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FAIC INTERVIEW WITH PAUL WHITMORE BY KELSEY WINGEL

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Location: Via Zoom Video Conferencing due to COVID-19 pandemic. Paul Whitmore in Woodbridge, Connecticut. Kelsey Wingel in Hockessin, Delaware.

PART 1

Paul Whitmore: I have to try to follow the thread of my backstory, back to the beginning. My discovery of conservation is a very vivid memory for me because it happened at a pivotal point in my life where I was beginning to wonder what in the world I'm going to do with myself for the rest of my life. I was in school. I was in college. It might have been my junior or senior year. And I was in the habit of going to lectures in the chemistry department given by faculty members from all over the place, who were invited or who stumbled through. I was at Caltech where I was going to college at the time. And one of the lecturers that came in was an analytical chemist from one of the California schools, and I can't remember which one, but he had somehow hooked up with, or probably he was sought out by, conservators at the Getty Museum, who were interested in his ability to analyze trace elements or trace minerals in marble stone.

The story of this lecture then was how this analytical chemist was trying to follow these veins of trace minerals across fragments of a sculpture that had missing pieces, so that you could tell by looking at how the veins connected across the break, or across the missing part, whether the position of the arm was up or down or whatever. And it sounds kind of sensible but goofy at the same time, but I was totally captivated by this notion that a chemist could find something, some application of those talents, to do something so wild and wonderful in the world of art.

Full disclosure here, to me, at that time in my life, I was not into the visual arts. I had only been in an art museum maybe twice in my life, and that was on trips to the zoo. When we had to wait for the bus to come and the art museum was air conditioned, so we would wait inside and sometimes wander into the galleries, but it was not a part of my life. Paintings and sculpture were just abstractions to me. I was much more into music, that was the art passion that I followed. I was teaching myself to play instruments. I was playing music with my sisters. That was the part of me that was the artistic bit.

So I'm in college and see this lecture, and about that same time in my life, I'm going to art museums because the Norton Simon Museum was just up the street, LACMA was a bus ride away. I think I might've gone to the Getty, I can't remember now. And I only did that because I was going out on dates with my significant other, now my wife, so I can say who she is. She might be listening from the next room so I can, I guess... So I was going out to art museums just as a place to get together. And that too was transformative. I'm going on a digression again, but it was at about that same time that I was discovering this world of conservation and conservation science that I had never heard of before, that I was also going to art museums and seeing art and having it affect me in a transformative way, too. So I do remember to this day, and it's like 40 some odd years later, I can still remember the emotional experience of the Rembrandt self-portrait that's at the Norton Simon Museum, that just riveted me.

I'm sure I'm not the only one like that, but it was the first time in my life I think I can say that a painting had just buried its way into me like that, that I couldn't take my eyes off it. I'd just stare at it for, well, for times that would make guards uncomfortable. So it was that time, that period of my life too, that this switch was being flipped or I was being wired in a different way to look at art inside of an art museum in a completely different way. It wasn't just a place to get together. It was a way to have those kinds of personal experiences that I had never had before. So this notion then, I was kind of primed. I was a chemist by inclination and by aptitude, I could do the science stuff, I was good at it, and I knew that about me.

The art side of me, it was more music, like I say, and then it became this... Or it expanded to include pictorial arts. And so I knew I had those kinds of two sides of me. And seeing this lecture by this chemistry professor that had blended those together in this really compelling story, it was just love at first sight. I knew that if could get extremely lucky and prepare myself, that's what I could find my life satisfaction doing, finding some outlet for scientific activities that would be aimed towards those kinds of outcomes. So I was in college and that began my kind of fitful, sporadic inquiry as to, "How do you do that?" I mean, the thing I had just seen was an academic chemistry professor, who had managed to kind of do this on the side. And I saw that and I said, "Okay, but that would mean I have to become an academic chemistry professor, and then wait for the call from the Getty." And that seemed kind of chancy.

And that kind of, in retrospect, that highlighted the problem that college students face trying to find careers in these obscure niche areas, because they're likely never to see that in college. Mostly what you see are... Most of your mentoring is coming from faculty members who only saw academic tracks as a career path, and they may not know about these kind of weird excursions that are also ways of applying your trade. And in fact, in academic circles, those kinds of deviations from academic tracks are looked down upon. I mean, the dichotomy is usually academia and what was pejoratively called industry, going to work for a company or doing anything else, that was viewed as selling out. You're just going for a paycheck. You're losing the purity of the scientific enterprise going off to make money at it, or helping a company make money at it, or doing something that doesn't elicit new knowledge, but you come up with Post-It notes or

something, which seems mundane, but transforms the world in ways that most academic scientists don't.

So, anyway, it was pure happenstance that I was at that lecture for that visiting professor, who had done that weird thing as a sideline, and I just wanted to do that. I wanted to learn more about it to see what other possibilities there are, who else does this? Where did they do that? How do you get into it? How do you get trained for it? All of those questions that everybody asks when you feel like you're seeing a potential career path, you just want to pull on that thread and see what you discover.

I didn't do that right away. I was graduating from college and I thought, whatever I decide to do, getting an advanced degree in chemistry was probably a sensible precaution. Well, because I didn't know, I didn't know much about conservation science. I didn't know if it was an actual career or if you had to do it as a sideline business. So I went to graduate school, I went up to Berkeley to do my graduate work. And at Berkeley, there are... Well, I mean, as you know, from having gone to graduate school, there are flaming hoops along your path that you are expected to jump through. And it's a price you are asked to pay to advance to the next step. And at every one of those flaming hoops you have to be convinced that it's worth it to you, that that is the direction you need to go in, you need to jump through that hoop to advance in that direction.

But that's a question you have to ask yourself, is this really worth it? Or should I go off and pick apples at an orchid? Which a classmate of mine actually did. And I can't say I didn't feel some jealousy for his choice in doing that as I stayed in graduate school and paid the continued price. But it was in my second year in graduate school when you have to do your qualifying examinations, and write your, it's not a thesis, it's a... I don't know what you would call it. But it's like a report about the research that you plan to do. And you have to, just like a PhD thesis, at least at Berkeley, you would have to write about your research that you accomplished, and you would also have to write, I think they called them propositions. They were like research proposals on something that was disconnected from your own research.

Your research is the brainchild of your faculty advisor. So they were asking you to come up with your own idea to, well, to see that you can do it, but also to give you the experience of doing that. And you had to do two of those. Anyway, so at that point, when I'm being asked to do something that's, I won't say hard, it's challenging. That's when I started asking myself, "Is this really worth it? Do I really need a PhD? Where am I aiming to make this worth the price I'm going to pay?" And at that point then I started inquiring more about conservation, conservation science. At that point when I haven't really committed to the full graduate school thing, I could still have done the reset and thought about going into conservation graduate school. And I actually did think about that. I did explore enough to find out where the schools were, and to send out inquiries and to get back the very polite invitations to submit an application, including my transcripts and my portfolio of artwork.

And when I got to that last qualification, I said, "My what? What is that? And how long ago should I have been planning for this to happen?" I didn't have a portfolio of artwork. I didn't make art. If anything, I was a Sunday painter and I did it just for fun. I wasn't serious or actually any good at it. So that quickly kind of quenched the fires of conservation as the career. I didn't... How do I put it? I did things with my hands, but they were mostly mechanical, right? I like to tinker. I like to build things. I like to repair things. I didn't like to... But they were all kind of already engineered, I wasn't fashioning them out of clay. So that creative side of it was just completely absent from the things that I had done, and I didn't know if I could.

So I think I put aside conservation as the career. And I decided I do want to get into conservation, but I'll have to get into the science side of it. And if I want to get into the conservation science side of it, I knew at the time that the market or the job prospects were limited, let's diplomatically call it that. Like, somebody would have had to die. So I decided, "Well, that's a high risk job choice, so I better get the PhD, which will provide me options, and it'll let me stay employed long enough so that I can wait for somebody to die, and hope that I hear about it before the job is filled."

It didn't quite work out like that, but almost. I mean, it wasn't the natural attrition, but it was the very slight expansion of the conservation science world that created another job, that somebody moved into, to create a vacancy that I could apply to fill. And that's what happened, but not for a while. Okay. So I'm in graduate school. I decide I'm going to strap myself in and get my PhD, and I finally did that. And then as I was reaching that punctuation mark in my career again, I was sending out letters to people who might know about a conservation science job as a next step.

And at the same time, I was also starting to go visit conservators. I had kind of decided that I didn't have the mailing addresses or the names for a lot of people. So I started going to the museums in the Bay Area, and there was only two that I could walk into with an invitation. I think it was the de Young and the Fine Arts Museum - what is it, The Palace of Fine Arts, is that what they call it? - that had conservation departments. And so I visited the conservators there. I kind of saw for the first time, the back rooms and the patients on the operating tables, and it looked professional. It didn't look like a Midas muffler shop. People were really careful and thoughtful, and doing things more meticulously than I could imagine, which still astonishes me to this day, the infinite patience you have to work with an optimizer, and do things a millimeter at a time. I can't get over the genome that you must have in your personality to put up with that.

But I saw that for the first time in my visiting. And so I think I visited the folks in the Bay Area, and I went down to LA to visit Frank Preusser at the Getty. And this was when they were still in the villa. They hadn't moved off to the shopping mall and then Getty Mountain. So it was Frank Preusser who was there and the old timers, Eric Hansen and Michael Schilling. And they had a couple of retired chemists who worked with them, whose names I'm going to blank on just now, but it was a science lab. That was the first conservation science lab I had seen.

And then I went to the LA County Museum and I saw Pieter Meyers there, who was the head of the department, and saw conservation science in that department.

And it was kind of intriguing and challenging. I had nothing in my graduate preparation that looked anything like what they were doing, but some of the problems that they were tackling, I wanted to help, I wanted to do that. So I didn't have anything except enthusiasm to offer, but it was the kind of thing that I felt would be the kind of challenge that the scientist in me would find rewarding, and find hard, to push me, to do something that was hard, that someone off the street... Only someone with a PhD in chemistry could do. It's not true, but I wanted to make that PhD somehow feel worthwhile, that I could do something now. I don't know. That's a whole nother story about graduate school.

So I'm graduating from graduate school, about to get my PhD. I'm scouting around in the museums, and finally getting into the conservation studios and the conservation science labs to see really what it's like, to see really what they do there and to talk with them about what the life of a conservation scientist is like. And that was... I didn't hear anything to turn me off. I didn't discover anything that seemed off-putting, or made me feel like that was a bad fit or I was a bad fit. So it was all kind of encouraging to me, seductive, kept me going, it kept me on the line. There were no jobs, as I discovered sending my letters... From those first contacts, of course, I get more names and more addresses. And this being the time of snail mail, I was sending letters to places and people, whose names I had, asking, "Do you know of anything, or do you know of someone else I should ask?" The usual sort of job hunting. And there was nothing, there was bupkis at that time.

And so I had a safety net, I was getting a PhD in chemistry. I could do a postdoc in chemistry. So that's what I ended up doing. I was graduating from Berkeley. I went back to Caltech to do a postdoc in the chemistry department there, doing kind of the same... Okay, so my graduate work, my graduate research project, was studying surface chemistry using optical spectroscopies, reflected spectroscopies that were very sensitive and very unusual. So it was all a build it yourself kind of experiment. There was no commercial equipment that could help you. So I got a lot of experience in building things, doing things on the cheap, figuring out how much I could do to save money. That confidence, that you can make something if you can't afford it, it was something that just carried me into this field because budgets are tight. Budgets are always tight. And you'll always be wanting to do more than you can afford. So not being put off by that, just figuring out how you can do what you want to do from kitchen utensils, that kind of MacGyver attitude was wired into me in graduate school.

Kelsey Wingel: That's amazing, Paul.

Paul Whitmore: Well, yeah, I don't know what I would have done if I hadn't had that confidence because at the time you had a microscope, a polarized light microscope, you had an infrared spectrometer, not an FTIR, one of the old fashioned ones that scans through a spectrum. You had like a UV-Vis spectrometer, a scanning thing that was as big as a car, and that was it. So there was no electron microscopy. There was no miniaturized anything. And yet that's all the samples you had, were these

miniaturized things that don't go into those kinds of instruments. What the heck are you going to do? Well, it turns out you don't do it. You don't do the analysis because you don't have enough sample to use those machines. That's why so many of the reports from back in the day say, "Well, and we found lead white. We found Prussian blue and organic red," because you couldn't identify the organic stuff because there wasn't enough sample.

So anyway, in graduate school, getting back to my... I don't know which train of thought this was. But I started off saying that my research in graduate school was surface science. So I was using optical spectroscopies to probe chemistry happening on surfaces when they would react with environmental stuff. Well, I can honestly say that that had precious little to do with conservation period, which is not to say that I didn't sell it as being exactly what you need in conservation science. But the postdoc that I went to was doing that kind of hardcore chemistry skill. There was a chemistry professor there at Caltech, who was trying to set up the kind of experiment that I had just finished making back in Berkeley. So I was able to make that equipment for him on the cheap, by making it myself.

And that was my two years of postdocing for him was pretty much building that contraption for him, and training his graduate students how to use it, how to fix it, how to make it themselves. And then my two year postdoc was coming to an end, so again, another comma, and I had to figure out what to do in the next stage, and so I sent my letters out. Had my Rolodex, sent my letters back out to the museums and the training programs, asking, "Has anybody heard anything?" One of those letters went to Chris Tahk in Buffalo. And I got back a letter from Chris saying, "Funny you should ask because I just got this inquiry from a professor at Caltech," where I was at the time. "He's in the environmental engineering department and he's just starting up a project looking at air pollution damage to art materials on a contract from the Getty. Maybe you should walk across campus and talk to this guy."

Kelsey Wingel: Wow-

Paul Whitmore: So I did. And I found... the professor in the engineering department who was named Glen Cass. And he had been working with Jim Druzik, who had started at the Norton Simon and then he went to LACMA and then he went to the Getty and... No, he was still at the LA County Museum while this contract was starting. And they had been looking at Ozone fading of pigments and dyes and they saw it happening. And so they got some money from the Getty to start looking more broadly at the damage of Ozone reaction to colored things, and then started exploring what other pollutants might have some consequence to art materials. Mainly the colored things, because that was what we could probe most easily.

So I walked across campus and I interviewed with Glen and with Jim and I think I probably had to meet with Frank Preusser who was managing that contract. And I got hired to be the staff scientist to do that experiment for them. And that was my apprenticeship in conservation science. I sold them, I feel only marginally guilty about it, but I sold them on my background as a surface scientist saying, "I've been looking at gas reactions on materials for the last seven years. And it's

exactly the same thing that you're doing here." Wasn't anything... I mean, only in the abstract, does it sound similar. I didn't know anything about pigments. I didn't know anything about color. I didn't know anything about air pollution, so I had to learn it. So I had two years of that job when I was... Well, and I was all by myself.

There was nobody to teach any of this stuff to me. So I was learning. I remember the first thing I had to do. The exposure chambers to do air pollution stuff was just a box. It was just a box that was lined with... I don't know, anodized aluminum or... That must've been it. I don't think there was Teflon in it, but it was something that was Ozone resistant. And then a commercial Ozone monitor to measure concentrations. And that was it. That was all you needed in order to do this, that, and some plumbing to make an air stream and put Ozone in it that you could adjust. And that was very, very easy to make it was already made, I just had to figure out how to turn it on. But what they didn't have, because they were broadening this into more comprehensive looks at pigments is they needed more pigments.

So probably from Frank's or Jim's connections, we got samples from the Forbes collection and I'm thinking it was the one that was at . . . Where was it? . . . It might've been at Harvard, somebody somehow we were able to get portions from a Forbes collection somewhere, but the portions were, tiny or I don't know, not even a quarter teaspoon, was maybe eighth teaspoons.

So enough to make a few samples, but you're not going to well, it was not meant to be an archive of any sort. It was supposed to be enough to make samples for this experiment. So I learned early on that, the labels on the bottles in the Forbes Collection, don't always tell you what's inside. And so I had to check, I had to do the analytical chemistry, whether it's x-ray diffraction or whether it was some kind of solution, visible absorption spectrum. But I had to do pigment analysis. I had to learn and then do pigment analysis to figure out what I'm actually about to do an experiment on. And I had never done that before. And so I learned how to do it by myself pretty much.

And then I had to figure out, okay, I got this dry powder. How am I going to put that on a piece of paper to get good color measurements from, and how do I make a sample that is good for this experiment? And as you know, from doing, color things, there's a Goldilocks sweet spot. You don't want them too dark. You don't want them to light, you want them like mid value so that it's sensitive to change in the concentration. And I had to learn all of that.

And I remember Bob Feller's papers were my guide to that because Bob and his wife, Ruth, were publishing about light fading of pigments. And so they were doing the same kind of experiments, looking at color changes and trying to get reaction rates and sensitivities, but from light exposure rather than pollution exposure. So I was kind of patterning my experiment after what I was reading about in Bob's papers. And, that was when I... Okay, so here's the guy with no demonstrable hand skill. And I was learning how to airbrush dry pigment onto watercolor papers to get uniform color areas at 40%, reflectance at the minimum. And I got pretty darn good at that, to my surprise.

I was able to do that. And then, the rest of it, which was putting them in a exposure chamber, hosing them with Ozone, taking them out periodically, making color measurements on a color eye. That part was just kind of, that was the science part. And it was routine. It was, turning the crank. The exciting new part for me was the samples, making the samples, analyzing the pigments, applying dry pigment.

And here's another diversion. I discovered that the airbrushing of the dry pigment worked best for me if I ground up the pigment in a mortar and pestle, a little tiny mortar and pestle, cause I only had a little bit, but using methanol as the fluid because as you're airbrushing, most of the methanol evaporates on the fly. So you're getting almost this dry application and there's not a lot of solvent, so it doesn't puddle and it doesn't make the application uneven that way.

It's of course toxic to do that. But that's when I was wearing my respirator and standing up the fume hood and airbrushing my slurries of pigment in methanol. And I was getting just these beautiful applications. Fast forward 40 years and Michael Anderson at the Peabody [Yale Peabody Museum of Natural History] is trying to recolor bird feathers and complained to me about the splotchy, gummy, "Uh, I can't get this thing to work because I'm using - " and I said, "have you tried methanol?" And he said, "What?" And so he did that and reported back to me, "It's beautiful."

Kelsey Wingel: Oh my God, that's amazing.

Paul Whitmore: So just this random, random factoid I was committing in my head had managed to help somebody someday. Who knew?

Kelsey Wingel: Wow.

Paul Whitmore: I'm in the basement back in the basement of Caltech environmental lab, doing this Ozone experiment. And I'm . . okay . . . I'm learning things. I'm reading Gettens and Stout to learn about pigments. I'm reading some of the kind of the foundational texts, Ralph Mayer's book on artist materials and trying to learn more about the materials their chemical nature. Kind of see if I can understand better why some things react to Ozone more than other things. Cause if you look at the chemical formulas, like everything looks reactive and it turns out not to be the case. So I had the good fortune too, and in this team that Glenn Cass had assembled, he had another air pollution engineer. His name was Daniel Grosjean, G R O S J E A N. And he was a graduate from that program, the air pollution environmental engineering program at Caltech. And he had set up this company to do air pollution monitoring for the state of California. And for anybody, he was off around the world doing air pollution monitoring.

And he was part of this team working on this Ozone fading of pigments project. And Daniel was the actual chemist in the group. I was a chemist in name only, I didn't know the chemistry, but Daniel did. And so some of the things that we did, some of the papers that I published in environmental journals were actually reports of the science that Daniel had helped out with or had done looking at, well doing things like analyzing the reaction products. Yeah okay. So you had

Alizarin Crimson, it fades to white from Ozone exposure and it makes many compounds, some of which are like vanilla. So it smells like vanilla, which is, I can't say I recommend you're inhaling your samples when you're done with your air pollution experiments, but it was...

So I was working with Glen and with Daniel who said, "well, okay, if stated, what just happened? Was that really Ozone reaction? Can we prove that it was Ozone reaction for this particular thing?" And so taking those - instead of making the applications on paper, some of those applications of pigment were put onto like silica gel, thin layer chromatography plates. So you could do the exposures, create the reaction products and then scrape the stuff and extract reaction products in using mass spectrometry or GC mass spec.

I say the words like I could do it, but as I discovered, and this was my first experience trying to do organic analysis with a mass spectrometer, I can't do organic analysis with a mass spectrometer because you end up with these, the molecules are fragmented and you end up with a mass spectrum that has dozens and dozens of fragments represented in it. And the job of the analyst is to reconstruct what the starting material was from the fragments. And I had no clue to this day, I still can't do that, but Daniel could. So I would do the experiment and then get the data and then hand it over to Daniel saying, "I can't make heads or tails of this. I hope you can." And sure enough, he could, he not only could figure out what the mass spectrum was representing, but he could reconstruct the Ozone chemistry that was necessary in order to get those products from Alizarin Crimson, starting material. So I had the good fortune of working with an actual chemist who knew how to do chemistry.

I'm trying to think, that might've been the last time in my career I was involved in doing chemistry, because I had a real chemist who was teamed up with me. I'm not sure if it's interesting or not. I'm just going to, who knows you probably, you will be bored with all of this soon.

Kelsey Wingel: No, no, no, no, no.

Paul Whitmore: During that air pollution project Glenn and Daniel were adamant that they needed to do the hardcore chemistry in order to prove it. So that this didn't become this kind of a story built on circumstantial evidence. We did this to the sample and it changed in this way. So it must have been this that happened. They wanted to prove it. They wanted to, if that happened, then you should see this chemical product. And so that's what they went looking for. They needed that kind of scientific underpinning to the story before they were satisfied.

Frank Preusser who was managing this contract for the Getty Museum - he hated that. At every meeting we would have, Frank would say, "you told me what faded and how much it faded. That's what I needed to know. I needed to know what sensitive, I don't need to know. I don't need the proof." He was satisfied with the outcome and that's a natural tension in conservation science. And you'll hear it talking to people and you'll see it in the papers that are written by people that there is a natural tendency towards mythologizing. That you think, or that you're making inferences from things that you know or you've read, that this

must be what happens. But in order to prove that it would require, another three years of your full time investment and everybody else has moved on. Nobody's interested in getting the clincher, the clinching evidence, or rather it's within conservation science circles - that's not necessary. Your storytelling is sufficient for the task.

But you can't publish papers in science journals with that because you'll get a reviewer kicking back saying, "it sounds plausible, but you haven't proved a lot." And so you're forced down that path by reviewers who are just like Glen and Daniel who are looking for that proof, that your story holds water, and you haven't made a mistake along the way. You haven't fooled yourself into accepting something that sounds plausible, but could be totally wrong.

And that continues that persist to this day, that conservation science, and I saw plenty of it as an editor of the AIC Journal and as a reviewer of papers that were submitted to other journals, there's still over reliance on storytelling to just stitch together some kind of explanation that seems to fit the available facts without really working at it, without investing any further effort to try to support that with, one more piece of evidence. Give me, something, anything that tells me that that story is really true besides you're seeing something, you see something happen and then you weave this tale about what you think just happened.

I don't want to sound too critical because I would do the same thing because you have limits to your capacity to engage in problems. You can't spend 10 years of your life. You could, people have spent an inordinate amount of time in your career studying a single, very focused thing and you may do it well, you may do it better than anybody else in the world. It's still this single thing. And you have to judge whether the impact of that is worth your investment.

Kelsey Wingel: Yeah, Paul, I think this discussion is so important and so interesting because as a conservator, I don't have as much insight as a conservation scientist would, but maybe this is touches on-

Paul Whitmore: Sorry you're winking out on me.

Kelsey Wingel: Oh am I? Okay.

Paul Whitmore: Transmission problems on the line.

Kelsey Wingel: Can you hear me all right now?

Paul Whitmore: Are you still there?

Kelsey Wingel: Can you hear me?

Paul Whitmore: I can hear you.

Kelsey Wingel: Okay. I just got a little message that said my internet connection is unstable, but it looks . . . I'm still connected. So can you hear me all right?

Paul Whitmore: I can hear you now. There was a freeze frame for 10 seconds, 15 seconds there. I'm sure that was where the best stuff was, too.

Kelsey Wingel: No. Well, let me know if I go out again, but I think it's really interesting to hear you talk about this because maybe it's, this is touching on one of the many reasons why conservation science is such a difficult field in that you only have one artwork and it's not necessarily reproducible and it doesn't have unlimited, sampling capacity and the making of representative samples is a huge challenge.

Paul Whitmore: Yep, yep.

No, that's absolutely true. And one of the foundations of scientific advancements is the ability to reproduce other people's work, to make sure or to build on it, assuming that their story is correct. And if you crash and burn, then that should add doubt to the original assumptions. And there's almost, like you say, there's very limited abilities to do that in conservation because, well, sometimes you have unique subjects. That's like trying to do medicine, medical research with individual patients. And you're trying to learn from that, build on that, and then guess what the next individual is different. That's what makes them an individual. So you end up needing, well, you need something else or you need something more. And some of that may be trying to do like population studies of similar kinds of things.

