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Round Table

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Round Table

John Durham Peters: My question is that if the interaction of object and apparatus is at the heart of quantum physics, why is it that popular conceptions of what science is are still so connected to just grabbing on to reality as such with sense data? Why is that so hard to shake? That's my question.

Frieder Nake: I don't have a question. Is it possible for a regular human being to understand quantum mechanics?

Thomas Bjørnsten: I'm not sure that I have a regular question, but I can say, which some of you already know by now, that one of the reasons that I'm here is that I'm interested in the topic, because I'm doing research on data visualization. So, for me, a really interesting question concerning what has been discussed here: apart from all the aspects of quantum theory and physics that I don't understand, the interrelation of what we conceive of as visualization as opposed to or parallel with simulation is a puzzling question for me that I'm dealing with. So it's just to sort of mention that there's a discussion around this topic. The differences, the similarities, between the negotiation of visualization as opposed to simulation. This is what I'm bringing with me from here, which we may discuss further on at some point.

Lukas Mairhofer: I'm still hooked up a little bit on the discussion from before. So the question that's running around in my head at the moment is where does the uncertainty come from? Does it only come from our imperfect knowledge, or does it also come from the other side of the cognitive process? Does it come from nature itself? And then, of course, is quantum uncertainty really the same as macroscopic uncertainty? Because after all, there are very different statistics that describe these uncertainties. So where does the uncertainty come from?

Mira Maiwöger: Right now I'm mainly wondering about what I'm dealing with in my daily life, like my Bose–Einstein condensates in this lab together with this machine. What is this? Because after all, they kick back sometimes, so this phrase by Karen Barad, the world kicks back, which I was thinking about before in the discussion about the ontological aspects of us doing science. For me, this is sometimes mainly the ontology, I guess. Sometimes things just don't work out, the lab does not work or the experiment does not work. Or it works differently. There is this element of surprise.

Stefan Zieme: Okay, maybe I can give an answer now. I think quantum mechanics doesn't make any sense beyond the mathematical framework, and that's fine with me. I never dare to think about it at all. What do I care? It works out perfectly. It's the same with quantum field theory, and, if you continue that line, it goes into string theory, and there you are at a point where you have a purely mathematical framework. You could call it mathematical physics. What drives me is, how do you chart new grounds in mathematical physics where you don't have anything? You don't have any phenomena, you don't have any settings, you don't have any data, and you don't have axioms. It's not math. It's something very strange; I don't know what it is. There's progress, and I don't know why there's progress, but I like it.

Eric Winsberg: I think I probably said enough.

Wolfgang Hagen: I don't know. I'm a little bit struck because of this day, and I learned a lot. I'm looking for this question of ethics to answer somehow, because I found out that it is very important to talk about ethics in the realm of physical knowledge, so to say. And I would recommend to hold, very soon, a workshop about that.

Jeremias Herberg: I joined in late. It was great to hear you guys speak. One thing I can just share to maybe, in some way, contribute: I just wrote a chapter in my dissertation on how the notion of fields traveled to sociology, and how it became a "Scheinbild," maybe. And actually occupying an entire area of sociology with only one Scheinbild, Pierre Bourdieu, and how you could maybe alternate that with another Scheinbild. So that's only to add to this discussion that we just had on, in this case, social ontology maybe, and the way we look at it and the interaction between those things. That happens in the social sciences in a similar way without, I think, simulation maybe. I mean, sociologists today hardly speak about that. That's it.

