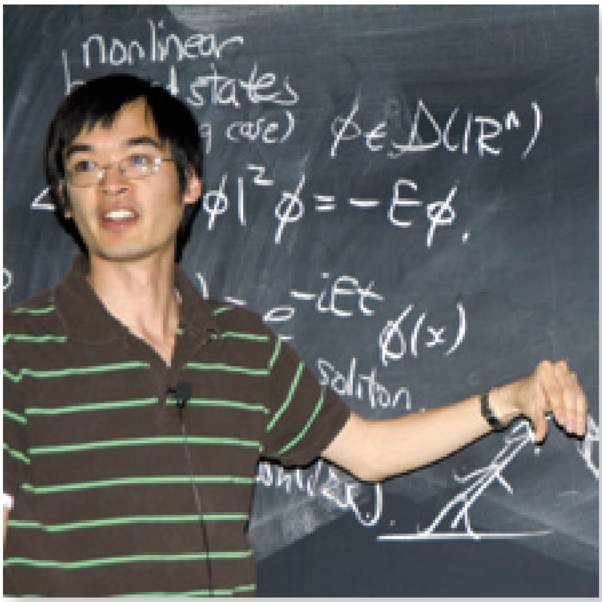
Terence Tao Talks to Robert McLachlan



Terence Tao

Terence Tao has just toured New Zealand as the inaugural Maclaurin Lecturer on behalf of the American Mathematical Society. He was interviewed by Robert McLachlan in Palmerston North, August 28, 2013.

Robert McLachlan: Thanks for doing this speaking tour of New Zealand as the first Maclaurin Lecturer.

Terence Tao: It was good timing as I was actually planning to go on vacation to New Zealand when I was contacted.

RM: Apart from your Fields Medal and some of your famous work I didn't know a whole lot about you, so I read about your childhood in Adelaide as a so-called child prodigy. It seems to me it's quite unusual for a child prodigy to grow into a top scientist. Is that a fair statement?

TT: Some do some don't. I mean I know lots of mathematicians, some of them had accelerated early education and so forth and many didn't — many good mathematicians only started getting into maths as undergraduates, or even as graduates. There are all sorts.

RM: So would you say there was something special in your early childhood environment, not that made you a prodigy maybe, but that let you continue developing?

TT: It's hard to say because I only really have my own experiences, though one thing I can say is when I went to Princeton as a postgraduate — I graduated from Flinders and I was used to very small universities, I was in the honours class which was very small, as small as three people, and it was sort of easy. I got in the habit of waiting until the last week of classes before actually studying, I'd take very sketchy notes for the whole class and not really pay attention and then cram at the last minute and pass, pass with not that great grades sometimes. When I went to Princeton I thought I could pull off the same thing. But somehow the level of difficulty was so much higher — the qualifying exams in particular for my postgraduate studies.

RM: They're famous, aren't they?

TT: Yes, they are oral, they are two hours and they are terrifying, but I thought okay I'll just study for a week before, and I did appallingly badly on my orals, very soon they poked huge gaps in my knowledge. I was only saved actually because you pick three areas of mathematics for your orals and I picked analytic number theory as one of my subjects. The person who was going to quiz me on that thought mistakenly that I had said algebraic number theory, which is a very different subject and so he didn't have any proper questions prepared so he could only ask very easy questions for that subject, so I answered those questions very well, but my core field subject, harmonic analysis, which was my specialty, I actually did prepare, well I thought I did and I really did embarrassingly badly on that, and my advisor came up to me afterwards and said it was rather disappointing that I had such good recommendations. (Laughter) What this did for me is that it really changed my life, because from then on, I was so ashamed into actually studying and working hard and I think for many people who are talented at an early age sometimes things are a bit too easy for them and they don't pick up good study habits and for the long haul if you actually want to do research, it's actually not so much your initial talents, it's really more your perseverance and maturity and your ability to stick at something for years which in the long run makes a difference. So there is a transition, and some people can make it and some don't.

RM: What was your PhD study like, were you basically left alone to work on your research project?

