples. And Martha Clemens went back to all the samples from the weather ship and showed that that animal - which I named *Neocalanus flemingeri* after a famous copepodologist who had been one of my teachers at Scripps - showed that oh, they're quite different; they not only look different, they have a different life history. When the males mature first in the spring in early June they go down to about five hundred meters and then the females mature; most of the other species rest as fifth copepodites, these rest as adults. So the females go down through the five hundred meter layer, they gather sperm from mating events, and then they go on down and rest until January when their ovaries mature. They go up a bit, not very far, and spawn.

So that's how I got really interested in resting phases, the diapause phase of copepods, which then I worked on a lot in the '90s on a project out of Woods Hole with Wiebe and many other east coast oceanographers, looking at *calunus pacificus*.

I'm sorry, *calunus finmarchicus,* the Atlantic dominant of this grazing group. They're a little smaller than neocalanus but they're very dominant in the Atlantic.

MD: Yeah that's one of the things that I did pick up from reading some of your papers and things that I could find, is that you went around the world trying to solve these questions, in Japan, Norway, things like that. So you're kind of inter—

CM: Interacting with people in those places.

MD: Yeah, you're internationally known and here you are in Corvallis.

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CM: Right, and because of the way the institution is set up you become more famous globally than you are locally. So I would be the only zooplankton expert around, and nobody else is very interested in those issues. There's a famous statement by Vonnegut that scientists basically only pay attention to what they're working on. And the story of ice-nine, you remember that? Where some general gets hooked on how some scientist has figured out how to make water that freezes when you look at it or something. Because all the other scientists were ignoring what he found, it was able to go off and become this disaster for the world, when it should have been stopped instantly. And I'm not talking about an actual disaster, it's just that it's recognized that scientists get incredibly focused, and you find that here.

For example, in the early days, one of my first projects here was cooperating with Bill Pearcy and Jeff Gonor again and classifying the zooplankton coming up in the samples of a study they were doing at the coast of the larvae of shrimp and fish, and shrimp and crabs and fish. And how that worked out was Bill Peterson walked in the door one day and said "I hear you need a check. I've been working on zooplankton at Hawaii, I know a lot of stuff about it, I'm very good at identifying; hire me." I said "okay, you're hired." And then that worked out to an, almost from then on, lifelong connection. And we worked out that there are patterns of the zooplankton that respond: species distribution patterns that respond to the circulation features of upwelling on the coast here. And getting the physical oceanographers to pay attention to the fact that they had these tags with names running around that showed them where the water had been and where it was going, it was impossible. They didn't care.

MD: It's putting all this science together to solve the big picture, and it's this little piece and this little piece—

CM: It's very hard to get the whole picture put together, like collegiality. And I think a lot of that's changed in recent decades, that more and more everybody understands that there might be something over there that informs us about over here. And one of the outcomes of that is these papers with massive lists of authors. Thirty authors isn't at all unusual. Did they all write the paper? No, they did some one thing. Did they read the text before it was published? Maybe.

MD: Well, and one of the things that actually struck me was, here you are doing this research on a specific type of organism in the 1970s way before the whole idea of global warming has been brought out. Now did you see signs that the world climate was changing based on your research?

CM: There were a very few signs that the world was changing, but there's a personal story involved in that. The guy who taught the chemical oceanography core course at Scripps was David Keeling, and David Keeling was, I'm sorry to say, one of the worst professors in the classroom that you would ever meet. He came and he said "for science you have to memorize the Greek alphabet and you should memorize the periodic table, and so all of you are going to do that, because I'm going to give you quizzes, pop quizzes on that," and there was a rebellion. Jack Corliss, the guy credited here with large aspects of the discovery of hydrothermal vents said, "I won't do that, that's crazy. I'm not memorizing the periodic table," because Keeling meant all the numbers to the seven decimal places, which he never actually tested because no one could actually do it. But I just memorized all the columns and all the rows and all the Greek letters and passed the test and moved on.

And I had been studying electrochemistry in my chemistry minor because I was pretty mad that I had p-chem and other stuff. So he thought that the most important science on earth was electrochemistry and everybody should understand it without blinking, and I did already. I've forgotten it since, of course, but so I got through, I got my A and went on my way. But he was also, early on, he was about five years, four years, '58 through say '63, '64, to measuring the atmospheric carbon dioxide on Mauna Loa, at the lab on Mauna Loa's slopes. And already it was obvious that there were these annual cycles in the CO_2 abundance, which were instantly understood as photosynthesis versus respiration variation with season, and it was going up. And by five years you could see it was going up.

