"Getting the advantages of theft without honest toil: Realism about the complexity of (some) physical systems without realist commitments to their scientific representations"[1]

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Warning. While I try to defend in this paper a realist claim within general philosophy of science, the argument relies on basic but important notions in computer science. I did my best to write a self-content paper and present the argument and ideas in a non technical way so that the paper be accessible for philosophers with no training in computer science. In any case, all comments are welcome!

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Abstract

This paper shows that, under certain reasonable conditions, if the investigation of the behavior of a physical system is difficult, no scientific change can make it significantly easier. This impossibility result implies that complexity is then a necessary feature of models which truly represent the target system and of all models which are rich enough to catch its behavior and therefore that it is an inevitable element of any possible science in which this behavior is accounted for. I finally argue that complexity can then be seen as representing an intrinsic feature of the system itself.

1. Introduction. The purpose of this paper is to show that, under certain reasonable conditions, if the investigation of the behavior of a physical system is difficult, no scientific change can make it significantly easier. This can be seen as some sort of impossibility result that says that some epistemic situation (in which investigating a system could be difficult for some agents and easy for others) cannot be met. It thereby shows that complexity is necessarily a feature of the model(s) (whatever it (they) turn(s) out to be) that truly represent(s) the target system, and of all models that are rich enough to catch its

[1] I am grateful to John Norton, Hervé Zwirn, Jacques Dubucs, Mikaël Cozic, Alexei Grinbaum and Mauricio Suarez for old or recent discussions about various versions of this argument. Remaining shortcomings are mine.

behavior. The complexity of the model can then be seen as representing an intrinsic characteristic of the system itself.

Though the idea supporting the claim is quite straightforward, it needs to be spelled out with great care since the validity of the argument hinges on its details. I first sketch this general idea in section 2 and present the thrust of the argument. Sections 3-5 are then devoted to the discussion of the steps and scope of the argument. I argue in particular that, in the described situations, complexity can be claimed to be an inevitable feature of any (mathematically possible) investigation of the corresponding systems.

2. Statement of the Problem and Sketch of the Argument. Progresses in mathematics or physics often make easier scientific tasks that were difficult, if not impossible, to carry out. For example, to decide whether a proposition of propositional logic is a tautology, one may laboriously enumerate all the 2^k possible cases; but once the tree method is known, things become much easier. Such progresses may originate both in mathematical findings and in advances in the empirical sciences such as the development of new modeling schemes. The invention of the boundary layer by Prandtl seems to illustrate this latter case. Navier-Stokes equations were derived in the mid-19th century but, because of their elliptic behavior, solving them was not possible for most practical problems like calculating the lift and drag on the first airplanes. By contrast, equations for the boundary layer were found to have parabolic behavior. This afforded significant analytical and computational simplification and the calculation of aerodynamic drag became possible for various situations. In such cases, the difficulty initially met by scientists is *epistemic* in the sense that it results from some lack of knowledge: Prandtl's invention showed that the investigation was *apparently* complex but not *intrinsically* so.

Is complexity always epistemic – and can we always entertain the hope that it may be swept away by scientific progress? Conversely, if complexity is not always epistemic, in which cases should it be seen as an objective feature of an investigation? People familiar with logic and computer science know that it can sometimes be proved that some mathematical tasks are genuinely difficult or impossible to carry out. The purpose of this paper is to make a riskier step by presenting cases in which *physical systems* can reliably be described as inherently complex.

Complexity is a property of mathematical models or problems[2]. Accordingly, establishing that some complexity properties intrinsically characterize some physical systems seems to require, at the least, showing that the corresponding models are *true* representations of these systems – and thereby solving the realism/anti-realism problem in such cases. A particularity (oddity?) of the argument to follow is that no such thing is in fact needed: the complexity of the models will be shown to characterize systems faithfully even if these models are false.

