James J. McCarthy – Podcast transcript

PART 1 (1970-1982) – Early Career and Development of a Cross-Disciplinary Approach to Oceanography

Alex Griswold: I'd like to ask you about early influences that prompted you to go into biological oceanography.

Jim McCarthy: When I was in college, I thought a lot about different careers, watching many of my friends head to various professions, various branches of academics. I was really very undecided. I discovered I was quite enamored with history and philosophy and literature. But, there was always an interest in every semester that grew over time in the natural sciences. My biggest problem in making a decision about a future was I couldn't decide which of the sciences I liked best. It seemed each time I took a science in an area that I wasn't familiar with, I got more excited about it. It happened, much to the chagrin of many of my colleagues, that I thoroughly enjoyed organic chemistry. I did. It was exciting for me. It explained things I never could have imagined would make such sense.

Then biochemistry really intrigued me, geology intrigued me. And as I was nearing time to graduate, I realized that a career in science was drawing me. I looked across quite a range of fields. I realized that I really wanted to be in a field where I could be, for a good portion of research, outdoors. I didn't want to spend all of my time in a laboratory, although I knew that laboratory science was going to be an important part of what I did.

And so in a sense, by moving towards a graduate program in ocean science I knew that I could find a career that would give me opportunities to be out of doors, in the field, observing nature, but also one where I didn't have to make a rigid decision between physics, chemistry, biology, or even geology, for that matter. That I could move around within that space, and enjoy work at the boundaries of those fields.

A lot of people would say, "Well, I just love marine organisms, or I love the ocean." For me it was really being drawn to the opportunity for an academic career that fulfilled my desire to work in multiple fields of science, and particularly, the interactions or the edges of them, the boundaries.

It turns out that when I moved into a graduate program, I quickly discovered that a lot of the exciting research was really at those boundaries. In my initial work on plankton and nutrient cycles, I realized after the first two months I spent at sea in the equatorial Pacific that there was some really interesting questions, relating to the physics of nutrient supply: all the way through the blooms of plankton, to the anchovy that were waxing and waning with what, at that time, was thought to be a very local phenomenon, the El Niño cycle.

And little did we realize, then, that this is not just a local phenomenon, but it's one that is connected to a larger cycle in the equatorial Pacific, and one that has ripple effects all around the globe in terms of climate. So, what initially led me into this field and kept me excited, as we moved forward, seeing how the ocean biology and chemistry were inextricably linked, and very much influenced, by the physics of the ocean; which was also, in terms of those inter-annual cycles, driven by a physical phenomenon that extends across the entire Pacific Ocean, the El Niño-Southern Oscillation.

Of course, it took another decade and a half to see that there were ripple responses in climate signals that were evident in North America, and Asia, Africa, and of course, all across the Pacific, where we knew, historically, that this was an important cycle in physical-chemical-biological activity. That's really where my interest initially, my indecision I guess, led to an interest in ocean science and its interdisciplinary character.

Alex Griswold: Were you at Harvard at that point? What happened between your postdoc and your Harvard years?

Jim McCarthy: I finished my PhD in 1971 and had worked on the development of an application of methods that would allow us to use, at that time, some very new techniques with a heavy isotope of nitrogen, nitrogen-15, to actually tag samples, and see the rates at which different forms of nitrogen are being utilized. At that time, we knew the importance of the dominant form of dissolved nitrogen nutrient in the ocean, which is the nitrate ion. We knew there were forms of nitrogen available in smaller quantities, like the nitrite ion, and the ammonium ion, and some dissolved organic forms, some amino acids and urea. But we didn't know how important those lesser quantities were, in part because they were at very low concentration, and we had no idea how rapidly they were being produced and consumed.

And so, with this heavy nitrogen technique, we were able to do comparative studies in different regions. Most of my work at that time had been either off Peru, and I did a number of months of experimental work off the coast of Southern California, Central California, too, to see how, under different circumstances, one form of nitrogen was preferentially utilized by the plant plankton, which were photosynthesizing, creating organic matter, and using these forms of nitrogen for their synthesis of protein.

What we found was that, even though forms of nitrogen like ammonium or urea are there in very low concentrations, they can at times be the dominant source of nutrition for nitrogen in those waters, in part, because they turn over very quickly. That is, they're a small quantity, but they're rapidly recycled or produced in the system by bacteria and zooplankton that are eating the phytoplankton. And we also discovered that, in many cases, they were preferentially utilized. Even though there would be a lot of nitrate present, it might provide a very small fraction of the nitrogenous nutrient, because the more reduced forms, that is, the ammonium and urea, were more readily assimilated, and with less energetic expense, incorporated into amino acids and protein.

When I finished my graduate work, I had an opportunity to stay in California, one to go to the state of Washington, and one to go to the state of Maryland. At Maryland, I would be at Johns Hopkins University. And I was intrigued by the prospect of using these techniques to try and understand what was happening in the Chesapeake Bay, at that time.

It was a time when people were beginning to be concerned about the changing character of the Chesapeake Bay. Part of that, we knew, was due to the introduction of nitrogen and phosphorous from agricultural and urban areas that was seeping into the Chesapeake.

At that time, there was very rudimentary sewage treatment. Washington, DC, up the Potomac, and Baltimore closer to the Bay, were releasing inadequately treated wastewater, that then was contributing to large plankton blooms in the Bay. This was changing the character of the Chesapeake.

We did some of the first studies to look at the relative importance of these different forms of nutrient, and again, see how they were being recycled and retained within the Bay, and to look at the different forms of plankton that were benefiting from these augmented sources of nutrition. This is a problem, that decades later, we're still kind of trying to come to grips with, in terms of a management strategy for the Chesapeake Bay.

I continued then, after my initial three years working on the Chesapeake Bay, to return there for subsequent studies during the '80s and the 1990s, with some new techniques and further advances in the sort of work we were doing. Using not only the tagged nitrogen, but using the process of seeing how the natural abundance of these forms of nitrogen, nitrogen-15, the heavy nitrogen, N-18, N-14, were being allocated in different parts of the nutrient system as a result of natural fractionation processes, and the assimilation and production of organic material and remineralization of that. Those approaches also helped us understand some of what was happening in the Chesapeake.

Alex Griswold: What year did you come to Harvard, Jim?

Jim McCarthy: I left Johns Hopkins and came to Harvard in 1974. At that time, I began moving my interests offshore, began working in the Gulf Stream and the Sargasso Sea, and continued, then, some work in the Caribbean, to expand my range of geographic interests to include further south in the Atlantic, further north in the Atlantic. On, subsequently, to the Indian Ocean, Arabian Sea, and other regions.

Alex Griswold: Was it during this time that you began to see these other interests that first drove you to oceanography starting to expand the picture?

Jim McCarthy: My first interests in the larger scheme of ocean, atmospheric, climate science, really rooted during the period of my graduate education at Scripps Institution of Oceanography. And although I arrived a year after Roger Revelle left Scripps, as the Director who had served for a long period, I caught up with him when he was here at Harvard. Because when I arrived in 1974, he had been here since the mid '60s, and continued for a few years thereafter, and interacted with him a fair bit. But I felt as if I had known him all that period, because his legacy at Scripps was everywhere.

When I first arrived, the time series that he initially conceived of, and the techniques were developed and implemented by Charles and David Keeling, were... What, I guess eight years into it. And everyone was excited about the findings. Excited, but also disturbed, because they were entirely consistent with what Roger Revelle had postulated more than a decade earlier, when he talked about the grand experiment that we were conducting with no control, and, the realization that we could be affecting the chemistry of the atmosphere and the chemistry of the ocean, and life on this planet in profound ways.

This really resonated with me because–although I say my interests were initially in plankton and nutrient cycles–those of us who were in that field realized that plankton have a very profound role in this carbon cycle. Although they've been studied for over a century as organisms floating around on the surface of the ocean, we had also known for a century that they are the source of most organic material, most nutritious material that gets into the deep sea.

Alexander Agassiz in the late 1800s, right here in this museum, came to that conclusion as he realized the organisms in the deep sea had to be nurtured by material that was falling from above. Small particles, maybe microscopic particles. And he coined the term the "rain," R-A-I-N, of detritus, falling from the upper ocean into the deep ocean.

We began to, when I was a graduate student, think about what some of those numbers might be. That is, how much of that material from plankton sinks into the deep ocean and where does it go? We now know, of course, that most of the organic carbon, that is, the residue of plant and animal material, is in the deep ocean. There's far more in the deep ocean than there is buried in all the reservoirs on land, all the soils, all the coal deposits.

We know that in the deep ocean, over time, over billions of years, this material has accumulated, and of course, it's little appreciated by people who haven't really studied these processes, that this is where our oil deposits, and the gas associated with oil, reside today. Geologists don't just say, "Well, let's go drill here and see if we can find oil." They look for marine sediments. And so, where the oil is today is where we either have marine sediments, that is in coastal waters, or ancient marine sediments, ancient inland seas down the middle of North America, or the Arabian Peninsula.

In once very productive waters, the organic material from plankton settling into the deep ocean, accumulating over time, cooking, and microbes using everything they can, but leaving this bit of goo that we can bring to the surface, and with a bit of refining, put a match to, and fuel our society's needs for energy and everything that comes from that.

We were kind of thinking about some of these questions. That is, how much of that material really goes down? Where does it go? As I began, in the early 1970s, talking more and more with Roger Revelle about those questions, during our overlapping period at Harvard, it was clear that he felt that we need to push this very hard with some big international efforts to try and better understand these questions. I was a willing partner in this, and Roger invited me to join in discussions, and at that time it was with a thought to prepare an international effort that would be akin to the International Geophysical Year, the IGY, in the 1950s, which was a very, very important period of international science across the realms of geology and atmospheric sciences, earth sciences general and broadly defined. But it didn't have a very significant biological effort. And Roger thought that this new initiative would be one that would have a strong biological component, and particularly the biogeochemical cycles. And so, I was very enthusiastic about that, and we were seeing more and more that a lot of what had historically been referred to as geochemistry had a strong biological component, whether it's manganese nodules in the deep sea, realizing the importance of microbes in those was a startling realization for some people. Or even the carbon cycle in the ocean, there were people who thought that you could do an adequate job of modeling carbon in the ocean through just the annual cycles of solubility/dissolution with changing seasonal cycles in temperature to explain carbon in the upper ocean, deep ocean, high latitudes, low latitudes. Some of us argued that the biological activity had been underestimated in these cycles.

