The Road to Maxwell's Demon: Conceptual Foundations of Statistical Mechanics
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Understanding the foundations of statistical mechanics is of great importance for its own sake but also for the light it sheds onto other very important issues in philosophy of physics and metaphysics such as the direction of time, the notion of probability, the status of laws of nature, and the role of the observer in a physical theory. The book of Hemmo and Shenker addresses and develops all these topics originally, and therefore is a welcome addition to the literature on the subject. The book is so dense and full of innovative contributions that it would require an extensive work to properly analyze them all. So I will simply discuss what I take to be the main problem of the book, namely that the authors fundamentally misunderstand Boltzmann's approach of statistical mechanics. If I am correct, this is a serious shortcoming: since the main motivation for their book seems to be the failure of Boltzmann's account, if the authors do misunderstand Boltzmann's work and it is that case that it does not fail, then their account loses most of its appeal.

After a detailed introduction, the second chapter is devoted to present thermodynamics. In the third chapter then the authors discuss the impossibility of deriving the laws of thermodynamics from the ones of classical mechanics. The following chapter is devoted to the concepts of time and time-reversal invariance in classical physics, while chapter 5 presents statistical mechanics. In chapter 6 the discussion centers on the role and meaning of probability. The authors' main idea is that in this framework the only notion of probability that has an empirical significance is the transition probability of the system from one macrostate at one time to another macrostate at a subsequent time. That is, the transition probability is primitive and it is given by the size μ of the overlap between the initial macrostate evolved forward in time (what they call the "dynamical blob") and the final macrostate. Hemmo and Shenker claim that the measure μ is to be selected, among the possible ones, in accordance with experiments. This perspective differs substantially from the most received approach in which probabilities refer to an initial probability distribution. In Hemmo and Shenker's approach these initial probabilities are only derivative, and as a consequence the initial probability distribution is underdetermined. Also, the fact that the measure μ has to be selected empirically goes against the received views according to which such measure is selected in some natural way.

Chapter 7 discusses the notion of entropy. This leads the authors to introduce another measure, ν, distinct from the measure μ above but also to be chosen on empirical grounds. The entropy, which contrarily to Boltzmann's account does not correspond to the size of the macrostates, allows the authors to define the notion of equilibrium macrostate, and this in turn provides them with a new account for the explanation of the second law of thermodynamics. They argue that Boltzmann's approach fails and they propose their own account as a valid and more desirable alternative. I am not convinced by their criticisms of Boltzmann, which I believe originate from a misunderstanding of his work. They in fact claim that Boltzmann's explanation of the second law "makes the dynamic of the system irrelevant" and this, according to them, is problematical: "it rules out the possibility that a large measure of the trajectories that start out in non-equilibrium macrostates may not evolve to equilibrium […] but nothing in mechanics prevents this case" (p. 173). It is surely true that mechanics does not prevent an evolution from a non-equilibrium state to another non-equilibrium state, but there is nothing problematical about that.

Indeed, this is the core of Boltzmann's response to Loschmidt's objection and the very reason for the success of this approach. The crucial point is that these "anti-thermodynamic" trajectories, in which the microstate does not evolve eventually into the equilibrium state, are not typical: they exist but they are very rare. In addition, the authors claim that Boltzmann's account is empirically inadequate because, considering the example of the free expansion of a gas in a container, "it implies it is more likely to evolve directly from the state in which it fills half of the container to the state where it fills the entire container, not going through the intermediate stages" (p. 174). It is supposed to be so because "since the number of arrangements in [M_2]" (the equilibrium macrostate) "is larger than the number of arrangements in [M_1]," (some intermediate non-equilibrium macrostate) "the probability of [M_2] is higher than the probability of [M_1]. This means that at t_1, when the system just starts out in [M_0]" (the initial macrostate in which the gas is all in one corner of the container) "it is already the case that the transition probability to [M_2] is larger than the transition probability to [M_1] […], and this means that the system is more likely to evolve directly to [M_2] than it is to evolve directly into [M_1]. Nothing in Boltzmann's combinatorial argument suggests that the system is likely to pass through [M_1] on its way to [M_2]" (p. 175). The point of Boltzmann's account is that the dynamics of a typical microstate will bring it, eventually, into the equilibrium macrostate because such state is incredibly bigger than the other macrostates. The trajectory of a microstate will cross different *accessible* non-equilibrium macrostates before typically arriving, eventually, to the one of equilibrium. To talk about the "probability" of entering this macrostate rather than that one *simpliciter* assumes that every single macrostate is accessible to every single trajectory at any moment, which is clearly not true: if the initial macrostate is far from equilibrium, then it could not evolve directly into it, since the equilibrium macrostate will not be directly accessible to the initial macrostate. For instance, consider a gas which starts expanding from the left corner of a container. Its initial macrostate is described by a given pressure, volume, and temperature. The first accessible macrostates, that is the macrostates in which it could evolve first, are macrostates described by a slightly different volume, pressure and temperature. The equilibrium macrostate instead is not accessible to the initial state directly if it corresponds to a pressure, volume and temperature which are markedly different from the one of the initial macrostate, as it is the case in this example. Having clarified that, it seems to me that Hemmo and Shenker's objections to Boltzmann's approach fail.

Be that as it may, in chapter 8 the authors discuss the notion of typicality, the view according to which the typical microstate will evolve toward equilibrium, which is at the heart of Boltzmann's explanation. Here "typical microstate" means that it is part of the vast majority of microstates, counted using the Lebesge measure. Hemmo and Shenker argue against what they call the "a priori" typicality approaches, in which the choice of the typicality measure to count the microstates is justified appealing to some sort of a non-empirical reason. They believe these views are indefensible: they analyze several non-empirical justifications of the typicality measure and reject them all. I am not convinced their criticisms are exhaustive, though. In fact for instance they seem to dismiss too quickly the view that symmetries, like time-translation, might impose constraints on the choice of the measure: the Lebesgue measure is the only measure which is time-translation invariant, given the Hamiltonian structure. Time-translation invariance is supposed to be important because the typicality measure counts trajectories, and to do so properly one needs not to privilege any time in particular. Since Hemmo and Shenker do not mention the role of time-translation invariance, they do not seem to address the proposal satisfactorily and so fail in their rebuttal.

Chapter 9 is devoted to the notion of measurement, while in chapter 10 Hemmo and Shenker discuss the problem of retrodiction (the fact that retrodicting the past, contrarily to predicting the future, is inconsistent with our memories). Chapter 11 deals with the Gibbsian formulation of statistical mechanics, of which the authors give a novel interpretation. In chapter 12 Hemmo and Shenker address the notion of information and the thermodynamic of computation, while in the final chapter the authors arrive to Maxwell's demon, which they consider the prototype counter example to the laws of thermodynamics, and explain how it is consistent with statistical mechanics. In addition there are two appendices: the first one provides an example of a "Demonic" evolution, the second considers some issues related to the role of probabilities in quantum mechanics. In B1 the authors address Albert's proposal connected to the GRW theory, in B2 they question the role of typicality in Bohmian mechanics, and in B3 they argue that Maxwell's demon is compatible with quantum mechanical dynamics with or without collapse. To conclude and summarize, I think that the account that Hemmo and Shenker propose may be compelling only to those who believe Boltzmann's explanation does not work, something that they have not yet managed to convince me. Be that as it may, the book provides an interesting, original and valued contribution to the literature in the foundations of statistical mechanics that surely deserves attention and careful reading.

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