Frank Pasemann: Well, I think I don't want to comment on quantum mechanics because I spent too much of my lifetime on that. But I would like to comment on something I still believe, and perhaps others also, that computer simulations can produce new knowledge. I showed you this Fermi–Pasta–Ulam problem as one of the first examples. But today, I think it's very clear why they call it exploratory simulations: it's that you really find new things. There are a lot of examples. Perhaps I can tell about my own way using this approach. As a physicist, looking into brain sciences, I saw all these very interesting fantasies about how brains work. But coming from outside what you learn first is that brains are oscillating and there is a highly recurrent connectivity between neurons. Everyone knows it. But the neural network theory was about input/output maps. And, of course, if you look at the recurrences and knew a little bit from cybernetics, you know that more interesting things must happen there, just because of the loop structure. One then may hypothesize that the capacity of biological brains relies exactly on these properties. Of course you cannot verify that on anesthetized cats or something like that. So I said forget it—I don't need this kind of knowledge. So what is the point? There is a hypothesis: Capabilities of brains rely on looping signals. And, of course, then you go to simulation and look for systems that have these loops. You then realize that there are too many parameters and processes to study, that there is no theory, and that mathematics can tell you almost nothing about the observed phenomena.

You only know there are very many interesting phenomena to expect, but you don't know what will be of relevance for brain science. That's why I came to robotics, assuming that to have a brain only makes sense if you have to control a body. Very many discussions were about results obtained from studies on dead brains, not reflecting the functions of living brains. So one may learn lots of things from exploratory simulations—for instance, that very different sizes of systems can all generate one and the same function. And then you may come up with new questions concerning stability of brain processes, plasticity of brain structure and all that. To have a test body for your simulated processes you may then use physical or simulated robots in real or simulated environments to control the boundary conditions of your simulation. Then I know what I'm doing, then I'm not fantasizing. So if you implement your results from this type of simulations to something which is real world like a robot, I think you can get quite a lot of new insights into how brain may work.

EW: Just very quickly, on the point about computer simulations discovering new things, there's, I think, supposedly announced a discovery yesterday of a new ninth planet. We got rid of Pluto—it's no longer a planet. They think maybe they found a new ninth planet using computer simulation. So, it seems pretty clear that computer simulation is...

SZ: Well, I guess we should pass it on, but I have strong objections to that. Just to put it on the list that you say there's a computer simulation on something. I mean, in 1846 Le Verrier did the very same thing. So what's not a simulation? You solve Newton's equations with some perturbations, and the solution doesn't work out, so he had a world view, he had a paradigm. He made a choice, he followed the path, something was left over, he experimented—with a mathematical tool, of course—but, to my belief, this is not a simulation. If you put it on a computer, you can do it on pen and paper when you're good, or you can put it on a computer. That's your choice. But I wouldn't call that a simulation.

Kristel Michielsen: Okay, so I have a question, and maybe it is more to the historians of science. My question is, I have the impression that, at this moment, the atmosphere, at least in quantum physics, is such that experimenters are trying to measure what theory predicted or has given, and unknown facts are simply thrown away because they don't have an explanation for them. And, as a consequence of this, by doing computer simulations, by playing around with computer simulations, we could maybe find new things. But the atmosphere is such that they're then considered to be strange, even not willing to test because they do not fit into the picture. So, my question is, why is this? Was this also the case maybe when they started to think about quantum theory as a new theory?

Hans De Raedt: Did you see similar things in biology?

KM: Yeah, because you have a very nice example that you suddenly find something new. But you proceed and do experiments in computer simulations to just verify some experiments or try to prove them in a more material type of sense.

Hans-Jörg Rheinberger: I think the basic question for the history of science—if you take the history of science in the broad sense and include not only physics but the other areas of working "scientifically" as well—then, of course, you realize that the kind of paradigms that we observe in the development of physics, and in particular during the twentieth century, are not at all characteristic of most of the other areas where science is being practiced. The conundrum, or the difficulty, is that philosophers of science have always been taking physics as the paradigm of what it means to do science. This brings with it a distortion, I would say, that prohibits us from seeing the entire complexity of scientific activity, be it if you go into the depth of history or be it if you spread out in all these different areas where scientific activities are going on. And I think we need a way and also a place to kind of bring this complexity to the table, and also take it seriously.

Angela Gencarelli: I'm a literary scholar and I only have a vague idea about quantum physics, but right now I'm working on a project about the narrativity of physical texts. So, my question would point to the direction of the history of science: How are we able to construct and reconstruct scientific processes through texts?