Roger was internationally revered in this area, and he was able to get these discussions going. It was a very exciting time because we could also begin to see that some of these initial proof-of-concept efforts relating to remote sensing, the satellite observations of surface ocean features, were leading to an era of trial that might prove the value of some of these sensors for ocean science. It was also a time when the electronics and all of what we were doing at sea was advancing.

Alex Griswold: How did these new technologies advance your research?

Jim McCarthy: When I first went to sea, I was actually for a two-month period on a Coast Guard ship. It was one of the first expeditions they had done with electronic sensors for temperature and conductivity, which we used to approximate the salt content of water. But they weren't, those instruments weren't fully trusted. That is, at the same time, you were making all the measurements in the traditional ways as well, with mercury thermometers, and with bottles to capture seawater, and on board analyzing the salt content of the water samples.

But as those techniques proved themselves to be increasingly reliable, they became less expensive. They became more compact. It enabled us to see that we could not only replace some of the traditional methods, but could now for the first time get sort of continuous data. That is to say, rather than sampling at a couple of dozen depths over a water column, you could now get a continuous trace.

The notion that you could complement that with sensors in the sky looking down, was a very exciting idea. One of the areas where I had seen, and some of this developed sort of first-hand, is related to the sensing of plankton concentration by looking at the most common plant pigment, chlorophyll. All land plants contain chlorophyll. All photosynthesizing microorganisms in the sea, the so-called phytoplankton, some of which are actually bacteria, some of which are eukaryotes. All have the same chlorophyll-A molecule.

By being able to detect the color of the water, one can make an estimate of the amount of chlorophyll. It's not as simple as it sounds, because when you look down from above at a water sample, you're seeing a very small amount of light that came from the Sun, penetrated into the water, and has now been back-scattered through the water surface, back to the atmosphere, and that's what your eye is seeing. Moreover, when you are sensing this from an airplane or from spacecraft, you have all the interference of what's in the atmosphere. Everything from water vapor, to aerosols that come off the land and coastal areas.

The sophistication that's required I don't want to underestimate at all today, to get a really good number out of that, is something that didn't come easily. But those initial efforts, to see whether this is possible, were really very exciting, because initially it was thought if you could see the signal, it would only be in coastal areas. You could not likely see anything in the open ocean. That is, it was just too hard to resolve variability of the open ocean, when in fact it turned out to be almost the other way around. That is, with the open ocean, the signal was much smaller, but it was actually cleaner.

And so there was this heady period when, as we came into the late '70s and were beginning to think more and more about designing some large studies, we had some new techniques that would allow you to–much more efficiently–collect data over large areas and over longer periods of time. And some surprising things came out of many of these early studies. That is, we had no idea, there really were no data, to allow us to know much about the blooms, that is the periods of intense activity of plankton far from shore. We knew about them near shore, because certainly if there was a marine station nearby, or people who worked in that body of water we knew it was happening in the Gulf of Maine, and maybe the Gulf of Mexico, and the North Sea. But far from shore, we didn't have any idea.

And yet, back to this message earlier about material sinking into the deep sea. It had been observed that there were pulses of material arriving at the deep sea floor that seemed to be timed with activities in the plankton above. So, the spring of the year, when you expect, in the Northern Hemisphere, the water circulation and nutrient availability and sunlight to be conducive to a faster growth of plankton: a few months later you could see the signal in the deep sea.

This kind of tantalizing clue that there were some signals that we hadn't been able to really detect, and now maybe could, and could actually quantify them and begin to think about how you could connect the observations made by ships. Although ships historically covered the ocean, but very, very sparsely. They move slowly. When you're moving, at the most you may be 10, 12 knots, and you stop a lot to collect samples. In the meantime, weather is moving by you, the ocean is changing, and you're trying to catch up. That is, you then come back from spending three weeks at sea and look at the satellite imagery and see how much that area changed over that period of time, it gave you a real sense of how blind you had been initially, and how much more useful your studies would be if you had that context.

Then, significantly, in the work we began in the late '70s and early '80s in the Gulf Stream, where you could actually, in advance of your study, pick out features of the circulation in the Gulf Stream. We spent a couple years studying the eddies that spin off of the Gulf Stream. You could actually see where they were. You could navigate right to them. You could sample the edges of them. You could sample the middle. You could come back two months later and sample the same eddy, its edges and its middle, and know how much it had changed.

So, it made everything that we'd historically done at sea far more efficient. Not only could you know just where you're going relative to ocean features, you could also see what was happening in cases like this between your observations, in a general way. How they were moving or changing. Then, of course, the advent of buoys, which enabled you to measure over time, when you weren't there, what was happening with some of the features.

We began conceptualizing some of these large studies, and I was very involved in one which we called a Joint Global Ocean Flux study, to look at how productivity on the surface was being processed. The plankton production being consumed by zooplankton. Fish, and what happens to that material. How much of it goes into the deep ocean. How much of it is used at mid water. How much of it gets respired in the surface, goes right back, in terms of carbon, as $CO2$ in the atmosphere.

This was, again, a very exciting period where it brought ocean scientists together from many nations. We had cooperative studies where we had ships from multiple nations. Coordinating their schedule, in a way which it would cover different parts of the area, different parts of the region we were working in, where they would maintain a continuous observation of a particular location over time. One ship would come and spend a few weeks, leave, go on somewhere else. Another one would come, maybe spend a week or two at that same location. It would give us a time series during the period of the year when there was a lot of biological activity.

And as a result of that, we saw changes occur, not only in the growth of plankton and its fate, but also in the water column chemistry. The draw-down of $CO2$ in the upper ocean, because of all the photosynthetic activity: far greater than had been hypothesized. That is, the rate at which it's resupplied by the atmosphere is much slower than it was taken up by the organisms.

We were able to then begin to make more sense of a lot of the global patterns that exist in the exchange of these constituents, like carbon dioxide or oxygen, between the upper ocean and the water column. Again, given that in a gaseous form they're going to be heavily influenced by temperature, we also had the biological processes that are consuming or producing them.

This was a period where we saw–not only considerable advances in the techniques–the arguments for the underlying science which led to making those techniques operational, or put into practice, in a way that now provide a sort of a continuous observing capability of these features of the surface of the Earth.

It was a synergy between the development of the technique, which was making certain science possible, but then a realization that a next generation of those techniques, deployed in the right way, would give us an observing capability that would allow us to know what was happening in different parts of the ocean without going there to sample it directly.

PART 2 (1982-1995) – Remote Sensing, the International Geosphere Biosphere Program, and Signs of Climate Change

Alex Griswold: Where were you when remote sensing began to be integrated with climate models?

Jim McCarthy: My first position after my PhD, I was at Johns Hopkins University, working on plankton and the nutrient cycles in the Chesapeake Bay, and one of the people I was attracted to right away, we had a long series of interesting conversations, was Francis Bretherton, who had arrived a few years earlier from Cambridge.

And Francis was a geophysical fluid dynamicist, with a strong background in applied mathematics, and he was keen to explore how some of the work I was doing could be more effectively modeled, and we had a lot of interesting back and forth as to what would or would not be an adequate way to express mathematically some of these interactions between organisms and their environment.

And Francis left Johns Hopkins the same year I did. He went to Boulder to head the National Center for Atmospheric Research, and I went to Massachusetts for a faculty position at Harvard.

And some years later, in the mid '80s, Francis was asked to chair a committee that had financial support from NASA and from NOAA to look at how we could make better use of the next generation of Earth-sensing satellites. We had seen just a few years earlier the proof of concept of many of these measuring systems for wind stress in the surface ocean, for sea surface temperature, for sea surface height, ocean color, and it was clear that, if we could make a strong case to move beyond just the demonstration products, that it might be possible to envision a whole new way of doing some of the science that we had been doing, historically from ships and occasionally from buoys.

Francis formed a committee called the Earth Systems Science Committee to explore how some of these new remote sensing capabilities could help us to develop a more robust research program, to understand how the Earth's system was changing.

And we'd seen from proof of concept missions, the potential of these systems to sense the temperature of the ocean's surface, the wind stress of the surface of the ocean, sea surface height, ocean color, sea surface temperature.

And the committee that he asked me to join had a very, very interesting task. Part of it was to say what could be done to advance the science in the way of these technologies, but also how could we more effectively model these systems so that we would understand how one part of it was affecting another. And if one part is affected by a particular change, are there feedbacks that would need to be considered?

The challenge was really quite huge because we had no idea whether the federal government would be inclined to support, in a research mode, some of these satellite missions, because all we had actually seen were proof-of-concept demonstrations. We had some pretty pictures. You could see how some of this could be useful. But could it be deployed in a form that was useful and near real-time so you wouldn't have to wait years to see the product? Could it actually be useful in real-time in a way that would allow you to design experiments while you were at sea to take advantage of what you could see from space?

And bearing in mind that we had, at that point, very little in the way of sort of global syntheses of any of this information. Not only were these early missions proof of concept, they were also not funded to produce a data product that scientists could really access.

For example, the ocean color satellite that was launched in 1978, the Coastal Zone Color Scanner. There were many other sensors on that satellite, and it had a limited power capability, so they couldn't run all the sensors all the time. And it had a limited data storage, so on each orbit, it would have to drop data, and so there was competition to say, “Well, gee, we'd really like to see images of this part of the ocean,” but there were other sensors that had other needs. And, there was a back and forth and a constant reevaluation of which was higher priority on which pass. So it took a long time over many, many satellite passes to get an image for anything other than a very small part of the ocean.

So that was never going to help us with a research project. What we needed was something more like continuous coverage. And with very little delay in being able to put the product into the hands of the researchers. Particularly if you're at sea and want to make use of it.

