ON COMPUTER SIMULATIONS, WITH PARTICULAR REGARD TO THEIR APPLICATION IN CONTEMPORARY ASTROPHYSICS

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ABSTRACT: In this *survey* contribution, which extends [GBa19], we consider and discuss computer simulations from a variety of perspectives with particular attention to computer simulations in astrophysics and cosmology. We begin by reviewing earlier, related science-philosophical literature on this topic. Then we point out several fundamental limitations which computer simulations are, as a matter of principle, not able to overcome. We conclude our considerations with the conjecture that computer simulations are technically amplified *gedankenexperiments* (thought experiments). Another important insight which follows from our considerations concerns the epistemic value of such simulations: the more complex the underlying models, the less we are able to learn from them.

KEYWORDS: Computer simulations; Astrophysics; Cosmology; Thought experiments

1. MOTIVATION

In recent years there has been a sharp quantitative increase in science-philosophical and science-historical publications on the epistemological and methodological questions arising from the practice of computer simulations since John von Neumann's programmatic statement of 1946 which asserted that numerical (as opposed to analytical) methods would be needed to overcome the contemporary stagnation in the progress of the empirical sciences [Gra11c].

Meanwhile the topic has reached the popular science literature for generally educated lay people [Pöp15b]. As a whole, the available literature addresses (amongst many other details) the following main questions:

- What is a *computer simulation* (and by contrast which type of scientific computer applications are not computer simulations)?
- What are, more generally, *simulations* (even without a computer)?
- Are computer simulations proper experiments or rather thought experiments (*gedankenexperiments*)?
- Is it possible to genuinely *learn* anything from the design and execution of computer simulations about the external world, (i.e.: not about the software and not the computer by means of which some traits of the external world had been simulated)?
    - If yes: what can be learned?
    - If no: what other sensible purposes could computer simulations possibly serve?
- Can the emergence of computer simulations be identified with the emergence of a *new type of science*, or do computer simulations fit well into the conceptual and methodological framework of what we know science-philosophically and science-historically as 'classical modern science' since several centuries [Bei11b] [Gra10] [Rup11a] [Rup15]?

All these questions have already been answered, albeit differently, by various authors. In this survey contribution we aim at continuing this discourse by some further thoughts which had not been emphasized in previous work. To limit the scope of our considerations we do not go into the closely related field of computer-supported discovery environments [dJR97].

In this survey contribution we philosophise about computer simulations from the combined perspectives of modern astrophysics, in which computer simulations are nowadays routinely conducted, and computer science, which provides the equipment for such simulations. After a brief recapitulation of some of the already existing science-philosophical literature on this topic we unfold our own arguments from various aspects and with regard to several of the questions mentioned above. Thereby, throughout the remainder of this contribution, we take for granted that the well-known *Church-Turing conjecture* of computability [Vol95](p. 151) demarcates the ultimate limit of what can (and cannot) be computer-simulated by means of a von Neumann/Zuse type of machine or by finite compositions of such devices in parallel or concurrent clusters. Examples of 'worlds' which are (because of the fundamental Church-Turing conjecture) in principle *not* adequately computer-simulatable are the 'ball world' described and explained by Penrose [Pen89], as well as (most probably) the biological brains of higher mammals [Pen89](ch. 9) [Vol95](ch. 6). This well-known technical limit of Church-Turing simulatability is additionally constrained by the physical limitation of the maximal knowledge possibly available to Laplace's Demon.

All in all the argument of our contribution has the following structure: After the recapitulation of relevant related work we discuss the *relationship between computer simulations and scientific hypotheses* on the premise that all science is, in principle, hypothetical. In a similar manner we then explore the *relationship between computer simulations and explanations* on the premise that the provision of explanations is one of the most important purposes of modern science. Thereafter we discuss the *epistemological position of computer simulations between real experiments and gedankenexperiments*, particularly with regard to *computer simulations in contemporary astrophysics*. Since astrophysical processes can be understood as special cases of *nature-historic processes* in general, we subsequently outline some more general philosophical thoughts about *computer simulations and the history of nature*, at a high level of speculative abstraction, which also includes the already mentioned thought-figure of Laplace's Demon. The question whether or to what extent computer simulations can make any terminological contribution to the *evolution of the theoretical language of science* is briefly raised thereafter. We conclude our contribution with a summary of our propositions as well as with some hints to open philosophical questions for future work. Last but not least an appendix provide further details for especially interested readers.

2. RELATED WORK

Due to the rapid growth of literature in this specific science-philosophical discourse we cannot provide any exhaustive nor comprehensive literature survey. Several science-philosophical journals have already published (even repeatedly) *special issues* on this topic: volume 29/1 (2019) by Minds and Machines} is the newest of which we are currently aware, whilst yet another Philosophy and Technology is already in preparation [personal communication by L. Floridi (ed.) via e-mail: 7th of March 2020]. Thus we have to be selective, choosing either representative or historically important publications.

Whereas a *definition by demarcation* of the (often rather vague) *notion* of 'computer simulation' was recently provided by Formanek [For18], a systematic literature review on this topic was published by Grüne-Yanoff and Weirich [GWe10]. Several *culture-philosophical and hermeneutical* remarks (which are not in the scope of our science-philosophical considerations in this contribution) concerning the *semiotics of the new media* and *new concepts of reality* in the presence of computer-simulated virtual worlds can be found in [Krä98], whereby it seems trivially true that computer simulations can produce for us new knowledge about reality if we accept, a-priori, the virtual worlds of cyber-space as 'real', and particularly in [Esp98] [See98] [Wal98] [Wel98] albeit with some flaws in their computer-technical accuracy. The early history of computer simulations since WW2 was recently recapitulated in [Gra11c] as well as in a sequence of closely related publications [Bei11b] [Gra10] [Sto10].

Addressing a false trust in the epistemological value of computer imulations and computer-supported modelling, Booβ-Bavnbek and Pate discussed an irrational mind-set of *magical realism* [BPa92] in the community of computer users. In that context, magical realism refers to the deceiving appearance of a quasi-reality in numerical simulations on the basis of wrong or wrongly understood mathematical-physical equations, as well as on the basis of un-understood or instable algorithms [BPa92](p. 235). Though the methodology of applied mathematics distinguishes clearly between the three concepts of modelling, computational approach and algorithm, it is often forgotten that these three concepts are indeed distinct and that different quality criteria are applicable for each one of those three [BPa92](p. 237). The essence of magical realism is thus a profound confusion between technical feasibility and theoretical understanding, as well as the fallacy of mistaking technical feasibility in specific individual instances with unlimited applicability of the techniques in general [BPa92](p. 239). Theory-less 'dabbling' under the illusion of 'knowledge' characterises such magical realism [BPa92](pp. 240, 245).

A number of answers to the questions: *'What are simulations?'* and *'What are computer simulations?'* can be found in a multi-authored book edited by Braitenberg and Hosp [BHo95]. A few essential points of this book are briefly recapitulated in the subsequent paragraphs: one for various answers to the question: 'What are (computer) simulations?', and one for statements concerning their *scientific novelty* or *epistemic power*.

According to Braitenberg [BHo95](p. 8), the situations which are simulated on computers are *abstractions* of what is empirically given: merely small fragments of what is known or knowable to us.

Puhr-Westerheide added that those abstractions are *models* which serve as 'representatives' of reality [BHo95](pp. 10-11). With reference to the *Overstreet-Nance* method of simulations [ONa85], Puhr-Westerheide defined any simulation as a 'game' which follows particular rules on the substrate of a suitable technical equipment called 'simulator' [BHo95](p. 15); 'game' must be understood in Wittgenstein's sense in this context since there are no winners nor losers. Thereby each computer simulation can achieve or explicitly reveal only so much information as was implicitly hidden in its underlying formal model and the instructions of its evaluation calculus [BHo95](p. 21). Puhr-Westerheide's definition of 'simulation' did *not* demand that there have to be *internal similarities* between the simulated real processes and the simulating computational activities *in silico*. For this reason there is more room for error in computer simulations than in real observations, because the wrong problem can be solved ('error of third degree') if the formal model or the formal evaluation rules were not suitably chosen from a semantic-pragmatic point of view [BHo95](p. 24).

According to Longo, simulations happen not so much in the realm of matter and energy, but rather in the realm of *information*, i.e.: in a mental or ideal sphere [BHo95](p. 26). Our mind makes it possible to represent parts of reality and to play with them. In this way even the mental creation of counterfacts becomes possible, which also sheds light onto the notion of *causality* in simulations: they must thus at least be intrinsically coherent and consistent [BHo95](p. 33). Bateson's Jung-inspired distinction between the realm of *pleroma* (things, matter and energy) and the realm of *creatura* (concepts, symbols, information) can be made fruitful for the philosophy of simulations, too: in *creatura*, anything can symbolically represent anything else, whereas in simulations, we need a sufficient degree of structural similarity or quasi-isomorphic correspondence between the simulating and the simulated entities [BHo95](p. 33). For the notion of 'similarity' and its relation to the notion of 'analogy' see [Sch91](pp. 9-10, 22-33). Also Psillos has explicated similarity in terms of various types of analogy (particularly w.r.t. the role of models) in the philosophy of scientific realism [Psi99](pp. 140-143). In the realm of Theoretical Informatics (particularly Automata Theory) the notion of 'bisimulation' between two rule-based transition systems is the strongest formal concept which fulfills Longo's isomorphic similarity requirements of above. For additional remarks on the mathematical-logical concept of bisimulation, which is indeed a special form of an equivalence relation, see [Tho97](p. 415). Thus, to judge the appropriateness of a simulation, one does not refer to absolute or total correspondence but rather to a *partial correspondence* of the simulation results with an observed phenomenon in reality [BHo95](p. 34). By the way Longo has also shown an important *limit* of simulations: intelligence cannot be simulated because 'fully' simulated intelligence simply *is* intelligence (and not merely a simulation thereof).