There are efforts in conservation occasionally. You see that probably more in cultural property that can be obtained in bulk like library books or prints, drawings, works on paper that have been produced in multiples . . . photographs. It's things that have kind of genetic similarities so that you can pool a population and hope to learn something. Paul Messier is trying to do exactly that kind of thing. That's his ambition is to do these kinds of, well, to group things and to try to learn something from the group, but it's a relatively recent advancement and by its nature it's also limited. As much as Paul would argue otherwise, it's still limited. That there are still... occasions when objects truly are unique. Either, well, for various reasons, maybe they weren't made with commercial materials, maybe they were made with handmade things or, materials that kind of had this moment of time when they were available. I mean, there's also - and of course there's the vagaries of aging history that will make things into some unique thing. I'll circle back to that as I get to my later years.

But anyway, where was I? I was still at Caltech and I was doing air pollution stuff. And I was telling the story about how Frank was the counterpoint to the scientific enterprise where he was the one who was pumping the brakes saying, "I've got enough now. You guys can stop now." And Glen would just ignore him or argue against him saying "you got what you want, just consider the rest of this a bonus then. We're not going to spend more of your money. We're just going to work until the end of the contract, but we'll do this other stuff too, because it helps us do the science better." And the implicit argument being, "and that'll help you tell your story with more confidence and security."

So I remember that Frank's attitude towards judging when enough is enough or judging what is enough for the circumstances, because I had the exercise that the

rest of my career, there's no chance of following up on these threads to the full satisfaction of a scientist for all of the reasons that you understand well. But mostly because there's other fish to fry, there's other problems to try to tackle that you can't go down these rabbit holes unless it's really worth it to get to the end of it.

And you just have to restrain your curiosity, your interest your hunch, that maybe there's something there that you should know about. You just have to continually use your judgment as when to call it a day and move on to other things.

And so I would find myself channeling Frank Preusser more than I would like to admit, honestly. That I would have staff scientists working for me, wanting to do more. And I would be the one saying, "Nope, that's enough. We have to do something different now." And that, when I said it out loud, I kind of heard Frank's words coming out of my mouth. And I don't want to say it was shocking, but I understood him better. I guess that's what I'm trying to say. As I reached the point in my career where I was having to make those judgments, I better appreciated the position that he was in at that time saying "no." In his view, it wasn't just, his impatience and his wanting to move on. It was his view that in his judgment, this was sufficient to answer the questions that were important for conservation.

It wasn't so important to know whether there was some reactive species in a stream that you wanted to put just Ozone in. Maybe there was, singlet Oxygen, or some other kind of reactant in that airstream that you didn't realize you were adding. And that was what was doing the reaction that... I could feel it, that Frank was saying if that happens, it's kind of a ... What do I want to say? Kind of a higher order, a finer grain investigation. Right now, we're trying to do the scouting mission. We're trying to explore. We're not trying to drill deep. We're trying to be broad. That's a choice that he was making and I fully appreciate because I've had to make the same kind of decisions along the way. So anyway, that was kind of the ... Again, I'm on the sidelines, I'm watching the two teams playing the field, Glen and Daniel versus Frank. And I am beginning to see that there's kind of different judgments, different ambitions, different goals that the two teams have.

Frank is trying to do what's needed for conservators to get kind of rough cut information that's better than anything, but it might still be flawed in some way. And then the scientists who want to drill deeper to have more security and because that's what they need to do in order to publish science papers and to publish things that they feel more confident, that they won't have to retract someday because they got it wrong.

Okay. So I'm in the basement of some environmental engineering lab. I'm doing that for two years for Glen on this Getty contract. Another Getty contract is coming online to do exposures to nitrogen oxide pollutants, which are important in LA smog and elsewhere, and I'm seeing that as being kind of a repeat. It's the same experiment with a different gas bottle, exactly the same experiment. So I'm thinking, "Well, I'm not sure what I'm going to be learning in this next phase, but I'm happy to do it because I'm getting paid." But I'm also starting to feel a little

uncomfortable because I'm still in the basement of a university and I don't have ... What can I say? At that point, when I'm looking to a career where I'm not following somebody's instructions, someday I'm going to have to think about these things on my own. I'm going to have to come up with my own problems ... That didn't come out right. I'm going to have to find the conservation issues that are worth a chemist exploring, on my own, so I can get my own grants so I can do my own work.

And I don't know enough about conservation because I'm stuck in a basement talking to a gas cylinder. So I decided I need to get out and work in a museum lab. I need that because I need to talk to conservators and find out more about conservation practice and the kinds of things that conservators struggle with and the problems that they need to be solved. The things that they don't understand well, that I can help.

So at that point, I was an AIC member and I had gone to an AIC meeting in Washington, DC and seen the panoply that is the conservation specialty group presentations and I just lapped it up. It was my first contact with conservation in all of its manifest glory and I loved it. I mean, I love going to the meetings and I went to every meeting since then, until last year or the year before. And it was my chance of engaging with conservators and conservation and not just listening to the lectures because I quickly found them kind of repetitive and boring, but talking with them. "What is it you're working on that you can't give a lecture on because you're stuck? You haven't figured it out or it's a problem you don't understand." Those were my trolling around for the next thing for me to do.

So my second AIC meeting, I was giving a paper on the ozone experiment. So it was my first presentation to a conservation audience reporting on the work that we had been doing under this Getty contract and it was in Chicago. And at that meeting, the paper went okay. In retrospect, there were a lot of graphs, which I learned it wasn't off-putting to that segment of the audience that was actively looking for scientific news.

As you know, you can't speak of conservators as a body of same, like-minded folks because their interest in science, in advances, in innovation, it's all over the place. There are some folks who were happy to matt works of art on paper, and that's what they do. And then there are other folks who are out there at the edge, trying different things, wanting to know different things, wanting to learn about gels, wanting to learn about the latest things so they can try it and see how they do, trying to make the advances themselves. So there's that spectrum. And in that spectrum, the audience that would find a reception for my work is at the one end of that spectrum. They were the conservators who ended up being my lifelong friends because they didn't run away. There's not many of them, but I cherish them.

So anyway, Chicago AIC meeting, I am giving this lecture and it goes kind of okay, and it still happens, but would not... Anyway, at the time it was the big bulletin board next to the registration desk that has the job postings on it, right? That used to be a much bigger thing when that was it. There was still the job listings and the newsletter but that comes out every quarter and by the time you

get it in the mailbox your chances of having the job filled are pretty darn good because it's been open for a while. Anyway, there's a notice on the bulletin board at the AIC meeting in Chicago of this opening for a job, for an assistant conservation scientist at the Fogg.

And this is the post. A little Fogg history. Arthur Beale was the head of the department there. He moved across the river to the MFA to be the head of that department and Richard Newman who was the junior scientist there he moved over to the MFA to be the scientist there, creating the vacancy at the Fogg that was advertised. Eugene Farrell was the senior scientist there. And so that was the conservation science lab. It was Gene and this assistant. So I... At the time, when Arthur left, the Fogg conservation department, sorry it was the regional center, it wasn't part of the Fogg. It was a weird thing - they had the Regional Center for Conservation and Technical Studies that was housed in the Fogg on the top floor, but it wasn't a part of Harvard. I mean, it wasn't part of the university or the university museum. It was this business operation that was housed there. We had Harvard status as Harvard employees, but Harvard didn't spend a dime on the department, it was supposed to be self-sustaining.

Anyway, there were the two scientists there. Arthur moves over to the MFA. Instead of having a single person be the director, it was dual directors, Jerry [Marjorie] Cohn was the head of the kind of conservation side of the operation and Cherrie Corey was the business manager for the business side of this operation. Jerry was the one who put the notice on the bulletin board at the AIC meeting. It had, "If you're interested, here's my room number at the hotel." So I call her up, I interview with her in the hotel, one of the chairs in the kind of a lobby space outside the meeting rooms, I'm there interviewing for this job. And the next week I get notice that I'm hired for it. So it was a job that was open for a week, and I just happened to be in the hotel where the job was advertised, to find out about it and to interview for it, and the next person who heard about it in the newsletter . . . too bad, it's already filled.

So that's how I got that job, I got the job at the Fogg and I became... As I was introduced, I was there at the Fogg for two years and during the entire two years there, I think I was introduced as I'm the new Richard Newman. My name was unimportant. I was just Richard. No.

Kelsey Wingel: Do you remember - what year did you start at the Fogg?

Paul Whitmore: It was 86, 1986 I was there. And that was my first job in a museum lab and it was just the thrill and the romance that I had dreamed of. It was just so totally engaging and exciting and fun. It was hard but I was... Everything I did I was doing for the first time. I was learning about how to do that's... The job there was very different from the Caltech job where I wasn't researching art materials, I was doing technical studies of objects.

So I was learning how to do samples, how to take samples, how to analyze micro everything, how to use a polarized light microscope. So everything I did was new and crude. Scientifically it was crude to identify proteins using, I don't know, ninhydrin stains or something. It was what chemistry things that you could hope

to do with micro samples and if you got lucky, then you might get an indication and if you didn't, you just wrote that in the report that it was inconclusive. And there was a lot of inconclusive results from some of your, from the organic things in particular. So things that are kind of commonplace now like medium analysis, bwa ha ha ha. Yeah, that was such a rare and fortunate occurrence when you could identify the paint medium from an infrared spectrum that didn't have pigment interferences all over the place to obscure - it was just such a crapshoot when that worked.

It was kind of the stuff of legend, we would be reading papers from John Mills and Ashok Roy about stuff happening at the National Gallery with their GC... The pioneering work of these analytical chemists who were doing this kind of development and then doing, and then extending that. So you could look at palmitic to stearic acid ratios and tell what kind of drying oil. And it was the stuff of legend, there was no way that I could have done that sort of work with the facility and my crude abilities at the time.

So I had a good time anyway, analyzing... Well I had a good time in learning the skills of an apprentice conservation or an apprentice museum analyst. There weren't a whole lot of things I could do, but I could analyze, I could identify lead white with pretty good confidence because I did so much of it. That was pretty much everything that I did was looking, trying to authenticate something by seeing whether there's titanium white in it, something that didn't belong. And that was the easiest thing was to find lead whites, or at least you knew it didn't look so obviously modern. Didn't mean it wasn't modern, but it didn't have the telltale signature of something that was accidentally modern.

So I did that a lot. And I learned all sorts of things that I hadn't learned at Caltech. I learned about things besides artists' pigment. So things about metals and grain structures and corrosion and stone and textiles and paper. And so I was analyzing all sorts of art materials that I had never had occasion to encounter in my prior work. And it was all new and exciting. And I was reading the books and having Gene kind of mentor me at the microscope. And it was just... And the best part of it all was I was embedded with the conservators. So it was the paintings and the objects and the paper conservators, both kind of the heads of the department and they had interns, they had two interns in each lab every year. And it was... And each of those interns had to do a research project for their fellowship. And so that was where the scientist would occasionally be asked to do stuff for them.

And it was my first experience learning about conservation and learning about conservation research and treatment development and things that are a part of the conservation enterprise that are sometimes intersect with the science, but sometimes not. I would just eat that up, I couldn't get enough of that. And the people there were just so sweet and accepting and helpful, and all of the things that you would like in colleagues, it was just a great time. And I would have stayed there for longer than my two years, but then I... Okay and I had an experience working there that was the pivot point in my career path. That was my work on the Mark Rothko Murals that, and I think I've told you this story already, but I'll tell it again for the recording.

Kelsey Wingel: Please do.

Paul Whitmore: The Rothko Murals that Harvard commissioned in 1960, they were installed in 62. They were large scale paintings in the classic Rothko style of color fields on unpainted canvas areas, some on painted canvas. And they had been commissioned to hang in this kind of a trustees meeting room on the penthouse of their administrative building that had big curtain walls, floor to ceiling glass on North and South walls. And the paintings were of a scale that they pretty much covered all of the interior wall spaces from the floor to the ceiling practically. So they were very large. And that particular room was brightly sunlit from the south facing wall, the window on the south wall. So much so that the setting sun would come and directly hit one of those paintings every day when the sun was out.

So the paintings were installed in 62, by 1970 they had been faded dramatically so much so that there was contention about what was going on and whose fault it was and what should happen to them now. And there's a whole story about kind of the caretaking of those paintings or lack thereof that the painting is not... The fading wasn't the only thing that happened to them because they were in this meeting space, they had had chairs from a table kind of slammed into them as the chairs pulled out during some cocktail party, maybe the same cocktail party that had been spills on the painting surface. Somebody had scrawled graffiti or initials or something in it with a pin. And it was just kind of a sad tale to tell about how these paintings had been abused. Mostly because many of the people didn't like them. They thought when they were commissioning paintings, they were going to get portraits of crusty old people and instead they got these looming abstract expressionists things that they didn't respond well to.

Anyway, in 1970-ish, I think around the time of Rothko's death, they were de-installed and rolled on rollers and put in the basement of one of the museums at Harvard. And there they stayed until 87 I think is when this started. And I think what prompted that was the Rothko Foundation had given to Harvard the construction paper sketches for those paintings. And so somebody there at Harvard had decided that we would have a show. They wanted to show the paintings again, they wanted that to show the studies and at the same time, use this as the opportunity to tell the story about the paintings and the fate that they suffered and the characters involved. Not to place blame but to kind of have full disclosure without trying to cast aspersions on people. But just tell the story about what happened and kind of... I mean there's plenty of blame to be placed. Blame is a bad word for it. There's responsibility, kind of fateful decisions, let's put it that way, fateful decisions that were made about the paintings that cost them the prime of their lives.

So I was brought into this project to analyze the paintings, analyze the studies, just kind of do the technical studies I've done. To flesh out the storytelling and to try to understand better what had actually happened to them and the color shift that had happened. So I did the reverse engineering of the paintings, the construction paper objects were... I had more limited abilities to analyze because they were small scale and I couldn't take a lot of sample. The murals on the other hand, because they were such large paintings and they were kind of... They were

already blotchy looking, what had been a monochromatic color field was already splotchy from the fading that had happened. And so there was the ability or the opportunity to take more sample without leaving obvious additional splotchiness. How do I put it? They were big enough you could scrape off surface pigment and what was left wasn't more disfigured than what you had started with. Let's put it that way.

So because of that, I was able to do more analysis of the organic pigments than I could have done from anything else, because I had more stock I guess, a quarter teaspoon of stuff. So I did some UV-Vis spectroscopy of the solutions. Well, at the time the diagnostic protocol was you do a series of solvents to see what the organic pigments are soluble in and that narrows down the classes of possibilities. And then from the solution spectra, you try to match up peak positions with reference materials. And so I had to first get some reference materials because the Forbes collection didn't have a lot of 1960s stuff in it. So I had to write around, write to pigment companies and try to get samples of things that I thought would be candidates based on the earlier analysis.

Anyway I ended up analyzing with my kitchen utensils. I identified the red pigment that had faded from the light exposure as being a lithol red, Pigment Red 49 or something like that, just from the UV-Vis spectrum and the solubility behavior. And as generations have passed first Narayan [Khandekar] got started looking at, re-examining the analysis of those pigments and using the higher powered tools including mass spectroscopies. And I held my breath while he did that, wondering how wrong I'd got it. And, as I understand it, I didn't get it wrong, that I got it right, even in the crude ways I was studying it. Which is just purely by accident. I was doing the best I could at the time. And sometimes you get lucky and I got lucky then.

So anyway, part of that job was to identify what it was, but just because I had just gotten this tub of Pigment Red 49 from the manufacturer as my reference material to check, I now could make reconstructions of the paintings using the same materials. The parts of the paintings that faded the most were this Pigment Red 49 mixture with ultramarine blue, so I made that, I made panels of that. I exposed them in a window and I exposed them with UV filtering plexiglass over it, to see if UV filtration somehow would have prolonged the life of the paintings. So I did this little mini study of the materials because I had it in hand and it seemed like a good compliment to the storytelling. And as I'm doing...

And this is what I shared with you, as I was doing this reverse engineering, doing the postmortem on these paintings, doing this reconstruction to try to understand better the chemical processes of their changing, I was grieving that these paintings in the 1960s, when we weren't stupid, we weren't primitive, we were, it was rich Harvard at the helm here, they died or rather they suffered this remarkable transformation that we should have been able to predict and avoid. We should have been able to do better than what was done with them. So I lodged that in my head that... and this was an experience that... Rothko was making his own paints. He wasn't reaching for Windsor & Newton, anything. He was getting the lithographic ink from the newspaper print shop down the street. Wherever he was getting it, he was getting other materials because they provided

the vibrancy of color that you couldn't get from Windsor & Newton's stuff. He was making his own things, which made these paintings are unusual outliers to begin with. That made them . . . It's intrinsically, then, harder to define the appropriate care for them because the materials are coming out of left field. I don't know if he painted with this particular stuff before or since, but it was unusual to have those on a museum wall or on any kind of exhibition condition.

It was already in the wings of any population you might try to discover. You start looking at paintings. You're not going to find other ones that look exactly like this, probably. So, these are on the wings this distribution.

Stepping back for a big, broad stroke picture of conservation at the time. At the time in the 1980s, late 1980s, collection management was the big thing. It was trying to zoom out. The argument goes, "You get better bang for your buck, figuring out how you're going to protect, take care of the majority of your stuff." So, it's big picture things like temperature and humidity conditions, lighting levels for kind of the average bear. You're not going to try to zoom in and figure out what individual objects need, because you have too many of them to make it cost effective.

So, the collection management approach was - figure out what the population, on average, requires and then, you'll be saving most of your stuff. You'll be doing right by most of your stuff. That was a very seductive approach, but it also costs you things on the wings of those population distributions, the Rothko mural paintings being on the wings.

And I couldn't accept that. As happens so often, when you start working on things, you get emotionally invested in them. These were patients that we could have saved if we had been smarter about it. Now, conservation scientists, folks like Stefan Michalski... I'll put him up as a counterpoint here. He's going around the world telling people to ignore the wings of the population. Do what you need to do for most of it. CCI is kind of built around that. They're trying to get the best bang for your buck.

So, they're doing this collection management stuff, this risk assessment stuff. Yes, you'll help most of your stuff and there will be casualties. I was so moved by the casualty that I was working with that I just couldn't accept that as being good enough. Or maybe it would have to be good enough, but I wasn't convinced of it yet.

Those two things, working on, well, a number of things. I was working on a work of modern art, modern and contemporary stuff, which I realized most of the rest of the world of conservation science was looking at oil medium, oil paintings, traditional old master stuff, traditional stuff. Most of the modern and contemporary materials had not been touched because they hadn't gone critical yet. So, until they changed, nobody was going to pay attention to them. As you appreciate, the time when you're most effective at intervening is before those things have gone critical and it's too late to do anything. So, you want to anticipate the needs of these new things so that you can give them a healthy

lifestyle so that they don't turn into the next generation's nightmares. Who knows what's in store?

That was one of the things that I became invested in, was learning more about kind of the potential for change for modern and contemporary things, painting materials in particular. The other thing that I realized was the questions that I was being... I was being asked questions about what is this stuff - that's what's at the heart of technical studies. I found myself asking questions, asking myself questions like, "What is it going to do now? How is it going to change? How can I slow that down?" I can't always answer those questions just from knowing what the stuff is.

It was kind of the pivot point of my career away from technical studies towards materials research, towards modern and contemporary materials, towards understanding processes of change so that we can see early signs of it, so we can anticipate it, so we get to learn more about things when they're still with us before they have been transformed so much. And before it's too late to go back. That kind of built in me while I was at the Fogg, built in me some dissatisfaction that I was on a path that wasn't going to allow me to do that so much, to follow that other path.

I was already kind of... I wasn't in the starting blocks ready to leave, but when I got the letter from Carnegie Mellon saying, "Bob Feller is retiring. We're looking for a replacement for him. Would you be interested in applying?" That was the first time that I really seriously thought about leaving the Fogg because I was doing the work that was exciting and romantic with a group of people that I loved being with and with the conservation fellows that I loved their energy, enthusiasm, and their interest in science stuff. So, I was already in a pretty sweet spot, but I just felt like there was something else to be done that I could contribute to that I didn't see a lot happening. Bob's lab was one of the few that was...I could almost count them on the fingers of one hand, the labs that were doing kind of material science stuff rather than technical studies. I got serious about that once I started thinking about it because I had had this kind of simmering dissatisfaction with technical studies alone. I wanted to do more.

As a sidebar, another sidebar, the Rothko show was eventually staged at Harvard, and I was able to visit it. This was the Rothko show back in the eighties when they were planning it, not the reboot with the circumspects. In that show, they had built a gallery space that was the same footprint as the meeting space in the administrative building. So, the paintings were staged in exactly the same in an environment that they had been planned, had been designed for, and that had been kind of trying to recreate that experience. When I went into that gallery space with those Rothko paintings kind of looming and surrounding me, it was like walking into the Temple of Dendur. It was like walking into Tut's tomb. Such a powerful voice from Rothko. The hair on the back of my neck was standing up. The paintings, even in their transformed state, I don't call them wrecks. I don't call them cadavers. There's so much power in them still. I have no idea how it relates to what they once were, what Rothko's original vision or voice was, but there's so much there now.

I tell that story because I never went to see them at Harvard when they were relived because I was convinced they don't need help. They don't need that in order to move people to communicate something. It's just like the Last Supper. Whatever is there is still a work of art. It may be changed, but there's still so much there that you don't need to be putting on the grease paint to make them into something, to try to recreate something new and young, more original, more authentic, to pass that, appreciate what's there. I found myself just kind of being... I don't think I'm the lone voice in saying that that was a technical Tour de Force that was kind of a step in the wrong direction.

It's ironic that one of my mentors in the world of contemporary painting and in Rothko, and the woman that I met in the context of this Rothko project at Harvard was Carol Mancusi-Ungaro, who at the time was at the Menil in Houston and went up to the Whitney and Harvard, and is a specialist in modern and contemporary paintings. She was the one who kind of drilled into my head that people hold modern and contemporary paintings to a different standard because people remembered them when they were new and fresh, and they want them to stay that way. They want them still looking like their baby pictures forever. It's physically impossible to do that because they will have a natural arc of their lives. They will grow up and grow old. To be upset by that, you might as well be upset that your children grow up. It's natural and inevitable. You should, not just make your peace with it, you should enjoy that. Enjoy the maturation of things. You don't want them growing old, but everything will end up growing old anyway.

So, when I say it was ironic because Carol was part of the Harvard project for relighting the murals to make them look new again, to, quote “make them look new again,” even though they couldn't possibly do that. I thought it was odd to have her kind of supportive of that effort, rather than being the voice saying, "We shouldn't be trying to keep doing this to make things look pristine, to be less accepting of their evolution as objects." I understand that the changes in those colors on the mural paintings would transform them from their original conception, utterly, like the Last Supper. It's fragmentary evidence of what they once were when they were new. But, that's what they are now. I wouldn't want to see the Elgin Marbles relit with their original colors either. Maybe photoshop a postcard or something, so I can see what it's like. But, I don't need that. I don't need that in order to respond to them as works of art in their current state and their current stage of their lives. I think I've grown up enough to be able to look at old things and not feel sad, but to appreciate what's there, to use my imagination, how wonderful it must've been when things were younger.

I find that that Rothko experience just kind of branded me in my psyche. It was such a formative experience, a pivotal one, and one that kind of in many ways, made me what I am, because it launched me on this weird path that I'm on. It was kind of impressed upon me, some kind of conservation lesson that I've never lost. That's... I almost said weird. I don't know. Maybe everybody has experiences like that, like the birth of your first child or something where it just kind of changes you. You're a different person after that. I guess that's life. But for me, I can pinpoint that that's set me on this path. It kind of uprooted me from the technical

studies in museum labs and set me off onto a even more perilous career path with even fewer opportunities.

Honestly, that's at every step when I went down this path. Well here, let me put it this way. At that moment that . . . in that Rothko project, I went from doing technical studies, to understand, that were aimed at understanding what is there, to wanting to do research that could understand preservation, figuring out what things are going to become, and try to make a difference in those arcs of life. If I feel satisfaction in my career, it's from that feeling that I've helped keep beautiful things in the world.

People ask me what I do. That's what I do. I didn't articulate it like that. I didn't articulate it like that until I was giving a lecture just after 9/11, when you're surrounded by stories of horror, horrific things that you can't escape. I had to step back from that and say there's a flip side to this world that is filled with people who are doing the opposite of that, that are working to keep beauty in the world, keep beautiful things that keep joy in their life that make you see the good things that humanity is capable of. That's what I devoted my career to. That's what everybody in this profession devotes their career to.

