

Craft, Curiosity, and Knowledge: Duncan Watts in Conversation with Philipp Brandt

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Abstract

Duncan Watts' work has helped define how we study social relations, collective dynamics, and digital platforms. It ranges from articles combining formal modeling and empirical discoveries to widely read books. He has moved between disciplines and problems, shaping the fields of network science and computational social science along the way. In this conversation with Philipp Brandt, he retraces his steps and maps out an integrative framework that flips the research design process to start with problems and produce generalizable knowledge.

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1 Arrivals

Philipp Brandt: Duncan, your path was anything but conventional, moving from Australia into a PhD program in engineering at Cornell and eventually into a sociology faculty position at Columbia. Having somebody who entered the field abruptly offers a rare glimpse of the boundary of our discipline. I'm curious about your impression when first arriving in the social sciences.

Duncan Watts: Well, I was very young. I was 26 when I graduated with my PhD, and at that stage in my life, I did things that I didn't really think too much about or what I was getting myself into. Four years earlier, when I was 22, I was in the Navy in Australia, and I got myself out of the Navy, left all my friends and family, and moved to the U.S. to go to grad school. I didn't know anyone. I got there, and I thought, "Oh gosh, what have I done?" And four years later, I did the same thing. I decided I wanted to stay, and I got this postdoc with Harrison White at Columbia.

I got a little bit of an introduction to sociology at that point through the Lazarsfeld Center, and I met David Stark, and I met Peter Bearman, and I met Chuck Sabel a little bit later. So, I met some key figures at Columbia around that time. But I wasn't that involved in the department during that year. My funding ran out, and I had to get another job.

I ended up at the Santa Fe Institute. I didn't want to go because I loved living in New York. But I had no choice, and I ended up moving to New Mexico for a year. This was again mostly Harrison getting things done behind the scenes. I did not like living in New Mexico. It was a very beautiful place. The Institute is great. But from a personal perspective, it was a bit lonely and alienating.

I arranged to do a third postdoc, this one at MIT in the Sloan Management School with Andrew Lo, who's a finance economist. He was an old friend of Steve Strogatz from grad school. I had been invited to give a talk out there, and Andy came to the lecture, and then afterward said, "This work seems interesting, I wonder if it has to do with financial crises, which is something that I'm interested in", and we agreed to work together.

So, I did three postdocs in three years. One in sociology with Harrison, one in complexity science at the Santa Fe Institute — that's why I started collaborating with Mark Newman, who, of course, is a physicist — and then one with an economist, at Sloan.

At the very end of my time at Santa Fe, as I was getting ready to move to Boston, I got a message from Chris Scholz at Columbia. He was in the Earth Science Department, or the Lamont Doherty Earth Observatory, as it's called. He was also someone who knew Strogatz, and Strogatz had visited there and given a talk about our work. And Scholz said, "I think this network stuff is interesting, would you be interested in coming back to Columbia in a faculty role?"

The background was that Michael Crow, who is now the president of Arizona State University, was a vice president or provost at Columbia at the time. He had this very unusual arrangement that I never quite understood. My vague understanding is that he had a pot of money that came from this particular patent that the medical school had gotten. He was using it as an internal venture capital fund, and he had these little projects that he would fund. Somehow, Scholz had gotten to him and suggested that maybe he could hire me for a faculty position, and I said, "That sounds great! I love that".

It took a few months to come together while I was in Boston. I did an interview with Michael Crow, and I guess he liked it, and things moved ahead. But at some point, Scholz said

to me, “Well, what department do you want to be in? I was thinking you would be in applied math, because that’s more your background, but you could be in whatever you want”. And I said, “If it’s all the same to you, I would like to be in sociology!”. And he said, “I don’t care!”.

Then I had to do a second interview with the sociology department. I went and gave what seemed like a regular job talk to the department. I’d be curious to hear what the faculty meeting was around that. My guess is it was presented to them as a free appointment. I think Harrison and Peter and David muscled it through. I’m sure that there were people in the department who were highly skeptical. But I never heard about what happened behind closed doors, and I don’t know how it was sold.

PB: Well, they stuck with you. But this was also just shortly after Harrison had hired Peter and David to begin with, right?

DW: Yes, I met David and Harrison the same day, because this was back when I was at Cornell at the beginning of ’97. Harrison came up to recruit David Stark to come and be the new chair of sociology. I did not know that, of course. All I knew was that I got a phone call one morning from Ron Breiger, who was in the sociology department at Cornell at that time, saying Harrison White was coming in and would like to meet me. I did not know Ron Breiger. I knew who he was, but I’d never spoken to him, and I don’t know how he got my phone number. I guess he looked it up in the white pages, as one did back then. I was totally shocked to hear from him, and also that Harrison was showing up later that day.

By the way, there’s a whole social networks story. One person knowing another person who knew another person, and ...

PB: ... but in a complicated way, that’s what I find interesting. It’s not a very linear social network story; it’s layered.

DW: Well, I could tell the linear version, but I would have to go back in time even further.

So, Harrison shows up, and I did an impromptu job interview with him, much to my surprise. He said, “There’s a whiteboard, go and tell me about your dissertation”. That was it. I did this impromptu lecture with Strogatz, Harrison, and David Stark, and a couple of other people. David Gibson, Harrison’s then-PhD student, and Shin-Kap Han. Anyway, Harrison said, “You should come be a postdoc”, which I thought was great, and that’s when I met David.

