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ERIC WINSBERG *Science in the Age of Computer Simulation*.
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SINCE THE PUBLICATION OF a much-referenced paper by Stephan Hartmann (1996), the philosophical literature on (computer) simulation has grown considerably. For example, there have been two special *Synthese* issues on models and simulation in 2009 (vol. 169, no. 3) and in 2011 (vol. 180, no. 1). Also, a comprehensive and helpful review of the philosophy of simulation was published recently, written by Till Grüne-Yanoff and Paul Weirich (2010). It is therefore no surprise that the first book-length treatment of this topic has now appeared. Written by Eric Winsberg of the University of South Florida, who is himself also the author of a recent shorter review article (Winsberg, 2009), *Science in the Age of Computer Simulation* is mainly a compilation of previously published texts, joined and edited to form a stand-alone book.

Simulations are concerned with applications rather than with the testing and justification of theories. But philosophy of science has not taken much interest in applications. As a consequence, Winsberg argues, the epistemology of simulation is unfamiliar to most philosophers of science and needs to be made explicit. Winsberg's book is almost all about the epistemology of simulation. As for the ontology of simulation, he does not offer a clear-cut definition, but he describes simulation in quite general terms in the introductory chapter, and later on he discusses the similarities and differences between simulation and experiment and how to distinguish them, touching on questions of demarcation and definition. But I do not think that Winsberg would oppose Hartmann's definition-like characterization that "a simulation imitates one process by another process" (Hartmann, 1996, p. 83).

What, then, are the main features of the epistemology of simulation? Winsberg lists two question types. First, there are questions related to the nature of simulations and what can be achieved by them: What is the difference between simulations and experiments? What is the role of simplifying assumptions (known to be false) in simulations? How should we evaluate the validity and reliability of simulation models? Secondly, there are well-known, philosophically more central questions that may be re-addressed – and perhaps re-assessed – in the light of computer simulations: What is scientific explanation? What about the realism *vs*. instrumentalism issue?

I will give a rather comprehensive summary of Winsberg's book, followed by a discussion of some of its pros and cons.

Following a brief introductory chapter, Winsberg's chapter 2 is about the characterization of the inferential steps in simulation. Among simulationists, a distinction is often made between *verification* and *validation*. Verification refers to mathematical exactness. Verification is fulfilled if the output of the simulation matches (to a desired extent) the true solutions of the mathematical equations that have been entered into the simulation process. Validation, on the other hand, refers to the appropriateness of the chosen equations in the first place. The simulation process is valid if the simulation output matches reality (to the extent that this can be observed). Described in this way, verification and validation seem to be more or less independent of one another. In other words, it seems possible to perform a verification without checking the simulation output, and it seems possible to perform a validation (judging the appropriateness of the model) without performing a verification. Winsberg argues that this picture is too simplified. In practice, he claims, simulationists are often unable to perform validation or verification independent of the other. There may be several explanations for this. For example, the empirical data may have been collected at other locations than those for which the simulation data are believed to be valid. Or the models underlying the simulation may be so complex that it is simply impossible to offer purely mathematical arguments for what results ought to be expected.

Is simulation experimentation or theorizing (or both)? What is the similarity between simulation and experimentation? These are the main questions of chapters 3 and 4. According to a naive (but widespread) view, simulations are nothing but brute-force implementations of solving difficult equations. Whereas simulations are sometimes used as a means of merely solving equations, there are many examples where they are more than number-crunching. For one thing, simulations often involve techniques that cannot be justified exclusively from a mathematical or theoretical point of view. For another, the result of a simulation is normally not merely a bunch of numbers, but an *analysis* of these numbers. So Winsberg rejects the naive view. According to a rival standpoint, simulations are (kind of) experiments. Simulations, it is claimed, are stand-ins for reality, and may be experimented upon just like reality. This view seems to have some support among simulationists themselves. Winsberg does not agree. To assume that simulations are experiments is to assume that simulations *reliably* mimic the physical system of interest, but this is not self-evident. To the contrary, whether simulations are reliable in this sense is open to discussion and must be assessed case by case. Simulations take established theories as points of departure, yes, but they involve methods and adjustments that may not be justified from a purely mathematical or theoretical point of view, and they do provide data which we cannot automatically take to be reliable. So Winsberg opts for a third view: Simulations are a new mode of science, between theory and experiment. The idea that there are activities in between theory and experiment is not new. Ian Hacking (1983, pp. 210ff) has named a middle category "calculation", though Winsberg prefers the term "model building". But Winsberg then maintains that simulation is not only model building, for simulation has more experimental features than the label "model building" would suggest. For example, simulation and experiments are both engaged in error management and use similar strategies for demonstrating the reliability of the process. Just as Hacking has argued that experimenters have a rich repertoire of techniques for assessing the reliability of experiments, Winsberg claims that a similar assessment palette is available to simulationists: comparisons with previously accepted data, with observations, with theoretical analysis, with intuitions, usefulness in engineering applications, etc.

