

Oral History: Eric Wieschaus / 2017/11/04

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File name: 2017_11_04_Eric Wieschaus_transcript**Key**

AFL: = Interviewer, Anne-Flore Laloë

EW: = Participant, Eric Wieschaus

[???at XX:XX] = inaudible word or section at this time

AFL: So we're here today, it's the 4th of November 2017 and we're in Gloucester, Massachusetts. This interview is part of the oral histories programme of the EMBL archive. My name is Anne-Flore Laloë and I'm the archivist at the European Molecular Biology Laboratory. Now please would you introduce yourself?

EW: I'm Eric Wieschaus, I'm currently Professor at Princeton University but was one of the original group leaders at EMBL between 1978 and 1981.

AFL: Fantastic. So would you please just give us a bit of background on what you were doing before you go to Heidelberg.

EW: OK, so I'm an American US citizen, I ended up in Europe because I had started my graduate work with Walter Gehring at Yale University and Walter had an offer to return to Switzerland and so I went with him, left New Haven, moved to Basel and finished my thesis work in Basel, stayed then in Switzerland for three more years for a postdoc in Zurich and then had a job offer to move to Heidelberg, which I took and arrived on February 1st 1978.

AFL: So what was your project plan for Heidelberg?

EW: So it was coupled in a way to a plan that Christiane Nüsslein-Volhard and I had developed a year or so before when she was a post-doc in Walter Gehring's lab in Basel and I had just left. We both basically defended our thesis in 1974, she then began a post-doc in Walter Gehring's lab because she wanted to learn embryology and do developmental biology. The way things had worked out in Walter's lab in this very early age of molecular biology, Walter had used his move back to Zurich to discard many of the people in his lab and build a new lab that was just dedicated to the coming phase of molecular biology, so Walter wanted to have a lab that did molecular biology, molecular biology of flies. He attracted talented post-docs who had done something like molecular biology before. This is of course 1974 so it's

not quite clear what molecular biology was gonna be! Janni fell in that group. She had isolated a piece of DNA from the lac operon that I believe bound the RNA polymerase, and that was an exciting thing. You weren't able to sequence it or do anything with it, but it gave her the credentials as a molecular biologist, I think she was attractive to Walter in that sense. She came there wanting to never do molecular biology again and wanting to do <chuckles> embryology. I was the only person in the lab who worked on embryos or did real embryology, and so we bonded and one of the ideas at the time that we had was to try to get a genetic handle on genes that control embryonic pattern.

I had been working on genes that affected oncogenesis in flies, and she had been working on, in Basel, in a short period there had identified the bicaudal mutation and the dorsal mutation, so we had ideas that such genes existed. We actually even wrote a proposal to go to the Max Planck to do those screens, like a five-page type-written thing that I still have a copy of. It did not work out. Her relationship with Walter did not work out. She left after two years, went to Freiburg, began applying for jobs and we continued to correspond back and forth and were working <5:00> on this project. She then, I believe ... you should interview her!

AFL: I should, I should! Yes. I will.

EW: Sensible. She had applied to Heidelberg and the feeling I think was that she did not have enough experience in flies, so when they were going to hire someone, they decided well it will be good maybe to hire flies and the requirement wasn't, even though this was the European Molecular Biology Laboratory, it wasn't really necessary that they do molecular biology but the idea was they would have to know about flies. And the feeling, I think, was that she didn't have enough experience, had only worked on flies for three years although she'd done spectacular things, and so they didn't want to offer her the job.

I was in Zurich at the time, I don't know quite how to document this or explain this but ... I had worked in flies for longer. One version of the story would be that I'd worked in flies for longer, had more publications and I had done interesting enough stuff, and so I got a letter from Sir John Kendrew asking me if I would apply for a group leader position at the EMBL. I think there were some people on the board or the Council who talked to those people in power who thought that I was clever and cool and so they ... and so this letter came and then Janni called and said, explained to me (this is for documentation I suppose) that I should do this and that if I did this ... and I was beginning to think about jobs anyway, that they would also offer her a group leader position and this would be then an opportunity for us to ... we all had our own projects of course, but we also had this collaborative idea.