So, in addition to identifying what was needed, we then looked at what was available and all the meteorological satellites as well, and which were in need of enhancement–which were, as Francis used to say, "In good hands”–and then went forward with this recommendation that we hoped NASA and with interest in a part of NOAA as well, and interest in a part of NSF, take seriously and help us to direct a major new science initiative that would have these efforts embedded.

The work we did in the Bretherton Committee with this Earth Systems Science Model was really the architecture that enabled us to home in on the key questions. Where were the big uncertainties? For example, the ocean will absorb carbon dioxide. We know it's a function of temperature, but we know also that the biology in the ocean will use some of that carbon dioxide to make organic material through photosynthesis, some of that goes into the deep ocean. So, to what degree do those processes in the ocean affect how much carbon dioxide can be assimilated by the ocean from the atmosphere? So, what is the fate of that carbon once it gets into the ocean?

Similarly, there were big questions in land processes. So, the gases that are generated by biological systems in wetlands–for example, methane, we know are released to the atmosphere. So how do atmospheric conditions contribute to the evolution of methane from those biological systems? And when the methane accumulates in the atmosphere, of course it warms the atmosphere, which then, in turn, affects the biological systems.

One of the interesting things that puzzled us, throughout much of this, was that these feedbacks are far more likely to be positive than they are negative. That is to say, you push a button and say the Earth will warm. If Earth warms, then you melt snow and melt ice, which makes the surface less reflective, so more of the incoming energy from the Sun will be absorbed rather than reflected back to space, so it will warm more, and this process continues. How do you reverse that?

So the conceptual model that came from the Bretherton analysis was really important in the melding of these big international science programs, where we had the physical part of it underway with the World Climate Research program and the companion part with the biogeochemical cycles and ecology developed under the International Geosphere-Biosphere Program. And it seemed in almost every meeting, at some point, as we were conceptualizing how this program could make the best use of the limited resources which we had for the science–hoping they would grow of course, but knowing you would always have to make decisions as to which were the most important questions–that the linkages that were conceptualized in that Bretherton diagram that, for many of us, defined what we called Earth System Science at that time. And a term that is still used today, but it's not as widely used as I had actually imagined it might be by this time. This was still a really, really important, foundational piece of work. We worked hard and kept those discussions going and it led to a very productive exchange.

Alex Griswold: What new funding sources became available to fuel the growth of this collaboration??

Jim McCarthy: It was fortunate that during the 1980s we had one of those periods happen every now and then when a suggestion is made that maybe we should increase funding for science. And a committee was formed, charged to suggest whether science funding was at appropriate level, and if it were to be increased, sort of by how much, and to do what sort of things?

This was during the Reagan administration. This committee was co-chaired by David Packard, who of course was well known at the time: Hewlett-Packard. The co-chair was Allan Bromley, who was Professor of Physics at Yale, and went on to become the science advisor for President George Herbert Walker Bush.

They produced the report that recommended that science funding, and particularly the National Science Foundation resources, be augmented. One of the arguments was to do some of these large studies that could not be funded in the budgets that were available at that time without severely curtailing some of the more traditional work of individual small investigators, small grants with individual investigators.

This timing was perfect actually, because the work that these groups I've been referring to doing was kind of on the table, it was a great example of how additional resources could be used efficiently. It was consistent with this notion that we could also benefit with international partners sharing in some of this work, because the groundwork's been laid by Roger and others for a new international program.

This new international program, which was to be the follow-on of the International Geophysical Year, was called International Geosphere-Biosphere Program. This, under the auspices of the International Council for Science, based in Paris, this program got off the ground. I was asked to be the first chair of the Steering Committee that was to formulate the priorities for this program. We had a committee of 18 people from 15 nations, including two from the Soviet Union, one from the German Democratic Republic, one from the People's Republic of China. We had quite a mixed group, all very distinguished scientists.

After a few early meetings, we readily came to an agreement that there really were some high priority areas that we should pursue. That led to the design of this program and its priorities, some of which I've already mentioned. The pushback also came from scientists who thought this was perhaps overly ambitious and maybe likely to overextend the community, or promise understanding that was beyond our reach. That was not terribly surprising.

One example was in the journal that I was asked to help start up during that same period, by Chuck Drake, who was then the President of the American Geophysical Union. The notion was that we needed a journal that recognized the emergence of more sort of biological contributions to the field of geochemistry. The title of the journal we came up with was Global Biogeochemical Cycles. I was asked to be the founding editor of this.

There were people who thought that “global” was just too ambitious. We couldn't possibly go with something as far-reaching as that. On the other hand, “global” is where we wanted to go, that's where we wanted to be. That was the objective. There were people who thought that the word “cycles” was too restrictive. After all, it's the cycles that propel these systems in their global importance.

It started off, and I carefully picked an editorial board that would, with people like Roger Revelle, who would help, and Francis Bretherton, who help advocate for both the involvement of multiple processes, hence the “cycles,” but also the global reach. I think after my term expired, Karl Turikian from Yale became the editor, and he took it to another phase. It is today a very, very successful journal, and no one would hesitate for a moment to use all those words in a sentence, but it seemed rather audacious and perhaps unwarranted in the mid-1980s.

I'll make just one final comment. It was important that the scientists who were involved in this planning effort for the International Geosphere Biosphere Program were all sort of well-established in their field. Many of them were still very involved in research as individual investigators, working on some small piece of these larger pictures. And there was a concern that any effort of this sort might be unnecessarily driven, or its agenda might be unnecessarily determined, by people who were no longer really involved in the science themselves but had ideas about where the science should go. I think we were able to recruit people who were themselves, individually, very involved in pursuing research, and that helped give it standing as well.

Alex Griswold: How much were the studies that were part of the International Geosphere Biosphere Program linked to climate change?

Jim McCarthy: In the early '80s when people began talking about something that ultimately become the International Geosphere Biosphere Program, there was a substantial effort underway in the physical climate community to go to the next level of climate models. And another thing that was happening in addition to our understanding of some of the linkages between the chemistry and the biology and the physics of the system was that the models were advancing pretty quickly. This came largely with the development of computing power that allowed scientists to run more complicated complex models and see results more quickly.

There were some pioneers at that time in the climate modeling community, like Suki Manabe and Kirk Bryan and Warren Washington, who were beginning to think about how the Earth's climate might function differently with additional greenhouse gases and began looking at what a doubling of pre-industrial CO2 would do to our climate. The physical oceanographic community was also moving ahead with some new ideas. This had come in part with the realization that the El Niño phenomenon was an ocean-wide phenomenon, the El Niño Southern-Oscillation as we came to call it, and the realization that one could be more adept in anticipating when the next El Niño would come if we had some observing capability out in the middle of the ocean.

At that time there was very little information coming from the Pacific Ocean on a routine basis that would allow people on the west coast of the Americas to know what might be coming their way in the form of weather. There were a few weather ships and a few buoys, but not many. And so there was a notion that if you put a buoyed array along the equatorial Pacific, we could begin to see the emergence of an El Niño pattern with enough foresight to make forecast.

This array that became known as TOGA, it was Tropical Ocean Global Atmosphere study that really assisted enormously in understanding what was happening in the equatorial Pacific and how those patterns would unfold over time to give us something like an El Niño or a La Niña. The idea that in addition to the traditional coverage of ships, now if you have buoys and these were not drifting buoys, they were buoys that made observations over time at one location.

This provided, again, more information that people began to incorporate into models. At that time, the ocean was a fairly simple part of the general circulation models used to model climate; and the opportunity to develop a more sophisticated ocean model, one that really, for example, interacted with the atmosphere, emerged as computing power enabled more sophisticated models to be run in a timely manner.

The World Climate Research Program, which was created by the World Meteorological Organization, which is the body that collects and compiles and distributes the global weather datasets, was a major contributor to the sort of international scientific effort to better understand how the climate was working and how it might be influenced by changes in, for example, a greenhouse gas concentrations of the atmosphere. One part of the World Climate Research Program was the World Oceans Circulation Experiment, known as WOCE. A major architect of that program was Carl Wunsch, who was at MIT at the time. And this was a plan to fill in those voids that one could see on any map of the world's oceans where we had very little data.

A plan was laid to sample in a systematic way all the ocean basins. This was an international effort. None of these efforts that I'm referring to could be done without the facilities, that is the ships, in some cases the aircrafts, the people. And not just numbers but in terms of ideas and experience in different parts of the world's ocean coming together and working on a sensible allocation of effort to accomplish a goal like this.

So, against that backdrop we had the opportunity to develop this program that would look at the chemical and biological aspects of the ocean and how they might be changing, and the study was designed in a way to take advantage of these physical measurements as well.

For example, part of the study to understand the carbon cycle in the ocean included sampling the complete carbon chemistry, the carbonate, bicarbonate, CO2, the pH. And this became a really important part of the overall dataset for the global oceans. What was missing, of course, when you think about the biological processes and how they interact with the chemistry, was seasonality. That is to say, if you had one section of a particular part of the ocean, it was deployed, and the samples were taken irrespective of season where it might be more or less active. But, of course, we know that seasonality is largely in the upper ocean, not the deep ocean. So, the overlay, then, of seasonal work in areas that we knew to be particularly active became an important objective of the International Geosphere Biosphere Program.

The other really essential part of all this was that the interaction with, and cooperation with, the funding agencies that were wishing to take advantage of this sense of opportunity and enthusiasm in the scientific community and the sense that one of the reasons we needed additional funds, was to look at some of these big questions that could just not be addressed with the regular funding levels.

So, there's a partnership that formed, and I think of three people being really instrumental in that. It involved the National Science Foundation. At that time the, person who was head of Geosciences was Bob Corell. NASA: at that time the person responsible for this branch of NASA science was Shelby Tilford. And NOAA: The National Oceanic Atmospheric Administration, and [J.] Michael Hall was the person overseeing this part of NOAA.