TT: Yeah, Princeton was famous for being very sink or swim, at least when I was a student there. Their attitude was we only admit the best and so we don't need to train you that much further, you already know what to do. So you come in, here's your office and here's your computer, here's your keys to the library, you've got an oral exam in two years and you've got to do a thesis in four, see you then. There were classes but they were basically not mandatory and so the students basically taught themselves, we would organise little informal seminars ourselves, there'd be classes but we didn't go to them. For some people this didn't work out very well so many people dropped out of the programme. It was actually a really good environment because it was so free form, so it worked out well for me. I think one thing that helped is that Princeton being a very small town there's actually not much else to do other than go to university and study and so forth. So you're obliged to go to the department and you go to the library because there's nothing else to do. The first two years I was there, I was there from 1992 to 1996, I discovered the internet and that took away a year of my time. (Laughter)

RM: I went to Caltech in the 80s and it does sound a little bit similar.

TT: Different universities are different, UCLA is much more structured. We have lots of course requirements and people are likely to check up on you.

RM: That must be a very large programme?

TT: About 50 full time staff and a hundred odd postgrad students.

RM: So how did you settle on harmonic analysis?

TT: At Flinders when I was an undergraduate I did a masters with Garth Gaudry when he was there, he was a harmonic analyst. I always liked analysis, sort of estimating things. I liked epsilons and deltas actually, more than other aspects of mathematics. I also liked number theory, but . . . it was intuitive to me, big and small and convergence, these things kind of made sense to me whereas geometry, topology and algebra, I have to think a lot harder to get my head around them. When I went to Princeton I was choosing between number theory and harmonic analysis and I went to lectures by the harmonic analyst Elias Stein and he's an extremely clear lecturer, he prepares very well and like every lecture he proves one theorem and it's all set out perfectly and I could understand everything and it was all so self contained. I went to number theory classes with Peter Sarnak and Nicholas Katz and they were great and we were learning really cool stuff, but the amount of knowledge that they presumed, I remember in one of the first classes I went to there, they said so you are all familiar with the representation theory of SL(2, Z), right? No! (Laughter) So I got intimidated out of number theory, it was too much work for me and at that point I didn't have the study habits to actually catch up, so . . . .

RM: So in a sense you got your own back?

TT: Yes, afterwards, most of the math I know now actually I learnt after my formal education. Actually most of my coauthors and collaborators — I learnt PDE by working with PDE people, I learnt number theory from working with people like Ben Green and so forth, somehow my whole education has been in a jumbled order, I skipped all these grades . . . .

RM: The story that Rodney Baxter tells is that of the two things he's most famous for in his whole career, one he did when he was unemployed on the way out to Australia and the other one he did when he was unemployed because he had retired. (Laughter) So looking at your vast list of papers in so many diverse areas, basically they are all analysis, would that be a fair statement? You've taken hard analysis into sorts of areas where it maybe hasn't gone before?

TT: Right, yes, although more recently I'm getting to appreciate that the other areas of mathematics and science are very important. I guess from algebra and logic and so forth, but I always, you know, analysis has always been my home base, but when I try to learn another subject I try to translate it into epsilons and deltas, because that's just my language. So if I had to pick a field I would say analysis, that is what I do.

RM: So the move to UCLA was then extremely influential, that was maybe the right place to go.

TT: Yes, it was fortuitous. When I finished from Princeton I applied for jobs and I got three job offers, one from UCLA, MIT, and one from New South Wales, and my advisors convinced me to stay in the States, just because it's a larger math community, and I chose UCLA for two reasons, one because it's slightly closer to home. Flying from Princeton is not very easy. Also it was sunny. Four years in Princeton got me sick of snow, but also at the time there were three harmonic analysts in the faculty and that was the field I had done as a PhD, although when I arrived actually two had left, not because of me, for different reasons, but actually that worked out well because then I started talking to a postdoc instead who was working in PDEs which was an area which I had sort of vaguely had had some exposure to but not . . . I got interested in applying methods from harmonic analysis to PDEs which was sort of a fashionable topic at the time. So from him I started brushing up on PDEs. So when I left graduate school I was very narrow, I basically knew harmonic analysis, and just a small amount of other mathematics but I found actually that collaborating with people in other fields was actually for me a lot more fun and I learnt a lot more and I just kept doing that. I think I collaborated with people from representation theory, number theory, combinatorics and so forth. I find that these sort of help, somehow better suited to my mathematical taste or style than staying in one field and becoming an expert in one field. Knowing everything about one subject, it's not something I have the patience for, I'd much rather collaborate with someone who knows everything about one subject.