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And Roger Revelle, the director who had sent him on to this project, had already said, as had Svante Arrhenius back in the 1800s, that burning fossil fuel was going to increase the CO_2 in the atmosphere. And as early as 1858 an Irish physicist had shown that carbon dioxide absorbs infrared radiation, he just called it heat radiation. But he was already aware of the water vapor effect, not interacting with CO_2, but water vapor did the same thing and that water vapor would be a partly controlling factor in climate levels.

So by the time I was studying at Scripps it was the talk of the town. We knew that this was coming, it would be very important - it wasn't yet - and that the thing to do was slow down the use of, and particularly the growth of the use of, fossil fuels. The news was important to us then; we had no doubts. I've never had a doubt, and now the changes are just every day. And what I've gotten involved in the last four or five years is community action and informing people about the science mechanisms, which is very weakly understood on the street - I've got a booklet I throw around - and fighting the export process from the United States. The law in 2005 that fossil fuel exploitation would have all kinds of eminent domain privileges, which has allowed fracking to be forced on landowners all across the Rocky Mountain front and Pennsylvania. And they've got more gas than they know what to do with, and particularly like to exploit LNG. So I've gotten to studying with colleagues here, geologists here about the subduction zones and the tsunami risks and saying—I go all the time to Coos Bay and Warrenton and say, you know, "we're not going to do this. Somehow we're going to not do this." And we're fighting major corporations, so global warming is a big deal in my life.

MD: Yeah, I want to get into your activism later. One of the things that really strikes me though is you're focused on one type of organism, and what you see in that, and then you can extrapolate that to the changes. Did the changes in the temperature of the work, does that affect the species of plankton as well, like the temperatures of the oceans that are changing?

CM: Yes, it's beginning to. There are beginning to be serious signs of that. What determines the distribution of plankton organisms is very largely the circulation; where the water flows. And so as things warm up, we see farther and farther northern extensions of high temperate species, extensions, at least during El Niños here, and a lack of subtropical species coming north along their coasts. And they wash back and forth. They usually have a core habitat and if that expands by an outflow of warm water they'll go, and the same for the other direction. And that was obvious in events happening in our annual variations of both water flow and zooplankton distribution, as early on as the '60s.

MD: Well you've talked a lot about some of your significant colleagues, but as a professor here and a researcher here, do you have what you consider to be major accomplishments, significant memories over your—I mean it's hard to narrow them down, but...

CM: Well I've taught the core course many, many different years in biological oceanography, and that allows you to become more of a generalist in that broader subject than just zooplankton. So as I built up over the years teaching for that core course, I started writing. And early on, as early as the early eighties, I developed a sequence of notes which were completely typed and originally Xeroxed and handed out, and later on you could have them printed out or put them on the web. I really never did that. I always handed them out on paper and I said "okay, everybody put your pencils away, and you don't need to take notes because everything I want to say is right there." And then we could discuss the issues. And I teach by a quasi-Socratic method; get people waken-up enough to ask questions and talk.

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A lot of students found that very difficult, so I always had a kind of, what's the word, two-peak student evaluations; "I hate this guy, he's terribly difficult because he makes me talk, and he'll say 'no' when I say something wrong," and students who really got into it, felt they were being challenged and that that was worth making something of.

MD: Have students changed between 1972 and when you were—

CM: I've often thought so. One of the problems is that—problems for saying anything about that—is that in graduate school, with a few exceptional courses in biological oceanography or that famous incidence of Oceanography 331, you're talking to very selected people. In American education the first truly selective step is grad school, med school, law school, seminary, all of those professional level, occupational and highly trained academic disciplines. And very few people make it up the glass wall to get over that barrier. It's five percent that become physicians and preachers and all the rest. So by the time you're talking to those people, they're pretty good. And coming in here, by golly they do know calculus; many of them know it vastly better than I ever did.

I remember setting a problem in the diffusivity theory; how fast do nutrients come into a phytoplankton cell down here in the viscous realm of very small spaces in the water? Jonathan Nash and his friend, who's on the faculty too now, produced brilliant analysis of this whole thing, a complete solution of phenomenal double differential equations, and it was great. So you know, you get really good kids. Some of them—a lot of them can't write very well, but writing is vastly more difficult than people credit it with being and it takes more than college to get it really rolling smoothly. So we worked a lot on that. I had everybody write term papers, I had them write it twice. They'd turn in a draft, I'd rework it with them and then they'd write it again. And that was the best teaching I got to do.

MD: Yeah, I relate to that. Now one of the things I can't get away without asking is, your relationship and the school's relationship with the Hatfield Marine Science Center. I understand in the early to mid-'70s there was a movement to move the School of Oceanography from Corvallis to the new Hatfield Center—I have seen that top ten, the memo that went out about that change. What's the big memories of that whole—

CM: Well the dean then was G.—I don't know what G stood for—G. Ross Heath, and he was in my class at Scripps when I started in 1963, end of '63. And he and a number of the students of van Andel, a former Dutchman who had been a research associate at Scripps and got on here as a professor of marine geology; Teddy Moore, Ross Heath and Jack Corliss came up with him. They had not finished their degrees and they finished them here but took the degree from Scripps. And they went on to be faculty members, all three of them, here. And Health was a phenomenally ambitious guy and when Byrne left to become a—I forget why he left exactly at time, whether that—let's see when that was... when he left he became vice president for research for a while and then went to Washington as the head of NOAA, or the other order around, I forget. There's another story about that.

