

AN INTERVIEW WITH DUNJA ŠEŠELJA, SAMULI REIJULA, AND
MATTEO MICHELINI
:: LORENZO CASINI

Abstract

This interview discusses the use of agent-based modelling in philosophy of science with leading scholars in the field, Dunja Šešelja, Samuli Reijula, and Matteo Michelini.

Keywords

Agent-based modelling; computational social science; philosophy of science.

How to Cite

Casini, L. An Interview with Dunja Šešelja, Samuli Reijula, and Matteo Michelini. *The Reasoner*, 19(2). <https://doi.org/10.54103/1757-0522/28587>

Dunja Šešelja is Professor for Social Epistemology and Reasoning in Science at the Institute for Philosophy II, Ruhr-Universität Bochum, and a member of the research group Reasoning, Rationality and Science. She is also the Initiator of the DFG Scientific Networks Grant on Simulations of Scientific Inquiry, a project she's going to describe in more detail during our interview.

LORENZO CASINI: Hi Dunja, thank you for accepting to be interviewed for *The Reasoner*. ABMs have become a powerful tool for

THE REASONER 19(2), April 2025.

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<http://doi.org/10.54103/1757-0522/28587>

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understanding complex systems. Can you tell us how you came to use them in your research? What questions were you asking at the time, and how did this tool help you answer them? Could you provide a concrete example of a philosophical problem that you found particularly well-suited for an ABM approach as opposed to a more traditional philosophical method?

DUNJA ŠEŠELJA: Thanks for having us, Lorenzo! To answer your question, let me start with a bit of background. My PhD at Ghent University was at the intersection of formal studies of scientific controversies—using formal theories of argumentation—and integrated history and philosophy of science. Around that time (early 2010s), ABMs were gaining traction in the social epistemology of science, particularly through the work of Kevin Zollman, Michael Weisberg and Ryan Muldoon, Igor Douven, and my colleague at Ghent University, Rogier De Langhe. These models introduced provocative and counterintuitive insights about the social dynamics of scientific inquiry. For instance, they suggested that too much information flow could sometimes hinder collective inquiry—what came to be known as the “Zollman effect” after Kevin’s work—or that “sticking to one’s guns” in scientific disagreements might, under certain conditions, be beneficial for the community of scientists.

What drew me to ABMs was their ability to illuminate complex, emergent social dynamics that traditional philosophical methods struggle to capture. While traditional methods allow us to ask, “What is a rational response to peer disagreement?” ABMs enable us to ask a different, yet equally important question, “If individual scientists respond to a disagreement in a certain way, how will that impact their collective inquiry?” This shift from individual rationality to complex social dynamics is crucial for understanding the

social dimension of science.

At the same time, I was skeptical of taking the results of highly idealized models at face value without scrutinizing their robustness. From the philosophy of modeling literature, it was clear that different types of explanations emerge from abstract and highly idealized models depending on their level of validation. Robustness checks—such as testing the stability of results under parameter variations or changes in structural assumptions—here play an important role. For example, if we increased the number of agents, or if agents in the model conduct their research in a slightly different way, would we still get the same result? If a result is highly sensitive to these factors, then we at least gain insight into its scope and limitations. In this way, we can not only generate surprising hypotheses but also zoom in on the specific conditions under which those hypotheses hold.

My initial engagement with ABMs was therefore motivated by a desire to critically assess the robustness of existing models and explore how their insights could be made relevant to the broader philosophical community, beyond the niche of formal social epistemology. As I was already interested in the argumentative dynamics underlying scientific inquiry during my PhD, I decided to take a further step and collaborate with others to develop an argumentative ABM of scientific inquiry, which served to test the robustness of previously proposed models.

LC: Transitioning from traditional philosophical methods to ABM often presents unique challenges. Could you describe the key hurdles you and other philosophers faced in adopting this approach, particularly in acquiring the necessary technical skills? And did you find it challenging to translate abstract philosophical

concepts, like “theory choice” or “scientific collaboration”, into concrete, operationalized models?