I met Peter when Harrison was recruiting him from North Carolina while I was a postdoc. So, I had a few people who I knew were supportive of me coming back, and somehow, between them and Michael Crow, they got it done. But it wasn’t until that moment when I showed up in the fall of 2000 as a new assistant professor in sociology that I realized what I had done. David says, “Great, why don’t you teach a course in networks?” And I said, “Okay, I can probably do that”, and I had the fall-off of teaching to get myself prepared.

At that time, I had also decided I wanted to write a book. I thought, “I don’t know how I’m going to do both. How am I going to write a popular book and teach a brand-new course?” It seemed overwhelming to me, and then I had the bright idea that I would write the book as the course. It would be the course text, and I would write it in real time. Every week, I would have to have new material to teach the students, and that would force me to write the book. And that was *Six Degrees* (Watts, 2004). *Six Degrees* was written under the gun of having to have stuff to teach my students the next week.

PB: How did the students react?

DW: I had a mixture of students. It was listed as a sociology course, but I asked them if they would advertise it more broadly. I don't know if they did or not. It was tricky to get students because it was a new subject, and people didn't know what it was. To the sociology majors, it looked a bit technical, and the computer scientists and physicists probably didn't think it was useful for them to have a sociology class on their CV.

I think it only really dawned on me as I was about to walk into the first classroom that I had never run my own course. I had done a bunch of TA-ing at Cornell, but I was teaching math, linear algebra, and calculus to undergraduates. I had never taken a course in sociology. I had certainly never taught a course in sociology. I had no background in sociology whatsoever. And all of this came crashing down on top of me as I was about to start.

I felt tremendously nervous, walking into my first classroom, that I would be exposed as a fraud. I was also kind of shocked at how little oversight there was. I thought, "They're not going to let me go and teach a bunch of kids who are paying all this money to be here ...".

PB: ... welcome to academia.

DW: But that's what they did, and I was like, okay, I'll go do something. David said, "Oh, you'll be fine," and I was fine, as it turned out, and we got through it, and they seemed to enjoy the class, and I wrote this book. That was the first network science class that anybody ever taught, at least in the modern tradition of network science — obviously tons of networks classes have been taught.

2 Regression

PB: Do you have a recollection of encountering the discipline for the first time? For example, at a conference or workshop meeting?

DW: I remember when I was a postdoc, I went to a regular sociology seminar. It was some quantitative paper that was being presented. The presenter did the usual thing of describing their theoretical framework and the case, and then the data.

At some point, he puts up a regression table with all of these asterisks. I asked,

"What's that? Why are there stars on those things?"

He said, "Those things are statistically significant".

"What does that mean?"

"It means they're different from zero".

"Why is that important? How big is the effect?"

"Oh, I don't know. It's statistically significant".

And I remember thinking, "What a weird concept". I wondered,

"What function is it that you're fitting?"

"Well, it's a linear function".

"How do you know it's linear?"

"That's what it is. That's what a regression assumes".

I'd done my PhD in nonlinear dynamics. There's a famous saying attributed to the Santa Fe Institute, I think to John Holland, one of the early giants of complexity theory, that calling something nonlinear is like calling it a non-banana. In my world, everything was nonlinear. Linear was the special case, and it required very strong assumptions.

When this person said, “Well, I’m fitting this linear function”, I thought, “That is such an odd thing to do. I don’t believe any of these results”.

On the one hand, it showed my ignorance that I had never heard of a regression equation before. I’d never taken statistics, which is a shocking hole in my education. Even as I have learned about what regression is, I still think it’s in some ways a very strange thing to do, and one should be very clear-eyed about what it is and how you’re interpreting it.

So yes, there definitely were some sort of culture clashes as I tried to learn how sociologists do what they do. But ultimately, it was a tremendous experience for me. I came in as this alien and had to learn this new language. I was learning it as somebody who had their own language and their own framework. It was different for me than being sort of indoctrinated into a tradition.

PB: There was friction.

DW: Yes, and really, that has shaped my whole career ever since, trying to square the circle. I’ve been trying to figure out how these round pegs fit into square holes, or whatever your metaphor is. From the very earliest days, I was reading across sociology and psychology and economics and political science, and physics and computer science, and I was getting extremely frustrated. All these different disciplines were ostensibly talking about the same problem, but were doing it in such different ways that it seemed to me they could never be fit together.

I started asking myself this question that I’m still asking: “What are we doing?” Really, “What are we doing here?” And is it the thing that we think we’re doing? Because it’s really not clear to me that we’re creating cumulative, useful knowledge about the world when we’re doing it in such a disconnected, incoherent way. I don’t think I ever would have had that perspective if I hadn’t forced myself to go and to become a sociologist.

It cost me a lot, it really cost me a lot. I forgot a lot of stuff that I used to know. I forgot a lot of math. I forgot a lot of computer science. I used to be able to do a lot of things that I can no longer do, because I devoted years and years of my life to really trying to learn sociology. It meant I lost a lot of productivity, and I lost a lot of skills.

But I think it was worth it, because what I got from it was this experience of trying to look at problems from many different perspectives, and really trying to figure out how they fit together, and realizing that they do not fit together. That shaped my agenda for the last 15 years, trying to figure out how to solve that problem, which is not a problem that many people are focused on. Everyone’s in their furrow, doing things the way they know how to do them. No one’s looking at all the furrows and noticing that they’re going in different directions.

3 Discoveries

PB: What has struck me in your work is that a lot of it has an element of a real discovery. A lot of what we look at in social science confirms or rejects things we expected, often by design, because we have these hypotheses, and because we’re specialists, we have subfields and sub-areas and so on.

I wonder where this enters your research process. There’s your larger trajectory, but then you also focus on concrete problems, designs, and writing. At what point do the discoveries that are genuinely surprising enter your research process?