[0:45:14]

So, Heath applied. And actually the faculty went to the president and said "this is the guy, he's smart, and everybody else they supplied doesn't really know what they're doing and they're not connected to us, and we want him." Well then Heath turned out to be a kind of guy who just did what he thought he should do without talking to anybody, and so he planned this whole thing to move the College of Oceanography to Newport. And at the time, a lot of us had children, and the schools there were nothing like so good as in Corvallis, they had no computer capacity and even had to come and go here to use the computer facilities of the university. And Newport was not the cultural center that Corvallis already was. And we weren't moving, by golly, so at one point I wrote some kind of memo, but I also called a meeting of the whole faculty, and we voted it down. I'm not sure it was unanimous but it was overwhelming and he said "well, okay."

MD: But there's still a relationship between—we just didn't physically move the department, but there's always going to be the relationship between Hatfield and the oceanographers here.

CM: It's very tenuous for many of the faculty here. Hatfield developed in several directions simultaneously, really. One was it started out as a building project that Wayne Burt created using depressed area funding to produce economic opportunities in depressed areas, which Lincoln County was at the time. So that kind of got divided early on between ODF &W and the university people. And Jeff Gonor was there and he brought in—Burt brought in Joel Hedgpeth because he thought Joel would be a big grant getter. Joel hated the grant system, he didn't write proposals; he didn't want anything to do with it. He thought the university should be paying its faculty and supporting research from funds they somehow generated.

Well, Burt had built this place, as I said way early on in the interview, by getting grants from not only ONR and AEC but NSF. And NSF built at least two of these buildings, the money came from there through Burt, that's why it's Burt Hall. And so there was all kinds of machinations to get Joel to kind of go into the background and finally leave, and it was quite unpleasant. He was hurt. But Byrne became sort of the official director and the place was actually run by a guy named Dave Zoff who hung out around here with a—he always did all his business inside an aluminum portfolio he'd flip open, and there's be a piece of engineering paper in there that had his notes. And he'd figured out all kinds of schemes to make the financing of the college work. He was funneling money off of everybody's grants at one time, he got caught at that and they shut it down. And it's very interesting. He's very hard—he didn't talk to people, he just did stuff. And that suited Heath just fine, and so they got along well.

When Caldwell got the job after Heath left, Zoff was gone instantly, and a kind of wave of relief spread over the place; "okay, now we don't have a kind of dictator system." And Caldwell was brilliant at bringing people together to write joint proposals, and that was kind of a beginning of lots and lots of cross fertilization that did happen. And he basically realized that I could write well, so he put me in charge for - during the writing - for our committee of people. We would write proposals. None of them were very successful but he was using a—he brought in a whole bunch of high-powered mathematicians, Bob Miller and Andy Bennett who were doing mostly Navier-Stokes equation modeling of lava flow, and they were spectacularly good at what they did, particularly Bennett, I think. And it was big, a very good time.

MD: Well I had to ask, because we've been doing a lot with the Hatfield Center because of their fiftieth anniversary, and that's where your name came up.

CM: Well, when I had that sophomore year invertebrates course at the coast that I talked about as kind of getting me into this, that was taught by Hedgpeth and Gonor, as I said, so I knew these people. And Hedgpeth was a cantankerous old guy and he loved to insult people. And one day I'm sitting at my desk at Scripps and he comes in the room - he was a friend of John McGowan's. Hedgpeth had originally had a job at Scripps and that's where he wrote his famous book in the geological annals of the American Society of Geology, whatever it's called, GSA, and he insulted me some way and I stood up out of my chair, I'm a good foot taller than he was, and told him off. And that was the right thing to do. It turned out I got all kinds of respect ever after that. And it was hard for people to learn that, that if he insulted you, you fed it back to him and then he'd behave. He had a very good memory.

[0:50:49]

MD: We've talked earlier about you being kind of internationally known, and so there's a lot of collaboration going on. Now is this collaboration within the zooplankton community? Or does it filter out from your specialty to—

CM: Both, yeah. Well, my involvement in the Georges Bank Project that was mostly done out of Woods Hole in Rhode Island. It probably got started because a man named Kurt Tanda [?] in Norway wanted to go on sabbatical in the United States. He had a sabbatical and he wanted to go to the United States and he had to pick a place that seemed suitable. And suitable turned out to be safe. A lot of both Japanese and Norwegian, northern Europeans, considered the United States terrifyingly dangerous. They heard about the murder rates in New York and the difficulty of the riots in LA and all these things were very much still very prominent in their memories. So Corvallis, what is that like? Turned out that if they actually looked up real police records of crimes per capita, we were just as bad a crime site as New York, but they didn't know that so they would come. And there were two people did that. One was Tanda.