It keeps me going. It keeps me going, and it's kind of a nutshell encapsulation of this conservation endeavor. We're trying to keep beauty in the world as a counterbalance to all of the rest of it. It's not even just entropy. It's like willful nihilism. There has to be somebody fighting against that, fighting in our own cloistered and sheltered way. But, that's what we're aimed at. That's what I wanted to do.

When I was young in college, I wanted to do something that I would feel fulfilled by, that I could look back on a career well spent. I can feel satisfaction that I spent my career working towards those ends. It's a no brainer. I don't know. I guess, maybe, everybody who has spent a career doing something feels satisfaction in doing it or they wouldn't have spent a career doing it. I say that with no [inaudible 01:35:53] at all.

I have another story that I won't burn you with, but it was-

Kelsey Wingel: No, please do, Paul.

Paul Whitmore: Okay. Here's my story. I'm in high school, freshman English class. I think it's freshman. Maybe sophomore English class. My English teacher is a freshman. It's the first class she's teaching. She's fresh out of teaching school. I don't know where to go to learn how to teach high school English. Anyway, first day of class, she's trying to get to know the students. She gives us all this questionnaire, this quiz asking about ourselves. One of the questions on there is, "If you could be anything in the world, what would you be?" I don't remember what I answered, but at the end of that, at the bottom of that page, I wrote back to her some of the same questions. I said, "If you could be anything, what would you be?" She graciously answered the questions that I had asked her so I could get to know her better. When she got to that question, she said, "Well, what would I be, if I could be anything? I would be a high school English teacher, of course."

That was the first time in my life I had ever heard anyone say that they were doing what they wanted to do more than anything in the world. My father wasn't like that. He was doing something that would keep bread on the table. He wasn't doing something because he enjoyed doing it or because he an ambition from an early age. He was doing that just keep bread on the table. I can't say he hated it, but it wasn't a burning passion. It wasn't a lifelong ambition. This was the first time I'd ever met anybody like that.

So, that's what I... When I say that people who are retiring from a long career look back with satisfaction, knowing that they had done what they wanted to do, they find satisfaction in it because that's why they got into doing that. That's not true. That's probably not true for most people. It's not true for most people, and that's why I thank my lucky stars that I've lived this charmed life that has allowed me the opportunity to scout around, to find something that seemed to fit me, to define this weird thing that kind of pushed both lobes of my brain, that satisfied the scientific curiosity, problem solving and the thing that I'm really talented at with this artistic side or this artistic goal to help keep beautiful things in the world, to keep this art stuff in people's lives that truly give me satisfaction. I know that few people in the world can say they found the perfect fit. They found it, they were able to get in it, and they were able to stay in it as long as I was able to stay in it.

I was like this magical existence. When I first started on a conservation science path, I felt like I was climbing onto a tight rope because I knew all it would take would be just the littlest nudge and I would be flushed out of this. Once you lose a position, then the chances of another position being there as you're falling are nil. I felt like it's worth a shot. I don't know how long I can keep doing it, but it was a good long time, a good, long run.

I only got to Pittsburgh. I hadn't even gone to Pittsburgh yet. Here it is three o'clock. We've been here forever. You're so sweet to be patient.

Kelsey Wingel: Oh my gosh. No, it's such a joy. It's such a joy to talk to you. It truly is. We have so much more to talk about, but I know that Selkie [Paul's dog] is waiting for you. Is it time for Selkie's walk, or can she-

Paul Whitmore: This is time for the frisbee.

Kelsey Wingel: Frisbee.

Paul Whitmore: In fact, I know she's not in the house because at three o'clock on the dot she is staring at me because she think I can't read a clock. So, yeah. It's time for me to go throw a frisbee outside.

Kelsey Wingel: Okay. Well, if you're willing, Paul, maybe we could do part two and we could pick up at Carnegie?

Paul Whitmore: Yep. That sounds like a good transition point.

Kelsey Wingel: Yeah. No, it's perfect.

Paul Whitmore: Yeah. You know my schedule. Three o'clock negated, but in the mornings I'm obligated to be out in the woods with the dog. But, other times. My days, one day is like every other day, so-

Kelsey Wingel: Well, what about next Tuesday, one to three again?

Paul Whitmore: Okay.

Kelsey Wingel: Will that work?

Paul Whitmore: Yeah. I think it will if I don't lose the link that you sent.

Kelsey Wingel: I'll send another.

Paul Whitmore: I have no idea what happened to that last one, because I guess it came in an email.

Kelsey Wingel: Yeah. Well, I sent it in the outlook invitation, so it can be kind of hard to find it from there. But, this time I'll send it in an email so it'll be easier to find.

Paul Whitmore: Okay, great. Well, it's so nice to see you again.

Kelsey Wingel: It's good to see you too, Paul. Yeah, hang in there, and I'm looking forward to hearing more.

Paul Whitmore: Okay. Well, I'll try to recover myself from this trip down memory lane. It's weird that the memories are some of the most distant ones I have, but they're some of the most vivid ones too, because they were just like characters and events that were formative and just left a mark that I'm glad of, at least of the good stuff. The good stuff left the mark. The bad stuff, I hope, has scarred over.

Kelsey Wingel: Paul, I'm so happy that you've been willing to share your experiences. It has been wonderful.

Paul Whitmore: Well, thank you again for being so patient. I'm glad that this isn't on a tape recorder where you had to change reels or something.

Kelsey Wingel: Yeah. We're pretty lucky.

Paul Whitmore: Okay. Well, you hang in there, too. Stay safe, stay huddled, and do that writing.

Kelsey Wingel: Okay. Thanks, Paul. I'll see you soon.

Paul Whitmore: Okay. Bye, bye.

Kelsey Wingel: Bye.

PART 1 ENDS

PART 2: May 14, 2020

Paul Whitmore: So you can see me now?

Kelsey Wingel: I can see you.

Paul Whitmore: And we're recording now?

Kelsey Wingel: We are recording now.

Paul Whitmore: Wow.

Kelsey Wingel: I don't know what I did wrong, but looks like it's going to be okay.

Paul Whitmore: Good for you. I'm impressed.

Kelsey Wingel: Thanks, Paul. You're much too kind.

Paul Whitmore: This is from the guy who lost the link that you sent me. I figured out what had happened is that you sent me an Outlook invitation that had the link in it. And I saw it on my phone, which has Outlook on it, but my computer doesn't have Outlook. It's not a Yale computer anymore, so none of that stuff. So that's why it fell into the ether somewhere.

Kelsey Wingel: That makes sense.

Paul Whitmore: Well, thank goodness we're back. Yeah, we're recording now and I'm going to try to pick up the pieces of our last episode. As I recall, I had I think pretty much finished talking about my time at Harvard and I kept on referring to the people there that I enjoyed immensely and I had neglected to name them and maybe it's appropriate that I name them just to kind of place me in historical context there.

Kelsey Wingel: Yeah. I think names and dates are great to include if you can.

Paul Whitmore: All right. So I was at Caltech working with Glen Cass on that Getty pollution project from 86 to 88, sorry, from 84 to 86. And then in 86, I went to Harvard and in 88, then I went to Pittsburgh to CMU. So during that two years at Harvard, I already told you last time, Jerry Cohn was the paper conservation lab head and Craig [Craigen] Bowen was the other faculty member in paper lab. Who was in objects? Henry Lie and Csilla Felker were in objects. And then in paintings, it was Kate Hensick, no Teri Hensick and Kate [Olivier] . . . blanking on the name. Senior moment. So those were I guess my faculty equivalents in that. Not faculty, we were all staff, but they were like the lab heads who were responsible for both the business side of those departments and for mentoring the fellows for their two year, no, one year stint. And then in the science lab, it was Gene Farrell and I was the other scientist. And I think those are the main players.

Paul Whitmore: Well, and Cherrie Corey, whom I mentioned last time, she was like the other head of the organization that was on the business side. And I'm trying to

remember now her ... There was a very odd ... No, I'm going to skip that part. And that's of interest only to me and just barely. So I described my time at Harvard, which was really my apprenticeship in the technical studies end of it, that collection; looking at collection objects and working with conservators, helping them do their jobs and witnessing them do their jobs, learning about conservation practice, ethics, kind of problem solving as a conservator would do it in approaching a treatment on an object. And at the same time, I was going to AIC conferences every year, the annual meeting every year. And I wasn't a member of a specialty group because I would session hop broadly across everything that seemed remotely interesting to me, both as a subject for me to puzzle over, but also just to get some background on how this business worked.

And through those meeting visits, I also met a lot of people. I met people who would become my mentors and my lifelong friends. I went to Harvard to learn about conservation and I felt like I was really getting that. I was satisfying that need by embedding with them and kind of, well, I mean, the Fogg probably like many conservation departments, they had regular coffee breaks in the day, in the morning and in the afternoon. And so sitting around the table, I would have the opportunity to just talk to people and listening to them grouse about whatever.

And like I described last time, hearing about the things that are troublesome are the things that I wanted to be listening for because I wanted to find if there was a scientist, if there was a possibility for a scientist to help in any of those issues. At Harvard, it was a constant challenge because especially for the research papers that the conservation fellows had to complete, there was a lot of intense engagement three months before the deadline. Three months before people leave, then they're coming to me to ask for help.

And then there is the crash course on learning about the subject and then trying to do something that is going to help them with their report. I mean it was intense, but at the moment the fellow left, then the subject was dropped. There wasn't any pursuit of anything. I felt the limitation of that, the limitation of those brief engagements that almost never would satisfy me that I'd figured something out or that I'd really answered the question well. It was just whatever was possible in that short time period and whatever I provided was good enough for a fellow's report.

And so there was just this feeling of jobs left undone. I can't say it was a deal breaker for me, but it was just kind of a growing dissatisfaction with not being able to do the science part to the extent that I thought was important to do. But I fully understood that was the nature of the job, that I couldn't turn it into a research job. It was still going to be a technical studies lab and that was okay. That's what they wanted and needed, but I wasn't sure that was what I wanted. So I guess as I was in the field longer, and here I was in the field, I was an old grizzled veteran of three years experience. That sounds as comical as it is. I was getting oriented as to what the possibilities were for a scientist, a conservation scientist. But I mean, I had only a vague awareness of there being kind of what I term now two flavors of conservation science. One is the technical studies collection object, studying something that may be unique so there's no generalizing that's easy, versus the materials study where you can start with the

generic so that it has more implications broadly. And those are jobs that are, as far as I can tell, they are separate. I'm struggling to think of a single person who does both of those jobs well. Really they take full attention to do each job well. And I didn't know that when I went in. I had just been doing it. I was working on the materials research at Caltech and I was doing the technical studies at the Fogg and it never really dawned on me that I would have to choose. If I were going to follow this career path, I would have to pick one because I couldn't think of trying to do both. And at Harvard, as much as I enjoyed it, I was getting, the scientist side of me was the one that was starting to rattle the bars on the cage because the questions were kind of repetitive and not terribly challenging if I'm looking for lead white, which honestly I would guesstimate that 75% of the time I was asked whether something was lead white and yeah, I got pretty darn good at answering that question, but that was not something that I felt like I was learning a lot from. And I didn't feel like it was even terribly important to know.

And part of that is me. And I guess my dissatisfaction with technical studies kind of derives in large part from my not feeling like those questions are the most important to me. Authorship, authenticity, those don't affect how I view the artwork so much. I mean, it's profoundly important if you're paying money for it, or if your art-historical career is being built on something, you need to know that it is the real thing, and you're not going to be embarrassed by some reveal in the future. But to me, it meant I could live without knowing that and that the fire didn't burn in my belly to learn those answers. And to this day, I can say that I'm still kind of a lukewarm observer . . . consumer of technical studies results that when somebody tells me the news, somebody tells me that Jackson Pollock really wasn't made in 1950s, I shrug my shoulders and move on. Like, okay, now we know. And again, that's part of my developing philosophy in pursuing conservation science was kind of making those judgements as to what's important to me because those are the things I'm about to spend years of my life that I'll never get back in trying to pursue them. And so this whole shift, the Rothko experience was the pivot point. But that whole shift from the retrospective look at where things come from and who made them back in the day and whose hands did they pass through, what changes occurred to them as they passed through those hands? Shifting from that historical perspective to the forward looking one, what are they going to do naturally, or with our intervention or with our poor decision making, how are they going to change?

And are those changes that we can anticipate and minimize or mitigate, or at least come to expect so that they're not upsetting when they happen. Nobody turns over tables when something starts to show signs of age. Because it's natural and there was no way to stop it anyway so make your peace with it. So I just shifted my focus to that single-mindedly eventually, and eventually meaning when I got the offer to go run the lab in Pittsburgh, that was my chance to do materials research, which I chose to aim at studying modern and contemporary things, and understand better what the future holds for those things that we haven't had long enough to know. So it was, I thought it was kind of ripe for inquiry.

But to this day here, however many years later, 30, 30 some odd years later, I mean, I still have that. That's kind of in my DNA now, is this notion that that's where my scientific talents belong is trying to do that materials research,

understand what's coming and well, there's another reason for doing that. There's another reason for understanding the materials better and understanding the changes. It's so that we can get past the collection management, the treating things like the average. And then when things are more vulnerable than the average, you just shrug your shoulders and we couldn't have done anything about it. The Rothko experience, Rothko's works were on the wings of that population distribution. They were more vulnerable, they were as vulnerable as watercolor paintings only they looked like oil paintings on canvas so people treated them like that, naturally.

And it turned out that that was a bad choice. And I guess my mission when I went to Pittsburgh was to try to come up with ways, resource efficient ways, of finding those special needs works of art, trying to assess whether there was something that was, I don't want to say worse than the average, whether they were more vulnerable to changing than the average. And then at least you've targeted something. You've targeted the object so now you can start to pay attention to it more, figure out whether there's something you can do.

And that notion of trying to deal with the individuals in the collection as individuals, but in a reasonable way so that you don't spend your entire life staring at one work of art, and in an efficient way. And so when I went to Pittsburgh, okay, so I'm at Harvard, I get cold called. It was a letter. It wasn't a call, I get a letter from a head hunter saying they're looking to replace Bob Feller in Pittsburgh, who's retiring. This is in 1988 when he was retiring. And I knew Bob and I admired Bob. Frankly, I worshiped Bob as having the career that I wish that I could someday have, that single minded focus on materials and understanding change, and exploring ways that you can intervene. I once termed it that Bob was the one who put conservation into conservation science. Technical studies, as much as people pronounce that you can't take care of things unless you understand them, unless you understand the materials and construction, the truth of it is you can do pretty good without knowing that. In fact, the world has done pretty well without knowing that.

So it's kind of a setup to justify technical studies in a not quite honest way, but understanding the changes in materials, the chemistries that are happening, the ways that you can intervene with environmental choices or with conservation treatments somehow, and the development of treatment practices and the testing of those to see if they're effective and if they're safe and if they're long lasting. All of that is what Bob was doing. And that's why he has the reputation, he enjoys the reputation he has among practicing conservators who engaged with him, is that he got it. He had that conservator's perspective on the chemical problems that very, very few colleagues have. And that's what I admired most of him is that that was the kind of work that I wanted to do. But mostly I wanted to think like a conservator or at least have that perspective of a conservator in my head so I wasn't so utterly reliant on calling my friends and asking about and talking with them. That I could do it myself.

So Bob is going to retire and they're looking forward to replacement. They send around some letters to people. I have no idea who gave them my name. They gave them . . . other applicants for that job, I know, are people who I also

admired. And I thought they were people that I would model myself after. So I had no conception that I was actually ready to take that job. I mean, I could have considered shifting over to work in Bob's lab, having him mentor me, having him kind of teach me how to be like him, but to go in with no more experience than I already had and just to start doing things as well as Bob, I don't know what prompted me to think that I could do that. There was nothing in my background, my experience that would say that I could do that well.

But when you get down to it, and I think that's what I probably told myself at the time, when you graduate and you go off to a faculty position, you're doing something at a level that you have never done before, and you have nothing but faith, self confidence to make you take that step and to give it a try. And so that's kind of what I did. I just took that step into the abyss to see if I could do it at all. But the reason, I can't say what led me to have confidence that I could do it, but I had the ambition, the desire to end up doing it. That was exactly the kind of work that I wanted to see myself doing. That's what I thought I could be good at and help the most people and find satisfaction in that.

So let's see. So I interviewed for the job at Pittsburgh. I met with Bob and I met with Bob's uber bosses who knew nothing about conservation science. They were just ... Okay, sidebar; Bob started the lab in Pittsburgh in 1950. It was the National Gallery of Art research fellow. The National Gallery of Art paid Mellon Institute in Pittsburgh to establish this research organization that Bob was going to head up. When Bob went to Pittsburgh in 1950 to do that, he had no experience doing this art stuff either, but there wasn't any pattern to follow. He was one of a handful of scientists who were engaged with art conservation things. So that lab in 1988, that lab had been there for 38 years. And it's a small wonder that Bob's bosses didn't treat him like a subordinate because he had a lifetime of experience and accomplishment that they just let happen. His bosses just let Bob be Bob and he'll be fine.

So when I went there, the impression, it was my first visit to Bob's lab, I met him for the first time I think. I had seen him at lectures, but I'd never met him. And then I met his staff. He had a staff of four people. Yeah. Four people. Catie [Catherine] Bailie, Mary Curran were the old timers. Val Colaluca was a medium timer. And okay, so I miscounted, Sang Lee was a paper chemist who was there and John Bogard was the technician who worked under Sang. So a staff of five people, and two of them, Mary and Catie, were like on the verge of retirement themselves.

I was used to academic labs where the oldest person there is a postdoc, and here I'm going to a professional laboratory that has been around for 38 years. And some of those staffers had been around for 37 of those, it seemed. So I wasn't quite prepared. I had seen their names on the publications, and I just had this mental vision of the young subordinate to Bob. And here I'm seeing like contemporaries of Bob. I mean, they are subordinate in their status in the lab and in their abilities, but they're like 70 years old when I met them. And I tried to hide the double takes I was doing at every turn. So that was the staff that was there. And the work was kind of partitioned into activities.

So Catie, Catie Bailie was working on paints and pigments and fading, Mary Curran was doing polymer science and Sang Lee and John Bogart were doing the paper degradation studies. And Val was also doing polymer degradation stuff, looking at volatiles, volatile evolution from photodegrading polymers. So anyway, and that's kind of the portfolio of Bob's research agenda that he had for many, many years.

So, I interviewed at Pittsburgh and you never know whether you're the first pick or like the fourth one and the first three turned down the job. You never know. I have my suspicions that I wasn't the first pick because I had no experience practically compared to the other folks that I knew had also applied, but they ended up offering the job to me. And I had to think about it because as I said last time, I was really happy at Harvard. I wasn't feeling like the job was the job of my ... The job was dissatisfying in some regard. It wasn't really the scientific research that I wanted to do, but working on collection objects and working with the people and working with the conservation fellows and doing those drive by research projects with them, those were fun. And I did feel fulfillment to some degree, so I could have stayed there and been reasonably ... Well, I could've stayed there and been Richard Newman for longer.

And to this day, I don't know if that was ... I don't know where I would be now if I hadn't done that, if I had decided to stay and just keep on plowing that field for another 35 years, but I chose not to. I ultimately, well it wasn't purely a professional decision. I was living in Cambridge and my wife and I were looking to like settle down and raise a family and real estate prices in New England at the time, in the Boston area at the time were kind of like they are now. You have to have come over on the Mayflower and gotten one of those when they were cheap because nothing is affordable, especially on the conservation science salary that I was drawing. It would never have happened. And we were ready to settle down and grow up to raise a household. So the move to Pittsburgh where the real estate was a fraction of the cost of Boston real estate, that was a big driving force. I mean, the change in our personal fortunes was one of the big drivers for pulling up from Harvard and making the move. So I did that in 1988.

I described to you that it was, let's see, when was the AIC meeting? It was probably in May, late May. That's when they usually were. And I told you, I interviewed in the hotel and then I, Oh, I'm sorry. This is the wrong job I'm applying for. So when did I get this? It must've been in the spring of 88' when I am interviewed and I get the job in late spring. And the first day of work in Pittsburgh was July 1st. So I pretty much quit on June 30th and then got in the car and drove fast to get there to Pittsburgh in time for the next day's work. I know, it was so stupid, so stupid. I kick myself, every time I do such a thing, go from one thing to another without a break, I don't think about it until in retrospect when it's too late, that that was my chance. That was my chance to take a vacation and see what that's like and to just recharge, before you get back pulling a different sled. And it never occurred to me. I guess I was excited at the prospect, excited at the prospect of taking this next step and to do something so entirely new. And I wasn't sure what was going to happen. Go buy a house, and do all of those things. And I just was so wrapped up in the newness that it never occurred to me to throttle it back. There's no emergency here. It's not like I have

to go put out fires in Pittsburgh, but it didn't happen that way. So July 1st, I'm there, I'm starting my new adventure in Pittsburgh. And I don't have a lot of granular memories of the start because it was kind of a whirlwind. I didn't know what I was doing almost ever.

I'm managing people for the first time in my life and managing people who are old enough to be my grandparents. And I don't know what they're capable of. I don't really know what research I'm going to be doing with them. I didn't want to tear the place down and then rebuild it with new staff. I didn't think that would be humane. So I was going to keep the staff. And then as I came to learn, they were experienced and they were really good at what they had been doing. So if I let them keep doing that to some degree, I didn't have to work through another learning curve with them. And as I came to appreciate on the second day of the job, I didn't have a budget to do much of anything. I had a budget to keep people on the staff. I didn't have a budget that could retool a lab to do something new or to hire somebody different in addition, in fact, I had to lay off somebody, that was my first management thing, was to lay off a staff member, which was horrible.

But because of that fiscal constraint, it was an easy decision for me to make, to carry over the big themes of Bob's agenda: colored things, and polymer things and paper things. And just ride that out. Well, maybe to try to craft research ambitions in those stovepipes so that I wouldn't have to completely retool a lab or hire a different set of skills. I could get along doing what people had been doing and were already tooled up to do. So part of it was just decisions of necessity that I didn't have much choice, but Bob selected some pretty good areas for investigation, things that were important, but not yet well understood. And there was plenty of work to do in those areas. So those are organic-y things. So I ended up not doing anything with metal or stones or all of the other things that a scientist could have investigated.

I just didn't do those. I started off doing the things that Bob had done. I'm flashing on a memory that I forgot to mention. When I was talking about my last time at Pittsburgh [Harvard], there was a moment of a month or two after I had accepted the job in Pittsburgh and before I had left Harvard. So I'm still working there, and it's during that period when I have a chance to meet for the first time, Neil Rudenstine, who was going to be my program officer at the Mellon Foundation, he was the senior vice president of the Mellon Foundation, had been Bob's program officer and I was about to become his new dependent.

And Neil was visiting Harvard because Angelica, his wife, who was an art historian, not affiliated with the foundation at the time, except maybe as a consultant, she was staging an exhibition at the Fogg. And so she came to town for that event. And Neil came with her and at the time, taking advantage of that, Neil was gracious enough to come visit with me. And it was my first conversation with him. And it was kind of stressful because just like I was expecting to see young people working for Bob, he might've been expecting to see a middle career experienced professional who was going to take Bob's place. And instead, he sees this snot-nosed kid and if he was shocked, if he was mortified, he hid it well. But I can only imagine that he must have done the same

mental double-take at seeing me and the early stage of my career I was in, for better or for worse, that was it. That was what you got.

But I had occasions to have conversations with Neil. And then when Neil went off from the Foundation to become president of Harvard, this was after I had left, he became president of Harvard, and then to take his place at the Foundation, they hired another program officer, whose name I remember but I won't mention. But that program officer did not have a thing for conservation. So it was kind of the wicked witch of the East after the house fell on her and her feet are like shriveling up and curling up under the house, that's what conservation was doing under this program officer, who was turning of the spicket of Mellon monies into conservation. And you can imagine what that was like. Because there were no endowments yet. That all came later. So, everybody who was using that money to pay for stipends, for students, or help support labs and conservation positions, that stuff was being clawed back.

And it was crisis mode. And mercifully that program officer was replaced and Angelica Rudenstine was asked to take that place. And that was the white knight that the field needed to get the Mellon Foundation back aligned with conservation needs. And I mean, Angelica being Angelica, she spent much of her time figuring out what the field really needed because then you ask conservators, anybody in this field, what they need and they say they need money. They need money for all sorts of random stuff - not random, but it varies widely, it's for everything. There's never enough funding. And that's what Angelica, I'm sure, heard. And she can't just start sprinkling money like the federal government is doing right now. She had to target it in order to see some be effective and to see some improvement, some lasting improvement. So Angelica came to the rescue after that period.