But in spite of their similarities, there is some fundamental difference between simulation and experiment – or so philosophers looking into the matter have argued. What exactly is this difference? One answer, suggested by Gilbert and Troitzsch (2005, p. 14), is that "in a simulation one is experimenting with a model rather than the phenomenon itself". Winsberg (quoting this very sentence but from an earlier edition) does not agree, for in experiments, too, the setup will differ from the natural system for which it is intended to speak. Hence, both experimenters and simulationists should be required to put forward arguments for the external validity of their activities.

Another candidate for the difference between experiments and simulations is this: in an experiment, the object (the thing being manipulated) and the target (the class of things that the object represents) share some material similarity, but in a simulation the relationship between object and target is abstract and formal. This idea has found many supporters, and it has been suggested that the material connection makes experiments epistemologically privileged. But Winsberg thinks that the notion of material similarity is too weak and the notion of formal similarity is too vague to do the work.

Instead he argues that the important difference between simulations and experiments is what forms the basis of the hope that there are formal similarities, and how researchers justify their beliefs that there are such similarities. For simulations, the hope is based on the fact that we (think that we) know how to build good models of the target system, whereas for experiments the hope is based on the fact that the object and the target belong to the same kind of system. In both experiments and simulations, background knowledge is needed to gain new knowledge, but the background knowledge is not quite the same.

Are experiments better than simulations at producing reliable knowledge? Some philosophers have thought so, but Winsberg disagrees. This follows quite naturally from his claim that the important difference between simulations and experiments lies in the background knowledge needed to run them. If the quality of the background knowledge is high (and it is put to use skilfully), the resulting knowledge – whether from simulations or experiments – will be reliable. Therefore, it is not a general truth that experiments are better from an epistemological point of view. But Winsberg grants that experiments are epistemologically *prior* to simulations. By this he means that the knowledge needed for running simulations is more abstract and depends on knowledge gained from experiments, whereas the reverse is not true.

In chapter 5, simulations using theories from several fields, so-called theoretical hybrid models, are discussed. As an example, the simulation of crack propagation in solids may involve three theories, one for each physical region surrounding the crack, with significant feedback between all three. Nearest to the crack tip, the atoms are best described by quantum mechanics, which can account for the breaking of chemical bonds. But quantum mechanics is computationally very expensive, so in a surrounding area molecular dynamics will do better. Molecular dynamics allows for small local variations in the material to be taken into account, for example thermal fluctuations. But again, molecular dynamics is computationally expensive for linear dimensions larger than 50 nanometers or so, so for an outer region traditional continuum mechanics may be used. At the boundaries between regions, theories "shake hands" in Winsberg's words. A discussion of this hand-shaking leads to three conclusions. First, the relationships between different levels of description need not be purely logical, as is normally tacitly assumed when discussing, for example, reduction in the philosophy of science. Secondly, hand-shaking regions offer examples of models of an inconsistent set of laws whereas, normally, one would maintain that inconsistent sets of laws can have no models. Thirdly, the happenings of hand-shaking regions offer a counterexample to Ronald Laymon's (1985) much-quoted idea, that an improvement in a model should always result in an improvement in its empirical accuracy. This is so because in hand-shaking regions, an improvement in the form of more accurate values pertaining to one of the regions may not necessarily lead to an overall improvement.