The three of them not only worked together as a very effective team, but they worked very effectively with the science community and this iterative process of what needed to be done, what could be done, how might the resources be deployed, how could they be staged in a way that, not only enabled the science, but was feasible with the funding resources?

And in addition to the shipboard work, of course, and the buoys, there was that really important component of the satellite observations. This was a more difficult aspect of the staging process because not only did it require the development and the proof of concept of these various missions, but going beyond that, to make them operational meant that you needed substantial resources to process all the data and distribute and it make available. And again, the realization being, that we needed international partners. And it wouldn't have been possible without the attentive leaders in the science funding agencies.

The other big player in ocean science, at that time, was the Office of Naval Research. They had historically provided most of the ships that the academic community used for its ocean research and were very involved in supporting a lot of the physical oceanography and a lot of the bio-optical work. Another player, historically, was the Department of Energy and its precursor, the Atomic Energy Commission.

When I was a graduate student, a lot of the plankton research in the United States was supported by the Atomic Energy Commission. Initially, they were kind of interested in knowing what might be happening in certain coastal areas with nuclear processing facilities. Certainly, a big interest in what was happening off the mouth of the Columbia River with what might be coming out of the Hanford facility. And we learned some things through that.

For example, people at Oregon State looking at marine sediments, came to realize that the fecal material from tiny zooplankton–these are organisms that might be the size of a sesame seed or maybe a small grain of rice–were carrying radionuclides that came into the surface water with the effluent of the Columbia into the deep ocean, much more rapidly than anyone anticipated. That, within days of the production of one of these fecal pellets in the surface ocean, they were making it into the deep ocean. The radionuclides gave us a clue as to how fast that process that Alexander Agassiz referred to as the "rain of detritus," was getting nutrition from the upper ocean to the deep ocean.

Alex Griswold: Was there a moment where you realized that "I can't just focus on biological oceanography without looking at all the bigger implications and I have to get this word out?"

Jim McCarthy: When I began graduate school, there was no sense that we'd be facing problems like we are today with the environment. Certainly, this was in the mid-sixties, we were concerned about a lot of what we could see locally in the form of pollution. Everything from degraded rivers to litter. Certainly, some cities had very bad air quality. Some had had bad air quality for decades or more.

And, again, these were mostly thought to be local problems, that they could be addressed at the local level and didn't require anything more than some resolve and regulations and some enforcement to clean up air quality.

The sense that these were not really local problems I suppose for me was first stirred by discussions that didn't really happen until I after I left my graduate program and was here at Harvard. And it was with Roger Revelle. And the realization that a lot of these systems that we had been assuming to be rather stable in the ocean were either beginning to change or likely to change. The notion that the chemistry of sea water would be changing as it absorbed carbon dioxide from the atmosphere, and the realization that it could become a serious global problem, probably best first articulated in my generation by Roger Revelle.

And then many people in the geochemistry community began to focus their attention on this problem. One of them who I note, sadly, in the recent news has just died, Wally Broecker. Wally was great company, often outrageous, prone to yell, particularly at people who weren't thinking clearly. I was often in sessions with Wally where he would get agitated and pound his fists on the table, that we were either not fully appreciating the points he was making or off on some tangent that he thought was unimportant. Wally is often mentioned as the person to first popularize the term "global warming."

He believed that this is something that was happening and would become increasingly clear, and this was back in the mid to late 1970s. He's also probably best known beyond the scientific community as the person who articulated a very clear image of how the circulation worked in the north Atlantic, and he called this the “Conveyor Belt.” And that imagery is something that allowed people to really understand how that worked.

So Roger Revelle became really the driving figure in this international program that was to be a follow-on of the International Geophysical year, which turned out to be more than a year, but over a period of a few years in the late 1950s, the follow-on program he envisioned would be broadened to include the biogeochemical processes, not simply geophysical processes.

And it was in that period when many of us began to see more and more signs that these systems were changing in ways that were directly attributable to human influence. However, you know, there continued to be uncertainty about natural cycles as well. Wally Broecker was extremely effective in arguing that what we were seeing was beyond the range of these natural cycles.

It was probably in the early 80s, '82 and '83 when we were beginning to sense that we had drifted beyond what could have been natural processes of cooling and warming over the last century or so. And I distinctly recall in 1986, which was the year that I took a leave from my academic duties to be at NCAR, the year that we launched the Global Biogeochemical Cycle journal, and it was Ralph, Ralph Ciccerone, who walked into my office one day and said, "Jim." He said, "I think the data now clearly show that the ocean is also warming." And until that time, there were bits and pieces of information, one reason or another, but to say overall the oceans are warming ... it was at about that point. And the pieces then fell together rather quickly. I would say, you know, by the time of the First IPCC report, you still couldn't say definitively that this warming had the fingerprint of humankind on it.

But within five years, by 1995, you could say that definitively. So, the people who were visionary in this area, like Wally Broecker and Roger Revelle and Jim Hanson, they just knew their hunches were right from having looked carefully at the system. Or in Jim Hanson's case, having modeled the system, to be able to say, “This is what I think should be happening and that's what we're seeing.” It was a handful of people who really led the entire field into this new era of widespread recognition that we–we all–are changing this Earth system in a very fundamental way.

PART 3 (1990-2004) – Climate Change Impacts: The IPCC, Arctic Council Report, and Union of Concerned Scientists

Alex Griswold: Let's go back again to when the alarm bells first start ringing about climate change. How did you get involved with the IPCC?

Jim McCarthy: The International Geosphere Biosphere Program and the World Ocean Circulation Experiment were proceeding with their field campaigns and modeling studies and there were increasing concerns within this community that the rate at which carbon dioxide in the atmosphere was rising was going to lead to some maybe unanticipated or unwelcome changes in climate at some point in the future.

Again, this isn't a new story. This had been forecast. We like to refer to Arrhenius, in the late 1800s, having postulated that changes in atmospheric carbon dioxide could have contributed to glacial cycles, and he speculated that if we continued burning coal at the rate we were, ultimately, we'd warm the planet. And he actually came up with a number for how much warming the planet would experience with a doubling of carbon dioxide, and it actually was surprisingly similar to what you would calculate today. Where he was wrong was, he thought it would maybe take a millennium for this to happen. Instead, we're doing it within a century. Of course, he couldn't have anticipated how quickly society would exploit not only coal, but oil, and develop a fossil fuel-intensive world that we have today.

But, as people were beginning to see not only where the Keeling Curve was headed, but noticed that there were some curious anomalies in it as well. That seasonal rise and fall of carbon dioxide, although the general trend of it is going upward, the amplitude was also going up. And we knew that that implied that there were some underlying changes in, probably, the terrestrial ecosystem.

The sense that we needed to begin to think more critically about what the implications of this might be, begin to stir in the mid-to-late '80s. And it was, of course, in the mid to late '80s that James Hansen, at NASA's Goddard Institute for Space Studies, began to suggest that not only was this a problem for the future, but it was the near future. We were now beginning to see the effects of this on our climate.

The World Meteorological Society and the United Nations Environment Program decided that they should convene an assessment of what was known about climate change and what some of the implications of climate change might be, what some of the impacts might be, on society and natural systems.

They formed something that came to be called the Intergovernmental Panel on Climate Change. And "Intergovernmental" was a very carefully chosen word. It wasn't "international," it was "intergovernmental." It was meant to represent the governments. The governments would be asked to pick authors for this study and the governments would have an opportunity to review the report before it was released, so the intention was that this not be something that the scientific community created and owned, but something that the governments created and owned. So, it had some very important ground rules. One was that they would convene a group of scientists to produce a report that would then be viewed by representatives of the government, but there would be no voting on it. It would be a consensus statement. In other words, you continued the discussion until everyone agreed that you could proceed with that part of the report.

So, as that group was being formulated, Gordon McBean, who was a Canadian climate scientist, was head of the World Climate Research Program, and I was head of the International Geosphere Biosphere Program. We were asked to write the final chapter in this report that would identify areas of uncertainty in our current understanding, which if addressed with the research efforts that we were involved in directing, we would at a future time be able to say more clearly what changes were occurring and also what impacts might unfold from those.

At that point, I don't think anyone gave much thought to a second Intergovernmental Panel on Climate Change, IPCC report. We saw this objective to produce a report and we also knew that the timing of it was very important. The reason being that the Rio Summit, scheduled for 1992, would take into consideration the report that we were asked to produce, and it could likely contribute to some sort of international agreement about climate. The report was produced. The Rio Summit happened.

From the Summit came the UN Framework on Climate Change, an agreement that was ultimately signed and ratified by virtually all nations. The United States was one of the first to sign it. Actually, President Bush was at the Rio Summit. It was at that point the largest gathering of heads of states ever assembled. He signed it. That was in June 1992, and it was ratified a few months later by the U.S. Senate.

Alex Griswold: In 1992, when the Río Agreement was signed, was this seen as a “mission accomplished” moment by the climate community?

Jim McCarthy: So to see within a few years, from 1989 to 1992, an initial discussion result in an agreement–the UN Framework Convention on Climate Change–that said that we would reduce emissions to 1990 levels. It didn't say when, but that was our goal, and the agreement said we would avoid dangerous interference in the climate system without actually ever defining what “dangerous” meant. So it gave us in the science community a real sense of optimism that there was now a serious commitment to use this science in a way that would lead to national policies that would begin to steer us away from what we had seen with the first IPCC report–as some very troubling likely future impacts if we didn't take action soon.

At that time, you couldn't say, definitively, that sea level was rising at a rate that was accelerating. We knew it was rising, but it could be a steady rise. We couldn't see any evidence of unusual frequency or intensity of extreme events. There weren't many strong signals of impacts in terrestrial or marine ecosystems. And you also couldn't say, with great confidence, that the changes that we were seeing were mostly due to human systems. They could have been mostly due to natural cycles of some sort.

Five years later, in 1995, the second IPCC report: enough had changed to begin to say with more confidence that there were now impacts that you could see. In part, one might argue that the results of the first report had stimulated research to look more carefully in areas where maybe we hadn't looked before, but also the modeling of climate, and the interaction between greenhouse gases and the physical climate system, had improved to where it could now be said that you could discernibly note the contribution of humans to this change.

