



## 09-12-2024 JAN MARCO LEIMEISTER – TRANSCRIPT

### 2 SPEAKERS

Fabian Tingelhoff  
Jan Marco Leimeister

Duration

25m 16s

### START OF TRANSCRIPT

#### **[00:00:00] Fabian Tingelhoff**

Hello and welcome back to yet another interview in our interview series here at the DSR Academy. I'm Fabian and today I have the awesome opportunity to talk to Professor Jan Marco Leimeister. Marco, you are a director and full professor at both the University of St. Gallen and at the University of Kassel in Germany. You have published a myriad; I think hundreds of research papers where you also did a lot of design science research. For which I think it's extremely valuable for us to talk to you today and to get your insights onto the DSR Academy and why DSR is important and where to develop towards. But of course, I also want to give you the stage to introduce yourself.

#### **[00:00:43] Jan Marco Leimeister**

Well, first of all, thank you for having me. It's flattering how you introduced me. I've always been a person interested in research that is somehow striving for an impact and as such I've always enjoyed to run large projects together with colleagues from computer science and other fields trying to build prototypes that already have a positive impact while we are researching them. As such, I've written a couple of papers that deal with these specific setups, we call that piloting innovations as a way of inquiry. And that's probably the backdrop of most of the papers that we would refer to today.

#### **[00:01:32] Fabian Tingelhoff**

Definitely. And I think that's the perfect bridge piloting innovations for the paper that I specifically want to talk with you about today. It's one of your earliest papers and one of your most cited papers, coincidentally. It's called Leveraging Crowdsourcing: Activation Supported Components for IT-Based Idea Competition. And you published it in 2009 in the Journal of Management Information Systems. Has a few thousand sites already. And I think this is probably a first incident of your innovation piloting research you were just talking about, right?

#### **[00:02:08] Jan Marco Leimeister**

Yeah, absolutely. It's a rather typical paper structure wise to what I've been doing over the last 15 to 20 years. And it's probably not the first. I think the first one that got published well was 2004 or 2005. A paper on trust supporting components in virtual communities. But the plot is identical when it comes to using well tested causal theories from empirical research backgrounds to inform and to translate behavioral insights into the design features, functionalities, processes and structures. And in the case of the activation supporting components. It's a rather classic topic because we know a lot about the underlying mechanisms of motivations and behavior. And a lot of the IT systems that have been designed in the past do not pick up on the usage side. Although a lot of thought has been put into a user-centered design for making things as usable and as joyful in usage processes as possible. Some of them do not live up to the expectations of being used heavily and intensively. In this setup, we were looking at idea competitions and back in the day's idea competitions were a very timely research topic. Because it's one of the mechanisms how to enable open

innovation or how to enable crowdsourcing and that was one of the hot topics back then and that probably also led to some of the attention of the readership that it was a timely topic back then. But the underlying research challenge is a classic one. How can we design systems, incentives, features and things alike around a given IT innovation so that people actively engage and actively use it over time. And in that paper, we have used the so-called me up model, which is a classic textbook type of proven model from psychology where you know what the root causes for a certain intended behavior are. The process of activation, so a motive is something that resides and sleeps with us within us as human beings need a little spark to ignite it, to activate it. That's the activation process and applying it to idea competitions, we looked at different classes of motives and potential ways that could activate them and use that as what we call theory motivated or theory inspired design. To think about what we can do so that this ignition happens. And that led to a couple of probably not so intuitive design choices and also incentive elements, which as a bundle then turned out to be, although counterintuitive maybe to conventional design, performing rather nicely and as such, it is also a nice test of the theory and apparently it serves to design a couple of good features for such systems. What's also been nice is that some of those features were picked up by companies and are now more or less standard elements of them designing new products.

**[00:05:53] Fabian Tingelhoff**

Well, thank you already for the summary because the summary already touched upon some of my questions, which I would have asked next, for example why you decided to write this paper in the first place and how you would summarize the key ideas. I think we touched upon that. I think you also said a little bit about that too. But I would be very interested in what main contribution of the paper you are particularly proud of in retrospect.

**[00:06:22] Jan Marco Leimeister**

So, pride is always a complicated concept. I feel very positive about our team effort in that project that we managed to design something that has been around in the life for more than a decade now with a software company as a standard mechanism for them to attract outside ideas for their products and services. So, I care about impact and that is a nice measure of impact for me, and it conveys a little bit of self-efficacy. We've done something that was to more people relevant than just the readership. Don't get me wrong, no disrespect to the readership but something that is useful for others. The other thing is I've always felt that as a field in IS, we're not that good at leveraging existing knowledge bases when it comes to designing things differently. To lift our design ideas and design knowledge about systems to the next level. Why is that? Because we have very many different traditions of IS research and a lot of the people doing wonderful work in the behavioral side of IS research very often do not speak to those people building those systems and vice versa. I always felt that's a shame and that's why I've always try to look at those fields and have asked myself, is there a systematic way how I can use that domain knowledge or that mechanism knowledge or that explanatory knowledge that I might get from my colleagues in the behavioral sciences or in the cognitive field to help me design better systems. That's what we basically then called theory motivated design and it's our way how to try to use those explanatory models as like a creative input to guide our search for functionalities and implementation ideas in a more structured and more guided way. And that's probably the part that is still very relevant to me.