To make things clearer from the start, I shall now present a short version of the argument. Here are the assumptions. One wants to investigate the target behavior $B(S_i)$ of

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[2] See section 4 for more about this point.

systems S_i, that is, of some set of systems of a common physical type S (e.g. Ising-like system) in different configurations (e.g. the numbers of spins, geometry and external fields may vary). A general model M (like "the" Ising model) yields the family of particular models M_i which is empirically adequate[3] regarding behavior $B(S_i)$. Finally, solving models M_i corresponds to a mathematical problem Π having irreducible computational complexity K. Let us now make the hypothesis that there exists another family of models M_i* which is also empirically adequate regarding behavior B_i and corresponds to a *simple* mathematical problem Π*.The claim is that this latter hypothesis implies a contradiction. Indeed, if Π* is simple, it should be possible, when trying to solve models M_i, to solve the corresponding easy models M_i* instead. It then becomes possible to solve problem Π easily, which, by assumption, is impossible. Therefore, if there is another empirically adequate family of models M_i* for $B(S_i)$, then the corresponding mathematical problem cannot be significantly simpler than Π.

Overall, the argument involves the main following claims:
(1) the investigation of the behavior of physical systems can be described as computational problems (in the computer science sense);
(2) such computational problems can be irreducibly complex;
(3) if a computational problem corresponding to the solution of a family of empirically adequate models is complex, then any other such family corresponds to an equally complex computational problem.

I provide in section 2 evidence for claims (1) and (2) and discuss claim (3), which is the potentially controversial core of the philosophical argument, in section 3.

2. Physical Investigations and Complex Computational Problems. In this section I argue that investigations about physical systems can sometimes be adequately described by means of irreducibly complex computational problems. A computational problem is an infinite collection of tasks of a common type such as "Given two numbers p and q, find the value of their sum $p+q$". The instances of the problem are the specific tasks that are actually being carried out, e.g. "1+1", "1+2", "2+1", etc. To theoretically study the behavior $B(S_i)$ of a system S_i, scientists need to investigate the corresponding property $P(M_i)$ of a model M_i standing for S_i. Then, they tackle the following task T_i "Based on (a suitable description of) M_i, find (a description of) $P(M_i)$". Since the physical parameters of S can indefinitely vary, the generic study of $B(S_i)$ corresponds to an infinite number of tasks T_i of a common type and therefore to a computational problem. For example, generic physical investigations like "given a Ising system composed of $p \times q \times r$ spins, calculate its

[3] Empirical adequacy usually characterizes theories regarding all observable phenomena. I here use this notion for (family of) models regarding some specific behavior.

equilibrium properties” or “given the description of a classical gas of n particles at time t_0, find its state at time t_0+t ” correspond to computational problems.

The next step requires showing that some computational problems having a physical interpretation are intrinsically complex. Fortunately, computer scientists and physicists complete themselves this step by applying computational complexity theory (hereafter CCT) to physical problems. A problem is regarded as inherently difficult if any algorithm that solves its instances requires significant resources. CCT formalizes this intuition by robustly quantifying the resources needed to solve problems and by identifying a hierarchy of robust complexity classes (like NC, P, NP, EXP, etc.). For example, a decision problem is NP-complete if its solutions can be verified in polynomial time (it belongs to NP) and any problem in NP reduces to it in polynomial time; a problem is P-complete if it can be solved in polynomial time by a Turing machine (it belongs to P) and every problem in P reduces to it through an appropriate reduction.

A crucial notion in defining complete classes is that of reduction. A reduction is an algorithm transforming one problem into another one. For example, multiplication reduces to addition (2x3 = 2+2+2) and, if you know how to add, you know how to multiply. Reductions can also be used to show that the reduced problems are not more difficult than the reducing problem – provided that the cost of the reduction is negligible. Typically, to prove that a problem is NP-complete, polynomial reductions are used. Overall, if a problem is complete for a complexity class, unless this whole class collapses to some lower class (which, in the NP-complete case, is believed to be unlikely), no algorithm can be found to solve it significantly more quickly – whatever our scientific progresses.