That is, certainly there are natural cycles that we're all familiar with: Solar variability, volcanoes, these internal rhythms like El Niño-Southern Oscillation. And we know now, that during years when an El Niño manifests itself, on average, the global average surface temperature is warmer a year before or a year after. If you look over time at the warmest years in a stretch of time, it’s often an El Niño followed by a cooler year. That is to say, El Niños pop out as sort of hot years. In an increasing trend of warmer years, the El Niños will be the warmest years.

Then by the Third IPCC Report in 2000-2001, a lot of this had become much more clear. One could now see impacts on all continents and over many ocean areas. Sea level rise was accelerating. Extremely wet and extremely dry periods were beginning to show up with different frequencies in many locations. And the confidence that humans were a major contributor to this was one of the most contested statements in this sort of final review phase of the reports, which involved the government delegations.

In 2001, when the statement about the contribution of humans to this change was being discussed, the choice of words is sort of interesting because, does it have a literal translation into another language? Does it mean exactly the same thing? You can take a word like "few," the literal translation of "few" in one language might not mean "two or three," it might mean maybe "up to 10." Well, that makes a big difference. So, in this particular statement, that was roughly as follows, "Most of the warming in the last 50 years is due to human activity." What does "most" mean? Well, it means more than 50%. But how much more? A lot of scientists at that point thought probably closer to 90%, but "most" was the word we settled on. It has a vast range, but it was the first time that there was an agreement that humans were not just a contributor, but we were the major contributor.

So that Third IPCC report, which was the report where I headed Working Group II, which looked at Impacts, Adaptation, Vulnerabilities, we documented impacts on all continents and many ocean areas, and impacts ranged from changes in the distribution of plants and animals, seeing that their range was now reflecting change in temperature. Again, these are exhaustive surveys of the literature which required a long enough period of time to really see a trend.

This process, the IPCC process, which is often I think misrepresented. It's often misrepresented by those who don't like the reports as something put together by governments. Indeed, we couldn't do it without governments, but the scientists who are the authors of the report have free reign to write the report that they think is truest to the science. And in the final review process, there's no change made to those documents by any of the government delegations that is inconsistent with the science. The changes are basically editorial in nature. They are designed to make the communication clearer, and not just in the language which, by default, ends up being English, but that when translated into other languages it's equally clear as to what the meaning is.

Alex Griswold: How were you recruited for the Third Assessment Report?

Jim McCarthy: I was involved in IPCC First Assessment, which we didn't think to call the "First Assessment" at that time, not knowing that there would ever be a second. And then I was a reviewer for chapters on the Second Assessment. As a Third Assessment was being formulated, I was asked if I would be willing to be the head of that working group. I say head, but in practice all of these working groups have co-chairs. There's the chair from a developing country and a chair from a developed country. The chair from the developed country is responsible for basically hosting the report. That is, putting together a staff that will not only handle all of the process of producing the report, arranging the meetings, convening the various sub-groups, developing the actual product, producing the report, arranging the travel for all of the members of the working group.

I was asked initially if I would consider being nominated for the head of this working group by Senator Tim Wirth. Tim Wirth had been the Senator from Colorado, earlier, he'd been a representative from Colorado, who decided not to run for reelection, then moved to the White House as the Undersecretary for Global Affairs in the Department of State.

I knew, quite well, the former Working Group II chair who was Bob Watson, who had run Working Group II for the Second Assessment. I talked with him at great length, and particularly since Bob was being nominated to be the overall chair of IPCC and I knew the staff support for this, which was embedded within the U.S. Global Change Research Program– an office that had been funded by those agencies that had helped to develop the programs in the 1980s–I knew a number of those individuals, and talking with them, knew that they not only could do the work that had to be done, but they were willing to continue in those roles if I became head of the Working Group.

I was then nominated by the U.S. and met with and talked with the members of the bureau which oversees the Intergovernmental Panel on Climate Change, and at that time, the chair of IPCC was Bert Bolin, whom I had known for many years, a Swedish scientist. Met with him and at some point, they decided to ask me to do this and I agreed.

My co-chair for Working Group II in the IPCC was Osvaldo Canziani from Argentina and we hit it off and had a great working relationship. One of our initial tasks was to invite authors. We had already put together a group who could help us scope an organization for the report. How do we want to parcel this overall task into reasonable units? What areas do we want to identify for impacts? Having done that, we then had to select people who we would see as the lead authors who would organize the author teams for these various chapters. We had nominations from a hundred or more nations, but I had a thousand nominations, and these were full CVs. It was a stack of paper like this. This is before any of these things were easily moved around electronically.

I went through every one of those and came up with a list of about 80 who I felt we really had to have. They were leaders in their fields. They would be the perfect person to head a chapter on terrestrial ecosystems or human health or a chapter looking at the impacts in Asia. And, of course, Osvaldo with his experience and being well-connected with a lot of developing countries was a major contributor in these discussions. And then I contacted that group of 80 or so, knowing that ultimately we'd need roughly twice that many authors, and fortunately, every single one of those people that I asked agreed to serve. This made the job a lot easier. Then they helped in the selection of the additional authors, some of whom were in that list of nominations, others who weren't, but we went through the process of, again, clearing with the governments that it would be fine if we asked scientist X from country Y, that country Y was fine with our inviting that scientist to join. And that set it in motion. Then we gave all these chapter heads sort of a schedule and, of course, then we brought together at regular intervals all the lead authors to exchange ideas, see where there was complementarity, overlap, gaps, as we iterated towards a final report. We generate the first draft. Send it out to all the governments for review. Get all their comments. Generate a second draft. By that time, we've also generated this second and third product which was a technical summary which is over a hundred pages then a briefer called the "Summary for Policymakers," which is something closer to 15 or 20 pages.

Among the major conclusions, we not only saw impacts everywhere we looked, we could see that many of these changes now and extreme events were beginning to show the trajectory that you would expect. Some areas were getting wetter, some were getting drier, some were getting wetter and drier. It was evidence that certain types of storms were increasing in intensity. There was a growing realization, and I think our report was one of the first to really document this, that the people who were going to be most affected by these changes were people who were already–you could identify them today–in a vulnerable situation. You could see that easily in the case of sea level where people living close to the coast would be more impacted, but the poor people living close to the coast are going to be even more impacted.

I can't tell you how many times at these various sessions a head of a delegation from a particular country would come up and say, “So tell me, who are going to be the winners and who are going to be the losers?” I said, “No. There are no winners and there are no losers.” In every country, there will be people who are more vulnerable than others. And you need only look at a case like the United States and see what happens when we have a Hurricane Katrina or a Hurricane María or a massive storm like Harvey or Sandy. That the people who are today having a difficult time making ends meet are the ones who will be most impacted by these changes.

Alex Griswold: How did you become involved with the Arctic Council's Climate Impact Assessment from 2004? You were are lead author, right?

Jim McCarthy: After I finished with the IPCC, and I did think I was finished. That is to say I wasn't eager to re-up and go through another round of that. It was someone else's turn. I became drawn to the work that had just been launched by the Arctic Council to look at impacts in the Arctic. One of the reasons it appealed to me was, I saw how difficult it was to generalize about impacts in areas we had considered for the Intergovernmental Panel on Climate Change. That is to say, how do you talk about impacts across all of Asia? It's very, very difficult and you can't easily generalize about even the tropics. But one area that you could generalize to a greater degree would be the Arctic. It has a similar climate. That is to say, unlike the tropics which have ocean and land and ocean and land, the Arctic is land that is inhabited that surrounds an ocean, the atmospheric circulation is largely a gyre sitting over the Arctic.

We know that climate is changing in the Arctic at a rate that is nominally twice that of the average for the rest of the planet and there are a number of reasons for that. High among them of course, the reflectivity of snow and ice and adding to that, the accumulation of small dark particles, whether it's soot or dust coming from human activities like the combustion of diesel, fires in the Boreal forest, and increasingly the combustion of very low-quality fuel, bunker oil, by ships in the Arctic. Methane. Methane from natural sources, but also from oil, gas extraction. Methane that is willfully or accidentally released into the atmosphere in those processes. But, also, it's a community of people, indigenous people in the Arctic who can trace their ancestry hundreds of years, in some cases in the same location. There's communities that, we know, have been in that location for generations and generations are now at risk because, historically, many of them were on that land edge because the ocean provided them with resources, fish, marine mammals, food, fuel, fiber, all from the ocean. So, it's a very precarious situation when you realize that what had been an ice-covered area in winter and adjacent frozen ground, is now ice-covered for a shorter period of time, and some of that frozen ground is thawing. And the people who have been residing and hunting and fishing and gathering vegetation for their livelihood for their entire life, and that of their ancestors back many generations, is now at risk.

So, this was a very, I think, important and, for me, a very interesting assessment. We attempted to not only understand how these changes were occurring, but how people would respond to them. We know that there are other changes underway–just under the general rubric of globalization and communications. These are communities now, 50 or 100 people, in some cases. At remote communities in Greenland you will find that the school has internet. People know what's going on in the rest of the world and are connected to it in that way. It's an area that's also under serious pressure today to develop for natural resources. As the ice recedes from the Arctic Ocean, there's increasing interest on the part of the oil and gas companies to explore and extract resources, which bring with them great risk, risk of mishap. It's a mess cleaning up a BP oil spill in the Gulf of Mexico, but it would be a far worse mess in the Chukchi Sea off the coast of Alaska. Not just the short-term livelihood, but maybe the long-term livelihood of communities, would be severely impacted. Maybe changed in such fundamental ways that they could no longer stay in that location by a serious spill.

So, I think the Arctic Climate Impact Assessment was a really important document in enabling us to see not only how the Arctic climate was changing, but how precious and precarious that region is. And I hope that everyone who is thinking about the opportunities for exploitation in the Arctic takes a serious look at that report and follow-on documents.

Alex Griswold: Was it around this time that you began to be involved with the Union of Concerned Scientists?