DW: It’s slow.

A long time ago, I read a book about complexity science that featured Brian Arthur, an economist associated with the Santa Fe Institute and one of the early pioneers of complexity in economics. He was quoted as saying that some thinkers have this mind that is like a blitzkrieg. They just assault the problem. They overwhelm it, they penetrate walls, and they can blast through things with their intellectual horsepower. But Arthur described himself as having more of a siege strategy: he would camp outside the walls and just sit there until the problem eventually gave up.

I liked that analogy because I am also more siege than blitzkrieg. In these movies, they start scribbling furiously and solve these impossible problems. I've never been able to do that. I was never that good at math, or really anything. I don't have these feverish, I-figured-everything-out-overnight eureka moments. I get puzzled by things. And I guess maybe my trick is I get puzzled by things that aren't puzzling to other people. Other people walk past these puzzles, and I come along and think, "That's weird".

PB: But that's so hard!

DW: I'll give you an example, which I have written about (see Watts & Dodds, 2009). Back at the end of the 90s, I got interested in social contagion, things spreading, cascade models, all that stuff. I started reading papers, and I read lots of them, but there were two that stood out to me. One was Granovetter's (1978) paper on cascades. The other one was a paper by Bikhchandani, Hirshleifer, & Welch (1992) in the *Journal of Political Economy* in 1991 or '92.

What was interesting about these papers is that they were motivated in almost exactly the same way, including the same examples. They asked: in financial crises, riots, political revolutions — why is there nothing happening, and then, all of a sudden, this big thing happens, and nobody sees it coming. How does that work? They both propose theoretical models to explain the same phenomena in the world. But I really struggled, asking myself, "What's going on here? They're not the same model".

It turns out, after I stayed there for a long time, not only are they not the same model, but they're logically incompatible with each other. They could both be false, but they can't both be true. They make assumptions that are fundamentally at odds with each other. The whole mechanics of how these cascades are supposed to work diverge, and they make different predictions.

What I found so striking about that is that if science operated the way I had always thought it did, it would immediately be clear that these theories made different predictions about the world. Someone would design a test to differentiate between them. Someone would say, "Wow, they can't both be true, let's do an experiment and find out which one is right. Or maybe they're both wrong". But not only had no one ever performed that experiment, it seemed that no one had even noticed that there was a problem.

By the time I read them, Granovetter's paper had been out for decades, and Bikhchandani et al.'s had been out for almost a decade. They both had thousands and thousands of citations. They were both published in the top journals in their fields by the most famous people in their fields. And yet, they're sitting there, and no one's concerned that they are not commensurable with each other. Of course, that problem can then be multiplied by all the other theories that are sitting around out there about how things spread through networks, which don't fit with either of them.

And so that really bothered me 25 years ago. And it's still bothering me. That's how things come about with me, where I'm just thinking about something.

It's the same with the small world stuff. I was mulling things over. I was talking to my dad

one night on the phone, in Australia, and he asked, “Have you ever heard this idea that you’re always six handshakes from the president?”

I said, “I’ve never heard that, but that’s intriguing. I wonder if that’s true”.

Then I thought, “Oh, that sounds like a math problem”.

And I started thinking about that. I was working on crickets and synchronization, scrambling around in trees trying to find these little buggers I couldn’t see. I’m thinking, “This is not a mean-field network. This is not a two-dimensional grid. What is this?” And then: “Oh, I wonder if it’s like that six-handshakes thing”. So those ideas started coming together in very rough ways.

PB: You’re persistent in your curiosity, making progress in figuring it out, and then it turns out to be larger than it first appeared.

DW: Right. It starts off as this very vague, nagging thing in the back of my head. Maybe I’m crazy. Maybe there’s some obvious thing that I missed. Somebody surely has figured this out. Then I go and look and can’t find anyone who has figured it out, and I think, “Maybe they haven’t”.

I try something. Then I try another thing. Then I try another thing. Over the years — decades in the last case — I keep circling around the problem. Again and again and again, making incremental progress. Just like Arthur’s siege.

4 Collaborations and Contexts

PB: How do collaborators deal with that? The point was that many people don’t pay attention to these inconsistencies. Of course, you pick your collaborators, but even then, not everybody sees the surprise at the same time. Do they push back? Are they not surprised?

DW: It’s different for different circumstances. For the small-worlds project (Watts & Strogatz, 1998), it was really Strogatz and me. I sold him on the idea, he got excited about it, and we worked together very actively on it. Mostly it was me doing the work, and he would say, “Try this, try that”.

But other projects have unfolded over much longer periods of time. I work with different people on different pieces. I don’t really ask them to buy into the whole thing. Often, they’re PhD students, postdocs, or junior people. I don’t want to compromise anyone’s career by getting them to work on something that they’ll never be able to complete in their lifetime.

In general, I tend to give messier problems to students than many people do, and they are sort of less productive. It takes them longer to do things. Some of my colleagues are amazingly good at giving somebody a chunk of a problem, and they can go and do it, and then three months later they have a paper. They’re extremely good at parceling out problems in ways that are doable for grad students.

I’ve always had a tendency to give them more ... — but I don’t try to give them the whole thing. It’s a bit of a combination of indoctrination and sparing them the agony of doing this with me for years.

Peter Dodds was the first person I worked with in this way, mostly on network and contagion problems. After that, I worked with Jake Hofman for many years, and he and I have written several papers on the theme of explanation and prediction (see Hofman, et al., 2017; Hofman et al., 2021). More recently, I’ve worked a lot with Abdullah Almaatouq at MIT.

