

2011 Distinguished Scholar Interview

Written by Chris Yenkey

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Interview with [Joel Baum](#) , OMT Distinguished Scholar

Conducted by [Chris Yenkey](#)



Among the pleasures of winning the Louis Pondy Best Dissertation Paper Award is that you are asked to interview the winner of the Distinguished Scholar Award. This year's winner, Joel Baum (University of Toronto Rotman School of Management), agreed to sit down with me (via Skype) to discuss his career path and share his thoughts on trends in the discipline. The following are excerpts from our conversation:

Chris: Let's warm up with some easy background questions: What did you do for your bachelor's, and what was your path to grad school?

Joel: I did my undergrad in psych, economics, fine arts, and math, which basically means I had no idea what I wanted to do! But at the time, my mother was dating an OB professor at the Univ.

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of Toronto who studied leadership, Bob House. He knew I was interested in psych and was thinking about a PhD, and he said, "Whatever you do, do it in a business school, then, when you're done, there will be a job that pays you well." And that's how I ended up in management. Shortly after I came to U of T to start my PhD, Bob left on sabbatical...

Chris: And once in grad school, what got you interested in institutional theory?

Joel: At the time U of T required all the PhD is to require the first year of the MBA program if you didn't already have an MBA. And, I had to take half a dozen more courses in OB in order to qualify for the PhD seminars. Around the time I was finishing up the MBA, the school hired Jitendra Singh. He was a student of the Stanford population ecology crowd, and he Bob and Dave Tucker were researching voluntary service social service organizations (VSSOs), and writing quasi-heretical organizational ecology papers about organizational change. When Bob left, I fell into Jitendra's orbit pretty quickly because I found macro organizations much more interesting than OB, particularly institutional and ecological theory because it was becoming longitudinal. One of the things that disappointed me about the OB literature at the time was that although the theory was within-person, over-time it was being tested cross-sectionally across people, so there was a real mismatch between theory and empirics. But in macro organizations people were looking within-firm over time, and I also felt like they were also dealing with more interesting social, meaningful problems, like maintaining social services within a community, things that led to improvement in or stabilization of a group of organizations over time. I felt like these were really interesting and important questions.

Chris: so the link with Jitendra coming out of Stanford makes a lot of sense. Was he your advisor?

Joel: Well, after completing my comprehensives, I went to Jitendra expecting he'd be my dissertation chair. He said he'd like to but he just got an offer from Wharton. I wasn't really prepared to pick up and leave for Philadelphia at the time, so I decided to stay and I went to Bob and I said "Bob, will you be my dissertation chair?" And he said, "I'd love to Joel, but I just got an offer from Wharton." So, I went to George Day, who was a marketing prof that was interested in ecological perspectives on some marketing issues, and I said "George, would you be interested in being my PhD advisor?" And he said "I'd love to Joel, but I just got an offer from

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Wharton.” So, I went to a guy named Martin Evans who was the director of the PhD program at the time and I said, “Martin, are you interested in getting an offer from Wharton? Because if I ask you to be my advisor, you'll get one!” He agreed, but unfortunately for him the offer didn't come. So I wound up with a micro OB advisor chairing my macro orgs dissertation, and he did a fantastic job. Actually, all the people on my committee were micro or meso OB people. Nobody studied anything beyond the organization level. But I survived. Although I never did publish a word of my dissertation. So I ended up doing an empirical classification study using data on child care organizations for my thesis, partly because I became interested in the boundaries between organizational forms, and my dissertation committee left me pretty much to my own devices. That was before classification of organizational forms was all the rage, as it seems to be now within the categories and identities literature, so maybe I should go back and look at it again. Nah.

Chris: But the empirical setting carried over didn't it? For example, your early 1990s papers with Christine Oliver?