Jim McCarthy: The other assessments that drew me about the same time were the regional assessments that were being done by the Union of Concerned Scientists. The Union of Concerned Scientists had looked at what climate impacts might unfold in the region of the Southwest, in California, the Great Lakes region, and was thinking about doing something similar in the Northeast and I became involved in that assessment, led by Peter Frumhoff at the Union of Concerned Scientists. And it was a really important report. It enabled us to see how our changing climate in the Northeast would affect fisheries.

It was a report that didn't make the cod fishermen very happy because we forecast that cod would become scarcer and scarcer as these waters warmed. It forecast that lobsters would be doing better in warmer waters. Both those things have happened.

It forecast that the recreational industry, dependent upon snow, would have a harder time in the future. Ski areas could get by with more snow-making, but the snowmobile industry, which turns out, economically, is extremely important in much of the northeast states. But you can't make snow for snowmobile trails. It looked at human health. Looked at how the effects on forestry, forest products, would be affected. But importantly, I think it generated imagery that resonated with people. It enabled people to see how different their climate would be relative to other regions or conversely, how similar their climate would become compared to regions they knew today that were further south.

The realization that one could work hard to reduce emissions, and see a moderate change in climate, and if we didn't do anything, the change would have the climate in our state looking much more like one of a southern state today, and knowing quite clearly what that means. We were seeing, at that time, the evidence of increasing intensity of precipitation events in northeast states. This is a pattern that has continued.

I had previously been involved with UCS, but I became more interested in the work of the Union of Concerned Scientists. As you know, it was founded in 1969 by a group of faculty and students at MIT, who were concerned that the government was ignoring a lot of important societal problems when it allocated money for scientific research, and was overly focusing on military objectives and that scientific research needed to be balanced. We needed a more serious commitment to major societal problems.

That's been the mission of UCS, to argue that the contribution made by serious rigorous technical analysis of problems that we were facing every day in society was an important mission. And that we owed it to decision makers to provide the very best scientific and technical analyses to make the world safer and healthier for all people.

And the Union of Concerned Scientists also appealed to me because it does not accept any corporate money, and it also doesn't accept any money from governments. It allows the voice to be completely untainted by any association with any funding source. It's supported by individuals. Thankfully, a lot of very generous individuals, who appreciate the value of science, and by foundations that wish to see programs like this flourish.

It's a group of very dedicated staff, and a wonderful place for young people to have an opportunity to become involved with an organization that is sometimes called an "advocacy organization." I know that that term is occasionally used in a pejorative way, but what UCS focuses on is delivering the very best scientific and technical information in a way that decision makers, policy makers, at all levels, can be informed and know, as they're making their decisions or formulating their policy, that they've taken advantage of the best scientific and technical information. But it also, unfortunately, has to spend a lot of time on another objective and that is to call out and to refute erroneous or mistaken or, unfortunately, at times deliberately misleading information about what the science or technical analyses would reveal, and to do that in a very straightforward way, and to push back, not only against the sources of that information, but to call out the people who are only too happy to publicize it and make it appear as if it's good science when in fact it's not.

PART 4 – Taking on New Roles at Harvard: Director of MCZ, Housemaster

Alex Griswold: Do you want to start back when you first came to Harvard and how your horizons expanded once you got here?

Jim McCarthy: When I arrived at Harvard, I looked upon this as another sort of three-to-five-year job, which is the way I looked at Johns Hopkins when I landed there. Sue and I both thought we would at some point return to the West Coast. She was from the West Coast, I was from the West Coast, we were really comfortable there, we had lots of family and friends there and all of recreational activities had been focused on the outdoors in the West.

In any event, I immediately sensed a real difference in the community at Harvard than the one I had been in at Johns Hopkins. And there was an openness and receptivity and a sense of a freedom to explore, in ways that I hadn't really noticed there. In other words, people in other departments were interested in talking with you. You were made to feel welcome in a way that made departmental boundaries seem like they were quite porous here. And that's not true at all schools, and not just departmental boundaries, but even here the boundaries between the Faculty of Arts and Sciences and the professional schools. So, I found that really, not only welcoming, but intellectually exciting.

Harvard had, at that time, a Committee on Oceanography, which exists today, but serves a very different purpose. And at that time, it was a cross-departmental committee. In other words, the Committee on Oceanography could offer a half-appointment to match with a departmental half-appointment to make a position in oceanography. And that's the funding structure that created the position that I was hired to. And it was through my very first interactions with that Committee on Oceanography that I met Roger Revelle, who although he was at Harvard, he was not here in Earth Sciences, he was in the School of Public Health. But he, of course, was not any less interested in what was happening in the ocean and was a very important member of this Committee on Oceanography.

The other position that I found myself in, that I hadn't thought too much about before arriving, was the Museum of Comparative Zoology. Because the half-position that was matched with the Committee on Oceanography position, to create the position that I was hired to, came from the Museum. And I looked around and saw that most of my Museum colleagues were involved in collection research, and in some way, had activities that related to a particular group of organisms–and mine, of course, didn't. But thinking back to the origin of ocean science at Harvard, with Alexander Agassiz and his protégé, Henry Bryant Bigelow, who went on to be first director of Woods Hole, their interests didn't relate to any particular group of organisms.

So, this was, for me, kind of an enticing opportunity to think broadly about what I wanted to do. I also found that teaching here was far more interesting than I had imagined it would be. I thought interacting with students who, in a course like Biologic Oceanography, which was the first course I taught, were interested in possibly a career in this area.

But I also had students who were maybe third or fourth year, they'd taken science, they were maybe headed to business, maybe to law, maybe to med school, but they really wanted to learn something about the ocean. And I found that quite inspiring that there was an opportunity to interact, not just with students who might pursue a career in the ocean realm in some way, but no matter where they were headed, there was this interest in learning. And again, this was not a survey course, it required a background in chemistry and biology and math in order to do well at it.

One of my favorite memories–of teaching students in that sort of unusual category–was a senior who came to me once saying, "You know I ... it's my last opportunity to take this course. I'm going to law school. But I'm really concerned about preparing for the LSAT and everything else I need to do. Could I take it pass/fail?" And I had previously said I wouldn't allow students to take it pass/fail. And I relented and said, "Sure." But I cautioned her. I was really worried that if she thought she could sort of take it easy in the first part of the course, and make up later, this might not turn out so well for her.

So, when we had the first exam, and she had the highest score, I was a little surprised. It was too late for her to change her registration from pass/fail to a graded course, but we chatted about it, and she had the highest score in the final exam as well. I can tell you it was fun writing letters for her later. Here is a student who was clearly not in the course for the grade, but she was passionate about the material and excelled in every way I could test her accomplishments.

Alex Griswold: Can you talk about how you were appointed Director of Harvard's Museum of Comparative Zoology?

Jim McCarthy: I became interested in some of the problems that we were experiencing in the Museum in that era, and I suppose because of the involvement that I sort of created for myself, when it came time in 1982 to identify a new Director for the Museum, the Dean at the time, Henry Rosovsky, and the president, Derek Bok, approached me and asked me if I would do it. And we had a series of discussions, and after sorting out all of the major issues in their minds and mine, we all decided, the three of us decided, we'd give it a try.

I had been tenured two years earlier. I was very involved in my research at that time and teaching at the level that I had before. So, none of that changed, but I was taking on this additional duty, and we had some serious problems to face. We had serious financial problems at the Museum. We needed to figure out how to develop a more efficient administrative structure, and it was a time of kind of great opportunity–with the blessing of the Dean and the President–to be creative and figure out how to do that. And it was a time when we had just formed the new Department of Organismic and Evolutionary Biology. It had earlier been a wing of a general biology department, and we created a new administrative structure, that brought together bits and pieces of the Museum, the Arboretum, Herbaria, the Faculty of Arts and Sciences-supported portion of Organismic and Evolutionary Biology, and managed to find efficiencies. And realized that we could more effectively use the resources of the Museum, through its various endowment accounts, to support faculty efforts, research efforts, curation efforts.

So that was my first taste in administrative work. And in the mid-80s, I was asked by then Dean of the Faculty of Arts and Sciences, Mike Spence, if I would become one of his Associate Deans for Natural Sciences, a companion to really the position held by Paul Martin, who was the Dean at that time of the Division of Engineering and Applied Sciences. So, the two of us were the members of Mike Spence's cluster of Associate Deans, who were involved in, particularly, all the appointments relating to the sciences. Everything from the discussions on the Dean's Office to the ad-hoc processes in the President's Office.

And that was, I thought, a very rewarding opportunity: getting to know a lot of other colleagues in other departments in a way that I wouldn't have otherwise had the opportunity to do.

And that really, in some regards, led to a better sense of understanding of interactions across departments within the university, and some of the possibilities for scholarship and interaction, that otherwise might not have occurred to me. And building upon that, over my period at Harvard, I've developed really close friendships and working relationships with people in entirely different academic fields. And although we get together, there are aspects of our own research that we share and discuss with each other, but I think what we talk mostly about are the larger issues facing higher education today here at Harvard, and elsewhere, and how we can deal with them effectively.

Had it not been for that opportunity to meet a lot people from other areas of scholarship, through my experience in the Dean's office, I think in retrospect I would've missed something that's been very rewarding to me, personally and professionally, in my years since then at Harvard.

The period following that became interesting in some other ways for me. I found that my involvement in these large national and international science planning and implementation efforts–and again, this is an effort where the people who were planning these experiments were also the ones who were going to be actively involved in them in the field campaigns, and all the synthesis efforts following those campaigns–and those for me occupied a lot of my time between the mid-to-late '80s and through the 1990s.

Alex Griswold: Could you talk about the years when you and your wife, Sue, served as Co-Masters of Pforzheimer House, one of Harvard's undergraduate houses?

Jim McCarthy: It was in the mid-90s when Sue and I were approached and asked if we would be interested in serving as house masters. This is something that we had talked about, Sue and I had, many times. Realizing it was a very important position–they were positions that had a lot of responsibilities–and as we thought about it in the past we said, "Well, maybe at some time in our life, this might be the right thing to do," and when we were invited to be considered, I guess we looked at each other and said, "Well, there's probably never the right time, but let's take a close look."