Abdullah came as an intern at Microsoft. He was my last intern there, and I had hired him to work on a specific project. He showed up and said, “I really want to give a presentation to you. I’ve got this big idea, and I want to tell you about it”.

I thought, “Ugh, I don’t want to listen to this”. But he kept nagging me, and eventually I said, “Fine. We’ll book a conference room, I’ll sit there for an hour, I’ll listen to you, and then I don’t have to hear about it again”.

Then he gives me this presentation, and I’m thinking, “Oh my God, this kid is thinking along exactly the same lines and has actually figured some stuff out. That’s amazing”.

This one conversation changed my whole view of him. Now he’s probably my closest collaborator, and we’ve written a bunch of papers together. He’s phenomenal, and we’re very much in cahoots at this point.

PB: I was wondering about your time at Yahoo! and Microsoft. They offered a different environment. They were called labs. I’m curious how they changed your process to be a social scientist, given the opportunities and the kind of dynamism you found there, and you could create there.

DW: People often ask me why I did it. I had this great job at Columbia. Why did I leave?

PB: Well, you had to write grant proposals.

DW: Haha, kind of, yes.

When I graduated from my PhD, I wasn’t sure what I was going to do with my life. I had loved the experience of being a grad student. I was doing research and thinking deeply about a problem. But I had never been particularly enamored of being a professor. My dad had been an academic, and he had left after doing a couple of postdocs and become a farmer. He got sick of sitting in the cafeteria and listening to people talk about the same stuff all day. He felt like he was a practical person who didn’t really love all the trappings of academia. I think I inherited from him a little bit of skepticism about the professorial class.

So, although I really wanted to do a PhD, I hadn’t gone into it planning to become a professor. In fact, when I pitched the small-world network idea to Strogatz, he said, “This is interesting, but I have to ask: are you interested in having an academic career? If you are, this is not a good idea. Academia is a disciplinary environment. You do math, or biology, or whatever — but you have to pick something people recognize and specialize in. This is not that. This is in between everything. If you do this, you’ll never get an academic job”.

I said, “That’s okay. I don’t care. I don’t want an academic job”.

And he said, “All right. You can do it”.

I had this mindset that I wasn’t going to be a professor. But by the end I felt like I wasn’t done. I thought I could figure everything out in a couple of years, but it quickly became clear that it was going to take much longer, and...

PB: ... you wanted to figure it out.

DW: Yes, there was still curiosity; I wanted to keep doing it. But I didn’t want to do all the stuff that I had heard about that made this so tedious — grants, publish-or-perish, and all that.

I made a deal with myself that I would keep doing it as long as I could do what I wanted. As soon as I found myself not doing the things that I wanted to do, I was going to quit. I thought, “I don’t care about any of this stuff. I’m not going to try to get tenure...”.

PB: ... which still happened...

DW: ... yes, that still happened. But back then I decided: I'm going to publish wherever I feel like publishing and write about whatever I feel like writing about. I'm going to work on whatever problem I feel like working on. And if somebody tells me I can't do that, I'm done. That was the deal I made with myself. I've tried to stick to it ever since, and it filtered into decisions about where to go.

My rule has always been that I want to be at the place where I can do the work that I want to do. I try not to think about prestige or any of those things. I can't help it, of course, those things matter to me now in ways that they didn't when I was 26. But I have always made the decision based on what I think I can do.

At first, Columbia was great, because they wanted me to be in sociology, and I wanted to learn about that and become a sociologist, and that was the place to do it, because they would let me.

PB: It was also a unique place with Harrison still there, who wasn't only a sociologist by training himself, but also the group he was bringing together. It wasn't necessarily representative of sociology.

DW: Yes. It was probably not a typical sociology department. But it allowed me to do what I wanted at the time, which was all that mattered to me. Then it no longer worked because the nature of what I was doing was changing, and I was starting to do computational social science.

There was more data and experiments and a lot of coding, and it was very hard to find graduate students who were applying to the sociology department who had the technical skills to be able to do this work. The computer science department at Columbia was not interested in talking to me at the time — that has changed now, but at the time, they were not interested. It was also very hard to get funding. I had been lucky early on and gotten a few grants, and then everything dried up. I was spending more and more time doing things that were not core to my research.

When this opportunity came up to go to Yahoo!, I didn't really take it that seriously at first. "Okay, I'll go talk to them", I thought. But I went out there, and it was so refreshing. They were so smart and interested in real-world problems. They were all computer scientists, and I had never talked much with computer scientists. I enjoyed it, and I thought, "I don't know, maybe I should do this".

Of course, at that point I was a tenured Columbia professor and starting to think, "It's nice, too". I would go to parties and people would introduce me as a Columbia professor, and everyone would be like, "Ooh, impressive". Yahoo! at the time was already struggling as a company, and nobody could understand why I would give up what I had to go there. It was a gamble.

But it really was a fantastic four and a half years. We built this little group. It was Sharad Goel, who's now a professor at the Kennedy School, Winter Mason, who is now at Meta, and Jake Hofman, Sid Suri, and Dan Goldstein, all three are at Microsoft now. It was the first computational social science group that anyone had put together, and it was a wonderful experience.

Then we got a CEO at Yahoo! who only lasted about four months before he was fired for lying about his CV. But he still dismantled the research lab in those four months. He came in and said, "What is this? Why are people doing research? Who are these people? They don't even meet with customers. What a waste of time. Let's get rid of them".