RM: There's also a bit of grand theory building though, like these recent papers on random matrices and universality. Of course that's all analysis theorems but at least you were kind of hoping or dreaming that was going to be a new greater theory of universality.

TT: Yes, that's the dream. There is whole phenomenon, you can see numerically that all these different matrix models, which have no *a priori* reason to behave in any similar fashion, but if you look at their eigenvalues or whatever they have almost exactly the same distribution. Much like the central limit theorem in probability, you average together a whole bunch of random variables, it doesn't matter that they are discrete or continuous random variables pretty much you are always going to get a Gaussian when you do so. It's this universal limit. We are seeing these universal limits in matrix theory and in random Schrodinger operators and number theory and random permutations and so forth and it's not completely answered why we have these universal limits. We can compute them and we have some where we can prove that two cases give you the same limit, but . . . .

RM: So there should be an underlying phenomenon that they are all exhibiting.

TT: Right, like for the central limit theorem there's lots of explanations, the Gaussian is the thing that minimises entropy, or maximises, I always forget which way it goes, it extremises the entropy for a given variance — it's very stable in various ways, so it can be viewed as an attractor of various processes and so we have very good intuition as to why the Gaussian is the output of the central limit theorem, but we don't yet have the same sort of explanation. So this is one thing I'm very interested in, in seeing if I can help figure out the answers.

RM: And there's this tantalising connection with the Riemann hypothesis. Is it a sensible question to ask if it could be proved along these lines?

TT: Right. The zeros of the zeta function seem to have the same statistics as the zeros of random matrices. I think the reason why, well, we don't know why exactly, I think it's not so much because there's actually a random matrix behind the zeta function, it's just that these limiting distributions are so universal, they should actually be your default guess as to what these distributions should be. We know that Gaussians appear everywhere in mathematics — if you create any sort of distribution, even if it's not coming from probability — if you just had to blindly guess what kind of distribution you would get, people would expect a Gaussian, they're not surprised any more.

RM: So that suggests there might not be a direct connection to the Riemann hypothesis.

TT: No, not between the Riemann hypothesis and random matrix theory, but if we had an explanation for universality, if we had a set of conditions or general principles that whenever you have a set of points on a line, whenever these points do such-and-such, they should naturally be distributed according to what's called the GUE distribution, the distribution coming from random matrix theory — then you could try to apply this general principle to the zeta function. It's probably coming from some assertion about the primes, that the primes are distributed in a very random fashion, the most random way to distribute the primes, the one that maximises entropy or something, and this should naturally give you these GUE distributions. There should be some explanation which we don't have yet. I think it is conceivable, not that we can prove the Riemann hypothesis yet, but this GUE hypothesis, the random matrix distribution for the zeta function zeros, we should be able to explain this in terms of other conjectures about the primes, which we also don't know how to prove, but I think we can at least make all our conjectures consistent with each other.

RM: A lot of the recent progress, like with arithmetic progressions in the primes, and with the prime gaps theorem, are more from using traditional techniques in a very complicated way — from working harder.

TT: Right. Somehow the primes are so hard to understand directly, that the way we make any progress at all on the primes is by taking the primes out of the problem as much as possible and working on other aspects. For example, my collaborator Ben Green likes to say that in our main result about arithmetic progressions in the primes, the key insight is not to try to understand the primes better, but to understand arithmetic progressions better, and to understand what kind of sets contain arithmetic progressions, and what kind of sets don't. Not to try to figure out the primes as much as possible, but to find some abstract criteria on a set which would guarantee the existence of arithmetic progressions, and make those criteria as simple as possible, and only then do you think about "do the primes obey these criteria?" You try to keep the primes out of it because they're just so difficult. Our approach is coming from analysis and combinatorics and so forth and not so much from number theory.

RM: Part of your work that I have the least feel for is PDEs — every PDE is different and there is this forest of different exponents and different cases — for an outsider it's hard to see where it's going. It's too much like chemistry.