Also Neunzert noted the already mentioned discrepancy between nature and model. According to him, considerable amounts or pure (hitherto un-applied) mathematics are needed for simulative modelling, such that the resulting models resemble traits of nature only at large scale, but not in the finest details. Pictures or images emerge as the result of computer simulations: they serve the purpose of predicting particular traits of reality, though they are not meant to reflect any more comprehensive truth [BHo95](p. 55).

Hahn, who discussed the computer simulation of human speech, did not distinguish sharply between the notions of model and simulation. He merely characterised the model as a description and the simulation as a process [BHo95](p. 85) without demanding any internal structural homomorphisms or isomorphisms between an original and its *simulatum*. Moreover he more or less identified a simulation with an executable program [BHo95](p. 86) whereby he ignored the conceptual and actual differences between a program and a process.

Mulser, a theoretical physicist, identified the notion of 'simulation' merely with the solving og sufficiently complicated (yet structurally simplified) systems of mathematical equations [BHo95](p. 111) in which homomorphisms between chains of events do not play noteworthy conceptual role. In a similar context, Untersteiner added that the digital computer's inability to represent non-natural numbers with unlimited precision can decrease the numeric validity of simulation results from iteration to iteration to such an extent that long-term simulations can become practically worthless [BHo95](p. 140).

All in all, on the basis of [BHo95] we can state that the *notion of 'simulation'* *varies from academic faculty to academic faculty* whereby psychologists, linguists and physicists have rather different methods and techniques in mind when speaking of simulations. Common to all of them, however, are the utilisation of computers as equipment as well as some inevitable discrepancies between their models and their corresponding traits of reality. While is is not easy according to Braitenberg to identify the specific novelties of computer simulations in comparison with traditional scientific methods and practices [BH095](p. 7), *examples exist of computer simulations of systems about which no knowledge could previously be acquired by any other means* [BH095](pp. 20, 22). Computer simulations also enable us to inspect counterfactual worlds [BH095](p. 33) from which indirect conclusions about the real world can be drawn *ex negativo*. Moreover, high-quality simulations can be fruitful for the design of future real experiments. More radical is Untersteiner's view that the *meaning of the term 'experiment' has been changed* by computer simulations to (now also) include solving mathematical equations by computing machinery [BH095](p. 136); a similarly 'softened' or 'widened' notion of (technological) 'experiments' can be found, for comparison, in [Han15].

In his recent article [Bei18], Beisbart, too, pursues the question as to whether computer simulations can be seen as experiments. According to him, computer simulations on their own are not necessarily scientific. They differ from (scientific) experiments because the reaction of the system is defined by the simulator rather than by nature. Thus, computer simulations do not reach out to nature in the way experiments do. Nonetheless, according to Beisbart, computer simulations create 'knowledge' because they provide models for observed processes and thus also for possible (future) experiments. Furthermore, computer simulations represent their targets in a way similar to mathematical and notional models. Computer simulations thus *trace the consequences of theories*} for certain applications. The validation or 'verification' of computer models however is generally difficult. In the empirical sciences (including physics), in which computer simulations are nowadays applied, it has become quite usual to speak about the 'verification' of computer simulations by means of tests and comparisons against observable world-phenomena. From a strict theoretical computer science point of view, however, we are not allowed to speak about 'verification' in this context, because the term 'verification' in theoretical computer science has a rather different meaning. From a computer-scientific point of view we may call any piece of software (hence, also: simulation software) 'verified' only if its internal logical properties have been mathematically proven to hold against a formally given requirements specification. This strongly formalist computer-scientific notion of 'verification' does not hold in most (if not all) cases in which empirical scientists claim to have 'verified' their simulation software. Various papers not only from physics but also from other computer-simulating disciplines (e.g.: economics) have already described and discussed those validation problems [Jeb13] [JAr19] [Kle95] [Kle98] [Mih72] [NFi67] [Sar13]. Some of these methodological discussions are dating back to (at least) the late 1960s [NFi67]. In such a context Beisbart arrives at the conclusion that computer simulations can be seen as 'arguments', which puts them on a common ground with *gedankenexperiments*: for further details see [Bei11a] [Bei12].

As one instance of several related 'special issues' already published by various journals we may mention at this point volume 196/3 of *Synthese* [FH+09] (guest-edited by Frigg, Hartmann, and Imbert with eleven contributions), which was dedicated entirely to the science-philosophy of (computer) simulations. In the subsequent paragraph we briefly recapitulate a few points of view from [FH+09] of skeptics and protagonists regarding the question as to what kind of epistemological value or novelty, if any, may be attributed to computer simulations. It is indeed a topic of ongoing dispute whether the introduction of computer simulations adds any genuine epistemological or methodological novelty to the system or the practice of the sciences: see [NR+11] for comparison.

According to Gelfert, rigorous results illustrate the capacity of computer models to go beyond fundamental theory (of physics) [FH+09](p. 517) and to give rise to new epistemological possibilities. Humphreys argued that computational science requires a new and non-anthropocentric epistemology, together with a new account of how theories and models are applied, in order to justify the claim that computational science is new and *sui generis* [FH+09](p. 625). While computer simulations as such do not qualify as experiments according to Parker, studies based on computer simulations do, because they involve intervening in a system to see how some of its properties change [FH+09](p. 495). At the example of computer simulations in sociology, Grüne-Yanoff pointed out that computer simulations in this field can offer at most a weaker form of explanation, namely *functional explanations* [FH+09](pp. 553-554). Frigg and Reiss argued that the philosophical problems raised by computer simulations all have their analogies in traditional contexts of modelling, experimenting, or *gedankenexperiment*, such that the classical philosophy of science can well cope with this recent technical phenomenon [FH+09](p. 611).

In his book [Win10], Winsberg has rejected the opinion that computer simulations would be merely computational applications of formal theories: see [Gru13] for additional remarks.

In [Gra11a], Gramelsberger diagnosed that the large volume of *in silico* experiments since the 1950s ought to be regarded as a unique feature of late-modern science, which leads to a philosophical need for a systematic methodological assessment of the strategies for evaluating scientific results based on mass-data output devices [Gra11a](p. 212). Further details of those model-based methods and problems were discussed by Gramelsberger in [Gra11b].

In [Rup11b], Ruphy discussed two procedures of model construction, including their epistemic goals, and asked whether or not the computer can truly succeed in providing us with a new window through which the universe can be observed [Rup11b](p. 178). Ruphy identifies a tension between the goal of computer simulations and the limits set by the path dependency and plasticity on the possibility of validating the results obtained, because the more composite a model gets to be realistic, the more you lose control of its validation [Rup11b]. Her application of these and similar thoughts to the domain of astro-physics can be found in [Rup06].

In a more recent paper [Ang15], Angius pointed out that algorithmic simulation runs are not always needed in order to discover interesting properties of the system under scrutiny. If the system of interest is sufficiently modeled, then model checking techniques from the field of theoretical computer science can well be applied for discovering interesting system properties.

To conclude this related-work section we mention (last but not least) the recent simulation-philosophical works by Juan Duran which are so numerous that they cannot be summarily recapitulated at this point. Most relevant in the epistemological context of our survey are Duran's simulation-epistemological analyses which can be found in [Dur13a] [Dur13b] [Dur18b]: all of them deal with the science-philosophical problem of 'explanation' as well as with the reasons and the limits of the 'explanatory power' which computer simulations might possibly have.

Though the literature survey provided in this section is far from being complete, it should have shown that the *philosophy of computer simulations is now a firmly established sub-field in contemporary philosophy of science*; it has also shown the *typical* questions and answers which frequently occur in its ongoing discourse.

3. COMPUTER SIMULATIONS AND SCIENTIFIC HYPOTHESES

According to Bunge [Bun98a] [Bun98b], with whom we agree at this point, *all science is hypothetical.* Science-work is thus, to a large extent, work with or about hypotheses. Thereby, according to Bunge, the following three aspects of working with hypotheses are especially important:

- Hypotheses must (somehow) be created;
- Hypotheses must be rationally (theoretically) scrutinised for their own intrinsic consistency and their logical consequences and implications;
- Hypotheses must be empirically (experimentally) challenged.

In this context, real laboratory experiments and *gedankenexperiments* can be seen as two dimensions of one over-arching scientific process. They sharpen concepts and scientific hypotheses, and can thus lead to new hypotheses as well as the conception of further experimental set-ups and tests. It is important to see that experiments alone, occurring in a laboratory or in a researcher's mind, cannot by themselves drive any scientific progress. Only their intimate interplay with hypotheses makes them powerful. Conversely, hypotheses alone remain speculative until they are subjected to experimental tests. Consequently, if computer simulations are meant to have any scientific relevance, we must ask a number of question about how (or how far) computer simulations can be related to those three hypothetical-scientific activities mentioned above. The subsequent paragraphs pose and discuss some of these questions.

Can Computer Simulations Help to Postulate New Scientific Hypotheses? Our answer to this question must be a definite *yes*, because the invention of hypotheses is a creative process which does not need to be fully scientific in itself. According to Popper's well-known 'Logic of Research' [Pop35], the genealogy of a scientific hypothesis is irrelevant if only the hypothesis fulfills the usual criteria of scientificness, such as well-formedness, testability, and the like. As creativity is often inspired by surprising or astonishing observations, also the observation of an astonishing run of a computer simulation can thus inspire the formulation of new hypotheses. In such creative processes, according to Bunge, scientific hypotheses may be originated by analogy, induction, intuition, deduction, or construction [Bun98a](p. 277). A comprehensive description and critique of the analogy-method in the natural sciences was already provided in the late 19th century by Wilhelm Wundt in his seminal trilogy on the 'logic' (epistemology) of the modern sciences [Wun]: since then, analogical reasoning has been confined mostly to the realm of the humanities (*geisteswissenschaften*), because in spite of their 'power to inspire' (new ideas) analogies do not have the power to provide the causal-mechanismic explanations which the natural sciences are supposed to find.