It is a fact that some (major) physical problems have been proven complete for some complexity classes. I shall present two. The Ising-model has played for decades a central role in the development of modern statistical physics (Baxter, 1982). Whereas Onsager solved the two-dimensional case in 1944, its three-dimension version resisted investigations for decades till Baharona (1982) proved that evaluating its partition function is a NP-hard problem. More simple physical problems, like lattice gas models, can be complete for lower complexity classes. The investigation of lattice gases started in the 70ies as attempts to solve the Boltzmann equation for extremely simplified gazes of particles with discrete velocities (Hardy et al., 1973). Further inquiries proved that lattice gazes could be used to simulate Navier-Stokes equations (Frisch et al, 1986) and exhibit physical behavior. Lattice methods are currently being used in computational fluid dynamics for various applications such as the investigation of air flowing over vehicles. They have been proven P-complete (Moore and Nordhal, 1997), which essentially means that sequential polynomial simulations are required to investigate them.

We have reached so far the conclusion that, unless the complexity hierarchy partly collapses, the investigation of some physical models (e.g. Ising-like systems or billiard ball models) is irreducible complex (the degree of complexity being defined by the complexity class these models belong to). This conclusion calls for three remarks.

First, the conclusion reached so far is not that, *within some scientific practice,* some physical models are *actually* investigated by solving some complex computational problems – otherwise, one could answer that *these practices* are complex ones and involve difficult tasks like computing Fourier transforms, inverting matrixes, finding optima, etc. but that maybe one is using sledgehammers to crack nuts and the difficulty may be bypassed by finding simpler techniques. But the claim is that the mathematical problem – versus some practices solving it – is a complex one. Intuitively, saying that a problem is inherently complex means that solving it requires large resources, *whatever the algorithm that is being used*, which already involves quantifying over possible methods. For example, if P≠NP, no algorithm can solve a NP-complete problem in polynomial time. Further, any problem of a low class of complexity can be solved via a complete problem of a high class of complexity, since low complexity classes are included in higher complexity classes. For example, deciding whether a number is even *can* be performed by reducing this problem to 3-SAT (a NP-complete problem) and solving instances of 3-SAT. But evenness is a simple problem and the complexity of 3-SAT does not lie in the set of instances that can be used to solve instances of evenness. By contrast, the results described above indicate that solving the Ising-model *is* NP-complete, which means that no polynomial algorithm can solve all its instances.

Second, when saying that some *physical* model has such or such complexity, I mean that all the instances of this model have a physical interpretation, which is the case for the Ising model and lattice gazes. If it were not so, the complexity of the computational problem may sometimes lie in a set of instances having no physical interpretation and then it would apparently but unduly characterize the physical problem.

It may however be rightly objected that using a complex model to study the behavior of a system does not imply that no simpler model can be used for the same purposes nor that complexity characterizes the system itself. Therefore, the argument still falls short of proving the claim that the complexity of the model faithfully represents some property of the system. Accordingly, to substantiate the realist claim, there is the need for an additional semantic assumption about the felicitousness of the representational relation between the models M_i and the target systems so that the features of the mathematical models be "tacked" to the physical systems. As we shall now see, the sweet aspect of the argument is that truth is by no means required to complete this step and empirical adequacy, a notion usually considered as innocuous and deceptive by realists, is sufficient to do the job.

3. The Core of the Argument. Let us now discuss the core of the argument. To put things briefly, it is assumed that an irreducibly model is used to study a system, that this model is empirically adequate and then it is shown by *a reductio ad absurdum* that it is not possible that another empirically adequate (possibly true!) *and* simple family of models does the same work.

Notations are as above. P_i (resp. P_i*) is the property of model M_i (resp. M_i*) that stands for behavior B_i and instances of problems Π (resp. Π*) are questions about these properties.

Assumptions regarding our epistemic situation:

- H1. Possibility of our practice. Target behavior B_i is in practice observable and models M_i

can be in practice described (simplicity of modeling) and, when studying B_i, their content (once identified, see H3) can be meaningfully (simplicity of physical content description) ascribed to their target systems (simplicity of reference).
- H2. Semantic assumption. Family of models M_i is empirically adequate for behavior B_i.
- H3. Mathematical complexity assumption. Computational problem Π has irreducible complexity K.