So, there was this diaspora of Yahoo! researchers who went to work for all their competitors. We went to Microsoft, Google, Facebook. All of a sudden, he saw all these researchers getting

paid more by these other companies. And he said, “Oh, whoops”.

But by then, it was too late, and the whole New York lab decamped for Microsoft. I spent another seven years at Microsoft, which was great, but then, again, the world had changed, and Microsoft at that point had become very focused on AI, which is a trend that has only continued.

Meanwhile, computational social science had taken off in the university world, which made me think, “Okay, it’s time to move back again”. I could do things in a university that I couldn’t do in a corporate lab.

So for me, all the back and forth has always been in service of a consistent goal. And I’ve always tried to follow the same principle about when to leave and where to go: where can I do the work that is most important to me?

5 Thinking

PB: Many of us operate more on the basis of what looks continuous in terms of some overt trajectory, but for you, it’s the principles and the consistency in the craft.

What has always struck me in the interviews you have given and in your *Sociologica* piece about revision is how important the writing process is for you (Watts, 2022). Looking at your work, it looks so math-first, so data-first, and so on. I find that tension fascinating: the process of collaborating with others, but then also stepping back and figuring out puzzles. How does it work for you?

DW: It’s funny, for much of my life, or at least my young life, I thought of myself as someone who was good at math and physics. And it was only in my final year of my PhD that I had this painful realization that I was actually not that good at math. I was good by normal standards, but compared to other people in PhD programs, I was at best in the middle. That was something that I had to wrap my head around.

But then I also realized that what I had always been good at was writing. In fact, in high school, English was my best subject. But I never really thought of myself as a writer because I had this identity as a math-science person.

On the other hand, I always loved reading. I was a voracious reader as a kid, and it had always been important to me to write well. But it’s come to mean more than that. Many people have said that writing things down is thinking. You can have all kinds of incoherent ideas in your head that seem to make sense until you try to write them down, and then suddenly you see all the holes in your reasoning.

So, writing has always been a process of ordering my thoughts. It’s particularly true of the ideas you mentioned, which have come together over many years — this pile of notebooks I have over here, it’s literally four feet high at this point. I used to take extensive notes. When I read back through those notes years later, it’s the same ideas over and over again, but evolving. There’s progress. I’ve used writing as a way of thinking for as long as I can remember.

PB: That really dovetails with how qualitative sociologists say writing is part of the analysis.¹

DW: Yes, others have said this, and I agree.

1. E.g., Glaser & Strauss, 1967.

What is also important to me, and I feel some pride in it, is that I try to be very clear. I try to say exactly what I mean when I write.

I've had problems with editors who change things when they think something sounds better. My reaction is, "No, you can't change that word. I used it because that's the exact word I meant, and it's not the same as the one you're proposing. Please change it back". They may think, "Why is he being so fussy?". But to me, it's always been very important to say exactly what I mean.

This gets at a larger point about these problems that I have hung on to for years. We sociologists, or scientists, are not always clear about what we mean. We're using words in ambiguous ways, and it's causing a huge problem for the advancement of science.

I teach a PhD seminar each spring called *Explaining Explanation*. It's all about what we mean by the word *explanation*. It's a fifteen-week course about what we are doing as scientists. Our job is to explain the world, right? That is what we think we're doing. But we're not doing what we think we're doing. We're using this word in different ways and conflating those meanings. It is not a small problem. It's a very big problem.

To me, using words clearly is not a stylistic question. It's a scientific question. It's a principle. It's hard for me to say what the cause and what the effect are, but writing and clarity of thought have always been wrapped up together. Both are core to the process.

6 Community

PB: I wonder if there is an interesting tension here between the insistence on clarity and your career trajectory. You have not only changed your own path, but you have also changed with how social science has changed, or science more broadly. You trained in an established discipline, in engineering and math, and then entered sociology, which is another established discipline. But then you were involved in network science and computational social science more recently.

There's room to make a point, there's new energy, new momentum, but there's also a lot of ambiguity. How do you reconcile your aim for clarity and the moves between different standards, disciplines, and emerging spaces?

DW: I think I have always been interested in the same questions. The way I have gone about answering them has changed depending on what was possible.

I'm interested in collective human systems. I was interested in that in 1995, and I'm still interested in that 31 years later. The tools are all different. The data is all different. Everything is different. But I'm still trying to figure out the same fundamental phenomena.

Along the way, though, I'm grabbing onto whatever I can get. I'm very catholic when it comes to disciplines, theories, methods, platforms, and sources of data. I'm a kind of scavenger. I'm not attached to anything other than the problem. I want to try to figure things out. In that sense, I was a network scientist when that was helpful, and then when it wasn't helpful anymore, I went and did something else, which became computational social science. Who knows, maybe I'll do something else again.

Other people had their own motivations for giving names to the work I was doing. It was never my intention to make network science a distinct field, nor was it ever really my intention to make computational social science a distinct field. I didn't even like the phrase computational social science. Back in 2009, that meant agent-based modeling, which was fine but not what I was doing. At some point, I realized that the ship had sailed and so I decided I would

call what I was doing computational social science to be consistent with what everyone else was calling it. Honestly, I think it worked out pretty well, but I can't take credit for the idea. I was really just focused on trying to do the work, not on branding it.

And the branding was contentious. There were lots of arguments about what should count as computational social science and how "computational" it had to be versus, say, just ordinary quantitative social science. The same thing happened with network science, which also pulled on lots of pre-existing work in adjacent fields.

I was also involved in the early days of the field of collective intelligence. And once again, we spent a bunch of time arguing about what that was. Tom Malone at MIT was one of the prime movers, and he had this very general idea of collective intelligence. I thought it was too general, and I was pushing back.

