Episode 90: Ben Hatton

# KL: Katie Linder

# BH: Ben Hatton

# KL: You’re listening to “Research in Action”: episode ninety.

# [intro music]

# Segment 1:

# KL: Welcome to “Research in Action,” a weekly podcast where you can hear about topics and issues related to research in higher education from experts across a range of disciplines. I’m your host, Dr. Katie Linder, director of research at Oregon State University Ecampus. Along with every episode, we post show notes with links to resources mentioned in the episode, full transcript, and an instructor guide for incorporating the episode into your courses. Check out the shows website at ecampus.oregonstate.edu/podcast to find all of these resources.

On this episode, I'm joined by Dr. Ben Hatton, an assistant professor in the department of Materials Science and Engineering, and cross appointed to the Institute of biomaterials and biomedical engineering at the University of Toronto. Dr. Hatton is an engineer who obtained his PhD in 2005 from the University of Toronto in the area of nanomaterials synthesis by self-assembly.

He has worked extensively on the design fabrication and properties of Nano and micro structured surfaces for biomaterial applications and worked at Harvard University for five years as a postdoc and research associate before coming to the University of Toronto in 2012. Dr. Hatton currently has over 40 peer review publications and over 50 presentations and conference abstracts and is focused on investigating the relationships between Nano composite structure and in a material service sign on bacterial contamination of biomaterial services. The Hatton Group works on the synthesis of drug silica nanomaterials for drug release, non-adhesive and non-fouling biomaterials, and microscale typography effects on Cell binding and service reaction. Dr. Hatton also has over 15 patents and applications from his research work at University of Toronto and Harvard University.

Thanks so much for joining me on the show today, Ben.

**BH:** Well thanks very much for having me here.

**KL:** So I know that you have researched in several different contexts, and this is something that I know, both myself and our listeners, can learn a lot from. So I thought we could start just by hearing about some of the different contexts where you have conducted research, so can you give us a little bit of an overview?

**BH:** Yeah, absolutely. In fact, I think I've been quite fortunate so far in quite a few different places in my through of research career like I said undergraduate, let's say. So my field is in Material Science and Engineering. In my master’s program, I realized I wanted to complete my project as a master's degree, not as a Ph.D., and I decided to take an opportunity to go work in Japan at the National Institute of material science, which is so big institute outside of Tokyo. And that was a really fantastic experience to see research in a different context of a different country all together. Although a lot of the problems are the same, it turns out. That gave me the chance also to think about what to do for my PhD research. I knew I wanted to do PhD just to kind of get into things in a deeper way, and I came back to the University of Toronto to do PhD here - this time also in the Department of Chemistry, and I thought I worked in a field of materials chemistry. We looked at how self-assembly at the Nano-scale can build up interesting structure of materials, and we used that to design kind of interesting nanoporous materials. And at that point - so I took some time getting through grad school, really, cause I did my masters, stopped, went to Japan for a year, I came back, did a Ph.D. in a different field. So that was time-consuming, but I think it gave me a chance to really look and find a field that I really liked. And then after I finished my Ph.D., I actually spent quite a few years doing postdoc research. And I first went back to Japan to the National Institute of Materials Science just for a year or so, and then I had the chance to go to Bell Labs in New Jersey. And Bell Labs has a really amazing history for kind of applied research and technology. When I joined it was sort of mostly on its final legs and stages of collapse, where they used to have a couple of thousand scientists - at that time when I joined it was down to maybe a couple of hundred, and most of them already looking for places to go. And nevertheless, it was really a fantastic place to work and to see kind of how I applied industrial research center have been. My supervisor there, in the process of everyone sort of leaving, got a position at Harvard School of Engineering and Applied Sciences. So I spent - I moved the lab up to Harvard with her and spent about five years working at Harvard—first as a postdoc, then as a research scientist—and I was also in this institute North [*indiscernible*] institute for bio-inspired engineering, which was really amazing place to experience. And then in 2012, I got a position here, back at University of Toronto as a professor, and I've been here ever since for about 5 years now.

**KL:** So I'm curious Ben, in each of these different positions as you were moving around, were you trying to kind of intentionally pick up different skills? Were you just kind of going where the jobs were? What was some of your kind of philosophy around your own professional development as you were working in these different spaces?