TT: Right. I know a top PDE person who refers to the theory of PDEs as the Balkans. There's an infinite number of PDEs and most of them are uninteresting. You restrict to the PDEs that are physically or geometrically interesting. There are so many types of terms you can put in a PDE — physically, they all correspond to some phenomenon, dispersion, dissipation, transport, energy minimisation. Depending on the exponents and signs of these terms, some are dominant. The type of PDEs I used to work on a lot are nonlinear dispersive equations, with a linear dispersive part. If you take a wave it will spread out, like water waves, or sound waves, or Schrodinger waves. The energy is conserved, the wave doesn't die down to zero, it doesn't dissipate but it spreads out, it disperses and decays. But then there'll be this nonlinear part where the wave can interact with itself and reinforce itself and maybe get stronger and stronger. There would be this race, if you watch the wave evolve, the nonlinearity might be trying to focus the wave and make it stronger, and the linear part's trying to spread it out. The whole subject is focused on trying to see which side of the equation is stronger, which one wins. Sometimes, if your amplitude is small enough, or if your exponents are small enough, then dispersion wins, other times the nonlinearity wins and your solution focuses and can blow up.

RM: You're trying to do this without dynamics, just by interrogating the equation at a point, rather than how you get from one point to the next.

TT: Right, we've been trying to use dynamics techniques — it's infinite dimensional dynamics, so already it becomes very difficult. Even in ODEs you can have chaos, it's hard to say what the longtime behaviour is, so it's always been difficult to use dynamic system methods. This is presumably where the future of the subject should be. We rely much more on things like conservation laws.

RM: The symmetries, the conservation laws, the equation, and very little else.

TT: You can squeeze a lot out of that! For many decades we've been squeezing everything we can out of things like that. Conservation laws are great because they hold for all time, they're one of the few things that you can say for certain about your solution way into the future. There's a couple of other things, there's monotonicity formulas, and you can squeeze a lot of information out of these laws, but not everything, because there's an infinite number of degrees of freedom and a conservation law just constrains one of them. There's a lot we don't understand, particularly in so-called supercritical equations where the nonlinearity is stronger. This means the nonlinearity if it so chose could make the equation blow up and develop singularities, but maybe it chooses not to. With Navier–Stokes for example, the equation is nonlinear enough that if everything went exactly the wrong way, it is conceivable that a singularity could form, but in practice it never happens, but we can't stop that from happening. It's like the digits of π, conceivably you could get a whole string of 7s, you could get really odd patterns in the digits which are not consistent with uniform distribution.

RM: The blowup could be for highly exceptional initial conditions.

TT: Right, that's another thing about PDEs: most of our techniques are deterministic, which means they work for every single solution. Those are the types of tools that we use. As a consequence, whenever we prove existence or whatever, we either prove it for all data in a certain class or for none. Whereas what we should be proving is things like, "for almost all data in a certain class, something happens". It is quite conceivable for Navier–Stokes that for almost all data things are good, that's what we see in real life, but there could be some exceptional bad set where things go wrong. But we don't have the tools. There are some tools in dynamical systems, like invariant measures, that should in principle help us, and there has been a little bit of movement in that direction. That's another future direction for PDEs.

RM: I read this great quote of yours in your paper "What is Good Mathematics" [Bull. AMS 44(4) (2007), 623–634]: "It seems to me that the pursuit of such intangible promises of future potential is at least as important an aspect of mathematical progress as the more concrete and obvious aspects of mathematical quality [listed previously]." Are you saying that we should just trust our intuition and our experience and just go for it?

TT: This rhetorical question, *What is Good Mathematics* — by the way it was not my choice of title, I was solicited by Susan Friedlander to write an article on *What is Good Mathematics* — what I'm trying to get at is you can't prescribe in advance what it is. Mathematics is a basic science. If you do cancer research then you've got this obvious goal of curing cancer and so you can structure all your research programme around that, but there's nothing like that in mathematics. The key problems, the key things, the key questions that you should be asking you only find out along the way. For example, one of the central problems in number theory is the Riemann hypothesis but if you're just starting out in number theory it is not obvious that this is the important problem, this is something that you discover along the way. It just keeps showing up. There's a limit to how much you can use metrics — how many theorems have you proven or how many applications have you got, or what impact factor, or whatever. There's a lot of serendipity in mathematics — someone pursues some crazy idea which by all the sort of standard metrics doesn't seem to be fruitful, doesn't connect with existing results, doesn't have an immediate application or something but they have some vision that this could be something interesting, something unexpected about this direction that's worth exploring further. Sometimes it doesn't pan out, in fact often it doesn't pan out, but occasionally really unexpected breakthroughs come out from that field because someone saw something which just smells funny.

RM: There's also serendipity in terms of someone knowing two unrelated things and being able to see that there is a connection. With modern search and communication could it be possibly easier to do that in future?