HJR: So this is actually the question we just had on the table, asked on another level. Can you really learn about the dynamics of science by stepping from one published paper to the other? I don't think that it works—it gives you an distorted vision of how science is proceeding. So we need to take into account not only the text but also the context to play with this kind of distinction, I would say. And, in the way of how we are able, or could, or should, construct our narratives about science, historically, there is no royal path to go. I think if you try to recount a shorter episode in the history of experimental science, to describe the trajectory of an experimental system is a possible narrative. But, if you, say, follow the history of physics over 150 years, you will probably not be able to choose such an entity as the point that can give you a consistent narrative. So you have to think about narrativity if you do history of science.

WH: Just one other short hint: maybe you should read one of the most important texts in the history of philosophy. And this is the "Crisis Text" by Husserl from 1935 [*The Crisis of European Sciences and Transcendental Phenomenology*]. It's an attempt to think about the possibility of a history of science, actually, and how Husserl worked it out. From the method, not in the philosophical arguments itself, it's very interesting, and will take you far beyond the concept of a text.

Janina Wellmann: Working on the history of biology and on processes, i.e. processes in general and modern attempts to create a digital embryo in particular, I am interested in the question how one transfers the reductive methods of mathematics onto living organisms. What happens to the liveliness of the organism when you work with a computer? What is life when it gets into the computer? And can you use the same methods for particles and for the living? We study a dead brain and even when we study organisms in vivo we do not know exactly to what extent they are still alive and how to detect or quantify or define the liveness of what we experiment upon.

HDR: So maybe I'll try to paraphrase John von Neumann. He said at some point, "If you tell me what intelligence is, I will put it on the computer." I think it's the same here. So, if you would give us precise criteria of what you think life is, then that means you'll specify certain rules by which we can identify what it means to be alive. And then you can put them into the computer as rules.

JW: I want to give you an example. Usually when you have these experiments on embryos, they are kept alive. It's a very difficult process, but it's never described what exactly it means that they are alive. What are the criteria? I just had a very interesting discussion with an embryologist who is working on apoptosis, programmed cell death, who said, "When you actually study these embryos that are still alive, 99% of the cells are dead. They are in a state of apoptosis." So the question arises: What is the living part of the organism? Usually, this is the part that is never described in scientific publications for these experiments, for example. It's just taken to be something obvious, taken for granted, but it seems to be much more difficult.

Arianna Borrelli: I would like to say something that connects both to the last presentation and to a question at the core of this conference, which is the atmosphere that apparently reigns at present in quantum physics, and which makes discussion of non-mainstream approaches like that of Hans and Kristel very difficult.

Certainly the fact that the development of physics is in some sense extraordinary and is taken as a paradigm of how science works plays a role. The formalism of quantum mechanics and its standard Copenhagen interpretation are a successful paradigm not to be challenged. In the history of physics and other exact sciences one can find different examples of this kind of situation. Yet what historians often see, and what we have seen very well in the last presentation, is the importance of plurality. We just had a very beautiful example: an experiment was made with great investments, resources, conviction—and it was a dead branch, a blind alley. On the other hand, there was a simpler experiment that worked. Now, you could ask: What did some do "right" and the others "wrong?" But, actually, the important message in the example is the role of plurality: in another discipline, outside of the paradigm, different experiments were being made. And in that specific case it might just have been chance that there was plurality, but the fact remains that if you don't have plurality then you may remain with only a blind alley.

And now we come to the question: Why is plurality not there in some cases? Why is there resistance against it? Of course it is also a question of science politics, of competition for resources, but in my opinion in physics and the exact sciences there is an additional factor: mathematics. Mathematics, and more precisely symbolic mathematical formalism, is a very, very powerful medium, if I may use this term. I speak here of medium because I think in the case of mathematical formalism one may have an effect similar to what McLuhan calls "narcosis": where the medium becomes transparent, so that what it mediates appears as an immediate reality.