**BH:** That’s a very good question. I mean, to some extent, I was trying to find things that were interesting. And so, you know I think in my decision to finish a master's or not do a Ph.D. in a project that I had, which was an opportunity, it was really because I just didn't see it as interesting enough, and sort of with enough kind of, you know, depth to kind of keep going. But to be honest it's like everyone else, you have to go where the opportunities are. So I think in some cases, it was looking for interesting things, like when I looked for a Ph.D. program that was certainly because I found this field and this idea that I found super interesting. And I'm glad I made that change to a different field, because that's really important for a Ph.D. that you’re sort of – it’s something that really grabs you. You know? It's not something you just want to settle for. But, you know, since the Ph.D. It's really kind of like everybody else. I mean, you sort of have to look for postdoc jobs where they are. I was super lucky, actually. I mean, I had some funding, for partial funding, for postdocs support outside of Canada that I kind of was awarded through my Ph.D. work. That gave me a lot of freedom, because I could sort of approach people and work in their lab partly supported, so they didn't have to find money to support me. And my route was extremely lucky, going from Bell labs, and then my supervisor getting a position at Harvard - got me at Harvard for quite a long time and that was just really lucky. But it’s – yeah. I think you really just have to look to see who has funding and where postdoc positions are. And then in looking for academic positions, it’s even harder really - there's not much out there, you really have to do your best to see what opportunities there are. So, you know, and I guess in the early stages it was following interest, and then later it was just seeing what was out there.

**KL:** Mhm. So as you’ve worked in all these different contexts, what are some of the similarities that you're seeing that cross the contexts in terms of conducting research? What were the kinds of things that you felt like translated well from one place to another?

**BH:** I think that's quite a lot of similarities in people and research. You see a lot of similar personalities types in a lot of people are doing things for the same reason - trying to answer similar problems, and you know, they're there for, often the same kinds of reasons. They’re really excited about it. I think, you know, a grad student in one part of the world will have a lot in common with kinds of problems that a grad student in another part of the world will have. You know, science - it really does translate very well, like the pursuit of science and researches. You know, it's quite similar across the world, which is why, you know, international conferences are so amazing, because people can sort of commiserate and talk quite easily from one country to another. I think researchers, so even a researchers are, you know, are quite similar and their personalities across different countries in different research environments. I think - I think the research environments themselves can change a lot in any part of the world, simply because the way that research environment is set up depends a lot on funding sources that are available. And so the types of research that are done can vary quite dramatically between different institutions. So I’ve been able to work in some different institutions that are really - are quite different in how they do research. So you know, in one extreme you have sort of, you know, research institutes, where the funding is quite well defined and is often the case that these pick research institutes they don't enable certified cooperation between different groups. In fact this is a problem in Japan and some other research institutes, is that their groups tend not to be working very well together. And you do see that in some places in the states too. I know a guy who went to visit a big army Research Institute in Virginia. And he visited a group that was working on drones – very sophisticated drones, and they needed batteries, and I just went to Radio Shack to buy their batteries. And then he visited next door, and the next building next door was a group that was working on really sophisticated new lithium-ion battery designs. They were actually going to RadioShack to buy their drones to test them on. And so this guy just walking around the whole facility, just sort of connected people who, you know, could really work very well together, and I think that's a fairly common problem in institutions around the world. Another extreme is sort of a place like the MIT media lab, where you have huge open space. All the sort of labs that are there are very unconventional – just big glass walls and everything’s extremely open and very much on display - encouraging a lot of server interactions. I would say maybe too much, because it's hard to really do things kind of individually, which you have to do sometimes in science, you know? Um and one of the best examples I've seen for research is a place like where I was working, the Wyss Institute for Bio-Inspired Engineering, which started some kind of within Harvard in 2009, I think it was. That's a place that really embraces sort of high-risk research, lots of blue sky thinking, and I have a lot of money to support that. They have the ability to bring in grad students from Harvard, they also have permanent technical stuff to help make things happen- Sort of with an inertia. They sort of help encourage a lot of interdisciplinary conversation and socializing, so that's, that's a very good example of how to support novel research and creativity.

**KL:** Well that is a perfect segue. We're going to take a brief break when we come back, we’re going to hear a little bit more from Ben about creativity and research. Back in a moment.

# Segment 2:

**KL:** Ben, I know that creativity in research is something that you are very passionate about. I would love to talk with you more about this and also some specific strategies that you think researchers can use to nurture their creativity. But to kick us off, I would love to hear first; why you think creativity is important for researchers?