Can Computer Simulations Help to Rationally Scrutinise the Logical Implications of Scientific Hypotheses? Our answer to this question must be an ambivalent yes-and-no'. The answer's yes-part refers to the well-known fact that Turing machines are indeed capable of rule-based formal-logical deductions of consequences from given premises [Mac95]. The answer's no-part refers to the following computer-theoretical and science-practical difficulties:

- From a computer-theoretical point of view it must be remarked that not all formal-logical systems are decideable. More precisely: given
  - a formally denoted hypothesis H as an axiom,
  - a formalised knowledge-base K which represents the logical rules of reasoning together with the already known propositions of some empirical-scientific theory,
  - an intended (desired or undesired) consequence C,
  - it might not be possible for the computer to deduce C from H by means of K within a finite amount of time.
- Secondly, from a science-practical point of view, it must be remarked that scientific theories are typically not provided in a fully and completely formalised syntactic *gestalt* that would satisfy the requirements of mathematical formalism.

Thus, computer simulations can help us to explore the logical implications of given hypotheses, but only to the extent that the knowledge-base K needed for such an exploration is small enough to be digitally representable, and only within the Gödel'ian decideability limits of the formal-logical calculi utilised for such deductive explorations. This may be feasible for some special cases of scientific investigation, if those special cases are small enough.

Can Computer Simulations Help to Test Empirically the External Validity of Scientific Hypotheses? In the following discussion the 'external validity' of hypotheses is understood as depending on their reference semantics outside the realm of language whereas their 'internal' validity would merely be a matter of their intra-linguistic well-formedness or formal-logical consistency.

The *formal sciences* of *logics and mathematics* [Bun98a](p. 27) typically aim at establishing *nomological statements* [Bun98b](p. 149) in the form of hypotheses. Such conjectures remain hypothetical until the mathematician or logician discovers their water-tight formal proof. Also in this classically `un-empirical' domain of science, computer simulations can indeed be helpful. Though the verification of hypotheses by stepwise algorithmic calculations is impossible in entity sets or entity classes of infinite size, falsification of such hypotheses can be possible if only one counter-example can be computationally detected within a finite amount of computation time. Even where computational verification is in principle impossible and computational falsification has empirically not succeeded so far, the repeatedly observed falsification-failure can help the mathematician to gain confidence in the probable truth of hitherto un-proven mathematical conjectures. One of the most famous examples of this kind is the conjecture by Lothar Collatz of 1937 according to which the algorithm of **Table 1** will terminate for all Natural Numbers. Indeed, every empirically observed run of the Collatz algorithm has terminated so far, but Collatz's conjecture of 1937 is still formally un-proven. Computer simulations have also been helpful in the field of pure mathematics to gain confidence and intuition about hypotheses concerning the membership problem and further properties of fractal sets, of which the spectacular Julia- and Mandelbrot sets (**Figure 1**) are well known [Man77]. In 'unfortunate' cases the Mandelbrot algorithm can continue to run forever without ever being able to decide whether a point in the Complex Numbers is a member of the set or not. The testability of hypotheses by computer simulations in this formal logical-mathematical sub-realm of the sciences is possible because algorithms themselves belong to this formal realm. The simulations in those cases do not attempt to signify anything in the empirical-physical world outside the realm of mathematical formality. Therefore no science-philosophical 'bridge arguments' are needed to close the gap between those ontologically different two realms.

TABLE 1: THE COLLATZ ALGORITHM	
0. INPUT positive natural number *i*; 1. IF *i*=1 THEN HALT ELSE CONTINUE 2. IF *i* is even THEN *i*:=*i*/2 ELSE *i*:=3*i*+1 3. GOTO step 1.	Having empirically halted for every *i* thus far, the algorithm's halting for *all* inputs is an unproven conjecture since 1937.

Hypotheses are also relevant in the domain of *(software) engineering*. Given a software system (or computer program) *S*, the software engineer is interested in its set of possibly unknown properties, *P*, in a similar way in which a natural scientist is interested in the still unknown properties of a newly discovered natural object. According to Dijkstra's well known aphorism, however, testing can be used to show the presence of defects however not their absence [Dij70]. By running a given software system *S* on the basis of a finite test input set *TI* it is thus possible (at least in principle) to falsify (in a Popperian sense) a hypothesis of the form: '*S* is faulty'. Since the admissible input domain *ID* of *S* is typically infinitely large, it is *not* possible to verify the error-freeness of *S* by means of computer-simulative runs. While this problem is well known to many philosophers of science [Ang14], it should be noted that simulative tests also in these software engineering cases do not attempt to signify anything in the empirical-physical world outside the realm of mathematical formality to which our software system *S* belongs. Therefore, once again, no science-philosophical 'bridge' arguments are needed to close the gap between those ontologically different two realms.

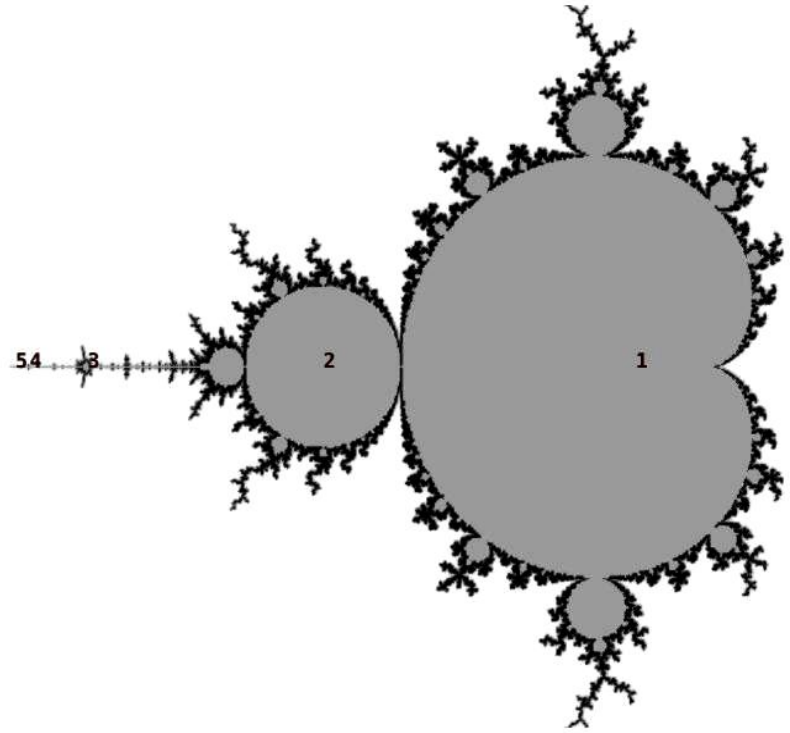

Figure 1: Over-simplified picture of the Mandelbrot set M the membership problem of which is not entirely analytically solvable. For some points c of the Complex vector space their membership hypothesis (c in M?) can only be algorithmically refuted, however not algorithmically verified.

At this point we can thus assume that no finite empirical method (and hence no computer simulations either) can verify any universal or quasi-universal hypotheses about the world. Thus the only remaining question is whether or not computer simulations can be used to falsify hypotheses in those remaining domains of reality which were not already discussed in the preceding paragraphs. Strictly speaking, the answer to this question must be 'no', because of the ontological distinction between the above-mentioned realm of *creatura* [BH095](p. 33) and the immaterial realm of computer programs and algorithms. To claim that a simulation in this immaterial realm could falsify any material-empirical hypothesis about the *creatura* implies postulating yet another quasi-universal meta-hypothesis, namely that the simulating algorithm would certainly represent a truthful (isomorphic) image of the *creatura* in all its relevant aspects whereby no relevant aspect has been forgotten or left out. By virtue of its quasi-universality, this meta-hypothesis is itself not empirically verifiable even *if* we *would* be able to decide with certainty (beyond our intuition) which 'aspects' of the *creatura* are actually 'relevant' and which are not: see [Pop35](ch. VI.36) for comparison. Moreover, also the notorious *Duhem-Quine dilemma* becomes applicable at this point, because all simulating algorithms must run on material computer hardware which is itself a contingent and not fully known member of the realm of *creatura* about which the algorithmic simulation is supposed to yield falsifying assertions. As a consequence of these considerations we can now state: if algorithmic simulations cannot be used to reliably falsify (nor verify) hypothetical assertions about the contingent realm of *creatura*, then the epistemological value of their widespread use and application in the empirical sciences must be of some other kind which is useful for neither verifying nor falsifying. This other kind of science-practical usefulness of algorithmic simulations will be clarified further in the subsequent sections of this contribution.

4. COMPUTER SIMULATIONS AND SCIENTIFIC EXPLANATIONS

Can computer simulations explain anything about the external world of the creatura? Any answer to this question will of course depend strongly on an accepted notion of 'hypothetical explanation', which we adopt for this section from [Bun98b](p. 192). **Figure 2** reproduces Bunge's illustration of the epistemological relations between an observed indicator *O* and an un-observed object *U* where it is the purpose to assert some (possibly explanatory) conjecture about *U*.

Most of the facts we know something about are *vicariously observable*, i.e.: they can only be inferred through the intermediary of perceptible facts by way of hypotheses [Bun98b](p. 192). In those cases we *objectify an unobservable fact by positing its lawful relation to some perceptible fact (or set of facts) serving as an indicator of the former* [Bun98b](pp. 192-193). This has the consequence that *the appropriate symptoms or indicators of unobservables must be devised, which raises a cluster of methodological problems* [Bun98b](p. 194). In particular, *such relations among indicators and the corresponding unobservables are postulated by theory and independently checked whenever possible* [Bun98b](pp. 194-195). Ultimately, *indicator hypotheses O=F(U) are not arbitrary: they are hypotheses which upon corroboration and theorification become law statements. In no case they are conventions, for example definitions, as claimed by operationalism* [Bun98b](p. 196).