In this way, mathematics rules, in a sense. Through the Schrödinger equation the formula of a wave function becomes an entity. And the possibility of "mathematizing" more and more phenomena brings the idea that we have to have unification of all natural laws in a formula. I'm not saying that this vision is necessarily wrong—in the question of ontology, I don't think one can say that it's right or wrong to think of wave functions as entities or not, but it's good if there are different people thinking about different entities, or not thinking about entities at all. And, in the end, you can get more tension and therefore more progress.

But, as I said, this specific element, mathematics, which is so apparently universal, can be an obstacle to plurality. And this despite the fact that if you look at the specific cases, the illusion of universality disappears. If you look into how precisely the Schrödinger equation is formulated and employed in various areas of quantum mechanics, diversity of formalisms and techniques rules. My presentation was an attempt to contribute to raise awareness of this question.

Britta Schinzel: What I have to say was already said. I'm thrown between the observation that computation and simulation can bring you insights, for example, those observed in computational neurology, which is able to implement a kind of extrapolated *Gedankenexperiment*, which couldn't be done in reality, and on the other hand, that method might produce a lot of *Scheinbilder*, which are wrong, because it more and more dissociates itself from reality. Also the observation that nearly all science is now doubled with the prefix computational, most profoundly by physicists, with always the same method—differential equations—and whether that's not a narrowing of mathematics, MATLAB and so on. All these methods are used now without rethinking whether it's adequate and whether we also wouldn't need a broader approach.

Martin Warnke: Yes, it's a bit about what Britta just said. What I noticed in my being a student of physics, the main slogan was "Shut up and calculate," because our professors did not actually want to talk about the justification of anything they told us. It was just there and it worked, so do it and don't ruin your career in doing, say, for instance, history of science. And after these two days, I have the impression that we are almost at the point of saying, "Shut up and simulate," which is equally non-satisfactory. So, what I really wanted to know is whether there is more, say, methodological evidence and method, in a way, to come close to the certainty that we had from mathematics in the field of simulation, not to then tell all our students they should shut up, but that we all knew better what it's about with simulation, as compared with math.

Anne Dippel: First of all, I have to say, at the beginning, I hoped we would create an imaginary arena and we would discuss. I have to say, we all here behaved like rubidium—well-behaved—we've all discussed and I'm very happy that we created a little thought collective. Because, at the beginning, I fantasized you could really scream at each other maybe, and have very, very different positions. I've observed screaming physicists in laboratories already several times. So I expected the worst, and I'm very, very pleased to see how respectfully you can reason and argue. So, on the one hand, there is Kristel who says, “Why don’t we use simulations to really explore new sides of physics, to tinker in a way with the computer?” And then there’s, on the other hand, Mira who says, “Reality kicks back, and the experiment shows something new.” And then there is Stefan who says, “Mathematical arguing produces completely new knowledge,” and all those three spheres, somehow, are puzzling spheres for us as humans to reason about nature. I would like to know if, for example, you could imagine that the work that is done by Kristel and Hans could be of any use, if there’s any need for it, or if it could help that there is some interaction between the works at the fringes? Because, currently, you all work in your little bubbles and there is no real communication, and this is one of the few spots, here, where there is communication. But maybe it is not at all possible, or even necessary.

LM: So, Martin said when I did my presentation that we are measuring things that the theorists cannot calculate. So maybe the simulators can simulate, or can, in their simulations, find effects that we then could try to find in our experiments. I’m sure that this could be productive. But, Martin also has pointed out that we are working together with a group of theorists who try to at least model what is going on in our interferometer. But I think if you’re able to simulate what is going on in our interferometers and find effects that we didn’t expect, or that you wouldn’t expect from our theory, I, for sure, would be happy to try to reproduce them in the experiment. Why not?

KM: So, I ask the question to you, because you asked us as theoreticians to make a theory or theoretical description of what is going on in the experiment. And, for you, it would also be wonderful if we as theoreticians find some new effect based on the theory, and then you are going to search for it. By itself, that’s nice. But, what we like from experimenters is that you give us data that do not comply with the theory and do not throw it away or say, “Yeah, but this is because it’s in the transient region and not yet in the stationary state,” and so on. Because we love to analyze these things and then simulate.