And we wrote a model based on some data he had of the life history of *Calanus finmarchicus* in the North Atlantic, which included the diapause and spawning rates and development rates. And they were very good data created at that time by a man named Robert Campbell at Rhode Island, working with Ted Durbin, on how rapidly this copepod went through its life history stages and what the patterns were as they changed with temperature, because they'd used four different temperatures of very careful rearing experiments. And they saw the effects of food limitation, which changes the amount of growth but not the rate of change of the different stages between molts, and all of that could be really mathematized in models, which I mostly did, and Tanda helped, and we published a paper and that's how we kind of got, I got involved in Atlantic oceanography. And then that led to the Gulf main project, and Georges Bank Project, it was called.

And that produced a bunch of trips. We created something which was the International Year of Calanus; the Trans-Atlantic Study of Calanus Project, TASC. And we had meetings in Europe and a final meeting at—that was '87 that we did that, and the, was it '87 or '97? '97, I think it was '97. Anyway, decades all get scrambled up when you're seventy-five. And that was a lot of good stuff came out of that. We know a lot about that animal we didn't know before. And still we don't fully understand the diapause process; how it knows that it should go to sleep now. Is it the temperature? Is it they've built up enough lipid stores to last them through the summer and subsequent early winter? Or something else? And we don't know how they wake up, although I'm pretty sure they wake up after they've been down long enough at depth.

So the Japanese interaction, one of them was with a guy named—oh, the SUPER project got me invited to a symposium in Nakhodka in the eastern part of the Soviet Union. To get there you had to go to Japan, and I went to Japan and the ship was a Russian ocean liner that took us from Yokohama to Nakhodka for the meeting. And aboard was a Japanese expert on plankton, and he invited me to come for it. He was the director of the ORI, Ocean Research Institute, at the University of Tokyo. He invited me to come for a prolonged visit and basically hired me as a Japanese full professor for three full months. Turned into four months that I spent in Tokyo and traveling around Japan looking at samples of the neocalanus in Japanese collections. And that produced all kinds of interesting information. We can skip the ecologic details, but yeah that was great.

[0:55:36]

And I got associated with a bunch of Japanese graduate students at the time, because I was in the lab with—in an office with one of them, and he later come and did a post-doc with me. Asushi Sudo [?]. And we did a fun thing, which was we got an enormous thing called a kreisel, which is a circular aquarium over a meter in diameter and a foot thick, thirty centimeters thick. And we got fifth copepodites, the last larval stage of our local calanus here, we brought them in and waited till they matured. The fifth copepodites were sorted into a beaker and they matured, so there's no males around. And we took males from the field and we put them in this kreisel, a dozen or so, and then we put a dozen or so of these newly molted females in there, so they haven't mated, and figured out how the male finds the female and gets ahold of her. And he was wonderful to work with because he had incredible, I shouldn't say it, but Asian patience, and we both sat for hours and hours in a cold room watching these things.

And what happens is the female hangs by her antennae, her body down, and slowly, slowly sinks. Occasionally she'll jump back a little, but slowly, slowly sinks. And in the kreisel she can get about this far down [holds arms about three feet apart] before this very slow flow would bring her back to the top and she'd float down again. So there's these females going down and the males are these gorgeous swimmers, and Sudo would say [in false accent] "oh, such beautiful swimming." That's German, not Japanese. But they go across and they go across. So they're searching at right angles across the tracks of the females and at some point you'd have seen a female go by and the male would stop and he'd do this crazy dance and then he'd swim right down and he'd hit the female in the head. And then usually she'd swim away, but he could follow and they'd go around and very few times would—it didn't end with her stopping and waiting for him. She'd be gone, he couldn't keep up.

And Frost had shown earlier that females are producing pheromones that are sexual cues of where to find me, I'm down below wherever you've come across this track. And he did that by all kinds of—he grew radioactive females using radioactive food and when a male had been in the tank with a radioactive female long enough he would have a radiocarbon stuck to these little olfactory sensors on his antennae. So she had gotten something from her to him and it had to be a pheromone. Sudo and I learned how it worked in the field by using this big tank. And we knew that it was impossible to mate copepods in ordinary beakers. And by going big enough we could reproduce this—let them reproduce this pattern that they would use.