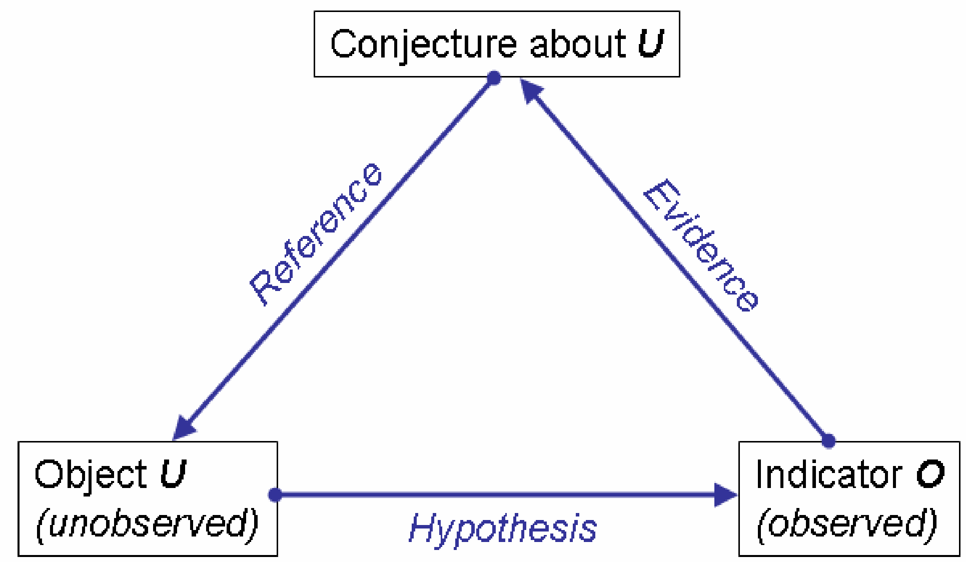

Figure 2: The object-indicator relation, a physical relation, is expressed by a hypothesis enabling us to infer the object from observations made on its indicator [Bun98b](p. 192).

However, as far as computer simulations are concerned, this already complicated web of epistemological relations is further complicated if we take into account that our observable indicator *O* is a non-natural technical artefact, while the immediate un-observed object *U* is now the electronic equipment inside the computer used for the simulations. This immediate *U*, however, is not the *U* which the scientist wants to reason about: the scientist is rather interested in yet another (ultimate) un-observed object *U′* which must now be hypothetically related to the intermediate un-observed object *U*; see **Figure 3** for an illustration.

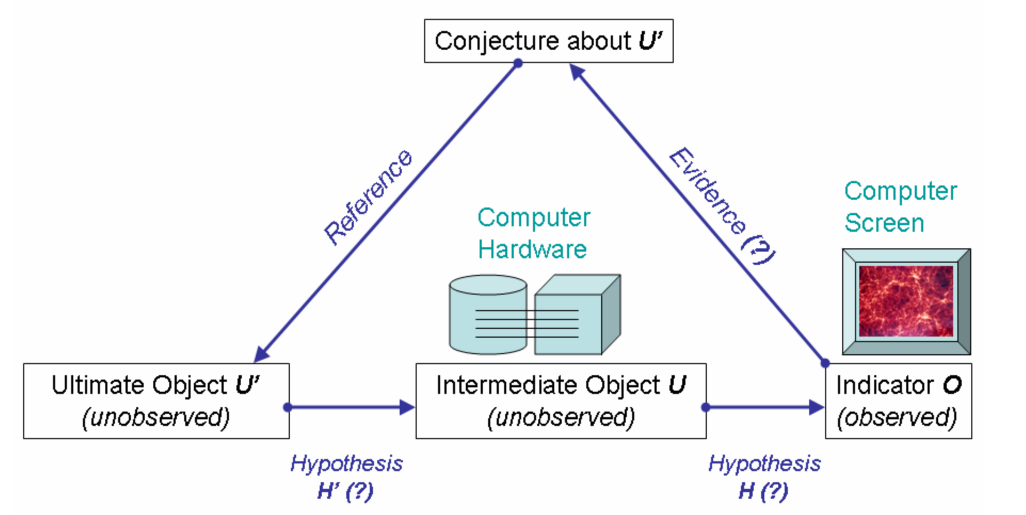

Figure 3: In computer simulations, the indirect relation between the technical artificial indicator O and the scientifically interesting ultimate unobserved object U' are mediated by the internally un-observed computing machinery U, whereby the mediating hypotheses are very difficult to formulate.

Though the situation sketched in **Figure 3** can generally be found in all large-scale apparatus-supported experiments of contemporary physics, the situation is even more complicated in the case of computer simulations. The crucial difference between a laboratory experiment and a computer simulation is that the relevant physical laws governing the laboratory apparatus itself are sufficiently known to the experimenting physicists, whereas the *physical laws governing the internal mechanism of a computer* are

- by-and-large *unknown* to the higher-level scientist (e.g.: biologist or astrophysicist) who is using this computer, and
- by-and-large *irrelevant* to the computer's myriad of behavioural possibilities in its role as a universal semantic machine: By looking at the flow of electrons governed by physical law in the computer's wires we cannot effectively infer what the computer is actually doing at any purposefully intended higher level of semantics: this information-philosophical dilemma, which is related to the 'layered' ontology by Nicolai Hartmann [Har49], was emphasized already by Heinz Zemanek many years ago [Gru16].

This picture illustrates thus why it is so difficult to use computer simulations as trustworthy explanations of anything, though computer simulations are such a helpful tool for the generation or postulation of interesting new hypotheses.

Part of the depicted mediation problem of U between O and U' according to **Figure 3** is the peculiar fact that *explorative software* (i.e.: computer programs intended to reveal hitherto un-seen phenomena) *cannot be specification-tested* as it is prescribed by the classical scientific literature on software testing [AOf08] [Ang14]. In classical software testing, we are given a behavioural specification, S, which describes precisely what the computer program under test, P, is supposed to do, in particular which kind of output P is supposed to produce. This makes it possible to compare the specified (intended) behaviour $b(S)$ with the actual (empirically observed) behaviour $b(P)$, such that the program acceptably passes the test if and only if $b(P) \approx b(S)$ [AOf08]. In 'explorative' software systems, by contrast (which include simulation software), *no* 'desired' output is a-priori known, (otherwise no exploration would be needed at all). In other words: since this type of software is *intended to produce novel phenomena* (for the purpose of generating new hypotheses) which nobody has known or seen before, it is not possible to provide a-priori any normative behavioural specification Se for such an explorative program Pe. All we could vaguely stipulate normatively in such a situation is the inclusion of some particular physical or theoretical set of law-like formulæ, F, the behaviour of which we would like to explore, into the software code of Pe. Consequently, it is not possible either to compare $b(Se) \approx b(Pe)$ as per software-engineering literature [AOf08] [Ang14], simply because no Se is given.

A fortiori it is very difficult to decide whether any surprising or astonishing phenomenon produced by a run of Pe during a computer simulation 'is a bug or a feature', i.e.: whether the observed astonishing phenomenon is real on the basis of the encoded theory F, or whether the observed phenomenon is merely a software artefact due to some mistake made in programming; (a widely accepted 'rule of thumb' in software engineering states that there is on average one 'bug' per 1000 lines of program code). This situation can be considered as a 'technological instance' of the notorious *Duhem-Quine dilemma*. Lacking Se, such a software-technical decision between 'bug or feature' could only be approximated by comparing several independently developed versions ($Pe(1), \cdots, Pe(n)$) of Pe, and then to accept Pe as bug-free if and only if the observable run-time behaviour is found to be $b(Pe) \approx b(Pe(1)) \approx \cdots \approx b(Pe(n))$ for all independently programmed versions. In practice, however, the financial costs of having *n* software systems independently programmed by *n* different software engineers are forbiddingly high. For further details concerning the conceptual connection of the *Duhem-Quine dilemma* with the problems of verifying and validating computer simulations the reader is referred to [JAr19].

Note, nevertheless, that in many large problem areas of contemporary astrophysics this method has become typical and usual: *various independently developed software codes* are regarded as 'reliable' in practice if they lead to similar solutions for similar problems – see for comparison the topic of '*Byzantine* Fault Tolerance' (BFT) in the theory of computing; ndependent development of several different versions of the most critical parts of a system has also been advocated for 'classical' engineering projects [PO+18]. Further noteworthy thoughts about the dependence of the epistemic trustworthiness of computer simulations on sound software engineering principles and software development practices have been published in [New15].

5. COMPUTER SIMULATION BETWEEN GEDANKENEXPERIMENT AND REAL EXPERIMENT

In the following we presume that the reader is already familiar with some typical real experiments from the history of science, particularly from the exact sciences of physics and chemistry. We rely on this background knowledge in the following paragraphs when we contrast computer simulations as well as *gedankenexperiments* with the characteristic features of classical scientific experiments; for further science-philosophical remarks about the methodology of 'classical' experiments see [Bun98b](ch. 14).

Testing hypotheses by subjecting their specific predictions to empirical tests is an essential part of scientific progress. However, if the equations needed to evaluate predictions become too complicated, either by their structure or by the initial and boundary conditions they require, computers are used for solving them. As a specific example we now discuss a scenario concerning the *growth of cosmic structures*.