And so I did and we both had offers. That's one version of the story. And that is ... true. I think if you look at the time, it's ... was striking that the ... group leaders, there were two group leaders who were hired in this bunch of moving into the building. Janni and Brigitte Jockusch, and they were the only two group leaders who were women, and both of them were hired into small laboratories where an

additional group leader was put. So Graham Warren with Brigitte and me with Janni. And so there's a certain interesting possibility that it had ... situations were complicated back then and it could have had things, you know ... could have been this kind of subtle sexism. You can look at an individual case, you can make the argument that Janni didn't really know enough people, she'd only been involved in flies in Walter's lab, she hadn't got along with Walter, there wasn't an obvious path for her to move into a laboratory, a high-level consensus run laboratory like EMBL. So there's lots of ways of understanding it that wouldn't be necessarily sexist but that is a reality that one has to consider.

I think it was a ... trying, somewhat difficult time for her. <10:00> And I think the way the job and the way our situation arose probably influenced our period at the EMBL in ways that we didn't necessarily anticipate. I think Janni, to a certain degree, felt insecure in the position and in the way she had got the job, that was if you will even a strain on our relationship but certainly a strain on her relationship to EMBL. We were ... easily close enough that those kind of strains could be overcome but it took ... I don't think it was very ... it was problematic overall, the relationship to EMBL was more problematic than I think necessarily history would like to imagine it to be!

<Laughter>

AFL: Fair enough.

EW: So we arrived, and it was great.

AFL: So 1979.

EW: 1978, February, we were there.

AFL: So the new building.

EW: The new building. We had to share a technician, so we had a technician, and we were extremely fortunate and got a spectacularly capable young woman, Hildegard Kluding, who is the person who was very effective I think. She later went on to medical school and is a doctor in the Heidelberg area. So we set up this lab. OK, so that's kind of the history of how we arrived I guess.

AFL: And so what did it look like then, when you arrived?

EW: It was mud in certain places, but the building itself, the central, the first building was completed.

AFL: John Kendrew was Director General obviously.

EW: John Kendrew was Director General. This lab was relatively small but was space for a bench run and a small bench run, probably somewhat about the size of ...maybe double the size of this table.

AFL: OK, so maybe 6 metres or so.

EW: Yeah, something like that. That has to be documented in some way – you can find that out! Janni had gotten an apartment in town. I got an apartment in Boxberg. The ground floor of the first apartment building as you come into Boxberg, so the closest possible to the lab. Used to be possible, before there was actually paths in the woods, but it was possible to ... I learned very quickly how to walk through the woods to get to the labs. I basically, my experience of Heidelberg was not really ... Heidelberg at all. It's basically EMBL and Boxberg. That's a terribly embarrassing thing to say, but I lived in Boxberg, I lived in the lab. Janni at least had a certain exposure to downtown Heidelberg, Schillerstraße.

AFL: But what did a typical day look like for you?

EW: Get up, you would go in, I would walk over. Depends, I'm doing the regular days, you'd sometimes eat in the cafeteria. On weekends I would actually cook beforehand and then I would carry the food that we were gonna eat that day through the woods. I had a little type of suitcase that had one or two pots in it and I would carry the food, stews and things like that, that you would ... or we would eat at *Bierhelderhof* or something like that. But basically you'd get up, go in, for the first year-and-a-half we spent time just trying to practically figure out how to get a mutagenesis screen to work, and we did a couple of failed <15:00> screens, we tried different reagents, we tried different crossing schemes, we tried a variety. The whole idea was to find what's the most efficient way of scaling up the traditional fly mutagenesis protocols to be able to look at the number of mutant lines. We really wanted to identify all things possible. Also at the time our strategy, our expectation had been to do screens for maternal effect mutants. They required more genetic steps, more generations, more selective screens. We spent a lot of time trying to get those kind of crossing schemes to work efficiently. We never could. Eventually Janni did some of the screens in Tübingen and Trudi Schüpbach did the parallel screens with me at Princeton. Janni did the chromosome, we did the second chromosome. But we could never get the experiments that we actually wanted to work and so we decided then, sometime in the summer of '79 I think, to cut our losses and rather than screen for maternal effect mutations, screen for zygotically active lethals, because you need one less generation and the idea was that the genes were acquired in the embryo zygotically, we knew such genes existed, we had a couple of samples, and so that was something we could do. So we started those mutagenesis screens I believe on the second chromosome, we did them in probably September of 1979 and finished them in November, so basically about two months, and then had flies and stocks left over and then a post-doc, Gerd Jürgens, joined the lab and we expanded with a second technician right about that time, and then in the spring of 1980 we decided to do the rest of the genome, the third chromosome and the X chromosome simultaneously, and the screens take about two months to do, and at the end you have a certain number of stocks, false

positives, real mutants, probably about ... if you had to narrow down for maybe a thousand lines to 500 lines. Before even getting that done though we discovered enough to write a paper that was published in *Nature* that we wrote in the spring, based largely on the results we picked up from the second chromosomal screen. That was published in July, so that was July '79 ... is that right? That was published in July of 1980 and ... by that ... yeah, OK.

