



David H. Zhu (Arizona State University), winner of the 2010 Lou Pondy Best Paper from a Dissertation Award, interviews Dan Levinthal (University of Pennsylvania), the recipient of the 2010 OMT Distinguished Scholar Award.

David: I really enjoyed your presentation this morning. It reminded me of many fun discussions that we had when we talked about absorptive capacity, random walks, myopia of learning, rugged landscape, and complex systems and so on at Michigan's doctoral seminar.

Dan: Thank you very much.

David: I am curious about how you came up with these ideas. I noticed that almost half of your dissertation was built on agency theory. How did you end up focusing on a different stream of research? How did you recently become interested in organizational goals?

Dan: Well, at Stanford I was kind of living a dual existence: my "day job" was a student in a math/econ doctoral program at the business school and I was "moonlighting" as Jim March student. Agency theory was very hot at that time and I was engaged with it in my coursework and choose to do a dissertation in that domain to do the sort of dissertation I thought was appropriate for my program. I had been an economics major coming out of college and so signed on for the economics program within the business school but had largely been attracted to Stanford because of Jim. As a result, I was kind of working at two PhD's at the same time. I think that I began to integrate the different strands of my work after leaving Stanford.

The issue of goals, from the economics perspective, was primarily about incentive alignment and motivation. For someone interested in complex adaptive systems, the issue of coordination and goals in a kind of directional sense comes to me rather naturally. I spoke today about this more recent theme in my work and I think it could be dated back to the 2000 piece with Giovanni Gavetti. That work considers the notion of bounded rationality in the context of someone forward looking intentional rationality whereas much of my prior work had been looking at feedback processes of adaptive learning in which an actor modifies their behavioral

repertoire behavior as a function of prior experience. It struck me for a long time that the idea behind bounded rationality was set out in a particular way by Simon but then as the literature evolved it got substantiated in a somewhat narrower form. So the idea of adaptive search and aspiration became the defining characteristics of bounded rationality. In the article with Gavetti and in work I did with Jerker Denrell and Christina Fang, I tried to incorporate into bounded rationality a forward-looking perspective on choice and cognition. In the standard learning model, the adaptive processes is driven by feedback, typically immediate feedback though possibly a noisy signal. Such adaptive processes are akin to learning in a T-Maze, the actor (mouse, or college sophomore) may go left or right, and receives some associated probabilistic reward. Christina, Jerker, and I borrowed from the literature on machine learning and introduced the idea of credit assignment and recognition of rewards, based on the actor's mental model, of the value of an intermediate step as a critical part of a multistate learning process. This structure allowed us to connect the learning literature to settings where the world isn't giving us immediate feedback and therefore some level of interpretation becomes important to assess the potential value and appropriateness of a given action. So, again broadly viewed, it's bringing in the notion of bounded rationality and some elements of intentional rationality.

David: Sounds like a very interesting stream of work. Some of my research tries to understand how executives and directors experience may influence the way they process information and make decisions. More broadly, I feel that the upper echelon literature was also significantly influenced by the Carnegie School of psychology. Do you see any emerging research opportunities that could tie your research to the upper echelon literature?

Dan: That's an interesting question. Certainly if you think about it, the broad prior context knowledge of members of the top executive team will obviously affect the way they interpret information and construe opportunities. We can think about that at the individual level, and there are also group wide applications of that kind of argument. In my 1990 paper with Wes Cohen on absorptive capacity, we put out some ideas about possible implications for thinking about diversity within a group. We talked about how the presence of boundary spanners or gate keepers would not be sufficient but that the organization would need individuals with overlapping knowledge structures within the organization as well for ideas to really be taken on, and understood. Within the top management team, as you know, there is a vast literature on the various flavors of diversity in a group and the sensitivity to different kinds of functional background and so on. I think the issue of diversity and some requisite commonality are both important. So I think there are certainly some opportunities there. In the Carnegie tradition, there is a sensitivity to think about the organization as a whole and a collective and not to necessarily privilege the actor or the set of actors at the top. Indeed, in some of my recent work, I think about the organization itself as a kind of population of initiatives with its own ecology. One of the issues about the interesting roles of higher level actors is their ability to influence the design of this broader system. So it's a different imagery of what leadership is doing, leadership in the sense of providing a context and making choices about the selection pressures that other actors are subject to versus a deliberate body or a single actor making choices about going

“left” or “right”. Clearly there are instances where such discrete choices occur, but I’m more focused on the indirect impact of leaders in their role as organizational designers.

David: Your absorptive capacity paper is one of the most influential papers in our field. How did you come up with the idea? Did that idea really come up to you when you were proctoring an exam?

Dan: (Laugh) Yes, that’s right. I was trying to catch up on some old journal issues and was intrigued by an article in *Econometrica* by Spence that explored the classic economic issue of spillovers, the level of appropriation, and trading off the incentive aspect of having tight appropriation regimes versus knowledge sharing. The paper ended with Spence posing a puzzle that one observed very high rates of spillovers in certain high-tech industries, particularly semi-conductors, and that at the same time these firms were engaged in high levels of R&D spending. It struck me that this “puzzle” emerges because Spence, as other economists had, assumed that knowledge is a public good that can spread costlessly in the absence of legal protection. That struck me as a kind of crazy assumption. The modeler instinct in me suggested, as I was ignoring my students taking the exam, that if I introduced another consideration, a function that determines what fraction of this “public” knowledge benefits a focal firm and to make this function take on an increasingly value with the firm’s own R&D, one could quite readily generate conditions with high spillovers and high R&D intensity and kind of explain this puzzle. So I took that idea to a lunch with my new friend Wes Cohen. Wes had come out of Yale and had worked with Nelson and Winter and so we were intellectually simpatico. Wes was very deeply engaged in the empirical literature on R&D. In fact he and Dave Mowrey had developed an outstanding data-set drawing from data from the FTC line of business data and the Yale Survey of R&D managers. So I presented this kind of conceptual perspective and he got excited, and it was really great that we could develop this model and we had access to a data-set that allowed us to test these ideas. That combination helped make the article a powerful package. I think it reflects the possible benefits of being engaged in two distinct intellectual communities: I was engaged in the economics literature, but with an outsider’s sensibility. As someone interested in learning processes, I was not going to accept the taken-for-granted assumption that knowledge is akin to a public good, which starting from early writings of Arrow had been a basic assumption in the economics literature.