In the context of the ongoing science-philosophical discourses concerning computer simulations, *astrophysics and cosmology* play a special role because they cannot generally subject their systems of study to laboratory experiments. This has two important consequences: *First*, the phenomena observed in astronomy and cosmology cannot be isolated. All we can do is to observe them in their natural setting and to try and separate essential from less essential or irrelevant aspects of the observations. *Second*, the phenomena of astronomy and cosmology usually cannot generally be repeated: they belong to the *'history of nature'* and are thus located at the intersection of what Windelband had classified separately as 'nomothetic' versus 'idiographic' [Win98]. The only way to suppress measurement uncertainties and fluctuations is to observe many different realisations of what is supposed to be the same physical system or process, and to assume that an average over different observed systems would be be equivalent to an average over independently prepared systems, if they existed.

Nevertheless, the physical model underlying cosmology and cosmic structure formation is simple and well defined: material particles move in a space expanding with time. In the simplest rendition of the model, the particles interact with each other through gravity only. This implies that the equations governing the particle motion are structurally simple. The complication arises by the very large number of test particles the calculation needs to follow in order to return a credible picture of cosmic structures.

Within narrow limits the initial conditions are set by observations. The boundary conditions are usually chosen to be periodic, i.e.: opposing sides of a cubical simulation volume are identified. With these rather minimalistic choices the simulation can begin. It results in a remarkably complicated pattern of cosmic structures which reveals properties that seem to be universal in the sense that they appear independently of the specific set-up and scale of the simulation. Further detail can be found in a large body of expert literature [FB+10] [GG+15] [MA+12] [RD+06] [RD+15] [SB+95] [SF+06] [Spr12].

Plain as it seems to be, the essence of this example is quite prototypical. The simulation begins with a simple state and implements a small set of simple physical laws. Yet, often surprisingly complicated systems and structures emerge in the course of the simulation. Without the simulation the consequences of the interaction between very many particles could not have been worked out in detail within a reasonable amount of time. Simulations thus allow studying systems whose fundamentally governing laws may be quite simple, but whose behaviour is complicated by the interplay of the many constituents the system comprises. In other cases the system may have much fewer degrees of freedom governed by quite complicated equations which cannot otherwise be solved. In either case the system's behaviour is complex either because the underlying model is or because many constituents interact.

Since the late 1980s simulations of cosmic large-scale structures were carried out on powerful *supercomputers;* see [GGr18] for a general science-philosophical discussion of the capabilities and limits of such devices. Since the early 1990s observations by the COBE, WMAP and Planck satellites have empirically fixed initial conditions for any subsequent computer simulations in this field. Images such as the one shown in Figure 4 are the result of such computer simulations which aim at mimicking the evolution of structures in the universe which were by-and-large dominated by two effects: *the simultaneous collapse of individual lumps of matter and the expansion of the universe which separates the lumps. The result is a complex, filamentary structure, with regions of high density stretching in streamers and sheets across the universe, surrounding large empty voids. According to theory, this kind of structure should have been imprinted on the universe by the time it lit up* [GGo98](p. 32).

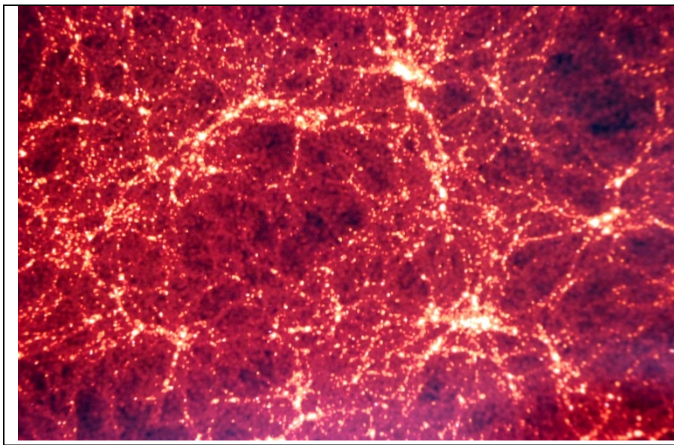

Figure 4: Result from a simulation of the large-scale matter-structure of the early universe, taken from [GGo98] (p. 33).

The reader should note the semantic difference between '*kind of* structure' and 'this structure'. Also note that the ultimate test of the theory is not the computer simulation itself, but rather the comparison of its result with the observations. From a science-philosophical point of view the following issue seems particularly important: As the observations cannot be identical with the computer-generated images like Figure 4, astrophysicists must use an intuitive and extra-theoretical hermeneutics of the notion of being sufficiently similar in order to decide whether such a computer-generated picture could be accepted as a scientific model of the cosmic structures. Statistical measures, especially correlation functions, are used for this purpose. In other words: we can find here an example of *analogy-supported reasoning in modern science*. This is philosophically remarkable because analogical thinking is typically regarded as a feature of pre-modern science which modern science wanted to discard as far as possible. A deeper look into recent publications on astrophysical simulations reveals the following typical and science-philosophically interesting points:

- Simulations are run to estimate and to predict rather than to explain –for example: it will thus be interesting to study the predictions of our model in more detail [SHe03]– whereas the authors of [XS+16] use the Illustris hydrodynamical simulations to estimate the amplitude of this bias, and to understand how it is related to observational properties of galaxies. In science-philosophical terms, these are hypothesis-generating research activities.
- Simulations are considerably idealised. For example: *for the purposes of this work, we neglect uncertainties* [SHe03]. These idealisations are not always motivated by nature-physical considerations alone. Rather, the capacity limits of the simulation computer can artificially enforce simplifications merely to make a simulation computable within an acceptable duration of runtime: *In this regard, an important question for comparing numerical methods is their computational efficiency for a given accuracy, or conversely, what is the best numerical accuracy which can be obtained for a given invested total runtime* [BS+16].
- The values of those simulation parameters which are not neglected will be constrained on the basis of underlying theoretical-hypothetical assumptions. For example: *we argue that the effective pressure of the gas should be a continuous function of the density at the onset of the regime of self-regulated star formation* [SHe03]. Thereby, because of the intrinsic non-linearity of many natural phenomena, even subtle differences in the underlying models can lead to significant discrepancies in the corresponding simulation runs. In extreme cases, even physical falsities can emerge as results of such simulations. For example: *point-like energy injection can lead to unphysical negative temperatures in the standard formulation of SPH* [SHe02].
- On top of the theoretical astrophysical layer of the simulation sits a mathematical-technical layer which can possibly influence the results of the envisaged simulation. In such cases, any choice of numerical

representation must be defended rationally-philosophically at a methodological meta-level with arguments from outside the physical object realm. For comparison see [Spr10] wherein several numerical techniques and their properties are compared and methodologically discussed.

- When the phenomena to be simulated are identified as composite or multi-layered, different simulation techniques with their own assumptions and constraints are applied to the different components or layers of the whole scenario. For example: *the mass fraction contained in cold clouds at a given density can be obtained with good accuracy by just assuming the equilibrium value expected for self-regulation... We can thus replace the explicit treatment with a simplified method based on these equilibrium values* [SHe03].
- Wherever stochastic processes are to be simulated, the quality of the simulation results depends strongly on the statistical quality of the utilised pseudo-random number generators, because Turing-equivalent computers are deterministic machines.
- Where similar processes occur in different sub-domains of nature, it is at least in principle possible to apply the same algorithmic implementations for computer simulations of those sub-domains or problem classes. This possibility is due to some laws of nature being valid for a large variety of macro systems regardless of the microscopic laws governing the internal degrees of freedom [Ta015](p. 6). However, if new algorithms for such problem classes are developed, it is mandatory to calibrate these for the sake of consistency with regard to previously known and accepted results within those problem classes. Such calibrations often use well-known borderline cases which are amenable to pure analytical treatment.
- The explanatory power of computer simulations is allowed to be rather low if those computer simulations are carried out in an explorative mode of research. For example: *due to the nature of the approximations made, it is clear that our model is still phenomenological to a large degree* [SHe03].

Those points we regard as *typical for all* computer-simulations, specifically also in the domain of astrophysics. They show that such simulations are particularly helpful in the generation of new hypotheses and in the derivation of the theoretical consequences of hypothetical assumptions:

- *Semi-analytic models of galaxy formation (...) have proven to be a very powerful tool for advancing the theory of galaxy formation, even though much of the detailed physics*

> *of star formation and its regulation by feedback processes has remained poorly understood [SW+05].*

Since a computer system including its software is 'closed', astrophysical simulations characterised above cannot by themselves experimentally test any theory which is program-coded into them. Yet, they can be used to generate clues and expectations with regard to later external empirical observations:

- *Testing this model requires that the precise measurements delivered by galaxy surveys can be compared to robust and equally precise theoretical calculations [SW+05]. Only those cases are considered trustworthy in which the simulation reproduces extremely well numerous observational properties of real galaxies [XS+16].*

From an epistemological point of view, due to the multiple *Duhem-Quine dilemma*, the explanatory power of those astrophysical simulations is limited. This is corroborated also by the assertion cited above that computer-simulations are used to advance theory-building [SW+05] whereas an accepted theory is needed in order to produce an acceptable explanation [Bun98b]. For example:

- *The principal problem remains: what is the possible range of realistic mass models? In other words, if a (...) model κ fits the data, how can one rule out that the corresponding (transformed) model $\kappa\lambda$ (...) is not realistic? [XS+16]. Moreover, the accepted fact that real galaxies most likely exhibit a fairly large range of density profiles [XS+16] makes the classification of model profiles into realistic and less realistic ones fairly difficult, if not impossible [XS+16].*

Such an undecideability between realistic or less realistic necessarily imposes limits on the explanatory power of those kind of computer simulations.