Assumptions about the existence of another possible epistemic situation:

- H4. Semantic assumption. Family of models M_i* is empirically adequate for behavior B_i.
- H5. Mathematical complexity assumption. Computational problem Π* has complexity K* and K* is significantly lower than K in the CCT sense (e.g. Π* belongs to P and Π to NP).

H1 guarantees that we can easily ascribe the studied appearances to the target system and that the complex models we are using to investigate them are not *ad hoc* unduly intricate ways to investigate and refer to systems S_i. H2 says that these models are empirically successful: property P_i catches behavior B_i, that is, M_i has an empirical substructure that is isomorphic to appearance B_i even if the underlying theoretical description is false. H3 adds that these models correspond to an irreducible class of complexity (in the CCT sense). H4 says that another modeling practice is possible, which is not controversial, since any family of models isomorphic to models M_i will do. Strictly speaking, P_x and P_x* need not be the same properties since they catch target behavior B_i *up to isomorphism* (e.g. models M_i and M_i* may correspond to different reference frames). H4 and H5 jointly say that there exists another empirically adequate family of models that is *in addition* simpler to solve (in the CCT sense).

Here is now the *reductio ad absurdum.* Because of the empirical adequacy of the families of models M_i and M_j* for behavior B_i, questions about M_i (regarding P_i) have the same answers as those about the model M_j* (regarding P_j*) that represent the same systems – up to isomorphism. It is then tempting to use the instances of problem Π* to solve the instances of problem Π quickly. For any instance i of Π, one then needs to solve the associate instance j of Π*, that is, to solve model M_j* instead of model M_i. All it takes is to be able to identify for each M_i the corresponding M_j, or, in computational terms, to find a matching procedure that, given the description of M_i, translates it into the description of M_j* and thereby reduces problem Π to problem Π*. Since P_i and P_j* represent the same behavior B_i up to isomorphism, if P_j* is to be used to solve instances of Π, there may sometimes be the need to translate back the description of P_j* into the description of P_i.

Overall, the indirect way to solve models M_i (and problem Π) is composed of three steps, the matching procedure, the solution of models M_j* and, if necessary, the final return translation of the result. Here is now the catch. Since Π has irreducible complexity *K* and complexity cannot vanish in the air, one of these steps at least must also have complexity *K*. In brief, the original complexity constraint in the models that are actually being used has the following consequence:

(a) Either, other empirically adequate models M_j* have the same complexity *K*;

(b) Or their complexity is lower than *K* but matching models M_i with models M_j* (if this is possible) has complexity *K* (e.g. if the original problem Π is NP-complete (resp. P-complete), then the translation procedure must be at least as costly, since Π* problem is comparatively easy).

(c) Or models M_j* and the matching procedure have complexity lower than *K* but translating back the description of P_j* into the description of P_i has complexity *K* – though the two statements say the same thing, up to isomorphism.

Since we are investigating the possibility of the existence of a simple *and* empirically adequate family of models M_j*, we need to analyze whether situations (b) or (c) are possible.

Let us first discuss case (b). The situation is the following. The two families of well-defined structures M_i and M_j* model the same family of systems and account for the same phenomena. Indeed, such correspondences between families of representations do exist in scientific practice, for example descriptions in Newtonian mechanics and Lagrangian mechanics, or descriptions in different reference frames, or standard representations and their Fourier transforms. The specificity of the situation is that one family of representation is (by assumption) intrinsically complex and the other simple. (Please note that when one makes a Fourier transform to make a calculation easier, one thereby proves that the original problem was not intrinsically difficult since you could transform it in a simpler problem). Finally, the investigated assumption is that matching the former to the latter is as difficult as solving the former, or even impossible: Is that latter assumption plausible?