LM: I think for this, I maybe have to pass the microphone to Mira, because, in our experiment, there were very few free parameters. And, I mean, if we don’t see interference, it’s usually because the machine is completely misaligned and not because something strange is going on.

KM: Maybe. Maybe.

LM: But I think that we are open... So, if I see interference that looks different from what I would expect, we, for sure, do not throw this away.

MM: So, my data are not yet collected. I am still figuring out how to debug my machine. But my colleague has stated that she does not understand them at all, and she's currently on the search for theoreticians who can try to explain them to her.

KM: But she did the experiment for somebody, so she had some expectations?

MM: Yes, yes, but her results are not matching the predictions at all. There is a collapse to a steady state that was not expected. So, no matter what she does to the atoms in the double well, it's always going to the same fixed point.

KM: But, by itself it's an interesting result.

MM: Yes, of course, but there is no explanation for it yet.

KM: But now, the next question: Can she publish it?

MM: She's working on the analysis—really trying to fit her data and really trust her data. She even phrases it this way, "I want to trust my data first." But yes, this is in development.

KM: Then the next question: If she can publish it, can she publish in *Nature* or *Science*? Because if you find an interference pattern, you can publish in *Nature*.

MM: I think she's afraid that she will not be able to publish it as it is now.

KM: I can imagine, yes.

MM: She needs an explanation or theoretical framework for it.

KM: And that was the atmosphere I was talking about. So if you find these interesting things in the meantime, you are not allowed to speak about it.

EW: I sort of want to respond to that. You have to be careful what you wish for. Experimental data that don't match our expectations are easy to find. We had people saying they had neutrinos going faster than light: "Oh, it was a loose cable." We had people saying they had cold fusion: "Oh, no, it was a bad detector." We had people saying they found gravity B waves: "Oh, no, it was dust." Experiments that find unusual and unexpected things are a dime a dozen. You have to have some argument that the data you've collected are trustworthy, and I think that means they either conform with existing theory or, if they don't conform with existing theory, there's a really, really good reason to think that what you found is stable and reproducible, and going to be found in a robust variety of different contexts, such that theoreticians should spend their time paying attention to it. You don't just find new data and then publish it in *Nature* because it doesn't conform with expectations. That would be pandemonium, right?

KM: No, but that's the first thing she should do. Is it reproducible?

EW: Yeah, but just reproducible—just in the exact same way that you produced it the first time—might not be good enough to get into *Nature* or whatever.

HDR: Sorry, I want to comment on this, because it is a little bit strange to me. I think in present situations, it's almost impossible to get money to repeat an experiment somebody else did. So if he does an experiment with his molecules, I would like to repeat it just for fun, let's say. Or to establish whether this is okay or not. It's impossible. Even if I will never get it, right? If I read a proposal saying I'm going to…

EW: The problem goes both ways. For example, now it's a widely discussed crisis in social psychology, something like 70 or 80%—I forget the number—of published results, when people try to reproduce them, they fail. But, of course, I mean…

HDR: Okay. Let's take it a step further. Let's take the experiments they do in CERN [European Organization for Nuclear Research]. They're simply impossible to repeat.

EW: Well, there's two groups.

HDR: Yeah. And, even so, I mean, I know of experiments done a long time ago trying to establish whether it was supersymmetry and things like that. And some people asked for the data in order that they could evaluate…

EW: That's a scandal. I agree with you that's a scandal.

HDR: Okay. So I'm just finishing the story for people who don't know it. The data are just gone. So the experiment was done, and the data are no longer available for re-analysis. This is what they call in science "scientific practice."

EW: No, that's a common practice. I mean, the data should always be…

FN: This sounds like art.