**BH:** Yeah great, yeah. I'm actually, I'm quite interested in how - where ideas come from in research and how creativity can enhance research. And I've sort of - I think I've been lucky enough working in all these different contexts to see that in different ways and different places, and then also to see how it can be nurtured. In fact, I think - I mean I guess the obvious answer is sort of that, you know, finding creative solutions for things, you know, is a great way to – well, to solve problems. And looking out for unusual results, that weren’t necessarily obvious is an important part of research. I mean, we can look at the examples like Fleming in the discovery of antibiotics, the discovery of the electron by Thompson, or you know. Um sort of chance discovery is where the researcher was sort of in a mind that was sort of open to seeing that sort of connection or that spark. That's what let them to the discoveries, and obviously in finding sort of huge discoveries like that, it helps research in so many ways. Its good papers, its good fund - sources of new funding, its sort of being the head of the fields, but it's also fun. It's just a big part of, I think, why we do research. Is those interesting discoveries in that creative element - at least to me is really a big part of why we do this. But there’s all kinds of different research - some research is extremely applied and is a bit more predictable and sort of logical. Which is fine, too. It's just the question about what kind of problems we’re trying to solve, really. So uh, in terms of, you know, strategies to try and encourage creativity, I’ve sort of come across a number of things that I think can really help. You know, for example, grad students or post office or even more senior researchers. One thing I’ve discovered is that conversation itself is really important. There's a - there's a kind of a sense out there that a lot of discoveries are made by kind of the lone genius working alone in a lab or coming up with something in their own space on their own. It just really doesn't work that way. It's kind of annoying in a sense, because I find a most sort of creative new ideas that I've ever been a part of or seen happened come out of conversations of at least a few people, often is even just trying to understand each other. Like to clarify and understand different fields, and you get sort of, you know, ideas coming out of - just suggestions and sort of trading ideas. So I think getting into the habit of sort of throwing ideas at people and see what they think and being open to suggestions of ideas is really important. You know, in improve comedy they always talk about the sort of yes, and type of mentality. So if somebody suggest something, you sort of go with it, you know? At least explore the idea. That's important. When I was at Harvard we had a lab that was set up with white boards everywhere, and that was great. Because you could just sort of set up just near your desk, and you could just stand up and draw stuff on the board and that would just stay up for weeks at a time, and you can add to it. Um and so conversations are really important. Going to lunch with your colleagues is really important, I mean, everybody eats lunch. So just being able to sort of chat to people in a way, that's kind of, you know, open and kind of casual is really important. To know what your peers are working on is really important. And you know, I think there’s many contexts of environments where that's really encouraged. Like at Bell Labs they had a huge, huge long hallways where people would just be hanging out in the hallways, and when they went to lunch everybody would sort of aggregate and, you know, have conversations in a way that really encouraged a lot of ideas. Bell Labs had something like eight Nobel prizes, and they had a patent clock in the front hallway that was like, you know, 30,000 or 40,000 patents or something. Incredible innovation and creativity there. Especially interdisciplinary type of work. So I think it's just important to embrace conversation, and you don't get used to talking about your work and trading ideas.

**KL:** I love your suggestion about the whiteboard, because I think it is a way to kind of take what is in our brain and put it out where other people can see it, and then when they look at your whiteboard they're like, “What is that? What you know what you thinking there?” They add something, or they contribute. It is a way to have a conversation in kind of an asynchronous way that is really interesting. So I’m really glad you raised that.

**BH:** Yeah, I was always sad when we clean the whiteboards every month or so, because you get used to seeing stuff up there. But the fact that it was up there for a while, it just reminds you of those ideas as well. Um so I think in a related strategy is also to try and seek collaborations, I mean, looking for interdisciplinary kinds of project is a great way to have something that's really kind of new, and novel, and creative actually. It's really the boundary between discipline sets, usually the place that's least explored, right? Because people aren't brave enough to cross disciplines. And so a lot of, you know, problems that are out there - like serious problems, I mean, I work on things like the contamination of medical device by bacteria. That’s a very interdisciplinary kind of problem, and many serious problems around the world are interdisciplinary. It's not just chemistry. It’s not just physics. You have to be able to you know you know your field, but you have to be able to go out and talk to people in another field and get to know their terminology, and how they explain things and that takes time and it takes a lot of energy, but it's generally worthwhile. So some places like the MIT media lab are - they describe themselves as anti-disciplinary, and they're trying to avoid putting people into pigeon holes and supplements in a few places in the world, and I think that's a great way to embrace art of inter-disciplinary work up to that because I do have a sense of the interdisciplinary like funding for interdisciplinary work.

**KL:** So I’m curious Ben, do you think the University structure overall has caught up to that? Because I do have a sense that that interdisciplinary – like funding for interdisciplinary work, publications for interdisciplinary work; it’s a little bit challenging. Do you see that changing in The Sciences?