DS: Since I don’t have a background in programming, one of the key challenges in adopting ABMs was acquiring the necessary technical skills. Fortunately, I was surrounded by collaborators who did have this expertise, and I was able to learn a great deal from them. My three main collaborators back then—AnneMarie Borg, Daniel Frey, and Christian Straßer—all had programming experience, which made it much easier to get started. I also benefited from exercises on NetLogo, a programming language commonly used for ABMs, which were originally developed by Conor Mayo-Wilson while working at the Munich Center for Mathematical Philosophy (MCMP), LMU Munich, and shared with me by my colleagues during my postdoctoral time there.

That said, learning to code for ABMs is much more accessible today than it was when we first started. Newer programming languages, such as Julia, offer alternatives that are both powerful and relatively easy to pick up. Moreover, ABM is increasingly taught to philosophers, too. For instance, in our research group in Bochum, Matteo Michelini teaches a course on NetLogo, which has been a great resource for students and researchers interested in this approach.

Beyond the technical hurdles, another major challenge with ABM is not just translating philosophical concepts (such as theory choice or scientific collaboration) into formal models, but also identifying research questions that are well-suited to an ABM approach. When conceptualizing a model, it’s important to avoid two extremes—modeling a question that could be answered without simulation, and tackling a question so complex that an abstract

model cannot provide a clear or interesting answer. Typically, this means focusing on how specific activities of individual scientists impact collective inquiry, while abstracting away from numerous other factors that influence real-world science. This approach allows us to ask “what-if” questions that shed light on the social dynamics of scientific inquiry.

For example, traditional philosophical studies of theory choice have focused on the criteria under which individual scientists rationally decide which theories to pursue, or which theories to accept. However, this leaves open the question how different standards of theory choice impact collective inquiry. For instance, is a scientific community better off if its members favor pursuing theories that have a larger explanatory scope than the rivals, or should scientists instead remain committed to their current theories—even if their scope is more limited—until they suffer from too many anomalies in comparison to the rivals? An ABM allows us to explore how a scientific community that prefers one strategy over the other performs over time, offering insights into the consequences of different inquisitive norms on the epistemic performance of the group.

LC: Introducing a novel methodology like ABM can elicit varied reactions within a well-established discipline. How did the philosophical community initially react to this new approach? Was it difficult to navigate philosophers’ expectations and demonstrate the rigor and significance of your findings in a way that resonated with more traditional philosophical standards?

DS: As I mentioned earlier with reference to my personal skepticism in approaching the method during my PhD, if we want the

broader philosophical community to take the results of our models seriously, it is crucial to clarify their epistemic function. A common objection to highly idealized ABMs is that they omit many factors that play an important role in actual scientific inquiry. However, this critique often stems from a misunderstanding of what these models are meant to show. In most cases, ABMs are not designed to explain or predict real-world scientific inquiry directly. Instead, they may show how certain patterns or phenomena can emerge from a minimal set of conditions—some of which we might not have previously considered.

For instance, a model might show that a scientific community can become polarized even if all its members are individually rational, provided they start with different background assumptions. The question is not whether real-world polarization is necessarily caused by this specific mechanism but rather whether such a mechanism is capable of producing polarization. Of course, in some cases, ABMs can also be empirically embedded to investigate whether particular socio-epistemic factors played a role in historical episodes of scientific inquiry. But this requires additional work to connect the model to empirical data.

LC: The field of ABM in philosophy of science has seen significant growth in recent years. Can you trace a map of the different directions in which the ABM community has grown? What results have been achieved and what lies ahead in terms of challenges and unanswered questions?

DS: Yes, the ABM community in the philosophy of science has grown significantly, both in terms of modeling frameworks and research topics. A substantial body of work now exists on net-

work epistemology, the division of cognitive labor, the conditions that are conducive to theoretical diversity in science, the role of cognitive and social diversity in collective inquiry, factors driving scientific polarization, and so forth.

Looking ahead, several avenues remain underexplored. First, most ABMs developed by philosophers are still largely abstract and not empirically informed. While abstraction is not necessarily problematic, an open question remains: Can some of these models be empirically embedded to provide normative insights, for instance, guiding interventions in specific contexts of inquiry? Second, ABMs have been extensively developed in other disciplines, such as the social sciences and computer science, and modeling frameworks emerging in these other disciplines could be fruitful in addressing philosophical questions. Third, the recent surge in machine-learning research presents an exciting, yet underexplored, opportunity to integrate ABMs with machine-learning approaches, potentially offering new ways to study the dynamics of scientific inquiry. For instance, Gregor Betz has started using such approaches to introduce natural language into argumentative ABMs (see [here](#)).