AB: On the Large Hadron Collider (LHC), even without the scandals, it's a fact that the two groups—this they say explicitly—when they think they see something and they start having some certainty, they make informal contacts between the two groups before they make an official announcement to be sure no one… I mean, they meet openly, so it's a question. But I want to ask another question about this fact you cannot publish in *Nature*, say, some experimental results that are not interpreted, and you say it's a good thing because you would have a flood—maybe. But, then I'm asking you, is it okay to publish some complex mathematical theory beyond the standard model? It's not a problem—I'm just wondering. I'm just saying, because you say these experiments are a dime a dozen. They are not so cheap if it's a well-funded experiment that costs a lot of money. It's not "I'll try to publish something I did in my backyard." And also, among these theories, they are speculative, there are a lot. Once, I asked some theorists, "How do you know that that published paper is not, for example, mathematically wrong?" And the answer was, "I don't know. Nobody knows." If it's very interesting, the result, maybe somebody will compute it. So I'm just asking why there is this difference in treatment.

EW: I don't think it's that different. The same thing happens in mathematics. People make claims of having proved things… For example, a few years ago somebody claimed to have proven that arithmetic was inconsistent. And everybody doubted this, but the proof was fairly sophisticated, so the blogs and Listservs were buzzing about it. And it took several weeks, I think, for someone to find the flaw in the proof. The LHC story… ATLAS (A Toroidal LHC Apparatus) and CMS (Compact Muon Solenoid) were two different groups that were independently looking for evidence of the Higgs. A friend of mine told me the following story—this was before the Higgs discovery was announced, and both groups were trying to get to a five-sigma level of evidence. My friend was visiting members of one of the groups when word got out that the other group had apparently reached three sigma. And the response from the first group was, "We need to turn up our sigma dial." I mean, they just very self-consciously said: "We need to just estimate the sigma a little bit more liberally because we don't want to be behind."

AD: I'll try to mimic now what one of the physicists of the ATLAS group said to me, if somebody says something like you said, like that. You know what you do? "Hahaha!" You turn around and you laugh, because when you see… That's one of my main studies—I'm studying the ATLAS and CMS groups and how they produce knowledge. The different hierarchical things that go on in there and so on and so on. The only difference is that ATLAS is much more filtered with checks and balances systems, and less hierarchically organized than CMS. That means that in order to come to a point where they say that knowledge is true, as you said right here now, it passes many more bureaucratic hurdles, and that's why it takes longer to come to a point. But it would be really, in a way, "vermessen," as we would say—I don't know how you say this in English—ridiculous to do something like what you said right now, tweak the sigma and they go from three to five and they were just cheating it up. You know what I mean? As Arianna said, "They don't do this in a backyard." It costs €6 billion, and they are very much aware that the epistemic stakes are very high. And that's why they wouldn't do this. They may joke here and there about that, and that's just the typical way of joking at CERN. I would relay this anecdote more into a narratology amongst physicists dealing with the epistemic burden they have in order to produce that knowledge, because the whole world looks at it and questions what they do.

HDR: I would like to make a comment on the context of simulation and what you said about arithmetic not being consistent or whatever. If we do a simulation, we work with inconsistent data methods all the time.

EW: Sure, but I mean there's still an open question about whether arithmetic is consistent or not, right? We want to know. It might be okay if it's inconsistent…

HDR: But if you work with a digital computer, we know it's not. We don't have to discuss it. We know it. And it's not the point.

EW: Just in response to that, nobody meant to suggest that they could just, on a flip of a switch, go from three sigma to five, but there are various places along the chain where judgments are made. So for example, right at the detectors, huge amounts of data are produced and not all of them can be stored. So people right there have to make judgment calls about whether they're going to count something as a possible event and record it or not. And how liberal you are about making that judgment or not is going to affect your sigma. So, if the word gets around that the other group has gotten ahead and the people at those stations start hitting the mouse a little bit more frequently, sigma goes up—though not from three to five, of course.