Suppose that the simple family M_j* of models is *in practice usable for modeling purposes*, that is, that it is easily possible to match a non-theoretical characterization of system S_i to the corresponding model (simplicity of modeling assumption). Then, it seems that one can always find a matching procedure between the two families of models: starting from models M_i, come back to its pre-theoretical identifying description (simplicity of reference, see hypothesis H1) and then remodel the system within the modeling framework of models M_j*. Going through the shared pre-theoretical description is a way to establish some translation between the two types of description. Now suppose that it is possible to faithfully describe this translation procedure algorithmically. Then there is a contradiction because the procedure is algorithmic and simple whereas it was supposed to have irreducible complexity *K*.

The other option is to suppose that the translation procedure, *which can be cognitively carried out by modelers*, is some mental operation that is irreducibly not algorithmic in the way it is carried out (even if it de facto computes the matching between models M_i and models M_j*). Then, we are compelled to accept that some mental modeling operation can, by some magic, quickly solve (all possible instances of) a complex (P-complete, NP-complete, etc.) problem. As far as I know, there is no serious evidence in favor of this general possibility.

Overall, this means that if the complexity really lies in the matching procedure between families M_i and M_j, then there does exist simple models M_j but the modeling procedure to identify them must be as difficult as solving a problem with complexity K – for example solving a NP-complete problem if we are in the Ising case. Such a family of easy models then floats in the mathematical realm out of our modeling reach – and it can hardly correspond to some possible-in-practice science.

It is worth insisting here: the modeling task does not lie *in the invention* of a new type of model. We can assume that models M_j are of a known type; what is here supposed to be difficult is the *standardized application* of this model type to physical situations of a known type, that is, finding particular versions of a general model that is known to correctly represent some type of situation. For example, the modeling task is not to invent the Ising model but to find the particular versions of the Ising model for particular systems of a common type (e.g. ferromagnetic systems having this or that geometry and number of atoms) of which we already know that the Ising-model is a good representation.

While the situation just described is implausible, I unfortunately have no clean, simple and final argument showing that it is logically, mathematically, or physically impossible. I even suspect that it is possible to cook up weird logical *ad hoc* constructs, possibly based on some costly transformations of the original problem Π, which make this situation possible. Typically, one may build into the modeling procedure the difficult steps of the solution of M_i and end up with some string of symbols computationally close to the solution of M_i; one may then claim that these strings are models M_j* and the trick is played. One may however doubt that the trick is acceptable, since all the complexity has been in practice transferred in the description of the family of models. Indeed, computer scientists do not seem to accept such descriptive procedures. Papadimitriou (a prominent computer scientist) notes: "There is a wide range of acceptable representations of integers, finite sets, graphs, and other such elementary objects. They may differ a lot in form and succinctness. However, all acceptable encodings are related polynomially. <…> In the course of this book, when we discuss a Turing machine that solves a particular computational problem, we shall always assume that a reasonably succinct input representation <…> is used" (1994, 26).

As philosophers of science, we may also add the acceptability constraint that the description of models M_j* should be made in a language that is suitable not only for investigating systems S_i but also other classes of systems – as can be expected from a language a) that is used within some general scientific theoretical practice which goes beyond the particular study of the systems the complexity of which is being discussed and b) that is appropriate to describe natural kind predicates.

Overall, and even in the absence of a formal proof, it seems safe to conclude that the matching procedure between family of models M_j and M_j, if these models are to be given acceptable descriptions, can hardly have complexity K – especially if we are discussing models that have supra-polynomial complexity, like the Ising model (cases involving polynomial complexity are in a sense more difficult to treat because "acceptable" encodings are usually polynomially related).

There now remains the possibility that the complexity might lie in the translation between the descriptions of P_i and P_j* (case c above) but a critical discussion can be made along the same lines as above. Indeed, since models M_i, M_j* and the appearance say the same thing*, up to isomorphism*, the translation between the description of P_j* and P_i can go through the description of appearances. Then we would have a case of an easy family of models having some acceptable description (since the complexity is no longer supposed to lie in the modeling procedure) but determining what the solution P_j* means about the isomorphic appearances it represents would be a complex problem. For similar reasons as above, this possibility also appears implausible and unacceptable.