Based on these conclusions (and in particular the third one), Winsberg claims that fictions may play important roles in scientific models. By "fiction", he does not simply mean "representation that fails to represent exactly", for there are many well-known fictions in science of that kind (such as point particles in physics). Instead, a fiction is something that is "offered with *no promises of a broad domain of reliability*" (p. 90). An example is given: In simulations of the behaviour of small pieces of silicon, so-called silogen atoms (silicon-hydrogen hybrid atoms) in the hand-shaking region are fictions in this sense. Although their employment adds to the reliability (and hence non-fictionality) of the simulation as a whole, they are not reliable guides to the way actual atoms in the boundary region behave. As for the question of how to decide whether a representation is fictional or non-fictional, Winsberg holds that the scientific community's norms (and not the intentions of the individual researcher) have the last word.

Chapter 6 offers an interlude on the role of non-epistemic values in science; or more specifically in climate models and climate simulations, which are infamous for their enormous complexity. Winsberg distinguishes three sources of uncertainty in climate modelling: (1) uncertainty about the exact design of the model, with its auxiliary assumptions, approximations, parametrizations, etc. (*structural model uncertainty*); (2) uncertainty about the values of the parameters involved in the model (*parameter uncertainty*); (3) uncertainty in the records of past climate data, whether they are actual observations or proxy data such as tree rings (*data uncertainty*). Winsberg claims that values play an inevitable role in the estimation of uncertainties of types (1) and (2).

His reasoning concerning (1) may be summarized as follows. Climate modellers have deemed certain modelling (and prediction) tasks more important than others, and these constitute clear-cut examples of decisions reflecting non-epistemic values. For example, the interest in modelling the future global temperature change has been considerably greater than the interest in modelling the global precipitation change. This has led to much greater uncertainty in the precipitation simulation data than in the temperature simulation data. Since the climate models are so complex, they are very much determined (or at least highly influenced) by their history. For example, the order in which features have been introduced into a climate simulation model might determine its possible future development as well as its overall performance.

As for (2), the reasoning is similar. There are parameters connected to the process of so-called parametrization for which one cannot claim that there are single correct values. Such a parameter may have a best value for a particular prediction task, not identical to the best value for another prediction task. If we ask two experts to give the best value for a certain parameter, it may be the case that one expert has worked predominantly with one simulation task and the other expert with another simulation task. Therefore, their judgments may well diverge, and it may be impossible to subtract the influence of non-epistemic values to reach "true" values of the parameters.

In chapter 7, Winsberg extrapolates his reasoning from chapter 5 on the role of fictions in simulations, claiming to have "counterexamples to the doctrine that success implies truth" (p. 121). A naive success-to-truth rule (this notion is from Kitcher, 2002) could be this: "If X plays a role in making successful predictions and interventions, then X is true". But Winsberg's analysis shows that this formulation needs to be amended in several respects, and he finally opts for the following version (with all additions to the naive formulation within parentheses):

> "If (the right sort of) X (in its entirety) plays a (genuinely central) role in making (systematic) successful (specific and fine-grained) predictions and interventions, then X is (with some qualification) true" (p. 126).

He then proceeds to show that artificial viscosity satisfies the antecedent but is still untrue. Hence, it is a counterexample to the rule.

Being the first book on computer simulations from a philosophical point of view, Winsberg's book is bound to be a reference work for many years to come. Very broadly speaking, the book could be seen as an elaborate rejection of the standpoint defended by Frigg and Reiss (2009) that there is nothing epistemologically interesting in simulation. In my opinion, Winsberg has succeeded; he has convinced me that there are several epistemologically interesting features of simulation. For example, I found Winsberg's discussion of the differences between simulation and experiment both illuminating and convincing. I also found his views on fictions thought-provocative. As for Winsberg's views on values, I found his chapter 6 somewhat detached from the rest of the book, but nonetheless suggestive and an interesting contribution to the discussion on values in science.