AFL: So quite a productive few years!

EW: Now I have to figure out the timeframe on this because I had also decided that I wanted to leave, for a variety of reasons it made sense for me to go back to the States. I had ... the dates I'll give you, right but I then yeah, had a job offer from Princeton and then went back. I have to reconstruct the time here, but that's certainly possible for anybody to do. <Pause> And then Janni left Heidelberg actually a couple of months before I did, to take a position at Friedrich Nietzsche in Tübingen. So that's the general run of how things happened.

As to what the life was like, we basically worked in the morning when doing the screens, you would do the <20:00> busywork from around noon, you had to mount and check stocks and then in the morning we would go over the slides that we'd made from the night before and identify new genes. That's pretty much how the schedule ran.

AFL: And so you said you were the first group to work on flies at EMBL, so as there a fly house, what were the facilities like?

EW: That was also interesting. They had decided they wanted to have flies and people, actually there was a sense, I think Vincent Pirrotta who'd been hired as a real group leader with a ... he was a molecular biologist, hadn't had experience with flies but decided he wanted to go into that. There was somebody, Nick Strausfeld, who was a neurobiologist who was working on flies, bigger flies. And so in the animal facility they had decided they would have somebody who would take care of flies, and so they hired a ... actually they hadn't hired, we hired, one of our first jobs was to identify somebody who would run, who could cook fly food and prepare and keep Nick's *Sarcophaga* and other insects. So we hired Adelheid Schneider, and initially her job was to prepare fly food and take care of Nick's flies but in fact if you set things up well, that turned out to be about a ten-hour a week job for her and so, because she was put down in the basement, and it wasn't pleasant ... so we made a space for her in our lab and she then would spend most of her time up in our lab helping us and would go down and prepare the food and prepare, do the other jobs that she had to do. So she was very efficient. I think that realistically it was a practical, sensible solution. Things weren't really structured, there wasn't a group animal facility so where are you gonna put somebody, someone who comes in and is going to work in the organisation but ... and I think this in part characterises a little bit the early years, that there wasn't a great deal of thoughtfulness about how, when you hire people, what they should do or what the job, you know, how you manage a facility. It was envisioned, built on a plan that

instrumentation was always viewed as this huge thing that was going to be ... and people were going to be making wonderful equipment but without a clear design of how to relate that to individual research groups or ... and it took a while to do that. I think you could criticise the first years ... my impression of EMBL was the first years were very rocky and it wasn't actually until Lennart Philipson became Director that many of these organisational problems were actually solved. The vision of this laboratory and the usefulness that it would bring to Europe was obviously the insight of all the smart, clever, famous, wise people who were involved in the design, but they ... I feel, my feeling is they hadn't really thought seriously about what the organisation should be, and maybe they couldn't have. And I think while Sir John was ... important and useful in getting this organisation set up, he was not a person to make the ... organisation run effectively in terms of understanding <25:00> what were the needs involved. I think EMBL really came into its own substantially in the 1980s, maybe even you could say ... yeah.

AFL: So did you ever work under Lennart Philipson actually? Did you ever know Lennart?

EW: No, 'cause when we left, and then a year or so later, '81 was also the time when Sir John's contract was ... I don't know quite how those things happen, and there's something that you want to figure out from the archives, there was a sense, I believe, that it was a sense of him not to continue as a Director and return to Cambridge I think, and figure out for the next phase, now that the building existed, now that the structures were there, that you needed someone with a different skillset. I think he had ceased to be a scientist. He was too old ... he was ... whatever, he ... and I think he seemed to be somewhat withdrawn and in a certain sense shy, so he has to do things and he could do things publically and he enjoyed doing things publically, he enjoyed the public role but it was a public role for a man that didn't involve the kind of spontaneous sense of enthusiasm for science that you would like to have, for us at least it didn't involve that spontaneous enthusiasm for science that you would like to have from a Director, then I think that Philipson and Fotis and subsequent directors have been able to supply. And that, I think is the potential ... criticism, or potential sense of Sir John's period as a Director.