LC: Collaborative networks play a crucial role in advancing academic research. You're the Initiator of the DFG Scientific Networks Grant on *Simulations of Scientific Inquiry*. Can you tell our readers what the purpose of the network is and what are its plans for the near future?

DS: Our network was funded by the German Research Foundation (Deutsche Forschungsgemeinschaft, DFG) in 2019, during my time at the MCMP. Originally, we planned to hold our first

workshop in April 2020, but with the outbreak of the COVID-19 pandemic, all in-person events had to be canceled. In response, we launched a series of online sessions featuring both network members and guest speakers presenting their work. This online series turned out to be a great success, with presentations not only from network members such as Patrick Grim, Christoph Merdes, Cailin O'Connor, Samuli Reijula, and Kevin Zollman, but also from guest speakers such as Igor Douven, Sina Fazelpour, Reiner Hegselmann, Paul Smaldino and Leonid Tiokhin.

After three years of virtual meetings, we were finally able to organize our first in-person event in February 2023. By that time, the number of junior scholars working on ABMs had grown significantly, and our workshops brought together a fantastic mix of PhD students and senior scholars from around the world. Over the next two years, we organized four major events featuring keynote speakers who are experts in ABMs (Gregor Betz, Ulrike Hahn, Toby Handfield, Rainer Hegselmann, Conor Mayo-Wilson, Cailin O'Connor, Erik Olsson, Samuli Reijula, Patricia Rich, Hannah Rubin, and Kevin Zollman), as well as philosophers working on the epistemology of modeling (Wybo Houkes and Paul Hoyningen-Huene) and scholars in formal philosophy of science using other formal approaches (including our interviewer, Lorenzo Casini, and Finnur Dellsén).

One of the most remarkable aspects of these events was the continued presence of PhD students, who became regular participants even though they were not officially part of the network. Additionally, inspired by our initial online series during the pandemic, we launched the *Computational Social Philosophy Seminars*, which continue to foster international interaction and collaboration.

Given the growth of the ABM community in recent years, we

are now preparing a new research network grant application to formally include the next generation of scholars. This new network will expand beyond ABMs of science to computational social epistemology, focusing on emerging trends such as argumentative ABMs, computational studies of epistemic democracy, and computational analyses of epistemic injustice.

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My second interviewee is Samuli Reijula. Samuli is affiliated to the University of Helsinki and a member of the Centre for Philosophy of Social Science (TINT). He is currently an Academy-of-Finland Research Fellow, leading a project titled “Modeling the Republic of Science: Collaborative Problem Solving and Collective Rationality in Scientific Inquiry”. He is also a member of the DFG network on Simulations of Scientific Inquiry led by Dunja.

LC: Thank you for joining us, Samuli! Your research focusses on the nature of scientific problem solving at different levels, from individual scientists to entire scientific communities. Could you please elaborate on what you mean by “well-functioning scientific research”?

SAMULI REIJULA: Thanks! I think the goal of the social epistemology of science has been nicely expressed by Philip Kitcher and by Steve Fuller. According to Kitcher (*The Advancement of Science*, Oxford University Press, 1993), “the general problem of social epistemology is to identify the properties of well-designed social systems” (p. 303). Fuller gives a bit more detail:

How should the pursuit of knowledge be organized, given

that under normal circumstances knowledge is pursued by many human beings, each working on a more or less well-defined body of knowledge and each equipped with roughly the same imperfect cognitive capacities, albeit with varying degrees of access to one another's activities? (*Social Epistemology*, Indiana University Press, 1988, p. 3)

Fuller's characterization highlights some of the key factors that feature in the models we often use: diversity in cognitive resources of boundedly rational agents, and the communication patterns between them. I think as philosophers of science, we're ultimately interested in the normative question of how the practices of science should be organized so that the production of knowledge is efficient, reliable and fair. How each of those values should be understood, is, of course, a topic on its own.

