



04-12-2024 ALAN HEVNER – TRANSCRIPT

2 SPEAKERS

Sebastian Reiners
Jan vom Brocke

Duration

22m 18s

START OF TRANSCRIPT

[00:00:02] **Sebastian Reiners:**

Hello and welcome once again to another interview round of the DSR Academy. My name is Sebastian, and I'm so happy to have Alan Hevner with me here today. Who is one of the most or maybe the most leading expert in design science research? I'm so happy to have you here today. Do you mind introducing yourself a bit?

[00:00:29] **Alan Hevner:**

Sure. Be happy to. Sebastian. And thank you for inviting me to be part of this, uh, this session. My name is Alan Hevner, and I am a distinguished university professor at the University of South Florida and it's been a real pleasure to be part of the information systems community that has, I think, expanded into some really interesting and exciting research. And I think design science research has been, you know, a big part of why we are so excited and passionate about doing research that makes a difference in the world. So I'll stop at that point and, you know, listen to, you know, your questions. Sebastian.

[00:01:17] **Sebastian Reiners:**

Perfect, perfect. You published an article 20 years ago. So we are at an anniversary, if you will, called Design Science and Information Systems Research and Mis quarterly. And it's rightly regarded to be a cornerstone in information systems research and especially for design science research. When I read the paper first, my main question would be first, why did you write the paper? So how did you come up with the idea and how would you summarize the key paper idea?

[00:01:52] **Alan Hevner:**

Well, to make this very brief, I my background is in computer science. My PhD is in computer science from Purdue. But I felt that I really was more excited and passionate about doing research in terms of applications. You know, bringing technology into business. So I did move to college of business at University of Maryland and now here at University of South Florida. And there was a group of us who were, if you will, more technical doing design, you know, building artifacts and we felt that the information systems community didn't if you will recognize I think the the important work that goes from taking theory, taking concepts and ideas and putting them into practice and, you know, making an impact in the practical community. So we felt and we were encouraged by a number of leaders in the field, such as Gordon Davis, Bob Zmud, you know, J. Nunamaker, and in particular Alan Lee, who was the editor in chief of MISQ at that point that we needed to define, you know, what good design research means in the information systems community. So that was the impetus. And along with Sal March, Sudha Ram and Jin Soo Park, and again, it took us about six years to to write that paper to get it through the review process and we were very, I guess, pleased and gratified that it has made an impact in our field.

[00:03:38] **Sebastian Reiners:**

Talking about the impact, maybe we can talk about the contribution looking back 20 years. Is there anything you are particularly proud of in terms of your main contribution of the paper?

[00:03:52] **Alan Hevner:**

Yeah, I think we brought into the information systems field and maybe even more broadly into the business disciplines, the tension that exists between scientific theory and, you know, pragmatic design. You know, actually building something that is usable and has utility in the real world. And we drew from the book by Herbert Simon, A Sciences of the artificial, where he said, you know, real world problems are messy. They're wicked. And, you know, we can't just reduce real world problems to things that are solvable by, you know, theory. So in all disciplines, there's a creative tension, if you will, between scientific theory and pragmatic design solutions. And I think we brought that to the attention of the information systems community that, you know, it isn't an either or. You're not, you know, either doing design or you're doing science. You're doing both of them. And it's important that we bring that to to young scholars, to doctoral students, and to the field in general that, you know, yes, we're grounded on theory. We build practical solutions, and then we grow theory on what works in the real world.

[00:05:27] **Sebastian Reiners:**

And you did a tremendous job in doing so. I mean, look at the impact it has created. Look at the citation count, if you will, over 20,000 by now, which is insane to say the least. Of course, 25 years ago or 26 years ago, if you will, when you started writing the paper, you had a different mindset. Maybe you had a different field in front of you. So would you change something in the paper today if you write it now?

[00:05:56] **Alan Hevner:**

Well, let me look at it a little different way. I don't think we'd change anything. In the 2004 paper. I think it provided the right, if you will, foundation for additional thinking for, you know, okay. What did what did we not emphasize? You know, and I think we've you know, we've written some additional papers in particular. Let me mention the paper that Shirley Gregor and I did in 2013. You know, where we emphasized the intersection between rigorous theory and practical design to say that as we make an impact, as we put practical solutions in, into place as interventions, you know, there's a role for rigorous research to be done. Rigorous evaluation techniques, whether they're empirical or, you know, positivist or qualitative, quantitative, you know, all these techniques can be applied in good evaluation of designs. The other thing I think we've realized over the last 20 years is, you know, you need a very, iterative, fast process to do this. And here we align, if you will, with the entrepreneurial innovation community to say we need to support, you know, the ideas of failing fast, you know, to building artifacts, to doing evaluation, but doing them in rapid cycles. And, you know, I think that's a challenge we in our community today face is, you know, it takes so long perhaps to, to publish. And, you know, we need a way to, you know, really understand and build theory around solutions where the technology is moving so rapidly that, for example, a AI and generative AI, you know, we can't afford to spend a cycle of 3 or 4 years in a journal publication to say, you know, is this going to work? You know, we need much quicker. We need to align ourselves better with the rapid pace of technology. And I think that's a big challenge for us as academicians and scientists.