TT: This is certainly a big cultural change, mathematics has become much more interdisciplinary, much more collaborative. I have seen it in my lifetime but if you read about mathematics in the 30s or 40s or 50s, it was much more secretive and individualistic. The subfields of mathematics didn't talk to each other nearly as much as they do now. For its time maybe it was the right thing to do because each separate field, algebraic geometry, functional analysis, they were still maturing. But they reached a level of development where to make further progress in one field you really have to import ideas from other fields and so people have opened up now. With the internet there are these amazing new ways to find out things — it used to be that if you had a problem in, say, algebraic geometry and you wanted to contact a geometer, you'd have to basically go to your department and talk to your local algebraic geometer to solve your problem. But now, for example, there's this great question and answer site on the web called Math Overflow, have you heard about that?

RM: Yes, it's very successful and the previous attempt 10 years ago to get that going didn't work, nobody was there, so it's great that this one is actually working.

TT: Some of the technology is finally there to make it work, it's hit the right balance of being easy to use and not overwhelming — you can sign into it and just look at the questions you are interested in. It's one of many sites that do a great job of matching up: you have a question which is obvious to somebody but not to you, but you don't know who that somebody is.

RM: It needs a critical mass of people reading it, doesn't it? It seems to have reached that.

TT: Yeah, it works, it's a great success and there's also these Polymaths projects.

RM: Like the Prime Gaps project, it was so fast, it was phenomenal!

*TT:*So fast, because the project naturally identified the people who could best contribute, there's about a dozen of us who are involved in that and we have managed to make a lot of progress.

RM: There were a small number of key people really contributing a lot, right?

TT: We don't want to fight and divide who the person is, it's sort of counterproductive. The thing is, before the project started if you asked me who would be the best people to make progress on this, I wouldn't have guessed half the people in there. So I think part of the strength of the internet style of collaboration, is that they can identify who they can collaborate with, it's not just people you know, or the people in your department, or whoever you just happened to meet at the conference.

RM: Polymath is new, are you hoping that it will grow and become thousands of people involved in many projects?

TT: It's still a very boutique project right now, I mean there's only 8 projects and not all of them have succeeded. We haven't yet sort of found the secret sauce that somehow guarantees that it works.

RM: The right problem.

TT: Picking the right problem is important, and like a traditional project, sometimes it doesn't work. But one down side of traditional research is that if you try something and it doesn't work you just put it in your drawer or your computer and you forget about it and no one knows. The point is to get it out in the open, say "I'm stuck", yes it can be a little awkward, but also that has some value. Some of the feedback we got from the first Polymath project was from postgrad students saying thank you for showing how research really works, how much failure is involved, how many dead ends and backtracking and "Oh, that was stupid"s before they hit on the right path. Because when you write up the results you don't go too much into all the dead ends and things you tried that were embarrassingly wrong, but those are an important part of the process. Often you have to make the obvious mistake and say, "Oh, okay, I should not apply Cauchy–Schwarz before I do this", or "I should not use this lemma, unless I can do this" and once you know the obvious mistakes then you can say "Oh, then I can proceed if I do this first, this first and this first" and then you find the right path, but then when you read the paper people just say "Oh, I will pull this out of the hat", and it all magically works and you don't see where it came from — the guy must be a genius! — but often it's because they tried all the obvious things first.

RM: You've also been involved with this debate over maths publishing and you were involved early on in the Elsevier boycott, is that correct?

TT: I am part of the Elsevier boycott, I've stopped publishing papers in Elsevier journals.

RM: It's a situation that's in flux and apart from the fact that there's a lot of unhappy scientists, it's very hard to see where it's going to go.

TT: So this is the problem: everyone agrees that somehow the current status quo is unsustainable and it's somehow ridiculous that we have got to this point, that we give up our own research. We work on our own research, we give it to journals who give it back to us to referee and then we have to pay them to get them back to read. It evolved from a decision that did make sense, when the societies were in charge of journals and because of technology there was an important service for journals, to actually distribute papers, but now distribution of papers is basically free, you can do it online. I almost never go to the library anymore, most papers I can find on the internet from the arXiv or whatever. But journals still perform two irreplaceable functions, one is refereeing and the other is certifying for the purposes of promotions that this person is doing research.

RM: Both of those are a bit problematic.