But during my career, most of my career in Pittsburgh, I had Neil Rudenstine and Angelica Rudenstine as the guardian angels on my shoulder. And I was so freaking fortunate that they were the ones who were there, not just helping me get the financial support I needed, but cheering me, cheerleading me, accepting and understanding the value of what I was trying to do and just encouraging me to keep doing it. And that was, I don't know about other folks, but back in Pittsburgh, I didn't hear a lot of that. I was working in a place where nobody else really knew anything about conservation and the only people who would tell me I was doing anything worthwhile would be the occasional handful of conservators I would run across. And I needed that. I needed to know that what I was doing was a worthwhile thing to do. I felt it. I believed it. But until you talk to your constituency, it's hard not to feel doubt.

So anyway, I have had the pleasure and the special privilege of working with Neil and Angelica and more thoughtful, wise, clear-minded people I don't think I've ever met, ever. They are just so outstanding in their approach to their own jobs and ambitions and passions, that it was a delight. It's still a delight. They're still some of the people, two of the people that I really enjoy talking to more than anybody, just because they're so smart and not just in a book learning way. Smart in the way of someone who has this eagle-eyes view of what the world is like and what the world needs and how you can get from here to there and be

effective. And they just are so good at that. And I depended on that because I was nothing like that. And it's part of the challenge of trying to do science and get it paid for. I mean, science is by its nature, it's this very reductive enterprise where you try to reduce things down to their essentials so that you can hope to understand that. And then you build back up, back to the real world macroscopic, if you're lucky and you live long enough, but when you're doing that deep dive, it's like digging a hole, that's just where you lose sight of the surface of the planet. And it's hard to get back to that eagle-eyes view and to recover that. And so to have people who are navigating in that stratospheric realm, it's essential that they... And not so much, I mean, Angelica was particularly careful not to try to impress on me her ambitions. She must have known what she would have liked to see happen in the world, but she never told me to go off and do that. She told me to decide for myself what needed to be done and then she would try to either sell that or she would do what she would do, take off her glasses and rub the bridge of her nose and sigh, which would not be a . . . signal you don't want to get from her.

She didn't do that often for me, but I vividly remember when it happened. Anyway, I just wanted to mention of the people that I worked with, the Rubenstein's, both of them were my program officers over the years. And I encountered Neil at Harvard just before I went to Pittsburgh and subsequently and it's hard for me to express the impression that he left on me about him and about his wisdom and the help and the feedback that he provided. When he would offer a view, you want to record that in your head because it's really coming from a place that's so thoughtful. Anyway, enough of the hero-worshiping right now. But I'm back in Pittsburgh, I'm back in Pittsburgh and having to manage a group of people with a budget that is tight. And I have no ideas. That's - I came to Pittsburgh with a wife and a car and I had no ideas. So I spent two years in Pittsburgh - that was the equivalent of my postdoc - I was freed of job obligations. I didn't have to identify lead white one more time. I didn't have to work on a conservation fellows research project.

I could shed that and clear my mind to think about what to do within the areas of color and polymers and paper and do the reading and the quiet thinking. I lied a little bit kind of when I said I didn't have an idea when I went to Pittsburgh. I did, I had an ambition and a low hanging fruit of an idea. My ambition was to prevent the next Rothko accident. To be able to understand a contemporary work of art well enough so you could tell if there was light sensitivity in store before the darn thing gets faded irreparably. That was the ambition because I had been marked by that experience. And I only had one trick, which was to identify pigments. So I went to Pittsburgh to try to figure out if I could identify, not pigments, but modern synthetic organic pigments. And I had the primitive tools, the primitive analytical technology of the time to help. And I just crashed and burned.

It wasn't so much that, well, I mean, there was a confluence of things, organic pigments, you need more stuff if you're going to get them analyzed with the crude machines. The machines are crude and synthetic pigments, there are hundreds of them. If you're looking at a synthetic organic red, you have maybe a hundred, 200 that you have to discriminate and you need to do it to the level that you can tell whether it's likely to be light resistant or not. Which is

extraordinarily difficult to do. And it's impossible to do if you only have a crumb or if you can't take a sample, at least at that time. And I tried to do that for a year. And all I managed to accomplish during that time was to expand Bob's pigment collection by a few dozen organic pigments that I had been able to get samples from the manufacturers. And it was a dismal failure. That was an ambition that I had to just decide I'm . . . it's not that I can't do it. I don't have the means to do it.

And even if I did have a machine that could do that, it takes so darn long that you will spend months to try to answer the question, how do you take care of a single object? And that's just not a very efficient way of going about it. You can tell where this is heading, right? This is, this is the unfulfilled ambition that is rattling in my head when I encountered the technology that is the next step. And that's the spectrometer, the diode array spectrometer that lets you take a spectrum in milliseconds. And then you can do fading in real-time with intense lights. And that's how the micro fading tester was born. It was the answer to the Rothko avoidance problem. How do we quickly learn whether something is going to fade really egregiously if we treat it in a collection management style? Treat it as if it is what it looks like or the average of what it looks like? So if it's an oil painting, we'll treat it like we treat old master stuff, every oil painting, we'll go to Garry Thomson's book and we'll treat it according to his prescription, which isn't really a prescription, but people treat it like that.

But that was the only thing I came to Pittsburgh with, was wanting to do that. And as I said, I experienced failure, utter dismal failure, not for the first time in my life, but for the first time in my life it was all on me. Nobody was there to help. Nobody was there to blame. Nobody was there to make the decision where to cut your losses and go on to other things. It was all on me to make those choices for the first time. And so that might've been the first time when I was faced with having to make those decisions about navigating a research agenda. When do you cut your losses and move on. I did it plenty of times. I failed plenty of times subsequently, and it's never an easy choice to make. But anyway, I had that idea. So some of the color work, some of the fading stuff was aimed at trying to identify light-stable or light-sensitive, modern, organic pigments. And that consumed one of the activities.

And then what else was I doing? I was doing something with polymers. I did kind of random things with the polymer degradation stuff. The paper chemistry stuff was... well, the other subjects that I had as vague targets, I had to go to the library and read and think, and I didn't come with ideas in my pocket. And so I had to go learn about the subjects I have never done. I can say at this time, I can confess at this late date, I had never done or even thought about those subjects before I went to Pittsburgh. I knew nothing about polymer science. I knew nothing about paper chemistry. So I had to learn it, before I can find where to start. And to this day, I am not sure. I guess if I had known more, I might not have done paper science because it is the world's oldest polymer science. There's like 100 years of research to catch up on. You'd go to the library to read up on paper chemistry and there's like the 25 volumes, encyclopedia of paper science. Not knowing any of it, I had to go through it and yes, I did learn stuff.

I learned a lot and I ended up learning it to a... Here's what I discovered early on; my natural tendency in school, when you're trying to pick up something, you go to the most recent publication on the subject. It has a reference list and then you work your way backward, right? You work from that to the references. You read those, you work back from those references and you kind of go backwards. Well, I learned that, and again, I won't name names, but trying to do that with a subject like paper science in conservation, you end up reading the most recent things that say this happens in paper or this happens in cellulose and it has a citation. You go back to the citation and it says exactly that same thing. This happens in cellulose and it gives you maybe a citation and you work your way back to the oldest citation you can find, which says nothing of the sort. It's a speculation, or it's an inference without support - it just says stuff. And after generations of citation it just becomes dogma, but it was based on nothing. It was based on a flight of fancy, some unsupported assertion somewhere. And that's what I was finding left and right, as I would keep on trying to figure it out. Okay. I think I understand what they're saying. Why do they say that? And I keep going back to see the original experimental evidence of it and the path just dribbles away - there wasn't any.

Kelsey Wingel: Wow.

Paul Whitmore: Yeah. Wow. And it shakes your faith when you're reading modern things now because at this point I can look at those papers and know that they're just telling stories. I can tell because they're saying things that I know I went back and I looked and they didn't have the evidence for that. And that became the shape of my paper chemistry program, was to go back to the basics and to try to find the experimental evidence for some of that stuff. So, to pick an example, one of the subjects that we studied was the oxidation of cellulose, from light, from chemical, bleaches, whatever. And the papers will consistently describe those as random oxidation with the implication being the outcome is unpredictable. When in fact, if you do UV exposure of cellulose papers, you get exactly the same kind of outcome. Exactly the same carbonyl development, carboxyl development, chain-breaking exactly the same, it's not random.

What random means in that context is that the oxidative attack on the glucose molecule that is the constituent of the cellulose can happen at any of the six carbons with varying degrees, but it can happen at any of those and depending on which carbon is attacked, then you end up with different chemical products being formed. So it's random in the sense of where that reaction happens on the glucose or which glucose along the cellulose chain, but it's not random to the extent that we can't tell what will happen. So it's statistical randomness. That was the meaning of the first usage of the term random, was a statistically random, but it's not unpredictable in its outcome. It's not random like whether a lightning strike is going to happen. But it has been perverted to sound like that. And so that was one of the things I was going back to was to try to... I see it's random and I wonder why and how can that be, it's chemistry, isn't it? And then you work your way back through the literature and then it's not unpredictable. It's not weirdness that you can't explain. It is statistical. And it looks the same every time. And it looks the same whether you're oxidizing with light or with chemical bleaches, you get the same product distribution. The same outcome from the oxidation, simply

because it doesn't matter what you're using to oxidize. It is statistically going to produce the same kind of outcomes. So anyway, it was that going back and trying to understand things, the trails went cold and I had to start over from the very basic things. Like what happens when you're oxidizing with peroxide or with UV light, UV radiation? And it turns out you get the exact same product distributions because statistics are statistics. And that was a revelation that the idea that you can look at those product distributions, and you can tell distinctly which are oxidized products and which are the products of acid hydrolysis. So that you can distinguish whether it was an acid attack or an oxidant. Anyway, going back to the basics, I was able to understand that and experimentally demonstrate things more clearly so that I could work my way back to the other questions about the material at large, like what's happening in a particular piece of paper? Can you distinguish whether the damage from iron gall ink is oxidation of the iron ions or acid attack from iron salts? The answer is yes, you can distinguish them, or you can tell how much of one or the other. Once you get past the notion that oxidation is a random, unpredictable outcome.

It took me most of the two years, most of the two years of my postdoc in Pittsburgh was reading cellulose literature, a hundred years of cellulose literature, because I'm not an organic chemist. I had never thought of such things before. I didn't know anything about polymers in general or cellulose in particular, so I'm reading a lot of the literature just to be functionally literate about it. Then, I can start to look for what are people saying that they don't have the support for and is that worth checking with an experiment. I ended up studying things back to that primitive level and then finding research questions of the basic things that could help me build back into a story that might be of importance to conservators somehow.

The polymer stuff, I'm not quite sure how to characterize that because it didn't really have the same... As I recall, the focus of the polymer work ended up being acrylic paint media because I didn't understand what it does and what it's likely to do. There is just kind of the only sense we have, since we don't have old acrylic paint around yet. I mean, old, old. We don't really have a sense for how fast the changes happen, what kind of changes occur, how they manifest. So, I wanted to start investigating that and kind of seeing for myself what old acrylic medium looks like, and how does it get there. What does it do?

I can't say that the experiments, the research that I did, was particularly deep or insightful, but it was among the first, because it seemed at that time, it was like the whole world was extracting stuff from oil paint and looking at solvent leaching and trying to identify which drying oil was used from fatty acid compositions and things like that.

Again, as the analytical technologies improved, then the level of your question can also become... You can ask the next level of questions, and that's what was happening. Analytical technology was just marching along so you could, instead of just asking, "What is this? Oil, or is it protein?" you can start asking, "Is it walnut oil or safflower oil?" or whatever. But, it seemed like all of the world of conservation science was engaged in that deep dive in oil medium and because I didn't feel like that was the conservation issue, or rather I wasn't sure.

So, I would ask one of the scientists. I once asked one of the scientists who had just given a paper on the leaching of fatty acids from oil paints, from solvent cleaning of the varnish, solvent removal of the varnish. Yes, you get a powerful enough analytical tool, you can see traces of stuff come out just looking at it wrong. So, I asked him, "Okay, you see that stuff? How does that change anything? Is that important somehow? Obviously, it doesn't naturally end up blanching the surface, necessarily. If you leach tiny amounts out, does it change physical properties, aging property, any of that?" It's as though I was asking the question in a foreign language. It just never occurred, or maybe it was unanswerable for the analytical instruments, to answer those kinds of questions about what is the consequence of such a thing happening. "Okay. You've proven it happens. Is it important?" I couldn't quite get over that. And if it's not demonstrably important...

Part of me is channeling Bob Feller when I say such things. Bob was very incisive when I would talk about stuff. Then, he would just tolerate my enthusiasm and then just say, "So, does a conservator need to care about this?" That was his pushback on my just going down rabbit holes. "Is this important to a conservator to know this?"

That's kind of the feeling that I had at the time with this oil paint [inaudible 01:03:23] on oil paint films. I had the feeling that I'm not sure that this is important – seeing examples of oil paints that suffered from having a varnish replaced on it. If it's not egregious, is that really an important enough problem to understand in this level of detail?

Anyway, the whole world was looking at it that way and it wasn't... I didn't see the demonstration that it was important to devote such attention to. So, I just shifted to something I thought might be important, but nobody is looking at it. That was the acrylic paint medium, this whole question about modern materials, acrylic medium being like a placeholder for alkyd enamels, you pick it and there's paint media that have been used in artwork, that hadn't been around long enough to know for sure what's going to happen. I just wanted to get ahead of that curve a little bit and see if there was something to be seen.

Like I say, I can't point to that research that I did and say that that was groundbreaking stuff that changed the world, but I can say at the time, it was one of the few looks at forecasting what modern things were up to and subsequently, more and more folks have been doing that. But, at the time when I started, it was a world of oil paint research.

Life goes on in Pittsburgh. In Pittsburgh. I had the staff of people. Well, I started off with Bob Feller's staff, and then I ended up... As the older members of the staff retired, Mary Curran retired soon after Bob did. Catie Bailie worked with me for a handful of years before she retired. Those slots gave me the opportunity to hire new people to do different things.

Trying to think when it was. 97. I went to Pittsburgh in 88 and in 97, I came up with the idea for the microfading tester. I told you the story, but I'll tell the recording the story. I was in a conservation conference in Canada, maybe in

Ottawa. They had the exhibition. What do you call them? The trade show exhibits from different vendors. One of the vendors, there was... I'm going to say Ocean Optics, but I might get that wrong. Anyway, it was a vendor who made the miniaturized diode array spectrometers that didn't scan through a spectrum, but they took the snapshot of the spectrum all at once. You could do that very, very quickly. Of course, this was a time when computers were starting to be interfaced with those instruments so that data collection could be very rapid and kind of happened behind the scenes. So, you didn't have to do anything.

The fellow who was at this booth had a fiber optic wand. He was pointing it at the fluorescent lights, and you could see the spectrum of the lights. He was pointing it at his shirt, and you could see the reflectance spectrum of the colors of his shirt. As he's doing that, I'm looking at this thing and I'm thinking, “Well, that's happening awfully fast, and you're doing reflectance spectra in the blink of an eye. Can you track fading? Can you measure changes in that spectrum as they're happening? If I use a bright enough light, can I do a little light aging experiment under that fiber optic wand that can show me what's going to happen in a microscopic spot, can show me what's going to happen to that color from light exposure over long periods?"

So, I went back upstairs to the hotel room with a napkin that must have come from the hotel bar, thinking, and sketched on that. Here's how this would work if I could make a device using the spectrometer thing I just saw. I need a lamp. I needed to do this. I want it non-contact. I want to do the color measurement like this. Then, I went back. When I got back to Pittsburgh, I wrote that down in my lab notebook. That design was the microfading tester that persists to this day. Exactly the thing that I drew is the thing that still gets made now, because it still works. I didn't know if it would work. This was an idea I had. I had no idea that the spectrometer would do it and if I could get a light source that's bright enough to do it. That was in... I want to say 97, but I think it was... That was in 95 that I thought about it. I had to wait two years because all the staff were booked up doing other things. So, I had to wait two years before I had a chance to finish up a project and hire a postdoc or hire another staff scientist to build one of these things for me.

The technology was two years more advanced by the time I got around to needing it. I just went through the catalogs and the technical specs with this chemist that I had hired, and we worked through the arithmetic. Here's what we would need in order to get this. Then, we need a lens that focuses like this, and we need this. We just had this list, the shopping list, of parts we would need. We bought those, and this fellow put them together and turned it on. It worked better than our calculations, but it worked. That pretty much is the machine that I use and that many other people have built is exactly the same as that first version of it. I can say that there weren't random choices in there, that we really thought about it and we really selected things so that they would work this way. While I was surprised, I was kind of caught flat footed because here it is, you built it, you turned it on and it works, now what? I hadn't thought that that would happen so quickly. I thought what would happen is it doesn't work, and we spend the next number of years making it work. So, I hadn't thought about what to do now.

It was pretty much, "Well, okay. Now, what do we do with it? We did some... This is all in that first publication that's in the AIC journal. We did some things like see how good a color measurement you get, do things like see how sensitive... If you're going to look at fading and you're going to measure fading under that high intensity light, how does that compare with fading that you would do under fluorescent lights, a fadeometer, or other aging apparatuses? Do you get the same outcome, the same kind of rates? We looked at is this safe to do. So, can you get an answer at a Delta E level that doesn't leave a blemish behind? Can you measure the temperature of that spot on a black paint so that it's absorbing all of the light and how hot the things get. You're not going to risk in incinerating things, I didn't think. I wasn't sure, but I didn't think so. What might be at risk? The whole notion of, "Yes, you can get an answer, but is it the right answer, and is it safe to point this at art objects in the presentation surfaces of art objects, not just under the rabbit of a frame or on the bottom of a sculpture where light exposure is not really the critical thing?"

It was a couple of years of doing that, a couple of years of kind of proving that it worked and it was safe to use, and it gave you an answer that was worth having. The thing was so just so darn fast, five minutes per color, essentially. You can screen things. The throughput is really high. So, in the course of a day, you can do a dozen or two dozen things, depending on how many colors you have to test. That's pretty decent.

Kelsey Wingel: Pretty good.

Paul Whitmore: [inaudible 01:12:53] you can do in five minutes. One of the more valuable ones. Once I had convinced myself of that, then it was the road test. That was the hold your breath moment. We're going to go to an art museum and actually point this at a work of art.

I had a colleague at the Carnegie Museum in Pittsburgh, Will Reel, who was the Paintings Conservator and the head of the department there. He was one of those conservators who really was eager to innovate and to discover. He was a willing participant in this adventure. I'm trying to remember some of the things that he let us test. They were all accessioned objects. There wasn't any study collection anything in there. These were all actual works of art. So, he had a lot of trust in me. But, he also was really excited at the prospects of being able to interrogate objects in this completely new way. So, he was doing... The first contact, first session, the first microfading session on planet earth, happened at the Carnegie Museum of Art. Will was taking out things like lithographs, one of which had been exhibited and faded and the other, which was pristine-ish.

You could, for the first time, see if the faded one had burned out sensitivity so it was now less sensitive to light exposure. That was Will's idea to check because he had lived through those arguments with curators saying, "You don't have to worry about that because it's already faded. It's not going to do anything else." There's no way of winning those arguments because as I've learned, flip a coin. Half the time you've burned out the instability and the other half of the time, it's just as bad as when it started.

For those lithographs, because the pigment is so kind of chemically pure and uniform, there's no sensitive stuff and light stable stuff mixed together. So, you burn off the sensitive stuff, like a green on a medieval tapestry, you'll lose the yellow and the indigo persists. The lithographic ink was homogeneous in that way. You burn out half of it, the half that remains is just as reactive as the half you lost. You haven't bought anything. It's still light sensitive, very light sensitive even if it's faded a lot.

That was one of those themes that occasionally is revisited by me and by others, when you have the opportunity to test things that share some DNA somehow, either because it's protected under a frame or... I tested a basket that had a woolen yarn, a dyed woolen yarn that was in the weave of the basket. So, there was the exterior of the yarn that had faded and the interior that had not, just being able to answer that question. "Is it more stable now than it was when it was young?" It's kind of a retrospective question asking about what happened in the past, but not really, because what you're really interested in is what's going to happen to the remnant, now, in the future.

In any case, the microfading tester has become one of these... It's become a tool that enables you to answer questions that you probably already had, but you couldn't answer in any other way. In that regard, that's one of the things that researchers will find valuable in that, because it's doing something completely different than the other kind of scientific instruments that you point at art objects. All of the other instruments, almost all of the other instruments, are aimed at answering the question, "What is this stuff?" The microfading tester is trying to answer the question, "What does this stuff do?" It's asking about the chemistry of this stuff, the chemical reactivity of this stuff, not its identity, not its material identity, but its tendency to do chemistry over time. That's a new kind of question to be asking of material objects, and I'm hopeful that it won't be the last kind of instrument that is able to answer those questions.

Anyway, that was... Jeez, we've gotten 10 years into Pittsburgh. That was, I would say, the climax of my career, and the rest has been downhill. To be honest, it was the fulfillment of an ambition... To me, a primarily important ambition. I went to Pittsburgh to prevent the next Rothko accident. Here is the tool that can do exactly that. In fact, it has done exactly that. In my use, and I'm thinking maybe in other people's usage, but I have been able to test pristine, modern works of art before they go on a wall and be able to find things that are like blue wool 2 and blue wool 1, things that would inevitably be faded significantly if you just exhibit them like regular stuff. Blue wool 2 or 3 stuff, you are going to damage those colors. Not that you should not exhibit them for that reason, but at least now you know. Now you can start to think about whether you want to put them under the spotlight, maybe how frequently you might want to rotate things, or whether maybe now's the time to think about replicas, surrogates. But, at least you have your attention focused and you have the needs of the object better defined for you so that if you need to get creative, if you need to get unconventional, here's the object that would benefit from that. If you want to build a $500,000 anoxic case for it, this is an object that would be helped by that. At least you have that assurance or at least that presumption for that object, and it's not just guesswork.

That device, that technology, has kind of taken on a life of its own for better or worse, I'm not quite sure yet. But, it was something that I was trying to do and I did it. I still had another 25 years of a career to go through. So, now what? Where's the second idea? I've been struggling, for 25 years, to come up with a second idea that's any bit as good as that. It whetted my appetite for more of the same interrogation, quick, accessible, efficient interrogation of an object's what, an object's chemistry, that can help focus attention on objects with special needs. That's where the gas sensing started coming in.

The research that Rui Chen has been doing, it started in Pittsburgh. She and another postdoc Muditha Senarath-Yapa, I asked them to find me, to make me, an artifacts sniffer, some way to tell whether an object is doing a particular kind of chemistry by sensing the off gassing compounds of - the off gassing products of those reactions. That's a hard thing to think of doing because so many materials will give off different compounds. There's a publication on the degradation of paper that analyzes the suite of organic compounds that is romantically termed the old book smell. There's like 40, 45 organic compounds that are created when those cellulose and other paper constituents do chemistry to make molecules that are small enough to evaporate. Those are the things that you experience when you open up an old book. That smell is kind of characteristic because it's the same chemistry that's producing them, more or less, unless there was cigar smokers in the house.

It's hard to imagine you're going to make a sensor to sniff for one of those gases, and then you're going to find another material besides paper that gives off those same gases. It doesn't sound efficient. It seems like each material would have to have a particular sensor. I came to believe that if you're doing oxidation chemistries, if it's oxidative degradation, then you're going to get peroxides that come off. Can you get, now, a sensor that detects peroxide emission? So, the research kind of split into two. One was to study materials that oxidized and see if they did give off peroxides. Then, the other one was developing this low cost, accessible, easy to evaluate sensor that can detect peroxides, which you can't buy in any store because they don't exist. The low cost versions of it don't exist.

It was that ambition along the same lines as the microfading tester. It's a nonsampling way of interrogating the chemical stability or the ongoing chemical reactions from a material that is aging, is passing through time, and whether we can harvest that information that somehow teaches us about what's doing those reactions in a big way quickly, because that, then, allows us to focus our attention on it. If you find something that is oxidizing rapidly, then you can start to think about whether cold storage or anoxic conditions are all of the expensive, resource intensive solutions to those problems. They exist, but you can't just use them in a broad way because you don't have enough of them. But, if you can target the things that are at risk and understand them better, that would be an advance, an advantage.

Another thing, another advantage to this being able to assess chemistry as it's happening, is the notion of using these tools to measure the persistence of your conservation treatment, your stabilization treatment. So, if you're going to try to stabilize iron gall ink... If you could measure oxidation from your iron gall ink

containing object, and then do your conservation treatment, whatever it is, your sensors should be able to tell you that that was effective because there's no volatile products of those chemistries. But, you should be able to go back at further times to see is that still the case, or did your treatment kind of lose its potency after some point?

