



09-04-2024 MUNIR Y. MANDVIWALLA - TRANSCRIPT

2 SPEAKERS

Sebastian Reiners
Munir Y. Mandviwalla

Duration

23m 49s

START OF TRANSCRIPT

[00:00:02] Sebastian Reiners

Hello everyone, and welcome back to one of our interviews in the DSR Academy. My name is Sebastian Reiners. I'm a research assistant at the University of Münster, and I have the tremendous opportunity of having an interview with Munir Mandviwalla of the Temple University. Munir is a professor of management at Temple University for almost 25 years now. So anniversary coming up. I think this year even, and also a co-founder of the Temple University spinoff PRO Community. And Munir, before I talk too much, do you want to maybe present yourself?

[00:00:43] Munir Y. Mandviwalla

Sure. Well, first of all, thanks for inviting me to speak to you and being part of this series. I think the project is amazing and hopefully will be of great use to others. So I'm very pleased to be included. So a little bit about me: I've actually been at Temple for 30 years.

[00:01:03] Sebastian Reiners

Oh, my bad then.

[00:01:04] Munir Y. Mandviwalla

No, no, I'm actually, you know, one part of me thinks I should have left by now, but it's maybe too late. But I love the place. I love Philadelphia, and it was my first job as we were discussing about you completing your dissertation. So, there was always something there that the university allowed me to keep reinventing myself, and that's what made me stay in the region. And, I'm actually a professor of management information systems, not management, so that my management colleagues don't get annoyed. And, I started the MIS department at temple a long time ago, and then currently I'm working at co-leading an institute that works with industry and that has a very design science flavor. We have a lab in which we do projects with industry. And, so, I'm still pretty much doing what I wanted to do when I started my PhD. So I'm very happy to be here.

[00:02:07] Sebastian Reiners

Still continuing on the first part, which is amazing. A lot of people just change it and amazing for you to still be on the interest you had like 30 years ago, which is great. Considering your academic career, not too long ago, you wrote a paper titled Generating and Justifying Design Theory, and you published it in the Journal of the Association of Information Systems in 2015. So nine years ago, it is, if you ask the design science research community, it's one of the leading papers of design science. And, you basically argue in the first pages of the paper that currently the scientific community of DSR addresses the what? So, the overall cycle of design science. However, there is a gap addressing the how in the sense of what steps do we need to produce theories and practical insights for design science research. And you then wrote the paper about it. Can you maybe take us back a few years ago when you decided to write the paper? What was your core idea? Why did you start writing this paper in particular?

[00:03:29] Munir Y. Mandviwalla

Yeah, that's an interesting question. The paper actually started in my head, at least when I was working on my PhD back at Claremont Graduate University. And I had my main advisor, Lorne Olfman, who's a behavioral researcher, and I was trained as a behavioral researcher and published quite a bit of behavior research. But I also kind of have in retrospect, none of this was known at that time. My undergraduate degree was in engineering, so I always had an interest in building. Whatever it might be, whether it's building human structures or systems, at that point, it was all about systems. When I was a doctoral student, I read the Walls et al. paper that had just come out. I was a doctoral student at that time. And, you know, I went wow. You know, I can actually build systems, and people won't ignore it. They'll think it's actually cool, and I can get academic credit for it. I can have a career doing this kind of stuff. You know, that was kind of a light bulb moment for me when I read that paper. Then I was fortunate to meet Omar El Sawy, who was also at USC for the last 30 years and who was one of the co-authors of that paper. Very well-known person in IS. And just meeting him was incredibly motivating. And then I read Simon. So, you know, series of accidents, Simon's book, *The Science of the Artificial*. So all of that kind of combined together. You know, I started thinking about okay, great in social science or behavioral science, which is what I was trained in; we tend to focus on the what, as you said. But for engineers, the how is perhaps even more important because, you know, if we don't build it correctly, then the bridge will fall down. Or the ship will hit the bridge, as what recently happened in the US. Whereas in social science and behavioral science, the how is just the process of getting to the what, the research methodology. So that was very poorly formed ideas as a doctoral student, and I wrote a first draft of it for my dissertation. And Lorne, who was my advisor, and Paul Gray, who started the program there, both said, No, no, no, no, no, this is all you know. You have no idea what you're talking about, and I actually didn't. So, then I went on this adventure of getting an academic career, building a department, but still, it bothered me—that paper that I had written as a doctoral student. So it's a longwinded story, and maybe the lesson is to never give up on your first idea. So then I started writing it in 2010ish or something like that. It's a long story, but hopefully if doctoral students read it, they'll understand that, you know, all ideas are good. You should never give up on them.

[00:07:09] Sebastian Reiners

I don't know if all my ideas have been good so far. I wouldn't necessarily agree on that. But, totally, I agree your idea was a good one. Interesting fact about that. I was talking to Tuure Tuunanen not too long ago about his paper, and he also had his first idea during his PhD time. So, funnily enough, a lot of great papers and design sense do stem from the time during the PhD career, which is quite astonishing to really see that how a paper can progress through decades, really, which is great. And maybe, if you can try to summarize, I mean, you talked about the process of developing the paper and the drawbacks from senior scholars during that time. What would you summarize as the key paper idea nowadays? What is the key idea?