David: For students who are interested in doing research on absorptive capacity, what are some of the things that you would encourage them to study in the future?

Dan: Well, I may not be the best person to ask that of. Perhaps I was foolish not to just focus and exploit this wonderful gold mine, this particular vein that we identified. However, in some

respects the fact that Wes and I let go of the concept may have contributed to its wide adoption and use --- an open innovation of a sort in the academic literature. Wes and I went off in other directions and the absorptive capacity idea was out there and people could make use of the idea as it seemed useful for them.

David: So you chose to explore instead of exploit and you discovered other great ideas as a result. This is very consistent with your theory of search.

Dan: (Laugh) Yes, you can say that. Coming back to research on absorptive capacity, my own sense is that further development of the micro mechanisms of absorptive capacity might be useful. We have a reasonably clear sense of understanding of the construct and how it plays out at the more macro level. I would also make an observation about measurement. Wes and I did not use R&D investment levels as a measure of absorptive capacity. We had an empirical model where we had measures that were reasonably indicative of the ease or difficulty to assimilate different classes of knowledge and we could make predictions regarding, given the saliency of these different classes of knowledge for different firms, what the R&D intensity might look like. In recent years, the availability of patent records has obviously been an enormous boon to research efforts and people have been able to do interesting work linking patent stocks to firms and individuals as proxy indicators of technical knowledge. These measures have some well known limitations regarding the domains of technical activity for which patents may be a more or less sensible proxy for firm's technical expertise but they do offer the advantage of offering a finer grained classification than what aggregate R&D measures would give.

David: Were you and Herbert Simon neighbors at Carnegie Mellon? Can you tell us a bit more about Simon's influence on you?

Dan: I didn't realize when I bought my house, but it turned out that he lived directly across the street. I had the chance to talk with him at times if our walks to the University coincided. It was a nice treat for me, part of the charm of choosing Carnegie as my initial job. But don't think that I was a stalker or anything (laugh). In terms of his influence on me, I would note that by far and away my deepest intellectual influence is from Jim March. Clearly, Simon and March have important common intellectual commitments. However, their orientations always struck me as rather different. Simon starts with the premise of bounded rationality but was interested in how one might engineer social systems that might, effectively, relax those bounds. Jim enjoys more the pleasures and pathologies of adaptive systems and, at times, will offer thoughts about how their performance might be enhanced but such considerations are not over-riding motivations for him.

In terms of Simon, when I began to revisit the issue of organizational goals in recent years, I went back to his seminal piece of 1955. I was struck by his argument that goals act as independent constraints on individual choice and recognized that this was a central part of his argument and a radical break from utility theory that is premised on the presence of compensatory tradeoffs among attributes. Sendil Ethiraj and I build strongly on this idea in our piece on organizational goals and our, in some sense, attack on the notion of a “balanced scorecard”.

I can also share with you one of my favorite Simon anecdotes. Simon once told me that every time that Paul Samuelson came up with a new edition of his classic undergraduate micro textbook, that he would write Samuelson this cranky letter arguing why Samuelson kept putting these U-shaped cost curves in his book when there was no empirical evidence to suggest that cost curves took this form. For Simon, this was an important issue because of his early work with Ijri on the size distribution of firms. For neoclassical economists, the convenient fiction of U-shaped average cost curves helped resolve the issue of how many firms would be present in an industry in equilibrium without having to engage in the evolutionary dynamics raised by Nelson and Winter. It struck me as funny that Simon would send this Nth version of this letter to Samuelson every time Samuelson came out with the Nth

edition of this textbook. I think being persistent was one charm of Simon’s personality. I think Jim has his poetic, playful side and Simon was the intentionally rational engineer.

David: What suggestions do you have for junior faculty members?

Dan: One is don’t be overly instrumental. I mean there are easier ways to make a living. If you’re not excited or passionate about the research project, there may be other options. Further, while you want exciting ideas that people will respond to, the first test is how you respond to it. Do you find it animating? That is a necessary and most important consideration. Another point to bear in mind is that any piece of work takes a lot of time. There are career points when a junior person is going to say “oh my gosh, I got this near blank vita and my problem is to fill it up. I know this isn’t the most dramatically exciting thing, but someone has data and I think I can sort of do this”. No! By the time you’re through two R & R’s there is no such thing as an easy publication. My speculation is that work can be less successful than you aspired for at the beginning of a project, and that happens to all of us, but a project that is not so inspiring ex-ante is unlikely to become so ex-post. Even as a junior faculty you should realize that you have a non-trivial opportunity cost. If you have some idea of a corollary project that comes as a byproduct of a prior research effort, it’s not crazy to develop such ideas. But don’t act purely out of convenience because you will be spending a lot of time with whatever piece of

work you're doing and you should aim pretty high even with a blank vita. Another highly pragmatic suggestion is that your own time allocation should work backward from the papers that are closest to publication. New ideas are always fun and seductive. Starting a project is vastly more fun than working on a R&R. Although finishing a project is typically not in people's top three of what they would like to do, the highest priority should be given to the work closest to publication and that will help keep the end of the pipeline moving forward.

David: It's great talking to you. Thank you so much for your time.