**BH:** It's - some places are better at it than others. Others definitely. I Uh – in fact at the University imp at here, University of Toronto, we’re quite a big university. We have a lot of - we have something like 90,000 students and a lot of professors, but we're also jammed in downtown, so everything is pretty close together. I think that's actually good combination of people who are forced to sort of share buildings and overlap, and people walking by your office. It encourages a lot of conversation, and you know what other people are doing other departments. I still find this University, you know, and others – I still find universities in general, but not just his place, but I can be quite siloed, but the more you can encourage people to be kind of mixed together, the better. So yeah, Universities are a bit slow on that, some places are better at it than others. But funding me find - it's not that they don't - funding sources, for example, they don't really… I don't think they shine into interdisciplinary activities, but I think a lot of funding sources are slow to fund things that are new. I think that's the problem. I think sort of seed funding and early-stage research funding is hard to find, and that's where a place like the Wyss Institute has deliberately tried to fund new ideas. A lot of funding organizations and sources, are - they still want to have a lot of preliminary results to show that the ideas going to work before they’ll fund it, but that's the problem, because that takes money and time. So if you have a new idea, but you don't have money to make it happen then it's just going to die and that's often the case I find. So I think that's the problem, is they’re kind of a conservative nature to funding actually, which I would love to see being more open to early-stage ideas.

**KL:** We’re going to take another brief break. When we come back we're going to hear some more specific strategies from Ben that researchers can use to nurture their creativity. Back in a moment.

# Segment 3:

**KL:** So Ben, I know that you have additional strategies for researchers to nurture their creativity, so let's dive back into that topic. What are some additional strategies that you have?

**BH:** Yeah, this is something I have just sort of been thinking about it. I think another strategy is sort of simply to have lots of ideas. There’s a quote from Linus Pauling somebody asking about how to have good ideas and he said, “The way to get good ideas is to get lots of ideas and throw away the bad ones.” And I think that's right. I mean not all ideas are good ones. But once you get used to sort of throwing ideas around maybe put other people to get kind of feedback, you'll find that some kind of persist and they'll sort of comeback in mind, and you know you realize that it actually might work, and there's no good reason it wouldn't. But you have to get used to, you know, not I'm not all ideas being good, so you have to get used to sort of failure in that sense. And I like Steven Johnson's book, *Where Good Ideas Come From,* he talks about what he calls the ‘adjacent possible’, or sort of combinations of smaller ideas to get something that's kind of new. And the idea that not everything comes out – rarely do things come out of the blue. Let's put it that way. It's usually something that's accessible and adjacent to what you're doing. Related to that is to write things down. That's where the whiteboard comes, or keep a notebook that you carry around so it's - you know, if you come up with the thought, put it down on paper, or else you'll probably forget it. My PhD supervisor would even talk about, you know, writing things down when he's having a shower, and he just has thoughts and he tested quickly write it down before he forgets. So I think I think those are all. Important things of generating ideas.

**KL:** I want to pop in and remind our listeners that we do have an episode with Dannelle Stevens, talking about journaling for researchers, which I think is exactly on target with what you're talking about, Ben. Having a space where you are collecting these things, whether it is a pile of napkins in your desk drawer or an actual research notebook; a place to collect your ideas. So we will make sure to link to that episode in the show notes for anybody who wants to turn to that. What other strategies do you have, Ben?

**BH:** Well, you know, in research and experimental research you have to be prepared to fail. So this is another strategy. Also look for interesting results that didn't work. You know experiments that didn't work that generated something interesting. At my postdoc research – my supervisor, Professor Joanna Aizenbergand, she was very good at this. She love to see raw data that students have and interpret in interesting ways when things really didn't work. You sort of see interesting pattern formation or something that was unexpected. So you need to be able to look for the unexpected. Look for things that are kind of interesting opportunities. I heard somebody say once that, “to do something new, you can either be just very smart or lucky, or you need to be smart enough to know when you're lucky.” So you need to be able to recognize something that’s kind of interesting. Louis Pasteur said, “No fortune favors the prepared mind” but you have to sort of have the preparation to notice it. But when you find something that’s kind of unusual, hopefully you're in a position to take advantage of it, and I've seen many - probably half of my research papers are things that or just accidental discoveries that we sort of developed and took advantage of, and some of those turned to be very good. So you know, you have to be able to try things. Maybe often they're failing, but you know sometimes there's interesting things in the failure.

**KL:** Well you mentioned earlier that so many kind of scientific breakthroughs have been accidents. You stumble upon something, maybe a coming out of a failure that you weren't expecting to see. But if you are not open to considering that it might be a thing that could be interesting or something you can build on, you could just pass right over it, so I think that's such an important point. You have to have your eyes kind of wide open about what could actually be something, and that takes a creative mind to be able to look at failure and see something else.