**[00:08:43] Fabian Tingelhoff**

Yeah, and probably also to us because this is why we are in the design science research academy after all, right? And I mean, you just framed it. I think theory inspired or theory motivated design throughout your last

answers. So how would you say that this concept refers to DSR? Because I think like back in the days, 2004 or five to nine, DSR wasn't like that established methodology yet. So how would you say does it relate?

**[00:09:10] Jan Marco Leimeister**

So, the first time we applied this early 2000. The term DSR was not well known nor well respected, nor were there any seminal papers out there published. The first one came 2004, 2005 that could frame that term. Nevertheless, the research intent and the research questions addressing design knowledge has been around since the 90s and earlier already. So, the jargon might be a little bit different, but I think the intent is very similar to what we had in mind. Actually, I have not used the term design science research in many of my papers, although they are design oriented research. That is very often because we try to frame our paper so that they fit to a certain readership. And for some readership, it's more a challenge and a stretch to reflect upon methodological approaches like design science research and they're better ways how to frame the paper so that they are well received. So both JMIS papers that we just talk about do not use the term design science research, although the 2009 paper could have used it. We felt like it's a different plot that we can use and offer therefore also other authors an alternative way how to build their ideas on other playbooks. If I was to reflect upon it today, I would say that design science research as we use it in the 2020s is probably something like a research strategy where you have multiple ways of using different methods to come to reproducible design knowledge. At the same time, hopefully also solving real world problems in the process of researching it. So those are the two layers that in most projects somehow correspond to each other. What I would always encourage, and that's something you can either frame, if you think of it like the cycle in the design science research where we look at the existing knowledge base. Something that theory motivated design could reside in there when looking at what mechanisms are we addressing and what do we know about them how could they inform what we design or you could also say it's part of the design knowledge on how to systematically translate certain causal effects into certain feature elements, right? And one could also think about developing something like pattern catalogues where you see certain mechanisms correspond very often with certain combinations of features or functionalities. So, problem solution pairs, one could even think. But that's probably something that needs more research and also formal analysis but that's something we're definitely looking at when doing our own research in the DSR field, how we can advance knowledge in that field.

**[00:12:28] Fabian Tingelhoff**

And as you just said, DSR is a very evolving field, which is not finite yet. So probably since 2009, since you published the paper, a lot moved in the DSR space as well, right?

**[00:12:40] Jan Marco Leimeister**

Absolutely.

**[00:12:41] Fabian Tingelhoff**

So, is there anything in retrospect with all the knowledge you have right now that you would have done differently in your paper that you might have phrased differently?

**[00:12:51] Jan Marco Leimeister**

I probably would be more integrating our theory motivated design into design science frameworks that we use more frequently now. Because that could help also position the work a little bit different and the types of contribution a little bit different. In the papers we have now published, we have different artifacts we consider theoretical contributions than they were accepted maybe 15 years ago. And that is definitely

something, if I had to do the research again and write the paper again, where I would maybe think about a third level of abstraction. So currently we have the lower level is the prototype and the real world setting that we're working in. The very upper part are the causal theories that we use to inform our design process. Then there's something like a middle layer where those different elements in a systematic way should inform each other and how we can also communicate and represent our knowledge of how to make this conceptual design and the mechanism design communicable and transferable to related fields. That's a layer that probably I would focus much more, grounded in the same type of theoretical contributions that we now see with some of the good published DSR papers.

**[00:14:23] Fabian Tingelhoff**

I think that's very interesting, especially also when we combine this answer with the one you gave before about how you can publish DSR and how DSR is seen in the different journals for the different audiences. And here at the DSR Academy, we also try to basically give tools to young emerging DSR researchers to get their work out and publish their work. I mean, we are just discussing papers where you use DSR and publish them quite early where it maybe was a little bit harder to publish with DSR. So, I think the next question I'm very eager to ask you, what would you recommend to those young DSR researchers regarding publishing their DSR in IS journals with respect to your experiences?