[00:08:30] **Sebastian Reiners:**

That's actually a very interesting topic, and I have talked to a lot of other researchers as well in this interview series, who on the one hand emphasize the iterative nature of design science research. But on the other hand, we have this normative process on how we conduct design science and phases. And there are also calls from researchers, authors saying we need to plan ahead. We need to plan ahead our whole design science research process. How do you position yourself as of now? Do you say we plan ahead? Do you say we fail fast? What's your what's your stance?

[00:09:05] **Alan Hevner:**

Yeah. Well, I go back to, you know, the literature on project management says you have to plan. I mean, you have to have a discipline of of planning. But as Mike Tyson would say, once you get punched in the face, the plan goes out the window. Well, it doesn't really go out the window because the discipline of planning is still there. However, you need to be cognizant of the emergent capabilities of the real world. And I always, you know, say as an example during an iterative Build and evaluate cycle. Once you build something, you evaluate it. You feel that it has potential to improve an environment. Once you intervene in that environment, you've changed the environment. You've changed the problem for the next cycle. So there's always a process of, you know, build a solution, intervene with that solution, and then see what the what new problems emerge at that point. So planning is important, but also having the ability to manage, control emergent results and say, okay, the next cycle we're going to do this. And we've tried to do that in a paper that I worked with my colleague Matt Malarkey, uh, where we talked about, you know, the action design research cycles, but saying, okay, there are diagnosis cycles where you're understanding the problem. There are design cycles where you're, you know, evaluating the solutions you come up with. And then there are implementation and intervention cycles. So we need to be able to emerge in emerging process, understand how to define those cycles throughout your project.

[00:11:07] **Sebastian Reiners:**

And if we look at this perspective from maybe publishing, do you follow recent calls from maybe the paper from Robert Winter, Jan vom Brocke and Robert Toononen on the eDSR post, the approach of saying we need to publish more, iterate, sorry, more incremental. We need to publish intermediate artifacts.

[00:11:28] **Alan Hevner:**

Well, there's no question, I think, and this is where, you know, perhaps there needs to be some guidance, if you will, from editors, from mentors, from advisers, particularly for younger faculty who are doing this, and doctoral students to say, okay, when do have you produced a result after, you know, some diagnosis, maybe a design cycle that would be publishable? And, you know, again, getting some rapid feedback through conferences, through workshops such as DESRIST and, you know, other even ACM, IEEE types of venues and then say, okay, now at what point then do we package for a journal presentation for a publication? Um, so, you know, I think there are tremendous number of opportunities. I agree with, you know, other colleagues who have said similar topics, but it does take some understanding and some guidance to say, okay, when can we get it out to a conference? When is it ready for a journal?

[00:12:36] **Sebastian Reiners:**

In the MISQ insider article that was published earlier this year, 2024, you were talking about these niche conferences and these niche, outlets where DSR could also look into. Is that something you recommend for young researchers or for the field in general to be more broad?

[00:12:56] **Alan Hevner:**

Right. And let me also emphasize something. And that's networking. You know, this type of research is not done in isolation. You know, you're not sitting in an ivory tower coming up with solutions for real world problems. Um, you need to interact with practitioners. You need to interact with your colleagues who are doing similar research, you know, all around the world. Um, you know, which is why I really encourage people to present to international conferences, to travel to, you know, attend seminars either virtually or, you know, in person, because the more you can network and interact with your colleagues. I think the first of all, the more you're going to become visible and your creative ideas are going to be vetted and, you know, put under peer review, which is which is important. The more feedback you can get, the better.

[00:13:55] **Sebastian Reiners:**

There was one interesting talk. I had one and a half weeks ago with someone who was inventing a new design sense or design oriented machine learning approached, and he was facing similar issues as you maybe did or other authors in the very beginning of the design science research field did, as in, you were combining something new that editors didn't knew. Um, do you see the pattern reoccurring in the design science research field? As in, we need to establish new research in some kind of editorials in the journals for others to actually conduct this research.