And we did, we went through the interview process, and were then offered the opportunity to be House Masters at the recently-renamed North House, named Pforzheimer House for Carl and Carol Pforzheimer, who had been very important donors for Harvard and Radcliffe over a very long period of their lives. This was a new and exciting opportunity for us, to become further immersed in the undergraduate education, undergraduate experience, but also in the broader missions of the educational enterprise: that is, not only interacting with students but now, increasingly, faculty and the full measure of the breadth of the Harvard undergraduate experience, in that area.

People used to ask us if we enjoyed it and I said, "If you didn't enjoy it, you couldn't do it two days in a row," because it was a heck of a lot of work. And I guess at the end of the first year, we were trying to figure out the word that best described the experience, and the word was "More." It took more time, and it was more work, but it was a lot more fun than we thought it would be, and that kept us going every day. There wasn't a day which you didn't worry about some of the students in the house who were having difficulty, but there also wasn't a day that you didn't have a really interesting and inspiring discussion with a student about something. Maybe it was about a recent news item, maybe it was what they were doing, maybe it was a course they were taking, maybe it was something about their future–but we found it really, really rewarding. We stayed in that position for 13 years and the remarkable thing is how different all the classes are. And over time, you learn that, within the first couple of weeks of the new year, you can see something about this class that looks different from the other classes.

That there are leaders who will percolate up in that period of time. You can see the directions this class is going to move. Things they're interested in. Activities. You've never had ballroom dance in the house, and suddenly everyone's into ballroom dance, because some group of this year's sophomore class are really excited about ballroom dance. So, there was always something like that that was surprising and interesting about every class.

I would say that our most serious responsibility, and one that consumed a lot of our time every year, was picking the roughly 20 tutors, who were graduate students or professional school students, who all had a responsibility in a particular academic area. Whether it was the tutor in biology, or the tutor in economics, or the tutor for the premed students who was helping them with their applications. But they also all had responsibilities for about 20 students each.

And it was interesting because sometimes when students had a difficult time, they have a floor tutor. Maybe there's a tutor in their subject area they know. But sometimes, the tutor they felt closest to, was the one that they'd had this great discussion with, you know, paddling a canoe somewhere. And they would go to them.

And just one other thing about selecting the tutors. When we did, I guess the best way Sue and I could describe it, we'd go through all these interviews, it was like: If you had the opportunity to pick people for your lifeboat, who are the people that you knew you could completely depend upon? You would not have to worry about them thinking about themselves before they thought about others. The people you would feel really comfortable having in that role.

And we would meet regularly, to discuss students who we needed to make sure all the strings in the safety net were properly knotted and in place, and ready to be deployed in various stages, to make sure that a student didn't slip. And it's not often appreciated that if you were in a funk and didn't go to classes for two weeks, it would be impossible to catch up. And just had to be ready to help students at the first glance. And sometimes the information that was helpful came from surprising sources. We worked very closely, with all the staff in the dining hall and the custodial staff. They were sometimes the ones to tell us that they had seen something that they thought was unusual.

The community effort that really I think, helps sustain one of the most remarkable statistics about the Harvard College class, and that is, the number who finish in four years. And at times, of course, students have to step away for a semester, but we do everything possible to avoid that, and to prepare them well before they come back.

PART 5 – Role in the Founding of the Harvard University Center for the Environment and in the Re-configuration of the Harvard Museums of Science and Culture

Alex Griswold: Let's go back to the early 1990s. You were part of a committee whose work resulted in the establishment of the Harvard University Center for the Environment. Can you tell us about that time?

Jim McCarthy: In 1990, then Harvard president, Derek Bok, formed a Working Group on the Environment. He appointed about 15 people across the different schools at Harvard to have a series of discussions and write a report for him to recommend how Harvard could be more effective in our work relating to the environment. And he was interested both in research and scholarship. What should we be doing that we aren't doing now, if anything, relating to educational opportunities for students? He was interested in the interconnections that might be found among the different schools of Harvard relating to the environment. And he was also interested in seeing if we might be able to envision ways that Harvard could be more effective in the policy arena, both nationally and internationally, relating to the environment.

And this was a time when there was not only growing interest in what was changing in the environment but beginning in the late 1980s and rolling into the early 1990s, we were having increasing international dialogue relating to policy matters and climate change.

Lou Branscomb appointed chair of this committee. Lou was a very distinguished and accomplished scientist. Remains so today. He was a physicist, he had been for much of his life at IBM as the Chief Scientist. He was very active in national science affairs. He was member and chair of the National Science Board, the governing board of the National Science Foundation for many years. And after he retired from IBM, he came to the Kennedy School as a professor of the practice in the area of science and policy. So, this committee had a series of meetings and it produced a report that they submitted to President Bok about a year later, so in the spring of 1991. And after Derek had reviewed the report, he asked Lou and me to join him in one of his regular meetings with all of the deans from the various schools. And he had given the report to the deans and wanted a full discussion of their views.

And it was a very interesting meeting for me. I had never met many of these other deans. Our own Dean of Arts and Sciences, Henry Rosovsky, was, of course, there. And many of the questions that arose were the obvious ones, "Why do we really need this? You've shown this impressive record of how much we're doing in the environment, what are you saying we need to do differently? And wouldn't it cost a lot of money? How are we going to pay for it?"

But Derek took away from that a very strong sense that, from the report and from his meeting with his deans, that this is something Harvard should do. And among the recommendations he included a serious consideration of some sort of new undergraduate program. It included a recommendation of a gathering place where people from across the University could meet and interact on topics of common interest relating to the environment. Included recommendation of a group of post-doctoral fellows who would be brought here for a year or two and talked about how they might interact. It was, in Lou's mind, not unlike the way the Society of Fellows worked. We'd have an area where they could gather to meet. We'd have regular dinners with faculty, and stimulate interaction that, within the group and between, not only the fellows, but between the faculty who were sponsoring each of those fellows, in a way that doesn't normally happen with post-doctoral appointments in various departments around campus.

And of course, a space for all of this. And a budget. So the disappointing thing for those of us who are on this committee was the realization that from the time that Derek Bok had decided to appoint the committee, and the time when the report was done and he was now considering what to do about it, he had decided to leave the position of President of Harvard.

And what he told us was that he did not want to make a decision that would in any way tie the hands of his successor, but he did promise us that he would tell his successor about this study and he would relay his strong sense that he, Derek, thought that the University needed to do something akin to what we had recommended. So, we felt comfortable that, with that representation, we had a good chance that something like this would happen.

As we sort of awaited the opportunity to have his successor, Neil Rudenstine as the new President of Harvard, deliberate on this matter, we, of course, had no idea what would happen but as we saw President Rudenstine's interest begin to be expressed in what he might do in his presidency which included more connections among the various schools of Harvard. More interdisciplinary work, more work relating to the environment, it seemed we were in a good position with this report. And indeed, he did decide to create a Center for the Environment. Mike McElroy, who had been a member of this committee, that I'll call the Branscomb Committee, was asked to be its first director. And after a period of time, it was decided that the only opportunity that could be made available, without some major renovations, was office space in Harvard Square sitting above the Border Café.

So, this was the first home for the Center for the Environment.

One of the recommendations that Mike began working on right away was the undergraduate program, and he and I and others had, over the years prior to that, experienced a lot of concern on the part of undergraduates that Harvard didn't have something in this area. And some of them were managing to create it themselves, through the special concentration route, but that was an awkward process and, in addition, the students who were doing that told us that what they really missed was a group of faculty who they knew they could easily turn to when they had questions or wanted advice. Sort of a group of students who, they knew, were maybe floating around somewhere, but hadn't yet found–through one of their various environmentally-focused organizations–were wanting to do the same sort of thing. So, it was sort of a sense of camaraderie that they thought would be enhanced, if we could do something more organized in this area. And so, Mike, and a few others of us, had experiences like that, to lead us to believe that this was the right time to develop an undergraduate program in this area.

Mike convened this group, which included E.O. Wilson, who had also been a member of the Branscomb Committee, and Paul Martin, who was the Dean of the Division of Applied Sciences, and myself. We had a series of meetings among ourselves, we met with the faculty from OEB. We met with faculty from other schools. And began to develop–largely, again, through a lot of listening to what students said they were keen to see–an idea for, a template for, a curriculum. We then went through the hurdles, and all the review committees at Harvard, that were required to advance any kind of change in the curriculum. And ultimately, received blessing to move ahead for this new concentration.

But as we thought about it, and we thought about what Harvard could do that would be really, perhaps, unusual, relative to a lot of our peer institutions in this area. It was not just Environmental Science, but it would be to focus on the interface between the science and the policy.

We have this extraordinary policy resource here, not only through the Kennedy School, but people in the Business School and the School of Public Health, the Medical School, the Law School. Where there's relevant policy in this area being studied and taught. Couldn't we tap into that?

And so a key part of our thinking, for this curriculum, was to not only include the faculty who might naturally gravitate towards an undergraduate teaching opportunity relating to the environment, but to try and engage some of our colleagues who were in the professional schools who, unlike those of us who on this topic maybe sit more or less in the ivory tower, they're more in the trenches. They know what kind of policy is effective in public health and what policy is effective in the business world. This idea of our tutorials, or junior seminars… we ended up calling this "seminars" rather than "tutorials" because of a requirement for listing things in the catalog. You could put a name by something called a "seminar," but you couldn't put a name by something called a "tutorial" the way the catalog listings were done.

But, in my own mind, I always thought of them more as tutorials. And many of these, we managed to design in a way that had paired faculty from one of the professional schools and the Arts and Sciences. So I, for many years, co-taught one of our junior seminars with colleagues from other schools. Initially with Dr. Paul Epstein, a medical doctor. And I co-taught, for a handful of years, with Dr. Jennifer Leaning, at the School of Public Health, another medical doctor, who has spent much of her life working with refugees. And the tutorial what we ran together was on environmental crises and population flight.