Joel: Yeah, Christine graduated a couple years before me from U of T. It was a heyday for Toronto, so there were a bunch of really cool people who've gone on to do some really great work. It turned out that part of the data I coded was a series of inter-organizational relationships between the day care centers and government and community agencies, which I didn't do anything with in my dissertation. I mentioned to Christine that I had these data, and suggested we should do a study based on ideas derived from what became her highly-cited 1990 *AMR* paper. We ended up writing two papers together, two of my first three publications – the first looked at organization-level effects of these ties, and the second looked at their population level effect, and it was that second one that got me into some trouble with ecologists because density dependence was challenged by the analysis.

Chris: Those first two papers read to me as counters to the Stanford-based population ecology work at the time.

Joel: That's how they read it, at least the second one. The first one is pretty neutral, basically examining the liability of newness questions. My data, which included time-varying size, were unique at the time, and so I never found the liability of newness. As we now know, once time-varying size is controlled, age dependence looks quite different. For the 1991 *ASQ* paper, the reviewers thought our data were completely wonky because we didn't find negative age dependence in the failure rate. We went to great lengths to explain to reviewers that we indeed found a liability of newness, but only when we didn't control for size. It was the 1992 *ASR*

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paper in which we argued for two types of legitimacy: a social embeddedness kind of legitimacy and a cognitive kind. The second is measured by organizational density and we were working to try to measure the other one using density of social ties (“relational density”), and whoops, the organizational density finding disappears when you model them together. So the social embeddedness dominated. We thought we were doing normal science, advancing theory, by arguing for two processes at work, and in this context one trumps the other. It made sense to us that relational density would do more because we were studying social service organizations rather than business firms, but all of a sudden we were heretics.

At the same time I was working with Jitendra on papers that later came out on niche overlap that made a related argument that there is density that was competitive and density that was complementary. Those papers took a long time to come out. They were both written by 1990 but didn't come out until 1994. The *Organization Science* paper was first summarily rejected from *ASQ*. When we sent the other paper to the *American Journal of Sociology*

we wrote the editor a letter saying that if you asked any of the “density dependence” people to review this paper they'll trash it, and so we hope you don't send it only to them. The editor was quite clever – he did a hypothesis test and sent it to one fan of density dependence. We got two very supportive reviews and one ... we'll let's just call it a negative review.

Chris: That sounds pretty related to your OMT address on patterns of skew in the discipline. It sounds like this has been an issue on your mind for a while.

Joel: Yeah, I've been interested in citations for a long time. I became better versed on this when we were starting up our new journal, *Strategic Organization*. There is a challenge to building reputation and legitimacy in the field because of reliance on Impact Factors that new journals don't have. And then when you are accepted, there is the worry about what's the first Impact Factor going to look? So I became very alert to these things and the impact they can have. ISI won't typically consider a journal for inclusion until it's completed five volumes. How do you get in? It's a mystery. We applied after completing our fifth volume, and a year and a half later they said “Okay, you're in,” but without any communication in between. Our first two-year Impact Factor in 2009 ranked the journal 8th in management with an Impact Factor over four, remarkably, ahead of *ASQ*

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Organization Science

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Management Science

and so on. Because we don't publish a lot, we jump around, but that was certainly a good place

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to start. So I guess I became a little obsessed with Impact Factors.

I'm also now in my fourth year as Associate Dean, Faculty at the Rotman School, which puts me into the middle of numerous tenure and promotion cases. There's this idea that if you get published in an "A" journal you're done. This struck me as odd. So I started to think about scholarly impact, and what I was reading resonated a lot with my interest in Power Law distributions and extreme values. The notion of skew links these interests and observations together. Obviously I'm not the first to notice this, but academic productivity is highly skewed, so are citation rates, and Impact Factors. And you and I can benefit from the small number of highly cited scholars as we publish in the same journals. The journals compete for these skewed few to publish in them because their Impact Factors depend on them. And these few people are on all the journals' editorial boards, which influences what is published and who publishes it. One impact of this skew is a high level of concentration among authors for publications, among journals for citations, among universities for prolific and highly cited authors. So I wondered "is being out in the fat tail of these distributions really a signal of research quality?" There are all sorts of instances of skew where being in the tail has nothing to do with quality, but it seems to be in our heads that being in the tail of the publication or citation distribution means quality. So I've been trying to deconstruct or construct why this would or wouldn't be the case.