[00:14:35] **Alan Hevner:**

You know, that's that's an interesting question. And I sort of disagree that we're doing something new here. We're doing something that has been done, I would say, since the beginning of time. You know, way back people would invent tools. And, you know, we even see that other animals invent tools. And that's essentially what we're doing here is we first have to understand what the problem is that we that we're working on. And so I always encourage my students and, you know, colleagues to say, what is the problem. So the first real artifact that you build is a representation of your problem with appropriate boundaries, with appropriate goals for your project. I mean, you can't solve every possible, you know, goal. Um, but what is new? I mean, what are the gaps that you're trying to fill? You know, just to say, hey, I'm, you know, I'm analyzing data in an exploratory manner, you know, is probably beneficial, but it doesn't really fit the paradigm of saying, I'm trying to understand a problem for which I have a creative new idea for a solution. And then when I build that new artifact as a solution, how do I demonstrate its effectiveness rigorously via the appropriate evaluation techniques? And there is where, you know, having a good collaborative team, having mentors, having networking for people who are working on similar problems will help you really define the contribution that you're making here. And I think it's a valuable insight that we basically in design science saying there are two types of contributions you need to make both a practice contribution where you're developing, you know, you're satisfying the practice stakeholders and then generalizing your results in a way that you can project or generalize your ideas into maybe a broader problem space and maybe additional, uh, you know, problem spaces that, you know, are related to what you're working on. So, yeah, it's, you know, I don't again, I don't think it's a new paradigm. I think it's a paradigm that's been around for a while. But I think we've made some comments, made some progress in defining what that Paradigm is and what it means to the information systems and in broader. The business disciplines.

[00:17:35] **Sebastian Reiners:**

Thank you very much for the perspective. Maybe as our outro, as our outgoing perspective, is there anything in particular that you desire for the design science research field that you want to happen that you would like to happen?

[00:17:54] **Alan Hevner:**

Yeah, to me, I think it what we're trying to do here is to say we want to make a difference in the world, you know, and entrepreneurs are out there saying, you know, move fast and break things. You know, make a ding in the world. You know, all those are catchy slogans. But I think we as researchers need to say, okay, what problem am I passionate about solving? You know, um, you know, I just met with a new set of doctoral students last week, and they were all presenting, you know, what they wanted to do. And I said, really? Tell me your passion for making that, you know, you're not just here to do checklists to say, you know, if I do these five things, I get my PhD. You have to say, I'm devoting my time, my energy to really making a difference. And you know, what type of impact do I want to have with this research? Um, and I think, you know, if you can answer that question and work with people who are similarly passionate about making that difference, then, you know, that's why that's why we do what we do. And that's what I get excited about.

[00:19:16] **Sebastian Reiners:**

We get excited for passion, which is amazing. I have one quote left, um, and I would like to hear your opinion about that: "Let's dominate the world with design". That was a quote from one other interviewee. Do you agree that we should do that?

[00:19:36] **Alan Hevner:**

Yeah, I agree with the idea. I don't agree maybe with the statement as it is, because I don't think we're we're here to dominate the world. I think we're here to work and collaborate effectively to make an impact. So, you know, I don't think we dominate other methodologies. I think we collaborate with other methodologies very well. It's like I said before, it's not an either or decision. I think if you work in a design science research paradigm that you can incorporate really good ideas methods, theories from any discipline, you know, and I like to think, you know, we are, if you will, a practical science. And we draw from the basic sciences because they understand the world, they build rules, laws of how the world works. We take those rules and laws and theories as foundations to say, okay, now I'm going to build an artifact that's going to allow people to change the world to make it better. You know, and, you know, there I'm certain artifacts, because we are dealing with, you know, ethical considerations. I think that's a big part of what our future research needs to be. You know, we change the world, but do we always change it for the better? And I think there's a tremendous amount of discussion now with generative AI and AI and blockchain to say, okay, we need to have some foundational theories around the ethical philosophies of what we're accomplishing here. And, you know, I hope to participate in those discussions in the future, too.

[00:21:32] **Sebastian Reiners:**

Amazing. Thank you so much. That was a very insightful from you. I thank you so much for being here. Do you have anything left to say or.

[00:21:40] **Alan Hevner:**

No I just want to want to thank you. And if as people hear this and, you know, get excited about it, I'm available. You know, be sure to put my email address. I'd be happy to interact with, uh, you know, with colleagues, with young researchers, because at this point in my career, I think that's a difference I can make is to encourage people to do good research and to do good design. Uh, you know, as part of their identity and, you know, making things, uh, different in the world.

[00:22:14] **Sebastian Reiners:**

All right. Thank you so much, Alan.

[00:22:16] **Alan Hevner:**

Very good. Sebastian. Thank you.

END OF TRANSCRIPT