This whole notion of being able to probe the continued effectiveness of your stabilization treatment, in some way, that happened... Before there is obvious signs that aging damage is occurring again, can you probe things before they have become manifested damage to your objects? Again, it's a broader statement of ambition than trying to find a peroxide sensor. Peroxide sensor is an example of that kind of interrogation of an object, chemical stability, that we should have more of. We should have more of those so that it's not just - This is partly to free us from the dependency on accelerated aging tests to tell us what the future holds in store. Everybody who does accelerated aging tests, especially in industries, they always try to do natural aging tests, too, that are slower. They can't wait for them, but they want to check their accelerated aging prediction somehow to see if those were on base or not.

There's precious little of that that we do in conservation because we can't interrogate the objects to see how they're doing, to see how they're doing and how they might have changed how natural aging conditions might have changed them in some subtle way so that we can check them against the accelerated aging predictions of them.

The people have been starting to do some of that with color monitoring, comparing them to micro-fading predictions, and that's a good step, but other things, other stabilization/preservation methods, they should be examined in the same way. We need, rather, we need the tools in order to make those examinations possible so that we don't build everything on this, well, on the untested hope that we're doing something good because, well, if we're not, we should do something different. Spend your time differently, or maybe stop what you're doing and do it differently. There's not a lot of opportunity to do that, except in the sense of letting nature take its course and watching to see what happens.

While yes, there is that, you can always do that, but sometimes when that happens, it's too late to do anything differently. I think this notion of interrogating the chemistry of materials, what are the materials doing, is the next big step that conservation science needs to address is that they need to start thinking about what they need, what we need to be examining, how, and how we can do that in some way that doesn't require a tablespoon-sized sample of an object being harvested the every few years and put into a machine to get an answer.

We need something that's going to get those answers in a noninvasive, non-sampling way and quickly, ideally quickly somehow, so that we can just see how we're doing. Conservation science isn't doing a whole lot to help people know how they're doing in conservation, right?

Kelsey Wingel: Yeah.

Paul Whitmore: Again, because you're identifying materials. That's the aim of so much museum analysis, technical studies. That's not enough. You want to know, how is it different now? How is it different next year? You can't even track the course of the changes, or rather, we can't even do a good job of detecting the changes, much less tracking their course and their rates over time to know whether, well, to pick a for instance, to know whether your iron gall ink stabilization did anything. There's no way to tell.

Kelsey Wingel: This was going to be one of my questions to you, Paul, was what would you like to see the conservation science field address more in the future. Sounds like this is your answer.

Paul Whitmore: Well, that became a driving ambition for me. I think only after doing it for so long, I'm better able to succinctly articulate what that was. Preventing the next Rothko accident was how I did it in my poorly-thought-out way. It was just a visceral utterance of how I wanted the world to change, but in reflection, a lot of the research I was doing, all of the preservation goals that I had were trying to capture that, trying to understand the changes in materials. Then as this notion of... Well, or rather, as my frustration at not having the tools so that I could for myself check to see how things are changing, how they're reacting, what is reacting now? In a collection of objects, which handful of them are doing chemistry at a rate that's troubling? There's no way of telling. You have no idea. Those are the tools that I would like to see the world produce somehow.

Well, I mean, again, it was a poorly articulated placeholder for that kind of ambition. Back in the day when I was just starting out, I was frustrated at not being able to analyze paper, to see how it was degrading without just taking a sample, dissolving it, measuring the molecular weight distribution of the cellulose, and then watching as that molecular weight creeps down as the cellulose gets chopped up. I wanted to be able to do that without taking a postage-stamp-size of paper, so that frustration turned into, "I want to be able to track the degradation or the loss of molecular weight of a polymer in a non-sampling way."

I think there are glimmers of efforts trying to do that. There are people who are trying to do that, whether it's with NMR or things that aren't particularly sensitive. IR is the first choice of many people, but it's not a particularly sensitive one. It's not sensitive enough. I mean, I guess what I'm trying to say is I have... Rather, before the pandemic, I had the faith in this inexorable advance of analytical science to be able to see smaller and smaller quantities of stuff happening, so I mean, that's the natural tendency of analytical instrument development is to try to do better or with smaller samples or more sensitively.

I think eventually, those technologies might catch up with my need to follow the deterioration or the degradation or just call it the aging changes in organic materials in a quick, accurate, reliable way so that you can get some sense of what's going on: What's happening naturally? What's happening in different environments? What's happening differently after you intervene, whether that

intervention is persisting? None of those questions are answerable right now, really, I mean, perhaps with the exception of color stuff, which is one of the few things that you can probe easily remotely without sampling.

Here it is, I don't know how many years later, I'm still winging about the lack of a tool to give me the answer I want without having to work at it, without having to take samples, which right away, there goes 99% of your opportunities once you need a sample. Then if you need a big sample, well, there goes most of that 1% that was left. Then should you be able to get a sample, then you got to find an expensive tool and a smart enough operator to run it and interpret the results. I don't know, to me, that's frustrating, even to me, a conservation scientist, who's supposed to be able to do all of that stuff. I can't do any of it.

Yes, if I had a magic wand and I could change the world of conservation science, first off, I would get enough smart people thinking about it, then the answers, the developments, the innovations will come. If only works of art were attacked by novel coronaviruses, then we would quickly have the screening technologies to see that happening because the world is focused on that.

So much of what we do is just not considered important enough or commercial enough or whatever. It's not important enough to another more sizable community that our needs are left for concert for poor conservation scientists to figure out instead of having the rest of the world helping. I mean, that's not something that's ever going to change. I mean, well, it's not going to change as long as artists keep making artworks with art materials instead of with something that everybody uses.

Kelsey Wingel: Paul, do you think that this kind of focus on technical study and the fact that not as many conservation scientists and teams are doing these material studies, why do you think that is? Do you think it's just because material study is so challenging or it's just for whatever reason, not as interesting to certain people?

Paul Whitmore: Well, it's hard to pinpoint a reason for it. I mean, there's a number of reasons. The materials study doesn't happen in a lot of places. It happens in very few and a shrinking number of them, it seems. I'm going to fall into a trap here, but I'll go ahead and step lightly into it. It's hard answering the question. "What does stuff do?" is harder than answering "What is it?" I'm going to offend all of my technical studies colleagues who are going to hold up this sand-grained size of sample and say, "You're going to tell me it's easy to tell what this is?" Relatively speaking, yeah, kind of.

It's hard and it's slow and progress is made incrementally. It's not like an analytical solution that you plug it in a machine and the machine, the x-ray diffractometer burps out the most probable identity of this material based on its diffraction pattern and then you're done and you're done because A, nobody's going to check you, and B, it doesn't matter whether you got it wrong, so nobody's going to check you. There's that immediate gratification that you've completed the job successfully. Well, you've completed the job successfully and it's what the curators and art historians wanted to know, so there is that built-in constituency of support for that endeavor.

In my experience, which is limited, there's not a lot of curators who care what's going to happen to collections after their show is done. That conservation preservation thinking of future generations kind of thing, there's not a big constituency saying that that's worth understanding, doing, investing in.

That was one of the things that was so special about the Mellon Foundation is I thought that they got it. That's what's so special about the handful of institutions that support the material science laboratories. God bless them, I hope they continue to support them, but I fear for them. I fear for their future because of the things, if budgets get slashed, there's not going to be a whole lot of defense.

It's like when the Smithsonian Lab was about to get plowed under. There wasn't a groundswell support or a groundswell of objection to that plan. That's why, because there's just not a whole lot of people. Everybody will acknowledge that, yeah, in the abstract, yeah, that's a good thing to know, but is that the thing that you want to know? Well, not really. There's that built-in and because it doesn't happen very much, it's hard to get young talent to even know that it exists, it's hard to get young talent to decide "I'm going to take a risk for the small number of job opportunities and I'm going to aim my career there." It's hard to keep that going.

The other strategy that had been hoped for is that someplace like NSF could make more of that happen in university labs, just by seeding grants on a small scale, but just keep those embers alive by letting academic scientists do it. There's pluses and minuses to that. The plus is, or among the pluses, besides that you get academic scientists who are experts in some of these areas thinking about these problems, but they also get their students to see them and to start thinking about them and to discover conservation as a field of scientific endeavor.

The downside is there's no, there's no... How do I want to put it? The academic scientists don't need to be doing that. If the grant runs out, then they'll just shift over to do something else that they can get money for. There's no built-in urgency to do these kinds of problems in academic science, or any kind of problem, honestly. Scientists will go where the money is. That's the nature of the beast. If there's not a lot of money for conservation stuff, as there never will be, it seems, then there won't be a lot of people working on it very continuously. That's what we've suffered from. We get good people who jump into the field and then jump out again. You're just sorry that we couldn't keep them engaged for longer, or forever, but that's what comes with being a niche activity, I guess.

It's kind of all of those things wrapped together. You asked about why it doesn't have a brighter future. It's because there's not the money, there's not the talent, and the people who are doing it now, they're doing something hard and there's not the victories every two months that there is for other kinds of scientific investigation in conservation. That's just the nature of it.

I mean, how do I put it? Again, I've already stepped into the bear trap, so maybe I'll just wallow in it for a while. It's hard to say with any ultimate confidence that you got it right, right? That you got it right in describing the chemistry that things are prone to doing over time, because you'll only be proven right in some

indefinite future. And what? It's easier, I think, it's easier for materials analysts to identify materials and you do an x-ray diffraction, x-ray fluorescence, an IR, and you have a suite of data that is supporting a particular interpretation, and you can say with pretty much confidence after a short period of engagement that you got it right and then you move on. That's the nature of that activity, too.

I guess what I'm trying to say is that I think the materials research is more of a prolonged process rather than an engagement. You can't just do a single experiment. For anything that I was able to discover and can now assert with some degree of confidence, it took me probably the better part of 10 or 15 years of continuous engagement, of continual experiments that were suggestive. That led me to another experiment and that pointed me in another slightly different direction. It's much more Brownian motion to try to get to right answers when you're dealing with the aging chemistries of stuff. It's complicated. I mean, it's probably not a single thing that you're looking at and it's just trying to tease out, pull on the threads, and tease out what might be actually going on. It just takes longer. It takes longer and it takes that kind of continued engagement with things for that long period before you can hope to be successful to the degree you need to be to be useful.

I don't know. I don't know that that's ever going to change. It won't change. The only thing that will change it is having more people doing it. The more people doing it, and then challenging each other by arguing over the end of the elephant that you were looking at and somehow trying to learn. I mean, you're trying to understand your stuff and you can be helped by hearing about other people's stuff that might help you understand yours better.

Kelsey Wingel: Right, it can all build on each other's progress.

Paul Whitmore: Yeah. Well, that's the way science is meant to work and that's the way science usually does work, except in these really narrow ones where there's not a whole lot of people. I mean, the number of people I had to talk paper degradation with is dwindled down to like one. It used to be like four. It's just kind of sad to see. I don't know what to say about it besides I wish it were different and I wish that there were more sponsors of it. I wish that there were more people interested in it and I wish that there could be more of, I don't know, a groundswell of demand for it that I just don't see. But that, I mean, of course everybody wants to be important. I want to be important. I want people to want what I have to sell. Okay, that worked for a while. It's not working so much.

Kelsey Wingel: It seems, Paul, you also have to have that drive, that passion for that particular goal of potentially being able to save something from the effects of its own aging.

Paul Whitmore: Well, it sure helps. It sure helps because, well, there's... Well, it's hard to describe. I mean, this gets very personal and psychological at some level. When I say "science," you're not really just solving an equation. You are trying to come up with an idea. You are trying to put together pieces of information that don't come with a story already and you're trying to make them make sense in a story. That requires, well, creativity and imagination that not everybody has. It's hard

work to a large degree, so you have to want to succeed and you have to want to do it more than anything else, really, because there are easier ways in the world to make a living, honestly.

I'm torn when young people come to me and ask about this as a career, or should they go off and be an academic scientist someplace. Part of me is thinking, "Are you crazy? Of course, want to go do that because you can make a living at that and to do this other thing, who knows what's in store?" But I think to your point, having that fire in the belly, to just know that what you're doing, if you ever succeed, it's going to be important to somebody, that fuels you to do the hard stuff and fuels you through the hard times. It also fuels you to be a passionate advocate when you try to get financial support for it. If you don't feel yourself that it's the most important thing in the world to be doing, then you're not going to convince somebody else of that.

All of that, well, having that fire that burns, well, it makes it more likely that you will succeed, but if you fail, it hurts like a son-of-a-gun. If you're so emotionally invested in success and you don't get there, then it's just crushing. To some extent, I feel some of that now, that I'm unplugging from this field without having finished all of the things that I wanted to do, even some of the things that I thought I might be on the right trail, might take me another 10 years to figure this one out, but if it gets there, it'll be the greatest thing, and now I can't, or rather I'm choosing not to. I'm going to have to get over that. I have to find something else to get passionate about just to make this not the most important thing in the world. It'll be second-most important so I can [inaudible 01:51:43].

But I think you're right. I mean, I don't think anybody does this work for a paycheck. You know, like I say, there are easier ways of getting a paycheck than this, but you really want to, I mean, the people who stay in it, besides the ones that are just amazingly fortunate, like me, the people who stay in it are the ones who want to stay in it, the ones who are flushed out and get back in it, the ones who, like I did, you wait 10 years to try to get into it because you keep on trying, you keep on asking. I guess you have to love it if you're going to be good at it and stick to it.

I guess. I don't have anyone else to compare to, so I'm just speaking for myself, as I always tend to do. That's what's fueled me. Well, that's what's fueled me and having cheerleaders on my sidelines who have helped me keep those fires raging, the ones who are telling me that "Yes, what you're doing is important," and "Yes, this was great, what happened," and, "Yes, the world needs more of this." Those other voices in my head, too, are helping me or had helped me. I think everybody needs those, too.

That's one of the other drawbacks of having such a small field, not a whole lot of shoulders to cry on or cheerleading to have. I mean, even among my scientist colleagues who do technical studies, I can't say that there's a deep appreciation of what materials research is like. I mean, they look on me as a colleague, as a rare scientist in this field who doesn't run at the sight of the graph in a PowerPoint, but not very many of them have done this work and truly appreciate what it's like.

I can fairly say that I have done the technical study stuff, so I think I have a better appreciation for what they do than they have for what I do, but it's neither here nor there. I think the world needs... Well, I used to joke that the scientists doing the technical studies were providing the cover for me to do my stuff so that I would be painted with the same brush – I'm a scientist working on art stuff without getting into the details that I'm not doing anything like what they're doing, but their stuff is going to be in the newspapers, the Pollock fake discovery is going to be in the newspaper, while the paper degradation, the peroxide sensors for using the same paper is not going to be anywhere in The New York Times. And, perhaps, rightfully so.

Kelsey Wingel: Oh, Paul.

Paul Whitmore: Well, I've used up another two hours of your life that you'll never get back.

Kelsey Wingel: No, it's been such a joy, Paul. I feel that we have a little more to talk about, but it's up to you. We've made it through your Carnegie Mellon years. We haven't really talked too much about your time at Yale.

Paul Whitmore: Yeah, well...

Kelsey Wingel: It's completely up to you.

Paul Whitmore: The recording is still on, so I'll be careful.

Kelsey Wingel: I can shut it off if you want to just talk about whether you want to continue or not.

Paul Whitmore: Yeah, could you? You mind doing that?

Kelsey Wingel: Sure.

PART 2 ENDS

PART 3: June 4, 2020

Kelsey Wingel: I just started the recording.

Paul Whitmore: That's okay. Are we on a time limit, by the way?

Kelsey Wingel: No. I don't think so. Does it say anything?

Paul Whitmore: It doesn't. I mean, I've only done Zoom meetings with my sisters and the first couple of them it just... 40 minutes into it, 40 minutes are supposed to be free and then you're supposed to pay for it. And the first few times we did that. After 40 minutes, we got this popup saying, "Hey, you've been upgraded so you don't have to pay for it." And for my sisters, that was great. Wasn't so great for me. Because I don't want to listen to my sisters for that long.

Kelsey Wingel: 40 minutes was enough.

Paul Whitmore: Well, it's not really a conversation so much as an infinite monologue.

Kelsey Wingel: I have sisters, too - I get it.

Paul Whitmore: Then this last time, after 40 minutes, they just said, "You got two minutes left." It didn't say “you get an upgrade” or anything. So my sister who was running the meeting, she had to start another meeting that we all joined up with. So, that might happen to you. If you are expecting to have the Zoom meeting turn into a pumpkin after 40 minutes. It might actually do that.

Kelsey Wingel: We'll see what happens. I think we should be okay.

Paul Whitmore: You'll probably be plenty tired of it. Okay. So let me hurry on to one of my themes. And the theme is something that goes like, here I'm going to become a scientist in conservation. What, in addition to the research that I think is my real contribution to the field, what, in addition to that, can I offer this field? So I'm a scientist. I don't know how good I am at solving problems. But I have... Well, especially after I went to Pittsburgh and I became a researcher who had a research facility and staff at my disposal. Then the question became, or the question I continued asking myself is, what else besides the lab research can I offer the field? And so, some of that... Eventually after I became established, those kinds of contributions kind of came my way. People invited me to participate on review panels or to be a journal editor, things like that.

And all of that was public service stuff. There was nothing my job description that required me to do it. And actually, I'm not sure I got any brownie points at all from doing it. So it was just something that I was able to contribute. But that's just sitting in a room and not falling asleep, most of that stuff. The committee work is committee work and I'm sure, you know what that's like. But it was early in my career when I had my first conservation job at Caltech that I started talking to people, because I wanted their help. I wanted samples of art materials. So one of my mentors, whose name I forgot to mention last time I think, was Zora

Pinney. Zora was the owner, the proprietor of an art material supply store in Los Angeles.

And Zora was also... And I'm not sure what the linkage is here, but she was also a member of the ASTM subcommittee on art materials. I don't know if you know of that, or if you have heard that there is such a thing. But the art materials that are sold now have like these good housekeeping labels. They have, "Conforms to ASTM standards, such and such." That subcommittee is the one that would come up with the standards. The subcommittee was composed mostly of industry representatives, because they didn't want standards that were written in such a way that they couldn't conform. And they also didn't want their competitors to gain an advantage by conforming to a standard that they weren't.

So there was some... It was mostly populated by industry people, who were looking after their company's interests, trying to get the standards and the testing, the testing of their own stuff and their competitor's stuff kind of on a level playing field so that nobody is like putting a thumb on the balance to make their stuff look good, look better. And Zora didn't have a dog in that fight, but she was on the kind of the consumer end of that. So she was representing the consumers, the artist, who would be using this stuff and would be wanting to know that the testing was legitimate, that the testing was sufficient to inform their selection of materials. And so I went to Zora, or Zora was referred me, to get some materials because I was doing my own art material testing.

And Zora was not only very generous with the materials that she gave me, but also with her own testing. She would do independent tests just to compare to what the industry people were doing. And she would also have these connections to art material manufacturers around the world, who if I were to approach them, they would clam up and not divulge any information to me because they don't trust me. Zora could approach them. And they would just see her as on their same team. And so they would be much more forthcoming. And then when I would ask Zora about details, like what's in this pastel stick? She could, using her own judgment, she could give me information that I would've never been able to get from a French manufacturer who doesn't know me from anybody.

So Zora was kind of my entryway into that ASTM activity. And that was something that I could offer to the, "field," in quotation marks, was some help. And be kind of a conservator representative on that subcommittee. Someone who doesn't necessarily want to be involved in how artists make their decisions, but whatever those decisions are, conservators are left with the consequences. And so being able to test things, and know kind of relative stability of things, and making sure that... The tests are easy to do. And you probably have done the same things, you make panels of pastels. And then you put them under banks of lights of various sorts, and then you measure color changes. All of that is routine. It's when you get to the end where you have now this kind of histogram of performance.

Now, how do you rate things with one star, two star, three star or whatever the rating turns out to be? Someone has to put in those boundaries between one rating and the other rating. And that's where it's totally subjective. Or rather, the

data can not help you in making the distinctions. You're hoping to have one hump that's separated from another hump in the performance, so that you can say, "There's the boundary," is the separation between those humps. But if it's like this smoothly varying blob, then where do you draw the line between permanent and impermanent or whatever your ratings are going to be?

And that's where I think the self-interest comes into play, because all of the industries are going to want their stuff to come out rated perfect. And their competitors want it less perfect. And then there's this tug of war. So having kind of impartial outsiders, like me, like Zora, kind of helped those discussions not end up just being food fights. And that was something that I began to feel. Being a scientist who doesn't have a vested interest in the commercial aspect of it, or even the art creation aspect of it, being a more objective outsider, then my opinion is viewed differently. It was a different, maybe more trustworthy opinion.

It was certainly less inherently biased by those other interests. And so that was something that I came to appreciate. And I was asked to do those similar things in evaluations of other kinds of products, or just being kind of the scientific, non-aligned interest to those discussions.

I remember, well I mean... I did kind of small bore kind of contributions that way. The bigger things were when the Library of Congress invited me to evaluate the bookkeeper paper deacidification process. I was in Pittsburgh, the company that invented that was a Pittsburgh company, and they're still there. Preservation Technologies is still in a Pittsburgh suburb. And I was invited to be, since I was doing paper chemistry and I was supposed to know something about paper preservation, I was invited to be on that panel.

And it was the same way. The Library of Congress was going to be the consumer and this Preservation Technologies company was going to be the vendor. And having people who weren't in those camps to evaluate what this technology was about and what the prospects were for taxpayer dollars being well spent. There was a number of outsiders, Sally Buchanan, the Preservation Librarian at Pitt was on that. Mike Domach, who was a Chemical Engineering Professor at CMU, was on that. And I think those two and me were the only outsiders. And then there were Library of Congress people, and then the company people, the Preservation Technologies people. And it got heated.

Kelsey Wingel: Really?

Paul Whitmore: I was doing my own evaluation of things. I wasn't just taking their brochures and transcribing them into my report. And I was doing things like, well, I mean, to add a little bit of detail here. That particular process applies magnesium oxide particles to the paper. In an inert fluid carrier. So there's no solution chemistry. It's just the suspension of particles and you end up just applying them. So the paper ends up with little chunks of magnesium oxide. Now, if you take that paper that's been treated, it's got the acidic paper and the magnesium oxide or whatever form it is, alkaline substance. And you do a pH measurement, put everything into solution, draw the acids out of the paper, dissolve the alkaline stuff. It'll come out

reading neutral or alkaline, because you have just added water to mix up everything. But without that, without that water dissolution step, your paper in its dry state is like an Alka-Seltzer tablet.

You've got the acid there, you've got the alkali there, but they haven't mixed together. You haven't neutralized anything. It's a system that given enough time and moderate humidity, you will end up slowly mixing the acids in the alkali and neutralizing the problem. But the question I had was, how fast does that happen? How long does it take to actually get the benefit of that treatment? And is the acid destruction of the paper going to be faster than your neutralization chemistry from the outside of the paper? So I was doing things like put pH indicator on the paper, put it through this treatment, and have the pH indicator tell me nothing. It's still acidic, because you haven't mixed the alkali with the acidity yet. And then I would put it in a high humidity environment, then overnight there would be enough moisture so that things mixed and the neutralization chemistry happen. So circling back, I would report back to this committee, what I had just done. That I had been curious about the outcome of the treatment, which isn't the preserved paper yet. It has to take some time in order to have this mixing happen. And I practically started fist fights with the chemist from the company, who I'm not sure that he was disagreeing with me, but he was threatened by my having done that, or asked that question and investigated it offline. And he was all set to go. I mean, he just came loaded for bear about what was flawed about my weekend experiment.

It wasn't a research project for heavens sake. I was just curious. And I came out with findings that didn't sound like the company's promotional literature. And they were just pretty upset. They got over that. They got over it. What? They might actually, because of that, they might... I confess, I've been out of touch with what that treatment is like, what the process of that vendor is like now. But I think they added a step to put the books that they treated into a moderately humid chamber to give it a humidity treatment, add some moisture there to promote the neutralization chemistry that they were actually trying to do in the first place.

It's no surprise that they never thanked me. But, if I had scared off the Library of Congress by having done such a thing, saying their treatment didn't actually do anything. I probably would be... I don't know what. It's not like I could have been sued. Well, I could have been sued. You can get sued for anything. But there was a lot of money at stake in that.

Kelsey Wingel: Yeah.

Paul Whitmore: That sale to the Library of Congress. I mean, the libraries around this country were waiting to treat their own stuff. They wanted to see what the Library of Congress did before they jumped on board and did the same thing. Because they didn't have, you don't have the resources to make those kinds of evaluations yourself. You wait for somebody who's got the pockets to do a deep dive and figure things out for you. And then you just parrot that. So if LOC had decided to go another path, like all of the libraries in the country would have shifted over to doing something else. And the company probably would have gone belly up or something.