In my own modeling work, I often adopt the perspective of viewing science as a distributed problem-solving machine, "problem-solving writ large". From the distributed-cognition perspective, each researcher can be seen as a component of a larger (eco)system. I don't want to overlook the significance of the epistemic feats of individual scientists, but I think it's fair to say that most of the genuinely interesting and important problems we face as humans exceed the cognitive resources of any isolated individual. It is only together, as research groups, collectives, research fields, and finally, as "the" scientific community, that we can hope to address those challenges.

I think that for understanding such distributed systems, ABMs can be a very useful tool. Like Dunja suggested, even when we work with simple assumptions about how individual agents behave and combine those with further, simple assumptions about the institutions of resource allocation, communication, division of cogni-

tive labor in the scientific community, we can sometimes come up with interesting insights about how the problem-solving machine of science works, and how it could work.

LC: Scientific collaboration is increasingly recognized as a key factor in successful research. What led you to focus on collaborative problem solving? Could you explain the kind of computational models you are using, and what they help illuminate?

SR: Ever since I encountered Herbert Simon's theory of *problem solving as search* in an undergraduate cognitive science course, I've been very inspired by the idea of scientific discovery as heuristic search.

Problem solving—and the creative discovery it often requires—is in some important ways different from “mere” decision making. Unlike in traditional models of decision making, in problem solving the set of potential solutions is typically not known beforehand, the candidate solutions must be discovered. For example, think about the problem of finding a chemical substance that would work as an antibiotic against a superbug. Since the abstract space of possible chemical compounds is large, how is one supposed to search in there?

By the way, the idea of representing a problem structure metaphorically as a high-dimensional space has recently become concrete reality in automated labs doing drug discovery and materials science. Such “self-driving labs”, as they are called, explore spaces of possible chemical compounds or new materials by combining optimizing algorithms with automatically run series of material experiments. Our traditional models of scientific experimentation suggest that we test hypotheses one at a time, but these systems

are massively parallelized and can quickly sift through thousands of hypotheses. It's wild.

There are a couple of reasons why I think heuristic-search models are particularly fitting for studying scientific problem solving. As I have argued together with my friend and colleague Jaakko Kuorikoski (see [here](#)), in order to study collaborative problem solving in science, the modeling approach must be capable of representing some key features of the process, namely (1) problem structure and problem decomposition, (2) diversity of cognitive resources, and (3) cognitive coordination between problem-solving agents.

I think that the models in biology and organization science, inspired by Simon's early work (see, e.g., work by Stuart Kauffman, Jim March, and Daniel Levinthal), provide a good starting point for developing models of scientific problem solving, as shown by recent work in philosophy of science (see [here](#), [here](#), and [here](#)).

LC: You've explored the trade-off between "transient" diversity and diffusion of ideas in group problem solving. Could you explain what this trade-off is, and what your simulations suggest about how to balance these factors for better problem solving?

SR: Cognitive diversity has been shown to trade-off against a couple of things. The first is the diffusion of ideas in a population of agents as examined in detail in network epistemology models, first introduced in Kevin Zollman's seminal "The Communication Structure of Epistemic Communities, Philosophy of Science" (*Philosophy of Science*, 74(5), 2007) and "The Epistemic Benefit of Transient Diversity" (*Erkenntnis*, 72, 2010) papers. There, Kevin derives the *Zollman effect* that Dunja mentioned before. It's

this counter-intuitive finding that theory choice in a community of scientists may be more reliable when the agents communicate less with each other.

The second sort of trade-off, the one that I've examined the most in my research, concerns the tension between group diversity and individual expertise. According to a famous finding by Lu Hong and Scott Page ("Groups of diverse problem solvers can outperform groups of high-ability problem solvers", *PNAS*, 101(46), 2004), called *diversity-trumps-ability theorem*, a group of diverse problem solvers can outperform a group consisting of individually best performing problem solvers. Although I've shown (see again [here](#)) that there are technical problems with the original finding, I think the basic idea is solid and important to the social epistemology of science: Looking at scientific progress we're often too focused on the role of exceptional individuals or geniuses. A more useful perspective on scientific progress is to look at the cognitive resources—research questions, solutions to problems, methodologies—distributed between the different members of the scientific communities.