There were a number of students who became profoundly interested in these subject areas, and because of these courses, went on to advanced degrees, and in some cases, employment, with various agencies that work in this area, that the students have never really thought much about before.

So, the notion, going back to introducing students to areas and opportunities and policy implications and opportunities for them to become engaged, is–I think–very consistent with the sort of idea that Derek had: addressing real world problems with an understanding of the basic science and some experience and knowledge of policy processes.

Alex Griswold: How has the Center evolved since the 1990s?

Jim McCarthy: The Center continued to develop. And it reached the point where it was time to think about a new director. At that point, it seemed obvious to many of us that Dan Schrag was the person who not only had the interest, but the energy and the connections across the campus, due to his great breadth of intellectual interests, that could really benefit the Center. None of us could've ever anticipated how this would have flourished, that is, the degree to which it has flourished, under Dan's leadership.

Not only was Dan successful in increasing the subvention from the Provost's Office, and particularly, to include funds that allowed for the renovation of the first space that the Center could actually call its own in the middle of the academic environment.

But what happened, in that initial period of Dan's leadership, was the explosive growth in membership, or if you wish, involvement. There isn't official membership, but people associate with the Center and really participate. It's a really striking list of people from all corners of the campus, who come to these events at the Center because they are truly intellectually interesting and relevant. And often have implications, that reach far and wide, and expand people's ideas of not only what could be done, but in some cases, what's actually happening on campus, in areas that they didn't know about that are relevant to their own work.

I'm sure Dan has some idea, but it would be quite remarkable if you could catalog all the grants and contracts, all the research papers, all the ideas for new courses, that have come out of those serendipitous encounters in one or another of the events at the Center.

And, it's also been a tremendous enhancement for the undergraduate experience. To actually have a physical home, and one where students feel welcome was a great achievement.

So, I think it is an example of one of those interdisciplinary activities, inter-school activities, that couldn't happen at a lot of schools, where the boundaries between departments, the boundaries between schools, are so rigid that the prospect of anything like Harvard's Center for the Environment would just be almost unthinkable.

Alex Griswold: I don't think it just happened spontaneously.

Jim McCarthy: No. It was sensing that faculty interests on the part of a president and provost and deans that really made it possible to think this was a good idea and worth spending some of their resources. But the level of faculty interest was critical. The notion that a Dean of the Faculty of Arts and Sciences didn't mind seeing a space in the Faculty of Arts and Sciences used to nurture something that was going to benefit faculty from other schools? Again, they're universities, so that just wouldn't happen. They'd be looking for some contribution from all the different schools.

Or the notion that, "Well, I guess that's the Faculty of Arts and Sciences version. We need another version sitting at the Business School, another version at the Kennedy School, another version..." and so on. That didn't happen, because it was so clear that this was for the University. That's one of the characteristics of this university that makes it really quite different from a lot of other places.

Alex Griswold: You were Director of the MCZ from 1982 to 2002. Can you talk about that time and about the public outreach component of the Museum?

Jim McCarthy: When I became director in 1982, there were a number of people within the Museum, and beyond the Museum, who were a little unsettled by decisions that had been made by my predecessor, A.W., or affectionately known as "Fuzz," Crompton. Because Fuzz's idea had been to bring the Museum more into harmony with the academic fabric of the university by only appointing people to curatorial roles who could also serve and meet the standards of a professorship at Harvard. So, there were professors in the Museum who were curators, but there were many curators who weren't. And going forward, he decided we should affect a plan that basically married the curatorship and the academic role in the Museum. This transition was still in progress, and there were a lot of financial needs at the Museum at that time, deferred maintenance, and we had a lot of expenses that were eating away at the endowment resources as they were available for current operations and faculty and curatorial positions.

So one of my challenges, working closely with people in the Dean's Office and the President's Office, early in my administration as Director, was to try and sort this out in a way that allowed the Museum to continue to thrive and to become even more intimately intertwined with the academic Faculty of Arts and Sciences. But also, to be creative and see what we could do with our own resources in ways that hadn't been so successful in the past. And one of them had to do with our public outreach efforts. Could we benefit more by developing activities that would attract people to the museum, not just as occasional visitors? But could we engage, through some of these outreach activities, people who might want to become more involved with the Museum and financially support some of its activities?

We worked hard to develop our, what we called, public programs. A lecture series, our travel program.

In those days, it was all by mailing and starting off with Friends of the Museum and friends of Friends of the Museum and alumni groups and the like. But we built quite a following and were very successful in engaging some of these individuals in a way that led to philanthropic opportunities for the Museum that were quite beneficial to us.

Our public lecture series, if you think about it and look across this whole corner of campus and realize, if someone has heard a physicist or a chemist or an anthropologist speak on Harvard campus, it's probably through the Museum lecture series. This has really been the center of public outreach for the sciences at Harvard, going back decades now.

We tried to bring about a more effective integration between what, at that time, was a very successful outreach program for the Museum of Comparative Zoology. But it was difficult, because there was a lot of territoriality that had grown up over the years. And although we had interconnected galleries, there just, historically, had not been a lot of cooperation. This combined effort brought together three of those units: the Botanical Museum, the Mineralogy Museum, and the Zoology Museum under the rubric of the Natural History Museum. And we still had, as a separate public program entity, the Peabody Museum, but also the growing interest in the Collection of Scientific Instruments, and under some new leadership, a growing interest in more public engagement with the Semitic Museum.

The Dean of the Faculty of Arts and Sciences, Mike Smith, decided that it was time to take a look at all of these museums to see if we couldn't come up with a new combined effort that we called Harvard's Museums of Science and Culture.

This took a couple of years. And we put forward a proposal to hire, from the outside, a new director for this initiative, and as a result of the national search, hired Jane Pickering, who had been at Yale, with the Peabody Museum at Yale, another Peabody Museum.

She was given an opportunity to develop this museum with an opportunity to make staff changes, to use the staff positions that had been provided by the parent or constituent museums, to create a whole that was greater than some of the parts. And we can see some of her successes everywhere we look at the public programs today. At the lecture series, which is more robust than ever, at the various educational opportunities that we provide for school children or for teachers, the renovations and re-installation of the exhibits, the fundraising that's required to make that happen, the admissions, the number of people who are visiting the museum, the number of times people open up the Globe and read about something new happening at the museum. It's completely changed our profile.

So, this has been a terrific success for Harvard. It's one of those examples that we point to when people talk about their relationships between the university and the community. All you have to do is go into a 5 PM lecture offered on a stormy Thursday evening in the Museums of Science and Culture, and see how many families from the neighborhood, coming to these lectures, as well as faculty and people who are interested in what our colleagues are saying on a particular subject of relevance to science and culture. I think Harvard is coming to appreciate how valuable a resource like this is and making those connections that allow people who are not a member of the university family to appreciate how important our work is here.

Alex Griswold: What kinds of educational opportunities are available to students who are committed to addressing the big environmental challenges facing us today?

Jim McCarthy: I don't think there's ever been a more exciting time for a young person to enter this broad field of Earth System Science or Earth Sciences broadly defined, meaning including everything from terrestrial ecology to atmospheric chemistry and solid Earth and fluid Earth processes. And there's an enormous space to move around from topic to topic in many university programs in this area now.

That is to say, you don't have to specialize early in a very focused way that leaves you, perhaps, with fewer options later on. And I think many universities are doing a good job with this now, giving students the opportunity to see the broad range of interesting questions and making it clear that fundamentals are really important. Fundamentals, knowledge in biology, and physics, and chemistry and as much math as you can possibly take will be helpful. But, the need to specialize beyond that at an undergraduate level is, I think, much less today. That is, you can move into a graduate program with a broader preparation across these areas and be very successful than would have been the case in the past.

So, I think as we look at the problems society faces today, you'll see that many of these not only require an understanding of science, but they require an understanding of how the science will or can be used.

And, in this regard, I think again, that many programs and universities today are doing a really good job of helping students understand that, if we want to be a scientist in this realm, and if we want to work on problems that we think are important to society, we also need to know something about how society uses that information. And so, some familiarity with the policy process, with how society uses science as one source of information and decision making, but there are other perspectives, as well, that will always come into play.

And I think the better we can prepare students to see how science and policy can meld effectively, the better they'll be prepared to be effective scientists in the future if indeed they choose. And some don't. But I think increasingly young people want to see that the science they're doing has some utility or value to society and the decision-making process.

So I think the future is extremely bright for anyone who has general interest in these areas, and wants to contribute to the understanding that society really needs and making important decisions about how we use resources, and how we respond to what science tells us are some important crises facing us today. Whether it's the rate at which we're warming the planet, the rate at which we're adding waste to systems that cannot tolerate much more of this waste, the plastic in the ocean, for example, or contamination of rivers in some parts of the world. There are opportunities today to work on those problems that we've never had before.

Students who take advantages of these interdisciplinary programs that are available in many universities today, such as our program here and Environmental Science and Public Policy, will go into a variety of careers. Some will be scientists, but many of them won't. Some will be business people. Some will be civil servants. Some will go into other fields, medicine. And yet, they all carry with them this knowledge that there are fundamental problems that need to be addressed. And there are ways that they will be addressed through all of these professions.

You don't simply have to be a scientist to feel you can contribute to the resolution of these problems. They'll be informed citizens, but in their professions, they will have the opportunity to influence decisions about how resources are used. And not only to minimize some of the problems we recognize today, but with the knowledge they have of these interactions, they'll be prepared to address problems that we are not envisioning right now.

So, I think this is a reason to be very optimistic, that the number of young people today, college age people, high school students, who know and understand these problems in ways their parents didn't and don't, is a very, very good reason to be optimistic that we are going to get our hands around some of these larger problems.

And it won't be the worst-case scenarios which are, unfortunately, what a lot of the trends appear to be pointing to now, but we still have time to influence these systems. And I think this generation of college students that are moving into their careers, high school students who are making choices about colleges now, are where a lot of the action that will be needed in the future will arise. It will be these new generations of students.