Michael Kearns, who's now my colleague at Penn and originally recruited me there, had what I thought was a very wise insight. He said, "When you're starting a new field, what really matters is who shows up. The field becomes the set of things that those people do. Get the right people in the room, and everything else will take care of itself". I thought that was a really good idea and I've tried to keep that in mind ever since.

Mostly how I've done that is through the International Society for Computational Social Science, which I helped to start, and which now oversees the main conference in the field, IC²S².² The way we've always looked at it is that whoever shows up to IC²S² is what computational social science is. So, we've spent a lot of time working on that to keep it from becoming dominated by any one discipline.

But you're right, building that community has been very different from how I think about my own work. In my own work, I get to do things exactly the way I want to do them and use words exactly in the way that I mean them. In building a community, I've had to embrace all the differences in motivations and meaning that people bring to it. So, it's been a different sort of challenge, but also very rewarding in its own way.

PB: But these commitments aren't necessarily so different. You are interested in understanding collective human activities. Other people you get in a room with are interested in that, too. If you assemble people like that, then the conversation will be coherent and productive in one way or another. If you're focused on a problem in the world, then you can build a community around that to do whatever needs to be done to solve the problem.

DW: To some extent, I agree in as much as my preference is to start with the problem and then work backwards to everything else, like the methods, the data, and so on. But the point I was making is that there are lots of other people who also identify as computational social scientists but who probably don't think that way. And I'm okay with that. Even though it means some incoherence — which I try to resist in my own work — I'm happy to tolerate it for the sake of building a vibrant community.

I go to these conferences, and I think they're wonderful and exciting. They feel to me like the conferences that used to happen 30, 40, 50 years ago — or at least as I imagine them based on what I've heard and read — before everything got so intense. We don't do proceedings. The review process is pretty light. It's really all about coming to the conference, talking, interacting with people, and sharing ideas. There's no line on your CV that helps you get tenure. The only reason you do it is because you want to go to the conference.

2. See: <https://iscss.org/about/about-iscss/>.

PB: Well, I can imagine younger people who attend these conferences thinking differently about them, including as a line on their CV and maybe even as a meeting place for getting a job.

DW: Yes, of course networking happens there, and that's important, but I think we have avoided the prestige and the labeling. In computer science, your CV is all about the number of NeurIPS papers and ICML papers, and that's what hiring committees look for. In sociology, it's *AJS* papers, *ASR*, whatever. There's a tremendous amount of importance attached to the label. But listing an *IC²S²* paper on your CV on its own is not going to help you get a job. So, it's about going there and interacting with people and ideas. I love that about it.

To be clear, I also have very strong feelings about science, about what is good science and what is not. And even at *IC²S²* there is a lot of variation in quality. Sometimes, I'm tempted to be judgmental about it, but then I try to remember a second piece of good advice that I got from Michael Kearns. We were sitting in a conference presentation by a senior statistician who was berating a room full of computer scientists about their lack of knowledge of the history of network science, which he claimed had all been invented by sociologists and statisticians. After about 20 minutes of this, Michael turned to me and said, "Come on, don't lecture us, don't tell us what to do. We're grown-ups. We're going to do whatever we want. If you've got a good idea, go and demonstrate. Lead by example. Don't hector people".

I thought that was a really good point, especially because I have a bit of that tendency myself, so I've tried to temper my urge to be critical by also trying to be constructive. Coming back to computational social science, I don't want to go to *IC²S²* and tell people they're doing the wrong thing, even if that's what I think. I would rather go and talk about what I'm doing that is cool and interesting — or better yet, have my students talk about it — and inspire people to come along rather than browbeating them.

Maybe that's the resolution to the paradox you brought up. These fields are big and diffuse, but if I stay in my lane and do a good enough job, hopefully I can pull some people along with me.

7 Craft

PB: One other dimension of your work — maybe a feature of your "lane" — is how you move between different research designs. You have mentioned that things are possible now that weren't possible before. But you have also made things possible: you have done highly innovative found-data analysis, and you have reinvented how we do experiments multiple times.

It also seems to me that the problems you study shift a little depending on the design. Public goods games come with a very long history and a very specific problem they solve. The small-worlds problem also had a history, but it operates on a very different level and probably requires much less institutional infrastructure to be interesting. How do you reflect on the interplay between the problems you study and the methodologies and research designs you use to study them?

DW: Let me answer that indirectly by first offering a very rough characterization of academic types, of which I think there are three.

First, are people who are very attached to a specific theory or theoretical framework. They have a theory-driven view of the world, and they take that view and try to push it as far as they can to address empirical questions. Second, are people who have a methodological toolbox. This is often true for statistical people, but it could be true for networks people as well. They

go around applying that toolbox to different substantive problems. And third, there are what we might call “area” people who devote themselves to specific substantive problems, such as early childhood development, or education, or immigration. They are less attached to specific theories and different methods, but they’re really focused on one problem they care deeply about.

I’m not really any of those. I’m curious about theories in general but am not attached to any of them in particular. There are lots of theories in the world.

PB: You mean theory as a set of ideas with a label, because you’re a systematic and abstract thinker. But you’re using theory here to mean a formalized set of ideas.

DW: Well, a formalized set of ideas that posits some mechanism that is supposed to explain some empirical puzzle. So, in that sense, if you take any nontrivial problem, there are lots of theories that are relevant to it. And although I find many of these theories interesting, I don’t have a dog in the fight.

But I’m also not a methodologist. I’ve never really had a toolkit. And to the extent that I use tools, they are all over the place and constantly evolving, and I’m not really an expert in any of them. So, I wouldn’t call myself a methods person either.