It's awfully hard to come to the conclusion that skew is a clear signal of quality, particularly because it's based in citations, which are weird things. They're gratuitous, to our friends, for political reasons, and for justification. There are lots of really high quality papers that we take for granted and don't cite at all. Too many citations go to review papers, which mean that they are seen as high quality when they contain no original research.

Chris: Do you see time variance in the skew? Is it getting worse, better, unchanged? Is there any promise that faculty websites make it easier for people not in the tail to be more visible and have more people see their work?

Joel: No, because that's countered by the fact that the work by those who are in the tail is also more available, and if you think of it as a form of preferential attachment, the more cited an author's work is, the more likely it will circulate widely relative to someone else. One of the points I made about electronic access at the end of the OMT address is that we don't look beyond the well lit areas. So, I have 9,000 journals available on my desktop but I only follow six. We tend to search on authors although rather than on topics. If we searched on topics we might find other people's work, but our bias is going to be towards people we know, journals we know. Even the rank ordering provided by the searches is often a function of citations. I'm never going

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to look at page 28 of my subject search results to find that little known article with its four cites; I'll just pick the paper that's on the top of the list with 2,000 citations. So I think there's a real challenge there.

Chris: What effect do you think this has on the work that's being done in the discipline? Are we too inwardly focused so that there are too few questions being asked into few theories being pursued?

Joel: For sure.

Chris: But is there an upside to it? Does it give us a more focused agenda so we can make more progress on a smaller set of issues?

Joel: No, we get locked into particular topics, and if you want to publish in a particular journal you have to cite a particular earlier paper from that journal and you have to use its language. You genuflect to the journal you want to publish in. For example, if you cite Teece (Teece, Pisano & Shuen 1997) and you use the words "dynamic capabilities," you'll never publish in *AS Q*,

but if you don't you'll never publish in *SMJ*

. You have to cite the right people and ideas and use the right language, and the right people and ideas are the ones cited to the point where it's essentially mandatory to cite them. We have someone working on an editorial essay for

Strategic Organization

who is conducting an analysis of citations to "dynamic capabilities," and the finding is that 95% of these citations are genuflecting in passing to the concept; there's no strong tie to the fundamental arguments.

You *should* have to show your intellectual debt, and you *should* be able to show that what you're doing is a cumulative activity. My concern is that there is overkill on well cited ideas, particularly well cited ideas where if you dig a little there's not much there. There are certainly cases where it's very good research. But in the fields of strategy and organizations, demands for novelty push us away from being cumulative. We don't have replication, it's not valued. One place where there's lots of replication is organizational ecology – the findings there were replicated hundreds of times as part of incremental advances in thinking. But that just doesn't happen more generally. It would take a lot of persuading for me to believe that the way we get

published is helping us to advance our field more than it is impeding us.

Chris: So what would you recommend to early career scholars trying to get a feel for the sweet spot? How do you balance replication of earlier results, which plays into the condensing part of skew, with wanting to push into new areas?