Perhaps I would have liked Winsberg to discuss which simulations are philosophically interesting and which are not, and exactly why. This would probably first require a clear definition of simulation; for, as noted by Grüne-Yanoff and Weirich (2010, p. 29), the debate on philosophical novelty "obviously depends on how simulation is defined". In any case, I believe Winsberg would agree that there are philosophically uninteresting simulations as well. Consider, for example, the situation where we have statistical distributions (known theoretically or empirically) for several input variables, and wish to amalgamate a distribution for some output variable taking all input variables into consideration. We may not be able to find the output distribution analytically and hence need to use simulation techniques (sampling techniques). We sample from the input variable distributions and enter the sampled data into a formula and get an output. By repeating this sampling process many times, we are able to obtain an empirical distribution for the output variable. This is considered a pure *statistical* simulation process and is an example of a so-called Monte Carlo simulation. As far as I know, philosophers of science have not considered Monte Carlo simulations to be of any particular epistemological interest. Winsberg mentions Monte Carlo simulations in a few places, for example on page 115 where they are presented as a standard procedure. Winsberg could have been clearer in stating that these simulations are philosophically uninteresting, if this is indeed what he thinks.

The above point is a minor one. But there is another matter where Winsberg's conclusion could be questioned. He has argued that there are fictions in science (in the sense he defines). There are silogen atoms, artificial viscosity, and so on. I agree that these are fictions in several senses, including the one advocated by Winsberg. But when he tries to make a broader point pertaining to scientific realism, I am not entirely convinced. He uses artificial viscosity and silogen atoms to argue that the success-to-truth rule must be abandoned, and hence the view that success implies truth. But are we really forced by notions such as artificial viscosity and silogen atoms in scientists' models and simulations to reconsider the role of truth and realism in science? After all, are we not all aware that artificial viscosity is a fictitious entity? And don't we all know that silogen atoms do not exist? In both cases, we should be extremely surprised if we found out that artificial viscosity and silogen atoms do exist. Since the springboard of Winsberg's argument is the demonstration that there are counterexamples (such as artificial viscosity) to the success-to-truth rule, it should be important to him that the version of the success-to-truth rule he is using is the only reasonable one when discussing scientific realism. But as far as I can see, this is not so. We could simply add another extra parenthesis to the rule, stating the quite obvious clause that we do not believe something to be true in the face of strong arguments against it:

> If (the right sort of) X (in its entirety) plays a (genuinely central) role in making (systematic) successful (specific and fine-grained) predictions and interventions *(and if we have no other strong reasons for believing X not to be true)*, then X is (with some qualification) true.

With the italicized parenthesis added, Winsberg's counterexamples no longer work.

My adjustment of the success-to-truth rule makes it weaker, of course. But I do not think I have made it too weak to be of interest. There may still be counterexamples to the adjusted rule. It just seems strange to invoke artificial viscosity and silogen atoms as counterexamples to a success-to-truth rule while completely ignoring that there are other strong reasons for believing these concepts to be fictitious. When philosophers have argued against (naive) scientific realism, they have often done so by attacking success-to-truth rules with historical counterexamples. These include the the phlogiston theory of combustion and the wave-in-ether theory of light, both of which were successful for quite some time and therefore supposedly show that science can be successful without truth, and hence that science does not necessarily lead to truth. But in these historical examples, there were once good reasons for believing the theories to be factually correct; i.e., to believe that phlogiston and the ether existed. In other words, these theories were proposed and defended in the explicit hope that they were factually correct. In the case of artificial viscosity, by contrast, Winsberg admits that already from the start it was introduced although it was known to be an "unphysically large value of viscosity" (p. 126f). If it is indeed unphysical, I assume there are rather strong reasons, derived from well-corroborated physical laws, for doubting its physical existence. As for silogen atoms, they are hybrids of silicon atoms and hydrogen atoms, and again from well-established physics we know that such hybrid atoms do not exist. We have *never* believed that artifical viscosity and silogen atoms exist, so it seems unfair to use them as arguments against realism.

Although I do not share Winsberg's conclusions on realism, however, I do consider his book an important contribution to the philosophical literature on simulations. It is simply a must-read for anyone interested in the topic.

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