**[00:15:14] Jan Marco Leimeister**

So, I think we're now end 2024, early 2025 we still see variance across the top tier IS journals about their openness and willingness to publish design science research or design-oriented research, let's call it a little bit broader. You probably still have to package your research differently according to your target community. One of the impressions I had in the past is that some of our excellently published colleagues that do design-oriented research, for instance, in information systems research never use the word design science to publish the work but rather hide the design knowledge in the prototypes that they use for theory testing. That is one way how to package that and that is a path that is still open and still functioning for everyone trying to do design-oriented research, especially in the field of analytics and AI. Because there very often you do not theorize in the way of someone from social sciences but a rather more driven by the mechanisms and the design and the data themselves. So that's definitely one playbook where I would see if you're very driven or doing a lot of research in that field then you weigh how to package your design research is probably one that fits to that type of research nicely. But there are other journals that do have editorial boards where a lot of our great day's DSR colleagues have editorial roles. That's usually a good signal that this is a home where you can feel comfortable in sending your work. What I would always ask our colleagues is about the intended contribution. Each paper can either try to make a subject matter contribution, so actually in solving a problem that is addressed and then the audience is everyone interested in the problem. And your contribution should resonate well with them and should be considered as valuable for anything that deals with those problems. So, let's say sustainability challenges or data privacy problems that you try to resolve, all those sorts of things. Then the way how to package that is probably using DSR as a way to inform the problem solving and you win or lose on the quality and novelty and validity of your results of the problem solving. We always say in design science research; it's not about truth it's about usefulness and utility. Well, that's what you bet on. If you have a good contribution there, if you feel comfortable that you will find good ways how to publish that in good journals that have corresponding editors, associate editors, senior editors or editors chief that appreciate that. They bet on usefulness of the research finding. If I may call it that simplistically. That's one strategy. The other one is, of course, and I see also a lot of opportunities in that field is to advance the methods. So, if you're after improving systematic ways of

generating design knowledge then you should speak to the audience interested in design science research as a method overall. And I think we can observe this distinction quite nicely at the Design Science conferences like the DESRIST and also some of the tracks at our major conferences that are more open to design science research. There's always an audience interested in novel ways of doing things. And just to give you an example anything that for instance helps us automatically translate things that we get out of interviews into design features because we now can auto transcribe them, can do text analysis on that, can use Gen AI to build something on top of that to me would all be candidates that we could explore for method contributions and for novel methodological additions, augmentations, improvements of design science research as we know it. But my advice to my younger research colleagues would always be consider your primary objectives with your research. And if it's about getting published fast, I would usually not do method papers. Because method papers, at least in my experience tend to last longer. They might have more impact. They might get more citations. That might be but it's much harder to get them published. Because the level of peer review feedback and everything I perceived as just lengthier and more laborious. So if you're in your first or second as a PhD student, it might not be the best idea to look for a method contribution on that level, but rather something where you can solve real world problems and get to self-efficacy faster because that's a great dopamine rush that you can get that way to see that your impact works and motivates you. I would always encourage to have that first and then maybe at a later stage, use all the experiences made on the way to then consider a methodological contribution.

**[00:21:03] Fabian Tingelhoff**

Thank you so much for these insights, Marco. I think they're very interesting and they perfectly tie into my last question for you because it just touched upon that DSR is evolving, that you can make method contributions still. And I think you also touched a little bit on the aspect of what is more important, generating knowledge or solving problems. So basically, DSR is evolving, but where to? This is my last question where I would really like to get your opinion too. What do you desire for the DSR field as a whole? And what would you like DSR to look like in another 15 years?

**[00:21:40] Jan Marco Leimeister**

So, the way how you ask the question triggers very different thoughts in my head. The first one is, since you mentioned it's evolving, that is probably a sign of either a nascent phase or a lot of activity in the field. I hope it's the latter. So that we have many different concepts of how to advance the shared interest to generate useful design knowledge and solve real world problems. And at the same time, I hope it's not an either or. Because to me, one is a means to an end to the other. So, I feel personally not so comfortable doing design science research that analyzes almost empirically how others do design because that's to me not the way how I like to experience my self-efficacy as a researcher. I enjoy much more trying to solve problems myself and on the way of doing that, trying to abstract and learn from that and offer insights for others that might have similar design challenges, but that's a personal flavor type of thing. So that's the first type of thoughts that crossed my mind when listening to your question. The second one is where would I love to see the field move? Well, I would love to see that we get to a level where design science research is not something that to some in our field feels like dubious, less mature, shady type of unclear and therefore not something you can consider to review, but rather something that is in the same category of accepted methods like qualitative approaches, interpretative approaches or also a rather positivist hypothesis testing type of empirical work. And once we are on that level, I hope that we can then go beyond the method conversation and think about what is the type of knowledge and insight that we offer our readers. Because that is probably what unites everyone in research trying to offer insight, inspiration and knowledge to an

interested, educated reader. If we manage to do that, regardless of the method, I think then we're on the level where I hope that we should be in 10 to 15 years. And I also hope that everything we're currently experiencing with the incredible amount of innovations coming out of AI and also generative AI. This will accelerate this hopefully. Because I hope this will be like a steam engine invention to design science research trying to get us to the next level of conversations and once I can talk to anyone interested in a certain problem domain no matter what research approach, he or she subscribes to then I'm at the stage where we are actually going beyond the method conversation. I'd love to see that.

**[00:24:48] Fabian Tingelhoff**

Wonderful final words. Thank you so much, Marco, for taking the time and for being my guest today. And I'm sure that we are all eagerly awaiting your further contributions and further publications that contribute towards moving away from these methodological conversations towards final readership.

**[00:25:11] Jan Marco Leimeister**

Thanks for having me.

**[00:25:12] Fabian Tingelhoff**

Perfect. Have a nice day and hopefully see you soon.

**[00:25:15] Jan Marco Leimeister**

Likewise. Bye, bye.

END OF TRANSCRIPT