It seems that discoveries and breakthroughs happen when those ingredients—you can imagine them floating around in the intellectual space of the scientific community—are brought together in the problem-solving activities of a diverse research group. Diversity can beat ability because high-ability experts often tend to resemble each other, and consequently, a group of high-performing experts ends up having access to fewer cognitive resources.

LC: Given your focus on institutional design, how do you envision your research informing practical strategies for improving scientific collaboration and problem solving in real-world set-

tings? What are its possible implications for funding agencies, research institutions, and individual scientists?

SR: Almost every extended discussion on ABMs eventually turns to this question! And it's a really important question. I tend to fall on the side of caution here: We should not oversell the policy-relevance of the findings coming from theoretical models. My caution comes from the work I've done on evidence-based policy. From the policy world, we know how difficult it is to apply theoretical insights to real-life situations—this is the well-known problem of extrapolation.

That said, I think the policy world might have swung too far in the direction of inductive skepticism. The core idea of evidence-based policy has been to replace theorizing with experimentation at target site, so as to avoid the risky inductive generalizations between different populations. But I don't think we can do without theory.

Research policy is an example of an area where models and data could work together: Modeling can help us uncover mechanisms, or at least abstract motifs, that can be detected across several empirical situations (e.g. cumulative advantage, belief polarization, transient diversity). Especially in complex situations where many of such mechanisms interact and trade-off against one another, our intuitions become unreliable, and therefore, we need models to understand the dynamics.

So, to answer your question, I don't think these models alone have policy implications, but I do think that modeling should often be a part of the process of research-based policymaking. ABMs, in particular, are a powerful way of bringing together theoretical ideas, assumptions, and data, and exploring what follows from

their conjunction.

I think that in the fields of *meta-science* and of *science of science* there are interesting examples of how to combine theory with empirical data for thinking about research policy, but I also think both of these research fields would benefit from more engagement with the philosophy of science.

LC: Looking ahead, what are the most pressing unanswered questions in the study of scientific collaboration and problem solving, and how do you see your research contributing to future advancements in this field?

SR: For a long time I was an AI skeptic, and even after the November 2022 introduction of LLM chatbots, I had decided not to jump on the AI-philosophy bandwagon. But somehow I started reading up on DeepMind's AlphaFold, namely the AI system that practically solved the protein folding problem, and self-driving labs.

Advocates of these approaches claim that automated discovery methods will revolutionize the scientific method in the coming years. That's of course a marketing pitch, but I think it is beyond doubt that now we do have existence proofs of automated scientific discovery. AlphaFold's protein structure library has influenced the field of biomedical research in a significant way. I've now been applying for funding for a project to study scientific problem solving and discovery in AI-enhanced labs.

Looping back to Herbert Simon whom I mentioned above, I think that concrete examples of automated discovery systems like AlphaFold could be what he always dreamed of: Simon thought sci-

entific discovery should be understood as continuous with everyday problem solving, and that it could be explained in terms of the models we have in cognitive science. Automated discovery systems could be a new source of evidence for that kind of theory: despite being huge in terms of data and computational capacity, their fundamental architecture is understandable and fully transparent as program code.

So maybe the emergence of AI discovery systems could stage a comeback for the 1980s agenda of the “friends of discovery”, who aimed to make of the study of scientific discovery an integral part of the epistemology of science.

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My third interviewee is [Matteo Micheleni](#), a cotutelle PhD student at Ruhr-Universität Bochum and TU Eindhoven, under the supervision of Dunja Šešelja and Wybo Houkes.

LC: Hi Matteo. After completing a Masters in Logic, during your PhD you turned to applying ABM to philosophical problems. What drew you to this approach? What do you think it uniquely contributes to social epistemology and philosophy of science?

MATTEO MICHELINI: What drew me to ABMs? The fact that I *hated* logic. Of course, I’m joking! Still, on reflection my shift towards ABM was indeed grounded in three differences between simulation tools and analytic/logical tools.

First, I’ve always been attracted more to theories that explain and assess social phenomena, as opposed to the study of languages, or knowledge per se. It is something I developed when I was do-

ing my Bachelor in Philosophy studying philosophers like Locke, Rousseau, or Marx.

Second, during my Masters, I did not just study logic but also game theory and computational social choice, which are used in computational simulations. These frameworks fascinated me. Given my background— I started out as a Mechanical Engineering student, I confess—I saw them as an exciting way to apply my math skills—and make my first years of study fit better in my life narrative—good choice after all, wasn't it?