**BH:** Yeah! And there’s is a great example of that in Bell Labs. In fact in 1965, Penzias and Wilson they ended up getting the Nobel Prize for discovering the cosmic microwave background of the universe, but that came through just having some buzz or noise in their radio telescope, and they couldn't figure out what was causing this buzz. So they just kept pursuing it and talking to people, and eventually they realized that it was something inherent to their measurements, so that led to a Nobel Prize.

**KL:** I love stories like that! Those are the stories that make the best movie adaptations. Maybe not for everyone, but I'm always fascinated by them.

**BH:** Another strategy is to, you know, try to push yourself to go and see speakers, or visiting talks or seminars. I find often grad students are kind of reluctant to go to see something that's not quite in their field. Even if it's fairly close. I think the exactly opposite, if you have time go and see something that's not in your field or maybe even very far from your field. If you're at a conference, like a huge conference, I was just at the Materials Research Society conference which was enormous. It was all kinds of parallel symposia going on. So try and go out and see what other people are doing. You’ll often find a lot of interesting kind of overlap or interesting ideas that can come out of it. Another thing is you know an experimental work, if you do think of something that might be interesting and it's not that hard to try it - It's not very expensive or it doesn't take much time, then set aside some time every week and just try something weird or random. I have a friend who used to call it ‘Friday afternoon experiments’. So he would to try some different chemistry synthesis on Friday afternoon’ just because it might work. And very often he found some really interesting things that way and we did some cool stuff that way. So for example, I do a lot of electron microscopy. Sometimes - very often, I'll just bring an extra sample and put it in the microscope just because there's time to do it, and you have your there anyway. So I find a lot of my favorite project come out of sort of simple test experiments that show you something kind of interesting. Show you that it could work, let's say, or may just fail completely, but it gives you a chance to sort of see what might be possible. So that's, I think, that's an important strategy from an experimental point of view. Then finally one last thing is trying to find funding sources and research environments that allow for creativity. And this is one problem, I think as I said before, I think there's a problem with a lot of our funding internationally. This is a problem in Canada, but other places where we don't have enough funding for kind of seed efforts or sort of blue sky projects. And it doesn’t have to be a lot of money. I mean even if it's enough to just funder a grad student or master-student for a couple of years or something like that -That's not that expensive, at least in Canada it's not very expensive. But there’s just such a reluctance and conservative approach to funding that doesn't allow for that kind of, you know, a new idea. I guess many times. It's high-risk at many organizations just us need so much preliminary results to show that it's going to work that kills the chance to do it, because you know a researcher – let’s say a new researcher who's going to be at an institution like University. I mean, they don't necessarily have a ton of students who can just try something, or tons of equipment, or funds to do something. So I think - I wish that more funding organizations were able to provide early stage funding, because I think that would really help. And so, you know, in looking if you have the opportunity to find a place that can enable some sort of consistent funding for, you know, fundamental science, or you could have new research, that’s great. And there’s some parts of the world where that really is done well. I think in Germany they have excellent institutions at the Max Planck Institutions, and so on. And in Japan in fact, they have quite a lot of funding that’s not guaranteed, but at least it's quite consistent. So, you know, and if you're a grad student try and hopefully get a supervisor that's kind of supportive of new ideas and new experiments. Not too much. You don't have been going in too many directions. That's the downside if you're so scattered all over the place, but if you're spending, I don't know, maybe 20% of your time on something that's kind of a side project, that's - I think very that's time very well spent. So those are some of the strategies that I was thinking really are some ways to help encourage creativity and research over all that I've seen in different places.

**KL:** Well I love those strategies, and I love especially the last one that you mentioned. I mean we see, you know, companies like Google and other places that are super innovative practicing this side project approach - giving people lots of time for things like research and development, and I think we see it bearing out in a range of different places. Ben, I want to thank you so much for coming on the show sharing your strategies creativity and research is also a passion of mine as well, so it was really fun to hear about some of these things that you have found from your work overtime. Thanks for coming on the show!

**BH:** Well, thanks very much for having me. I think it says it's a great show, and I'm really happy to be here.

**KL:** And thanks also to our listeners for joining us for this week's episode of Research in Action. I'm Katie Linder, and we’ll be back next week with a new episode.

# Show notes with links to resources mentioned in the episode, a full transcript, and an instructor’s guide for incorporating the episode into your courses, can be found at the show’s website at [ecampus.oregonstate.edu/podcast](http://www.ecampus.oregonstate.edu/podcast).

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