AD: First of all, humans may sit at the trigger desk and the data acquisition desks, but it's the simulations that are done in advance, in dry runs, to test it out and see what you want to see. And, yes, 99.8% of the data is getting thrown away in the simulation, which makes visible what you want to see. But, actually doing that fieldwork at CERN and seeing how those people… Maybe they migrate knowledge from A to B, and, yes, maybe people marry in CMS and ATLAS, but even married couples tell the same story. And I think that's very important: they take this as a challenge. They are scientists. They are not doing fake experiments in order to become famous. They do not even have that need to become famous because there are 3,500 people working in a collective. So, they are really trying to take those sister experiments, as Karin Knorr-Cetina named them first, in order to really come to a point, because you can't reproduce the experiment because it's so expensive. That is ethics and science. And if you question that kind of ethical stance of somebody who stands in the control room in ATLAS or at CMS, you're, in a way, saying they might be unethical in their behavior. But I can assure you that if there are two unethical people, there will be 15 ethical people behind them, and they will argue.

EW: It's not at all a claim about them being unethical. It's a claim about them having to exercise judgment; they have to, it's unavoidable. And how you exercise the judgment affects the sigma. That's all.

AD: For example, also something: before the run was starting, everybody received a note entitled, "Be aware of the 'Look Elsewhere Effect.' Take care that you do not see things that you want to see." And that's a very, very funny paper because it starts with: you might sit there in the lab and you have your second gin and you want to see something, and then you see "Be aware." It's not about not drinking gin, it's about…

EW: Why do you think they need to caution about this? Because it's a real thing.

AD: Because people want to see something new. And that's something everybody discusses here. They want to see something new. They want to find something new. Theoreticians, simulationists, experimentalists, because that's something…

EW: They're not cautioning about that because they're worried that their workers are unethical. They're cautioning about it because it's a normal, real feature of science.

AD: But that's also something where we actually differ, because you are not in the field. You are not empirically seeing how they do and practice science, and that's something I would really stress to everybody who says they do mere semiotics there, and that this is just playing with science and so on. It's not just a mere play with science that doesn't matter. There's maybe something fundamentally wrong, but it's not at all… I can't even judge this.

MW: So, may I ask Wolfgang what the horizon of any symposium about ethics now would be if you're here to listen to all of that?

WH: First of all, we have to recognize that all people coming from literary studies and humanities say, "Oh, quantum physics. Oh, quantum physics." So, the first thing is to tear down the barriers between natural science, in the way we have talked about it in these two days, and humanities. This is very important. There are academic traditions in the world, especially in the USA, where this barrier is huge. And there are some other traditions where the barriers are not so big, and we have traditions in France, for example, and in Germany. We should follow up, too, I think. This is the reason I would recommend a workshop about the ethics of simulation, because there are ethical questions in the simulations, if you grab deep enough. Ethics is the philosophical discipline that is so important, even in America now, and very lively now. I mean, in all areas, they talk about ethics, and ethics I mean in the tradition of Aristotle and in the reception of Lacan, and in the way Blumenberg dealt with it, the German philosopher. Not ethics in a pseudomoral way, just ethics as in the very strong understanding even of analytical philosophy, which has a branch in America now. They call themselves "Ethic of Design," for example. And this is very important, to talk about ethics, because of this new branding, "Design Thinking," and things like that. So we want to have another workshop in Lüneburg, "Design in Simulation," because design is very important as a concept already in this field; we didn't talk about that at all.

This is my background and the horizon for my question. After having read the Karen Barad book, we should invite her, because she and Donna Haraway and all the guys there in Santa Cruz would be very interesting to talk with because they obviously thought about that from a gender studies perspective, from a post-colonial perspective, from a complete cultural studies perspective. I mean, it would be really interesting to get them known in this field as an enrichment of our discourse. Again, with the aim to tear down the barrier between natural science and the humanities, because this is a barrier of power, actually. It's a barrier of economic power, it's a barrier of political power, it's a barrier of social power, and we have to deal with that now. I mean, seeing all the problems we have in nature, so to say, it's also an environmental discourse we're talking about. And, at least this point should be worthy enough to talk about—this tearing down the barrier.