Finally, I also liked the idea of *improving* society—whether scientific communities or broader groups navigating epistemic challenges. My family upbringing instilled in me a strong sense of social responsibility, so this naturally resonated with me.

At some point, I just thought, *wow, I can use the formal tools I love to answer questions I care about—and even tell myself they have some societal relevance.* That said, I quickly realized that achieving this last goal wouldn't be so straightforward—but I suppose that's a topic for another question.

As for the unique contribution of ABMs, the answer is complex, as Dunja already made quite clear. In fact, their epistemic role is subject of an ongoing discussion within the community. Personally, I like to think of them as thought experiments made precise, computational tools that allow us to rigorously derive the consequences of our assumptions (especially if these assumptions are simple—something which both Dunja and Samuli referred to). As such, they are the perfect tools to study how social dynamics may bring about certain phenomena, or test explanations for collective behaviour. In short, ABMs enable us to reason about collective phenomena in a precise and grounded way—without blurred lines or half-sketched consequences.

LC: Your past research explores how epistemic practices detrimental at the individual level can negatively affect the scientific community. Currently, you're investigating how these very same practices may also—surprisingly—turn out beneficial for the community as a whole. Could you share paradigmatic examples of this sort of phenomena?

MM: Sure, there are plenty of epistemic practices that are detrimental to individual inquiry but can have mixed effects at the collective level—beneficial under certain conditions and detrimental under others. In my work, I've primarily focused on two such cases, namely the effects of evidence misinterpretation in scientific inquiry and the role of individually vicious cognitive dispositions. For both of these, I first published papers outlining their potential downsides (see [here](#) and [here](#)), and I am now working on follow-up research exploring their positive contributions. As this is the most surprising aspect of my research, let me briefly explain how each of these mechanisms might actually enhance inquiry.

Evidence misinterpretation happens when someone draws the wrong conclusion from scientific evidence. Naturally, misinterpreting evidence is usually seen as something scientists should avoid at all costs. However, using ABMs, we've shown that—under very specific conditions—a moderate likelihood of misinterpretation can actually benefit the scientific community. The reason is that when scientists have a small probability of misinterpreting evidence, they are more likely to form diverse beliefs about which method is superior. This diversity leads to a more comprehensive exploration of available methods, ultimately increasing the chances of identifying the best one. This phenomenon is known as transient diversity, as Samuli has already explained.

A second example involves “myside bias” and laziness, respectively the tendencies to only produce arguments in favour of one’s opinion and to never critically evaluate such arguments before sharing them. My current research suggests that a group of biased and lazy reasoners might actually be more likely to reach the correct solution to a problem than a group of diligent and impartial ones, whenever cognitive and material resources are limited. The reason is that biased and lazy reasoners can sometimes better and faster leverage the collective competence of the group. But I’ll stop here—more details will be in my next paper, and I don’t want to spoil too much!

What’s important, though, is all these results hold under very specific conditions. Misinterpretation is beneficial only if each scientist is only very moderately inclined to misinterpret evidence; as soon as someone is highly likely to misinterpret evidence the scientific community will never reach consensus on the superior method. Myside bias and laziness are helpful only if reasoners start out with more or less balanced perspectives, and material and cognitive resources are limited. ABMs are ideal to highlight these conditions: they allow us to derive them from the outcomes coming from the interactions between individual agents.

LC: What are some challenges you’ve faced as a PhD student working at the intersection of philosophy and computational modelling? Any advice for others considering a similar path?

MM: Well, I’d say there are two main challenges in this career—one more fundamental, the other more contingent.

The first is a persistent feeling of not fully belonging anywhere. While most philosophers openly appreciate our work, I always

get the impression that some don't see us as really doing philosophy. The same happens when interacting with social scientists: we seem to be doing too much theoretical modeling to be fully accepted as social scientists. Compare this to someone working in other, more traditional areas of philosophy—other philosophers might disagree with their views or even their methods, but no one questions whether they are doing philosophy. In my case, I often feel like I have to prove to philosophers that I belong. In a way, as a PhD student, I live with double imposter syndrome! s a result, I often find myself striving to prove the relevance of my results to social scientists and its philosophical bearing to philosophers.