So there was a lot of stake for them. And I fully appreciate it. One of the reasons why I'm telling the story in such detail is it was... Through the course of this engagement with industrial representatives on ASTM committees, on these kinds of technology evaluation committees. I came to better appreciate what they have at stake. And their perspectives on this. It's not that they're money grubbing whatever. But they have a livelihood that they're trying to... This is one of the products that you're trying to sell and they need... it's helpful to them to have a non-aligned person vet it for them. Because that's a much more convincing testimonial than having their promotional literature saying it's the bee's knees.

So I found myself doing that on scales large and small over the course of my career, as something that I could do to help. It's not so much to help companies make a go of it, but it's to help protect artists and conservators from things that companies might want to foist on them. Maybe with the best of intentions, but without really testing them. It's kind of like, I don't know, hydroxychloroquine or something. Something that's well intended but if the only testimonials are coming from the company scientists, then you have to be skeptical. And somebody someday has to come along who's not a member of that, who doesn't have that vested interest, and it has to do those evaluations. So, that's what I did. And then through the course of those kinds of engagements, I learned to appreciate more the perspective, the point of view, and kind of the interests of the people across the table.

Okay. And then I found myself on this slippery slope, that I was invited by companies to be on their committees or their advisory boards. And kind of join with them to promote their stuff. And I ran away from those invitations simply because, well, the obvious, I couldn't afford to acquire the perception of being a shill for a company. I couldn't become a spokesperson for some company's products. And if I joined the company, even as part of their independent quote unquote, "advisors," I would still have this sneaking suspicion that I would be viewed as in their pocket. And honestly, it's kind of like the CDC now. I don't trust what they tell me anymore, because I don't think that they're thinking independent. I don't think they're independent anymore.

I became kind of keenly aware of that loss of independence, or sorry, the loss of the perception of being an independent evaluator, an independent researcher, scientist, evaluator. Because once you lose it, it's gone forever. You're not going to be able to get that reputation back. So that brings me to the other big thing in my career, which was this microfading tester. And this whole notion of that people speak to... Universities in particular, speak about disseminating research and having it finally have an impact on society besides a published paper that three people in the world will read. And probably not in your lifetime.

And so there's a lot of talk about disseminating research should become a regular part of your business as an academic researcher, as an academic scientist. It shouldn't just be a publication of a paper. And I have lots of things to say about that. And this is the part where I tried to organize it and I couldn't. So I'm just going to start rambling about this particular topic, because there's lots of points that I wanted to mention, but I can't stitch them together and make it sound coherent. So, here it goes. So, the microfading tester I invented in the mid '90s. It

was the first thing I had invented out of nothing. And nothing like it had existed before. And so, as an employee of a university, I was obliged by their intellectual property whatever, whatever the heck I signed. I was obliged to - what do you call it? An invention disclosure for their Technology Transfer Office to decide whether they wanted to pursue a patent, find a company to license it to, or spin off a company and all the rest of that stuff. That ultimately is why disseminating research is of interest or is a big value now in universities. It's not to change society. It's to make revenue for the university. There's no sugarcoating that. Anyway, I filled out my invention disclosure, met with the Technology Transfer Officers. Their first question was, "Well, is there a market for this, besides the 20 museums in the world that you can think of? Can you sell 20 million of these instead of 20? Because if you can't, then we're not going to get a company interested in making them, and we're not going to make money. So we're not interested in buying a patent." And on, and on.

So they pretty much turned to me, said, "Find us a market before we can make this decision." And I wasn't capable of, or interested in doing anything of the sort. I knew the market that I had in mind, museums and libraries, who had $20,000 and a staff person who was skilled enough and interested enough to try to do this. And that you can tick them off. There's a couple of dozen in the world maybe. Or at the time there might've been as many as a couple of dozen. And that was kind of it.

For all the rest of light fastness testing, you can do destructive tests. You can put things in fadeometers and measure things with colorimeters, and you don't have to have this delicate, noninvasive tool to get those answers. Except when you're dealing with artworks, things that you can't deface. But things that are so unique that you can't replicate them with a sample and that you don't want it to deface at the end. Because of those very special features of this device, the marketplace for it was almost nonexistent. And so the university passed on it. And rather passed on the paying for a patent thing.

So I went back to Angelica Rudenstine, my Program Officer at the Mellon Foundation who had paid for all of this research. And I said, "Well, I got this thing. And it could get patented. And I don't want to spend my $5,000 to do that. So does the Foundation want to see this patented?" And Angelica had no idea and nobody at the Foundation had any idea as it turned out, there's was months of flailing, Angelica asking a judge at a dinner one day, what he thought. Wait, what? I guess that's patent law, I guess to ask a judge. And it turns out that as Angelica told me, none of the Mellon Foundation's prior grants up to that moment had ever ended up with such a thing. It had always been a book or something. You copyright it and you're done.

They had never been faced with this technology and the possibility of... Well, the prospect of patenting it, which is the first step to getting it commercialized and selling it to people. And being a nonprofit, of course, the Mellon Foundation is really nervous about anything that looks like it's on the road to commercialization, because the IRS would slam down on them if it looked like they were engaged in a for-profit venture at some point. So the Foundation, through Angelica, the Foundation came down with two things. One is, don't ...

They ended up saying, "We'll pay for the patent, but the university has to hold the patent." So it couldn't be in my name. It has to be in the university's name. And I thought, well, that's a nonstarter, because a patent on it... At its essence, a patent is a license to sue people who do this without your permission.

And that's kind of antithetical to what I was trying to do. I'm trying to disseminate this. I'm trying to promulgate this to the rest of the field. Why would I want to discourage people from doing that? Why would I want to tell The Getty that they have to get permission from Carnegie Mellon University to build one of these things? Because I don't know the chances that they'll get that permission. I can't predict who's going to make that decision and whether it'll be rational or just make everything grind to halt.

So I didn't want to do that. I didn't want to end up with a patent in the university's name. I didn't want to pay $5,000 of my own money that I would never see back again to get a patent in my name. And at that time, the patent seemed like an obstacle to what I was trying to do, which was to get more people to be doing this. That's when I ended up publishing the paper, gave the parts list in that publication. Parts list and how to put it together and make it work to try to grant other people the license to do it themselves without asking permission.

In retrospect, I'm not sure that was a good decision that I made, because I found that if you were ever going to want a company doing this for you, if you don't want to be the one who's going around the world putting this stuff together for people, if you want some other company to be making these like microwave ovens, that they just sell at a big box store, they're not going to do that without patent protection, because the competition is infinite then. Anybody can do this and compete with you. And companies don't want to invest in it, develop it, put in anything, any time or resources to make this happen, if once it's happening, somebody, another company wherever can just start selling knockoffs. And they're at a competitive disadvantage. Unless they have a patent that can protect them, they're not inclined to do that. So my not having a patent kind of put me at a disadvantage then in trying to get a company interested in making these and selling them.

The other thing that put me at a disadvantage is anybody else who wants to come up with a design that's tweaked, they can patent that then, or they might be able to patent that. And then I lose control about who's doing it and how well it's performing. And that's kind of my, I don't know. Over the years, over the recent years, I've had to make my peace with that, because people are starting to do that. They're starting to come up with these variants of the original instrument. Most of which I think are flawed, let's put it gently. And I'm just watching people promote it, and sell it to other folks and kind of do things a little less well than I would have hoped.

I feel kind of like a parent feels when you rear a kid, and then you turn them out into the world, and you hope that you did a good job and that it doesn't come to bad things. And that's where I'm at right now is that this technology... I think most of the people that I helped will do it okay. But the people who are coming along after I've left the scene, and after my peers who were of the same

generation, after they've left the scene, then they're at the mercy of other folks, who may not be doing things as well as I would've hoped.

At some point, people have asked... over the years, I have been asked by other people if I was concerned, if I was bothered by other folks doing this without my permission or my guidance or even without telling me, and my general rule of thumb is, I really don't care as long as they don't mess it up for the rest of us. As long as nobody builds a flame thrower, it calls it a microfading tester. Then somebody burns a hole in something and blames the technology and says the technology is dangerous or something. Or if not that, then come up with somebody who makes a machine that gives a faulty result.

Then somebody reports back that this machine tested out that this color was fine and it turned out to fade in a week. And this machine was unable to catch that. Occasionally things like that crop up, where reports on... through the grape vine things are reported that you know, it, the machine prediction didn't... did not conform to what actually happened. Or one conservation scientist tried to test a painting. A canvas painting while I don't think it was even on the wall, but it was in the lab. Then reported to the world in a conference paper and a publication that because of the movement of the canvas the signal was not steady and it wasn't reliable. So the conclusion was one cannot do this testing on canvas paintings. All of the rest of us who have been doing it on... doing tests on canvas paintings for years were baffled by that until I talked to this scientist, who said, "Well, we did have construction in the building and in the room next door and heavy equipment and jackhammers and whatever else was going on, that kind of compromised the stability of whatever the furniture was that they were situated on." So it's things like that, that really... they kind of bother me in a low level way. I think that... I don't think the world is shaken much by these one off accounts of bad outcomes. Especially if it's not a person whose reputation... whose opinions you would trust implicitly.

If this is coming from like the farthest reaches of the field. Then you kind of shrug your shoulders and say, "Yeah, whatever. I haven't seen anything like that. Nobody I know has ever seen anything like that and life goes on." So it doesn't have much of an impact, but as time goes on, there's fewer and fewer people who have been doing it for 20 years. So pretty much everybody is going to seem as experienced as everybody else, because everybody's inexperienced. That's kind of... it troubles me, but honestly I can't do anything about it. So I'm just going to have to let nature take its course. If this turns into something that was good at one time. It could end up being like, I don't know, I'm not a conservator. So I'm going to struggle for an analogy. But maybe like wax resin lining. You know? Where it's not good for everything, and you have to kind of know what you're doing to have a good outcome and eventually you feel like there's alternatives so that's not a thing to be doing anymore. Opinions kind of drift away from that as an option. I don't know if that's true or not. I'm just making it up. I'm trying to come up with an example of something that... a technology that it was new once and it was offered to the field. It seemed like a great advance. Then it's just the field moved past it. Moved away from it. Regardless of whether it was a good thing or a bad thing. It's just... life has shifted. If this turns into that, well honestly... here's the

griping of a retired guy. A newly retired guy. You know, I can't do anything about it and I gave it my best shot. So that's that.

So back to my best shot, I started talking about this patent thing as part of this dissemination thing. I didn't start doing that. I started talking about industry people who have kind of vested interests that you have to be aware of and will probably color your judgment of what they're saying to you and then this dissemination of research thing. Which is supposed to... which is promoted as a good thing to do.

But, the question I ended up with was, how much of that dissemination can you do before you start looking like a promoter of the technology who has a vested interest in it? You know? How long can it be? That was something that... I guess I was sensitized by it because of my experience working alongside these industrial guys and having industries invite me to get in bed with them more closely. So when this whole idea of this microfading thing . . . Do I want to start a company? Do I want to spin off a company that makes microfading testers? Even if it's not going to be a company that makes any money. Do I want to kind of control the technology and disseminate it more seriously than my university job would allow? I just said, "No, I have to keep all of them at arms length." I want to keep all of that at arms length. I don't ever want to be – I said at the time, and I think I still feel that way - I don't want ever want to slip into feeling like I'm so... I want this to succeed, so that I can no longer fairly judge what's happened. Right? I don't want to want it so badly that I don't pay attention to somebody who has a bad experience or somebody who discovered something that wasn't what I expected. I still want to have the option of saying, "I thought it was a good thing, but it turns out it's not as good as I thought." I still wanted to have that ability to be a fair judge. An open-minded fair judge of this as a technology.

Once you start putting your name on it, then you kind of lose that ability. Or you lose... you lose that perception of being an unbiased arbiter of things. So I never wanted to have my name attached to it. I never wanted to have a company. I never wanted to be affiliated with a company who was going to make it, because I wanted to keep being the researcher. A researcher who could one day say, "Well, I found this bad thing about it." And not have to worry about whether I'm going to make a company mad at me. So there's that. So, I mean, that's one of my points. Is this disseminating of research kind of puts you in that position of, you're at the front end of this commercialization pathway. That's what dissemination is. Really it can end you up in being this commercial provider.

Once you end up on that path, then you kind of... certainly once you end up on that path and you sink some of your money into continuing the development of it. Or commercializing it or publicizing it. Then you're in it and you're not... you're no longer in a position to judge anything. You're a salesperson for it and that's kind of something that is... that you need to be aware of. I mean, there are... the colleagues in my field. The few colleagues, but a handful of them, who have discovered something, invented something, developed something and wanted to get it into conservator's hands, because it was a better mouse trap - they end up looking like the shills for that mouse trap. I mean, Gustav Berger is not ever going to be remembered as an impartial observer of BEVA.

Right? Although at one point when he was still doing research and he and Marion Mecklenburg were screaming at each other at conference meetings, then there was some feeling that he was still trying to figure things out. Gustav was still trying to figure things out. But eventually when BEVA is on the shelf for sale, then he's not going to be looking for shortcomings and he's not going to look kindly on people who find them. That's kind of a slippery slope, that I kind of dragged myself off of it to take it, but you pay the price. Then your dissemination efforts are kind of...they're half-hearted. I'll only disseminate things if I personally am doing it off the books. You know? Without publicity. So it doesn't look like that's the business I'm in now. That's kind of limited, but it should be a choice.

It should be something that you consciously decide whether you're going to do it seriously, for real, in a commercial way or not. For I mean... it is particularly pointed for researchers, for scientists. Because we're... just like the CDC. People depend on the scientists being the objective arbiters of things. You don't want colored judgements. Then you're less... you're not a good scientist if that's what you're providing people. So we're utterly dependent on this perception of being a... an objective judge of fact. I don't know this dissemination thing, especially if it's disseminating things that... well, what? I mean, I could I put another name on it? I mean, I could find another example of technology? What the hell? You're from Winterthur. I can talk about this with you. When Richard Wolbers came out with his gel cleaning stuff, and he started hosting... holding workshops that teach people how to do it, I thought it was premature. I thought he should have been just testing that still. Because there were still unanswered questions about things. Like clearing gel residues and all the rest of that stuff. Just the basics of how what he has been doing actually works and will it not work somehow? What are the consequences of that intervention? That aren't... he was just telling people that this stuff cleans and it wasn't... he hadn't yet investigated what else it might do besides that. So I thought it was premature for him to be hosting workshops and telling people, "This is something that you should be doing all over the world." Then there's the blow back. Then there are conferences where people get up and report that we did this and it dissolved the paint. Or there were just some horrific, horrific consequences of doing what he said to do.

At the same time, Richard was like working with the Getty to study the gel clearing issue. Like years after he had been giving the workshops, telling people to do it. So it was... I was going to some of my painting conservator friends, and kind of on the sly saying, asking them, "Do you see what I'm seeing here? Is this really ready for prime time in your view?" Many of my friends were a bit skeptical in the same way. Saying that there's so many unanswered questions about this stuff. I'm not sure it's a good idea to be teaching people to do it. Or advising people to do it, not just teaching them how to, but telling them to.

Okay. Once you've done that. Once you've gotten on the soap box telling the world to do something. It's hard to like walk that back. It's certainly hard for you to do research that comes out saying it's a good thing without looking like you wrote the conclusion first and then you did the experiment. Is that really what happened? Or are you telling me the whole story of what you did? So there's just the natural suspicion that clouds any attempt at doing research from that point on.

That's what I was desperate to avoid because that's all I really do. That's all I have to offer is the research stuff and I can't possibly afford to have people be suspicious of my motives.

So anyway, that was kind of one of the points that I wanted to make. Because it seems like the last... like much of my time at Yale has been spent trying to do this microfading …I don't want to say promotion. Dissemination we'll call it. I wasn't trying to encourage people to buy it. I was showing people how it works when it works and if you want to buy it, that's great. I don't have any financial interest in that. Which is maybe great, maybe not. I don't know. But there's an upside to this dissemination thing too and I want to get to that as well.

So here I am, working diligently in the lab. Come up with this fading tester idea. Make it work. Tell people how to do it. Show people how to do it. Help people to do it. And the farther along in that... further along in that activity, the closer and closer I get to the trenches, where it's actually used. The results of the tests are supposed to inform decision making. I am there helping with the decision making. Or at least hearing what the decision making is like. I would never have experienced any of that if I had just done the lab work and published the paper and called that it. That's really what I have learned from my time at Yale. Was being much more heavily engaged in kind of the after sale part of this deal. Where people are actually doing it. Well, as I experienced … I hadn't been at Yale for two weeks and I got a phone call. I get phone calls from like Marie-France [Lemay] in the library asking me to come do microfading.

Teresa [Fairbanks Harris] wants me to do microfading. It seems like everybody wants me to do microfading because I guess that's the trick that they knew I could do. So I do the microfading. I write the reports. Send the reports out. It's like one, two, three, four, five phone rings after I've sent the report. Five seconds later, the phone rings, "Thanks for the report. What do I do now? What is the report telling me? You tell me it's a blue wool 2. What the heck does that... how do I interpret that? How do I use that result to determine whether I should approve this loan? Or the loan for this extra venue or all of the rest of it. Like, how do I use that information now to help me make the decision?"

That was my baptism in the actual rest of the story. It's not an abstraction now. I say that information is a good thing for making decisions. Now help me make a decision and it's not so easy. Well, I ended up going... getting deeper into the weeds of exhibition guidelines, lighting guidelines, and whose doing it. Who's doing such a thing? How are other people trying to use this information to make decisions?

Well, I got as far as figuring out what... let's see, how do I put this? The easiest thing to say is that the fading tester can identify the things that you're about to make a mistake on. The things that are light sensitive, to a degree that you aren't expecting. If you put them on a wall and treat it like a normal thing, you're going to wreck it. So it will help you avoid those circumstances. When you are going... about to exhibit something. You're about to exhibit a painting on canvas like a painting on canvas - 350 lux for forever. You're about to burn out a sensitive color on it. I can tell you that it's there and you probably should start thinking of

this as a watercolor painting instead of an oil. You know? Instead of an easel painting, because it's got that kind of light sensitivity built into it. So that's the easy thing is to come up with the extraordinarily sensitive things, just to recalibrate your expectations of what it can stand.

It's kind of... its robustness. You just need to like dial that back on the basis of those findings. But if you're trying to do things that... we're hanging them up on a wall … if you're already planning on treating things like watercolor paintings, there are more and less stable watercolor paintings. So okay, you're going to put them in a print gallery at 50 lux now and you're not going to put them up permanently. But can you put them up for three months? Can you loan it to somebody for their nine months show? Can you do this, that, and the other thing to it? What are the... and it's all cumulative damage. It's cumulative change. So there's no, "If you only do this much, nothing will change." There's no threshold beyond which bad outcomes are likely. No. It's like any light that you deliver, is going to effect a little bit of change.

It's got so much useful life and you're going to use up some of that with every exhibition forever. Now how do you decide whether to do a little or a lot? Then it's turns... you risk turning it back into the conservator/curator tug of war. "I want to exhibit it." "No, I don't want you to exhibit it." That's where we started. So the question on the table then was, how can this microfading judgment of blue wool rankings, help make those kinds of decisions? That's when I learned about kind of the prescriptive policies that are built around, Oh, I don't know. Like CCI stuff. Or the VNA, or someplace that says you have a light budget. You have, every year, you're allowed to have so many lux hours and once you've exceeded that, put it back in the dark.

That's a very restrictive one. That's the one that you're... I don't want to say doomed, but you are going to be in arguments all the time with that. Because if six months is okay, then seven months isn't that bad, right? That's a hard argument to win, right? So the other alternative is not to have these prescriptive light budgets, but to have these rules of thumb. So blue wool 3 things can be on the wall 50% of their lives on average. Blue wool 2 on the wall 20% of their lives on average. These are percentages that fall out. Once you decide things like how long do you want this object to last? If it's 500 years… I think if it's 500 years, then those are the percentages that come out. I don't know, I worked out the math once, somebody else can do it the next time. But you make some assumptions about how long you want the lifetime to be.

Then you say... then you can count by calculating okay, so that means that I have this many lux hours, what does it turn out to be? It's like... no, I'm sorry. I didn't say... I said 500 years, I meant 50 years. So we're talking about blue wool 2. You want it the last 50 years with no perceptible change, but it's only got 10 years of exhibition life in it. That means if you want that 10 years to spread out over 50, you can only have it on the wall for 20% of the time on average. That's where the math comes out. But that means if it's 20% on average that means you can use up those 10 years. You can use up nine of those 10 years, and then pinky swear that you're not going to put it on the wall for the last... for the 10th year for the next, [inaudible] right?

You're trying to relieve the lighting police from the job of measuring and monitoring and putting the stopwatch on things. Leave it up to curators judgments to say, "Well, I can't loan this one because it's been on the wall for this long already. I don't want to not be able to use it myself or to loan it to anybody else for the next 30 years, by giving it to you now or something." Just give some sense of proportion. If blue wool 2 then is 20% of the time and blue wool 3 is half the time, then that gives you that kind of vague target then. When you're putting things, more robust things, on the wall more frequently for longer and less robust things, less so.

Anyway, long story short. From my being at Yale and doing these tests for people and providing these results for people, then I was also asked by those same people to help them use those results. To make those decisions about loans and exhibitions and what? Rotations schedules and the art gallery was particularly engaged in that whole evolution of how to do this better. Mainly through the energies of Ruth Barnes. Who was a good advocate for this. I think she was the one who carried the day with Larry. To come up with this policy that would say microfade things and then make your decisions according to some guideline that they had never had before. A loose guideline like I described. When you come time to... this was Larry's vision, I think. As you are testing things for rotation into exhibits, like Ruth does all the time - I mean, she has to rotate things frequently because she's got these textiles - as you're doing the testing, you will get a better sense for the things that don't need to be rotated so frequently. So you can concentrate your energies on the things that you know need to get rotated. So over time, you will have this better sense of your collection. So that some things need... can be on the wall for longer and then other things need to get turned. Because they can't stay on prolonged exposures. Also, you'll end up testing fewer and fewer things, as you are building this database of results. You end up testing only the things that haven't been tested before, which hopefully will be fewer. So anyway, it was just this kind of sensible strategy about how to do this, that the art gallery was all over. It was Ruth and Lisa Hodermarsky and Carol Snow and Jeffrey Yoshimine. They were all over this... okay, now we have this tool now.

That can help us. So, that this... we're not like sagging under the burden of these exhibition rotations. We can start thinking about how we can possibly make our work load a little bit lighter by doing this. You know? Adding this information sensibly. I have no idea if that's going to be sustained at all. Because I think some of that messaging got... might've gotten garbled a little bit. As I was getting requests from curators to test like the 200 things that were going to go into the next show. 90% of which were going to be like sumi ink on rice or on kozo paper or something that's not going to fade. So I mean, testing things that are... you have reasons to be suspicious about. Let's start to focus a little bit on that. So anyway, I mean it's a learning process.

I can't possibly hope to teach people to do that. But I had a good chance to try to teach the folks at Yale or the Yale Art Gallery how to do that while I'm learning it myself. While I'm trying to do this. Trying to do some basic light management and things. Like record lighting levels and monitor color changes in real time. Things that are... people know how to do, but they're hard to do. Especially hard

to do well. So I got to do some of that. But I also got to do this, you know? How does microfading fit into this scheme?

I also appreciate it better. The microfading is just this predictor of future performance and I don't know yet. No one really knows yet whether that prediction is going to be all that good. The things that you... that's the cruel irony of it. If you do your job well, you're not changing the colors enough for you to check how you're fading them. You're trying to avoid that happening, at least in your career. So it's kind of a hard thing to really check.

But the opportunities that people have had to check them, have been okay. I haven't yet heard of things that were supposed to be stable and turned out not to be and vice versa. So I mean, that's what I'm really fearful of is getting the numb... if it's a binary thing, safe or not, and you can't even get that right, then I'm worried about the future. But so far I haven't heard, with perhaps the one exception, I haven't heard things that were egregiously wrong. The one exception actually is interesting. Not maybe... not to you, but to me. It was the test of like a grass. It was a plant material that was going to be exhibited, or it wasn't going to be exhibited. It was in this collection, this study collection in a natural history museum. This is Vincent Beltran at the Getty, he tested it and … and then he tested it with microfading and then he tested it, like the whole thing under a bank of lights. So he did like the macro testing and the answers were like in opposite directions. The microfading said it was stable-ish. The macrofading said it was not anywhere close to stable. I thought, "Well, okay. I don't know what you did wrong, but I'm hoping you did something wrong." Then I had an undergraduate student at Yale, who was working with the collection manager at the Peabody. What collection is it? Whatever you call plant collections.