LM: I would just like to add something to that. Because, for me, this is absolutely not about some philosophers coming and telling people how to do ethical research. It is much more about the ethical implications of our theories. This became, suddenly, very clear to me when I worked on a very different project that was far outside of physics. And I stumbled upon a discussion that Hans Reichenbach had with Bertolt Brecht when they were both in exile in America. So, they were discussing uncertainty in quantum physics—that's actually where I think you're totally right. Uncertainty is not limited to quantum physics; it's something that is there in our everyday experience. And Brecht, he was describing his situation as a refugee by saying, "I am living in a huge casino, and I have to act as a gambler who can never predict what the outcome of the bet he's placing is." And then, I read a lecture that Reichenbach gave at the same time, and in this lecture Reichenbach says, "Well, in the times of quantum physics, the physicists act as gamblers who only place a bet on the outcome of the experiment, and they choose their bet such that they are most likely to win." And then, I suddenly realized that at the same time John von Neumann was publishing his theory of game and economic behavior.

So, there was a completely new ethics derived from the uncertainty that was encountered in quantum physics, and, as well, in the everyday experience of these German exiles. So I think that's maybe what ethics, or what the relation between ethics and physics, could be.

MW: Almost an abstract for the next symposium, maybe?

HDR: So maybe I can say something about uncertainty from our perspective. I think the question is not where does it come from—the question is where does your certainty come from? Uncertainty is everywhere, and it's part of, say, the way we process our information that we get from our senses and so on. Nothing is certain. I mean, we can talk about going out, but maybe there is no door when we get there. But, of course, our brain somehow builds a picture of the environment and so on. And we live in the picture, and the picture that we create is probably reduced to the part that is most certain. Otherwise, I don't think we could survive. The whole way of thinking and processing this information is derived from the desire to survive, I think. Evolution has done this for us. I mean, I don't know how this works, but, in some way, it did it, and it also dictates how we think and reason about things. So, uncertainty itself is something we have to deal with from the start, and I think the major problem with quantum mechanics, when it was somehow created, was that physicists had been thinking always in a world in which everything was taken to be certain. And then they were faced with the situation of "we really have to get rid of this idea." And that was, of course, a big shock for them. But, once you realize that uncertainty is everywhere, I don't think it should be a shock. In that sense, I would say, everyone can understand quantum mechanics. Apart from the mathematics perhaps.

FP: What's the origin of certainty? For me, nature is noisy. Everything is noise. So, if you can be certain about something, it must be stable for some time. So you are studying stable situations, and then you learn nothing is stable in this world. That's how to think about it. It's just an approach to get to stable things, or realizing stable things. And, because there's so much noise, it depends also on your techniques, how to identify these stable situations. So there are different points for this uncertainty, of course. If it's part of the object, if it's part of your reception, if it's part of your own receivers. And this, of course, is difficult to disentangle, I think. I haven't read about it so far.

HJR: This reminds me of a very nice book, which is now almost 40 years old. The title is *La Nouvelle Alliance*. It was written by Ilya Prigogine and Isabelle Stengers. The main message of the book is that physics, for centuries, has been looking for the situations that are, more or less, in one way or the other, stable. But what physics should really be interested in is learning about how things change.

MW: So we are now absolutely into the philosophy of nature, and we, as organizers, took another resort, which was to bring up the media question, as one way to avoid this, or to put it into another perspective. Anne, I think, would like to make a short remark on that.

AD: Yeah. As you said, nature is noise. I'm, of course, stumped after thinking about Shannon's paper. And even 80 or 90 years ago, the idea of noise, or message carriers, would not even be thought possible to have allegories. And that shows how much our current media are determining the way we use our vocabulary for nature. What you said about uncertainty and certainty reminds me that that might be one of the deep gaps between humanities and social sciences on the one hand, and physics or natural sciences on the other. When I teach anthropology, I say, "The goal of studying anthropology, at the end of the day, is to have a fundamental uncertainty about things." And when physicists teach the goal today, most of them, it's to find a fundamental certainty. And what Hans-Jörg Rheinberger just mentioned would be one of the possible ways of bridging the gap from the physics side, if it could come to the point to look for the uncertainty. It would be lovely.

MW: So, phew! We are back to media cultures, and maybe even to computer simulations again. Anne and I propose to give in to our exhaustion and our puzzlement and end this symposium.