TT: Journals do them imperfectly, but no other system that we know of does them any better, so that's a problem. Somehow we've given the control of many of our journals to big commercial companies who don't care so much about the academic, they're focused on maximising profit, so there's this mismatch of different incentives. There's a lot of experimentation with different types of journals now. There's low-cost journals and there's even zero-cost journals with everything online, the authors do their own typesetting. Then there is open access, you pay the journal some fixed amount and the paper is then freely accessible for everyone, or you just put the paper on your own website or the arXiv. The journal is basically a whole bunch of links. So it's not clear what will work.

RM: Has anybody stopped publishing in journals and only releases preprints?

TT: I have a lot of mathematics on my blog which I do convert into a book, much of it, but there are lots of math blogs now with quite serious math content, where they don't end up publishing it in any formal venue. Now one downside to that is it's a bit more difficult to cite. Every so often someone will want to cite one of my blog posts because I have something which is not contained in a formal article. One nice thing about a journal citation is that it's permanent. If you cite Annals of Mathematics 1986, they're never going to change the content. But a blog can change. So it's not a perfect replacement for the journal system. People do cite arXiv preprints quite frequently now. There needs to be some sort of cultural shift. Part of the reason why everyone still uses journals is because other people only accept mathematics as sort of finished or certified if it comes from a journal. Somehow we need this critical mass. Once we have a good alternative system which a critical mass of people start accepting, then it can take off. The arXiv is like this. A huge fraction of mathematicians now use the archive.

RM: Huge?

TT: Why not?

RM: More than before, maybe, although mathematicians were certainly slow adopters.

TT: It depends on the field, I know some fields where it's almost total, but certainly everyone's heard of the arXiv and there's no controversy about citing something from the archive, it looks like it's here to stay.

RM: The other players are the employers and the research funders, they could press for open access as some of them do.

TT: The best solution is to have these consortia where the top universities and the funding agencies fund the open access journals for researchers in those countries. Right now, funding agencies have to pay universities for library costs to subscribe to journals, millions of dollars in some cases — those same funds could be used much more cost effectively to fund open access journals but it's a different pot of money and so you can't just transfer it so easily. People are trying all kinds of experiments, its not clear what's going to work. One experiment that just started up is something called the Selected Papers Network. What you can do there, you can take a paper online from the archive and anybody can write a comment on the article, pointing out citations or pointing out maybe an improvement to a lemma or something and the thing is you can post your comment on one of the social networks, like Twitter or Facebook, Google+, and this network will just collect all these comments and put it in one location so every time you go to look up a paper online you can, if you want, also look up all the comments. So this is not quite the same as peer review, it could be different.

RM: Sort of a facebookisation of the project.

*TT:*Putting it that way it doesn't sound so good! As you say it's in flux, there's a lot of possibilities for what could happen. It is sort of clear that things have to change. Already many small libraries can't subscribe even to all the must-have journals in a field. I think there's a lot of people who once they know of an alternative which already has enough acceptance that it can be used for things like refereeing and promotions and so forth, there will most probably be a dramatic shift. We are not at that stage yet.

RM: Getting back to education, do you have PhD students?

TT: Yes, at any time I have like four or five students plus one or two who are talking to me about maybe becoming my students.

RM: Do you like to have them in all different areas?

TT: Yes, I try to keep them apart from each other and to some extent apart from what I'm currently working on. I think it's important that students' first work should really be their own. I'm a great believer in collaboration in general, just not for your very first paper.

RM: They're mostly working independently, is that your philosophy?

TT: Yes, they meet me once a week, they talk to each other, but I kind of want my students to develop some maturity and independence so they shouldn't expect that even after they graduate I should still supply them with a source of problems to work on, they should be able to have their own research projects.

RM: It's sort of a critical time but you don't realise it at the time, you don't see the choices that you are making and how influential they are going to be.

TT: Yes, for example I know several students who keep trying, they reach their fourth or fifth year and they've already done enough to get a PhD but they are kind of afraid to go out in the real world and become a postdoc and be responsible for their own research. "Can I stay here for another year?" and have this comfortable life as a graduate student where you don't have responsibilities. Sometimes you have to actually push them out a little bit, it's actually better to start, if you finish one year earlier then everything else happens one year earlier too, you get your promotions one year earlier and so forth. It's better to go out once you are ready. It's true when I was a grad student I had no clue how the academic world worked, I did what my advisor told me to do. Fortunately I had a good advisor.

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