Well there's probably a more technical name for plant stuffs, than what I'm thinking of. It's like leaves and flowers from plants that are... I say there's a fancier name for it, but they just like pressed them between newspapers like I did when I was a kid, I'm not sure how fancy they really... deserve a fancy name. So anyway, she, as part of this undergraduate project … you know, they were interested at the Peabody in putting things on exhibit. They wanted to know the obvious thing, are we going to wreck this stuff by putting it on exhibit? Then we have to go out in the marsh and get another one. So she did this testing of all sorts of things. But one of the things she did was some kind of leaf. She also did the macrofading under my fluorescent lights. Sure enough, same thing. Microfading says it's reasonably good and the macrofading says it's not good. No, it was the other way around. Micro-fading said it was going to fade. And the macro-fading said, "no, it's not," something like that. But it was like diametrically opposed outcomes. And I said, "Okay, well, I don't think I did anything wrong." And then I started reading up. Obviously, I try to avoid doing reading until I need to. But one of the things I discovered in reading was, and I think the solution to this mystery is that these biological things are complicated. They're not just pigments in a medium.

They are these complicated structures. They have all sorts of built in chemical features to protect the plant so that the plant doesn't fade when it's out in the sun. It has these antioxidant reservoirs that kick in when it gets too much light

exposure so that the chlorophyll bodies can't process it and it risks doing damage. Then these antioxidants kind of kick in to prevent that damage from happening. So, when you have like small scale light exposures, where there's only so much of those stabilizing features around and you can exhaust them, then you're going to end up looking like you're doing damage.

When on the large scale things at lower exposures, lower intensities, then those biological features, which must persist after the plant has been harvested, they're still there. And so they can still do this kind of chemical stabilization of it. At least that's... I'm trying to talk as if I know what I'm talking about. That's my hypothesis about why I'm getting different outcomes, depending on whether the light is very localized or is broadly applied. But this whole notion that the biology of the specimen is so much more complicated and chemically complex than any art material I have ever encountered or am likely to encounter, that was a revelation to me.

So once you start talking about other kinds of things, and this could be plant or animal. It could be butterfly wings on a Damien Hirst painting or something. It could be anything that has these kinds of biological parts to it. Those parts aren't simple. They're not chemically or physically simple. And that complexity can end up producing outcomes that are totally unexpected and baffling because you're not prepared for that kind of chemical mechanism to start churning.

I'm not sure how I got onto that track now. I think I got on that track by starting off with my... I don't want to be in a position where I can't, like, question what's going on with this technology. That I'm so invested in its success that I have to not ask more questions about it or ignore the reports of people who have questions about it. I mean, I could do that, but I don't want to do that. I don't want to turn into a salesman for that rather than a researcher that I'm... Well, that's the only skin I'm comfortable wearing, I think.

I think those were... well, that was the story that I wanted to tell most, I think, was this whole... It has nothing to do with the lab research. And that's the reason why I wanted to talk about it because I don't think I had covered any of that. And honestly, I'm not sure I did any of that before I came to Yale. I was most engaged in the outside of lab stuff after I came to Yale because the lab stuff had been done. That had already been done 20 years ago. And the rest of it was dissemination, but dissemination with all of the complexities and the downside that nobody tells you about.

Dissemination sounds so easy. Just sounds like you're just going to like say it louder, and it's nothing like that. No, I guess that's what I was... that was the point I was making, was that the dissemination... well, I mean, there was another point I made was the dissemination, in addition to having this, becoming this gravitational pull towards a non-scientist role, it also afforded me the opportunity to get down in the trenches and learn more about what other people do. How I can help other people do another part of their jobs that I really wasn't appreciating when they asked me, "Okay, what do I do with the answers you just gave me?" I had no idea really what they meant and how I could help that. I was able to learn some of that too, from trying to do this dissemination thing.

The other downside to all of this, which I won't belabor, but as much as universities promote the value of research dissemination, I already said what they really mean is revenue generation. But apart from that, apart from spinning off another Google, there's no recognition or there's no accounting for that. You don't get any... I didn't feel like I was getting any brownie points from this. This was stuff that was tolerated.

It may have been encouraged, but not at the expense of the other stuff, not at the expense of the lab research stuff, the getting grants, writing papers, all of that was supposed to happen, too. And that's the stuff that counted. So if I'm spending time doing all of this other stuff where I'm learning things and I'm helping people, and I'm able to articulate better how to do this, how to use a technology that was born in my head, meanwhile, I'm not doing the grant writing and the paper stuff, which is the only metric of success in the academic community.

And so I'm taking hits. I'm looking like I'm not active, I guess what I'm trying to say, I look like I'm not active when I'm doing what people say they value, but they don't really, and that's not right. There has to be some middle ground. I mean, if the only research dissemination that is valued for real is the one that makes billions of dollars, why would anybody bother doing that? Why not just do the grant writing and the publication writing because that's what will be viewed as success anyway?

So, anyway, there's kind of a mismatch here between the professed value on research dissemination and the real value of research dissemination, but that comes at a cost in other ways, and it shouldn't come at a cost. It should be valued in itself as being something else that is an activity that is to be promoted. And especially if you can get it paid for. And I just never felt like that was true. I didn't feel like that was happening. In fact, I know that wasn't happening because I was... my scorecard never showed that that other stuff, the dissemination stuff that I was so heavily engaged in. That wasn't even counted, and that wasn't right.

I mean, that needs to change, that recognition that if you really want to have this... Well, and I think Angelica Rudenstine was the one who kept on drumming it in my head that the Mellon Foundation wasn't... they weren't investing in me to write papers. They were investing in me to help museum conservation, to change museum conservation. And so I felt like for them, for the foundation, my dissemination efforts, as meager as they might've been, they counted. They always counted because I was actually having an impact, which was what they were really paying for.

But it's when your actual employer or your supervisor has a different opinion of it, then that's when it gets difficult. And honestly, as the Mellon Foundation has kind of withdrawn from conservation science, conservation research, I don't feel like there is that support for this dissemination stuff if it's not going to make money. Only if it makes money, is it going to be supported. And you kind of... I don't know. I don't know. I'm sensing more and more that innovation is coming from Europe because the European community grants, they have that built in. In fact, that's the point of those is to have products to sell at the end of them.

Sometimes they don't do the research that they need to do. They just skip ahead to the products, but clearly they put the value on that improving or providing innovation to the field. That's what they're paying the money for. They're not paying money for scientists to write papers, as fun as that is, as valuable as that might be. And I'm sure yours is among the valued ones, but you kind of get it, right?

Kelsey Wingel: Yeah.

Paul Whitmore: I mean, you feel that. You're writing papers that probably the next generation of students might pick up the thread.

Kelsey Wingel: Yeah.

Paul Whitmore: It's not like you're writing to a big community of like-minded people who are all after the same thing. You're not trying to come up with COVID virus vaccines that the rest of the world is equally interested in what you are discovering. This is kind of … you're contributing what you can. And the next step may be taken by a colleague of yours, or it may happen in the next generation or two generations. Who knows? You don't know, but it's harder to gauge the impact of those kinds of contributions. You have faith that you are contributing, you have faith that people will take advantage of what you have learned, but it's harder to point to something that has changed as a result of it.

That's true for 95% of what I've been able to contribute. It's papers written. And, yes, I've lived long enough to hear from the students who have picked up the threads of things that I want. And then they ask me questions about experiments I did 30 years ago, and I try to fake it like I remember. And this notion of, okay, so this is like tying up to my last point, was the sponsorship for conservation science. Who's going to sponsor it to do what? And that's kind of the unanswered question, or maybe an answer that changes over time.

But the Mellon Foundation was the perfect sponsor because they were aimed at what I was hoping to do, what I was hoping to achieve, get smart enough or innovate and have the conservation profession take advantage of those advances and work hard so that they do. But the foundation decided, like 15 years ago, they decided that they were going to try to find other sponsors to pick up the tab or help share the tab. And then that's when this engagement with the National Science Foundation began. Trying to get... well, I mean, that was kind of the chicken and egg problem.

Conservation science is kind of crippled because we're either very focused in our interests, and so we learn a lot about a very narrow subject, or we're generalists, and we are amateurs dabbling in things that really require professional intensive engagement. And we just don't have the time or the opportunity or the background to do such things. So there is a kind of an accepted opinion that conservation science could do better by engaging academic scientists or industrial scientists, the true professionals.

Why should we try to play at being paint chemists, if can go to an industry who has paint chemists and they've been doing that and nothing but that for generations? Why not let them help us do the research? Or what? Or you're trying to make a better mouse trap, and so you go to an academic scientist who is working on a related thing, and you try to do the course correction so that they can do work that is better aligned with conservation needs so that when they're successful, then the conservation field can take advantage of it.

NSF would be a great way of catalyzing academic scientists engagement with those problems, because that's what academic scientists need. They need the grants and the publications, and they're unlikely to be interested in the dissemination stuff because, well, they're not likely to be so engaged in conservation to know how to do that or to be trusted to do that, and that's where the conservation scientists can be those liaisons, the ones who can like take it the rest of the way towards conservation practice.

Anyway, the NSF thing kind of came and went. They got interested for two years, three years, gave grants to scientists and museum scientists and academic scientists working as partners to do things. And then that special program stopped. The people who had gotten those grants in the first round, they're in a better position to go back to NSF and try to get more grants because they have a track record for having gotten one. The built-in problem with that program was they wanted 100 applications, but they required a partnership with museum scientists or conservators or somebody in a museum. It had to be a museum-university partnership.

There's not 100 museums in the country. There's not 100 museum partners on that side. So that kind of put the lid on how many applications they could get. So it was kind of a self-fulfilling prophecy when they say they need that. And then they only get two dozen applications. That's as many as you're ever going to get, if that's what you're going to require. Now, it's not that academic scientists couldn't do their part of this by themselves. They didn't need... Research that is of value to the conservation field can be done by academics without having museum partners to a point.

So, if you really do need the deep dive, you need the invention, the technology, discovery and evaluations, the academics can do that. They can't do the rest of it. They can't take it to the conservation bench, but they can do the research part. So, anyway, I thought it was peculiar that NSF would require those kinds of partnerships because they're doomed to have only a handful of applicants. And then when they got only a handful of applicants, then they turn around and say, "Well, so I guess there wasn't really a constituency for having a regular program at NSF on conservation stuff. So we're not going to have one."

I don't know that that's true at all. In fact, I'm sure it's not true. I'm sure there's plenty of academics who would be looking at aspects of conservation saying, "Hey, I could do that." And then just sort of doing the course correction of their... or just the grantsmanship statement of what their research is good for and make it aimed at a conservation objective. Anyway, that's kind of where we're left in this country is Mellon Foundation is pulling out. I'm kind of the poster child for that.

And there's no other sponsor that's ready to step in except NSF for those applicants who got the NSF grants in the first round.

NEH is trying to do that now. They're trying to offer some pretty hefty science research grants, but those in my long view of the field, those come and go. Because NEH, well, what? They're not in the academic science business. So it's hard for them to sustain that. And it's hard for them to make good judgments about science projects that are proposed to them. They just don't have that background, that scale. So, anyway, it wouldn't be surprising to me to have that NEH program kind of cyclical. It comes around and then it disappears, comes around again.

NEA is never going to have the resources it seems to fund kind of science work or the interest or the staff to evaluate science projects. That's just not in their DNA. So, but that notion of, who's going to support it? Well, right now it's the institutions that are supporting. The museums are having to support the science labs. If they get a research grant, if the scientists are able to get a research grant with other monies, that's a bonus, but that's not going to keep people on staff.

And that's the tenuous tightrope that this profession keeps on holding out for people, is that, yeah, it's a great career if you can stay in it. And once you fall off of that tight rope, then you're forced to leave the field because it's not like you could just go to another university and keep on doing it. So, that's kind of the... I don't know, that's the depressing semi-colon that I feel we're approaching here. Me personally, I definitely felt that I've definitely hit that punctuation mark. It wasn't a semi-colon. It was more like a period, where the future of my activity is in question. I'm not sure that there's the financial support for it or the interest in pursuing it.

I think, that's to be expected. If what you're producing is going to be helpful to the next generation, then it's a hard sell. It's hard to get someone to invest in it for that kind of long-term outcome. And, I don't know, another enlightened philanthropy is going to have to step in. Honestly, I don't know. I don't know what the future holds for the financial support, the sustaining of conservation science labs in this country. Because, well, it was dire enough to begin with, but once you get museums, all these cultural institutions just hitting the skids now … they'll be lucky to keep a skeleton crew of conservators in the house. And now who's going to pay for research? Who's going to pay for science research? Nobody.

Kelsey Wingel: It’s scary, it’s scary for sure.

Paul Whitmore: So, I don't know. It's hard to be optimistic about the future for this particular... for the science research part of this field, just because it's such a big investment for long-term payoffs if you're successful. And there's no guarantee of that success. And there's so many other ways of investing in conservation things that it's hard to make the argument that the science research is really the best place to put it. And I can't argue with that. So it's going to be a rough patch for all of us. All of you, who are still trying to be employed and to keep these institutions going and to keep arguing for an essential role that you play in those institutions. I don't

know. It's almost like turning the clock back. Conservators, it seems like this is where you all started, isn't it? Trying to convince museums that you were worth having on staff, not just contractors to use and then wish you goodbye until they need you again. That'll be a whole new world.

And I guess, I don't know. I should pinch myself and reflect on how fortunate I have been that I was able to ride this gravy train for as long as I did. And to do the things that I felt like I was good at, and that I made a difference. That I was able to contribute something to the world. That's what you hope for in a career. And I got lucky enough to do it, so it could have turned out differently.

Kelsey Wingel: You have absolutely done that, Paul. You've absolutely done that. I have some questions for you, some other ones.

Paul Whitmore: Sure, sure.

Kelsey Wingel: If you're willing to answer them, and it's all right if you're not.

Paul Whitmore: If the statute of limitations has run out, I'm fine with it.

Kelsey Wingel: Well, I guess, more pertaining to what you've been talking about this session. I'm not aware of many or any PhD in conservation science programs in the US. Do you think that's something that could and should be pursued as, I don't know, as a way of generating this new generation of researchers who could then kind of go out and try to establish their research labs across the country?

Paul Whitmore: Well, this is the problem that Angelica struggled with when she first engaged with conservation science. When Angelica came to the Mellon Foundation, her first thing was photography conservation, and that's where she focused her energies and her investments. And then when she felt like she had done the first chapter of that, then she turned to conservation science, and it turned into... it kind of distilled down to these kinds of questions, like yours. Do we provide this supply of new talent?

If you do that, where are they going to go? So we need to create jobs for them. We need to create opportunities. So like postdocs, so they can gain experience and be available on short notice. And you need to have jobs for them to go into. And so you can't just do one, right? You can't just provide the supply and you can't just provide the jobs and then hope that you get lucky and find somebody who's just hearing about conservation to walk into those.

So, Angelica was trying to do both. She was trying to provide... She didn't do the PhDs, but she did the postdocs in various museums around the country. And she was able to create or endow and invest in the science labs at various museums to try to make positions for those postdocs to go into. But, yeah, it isn't very long before you have filled all of the slots and then what? And then you still have the spigot running with creating more talent to come into the field to see that there's no jobs in it. And then they drift away.

Kelsey Wingel: Yeah.

Paul Whitmore: That's kind of built into such a small profession that has such small turnover. Somebody has to die to make a spot it seems, and we're not dying fast enough. Okay, I'm going to be honest. I was not a big fan of the postdocs. I didn't want to host postdocs myself for the very reason that I didn't know where they would go. I didn't see the job opportunities for them. So I didn't want to be exploiting them for their talents at that critical point in their careers, and then they're no better off. I am not in a position to help them get a permanent job if it's not in conservation, if there's not any in conservation. I can't help them get jobs at industries or academic institutions.

I felt like that was exploitative to just... Yeah, I see what they bring. I see what they offer the field. And it's great to have young talent and imagination and new ideas and new perspectives, but what are we offering in exchange? And there just wasn't enough to convince me. It's at this point when Angelica took off her glasses and rubbed the bridge of her nose and shook her head and sighed because she desperately needed people to host the postdocs that she wanted to have happen.

And so when I saw that, and then I said, "Oh, sure, I'll take a postdoc. I'll overcome my misgivings and I'll do it too." But it was... so how many postdocs did I host? One, two, three, four, maybe half a dozen postdocs came through my lab. I'd say about half of them ended up staying in the field. The other half went off to academic institutions, to companies selling microscopes or something. And they did other things with their lives.

I have the same feeling maybe even more so for PhDs in conservation science. The problem with that is what good is that degree when you go competing for a job outside of conservation? None. In fact, it's a disadvantage, I think, because of the things that you are learning in your doctoral career. It's not exactly transferrable easily to lots of other careers. So I think I continue to feel like I felt when I was in graduate school, "I might as well get a degree that is a strong marketing tool in anything, because I might be forced to do anything, to get a job."

The conservation science degree, I think, it's not going to get you that interview like it would if you got analytical chemistry or physical chemistry or a more hardcore science. The more hardcore it is, the better it sounds, the smarter you sound. And the more transferrable you can argue with it. If you became an expert in paint drying, there's not a whole... but I guess the paint industry is kind of big, but I think they understand the drying process pretty well by now. So maybe not that, but you get my point is that it's kind of a tailored education in a difficult to transfer kind of technology.

It certainly doesn't carry any cachet. If anything, I would think it's a disadvantage outside of conservation to have a conservation science degree. Because it makes it sound like a humanities degree to some extent. It's a softer, less hardcore science, I think. And, honestly, getting back to my point, chances are you're not going to get a job in conservation. So you better have prepared yourself to be competitive in applying for jobs outside of conservation. And ask yourself

whether that conservation PhD, conservation science PhD is going to be doing that.

Kelsey Wingel: Yeah.

Paul Whitmore: Students have asked me over the years, "How do I prepare myself for a conservation science career?" My advice has always been "Go to a graduate school teaching you good science in something, in something that can be overlapped, with a Venn diagram, overlaps with conservation, whether it's going to be metallurgy, or if it's going to be archaeometry, or it's going to be analytical method development." Something that conservation occasionally uses, needs. Chances are the next job in conservation is not going to be in what you trained in. Hopefully you will have something in your background that is going to overlap with the job requirements. I think your chances of doing that are better than narrowing your specialization that early in your career, because chances are that's going to be a bad choice. Or, you're going to have guessed wrong what the next job is going to look like.

Kelsey Wingel: Yeah. Speaking of students, Paul.

Paul Whitmore: Yes?

Kelsey Wingel: I know that in 2018, I think, you started an undergraduate course at Yale.

Paul Whitmore: Oh yeah, oh yeah.

Kelsey Wingel: I'm wondering if you wanted to talk a little bit about that and what that experience was like and why you started it.

Paul Whitmore: The why, it had been kind of simmering for a while. I had done occasional teaching, mostly as guest lecturing in other people's classes. And I wrote a chapter in a textbook on paper conservation, Irene Brückle's *Paper and Water* text, that allowed me to like put down on paper for the first and last time my peculiar view of the chemistry of paper. That was my experience, my one experience … I'm getting a pantomime hand signal here. I think I understood what my wife was saying. She was waving out the door and I think she meant she's going out … Oh, okay. Yeah. It's my three o'clock dog session. My wife was standing in. I was going to say, it's either she's leaving the house or the house is on fire and I should be leaving. Okay, so I had written that chapter and I felt like I do have this kind of point of view of the synthesis of the things that I've read and the things that I know, the things that make sense to me that are not in any books. I didn't get them from reading books. I got them from reading books about other stuff, and then putting the pieces together myself.

I thought maybe there's something there that is, well, the other thing that I thought was different about me is that I was trying to stitch together the chemistry of materials and the physical properties of the materials, because the physical properties, including the appearance, because those physical properties are like, those are the things that are of value. Those are the things that are supposed to be preserved. Those are the things that will make materials work in

an object. When the materials lose those properties, then that's when the object tends to suffer it's changes of age.

So, I had to … pick paper as an example. I was trying to understand the cellulose chemistry and what goes on there and yes, okay, cellulose molecules break apart. That's the chemistry that's going on. Why does that end up with paper that it becomes weak, becomes brittle, and becomes discolored? What is the connection between the chemistry at that level, on the molecule level, and the macroscopic performance of that material? That's not an easy bridge to cross. That is one that few scientists have attempted to do in a convincing way. Sometimes it's good storytelling. It's not always proven that that's the way the world works. That's what I was able to capture in that book chapter in Irene's textbook was my trying to put together the chemistry of cellulose deterioration with the impact, the loss of paper strength, paper flexibility, things like that that are the manifestations of that chemistry.

I worked very hard at that, trying to understand that. That's kind of been a theme throughout my career, trying to understand the chemistry on that basic level and how it manifests in the things that you really care about. I hadn't thought to teach a class on that, on any such subject, because I wasn't sure that I would have anybody interested in learning that. It does sound like a well, I'm not sure what it sounds like. I'm not sure what kind of class that sounds like, but one of the faculty members that was on the advisory board for the Institute [Institute for the Preservation of Cultural Heritage at Yale University] was Kyle Vanderlick, who at the time was the Dean for the School of Engineering and Applied Sciences. Kyle had known about our Institute and she was really interested in my work, because it was the hardest core of the science that was going on.

Honestly, she saw us, my group, as being this kind of bridge between the conservation world and the engineering department, the students and the faculty in the engineering department. She invited me to think about whether I would be interested in coming up with a class. She actually invited Anikó [Bezur] and me to think about teaching a class together and when the time came Anikó said, "No." But it was really Kyle who was trying to convince me that this would be a way of bringing the Institute in from the fringes of the university more centrally into the academic function of the university.

That's one of the things that has always been a threat hanging over me in my career was I was never a teacher. If the university is all about teaching, then a provost can always ask me, what am I doing here, you're not teaching. You know, why aren't you in a museum lab? Why are you at a university? That was always a hard question to answer. Anyway, here was an opportunity for me to become a teacher, for what it was worth. So, I agreed, I agreed that I would teach what turned out to be called a material science class in the engineering department, material science of art, I think it was called, which was kind of ironic because I don't know material science. I don't know what that means even. I think there is such a thing called material science, but I don't know any of it. I was going to just teach what I know and we'll call that material science, because it has something to do with materials.

I was trying to ... what? … I'm not sure how it began, because this wasn't going to be a conservation class. This was going to be a science class. I was trying to get it to be more basic than what can become, I couldn't start off with arcane things like varnish removal for a class of students that I couldn't predict would know that paintings were varnished in the first place. I turned it into a class that was about kind of the manifestation of basic principles in making art. Things like color, things like material strength, flexibility, things like the purist, chemical art-making photography, silver photography, and kind of build the subjects of the class around headlines ripped from today's newspaper.

The color and fading, not fading, the color component of the course was built on the Rothko recoloring, relighting to recover the original color, to teach the principles of, well, how things look, how you perceive the color, how the lights are reflected off of materials, and how what you see is intrinsically related not just to the material, but to the light that you are delivering to it, and so you can kind of tune that to recover any appearance within bounds.

The lecture and the lab modules were built around these things that I had seen in the newspapers that had newspaper coverage. I could point to that as being the apotheosis of this knowledge and technology, and you can do it too. In the lab, I gave the students swatches of paint that had been faded on one half, and they had to make the spectral measurements, calculate what kind of lighting would be needed to make them look the same, and then use a PowerPoint projection with the appropriate lighting that they had calculated, the RGB that they would need.

They would make a PowerPoint projection of that RGB, and they hold up their swatch of paint to it. They see the magic transformation of the faded one that looks identical to the unfaded one. It was like gasps in the room and applause when people are successful. It was just so satisfying to be able to reach the students that way. It was fun. It was just so fun. The hardcore part of it, yeah, and okay, so that's the way I designed the class was to have those, the lectures that were amplifying the theme of the lab that was supposed to be this kind of hands on manifestation of something that was in the news.

We had like a lab on fluid paints, because there was some article on the fluid mechanics of Pollock paints, what made them special and why some of the patterns in the Pollock applications of the paint weren't hand drawn, they were just manifestations of how the fluid properties create those structures. There was that one. Oh, the big lab at the second half of the semester was on solid mechanics, and the subject was the cracks in Michelangelo's *David* statue in the ankles. Because the pedestal got kind of tipped over and torqued the statue so that the backs of the ankles were getting hairline cracks. One of the other engineering classes prior to my class, that was one of the class projects was to build this mechanical test or essentially build this homemade mechanical tester from the sensors in a kitchen balance.

They took off the balance, used the pressure sensors on it, built another set of plates to apply pressure like a book press with those sensors, creating the data stream. Then, that was what was used to crack little plaster replicas of David's legs. There was this kind of art-making part of it where you're making silicone

molds of Davids, you're casting plaster, and then you're testing the compression testing of the plaster cast with angles of inclination to replicate the torqueing of the statue and see how the strength measurements change when you do that. It was kind of, there was a backstory of hardcore engineering stuff and material science stuff, but it was aimed at something that was very present, an object with something happening to it, materials that something happened to them.