MD: And these were with chillers, because this is cold water because of the depth and—

CM: Yeah, we're in a cold room keeping the water cold with the air of the room. So we were in winter clothes, running around with gloves. And he came back after finishing a post-doc with a camera just like that one basically and we got videos, and we could work out the speeds and the timing and he wrote a lovely paper, which I put in good English. And that's published in the *Proceedings of the Royal Society of London*, thanks to a copepodologist in England who organized a series of how the males find the females among copepods in the ocean. Copepods are the dominant kind of animal in the plankton. And so we had one photograph of a male grasping a female, and I drew that up and put in the paper. So I figured the queen should have a picture of copepods copulating. And now she does.

[1:00:09]

MD: Now that's one of the things that I've noticed, that, well I guess it's publish or perish, but you're well-represented in the literature.

CM: Actually I'm not overpublished.

MD: Really?

CM: There has long been a practice, and it's gotten worse, of least publishable units; break it up into as many pieces as you can because, as John Byrne once said to me; "deans can count, they can't read." And so the length of your bibliography is very important. And I tried not to let that happen. About half of all scientific papers ever published are never cited. I haven't written a single paper that wasn't cited. Am I proud of that? You can hear it. [Laughs].

MD: Yeah, yeah. I would like to get into your lifetime of activism. You are an activist on a scientific level. Now you were talking about the whole idea of them building down in Coos Bay this terminal in a tsunami zone.

CM: Yes.

MD: It's a current issue here in 2015, but when somebody watches this oral history twenty years from now, what are they going to find? Is there going to be that terminal down there? Is it going to be in danger of a tsunami?

CM: We don't know yet. And it won't be in danger of the tsunami if it's already happened. If it hasn't happened, it's closer than it was now. It's not clear how it's going to come out. The most powerful public opposition to building the terminal comes from the landowners, around five hundred of them who are spaced out along the two hundred and eighty-two mile pipeline that will come from down near Klamath Falls, catty corner up to the northwest to Coos Bay. It crosses four hundred waterways or wetlands, it crosses three major rivers by submerged hydraulic drilling under the river bottoms; the Coos River, the Rogue, a third one I forget. And they are up in arms.

And the reason is that the pipeline is going to be a three-foot diameter pipe, it's going to be buried, it's going to carry natural gas at 1500 PSI. The atmospheric pressure is 14.7, is the average PSI, ounce per square inch. And so that's a hundred atmospheres pressure, which is the pressure at a thousand meters in the ocean. A lot of pressure. So any weak point in this pipe is going to pop at some point after a little corrosion or anything from the surrounding soil and water. And because of the eminent domain procedures that have been legislated by Congress, the company that wants to do this can take the wetland away for a song. It's about a third of the assessed value, whatever it is. And of course they don't want all the land; they just want the ninety-foot wide stripe that goes across your piece, sir.

And so far, almost none of these five hundred people have let the surveyors on the land, so it's kind of surveyed from the air and guessed at. You can do very good surveying from the air, but in many aspects of the thing its travel along a tortuous route because of avoiding the most vociferous of these people and so on. There is a very active, very excellent group of opposition in Coos Bay led by a fine woman named Jody McCaffree who's been at this for over ten years. When the plant was first proposed it was as an import plant, which is now seen as a stalking horse for the eventual export plant.

On the other hand, in Coos Bay there is a fairly large coterie of people who want to build it. They include the city council, the county council, the local school boards. The local school boards have been promised a lot of cash down a decade or so. They're already fighting over how to divide it up. I think it's fifty million dollars or something like that. But the opposition has suddenly gone statewide. All kinds of groups in Eugene, in Corvallis and Portland have been saying "we can't have this," partly because it's just one element in the bakken crude export, the coal export, that are sought for by activities in North Dakota and across the Wyoming and Montana Powder Basin.

[1:05:24]

So how this will go is, I don't know. We're having, for example, a statewide gathering on the capitol steps in Salem on the twenty-sixth, and the idea is to tell Governor Brown that she indeed can do something about this, and what she can do is declare that these facilities do not meet with the requirements of the Oregon Coastal Zone Management Plan. The plan was developed by the state Board of Land Conservation and Development, LCDC. And they have a very good plan and it represents a law passed in 1995 that no hospital, school or dangerous industrial facility can be built in the Oregon tsunami zone. And that bill included money to survey the tsunami zone, so we have these gorgeous tsunami run-up maps. They aren't all we need, as they only represent the first wave arriving, and the first wave is not the last wave, but okay.

So when you go to a meeting about—a hearing, let's say, a Federal Energy Regulatory Commission Hearing, the commission has already produced a draft Environmental Impact Statement for Coos Bay. You end up in a room with four or five hundred people of whom about eighty-five or ninety percent are wearing green shirts and they're union guys, many of them trucked in, and they want this job. And that's understandable; most construction union people work on temporary jobs, one after the other if they can pull it off, and they don't care that this is temporary work, a couple, three years, probably four years, to build this thing, and it'll be over for them. And the jobs will drop off to about a hundred and twenty or so. But that's how they live is time to time. So they're an important component in this.