And finally, I’m also not really an area person. I said earlier that I’ve always been interested in the collective dynamics of human behavior, and that’s true. But it’s an interest that covers such a broad range of phenomena that it’s not really a topic or an area in the usual way. And below that very abstract, high-level interest, my substantive focus has shifted over the years almost as much as my theoretical and methodological toolkits. In some of my work, I study diffusion, and in other work I study teams or cooperation or deliberation. And in other work still, I study human mobility and media bias and even common sense. I find all of these problems interesting and I’m more invested in some of them than others — for example, right now media bias is something I find very concerning. But the topics that I’m working on now look very different from what I was working on 20 years ago.

What I think has stayed consistent throughout all this time is that I am committed to trying to make social science “work” better. To some extent, that means contributing to theory, and to some extent, it means developing new methods. And to some extent, it means weighing in on substantive problems. But the way I think about these problems is often as a proof of concept: a concrete case in which I can demonstrate the value of a new idea.

To take one of your examples, I have written a number of papers about public goods games (PGGs), but I don’t think of myself as someone who studies public goods games. In fact — and this is going to sound both controversial and hypocritical — I don’t think it’s a particularly interesting or worthwhile problem. To be clear, I think that the broader problem of human cooperation — and the conditions under which it does and doesn’t occur in the world — is a really important, profound problem. It’s just that I don’t think we’ve learned much about this big, important problem by studying simple models of public goods games or Prisoner’s Dilemma or the like. It seems to me that these models were useful abstractions fifty or sixty years ago when people first started trying to model cooperation formally, but that the field has way over-indexed on the models at the cost of trying to say something useful about what’s happening in the world.

So, the reason I write papers about simple games of cooperation is not that I am particularly motivated to understand how people play them. Rather, it’s because I need a vehicle to demonstrate some other point that I’m trying to make. For example, I wrote a paper on network structure and cooperation that used PGGs, which also introduced new methods for

running networked experiments online. Some years later, I wrote a paper about repeated Prisoner's Dilemma that also demonstrated what can happen when you run experiments for much longer than is typically possible in a lab. And recently I wrote a paper in which we used the PGG to demonstrate the utility of what we call "integrative experiment design" (Alsobay et al., 2026). In all these cases, I think we did learn something interesting about the substantive problem — for example, that network structure didn't matter when many people had assumed that it did — but that was never the sole goal or even the main goal of the project.

In all these cases, and many more, what I'm trying to do is to figure out a way of doing things that works better than the way we've been doing them up until now. And in order to translate it into something concrete, you have to pick a domain. If you speak in the abstract all the time, nobody understands what you're talking about. In that sense, the particular problems that I've chosen to work on are a bit arbitrary.

PB: Well, not entirely arbitrary. Public goods games have received so much attention that they're a good problem to improve on because people care about them.

DW: Yes, that's certainly one of the reasons why we chose to work with them: even if I'm not convinced by their utility, other people are. Interestingly, one of the reviewers for our most recent paper, who was clearly not a cooperation person, said, "I don't really understand why people study this problem". They liked the method, and correctly understood that we were using the PGG as a specific case in which to demonstrate a general method, but they were skeptical that public goods games were in themselves useful to study. It was a surprising comment from a referee, especially because all the others were very much invested in PGGs as an end in themselves, but I found it encouraging that someone else could feel that way and still appreciate the paper.

And honestly, I feel the same way about lots of the problems that we study in academia, very much including small-world networks. The fact that people are still studying the Watts-Strogatz model 30 years later is mind-blowing to me. Why? It's clearly wrong. It was meant to be wrong. It was — as we said quite explicitly in our first paper — "deliberately simplified". We were trying to make a very simple, abstract point, and the simpler the model, the more clearly we could make the point. But we never thought that this model was actually how the world works. In our view, it was clearly wrong in such important ways that there was no point studying endless minor variations of it, because small variations wouldn't get you any closer to reality.

After writing a few papers about it (Watts & Strogatz, 1998; Watts, 1999; Newman & Watts, 1999a & 1999b; Newman et al., 2000), I felt like that point had been made, and so I moved on to other things. I assumed that everyone else would do the same thing. And yet here we are almost 30 years later, and we have thousands, even tens of thousands of papers, that are studying the properties of this model. Obviously, I've benefitted from all that attention, and I'm grateful for that, but it's always struck me as odd how people tend to glom onto these simple models of the world, as if they are the world. So, to wrap up a very long and convoluted answer to your question: yes, I do tend to move around a lot from project to project in terms of theories, methods, and substantive problems, which means I've never fit neatly into any of the three academic archetypes that I mentioned. I don't know what I am.

PB: You're a person who does science.

DW: I would like to think so! But more specifically, I'm a person who wants science to live up to the promise that I grew up believing in, which is that science is the best way we know of

for building cumulative, generalizable and useful knowledge about the world. And right now, a big problem that I'm interested in, and one that I would like to spend the rest of my career trying to solve, is that, at least in social science, we are not building cumulative, generalizable, useful knowledge in as much as we think. That's what I want to do.

8 The Integrative Framework

PB: And what's your hunch?

DW: I think this integrative experiment design framework that I mentioned is the beginning (Alsobay et al., 2026). I feel like I have a good understanding of the problem, and I've been writing about it for at least a decade now. And the challenge has been, "What's the solution?"

Returning to my earlier point about the inadvisability of lecturing people, you can tell people they are doing things wrong forever, and they will never change unless you give them an alternative. That alternative must be as easy to use as the thing they are currently doing and somehow benefit them.