Joel: Well, let me be a little clearer. Many of the ideas that we cite frequently are not actually tested, because the citations to them are not substantial. Maybe I'm studying organizational change, so I'm going to cite dynamic capabilities. Why do I cite dynamic capabilities? Why do I cite that? There's 50 years of research on organizational change that the dynamic capabilities literature does not connect to, but we leave all that behind and cite dynamic capabilities and then the study we do has nothing to do with the basic ideas of dynamic capabilities. We can always cite broad ideas and use them as a resource, but too often we are not advancing or connecting with the real theoretical argument, and so it *looks* like there's a lot of connection and a lot of development in the literature, but in fact all that's holding it together is a string of gratuitous citations. Meanwhile, the substance of what's going on in the papers may or may not be moving forward. And so my concern is that a few things get cited like crazy but in a way that's not substantial, and therefore that work is not being replicated or moved forward. So to answer your question, I would hope that junior faculty and people who are starting their careers work hard to cite things substantially, not just to link to a popular trend. It's great if you want to study dynamic capabilities, but if you want to study organizational change and you're not going to address the specific underlying issues that form part of the dynamic capabilities theoretical frame, then don't cite it.

Chris: Do you see more of this problem coming from the bottom up, where people are making unrelated citations in order to gain entry or seek legitimacy, or is it enforced from above? If journal submissions ceased these weak tie citations do you think the average reviewer won't miss it, or do you think the average reviewer will push back?

Joel: Well, I think it's a bit of a prisoner's dilemma in the sense that if you're the first to do that you're likely to face the sucker's payoff. I think there has to be a broader desire within the field to tackle some of the issues. There is not a lot of discussion about how our field works, and our field is very competitive and increasingly based on counting publications in journals that are viewed as top-tier. The metrics that we're using to evaluate the quality our own research don't meet any of the requirements that you and I would have for a measure of quality. The Impact Factor was developed to suggest to librarians whether or not people are paying attention to a journal and whether they should include it in their collection. It was never intended to be a measure of quality. And now it is being used to draw implications for individual articles in the

journal, for which it also was never intended. Even in *ASQ*, the median article gets substantially fewer citations per year than the Impact Factor would imply because a few highly-cited authors publish in that journal. So it's just wrongheaded to say that if you get an

ASQ

paper you're done. You're not done. Let's look at the paper. For

Org Science

, the variance of citations to papers in any given volume is so wide that a particular paper could be as cited as frequently as the top papers in

ASQ

or it could be uncited. So, when people say, "It's in an "A" journal so it's good", that's what I'm against.

That's one piece of it: how we evaluate ourselves as a field. The other piece is whether or not the most highly-cited work is also the highest quality in the field. The assumption is that the most frequently cited work is of the highest quality because it's well cited. And because of this inference the papers by these few authors are well cited, and the journals clamor for this work and for these few people to be involved in their editorial boards because it's good for their Impact Factors, and work that looks like theirs and cites their work is viewed as citable, and their students are in demand as well...

Chris: What are some of the new lines of work out there that you'd like to see more of?

Joel: There is a group, the Strategic Research Initiative, that has emerged that is comprised of organizations and strategy people who are arguing for a range of criteria to define high-quality research. Whether or not you agree with their criteria is another question, but at least they're having that discussion. We published their "manifesto" in *Strategic Organization*, and it's been very controversial. Lots of downloads, lots of agreement, lots of disagreement. I think their main ambition is to generate discussion, and I think that's good. I'd hate for the field to wind up thinking that there is one type of research that's high-quality. My own view is that there are many ways to be rigorous and sound. There are good and bad ways to do all varieties of research – archival, survey, experimental, grounded theory, simulation, whatever.

I think one of the things lacking in our field is attention to research methods, not empirical methods but research methods. I don't think anyone reads Cook and Campbell (1966/1979) anymore, despite the fact that if they did they'd design better studies and understand why our more economics-oriented colleagues complain that we have endogeneity problems due to the design of our studies. I also think we lack an understanding of epistemology, and it would be useful if we all had a better sense of contemporary philosophy of science. If we knew that none

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of us were ever positivists or relativists we'd all be better off. The idea that there are objectivist and subjectivist perspectives, and never the two shall meet, is just wrongheaded. Philosophy of science has long rejected this distinction and advocated for integrating these approaches.

Chris: Joel, thanks so much for your time and for sharing your thoughts.

Joel: You're very welcome.