AFL: So that's all really interesting perspectives of that time of EMBL, but coming back to you actually, what got you first interested in flies?

EW: Oh gosh! OK, so as an undergraduate I had been interested in embryos, in frog embryos, I worked with chick embryos, I took the embryology course at Woods Hole in 1969 before going to graduate school.

What got me interested in flies? As an undergraduate I needed money and so I got a job making fly food and washing bottles for a couple of years and gradually worked my way into Harvey Bender's lab as functional. So this was '68, '69, in the United States this was the middle of the Vietnam War. I was politically active enough and I was applying, actually I applied for a conscientious objector status <chuckles>. In Alabama, no one had done that before in my little district, they were

thrilled to have an applicant to do this, but they were not really willing to give me the status, because it had to be religious basis and although I was, at the time, actually religious and went to mass every day because I was at a Catholic school, they didn't see conscientious objection as a serious part of Catholic theology.

<Laughter>

And so my boss at Notre Dame, Harvey Bender, was ... and I was <laughs> this was back when you were young and you're kind of into prison literature and you can do all this idealism, and so he ... I'd applied to graduate school and he wrote to the only person whom he knew at Yale, who was someone who worked with flies, a man named Don Poulson, who as a student of Alfred Sturtevant <30:00> and in the thirties had actually described *Drosophila* embryology. So I arrived, we didn't have rotations, I was put in to Don Poulson's lab, Don Poulson wanted to retire, he didn't want a graduate student, but he took me and I learned that flies had embryos and so I learned everything from the great master. And then Poulson arranged for me to work with Walter Gehring. I went over to Walter Gehring. Walter decided to go ... and so then everything was fixed.

My path as an embryologist ... Poulson knew embryos, Walter didn't really want, Walter wanted molecular biology, he didn't really want embryology, but that was what I do, that was what I did. It's still true that Walter asked me to come with him, I was the only person who came with him from Newhaven, but once in Basel I became the fly embryologist off in the corner working on flies, and so lineage and happy ... in a way amazingly happy, I don't understand ... I think ... I must have had some inner sense of security or something, or some inner sense or some belief or some love enough of what I was doing to not go through the drama or questioning ... I didn't talk to Walter anymore and I just did my stuff for the last two-and-a-half years and yet I did it, I enjoyed ... it was somehow that enjoyment, you kind of find, yes, this is what you're good at, this is what you wanna do, this is how you're gonna ... this *is* the thing that you're gonna do. And I think that when Janni arrived I think more than anything that was what attracted her to me, formed one of the bases of our friendship, was that she arrived, new molecular biology, felt insecure, didn't know embryology, had pressure on her to grind up and isolate more DNA binding sites and chose not to do this, 'cause she's very tough. But still that idea, that there was me off in the corner cutting up and grinding up little embryos and trying to label cells and stuff like that, was something that was probably attractive to her. So that was how we kind of bonded and then we spent time talking to each other.

AFL: That's fantastic. And so yeah, you've stayed with flies since obviously.

EW: Yeah. Well partially it's that you ... you learn how to do things. I still work maybe four hours a day at the bench, so I *do*, I've always had a small lab, I always work and I always do stuff. And I got into the business, not 'cause I wanna make opportunities for other young people, I got into it because I wanna do these things. And so my lab functions and there are people, they actually do the good stuff you

realise! But it's part of the aspect is that the lab kind of functions efficiently because I wanna do stuff. And so if I were to switch to mice or to fish or something, it would cost me much more, I wouldn't be able to do things ... and it's also true that, 'cause there are certain things where I can still compete with my post-docs, and I'm good at those things! And if I were to go and ... but that's just 'cause I [??? at 34:24] them and I know how to do stuff and I'm milking it for all it's worth in this little competition thing! And so my post-docs, they come from other organisms or experiences and they may be smart and they may have skills and they may know all these things, but I work with flies so I can still compete in terms of producing results. And so ... if I didn't work with flies that wouldn't be true. And so I don't wanna give up this little advantage that I have. If I wanna continue to do science, then I <35:00> want that.

AFL: Fantastic. So we've gotten to the end of your EMBL time. Can you just, for the context, just recap what happened after you left EMBL?