Think about p-values, going back to my very first example. It turns out that for as long as social scientists have been using p-values, other social scientists have been telling them not to. In the 1990s, Jacob Cohen, of Cohen's D, famously said "among many other things, it does not tell us what we want to know, and we so much want to know what we want to know that, out of desperation, we nevertheless believe that it does!" (Cohen, 1994, p. 997).

Every decade or so, somebody else comes along and writes another version of the same paper — a point that Cohen himself made, but has continued since then. But none of these critiques has made the slightest bit of difference to the practice of statistics in the social sciences. Nobody has an alternative. Someone says, "It's wrong, don't do it". But what are you supposed to do? You go back to work and keep doing whatever you are doing.

This is what I've been working on with Abdullah. We're trying to construct an alternative, which is what we demonstrated in our recent PGG paper, and now we're trying to make it easy to use. Hopefully, we can make it something that people want to use and feel like they're going to get rewarded for using. If you want to change how people do things, it's not enough to have a good theory. It's not enough to have a method. You've got to figure out the whole research process.

PB: What's that process? What is the integrative framework?

DW: It really flips the whole research design process on its head.

Roughly speaking, the way we do things right now is you start with a theory. From your theory, you articulate a hypothesis, and then you design a study to test your hypothesis. It could be an experiment, could be an observational study, could be ethnographic, whatever you want. Then you do your study, you get your data, and then you argue in favor of your hypothesis or not. In social science, that's typically through null hypothesis testing: you reject the null that there's no effect. Then you say, "This is consistent with my hypothesis", at which point you go back to your theory and generalize from it to whatever it is that you care about.

It sounds like the scientific method, right? It's worked for centuries. What is wrong with that? The problem is that mostly where it has worked is in physics, which turns out to be very different from social science.

In physics, there's often one force that dominates everything else at any scale, which then allows one to write down theories as formal models that make precise predictions. So, when

Einstein proposed his theory of general relativity, he was able to say, “If this theory is correct, then when you go and look at the stars behind the sun during an eclipse, the distant starlight should be deflected around it at exactly this angle”. And then Arthur Eddington takes his crew to the island of Príncipe in the Atlantic Ocean, just after World War I, and they observe the stars during the eclipse. Sure enough, it’s exactly that angle. Mind-blowing, right?

Unfortunately, nothing is like that in the social sciences. Pick any problem that you care about — team performance, cultural change, financial crises, you name it — and there’s not one thing going on. There are 50 or 100 things going on, all interacting with each other in complicated ways, and the way they interact depends on other aspects of the context. In this kind of world, the idea that you can advance theory one mechanism at a time is like focusing on just one string in a bundle and saying, “My theory is that this string exists”.

You can probably do it, but who cares? Because someone down the corridor is focused on a different string, and that also exists. In this way, we can write hundreds of these papers, each of which shows that the effect of some theoretically inspired mechanism is not zero. It looks like progress until somebody comes along and asks, “I have a problem: people in my firm/community/organization are not cooperating, and it’s hurting our collective well-being. What should I do to help them cooperate more?”.

Unfortunately, there’s no way to answer that question from the literature, because the literature hasn’t been trying to answer that question. What it can tell you is 100 different things that someone has at some point found to matter to some degree under some conditions. But if you say, “I don’t care about all that, I just want to know what to do. You told me about 100 levers I could pull. But which one, or which combination of them, should I pull?” we have no way of answering that question.

That’s why the integrative framework says: let’s flip this around. Instead of testing a theory, start by essentially enumerating all the levers you might pull. Call those the dimensions of the space. Every point in that space is now an experiment or a study you could run. You get some data and learn from it using whatever model you want — some fancy machine-learning model, AI, or a regression model. The details may or may not matter, but what does matter is that the model then makes predictions about all the points you haven’t looked at. You then go test some of those, evaluate your model on the new data, incorporate that data into your model, and keep turning the crank until you’re done.

And what counts as done could be a lot of different things. It could be that you have some problem you’re trying to solve, or it could be that you have some level of predictive accuracy you’re trying to reach. It could be that you convince yourself you can’t learn anything, that everything is so different from everything else that learning is impossible, in which case, we should pack up our bags and go home.

Regardless, at the very end, you say, “Okay, my model is good enough, or as good as it’s going to get”. Now you can interpret it, and try to reconcile it with theoretical ideas that you already know about, or potentially new theoretical ideas you didn’t know about before. The theory, in a sense, comes at the end.

That’s the integrative framework. The idea is that everything you do is integrated into this single, coherent, commensurable framework. Somebody else can come along and run a different experiment, or can add dimensions, or can take them away. But as long as you’re all speaking the same language, and you’re describing your research designs in that language, then everything can be reconciled with everything else, and we can accumulate knowledge.

That’s the goal, and we showed how to do this in a couple of examples, including the public goods games. That’s why we saw it as a proof of concept: it’s a very clear context in which to

demonstrate the approach. Ultimately, we want to apply the integrative framework to problems that aren't toy problems, but for the purpose of demonstrating the approach, we needed to start with something simple that is already familiar to many people.

Naturally, there are lots of complications, and we've already run into a bunch of them. But I think it has legs, so we're now trying to apply it in a bunch of different domains to make the argument that this is how we should do things. At the same time, we're also trying to build infrastructure that makes it easy to do. Now, of course, that process has been upended by language models and these new AI agents, so we're trying to figure out how to incorporate that.

PB: Which is a whole conversation on its own.

DW: Absolutely! But the big idea is still the integrative framework.

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