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Intervention

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As announced beforehand, I will not be able to present a fitting, full-length contribution to this workshop, for two reasons: first, I am not a physicist and in particular not familiar with the mathematics involved in most of the argumentation of our preparatory reading; and second, I am not an expert in computer simulation either. Therefore I am actually here to listen to the specialists and learn from them rather than being ready to present my own work pertinent to the topic.

What I have always been most interested in as a historian of science is the question of how to describe and understand scientific experimentation and the shapes it assumes over time. For our purposes, it might be safe to start from the observation that the forms of experimentation span a broad space between two extremes. On the one hand, we have what could be called the demonstration experiment. You trim your experiment so that it fits, or fulfils the conditions of, certain theoretical assumptions that are being taken for granted. This is the traditional idea of an experiment as a testing mechanism. I myself have been more inclined to look at what Friedrich Steinle and others aptly call “exploratory experimentation” (Steinle 2005; Waters 2007). Exploratory experiments ideally produce knowledge that is not yet to hand, that is, genuinely new knowledge. The question is how experiments should be arranged and conducted so that they potentially give rise to knowledge that we do not yet have and could not even have imagined.

My interest is thus less in experiments designed to test consequences that can be derived from certain acknowledged premises, but rather in experiments that are designed in such a way that they allow for the potential detection of things that are not in the realm of the possible consequences of our present knowledge. This is precisely what conveys to the sciences their intrinsic historicity. Historicity is unthinkable without that element of contingency—not pure contingency, but a contained contingency of sorts. Let me relate at this point to the title of our workshop: this is exactly what I understand by the notion of "event." Experimentation in this sense is "eventuation."

My question in the context of this workshop is whether this distinction maps onto the two kinds of experiments described in the two papers that we were given to read. The matter-wave interference experiments might fit this latter description (Hornberger 2012). The question is whether computer experiments as described in the second paper (De Raedt, Katsnelson, and Michielsen 2014), which basically lack the resistance and resilience of matter to interact with as in real world experimentation, can do this at all. This is one point that interests me here. I would like to learn more about the concepts involved with *in silico* experimentation (see e.g., Gramelsberger 2010), and in particular the relation between laboratory experiments and computer experiments.

There is a longstanding tradition of what are being called "thought experiments." Does computer experimentation give thought experiments a particular shape and power? Or are they simply a means to test the consequences of certain mathematical assumptions, or to follow the deployment of certain algorithms, a tool to check virtual models as Jülich neuroscientist Katrin Amunts recently put it (see Zauner 2016, 16)?

There is another point I would like to briefly address. Reading the paper of Hans De Raedt, Mikhail Katsnelson, and Kristel Michielsen on "Quantum theory as the most robust description of reproducible experiments," (De Raedt, Katsnelson, and Michielsen 2014) I was intrigued by two things.

First, there is the plain and unmistakable rejection of any ontological commitment as far as the quantum behavior of particles—or more generally, matter—is concerned. "Quantum theory," the authors write by appealing to Niels Bohr, Max Born and Wolfgang Pauli, "describes our *knowledge* of the atomic phenomena rather than the atomic phenomena themselves" (ibid., 46). This is a radically epistemological statement. The question is: Can we generalize it? The first step would be to ask whether this would also pertain to computer experimentation. Computer models, say, of climate change, would thus describe our knowledge of the dynamics of the climate and not of climate change itself. The second step would be to ask whether we could include the scientific assessment of any macroscopic phenomenon as well, to the extent that *any* theory scientifically addressing the phenomena of our world would describe our knowledge—and test its consistency—of these phenomena rather than representing a statistical or cause-effect characteristic of the respective phenomena in question.

That would amount to the conclusion that scientific reasoning would be radically situated at the level of epistemology, including the measurements and counts that are being recorded as the outcomes of an experiment. These traces would always already belong to the level of representation, with the additional consequence that "representation" would be the wrong word to use in that context altogether. Ontologies in science would thus have to be qualified as metaphysical, following a tradition that goes at least back to Ernst Mach and his peers at the turn from the nineteenth to the twentieth century.

This latter point appears to me actually to be implied by the second claim of the paper that I found intriguing: that a macroscopic experimental setup with the characteristics described in that paper—"There may be uncertainty about each event. The conditions under which the experiment is carried out may be uncertain. The frequencies with which events are observed are reproducible and robust against small changes in the conditions" (ibid., 50)—can be given a quantum theoretical description. And in addition, that such a description is postulated to follow from ordinary reasoning in terms of logical inference. Does that mean, in the last consequence, that the distinction between a quantum level of description and a macroscopic level of description actually collapses? The following sentence appears indeed to indicate such a collapse: "Our basic knowledge always starts from the middle, that is, from the world of macroscopic objects. According to Bohr, the quantum theoretical description crucially depends on the existence of macroscopic objects which can be used as measuring devices" (ibid., 47). And the accompanying question is whether "our basic knowledge" includes "the principles of logical inference" (ibid., 46). The overall question is whether the argument can be summarized as follows: take an experimental setup with the above described (macroscopic) characteristics, apply the principles of logical inference, and you will arrive at a quantum theoretical description of the situation. Formulated otherwise: a quantum theoretical description is implied in and follows from a particular kind of experimental situation and the events resulting from that situation. The conclusion is that quantum theory would thus be a description of our very experimental way of knowing, if I see it correctly.