EW: OK. So I applied for jobs, got an offer from Princeton, decided that I would take it, told Janni, she knew that I wanted to leave. She was considering staying so she suggested I talk to Sir John and that Sir John would make some kind of counter-offer or something like that, or whatever. So I went and talked with Sir John. That meeting did not go well. Our relationship with him, Janni and my relationship had deteriorated to a point where his response was well, yes, he was hoping that I would be successful later but that I should tell Christiane that she should not feel that she could expand into the half of the room that we were occupying when I left, that he would initiate a search to find someone else to put into that room. And so when I got back she asked me and I actually told her that, and within about two months she had arranged to leave Heidelberg and EMBL, and we had finished the screens, we were doing these things to move to the Friedrich Nietzsche in Tübingen, and so she left and I actually ended up staying about three, four months in the lab alone before I then left for Princeton.

AFL: Can I ask what this relationship with Sir John, what happened over these few years? If you're willing to ...

EW: OK. I think ... <pause> my deeper sense of the situation, the plan to build EMBL in the early seventies, molecular biology was coming, no one knew what it was going to be, and the view was that it was going to be *big* and so you had to be something like CERN or something where you would have to go and be the place all over Europe where you would come to do molecular biology. And by the time everything had been finished and they'd hired the people and moved in and had the building finished and everything, 1978, molecular biology had actually happened, and it was not big, it was small, little things and so no individual member nation was willing not to do molecular biology and every major research institution from Pasteur to Cambridge or Oxford, in their hiring, would try to identify the most promising person in the country and offer them the job. And so this is something that you can't ... not sure it's really fair to say but there's sometimes a worry that the people who were hired in the first round were not necessarily the golden boys in each of their individual countries, but the second runner ups. I think that's possible, a way of

thinking about it. There was a slight element of that, and so that led to more rivalry. I think that did lead to more rivalry among individual groups, and people like Kai were able, because of their natural leadership qualities and their natural generosity, were able to somehow form a unified group and bring people together and have a common goal, and Kai has this extraordinary quality, that's what he did, he was remarkable. But there was nobody like that on our floor. And we were kind of young and <40:00> ... the relationships between all the individual scientists deteriorated and I think that John ... was sensitive to, it seemed to me that there were certain ... that people were friends with him or not. And we were not good enough about being friends, and we were young and naïve and I think Janni even more was prickly and insecure at the time, and so she had a negative opinion of some people and she's not able to really control that negative opinions of people, and so there she is, this young ... saying these things, heckling, or not heckling but you know ... and so that kind of charged the situation. I think we were both sensitive to not do service for people ... and overly sensitive in that respect, and then we did this one terribly disastrous thing, which really killed our relationship to Sir John. This is the moment where we're actually gonna document on this tape something that we have to decide whether we suppress later or not!

After about a year, Sir John had decided to initiate a special lecture series where he'd bring in famous scientists from the different member nations so once a month would be this special thing, and since Germany was the host country, from Heidelberg, he decided the very famous German Scientists Alfred Gierer. Alfred Gierer did a number of things but he also developed a model for pattern formation, the Gierer-Meinhardt model. Janni knew Gierer very well, I knew Gierer a little bit. He gave a talk and we were not on his schedule but he asked if he could come by and see us. They had arranged, for between his talk and he had a little bit of free time, so he came over to our lab and we were sitting around and he was to go to dinner and ... <sighs> so Alfred, he was there, and then I think Bernhard Dobberstein came by to pick him up and we were talking about the screens and the things we were doing, and then he said, Gierer, 'Why don't you come for dinner?' <Chuckles> 'We're going to the [??? at 43:06] it's a restaurant in Heidelberg. And <laughs> he leaves and Janni and I were sitting there, we did not understand what we should ... and I can't believe we did this, but we did, this is terrible – we said, well let's ... he said we should go ... and so we showed up at [??? at 43:42] and ... it was horrible. It was absolutely horrible. John had his ... and this was important, this was the first sense we had his favourites among the group leaders there, and then he had one or two of the assistants, so there was a couple of the women who were ... administrative, and it was a table that was already crowded and then we showed up and he had to push some chairs and move these chairs in and then we were all kind of crammed like this and it was awful and then we realised it was a dumb thing to have done, a totally dumb thing to have done. And so then kind of <laughs> left, 'We have to go back to the lab, sorry. See you!' And then ... it was a stupid thing to have done, particularly, I believe, did offend Sir John, because we were already, he already knew the abrasive relationships in the labs and were already somewhat labelled in that respect, so we used to meet every other week Friday, the group leaders, <45:00> and he actually announced that ... EMBL's growing, we have more and more group leaders showing up ... and it may not be so conducive to discussion and so perhaps it wasn't really necessary

for all group leaders to attend the group leader meeting all the time ... and particularly if there were cases where two group leaders were occupying the same room. Of course there was one case with Brigitte Jockusch and Graham Warren where they each had ... but it was important that they would both continue to come.