Untersteiner has described in [BH095](pp. 135-138) how *matter-matter simulations* had already been conducted long before the first computer-matter simulations: Already in 1902, Birkeland and Tevik had used an electromagnetic metal ball called 'Terella' to reproduce visible effects similar to the aurorae observable near the North- and South poles of the Earth. Thereby the metal ball served as a tangible physical small-scale *model* of our planet. In 1951, Fultz used rotating bowls filled with liquid to produce atmospheric effects resembling the stable jet-streams observable in the sky above our planet [BH095](pp. 135-138). Thus the question arises: Can we identify any science-philosophically relevant differences between our computer-matter simulations of nowadays and those early matter-matter simulations? *Two differences* seem to be particularly relevant:

- *In matter-matter simulations it is easier (i.e.: prima facie more plausible) to conjecture hypothetically that similar observable effects may have similar causes* (in the physical sense of causation) under the presumption that all matter is subject to the same universal natural laws. The actually observable phenomena produced by the 'Terella' model ball could have been produced only in this one way in which they had actually been produced.
- *A matter-matter simulation* (such as Fultz's rotating liquid) *can be constructed in an attitude of 'functionalism'*, i.e.: without the need for deep theoretical assumptions about the natural cause-effect relations which are at work behind the scenes of such scenarios. Fultz was thus able to reckon in a rather straightforward or 'common-sense' manner:
    - our Earth consists to a large extent of water → therefore use a bowl with liquid;
    - our Earth is rotating → therefore let the bowl rotate, too;
    - our Earth is warm around the equator → therefore heat up the liquid at the rim of the bowl;
    - our Earth is cold at its polar caps → therefore cool down the liquid in the centre of the bowl.

  In both examples, *tangible* models (as opposed to merely theoretical thought-models) are the indispensable basis of matter-matter simulations. Computer simulations, on the other hand, cannot be designed in such a straightforward and common-sense manner because the computer has the potential of nearly 'infinitely' many different behaviours. Fultz's water bowl can only behave in the way it actually behaves. Natural constraints do not allow it to behave in infinitely many different ways. By contrast: in order to make a computer simulation right and plausible, it is we who have to 'tell' the computer (by means of programming) how to 'behave properly', which requires deeper theoretical knowledge about what may be accepted as 'properly'. The algorithms, which drive the computer in a 'credible' and plausible computer simulation, are formally encoded representations of already existing hypotheses about the natural laws which supposedly govern the natural phenomena which the computer simulation is about to simulate. Fultz with his rotating bowls, by contrast, did not need any such theory to successfully conduct his matter-matter simulation in a 'functionalist' attitude.

From this comparison we may conclude:

- Whereas a computer-matter simulation can be used to explore the hitherto unknown implications and consequences of an already existing theory, it is not credibly applicable in the absence of any such theory, particularly because of the Duhem-Quine-related dilemma that many different computer programs could potentially generate the same visible output on the computer screen.
- On the basis of the presumption of the universal validity of natural laws, a matter-matter simulation, on the other hand, can be plausibly conducted even in the absence of any law-like hypotheses about the effective causal mechanisms, and is thus better suited for stimulating the initial search for effective causes in the early phases of scientific investigations when only little theoretical knowledge is available.

A *gedankenexperiment* is an exploration of logical consequences on the basis of general theoretical presumptions and additional situative specifications. It typically presumes that one specific situation can only have one specific lawful-logical consequence. Popper added a methodological chapter on the topic of 'Use and Mis-Use of *gedankenexperiments*' [Pop35](New Appendix: ch. *XI, pp. 397-411) to one of his later revised editions of *Logik der Forschung*. He distinguished between 'critical' and 'heuristic' uses of *gedankenexperiments* [Pop35](New Appendix: p. 398) and postulated that the disputative use of a *gedankenexperiment* is justifiable only if the underlying abstractions and idealisations are acceptable from the viewpoint of the scholarly opponent against whom the disputative *gedankenexperiment* is directed [Pop35](New Appendix: p. 399). Though many computer simulations are conducted merely in an explorative mode, there are also disputative uses of computer simulations in science-politically contested areas, for example in the context of climate change. In such cases, Popper's normative-methodological demands concerning the proper use of *gedankenexperiments* are applicable to computer simulations, too. However, whereas in ordinary thinking the imagined consequences of imagined situations or actions in an imagined space are known to the thinker only tacit-intuitively on the basis of precedent life-experiences, they are known in a *gedankenexperiment* explicitly and theoretically on the basis of a regulated deductive calculus. This point is illustrated further by the following example of a historically well-known *gedankenexperiment*.

One of the least acceptable consequences of quantum mechanics, at least in its most widely accepted interpretation, is that quantum mechanics gives up the claim to describe physical entities as such, i.e.: as parts of an objective physical world. Rather, quantum mechanics limits itself to predicting the probability for the results of future experiments. This radical turn from classical physics was intolerable even for some of the most prominent physicists at the time, Einstein among them. Together with Podolsky and Rosen, Einstein devised a *gedankenexperiment* intended to sharpen the arguments against the consequences of quantum mechanics, leading to what is known as the Einstein-Podolski-Rosen paradox (EPR). This famous *gedankenexperiment* was devised to demonstrate that quantum mechanics, at least in its common interpretation, is incompatible with Einstein's theory of Special Relativity, because that theory forbids any information to travel faster than the speed of light. In a letter (1935) to Popper –see Figure A– Einstein himself has further explained and clarified some crucial points of the EPR, and Popper has subsequently added several entirely new sections about this EPR topic to a later revised edition of his *Logik der Forschung* [Pop35](New Appendix: ch. *XI: pp. 399-406, ch. *XII: pp. 412-418).

The resolution of the paradox, however, lies at the conceptual heart of quantum mechanics. Regardless of their distance, the two electrons in question are still described as one quantum-mechanical state. Quantum-mechanical states allow the prediction of probabilities for the possible results of future measurements. In contrast to classical mechanics, they do not claim any more to represent a physical reality beyond its concrete empirical manifestation. The EPR paradox, intended to shake the foundations of quantum mechanics, could thus be turned into a helpful tool for further clarifying and sharpening the theoretical concepts of quantum mechanics and their interpretation. The EPR *gedankenexperiment* has thus made a highly valuable contribution to the clarification of the language of science.

Thus, the deep-exploration of the logical consequences of already given theories can lead to an unforeseen and hitherto unknown surprise, which then motivates further research with fruitful results. This is what we regard as the essence of every genuine *gedankenexperiment*. Whether computer simulations, *gedankenexperiments*, can also make contributions to clarifying the language of science is an *open question* at this point.

Without particular emphasis of the computer as a technical device or tool, Bunge stated in his philosophy of science that there must be a *systemic relation* between *models*, *simulations*, and *gedankenexperiments* [Bun98b](p. 298). Though we agree to a large extent, this contribution serves to further qualify Bunge's statement and to re-adjust it within our more recent contemporary perspective. Especially as theoretical astrophysics is not an applied science, and since computer simulations indeed play an increasingly important role in theoretical astrophysics, too, Bunge's early and rigid categorical association of computer simulations with the applied sciences (including technology and engineering) might be regarded either as dogmatic or historically premature.

Related to the notion of *gedankenexperiment* is the concept of computers or algorithms being 'think-equipment' (*Denkzeug*) [Vol95]. Though this characterisation of algorithms is already quite old, it only begins to turn out nowadays that those old characterisations of algorithms as *Denkzeug* fit so well together with our new characterisation of computer simulations as technically and algorithmically implemented *gedankenexperiments*. In the 6th chapter of Vollmer's book on modern philosophy of nature [Vol95], its author placed an entire section under the heading "Algorithms are *Denkzeug*" and explained how algorithms are used to carry out (at least partly) the 'labour of thinking' or 'think-work' (*Denkarbeit*) which the un-equipped human 'computer' would otherwise have to do all by himself [Vol95](p. 142). *A fortiori*, any Turing machine is *Denkzeug*, too, namely because of the conceptual equivalence of Turing machines and algorithms [Vol95](p. 152). Other authors have expressed similar thoughts about the nature of algorithms, too [Bau72] [Coy93] [Nak92].

In this context we briefly remark that there is a deep connection between algorithms and thoughts via the medium of *language* in which both of them are expressed. Not only our algorithms but already the language behind them can be characterised as *Denkzeug* in the above-mentioned sense. In fact already in the 1st third of the 20th century, long before any computer simulations became possible, no lesser man than Heidegger himself, in *Sein und Zeit* (Being and Time), has characterised language itself as *equipment* (*Zeug*), namely "Symbol-*Zeug*" (semiotic or symbolic equipment) in several paragraphs of his book [Hei27](§17: pp. 76-82, §23: p. 108).

In summary, the foregoing philosophical considerations support the characterisation of computer simulations as *machinised gedankenexperiments*, whereby not only the electronic computing machinery (hardware) but already the (algorithmic) formal languages *Zeug* (in Heidegger's sense of the term) which is needed for the implementation of such machinisation.

6. COMPUTER SIMULATIONS AND HISTORIC PROCESSES

Given a physical process *P* to be computer-simulated, it has been argued by some philosophers, for example Longo [BHo95], that a computer process *S*, to be acceptable as a genuine simulation of *P*, must accurately reflect (or 'mimic') the internal structure of *P*'s inner events and sub-events. More formally:

- Let $P = E(0), E(1), E(2), \cdots, E(n)$ be a sequence of distinguishable *physical* events.
- Let $S = E'\_(0), E'\_(1), E'\_(2), \cdots, E'\_(\mathrm{n})$ be a sequence of distinguishable *computational* events.
- Then, according to these philosophers, *S* is a simulation of *P* if and only if there exists an isomorphism, $I: P \leftrightarrow S$, such that for all i in $\{0,1, ..., n\}$: $E'(i) = I(E(i))$ is a simulation of $E(i)$.