[00:08:02] Munir Y. Mandviwalla

Yes, it's perhaps easier now, in reflection. I know, first, the paper went through a lot of pain of rejection. I think the premise of some of the reviewers, which one part of me actually buys into, was that in that era of design science, there have been a lot of people talking about how to do design science, but not enough design science. So always the pushback was, you know, don't talk about the how; just do it. Which I actually totally believe in. And then there were some flaws. But anyway, I think, in retrospect, I think this is still one of the few papers that talk about the how. And really, the how came from the notion of prototyping or iteration, which is Simon's major contribution that we all kind of take for granted that all science, forget the physical sciences or the natural sciences, it's all of the sciences, not just social sciences. There is iteration involved in all of these sciences. And, you know, we often ignore that. That's just the pain of working on the research project. But in the case of design science, the iteration is in itself critical in terms of building the

rigor and the relevance. The iteration of the design. And I think that it is still a contribution to say that. As you develop your idea in a system, technology constraints dictate what you can do, and then provides millions of opportunities today, right. We're no longer writing, Pascal programs where you have been extremely constrained in what you can do. Today, you have so much power with all the libraries available, you can do almost anything. So on the one hand, technology is frustrating because it constrains, but on the other hand, it's overwhelming that there is so much you can do. And if you're building something, then you're most likely have a target user in mind. You're not building for yourself, because that's lonely and boring. And there is also a constraint because the target user is demanding, will not use your system if it doesn't pass the 30-second rule. You know, it used to be like the 30-minute rule, but now it's down to 30s today. So that process of iteration builds so much rigor in the quality of your idea. And it also automatically builds relevance because, on the one hand, you have to make the thing work. But then, on the other hand, you kind of have to satisfy the target user. So, I think that is still important and will continue to be important. And I can talk more about it as we go through it, but those would be the points I would focus on now.

[00:11:32] Sebastian Reiners

You are talking quite a lot about the contribution of the paper, and you were mentioning rigor relevance, which is a topic that is very important in DSR, obviously. Maybe looking back again to have this richer perspective again. What is the contribution that you think your paper adds to the DSR community in its state right now?

[00:11:58] Munir Y. Mandviwalla

Several different ones. I think one we already talked about, so I won't go back to it, which is the rigor and relevance. But I think the part that we don't give ourselves enough credit—all of us who build systems for research purposes—is the currently accepted practice in DSR is that you must evaluate. Which as a behavioral researcher, you know, that's obvious to me that we must evaluate, but I think we don't give enough credence to the process of building and how much evaluation is built into that and how much rigor is built into that. And I think that is a contribution in itself. We still need to do a better job of demonstrating that. But anybody who's built a research system knows that. So, I would say that's one. The other is I don't see any divide between behavior and design. Maybe because of my background and training, you know, I think they're both mirrors of each other. Human behavior is reflected in the design, and design influences the human behavior. So, another contribution is that it's not one or the other. It's really both. You really need to understand human behavior, and one way to understand human behavior is this notion of Kernel Theory. You know, I wish, I think Walls and Widmeyer invented that term; I forget who did. So apologies to whoever invented that term. But the word Kernel perhaps was not the best one. It's just understanding behavior, and looking at the literature to understand behavior is a shortcut for a designer because there you have all this whole body of literature that has investigated this problem in depth, and they have come up with a series of conclusions for it. Why wouldn't you use that information to inform your design? And then we all know that technology influences behavior. It's not unidirectional. So why wouldn't you then give yourself credit to say that what you've built will change the behavior? That behavior doesn't have to be, you know, technology doesn't have to be evil. It can cause positive change in behavior. It can make people's lives easier, simpler, or happier. So I think that I would say is another contribution, and then smaller contributions, I would say, are, going back to using Simon as the basis, most people are aware of Simon's notion of the inner and outer environment. Inner being, the design process, the outer being back to the behavioral world that we live in. Although he talked about it in much more abstract terms, I think my contribution was to make it more specific and accept that tension as a key part of DSR. Think of it as a positive duality. And then I, you know, I wrote some stuff about how to start, how to document and end, but

others have done that. Probably of that, the notion of the stopping rule is the most interesting because most we still don't know when to stop. That may also be something we can talk about near the end. I think it's still an opportunity for folks to work on the stopping rule. You know, when is the thing you're building envisioning? When is it ready to be unveiled? When is it ready to be tested? We don't know that. We all just guess right now.

[00:15:54] Sebastian Reiners

You were talking quite a lot about these multi-directional relationships that exist during design. There is a lot to talk about, really. And we can only cover just a small scope of that. Maybe when you look back at what you did and what you know now, a few years later, would you put more emphasis on specific points of your contribution or for your paper? So would you change something if you write it now?