Another important component is the Rocky Mountain State members of Congress; John Barrasso is the first signature, he's a senator from Wyoming, Republican senator from Wyoming. He's the first signer of a letter to Cheryl LaFleur, the chairman of FERC, Federal Energy Regulatory Commission, saying "just go ahead and give them the final EIS and approve this thing, because we need the jobs of bringing the gas up from fracking, we need the taxes that the gas will produce, we need this resource exploited in our state, because it's money in the bank for everybody here." And the quality of that threat is that those guys can write a bill which will use the Interstate Commerce Clause of the Constitution to override all local things. So we need to get the governor to say no as soon as possible so they can hear that those people are opposed by people here. Lots of them.

And the people in Warrenton, the Columbia River terminal, are extremely well-organized, both the pipeline and the people in town. And they have gotten the County Commission to vote five to nothing against any kind of county permit for the pipeline, and that's been upheld finally, after lawsuits and years of delay, by the Land Use Board of Appeals, LUBA, in Salem. And the document from LUBA is beautifully written. It's not going to be successfully challenged in court. So, so far so good up there. But whatever the Congress produces to force through the JCEP plan, the Jordan Cove Plan, will surely include Warrenton. So it's very dangerous times with international, national, interstate competition over whether this should be done.

MD: And your involvement with this is altruistic, based on your scientific knowledge? Or do you just know that this is a bad idea?

CM: The advantage of a scientific education, particularly in oceanography where you have to do your chemistry and physics and geology as well as your biological specialty, is I can read the literature and I can read technical documents about engineering designs and other things. And so I've been writing comments on all the government things and talking to the people and trying to explain things, explain how liquid—how natural gas is liquefied to a liquid, changed to a liquid. It's not compression, you don't just squeeze; you refrigerate it, and that too is very dangerous because your refrigerants on the inside of the refrigerator pipe, as opposed to the outside, are also methane, but ethylene in the middle, in the center of a cask, a refrigeration in cascade in three steps. You take it down to -260# Fahrenheit, which means that it will sit there in a puddle slowly evaporating rather than just explode into gas, like water sits in a dish until it slowly evaporates.

[1:10:46]

Liquid natural gas that cold will do that too. So you can keep it in a thermos bottle, it doesn't have to have an extreme pressure case. And that's how the tankers work. And any gas that does evaporate they suck off and run through the engines and it burns and runs the ship. Which is cool, we should do it that way.

MD: This is along the same thing as the coal trains that are a big issue in Albany and in—

CM: Absolutely similar.

MD: And there are coal trains coming through and also the oil, what with the recent derailments and explosions. It harkens back to the mid-nineteenth century where the Northwest has been an extraction colony. We still are.

CM: Right. At this point the main concerns are the export route problems for all three of these fuels, and there are twenty-eight proposals currently for oil, gas and coal terminals from Bellingham south to Coos Bay. And with respect to natural gas, both Washington and California, which have better ecological laws and, in California's case, the Coastal Commission, which said "no LNG" long ago. And both of them have many more representatives in Congress than we do, so even though our representatives are mostly Democrats, our power to say no is very limited. It's partly very limited because state law doesn't have very many places to grab on.

The DEQ legislation is ridiculous. Those guys, in order to write a permit, have to write it from the material that's submitted by the people who are applying for a permit, and that's true about air quality, water quality, everything. And they are not allowed to bring in external research. They can look at the text and say "this isn't good enough" but they also have to say "provide us something that is," and of course the company that wants a permit goes ahead and does that, somehow. So DEQ is very weak, and friends of mine who work there admit that, but they have to stay within their legislative framework. And they do, or they get fired. Well, I said "go ahead, get fired," as if I could hire them.

MD: Well it's good to see a person who lives the good fight.

CM: Well, we're trying. And there are a lot of people in town engaged and some of them are getting mentally disturbed over the stress of the whole thing. They just see the future as burning up. And I don't worry quite as much about that as they do, because I know something about the long past. Somehow life on earth came through the Permian and Triassic extinction, which was largely based on CO2 in the atmosphere coming out of huge volcanic eruptions in Siberia. And huge ocean acidity, 90% of ocean species died. And yet we have an elaborate system now. It'll come back. In the Eocene, I don't know, fifty-five million years ago, there were tree ferns and crocodiles in Ellesmere Island in the Canadian Arctic. And that was a CO2 issue. It was much more and the world was much warmer, and the tropics didn't go away, they were still there. They're less affected than the Arctic. So something will be here, whether it can be people in any reasonable numbers living comfortable lives and enjoying videotaping of their [gestures at camera], I don't know [laughs].