References

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Discussion with Hans-Jörg Rheinberger

Janina Wellmann: Thank you very much. Hans de Raedt, you want to answer first to the interventional questions of Hans-Jörg Rheinberger?

Hans De Raedt: Well, I'm not so sure. Too many questions at the same time; there's a lot of uncertainty here. I think what we try to show in the papers and data is that if you have these different uncertainties in the problem, and the way of collecting data is statistical, that it unavoidably leads to the kind of description that we now characterize as quantum mechanical. Whether or not you really then see the effects depends on the scale of the uncertainty versus the set of knowledge that you have, so to speak, for certain. But the structure of the theory is always like that; I think that is true. From a purely logical point of view, if you agree with the principles of logical inference, then the answer is it has to be the quantum mechanical description as a framework.

Lukas Mairhofer: Although I agree that we should be really careful about deriving ontologies from our theories, I have the strong feeling that you always have to assume something about the entities that your theory's operating on, that is on the logical level. Quantum theory doesn't tell us about the fundamental entities, what they are. It doesn't tell us, "Well, it's a particle." Or, "Well, it's a wave." But, it tells us that we should not think about it as only particles or only waves. That's what I think. And, to come to my question: What do you have to assume about your entities to logically infer quantum mechanics from microscopic classical physics? Because I don't think that you can. How do you get there?

HDR: So you have the paper there?

Hans-Jörg Rheinberger: Yes, I have the paper here.

HDR: If I remember well, maybe on the first page there's a quote from Bohr. You can read it, maybe, for us about what the aim of physics is supposed to be.

HJR: Yes. We have here three sentences that are ascribed to Bohr. First, there is no quantum world; there is only an abstract physical description. Second, it is wrong to think that the task of physics is to find out how nature is. Third, physics concerns what we can say about nature.

HDR: Okay, that's the one I want to have. So, the starting point of these considerations is exactly that we are not asking about what is, but only about what we can say. The starting point is the notion of the event, and event means the perception of the event that we as humans have. And, we can, of course, also start asking what is this? But this question we do not ask.

LM: But why not? Because to have…

HDR: Because we don't want to.

LM: But if you have an event, you already… I mean, you already ascribe to nature that it's possible to affect your senses. That's already not a logical statement. You cannot talk about… I mean, I'm completely with you that we should not put everything into ontology and that it is not the task of physics to teach us about nature. But, even if it is the task of physics to tell us about our perceptions, we have to assume, in our theory, some things about nature. I don't see how we can completely get rid of ontology.

HDR: No, no. But the paper is not against ontology, not at all. It's about how you reason on the basis of the information that you have about experiments. It is not against ontology. The talk that Kristel [Michielsen] gave is much more on the other side than this one. And there is no contradiction. There's absolutely no contradiction nor a conflict in these things. So, this paper is about how we can reason in the presence of uncertainty, and that is all it tries to do. And the starting point is then the data that we accumulate in the experiment. Where these data are coming from and whether there is an ontology behind it, which we don't know in any case, simply doesn't matter for the description. That is the message.

HJR: But the message is also that the kind of uncertainty we have on the macroscopic level is not qualitatively different from the uncertainty that you have on the microscopic or the atomic level. And that's a strong kind of claim.

HDR: So the underlying assumption in the whole story, of course, is that we never know everything. So, certainty would mean that for a given situation we are dealing with, we really know everything about the situation. Now, this is an assumption I'm not prepared to make. We always deal with situations of uncertainty. I'm not talking about real laboratory experiments whatsoever. Not about some mathematical theory. Because, in that case, of course we do know everything. It's hidden in the axioms. That's another situation. So we're not talking about this situation, we're talking about a situation where we have data that we accumulate by whatever means, say by looking at it or just registered by means of a computer or whatever. We have data and we want to make inferences based on these data. And then, completely in line, I think, with what Bohr was saying, is that in that case, quantum mechanics is exhausted in the sense that you cannot do better. It's the best inference you can make on the basis of the data that you have. In that sense, many of the experiments that are being done today, which are, so to speak, quantum experiments, are merely some kind of demonstration that people are searching for the conditions under which this is realized. And they have to work very, very hard to realize these conditions. If you don't have the right conditions, you'll also see something—why not analyze it? You don't, because you cannot. You see? You have no tools to do that. You don't see systematics—no.

Only in special cases you get systematics and so on, and what the paper tells you is that this is because it is the best. It is a kind of fixed point. It's an optimization in terms of theories. That is what the paper says, but it doesn't say anything about ontology. Not at all.

HJR: That was my addition.

LM: And I think my question was more directed to this addition.