Let us wrap up. Situations (b) and (c) describe would-be situation in which there are two different ways of modeling, one tractable, the other not, but there is something like a computational gap between i) these two possible practices, either in terms of identifying the easy models or of translating their solutions; ii) between non theoretical descriptions of the target systems (and their appearances) and the description of the easy models (or of their solutions) – and of course, the more intrinsically complex the original problem, the larger this gap must be. The argument above shows that such situations are extremely implausible or "non acceptable". The converse conclusion is that in such cases, it is extremely plausible that any acceptable empirically adequate family of models (including the true models) have the same complexity *K* as the models we are actually using.

4. Discussion. I now want to clarify a few points about the content, validity and scope of the argument.

i) Strictly speaking, complexity characterizes computational problems (and models), so it cannot be directly and meaningfully ascribed to a physical system. The precise claim is that, in the discussed cases, *all satisfactory representations of a system* cannot but have this complexity property – which is a high-order property, since it describes a common feature of all possible algorithms that solve some models.

ii) It can however be claimed that the corresponding systems have been characterized intrinsically. Indeed, not only is the complexity property a feature of their true representation, it also characterizes all the representations that can be used to investigate the target behavior. This second stronger statement secures the intrinsicalness claim since it does not make it relative to any particular representation and shows that it is an essential feature of any possible investigation of the system (*versus* a somewhat accidental and neutral feature of the system or its representation). Indeed, if using the true representation of the system to investigate its behavior was difficult but the difficulty could be sidestepped by using proxy representations, complexity would be a true but shallow, without epistemic effect and somewhat contingent feature of the system. By contrast, the claim is that its nature is such that it is intrinsically difficult to investigate it, whatever the nature and "degree of truth" of the representation.

iii) The claim made is immune to progresses in computer or physics. Typically, if a system is said to be inherently complex, the advent of quantum computers will not make it theoretically easier – even if, for practical purposes, solving it may be much faster. Going from Marathons to Athens by car is quicker than running all the way but it does not make the distance shorter. In the same way, quantum computers may remove computational constraints for scientists but it will change neither the complexity hierarchy nor the interest for refining low complexity classes and seeing which models and problems belong to them.

iv) In the argument, I did not have to specify whether the would-be families of empirically adequate and simple models were to be derived from the same theory or result from some more substantial theoretical change. Therefore, the result describes the limits of the progresses possibly generated both by findings in modeling *and* theoretical revolutions.

v) I did not have to root my realist claims about complexity in the supposed truth of some aspects of some representations. Thus, whereas most discussions of realist claims need to bring answers to anti-realist arguments (see for example Psillos, 1999), the present argument is noncommittal about but compatible with the validity of anti-realist arguments, like those in terms of pessimist induction, under-determination or skepticism about inference to the best explanation. Therefore, anti-realists may also have to bite the bullet and be realist about the complexity of, say, Ising-like systems. But conversely, as far as I can see, the argument is also noncommittal about existing realist arguments regarding scientific representations.

vi) Since irreducible complexity cannot vanish mysteriously, anyone willing to defeat the argument need to explain where the complexity of the original models has gone and why no translation between models doing the same work is possible; if this ever happen, we will definitely learn something valuable about possible sciences.

vii) I have however claimed that if such simple families do exist, they can hardly be part of an actual tractable scientific practice. Accordingly, even if the reader refuses to buy the realist claim, she may still have to buy the inevitabilist claim about what usable representations of such systems must be like in any possible-in-practice science.

5. Conclusion Fluid dynamics problems tackled by Prandtl were difficult but boundary layer models made them tractable. If the above argument is valid, no such progress is to be expected when the family of models that represent some system is both empirically successful and corresponds to an intrinsically intractable problem. In such cases, complexity is presumably an unavoidable property of all its acceptable representations and therefore faithfully reflects an intrinsic and essential property of the system.

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