MD: Well we always like to catch up on our subject's family life; kids, grandkids, that type of thing.

CM: Oh, do I really have to go public with all my peccadillos?

MD: Well maybe not the peccadillos, but how about-

[1:15:03]

CM: Well I married before graduate school after college. I got married and we—I was married through grad school and my former wife and I had two sons, one of them is age fifty in January; he's a transportation planner for the city of Bellevue in Washington, and my younger son who is turning twenty-seven on the twenty-third of May—forty-seven on the twenty-third of May—is in a—Freudian slip, forty-seven—is an author. He has a really interesting novel out about his former job as a travel magazine writer, for who he still is working as a stringer, freelance, and at the moment is in Australia on a month-long tour, all paid for by tourist boards and the like. He's been on three multi-day hikes through the outback and southern mountains and other places and he's, as much as a writer he's a tango fanatic.

So eventually we divorced, that woman and I divorced, and she became a Unitarian minister, quite successful at that. And I went through several four-year relationships, and those were good and difficult and ran aground on one thing or another. Actually the children can run a trial relationship aground because they're already there and they don't necessarily want anything to do with it. And then I married my technician in the SUPER project, and we've been married for thirty-two years. And we have a daughter who's thirty-one years old and currently—well she's a very bright person; French and Spanish major at—Romance language major at U of O, Phi Beta Kappa and so on. Well, there's not a whole lot of jobs doing that, so she took off and was a ski, snowboard bum and waiting tables in a very expensive restaurant high up on Steamboat Mountain in Steamboat, Colorado. And now she's living in Portland and is a pre-nursing student, hopes to get into nursing school by next year. I hope so too.

MD: You've been officially emeritus of a number of years, and now you're sort of officially kind of kicked out of your office and you're really, really, really retired now. How do you spend your days?

CM: Oh, well I retired in 2003 and at that time the state was suddenly waking up to the fact they'd made terrible mistakes around PERS. And the handwriting was on the wall and it eventually came out of an invisible ink stage and was readable and I filled out the retirement form so fast you couldn't—I literally jumped in a car and drove them to Salem and handed them in. And that got me Money Match. And I'm not as well-off as the football coach at U of O was, but it's very generous. It's absurdly generous, but thank you very much, I can do good things with it. And then immediately I got a very nice grant to study the mortality rates of copepod eggs in the field. The ones that are freely spawned were believed to almost entirely have been eaten or died before they could hatch, which the study showed wasn't true, and that was a lot of fun, took about four years in the middle 2000s.

After that I had written, right at that time, for publication, a textbook of biological oceanography, which was quite successful. And in 2010 to '12 - 2010, '11, '12 - a former colleague here, Patricia Wheeler, who was a phytoplankton ecologist, and I wrote a second edition. And that's still selling, it has very good results. It's really the only book that's taught that addresses a graduate level of interest in the subject matter. It covers benthos and plankton and phytoplankton and zooplankton, phytoplankton and climate change and many other issues. And it was studying for the chapters on climate change at those two times that partly kind of prepares me to talk to the public about climate change.

[1:20:03]

MD: And then you have a business where you help edit scientific papers for people?

CM: Right, I gave you my card, right? So I was editing this morning starting at seven o'clock for a couple or three hours - I put in four or five hours yesterday and three today - a paper from a combination of Greek and Spanish authors, of all things. A paper for the *Journal of Plankton Research*, *JPR*, which is now—it started off as one guy's personal journal, David Cushing. And as he aged out and then died, it was taken over by several different people and now it's run by a really crackerjack plankton ecologist and biologist named Roger Harris in Plymouth, England, who's a very good friend of mine. And so I've started to get some jobs for that journal, and this one was for that journal. And what's typically the case is that scientists all have to be able to read English and they also have to be able to write it for publication, because if you don't write in English nobody reads you except a few guys around your shop. And that's one of the reasons that the world literature in every scientific area has exploded, because English has become the mandatory Latin of the modern age.

MD: Yeah. And for a while it was German, then it was-

CM: It very limited parts of science. The Norwegians very early on could write in both German and English and mostly chose English; Norwegians, Scandinavians, because they were very dubious about Germans, as proved to be a good idea at that time. Not anymore, I think. Anyway, so you get these papers and basically the English is good enough you can understand what they're saying but the Spanish always put the adjectives on the wrong side of the nouns and yadda yadda, and it's really difficult to conjugate verbs properly. And oriental languages don't have articles - an, a, the - and so they don't know how to use them. So they kind of stuff them around and they're missing where they belong and they're present where they don't belong and I go through and change all that.