Many practising scientists behold their computer programmes as 'simulations' even though they do not meet this criterion of isomorphism. For example: the well-known Monte-Carlo simulations (MCS) are often applied in practice though 'purists' would not accept them as a proper 'simulations' because they are essentially black-box processes with only two distinguishable events (input and output) which cannot be isomorphically mapped onto any physical process which is composed of more than two distinguishable sub-events. This illustrates a peculiar discrepancy in the conceptual notions of simulation held by practicing scientists and by philosophers of science. An even more severe problem arising from the criteria stated above is their circularity: They define a simulation relation between *P* and *S* on the basis of simulation relations between all $E(i)$ and $E'(i)$, without explicating the notion of simulation on the smaller scale between all $E\_(i)$ and $E'\_(i)$. The science-philosophical question, as to what a simulation really is, was thus merely shifted from a larger (more coarse-grained) to a smaller (more fine-grained) ontological scale. In addition to this obvious problem, however, another and more subtle philosophical problem arises, too: it is related to questions concerning the *flow of time* and the issues of *historical periodisation* [Pot99].

To our knowledge nobody has so far discussed in publications the relation of the above-mentioned problem with the history-philosophical problem of periodisation [Pot99]. The most elementary and shortest possible 'epoch' in the 'history' of a digital computer simulation is one 'tick' of the computer's discrete internal clock. Now, if we would presume for the sake of argument (together with Leibniz and Newton) that that our physical reality is continuous, then it would never be possible to produce computer simulations in the field of physics if the isomorphism criterion for `simulation' would be rigorously imposed. We have to conclude from these considerations that proper computer simulation relations between a physical process P and a corresponding computational process S are almost inevitably characterised by a non-isomorphic mapping scheme between simulations and reality. Like historians 'cluster' the continuous 'flow of history' into 'epochs' by means of historical narration with 'suitable' categorisations, a computer simulation can represent a flurry of concrete physical events in coarser clusters of computational events (at a higher level of conceptual abstraction) and still remain a acceptable simulation. It should also be obvious that any 'historically complete' computer simulation of the entire known universe in all its details would be limited by the maximal computational capacity of the universe itself, because computation is physical and requires energy [Llo02]. Ultimately, however, the historically complete computer simulation of the entire universe would run into the paradoxical situation of having to simulate the simulating computer itself. Thus, also Lloyd's extremal energy-physical considerations on the maximal computational capacity of the entire universe [Llo02] provide us with much reason to believe that the 'history' of a computer simulation is a more-or-less incomplete 'story' which can never be matched perfectly with the physical flow of reality outside the simulating machine.

From an epistemological point of view, computer simulations are motivated by the desire to 'learn' something about that part of reality which exists outside the simulation. The epistemologically most extreme opinion would assert that nothing at all can be learned about the external reality from a computer simulation. But as we reject such null-extremism, the more general philosophical question arises: How much could we maximally 'learn' about reality by conducting a computer simulation of reality?

The philosophical relationship between this question and Laplace's determinism was noted already by several authors [Coy93] [Pöp15a] [Zem93]. Indeed, the 'worlds' of *Laplace's Demon* and of computer simulations are (at least ideally) the same world. *A fortiori:*

- The computational and epistemological limits of the world are also the computational and epistemological limits of the world of all computer simulations.
- The computational and epistemological limits of *Laplace's Demon*, which have already been analysed by many scientists and philosophers, are also the computational and epistemological limits of the world of all computer simulations.

Thus: though the most general *gedankenexperiment* of *Laplace's Demon* is as unfeasible in reality as the research program implied by Wittgenstein's atomistic *Tractatus Logico-Philosophicus* [Zem93], its epistemological value can be found in hinting at an absolute limit, which certain other types of *gedankenexperiments*, such as computer simulations, will never be able to overcome.

An important function of science is the conceptual development of new scientific terms –the 'vocabulary' which scientists use to reason about new ideas and new experiences– such that we cannot ignore in this contribution the question whether or to what extent computer simulations can possibly support this important scientific function. According to the historian of science Thomas Kuhn this business of creating new terminological concepts is so important that the establishment of a new conceptual framework (a 'paradigm') during the course of history can even produce the effects of a 'scientific revolution' [Kor14]. Even before Kuhn the historic development of scientific terms and their slowly 'shifting' meaning was already discussed by Ludwik Fleck [Fle35]. Popper, too, has asserted that scientists cannot talk in any rigid language system which would be semantically invariant in spite of the development of new theories [Pop35](rev. ed., p. 90: new footnote *3). Consequently we also need to ask whether computer simulations can help us to arrive at new concepts and theoretical terms for the language of science. In this context, however, we have not (yet) been able to find compelling empirical evidence or rational philosophical arguments supporting the idea that the design or the execution of computer simulations might be strongly connected with the widening of scientific vocabularies or with noteworthy semantic shifts within the words of an already existing scientific vocabulary.

7. CONCLUSIONS AND OUTLOOK

Based on the preceding discussion we can now summarise and answer *eight questions* of science-philosophical relevance with regard to computer simulations, in particular in the field of contemporary astrophysics. Thereafter we finish this contribution with a few more general remarks which may open the door for a continuation of this discourse; (an appendix follows behind the literature references).

Under which circumstances will computer simulations have large 'explanatory power' (instead of merely being interesting 'games')? A scientific model, on which scientific computer simulations are based, has large 'explanatory power' if it is amenable to methods of falsification. A 'failure' (i.e.: a successful falsification) of such a model can help us to identify those 'elements' and properties of a system which are especially important and relevant for its behaviour. Highly complex models, however, with too many variables and parameters, suffer strongly from the notorious epistemological Duhem-Quine dilemma in cases of their empirical falsification: in those cases it is almost impossible to determine which ones of the many variables and parameters were particularly 'responsible' for the empirically observed 'failure'. Highly detailed computer simulations are meant to provide a 'maximally' precise and accurate image of reality. Their 'artificially' computed phenomena in their variety and subtleness do thus not differ very much from the multi-faceted 'real reality' any more. Whilst such a strong similarity between a highly detailed simulation and its simulatum might prima-facie 'look nice', it is in fact no longer possible to 'learn' anything about reality from the underlying simulation-models, because only by 'contrast and difference' can we identify those ones of the model's many parameters which are genuinely salient and relevant for a genuine theoretical-scientific 'understanding' of the empirically observed phenomena. A mere 'deluge of data', as propagated by some radically 'instrumentalist' advocates of the so-called 'data science' [And08], would indeed be the 'death' of science and of our historically hard-won scientific world-view. At this point we must remind ourselves of Einstein's famous aphorism, according to which we have to make our models "as simple as possible however not simpler", in order to produce 'realistic' computer simulations which have, nonetheless, scientifically sufficient and satisfactory 'explanatory power'. A computer simulation which is not amenable to any method of falsification might well be 'intellectually stimulating' or 'thought-provoking' but is epistemologically worthless.

Of what type are the typical astrophysical insights which are sought by means of computer simulations? The global physical properties of the universe are captured by an astonishingly simple cosmological framework model which describes the mean densities of matter and energy together with the time evolution of the universe with a small set of six parameters. The theoretical framework of simulations of cosmic structure formation as well as their initial conditions are thus tightly constrained. The precise questions to be addressed with the simulation results are thus: Can the amplitudes of today's density fluctuations be reproduced? How can the coherence of the cosmic structures be reproduced, in particular the amplitude and the scale of density correlations? How do the structures grow and develop in time? How many structures per volume are seen to form in the simulations which are gravitationally bound? How does their abundance vary with their mass, and how is the matter distributed within these bound structures? For all these questions, precise observations exist nowadays. The simulations thus serve a typical purpose of experiments because they test under well-defined physical circumstances what the essential physical input is for a well-defined set of observable properties to be reproduced.

How, and at what coarse or fine level of abstraction, are those simulations typically carried out? The setup of the simulations and their procedure is quite straightforward. The cosmic density field is decomposed into artifical particles, which represent imaginary, discrete packages of the cosmic material and could thus also be called pseudo-particles. These pseudo-particles are chosen for the simulation to achieve the required spatial resolution (granularity of modelling) with the available computational resources. At the beginning of the simulation, a position, a velocity and a mass are assigned to each of the pseudo-particles. Their dynamics is then described by an equation of motion, which sets the acceleration of a pseudo-particle equal to the force acting on it. For a cosmological simulation, the force acting between particles is mostly taken to be Newton's gravitational force acting between one pseudo-particle and all others. Since the total number of mutual interactions of pseudo-particles may be forbiddingly large even for powerful computing machinery, approximate methods of including the gravitational interactions are then applied.

Does the simulation software encode the relevant laws of nature accurately and precisely, or are simplifications introduced and encoded for merely computer-technical reasons? Even though the applicable laws of nature underlying those simulations are already quite simple in and by themselves, the gravitational force law has to be modified in those simulations to avoid arbitrarily large accelerations of pseudo-particles coming arbitrarily close to each other. The gravitational force law must therefore be softened to keep the gravitational force finite even for very close encounters between two simulated bodies.

What are the typical outputs of such computations, and how do the astrophysicists interpret the meaning of those results? The result of simulations like this is the spatial distribution of the pseudo-particles in the cosmological simulation volume at a later time, typically much later than the initial time and close to a time corresponding to the present epoch in the evolution of the universe. Since each pseudo-particle represents a finite matter package, the spatial pseudo-particle distribution is a discrete representation of the smooth matter distribution in the universe. If the pseudo-particle masses have been chosen sufficiently small compared to the smallest structures to be studied by the simulation, then the discreteness of their distribution is irrelevant. Those structures cannot directly be compared to the natural reality, because most of the matter in the universe is dark and thus cannot be seen directly [DLi15]. Therefore, the simulation results need to be post-processed in a way expected to represent the physical mechanisms responsible for matter to emit light. For example, post-processing criteria need to be applied in order to populate the simulated cosmological structures with the objects representing the galaxies observed in the real universe. By varying these criteria and comparing the resulting galaxy distribution in the simulation with that seen in the universe, information can be obtained on the actual galaxy formation process.