[00:16:26] Munir Y. Mandviwalla

Yes, I did actually think more about that. You know, some of the things I would change is work more on the stopping rule. I think that's still an opportunity for others. Try less hard to fit the norms of social science theory, because that's what we're all trained in. So, I think it's okay, and I've heard from others, I'm not the only one thinking that; I've heard from others that, you know, it's okay to be different from social science. We don't have to narrowly fit that rubric. Even the social science researchers are happy to help us do that. Perhaps be bolder. Which relates to the earlier comment. The other contribution in the paper, which I think has been less useful to people, is that I tried to typologize the types of innovation or types of designs that people can work on. But I tried to be too generic. I should have named those typologies rather than just say type one, type two, type three, type four, and all that stuff. That made it too generic. So, probably, that I think could be improved. And then the last one is my idea of documentation, which was to use, for people who are familiar with social science research, borrow ideas from case study research, specifically Yin. The notion of a case study database. I know other DSR researchers have worked on that since then, but I'm not convinced that's practical. We need to find something else to do the documentation. So, those would be kind of three or four things I would do differently, or somebody else could improve on.

[00:18:15] Sebastian Reiners

Absolutely. I mean, future research is always available, just more and more bridges, respectively. Maybe to jump onto that train, really. I mean, there are going to be a lot of researchers, young researchers, starting with DSR, writing their own paper, maybe being in a situation like you 30 years ago, having an idea and not really sure where to follow that idea. I mean, I was in the same boat. I'm still in the same boat. I'm still not sure, like, which ideas to follow and which ideas not to pursue. If you look at this DSR domain, this DSR research domain, do you have any recommendations from this paper, from your time as a senior scholar, from your experience, how to conduct DSR research, how to write these papers?

[00:19:08] Munir Y. Mandviwalla

First, I think the opportunity now is fantastic. You know, 30 years ago, DSR was seen as an odd thing that nobody understood. And it was mostly ignored. So there's so many other people that we all know about who've made it normal. So I think the sky is the limit in terms of what somebody starting out today can do. I would say be bolder. You know, we have the opportunity, especially with the tools available today. When I was building whatever system I built for my dissertation, which I don't even want to mention because it was a huge amount of effort. But today I can probably build that, not necessarily in a day, but almost.

[00:19:58] Sebastian Reiners

Two Days.

[00:19:59] Munir Y. Mandviwalla

Two days. Yeah, because of the libraries and tools available. So really, we can be a lot bolder in our impact on society and on business. I would encourage folks to develop systems that solve real problems in society and business. I think we don't see enough of that. There's nothing wrong with then taking that research and publishing it. You know, depending on whichever model or region you're part of in conferences and journals, but I would also encourage them to commercialize their inventions or at least put them in some kind of practical use. That's what DSR was invented for. And I think the opportunity now is fantastic to be able to do that. And I think part of what I wrote in my paper can still be useful on that. But there are lots of other people who've done interesting work as well. In terms of enabling that opportunity.

[00:22:17] Sebastian Reiners

So, you were talking quite a lot about what you wish for young researchers to focus on to do. You were talking about solving real problems. And if you now have the entirety of the DSR community sitting in front of you, what would you express for them? What is your wish? Is it about solving real problems?

[00:22:38] Munir Y. Mandviwalla

Yeah. There are so many problems that we have in industry. I see people doing more on the social, societal level. But I would wish for more engagement with industry in terms of solving their problems. The industry still has a lot of need for new inventions. And I think with the training that people in DSR get, which is unique in the sense that we can't ignore the environment. We can't ignore human behavior. We can't ignore organizational structures. Yet we have the ability to create new things. I think that is a unique combination that no other group of researchers or area of study has. As you know, DSR relates to IS, computer science, engineering, so many different fields. But I think that the niche we occupy of that tension between, you know, what the world needs and what we can do is a fantastic opportunity. There's so many, as part of my other role at Temple, working with industry, they're hungry for us to invent new things to solve their problems. Even day-to-day problems like processing the payroll, which have been considered solved for the past, you know, even before I was born, people thought the payroll problem had been solved. But, you know, how do you do payroll for people who always work part-time? So, I mean, I picked probably the most mundane example you can think of. There are obviously much more exciting ones. So, I would encourage people to do that. And based on my experience and evidence, the industry is more than willing to engage with us. But we have to show up.

[00:24:31] Sebastian Reiners

That's a very valid statement. I can totally support that. That's actually all from me today. I think we had a wonderful interview. I thank you very much for your time and for your insights into designing DSR. Are there any last words from you, any last suggestions for the DSR?

[00:24:54] Munir Y. Mandviwalla

No, I think this project you're doing is awesome, and I wish you and your colleagues the best of luck.

[00:24:59] Sebastian Reiners

Thank you very much, Munir, and we will see each other next time. Thank you very much.

[00:25:04] Munir Y. Mandviwalla

Thanks. Bye.