And this has been made really feasible by electronic computer word processing, and I send them a file with change tracking showing all their stuff crossed out and what I suggest as substitute put in. So red is what's out, blue is what's in, and then I make notes. And if it's a subject I know about - and I almost never get anything that I don't, because it's plankton or marine ecology of some kind - and I run into statistics that are wrong or odd or misinterpreted, I'll highlight the text and put in a numbered note and go off and write an explanation of why I see it's a problem. And if there's a phrase I don't understand I'll highlight that to get a note number to go with it: "I do not know what this means, you have to try again. It could be this, it could be this, it could be this, you can see that it's ambiguous, let me know and I'll read again when it comes back."

So often the exchange, once I've sent it back, after four days or a week it comes back within a week and after three weeks we've got it down where—it doesn't mean that the English referees, that referees don't criticize it. They would criticize Hemingway and Jane Austen, you know. And the people who are most critical are clearly themselves English as a second language people. They think they've really got it. So it's fun and you save people from things like, the previous paper, last week's paper, had an equation in it and the words described in the equation just didn't describe the equation. And it was clearly the equation described by the words that they wanted to be using, but this wasn't the equation. So I wrote all that out and I said "no, the square root of the sum of squares is not the same as the sum of the square roots of the squares," which are just the numbers. And so "you've got to get this right," and she comes back, she says "oh, thank you very much. I miscopied the original equation." From somebody else's paper.

MD: Yeah. Well, after years of service to Oregon State University and your field, do you have any final words of wisdom that you'd like to impart to—

CM: How much time do I got?

MD: Yeah, well yeah. I mean within bounds of reason.

[1:25:15]

CM: Yeah. I do wish that more of my colleagues, who are very attuned to climate change and the scientific specifics, would get out and campaign for sensible reductions in fossil fuel. And some of them are really good at interpreting their science to the public. Some of the geologists in particular have been talking about the subduction process and the tsunamis that can result and getting on public television with trips to Japan to sort of look at the T#hoku case. Chris Goldfinger in particular has done that. But he doesn't show up in Astoria to say, you know, "we shouldn't be doing this, how can I help, what can I do?" And he doesn't write to FERC and say "you shouldn't be doing this." And it turns out that everybody recognizes that if you're a plankton ecologist, you do not have professional credentials for talking about tsunamis and other things. I just ignore that and do it anyway. So, they should all do that too, I think. More of it would be great.

And at the same time I recognize they've got a very difficult job here, because they're still raising all their salaries out of grants, which is a punishing process. And for, particularly the young people, that's become very, very difficult. I don't know how they're going to make it, some of them. They produce as many as ten well-founded proposals that go down, go down, and that's because there's so little resources – a) there's so little resources and b) particularly in the oceanographic university system, all the places that have oceanographic departments - Scripps, Woods Hole, Texas A&M, Miami, it's a huge list - have overbuilt. There's too many people begging for money and not enough money to make it happen.

Actually there's going to be cut-backs again, coming, pretty clearly. There's an NSF memorandum today from Candace Brinkley at NSF saying "hey, a thing called sea change has just come out from the National Academy and it's requesting a 20% reduction in this and a 5% reduction in that, 10% over here, three different reductions," and then it says "and we agree with all of those proposals and we're working on how to do those reductions." Well if you're over here in the halls, that's squaring the punishment.

MD: Oh yeah.

CM: I saw many NSF proposal reviews, and the most recent case was about three years back, I finally told them I didn't want to do that anymore. I can't keep up with the literature, for one thing. It's just coming too much, too fast. And there were proposals in there which were badly written, bad mathematics, bad, bad, bad. And I say, "wait a minute, these authors on this proposal are really good scientists, they've done great papers, I read their stuff, I teach them; what's going on?" And one of the things that's going on at soft money institutions is people have to look like they're trying, and you can try by sending a proposal. It used to be, in the '70s, Byrne actually read your proposal before you could submit it. Nobody reads them. They look at the budget to be sure you've covered everything, they're very careful about, did you correctly characterize the changes in facilities that need to be supported out of overhead, blah blah blah blah. But to read the text about the science for anybody, there's nobody else checking the equations and other stuff. It's really hard times in soft money science, at the same time that it's going forward.

MD: Yeah. There's advancements, but it's all about the money, yeah.

CM: A lot of it's about money, and it's very important that it not all be about money; that we do have a lot of new things we need to learn and can learn about nature that would be valuable. And so it needs to persist, but I'm not sure how to make the financing better, or how big it should be as an enterprise in society. Certainly no bigger than it is.

MD: Well Dr. Miller, Charlie, it's been an honor to learn about the Oceanography department and your career and more information about plankton than I have ever known, so on behalf of the OSU Sesquicentennial Oral History Project, we want to thank you for your participation, and enjoy your retirement.

CM: Oh, I am enjoying it, and thank you for letting me do this. I hope you're very careful with the editing [laughs].

[1:30:22]