The second challenge is that, since this is a relatively new approach, there's no clear roadmap for what you need to learn to become a good scholar in the field. Until now, I feel I've learned many different tools and yet I've only successfully applied a few. Did I learn them for nothing? To-be-determined. At the same time, I constantly feel like there are tools I haven't mastered even though I should— not because I didn't apply myself enough but simply because nobody ever said (or thought) I was supposed to learn them in the first place!

I imagine that someone working in, say, epistemic logic has a much more structured path. They learn the foundational theorems, the standard approaches, and the key techniques for carrying out proofs. In contrast, for those of us at the intersection of philosophy and computational modeling, the path is far less defined.

So, my advice? Try to connect with as many people as possible. Building connections helps you gain a sense of identity and a better sense of what's worth learning. In fact, I'd say the DFG network I am part of, *Simulations of Scientific Enquiry*, was incredibly important in both respects—and I'll never thank Dunja

enough for bringing me in.

LC: Right. Being part of the DFG network must have given you exposure to a wide range of perspectives. How has this collaboration influenced your work? Are there any insights or directions you wouldn't have explored otherwise?

MM: There's no doubt that the DFG network has had a major influence on my research. Conversations with network members shaped my understanding of what methods are useful and how I should approach my work.

For example, during my Masters in Logic, I didn't work extensively with Bayesian methods and had no plans to use them in my PhD. Then, at my first DFG meeting, I realized that almost everyone was working with Bayesian epistemology. So I thought, *hmm, maybe there's something to this*. From that point on, Bayesian tools have become a central part of my work in some way or other. The same goes for many of the methods, approaches, and ideas I've used—most were heavily shaped by my interactions with the network members.

Another thing that made the network so valuable was how close-knit it was, bringing together both senior and junior members. This gave me the chance to observe how senior people handle things and to form my work habits in the image of great role models. Honestly, if it weren't for the DFG network, I don't think I would have been able to produce—I am not ashamed to say—such interesting results, or to learn so much about how academia works.

LC: Looking forward, if you could see one of your research findings tested in a real-world setting, which one would it be and why?

MM: That's a billion-dollar question. In principle, testing any of the findings I mentioned earlier could bring valuable insights. While our computational models produce fascinating and promising results, they primarily serve to generate hypotheses and propose possible explanations. They don't allow us to make concrete predictions or offer definitive explanations of real-world phenomena. So yes, empirically testing our results—and calibrating our models accordingly—is absolutely crucial.

If I had to pick one hypothesis to be tested in a real-world setting, I'd go with my work on bias and laziness—the idea that these cognitive tendencies might actually benefit reasoners engaged in deliberation, particularly in situations where evidence and time are limited. I see this as a strong candidate for future empirical testing for a few reasons.

First, the hypothesis is closely tied to cognitive science, a field with well-established empirical methods for testing claims about reasoning and group epistemic performance. In fact, similar claims about the benefits of certain practices in collective reasoning have already been explored within cognitive science.

Second, if validated, this research could have a meaningful societal impact. Right now, we teach students to suppress their biases—to actively counter their natural tendency to favor their own beliefs. We even design strategies to help with this: for example, encouraging students to “consider the opposite” to push back against their own bias and mental inertia. But what if, in certain contexts, bias and laziness actually improve group reasoning? If future studies support this, we might reconsider how we teach crit-

ical thinking. We could instead tell students that, in safe deliberative environments—where bias isn’t likely to cause harm—there’s no need to fight these tendencies. Instead, they can lean into them.

On the other hand, testing our models on scientific communities is much more challenging and would require significantly more groundwork. One major hurdle is the lack of a clear, universally accepted measure of epistemic success for real scientific communities. Without such a metric, it’s difficult to assess the impact of different mechanisms on scientific inquiry. That said, I don’t think this is an insurmountable problem. Developing a measurable notion of scientific success—one based on publication data and expert assessments—is something we should strive for. As it happens, I’m currently working on this with [Eugenio Petrovich](#), a philosopher of science and scientometrician. Together, we hope to advance the current understanding of the (elusive) notion of scientific success, and consequently to deepen our understanding of “good” mechanisms of scientific inquiry.

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