It was either faded off Rothko paints, or it was cracked marble. I tried to make the labs have a point to them, rather than just demonstrating abstractions. It's a good thing that I did, or maybe because I did that, I was thinking that, and Kyle Vanderlick, the engineering team, was thinking this is going to be a way of teaching some of this application of engineering stuff to the art world so the engineering students can feel like there's a broader impact that I'm capable of. There's other things I can do besides build a bridge with the things that I'm learning.

It turned out that there weren't any engineers in that class. It was almost entirely, 35 students, almost all of them were humanities majors, upperclassmen who needed a science credit to graduate. This was a science credit that seemed accessible to them because it wasn't abstract. It was about art stuff that they understood, that they probably knew better than I did, but it was teaching them the backstories about the things that they already understood. I wasn't expecting that. I had no control over that, but that turned out to be the ... That was a class that the engineering department missed. Right? When religious studies students are looking for science credits, who knows what they go looking, but they're unlikely to enroll in engineering lab classes, but here was one that sounded something that was more up their alley that they could get, or at least art making was something that was familiar to humanities students at Yale, art is something that's more familiar to them than maybe suspension bridges, I'm guessing, but I'm thinking that might be true.

Anyway, it was challenging in a different way because I had some of these humanities students who were just, well as one student put it, "I understood what you were saying until you make a graph of it, and then I just glaze over." She was completely graph averse. I guess that's not unusual, but I had a hard time reaching her. I had a hard time reaching students who just didn't have that, just the quantitation of it was off putting. But I couldn't not do it, because it was supposed to be a science class. They were supposed to be getting a science credit. I had to get the class approved by some committee or committees at Yale, and I had to demonstrate or prove to them that this was hardcore enough to earn a science credit by telling them exactly what I was going to be doing. Then, the students were, they had to earn it.

It was, how do I put it? I hadn't taught before. It should come as no surprise to anybody else that it was a lot of work to teach the class for the first time, to come up with the syllabus and the lab exercises, so I knew what the lectures were supposed to lead up to and then to do, what? 50 minute lectures, twice a week every week for 10 weeks, and the labs on the Fridays. If Kate Schilling hadn't been working with me on this, I would never have been able to do it, because she did probably more than half of it. That's not quite right. I did the conceptual part

of it, which is always the hardest, but Kate did all of the lab stuff, all of the handouts, the instructions and managed the lab things pretty much without me. I was in the room, but I didn't do very much.

She also did all of the grading of the homework and tests. I did the, well, we shared the office hour things, but Kate actually, she did the heavy lifting on the actual lab stuff and the mechanics of the class while I did the slide lectures and coming up with the exam questions. It was hard work - the second time, which I wasn't around for, Kate taught it the second time. I think she learned the same kind of lesson that I did, that it was just too much stuff to cover, that it was like ... For a class that gives you one credit, there was like two lectures and a three hour lab every week. That was a lot of time investment that was way beyond the one credit you were supposed to be earning. It was kind of poorly calibrated in that way. Also there were just too many subjects. I felt like everything was like drinking out of a fire hose and it would have been easier to just pare it down to do one before the semester break and one after.

I think that's closer to what Kate ended up doing. She just trimmed it so that there were fewer subjects and the labs weren't every week or every other week or something like that. Kind of throttled it back to something more humane. I wasn't around for the second round for that. I wasn't interested in doing it the second time, because I was promised that there would be something in it for me, that this would stitch us together with the engineering department better in more than a psychological way, that there would be the chance to like ask for sponsorship for alums that give money to the engineering department. I saw a whole bunch of fundraising going on. There were certainly tour groups and publicity that came out of our lab class, but it all went to the engineering department and Kyle ended up rotating off the deanship, so there was no like resident persistent interest engagement.

I didn't feel like I got anything out of it at the end. All I got out of it at the end was it took a year and a half of my time, it consumed a year and a half of my time to do just that. Then I'm not writing grants and I'm not publishing papers. Yeah, if you're an academic, if you're on faculty, you get credit for the teaching. On research staff, it's just lost time. There's nobody who's thanking me for it even. Anyway, I just felt like that was fun, but I'm not sure it was the best way to have spent that time. If there was an institutional commitment to that, if the Institute were committed to teaching like that so that it wasn't just a time suck that you paid a price for, that would be worth doing.

I think, well, Kate is interested in continuing the teaching engagement because she's being paid by the engineering department. She feels that to earn her money, she has to do such things. I don't know. I think we should. I think the Institute should, not that anybody is asking me, but the Institute should do such things because it does make you less of an outlier at the university. It does make connections and embed you with the academic departments in a way that we have a hard time doing otherwise. Honestly, I think we have that to offer. We have this attractive, romantic end point for the things that scientists and engineers know and do that can engage the students. The engineering students, not that I would want to encourage them to become conservation scientists, but at least

they would see it. At least they would see it and know that there are other things in their futures besides building bridges and buildings that they might find satisfying and important. That's a hard thing to come by.

I think I mentioned before, in part two, maybe it was, it's hard to keep them straight. In volume two, that I stumbled on conservation from pure happenstance because almost none of the people that you encounter in academia know anything besides what happens in academia. Yeah, they're able to tell you upside and down what your career can look like in an academic environment. If they know that other niche professions exist, they don't know very much about them, because they haven't experienced them. None of the people in the university have experienced them. That's something that the Institute could offer is that window to another world. One other world, there's lots of other worlds. There's lots of niche professions you can find satisfying that would let you apply your particular talents in. I think that class was one opportunity.

I don't think I persuaded any humanities students to become conservation scientists, but you just never know. I had interns in my lab in Pittsburgh who were, one was a physics major and she just decided she was going to become a conservator. She went off to conservation school and now she's a scientist at the Library of Congress. You never know who you might grab, who you might snag by, you're not even promoting it, just by showing people that such a thing exists. That should happen, too. Especially at a place like Yale. You know what I mean? It's not MIT. You're not churning people out to the same old, same old professions and institutions. People at Yale are supposed to be like Johnny Appleseed going off to find your own way. This is one of those paths that might be appealing, or more importantly, when the Yale graduate goes off to become a hedge fund manager, they remember they saw this profession of conservation that really did need investment from somebody, anybody. They might drop a check someday.

Kelsey Wingel: Yes, we could use more of those people.

Paul Whitmore: Yep.

Kelsey Wingel: The more people-

Paul Whitmore: I-

Kelsey Wingel: Oh no, go ahead, Paul.

Paul Whitmore: Well, I mean, I was trying to recover the distant memory of your questions for me, which was about the conservation science PhD thing, and my lukewarm feelings to that. It would be great to have talent, but it's risky for the student to pigeonhole themselves like that at that early point of their career. I think doing it the way I did, the way most of my colleagues have done, of getting a more generic kind of training experience, and then hoping just to be competitive when a job opportunity in conservation comes along. There have been plenty of talented people with generic and powerful backgrounds that we're not able to

capture. Part of it is because they weren't the one selected for the one job. Part of it is, to be honest, it's hard to make this future sound better than other futures.

We're not just competing for talent. I mean, yes, we're competing for talent, but we're competing for talent. We're competing with other things. I have always said that smart, capable, young people can do almost anything with their lives. We're trying to persuade them to do it for us. What do we hold out for them? Well, meager job prospects, almost no lateral mobility, almost no vertical mobility, just not very much in comparison to what they might get. I mean, there's no tenure. What exactly are we offering? We're offering the romance, the excitement, the satisfaction. Well, it's hard to pay the ... I mean, you can honestly, I am convinced that there's lots of things that you could find satisfying and exciting besides conservation. I mean, I've always thought that, there's lots of other things out there in the world that you can become as passionate about as conservation.

I don't know, if you're trying to attract good people, you're going to have to make it look attractive for them. That's hard to do. It's hard to do at least with the situation as it's currently defined. Yeah. Lots of things. Well, among other things, it's just such a small field that there's not much of an ecosystem. It's not like going to a chemistry department where you'll have two dozen colleagues, and among those two dozen colleagues, you might be able to find 10 to become your professional circle and maybe three to become your real go-to companions.

When you go to a museum lab, unless you're at the Getty or the National Gallery of Art, you go to a museum lab and you might be one of a handful, or the only one. That's hard. That's a hard life, unless you're an introvert and you don't want company, in which case the quarantine is a welcome thing for you. To do good science, I mean, you really do need that ecosystem. That's a hard thing to build. We're not anywhere close to a sustainable one now. It's rough. I mean, it's rough outside of the big organizations, the big science labs. So do you have another question?

Kelsey Wingel: Well, I don't want to keep you for too long, Paul. We are past the three o'clock for Selkie.

Paul Whitmore: Well, I'm not going to do chapter four.

Kelsey Wingel: I guess my only other question, it's not really so much a question, just you talked about Dr. Feller, but I just wanted to ask if you had any thoughts about your experiences with him that you wanted to share or any memories that you wanted to share.

Paul Whitmore: Yeah. I kind of got the same, I felt the same thing in retrospect that I had mentioned him and my idol worship of him, but I didn't really talk much about working with him or our engagement together. Honestly, there wasn't a lot of engagement together for much of my career in Pittsburgh and that was purposeful on his part. I was inheriting his staff and he was intent on not hanging around so that it looked like he was still the boss or that people would treat him like the boss instead of me like the boss. So when I showed up in Pittsburgh, he tossed me the lab keys and said, "See ya, I'm going off to Maryland where I have a lake

home." And I almost never saw him. I did see him because I tried to make it clear to him that he was always welcome to come in and to sit in his office, he still had an office, can do whatever he wanted. He could go in the lab, he could do experiments. As long as he didn't ask my staff to do things for him without asking me, then he was welcome to come in.

And he did that. He came in and I don't want to call it puttered, but he continued to do research, he continued to do experiments, and I would see him regularly and it was great. The first time and maybe the best time I had working together with Bob was when I had, for reasons I can't recover now, I decided that I wanted to publish that anthology of his papers, so many of which were in obscure journals or magazines or places that didn't exist anymore. So our reprints in the files were the only remaining copies of some of those works. And that was a tenuous legacy that he was leaving. So that's why I wanted to do the book with him.

And that was our chance to sit down shoulder to shoulder and go through his stuff and get him to help me select like, "What do you think is the important thing? What are the subject areas that... What are the groupings that you want to see these organized in?" And then he wrote the little, the introductory passages for each of those chapters, subject chapters, just to give some kind of more personal flavor of where they came from and how they came to be. It took us, I don't know, six or nine months to put that book together. It was mostly my backbreaking work. That was my second job. So I was working with the CMU press editor, and I would come in at like, I don't know, six in the morning, work on their Mac. I didn't have any Macintosh computers. I had to work on their computer doing this. Doing the layout and the proofreading. And then at 10 o'clock, I would go to my paying job and do that until dusk and then do the same thing the next day and the next.

So I was working to try to get that out. And that was when I had a chance to learn from Bob all of these things, all of the backstories about this stuff and how these papers came to be and why they came to be in that particular sequence. It's just things that I, memories that I can't even put down, but it was our first opportunity to actually do something together. And it wasn't really a science research thing, but it was a project. And then over the years, a little before that, and certainly after that, then Bob became more engaged in, well in helping me really, where I was trying to find my own way. I was trying to puzzle over things.

Most of my research, aside from the microfading tester, the rest of the chemistry research were all subjects that Bob had been engaged in, polymers and color and paper. So when I had data that I didn't understand, or when I had data that was intriguing and I thought I knew what was going on and then I would have Bob listen to me and look at the data and kind of give me that second opinion. And to distill that experience, I mean, there were many occasions when we would do such things or I would just throw things at him, mail things to him, and he would chew on them. And then he would send me back these six pages of thoughts. Or when I'm writing a grant proposal and I had ideas, or I was trolling for ideas, he would give me like six pages of his thoughts about what would be worth doing or

not worth doing, but kind of the threads that were left that I could consider picking up and following without making real judgements.

I kind of got a sense, I got an instinct of what he would've done if he had been given the opportunity, but he didn't try to steer me. He just would point out to me that we still don't know this, or this would still be a good thing to do. And he would just give me long lists of stuff that were his ideas of unfinished business. But to distill all of those conversations and those kind of drive by encounters, we got into a habit of having maybe every month, we would have "lunch." And I put that in quotation marks because we would go to a restaurant that was in the bottom of a residence hotel. So everybody there was like 90 years old and playing [inaudible] in the back. Oh, hi Selkie [Paul's dog comes in]. Oh hello, you're back. You're back. Oh boy, did you have a good time? Oh my goodness. Now I smell like her, so she's satisfied.

Kelsey Wingel: Awww.

Paul Whitmore: So it was a restaurant that was like a block away from Bob's apartment. And so he could walk there and I would go there. We would show up there, lunch at noon, and it would have lunch until, I don't know, three, four or five o'clock. We would just spend the day together, spend that half day together, just talking about stuff. Some of it was science stuff. Some of it was professional, but non-science stuff. A lot of Bobs, yeah. I mean, I don't want to call it carping. A lot of the things that he was struggling with were his... He was a rare book collector, or he collected books on art materials and stuff, but like the historical ones, the ones you would go to a rare book shop and spend hundreds or thousands of dollars on, he had a collection of them. And as he was looking forward to fewer years than behind him, he was looking to bequeath that collection to someone.

So there was this kind of looking for someone who would be interested in taking the collection, but making it accessible and not walling it up so that it was not available to anybody. So eventually it ended up going to the National Gallery of Art. And of course, Bob being Bob, he wanted to have book plates in all of them to designate their provenance to his collection. And he designed it and had it printed, and just went through endless... Well, anyway, many of his side of our lunchtime conversations were just the news reporting or the venting about the extraordinarily difficult time he had donating the dang books to the National Gallery of Art to have them put them someplace and designate them somehow and put the book plates in and all the rest of that stuff that just took years. It took years, which honestly, I can't say I'm surprised by because who would make that an emergency priority? No one, except the guy who wants to do it before he dies.

So he would, a lot of times, complain about him trying to contact people or emailing them and not hearing back or something. But that was one of the final things that he was engaged in was trying to get his books placed in a proper home. And the rest of the time well, what? I was telling him about my life and about my experiments and getting him to talk to me about his experiments and his publication. I think after he published the, or after his anthology of collective works came out and in the back of that book, there is a bibliography, a complete bibliography with the highlights on the ones that were republished in that book.

And I don't know, there was like 141 of them or something like that. And then after that book came out, he published one more paper in a science journal that was on polymer degradation study.

And that's the only one that's missing. That's the only one that is not accounted for in that book. I'm not sure I would have put it in the book, even if it had been available. But anyway, it was kind of my offering to him to cement his legacy as best as I could help him do, to make his publications available, to make his greatest hits that he had personally selected, to make them available in some, hopefully more lasting form. And if anybody wants any more of those books, I have like 600 of them in the warehouse at West Campus. Yeah. Somebody tried to sell them once and it was, I don't know, I think I was giving them away faster than anybody was selling them. But that was, I did it, yes, as a way to contribute to the profession to make sure that they could take advantage of his contributions to the field. But I mainly did it as a good thing for Bob that I wanted to see.

I wanted to do something nice for him and I wanted to give him the opportunity to be recognized by his peers and the generation that came afterwards. So there was like a book signing party at the National Gallery of Art, where he got to give a lecture and he got to sign books. And this was after he had been retired for a dozen years. And so it just got him in the public eye one more time so that he could know the people hadn't forgotten him and that they really did value, treasure, the things that he had done for the field. He was a one of a kind guy and it's hard to imagine anybody having as big an impact in the professional and the personal ways that he had.

I'm still speaking of him with hero worship but he was the ideal that the rest of us can try to measure ourselves against and we'll fall short, but we know what we should be aiming for. He was one of the most remarkable people to have ever been in this profession by far. And it probably, you could say the same about many of his peers. He showed me pictures of those early IIC meetings in the '50s where the entire conservation profession is there on the steps of some building, all 25 of them.

Kelsey Wingel: Yeah, that’s pretty amazing.

Paul Whitmore: Yeah. And every one of those names pretty much, every one of those names is like these iconic figure that is etched in marble somewhere. And those were certainly different and exciting times. But Bob, after he retired, he pretty much disconnected from the business, the profession. He stopped going to the AIC meetings, he stopped going to the graduate programs. So he wouldn't travel so much. And I wanted him to know that his legacy endured, that people still remembered him. I still remember, I was the instigator of his lifetime achievement award, the AIC award that bears his name. I had to do that in secrecy because I knew he would've put the brakes on it if I had tipped my hand.

But he was awarded the first one, it was in Philadelphia, whatever year that was. And I got to... No, I didn't take him. Maryland Kemp Weidner and Abby Quant. I don't know if you know those names, they're old timers of his generation, but maybe the next younger generation, I don't know. They were old and retired.

They drove across Pennsylvania, picked him up and drove him to the AIC meeting in Philadelphia so he could receive his award. The room is packed, 1,000 people. 1,000 people giving him a standing ovation, prolonged standing ovation. And I almost cried. That was what this was all about.

Just to thank him one more time, because, well, it's not a job that gets lots of people thanking you. And so I think he was starting to feel like he was on the sidelines or just left behind. And so this was a chance to just tell him, "Nope, you did a good thing. You did a good thing that we're all taking advantage of forever after." And that's, well, of the things that I did that was the least professional of them was to do things that I wanted to do because they were nice for Bob.

Kelsey Wingel: That's wonderful Paul. Wonderful.

Paul Whitmore: Well, I mean, we owe people like him so much. So you feel like, you're not powerless to do anything, but you feel, I don't know. It's easy to just let time slip away and before you know it, they're not there anymore. And especially since I was seeing Bob so regularly and I could hear him talk about how, just mention kind of wistfully, how he wasn't as central in the community as he once was and just kind of felt like he was wandering away into the sunset. So anyway, I didn't want him to go without having the chance to say "thanks" one more time. So that's how that happened. The book and the lifetime achievement award were both kind of the things I could think of that I could get him in the spotlight one more time. So anyway, that's more of a testament to my feelings for him than an actual articulation of them because I can't really speak to that more than I have. Maybe it's just, I thought the world of him.

And I just wished that I had been... Well, I don't know, I wish that I'd had a chance to work in his lab under him. That was the thing, I was coming to run his lab when I'd never run a lab before. And I didn't know the subject matter of the things that were going on in that lab, except what I had read from his publications. So I felt like I could have been so much better equipped if I had had a chance to spend a couple of years working under him before assuming his position, but that's not the way it worked out. And for better or for worse he disconnected when I came.

I mean, he didn't have a chance to mentor me because he wasn't in town. And I'm not sure that was a good thing or a bad thing. I would've liked to have him around more to help me, but I don't know what else would have happened if he'd been around more or if he would have liked being around more. I have no idea. The guy was supposed to be retired. He was supposed to be off with his wife at his lake home in Maryland. So that sounds pretty darn okay.

Sounds better than going, trudging into the lab every day. But he loved that. He lived in his apartment, at least on the old lab building, the old lab in the Mellon Institute building was like a block and a half away from his apartment. So he could just stop by or he could hang out and it was easy for him. It was like the back bedroom for him. So he spent a lot of time when he was in town and he spent a lot of time just puttering and doing his lab experiments there. And that was, I mean, he lived until he was 98 or something obscene but he lived that

long, partly because he kept on doing his work. He kept on walking that block and a half and having his mind puzzle over things and just, it kept him young. I have no intention of staying young like that though. I'm just going to go fishing once I can get out of this place. That's the last you'll hear of me.

Kelsey Wingel: Oh Paul, thank you so much.

Paul Whitmore: So yeah, that's volume three.

Kelsey Wingel: Volume three. Well, you very thank you so much for being so honest and sharing your wisdom with future generations with this interview.

Paul Whitmore: Well, please do let me know what future generations have to say about my wisdom. Sure it'll all be good. Thank you. Thank you for taking the trouble, Kelsey. I mean, I had half a mind to put this off until the fall. I think that was one of our original alternatives. And then I thought, "Well, the longer I wait, the dimmer the memories, the less interested I'll be in any of this. And I might as well just capture whatever is there to capture." And I'm so pleased that you were interested in doing that. And some of the stuff, not so much the stuff about me, but the stuff about... Well, stuff about the people that I had help me, those were memories that I'm happy to have recorded somewhere.

I don't have any emails to archive. Those all got torched someplace. And I told you I went through the paper correspondence I had and it was so sketchy and thin that I really kept it … it's kind of like keeping old Christmas cards, I keep it for the handwriting samples more than anything else. And just kind of reliving the memories of the people. It's not like there's anything of historical value in any of that correspondence I think. Certainly not the letters I wrote to anybody. So anyway, thank you. Thank you for being so patient and gracious and happy at the other end. But it helps. I don't run across people who are smiling, that I can see are smiling because they are wearing masks. So it's nice to see a whole human face again.

Kelsey Wingel: It's been my pleasure, Paul. It really has. Like I said at the beginning, I learn so much from these interviews. I feel so fortunate to be able to learn from the wisdom of so many people in this field and yeah. I'm grateful for this time. I really am.

Paul Whitmore: Okay. Well, take advantage of whatever wisdom is there. I'm not sure there is any, but go ahead. And yeah. And I hope that you get unlocked soon so that you can get out of Shawshank.

Kelsey Wingel: I hope so too Paul. It seems like it's going to be a while yet, unfortunately-

Paul Whitmore: Really? I heard we're trying to unleash the energy of West Campus again, but I don't know.

Kelsey Wingel: I think they are. I think, I mean, we had a hope that the conservation department could go back earlier than the rest of the museum. But the latest news that I heard

from our director is that she doesn't want us going back any earlier than anyone else. And right now we're under phase three, going back part-time.

Paul Whitmore: Of how many phases?

Kelsey Wingel: Out of three phases. So that puts us in like August, I think.

Paul Whitmore: Okay.

Kelsey Wingel: Which is an unbearably long time away but we'll see.

Paul Whitmore: I don't see the little pencil marks of the days on your wall yet or that's behind the lamp.

Kelsey Wingel: I haven't gotten there yet, but there's still time.

Paul Whitmore: I'll be doing that some day soon.

Kelsey Wingel: Oh my God. This is the part of the interview, Paul, where I would give you the release form and have you sign it. But if it's okay, I will email it to you. I don't know - do you have scanning capabilities?

Paul Whitmore: It is sporadic.

Kelsey Wingel: Sporadic.

Paul Whitmore: Whenever my wife or I have to print something or scan something, there is usually some period of attention where we have to regenerate the thing that hasn't been used in a long time. So yes, occasionally I can print things out and scan them.

Kelsey Wingel: Or maybe even a photograph, if the scanning doesn't work. That would probably be fine. And Joyce has also requested a photograph if you would like to share-

Paul Whitmore: They got like 12 hours of video, what do you need a photograph for?

Kelsey Wingel: That is true. It's true. If you wanted to share any photographs, those are most welcome. And she also asked for a copy of your CV if you have that, just all of those materials to be kept in archive.

Paul Whitmore: Okay. Yeah. It's around here somewhere. I mean, I took all of the stuff that was on my work computer and it's on a flash drive that I don't think I've lost yet, but none of that stuff is on the computer that I use now, on purpose. This is the clean slate, absolutely.

Kelsey Wingel: Yeah. Yeah.

Paul Whitmore: All right. So you'll email me something that I should sign and try to do something with?

Kelsey Wingel: Yes.

Paul Whitmore: And I should try to find my CV and get that to you. And I'm thinking there's no real deadline here except my memory. So if you haven't gotten things from me, then just throw me a rock and remind me I forgot.

Kelsey Wingel: Okay, I will. I will. Thank you, Paul.

Paul Whitmore: Well, thank you, Kelsey. So good to see you and spend some time with you, even if it is remote.

Kelsey Wingel: Yeah. I hope to see you in the halls of Building 900 at some point.

Paul Whitmore: Well, okay.

Kelsey Wingel: You can give me a copy of one of Feller's books.

Paul Whitmore: Oh, wow. Once there's people, other people in Building 900, I think giving you a book is easy. I think there's an open box of them, might be in the library. I don't know where it was. It was in my office. It's not in my office now. So Kate Shilling would know where it is, I'm sure. Just tell her I said it was okay.

Kelsey Wingel: Okay.

Paul Whitmore: If you want an autograph though, that's going to be harder.

Kelsey Wingel: Not going to happen. Yeah.

Paul Whitmore: You're going to have to wait for me to roll into Building 900. Not sure when that'll happen.

Kelsey Wingel: Yeah.

Paul Whitmore: Okay. Well...

Kelsey Wingel: Well, hang in there, Paul. Stay safe.

Paul Whitmore: Yep. You too. Happy cloistering to you. All right. Thanks so much.

Kelsey Wingel: Okay. Thank you, Paul. Bye-bye.

Paul Whitmore: Bye-bye.

PART 3 ENDS