Can the obtained results be anyhow astonishing or surprising, or do they merely make 'visible' what was already 'implied' by the simulation's underlying program code? Cosmological structures on the largest scales resemble a network of filaments and two-dimensional matter sheets: see **Figure 4** for comparison. The appearance of such structures had already been expected from analytic calculations way before they were seen in the first simulations and later also in the physical universe. Nonetheless, once the simulations reached a sufficiently fine spatial resolution, they revealed that the radial density profiles of gravitationally-bound lumps of matter attain a universal form, quite independent of the mass of the object. This universal density profile has meanwhile been confirmed observationally. Its origin in fundamental laws of physics is entirely unclear. This result did, indeed, come as a surprise.

What is typically said to be learned from such computer simulations? The numerical results taught cosmologists many details about the structure formation process during the evolution of the universe. Perhaps one of the main lessons learned was however that large ensembles of particles, even if they interact by very simple physical laws, can form morphologically complicated structures revealing universal properties. Largely driven by simulations like those, the emergence of structure in systems with many degrees of freedom interacting by very simple physical laws has given rise to the development of analytical methods for improving the understanding of many-body systems and for finding fundamental explanations for their collective behaviour.

Were the initial anticipations or expectations met by those simulations, or were they considered disappointing, and if so, why? When running conceptually simple simulations like these, it is often expected that phenomena observed in nature could be reproduced once the relevant physical laws were incorporated into the simulation software and once its parameters had been appropriately chosen. This expectation is often fulfilled. More importantly, however, phenomena often appear that had not been foreseen. Disappointment usually occurs when the results of a simulation are inconclusive. This routinely happens when the variety of physical processes included in the simulation is so diverse that observed phenomena can no longer be uniquely related to known physical mechanisms. This situation is related to the notorious Duhem-Quine dilemma.

Concluding this survey contribution we would like to propose the following *'theses' for further science-philosophical discussion:*

- Theory and practice of *computer simulations do not transcend the conceptual and methodological boundaries of classical-modern philosophy of science.* It is well possible to understand the principle and the practice of those computer simulations with the already established terminology [Bun98a] [Bun98b] of classical-modern philosophy of science.
- *Algorithms are Denkzeug* ('think-equipment') in analogy to Heidegger's notions of 'tool' (*Werkzeug*) and 'equipment' (*Zeug*) in [Hei27], and so are

(*a fortiori*) the algorithms deployed in computer simulations. Being *Denkzeug*, algorithms can help us well to systematically process much larger quantities of data and information in a much shorter period of time than what we would be able to do without such *Denkzeug* [Bau72], though there is *no difference in principle;* (indeed, when Alan Turing designed his conceptual model for the all-computing Turing Machine, he still had in mind the 'human computer' in the role of an office-clerk). In other words: algorithms can only do what also we ourselves could do, too, albeit only very slowly and in a very long period of time. This is a corollary to the widely accepted Church-Turing conjecture at the methodological and science-philosophical basis informatics [Vol95](p. 151).

- Consequently, *computer simulations are not more and not less than instrumentally supported gedankenexperiments*, whereby it should be noted that the usual definition of the term *'gedankenexperiment'* already refers to the notion of simulation, namely a simulation of an experiment 'in the mind'.
- Thereby, the device-enabled acceleration of thought-velocity in computer simulations is undeniably comfortable from the practical perspective of scientific research-work, though it is not philosophically essential: beheld from a 'sub specie aeternitatis' point of view it does not matter when a correct computational result emerges, as long as it eventually emerges at all.
- In this context, computer simulations can also be regarded (and used) as 'enablers' within *gedankenexperiments*, such that particular steps of a larger over-arching *gedankenexperiment* are carried out in an automated manner. If those steps cannot be done in any other than the computer-supported way then also their corresponding *gedankenexperiment* as a whole cannot be done to completion. Practically, computer simulations thus play the important role of *technical accelerators* of theoretical *gedankenexperiments:* as such they are *also* a topic for the *philosopy of technics and technology*, (and not only for the philosophy of science).
- In this sense, *computer simulations are indeed opening new spaces of thought and research*, though they cannot create by themselves any new hypotheses (nor theories), and are in principle subject to the same constraints and limitations by which all *gedankenexperiments* are restricted in general.

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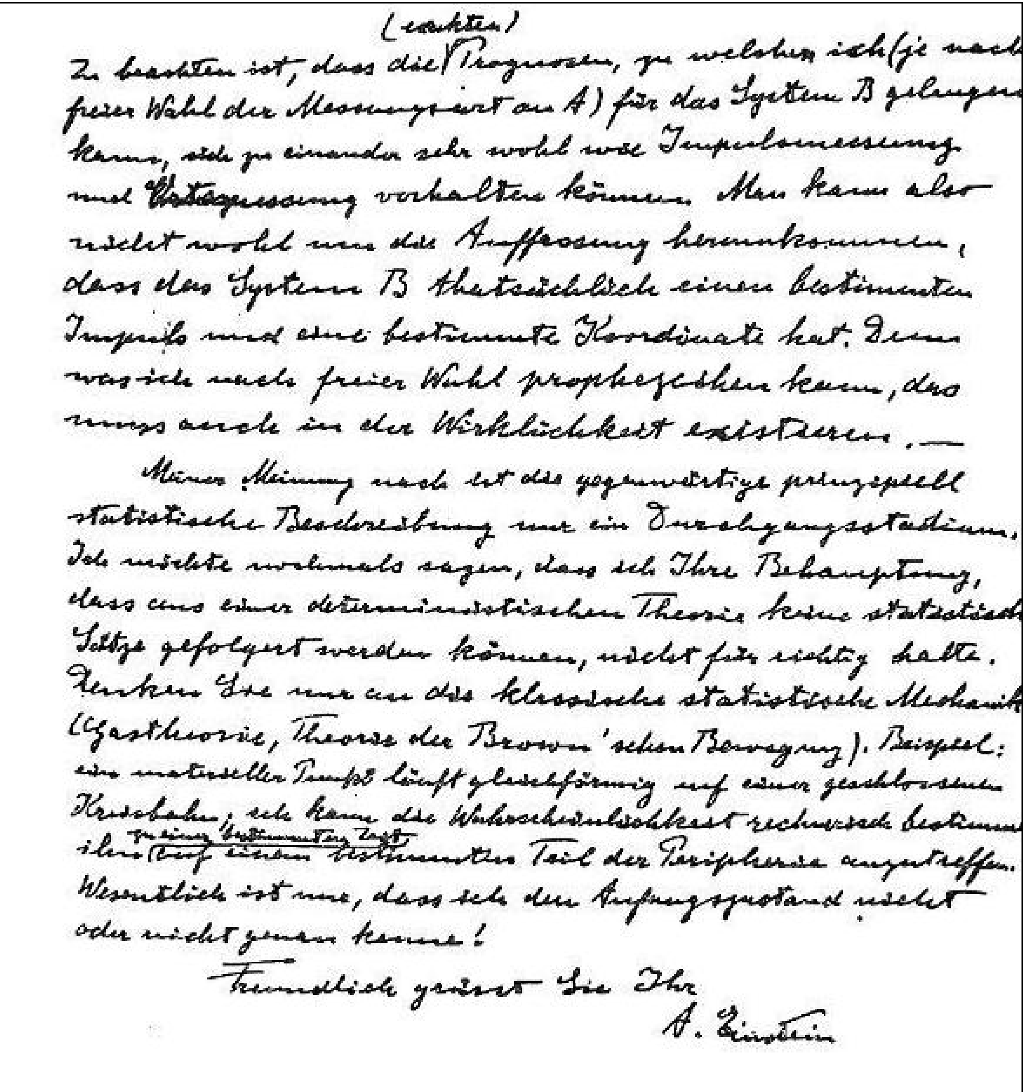
APPENDIX

Letter from Einstein to Popper: in 1935, Einstein wrote a letter to Popper in which Einstein clarified some issues concerning the Einstein-Podolski-Rosen *gedankenexperiment* (EPR). Figure A shows an excerpt of that letter which can be found in its entirety in [Pop35](New Appendix: ch. *XII, pp. 412-418).



(erachten)

Zu beachten ist, dass die Prognosen, zu welchen ich (je nach freier Wahl der Messungsart an A) für das System B gelangen kann, sich zu einander sehr wohl wie Impulsmessung und Ortsmessung verhalten können. Man kann also nicht wohl um die Auffassung herumkommen, dass das System B thatsächlich einen bestimmten Impuls und eine bestimmte Koordinate hat. Denn was ich nach freier Wahl prophezeihen kann, das muss auch in der Wirklichkeit existieren. —

Meiner Meinung nach ist die gegenwärtige prinzipiell statistische Beschreibung nur ein Durchgangsstadium. Ich möchte nochmals sagen, dass ich Ihre Behauptung, dass aus einer deterministischen Theorie keine statistischen Sätze gefolgert werden können, nicht für richtig halte. Denken Sie nur an die klassische statistische Mechanik (Gastheorie, Theorie der Brown'schen Bewegung). Beispiel: ein materieller Punkt läuft gleichförmig auf einer geschlossenen Kreisbahn; ich kann die Wahrscheinlichkeit rechnerisch bestimmen, ihn zu einer bestimmten Zeit auf einem bestimmten Teil der Peripherie anzutreffen. Wesentlich ist nur, dass ich den Anfangszustand nicht oder nicht genau kenne!

Freundlich grüsst Sie Ihr

A. Einstein

Figure A: Excerpt of a letter (1935, in German language) from Albert Einstein to Karl Popper [Pop35](New Appendix: ch. *XII, pp. 418), in which Einstein explained some issues concerning his EPR *gedankenexperiment.*