AFL: So pointed tensions let's say!

EW: And we were never really able ... and I think it certainly ... things could have been better, we could have done better. The tensions on our floor weren't really good for the institute as a whole. It was one of the reasons why I decided that I wanted, one of three or four, a bunch of reasons why it made sense for me to leave, but that was among the reasons. I think Janni wasn't quite so sure but ... she's a remarkable scientist and was insecure I think whether moving from Heidelberg would be a good thing, but once it was clear that staying in Heidelberg was a bad thing for her, she ... focussed and had enough admirers and people who appreciated her work that it became possible for her to move out of Heidelberg and move out of the EMBL, and it was just an unfortunate circumstance. It was the greatest period of our lives, scientifically, wonderful opportunities, it was for me and her, we worked together, it was like a great thing. It was a little bit messed up in that respect. And so I think it didn't have to be that way but ... it was probably coloured by everything, it was coloured by how we got the jobs, it was coloured, certainly an important thing for us both was that in spite of the tensions in the way that we got our jobs, that we maintained our friendship. So we hunkered down if you will, and were defensive of each other in ways that ... may not have helped the whole social circumstances in the institute as a whole. So we profited so immensely from our time there, and ... but obviously ... leaving didn't hurt us.

AFL: So when she was in Tübingen and you were in Princeton, clearly your collaborations continued.

EW: Yeah, not quite as easily but ... and we remain very close, and because ... we had decided that ... Trudi had decided, my wife, Trudi Schüpbach, had decided that in going ... she'd come up to Heidelberg, we were in the middle of the zygotic screens and had been involved in looking at the rare survivors to see if they had maternal effects or actually sex determination, which she was interested in at that time, but she decided she wanted to do maternal effect screens. When she came to Tübingen, Janni had decided, and she continued on with her work on the second chromosome, Janni continued work on the third, so there was that other ... there are all of these things ... we were giving out. So the whole fly community and the whole molecular biology community was very fluid and open at the time, so it was very easy to maintain that type of collaboration.

AFL: That's fantastic. This is really great, Eric.

EW: <Laughs> It's all the dirt, comes from years ...

AFL: It's context, and it's how science was back in those days, and it's got so many different facets.

EW: Absolutely. An interesting time. It was, for me, like my big growing up as a <50:00> scientist. I have been at Princeton for 40 years, there is nothing that's ever happened to me in the administration there, the whole aspects of being a faculty member of an American university, that I wasn't prepared for from my years in Heidelberg!

<Laughter>

Everything's been so wonderfully smooth and everybody thinks I'm such a calm, sweet person at Princeton! They have no idea how much I've been run through this whole formative experience and became pretty much who I am, another way that I probably profited greatly from the years that I was in Heidelberg.

AFL: Fantastic. So just as one last question, what's a piece of advice that you'd give to a young scientist starting out today?

EW: Hmm.

A piece of advice is that you ... it's hard to know what other people are going to think is interesting, so if you make your choices based on that, it's a dangerous game. It's probably better to find the things that you think are interesting and that you think are doable <chuckles>, and do that. Different scientists are different in the ways that they approach things. I am horribly ... very reactive at what I think is doable at the moment. I don't have very high ideals in science in terms of I'm more driven if I see this is something really interesting and we could solve this problem this way and I can do this and I wanna do this and I'm gonna do it tomorrow, is kinda my approach as opposed to this is a really hard problem and I'm gonna work on this and we're going to solve this thing and I'm confronted with an opportunity to do something interesting, if I see this opportunity I will do it. And so some people are like that, and so if you're like that, then that's what you should do; but if you are ... I think Janni for example has a certain vision of what she wants and has a harder time compromising, in the sense of I will do something, I will figure out how to, I will do a screen in zebra fish which is going to involve coordinating eight post-docs, and she will figure how to do that if that's what her vision is. And she's probably not opportunistic, as opportunistic as I am. Both ways is kind of a good way to be scientist. As a scientist you have to figure out who you are and then do that. And who you are and how you can be successful. Who you are and what you are points to a certain path for being successful and then follow that path.

AFL: That's really fantastic. I think those are great closing words so I'll stop the recording now. Thank you very much.

EW: OK, good. OK.

<End of interview>