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ARTICLE

Two Approaches to Reduction: A Case Study from Statistical Mechanics

Bixin Guo

Department of History and Philosophy of Science, University of Pittsburgh, Pittsburgh, PA, USA
Email: big15@pitt.edu

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Abstract

I argue that there are two distinct approaches to understanding reduction: the ontology-first approach and the theory-first approach. They concern the relation between ontological reduction and inter-theoretic reduction. Further, I argue for the significance of this distinction by demonstrating that either one or the other approach has been taken as an implicit assumption in, and has in fact shaped, our understanding of what statistical mechanics is. More specifically, I argue that Boltzmannian statistical mechanics assumes and relies on the ontology-first approach, whereas Gibbsian statistical mechanics should assume the theory-first approach.

The relation between thermodynamics and statistical mechanics (SM) is one of the most paradigmatic instances of reduction. When one attempts to develop an account of reduction and needs an example to demonstrate how exactly that account works, the reduction of thermodynamics to SM is the canonical case to which one appeals.[1] However, it is in fact questionable whether, and in what sense, thermodynamics can be reduced to SM. Worse, it is not even clear what the correct theoretical framework of SM is: there are the so-called *Boltzmannian framework of SM* (BSM) and *Gibbsian framework of SM* (GSM) in the contemporary literature, and it is under contention which is correct.

Instead of assuming we have a clear grasp of the reduction between thermodynamics and SM and using that as a paradigmatic case to understand reduction, I propose to approach the problem from a different direction: I argue that there are two distinct approaches to understanding reduction—what I call the *ontology-first approach* and the *theory-first approach*. Furthermore, I argue that either one or the other approach has

[1] See, for example, Nagel (1961) and Dizadji-Bahmani et al. (2010).

been taken as an implicit assumption in, and has in fact shaped, our understanding of what SM is—in particular, whether its correct framework is Boltzmannian or Gibbsian.

To clarify, I don't intend to argue, in this article, that either the ontology-first or the theory-first approach is the right approach to reduction. Rather, the point is to show why drawing a distinction between these two approaches is important and useful. How we understand reduction—either as ontology first or theory first—often is tacitly assumed and shapes our understanding of particular instances of reduction. To demonstrate exactly what role the two approaches to reduction play, I turn to the reduction of thermodynamics to SM as an example. In particular, given that a significant part (if not all) of SM is to be a reductive underpinning of thermodynamics, these two approaches shape our understanding of not only this particular instance of reduction but also SM, the theory itself. My focus will thus be on SM.

In this article, I first explicate the distinction between the ontology-first and theory-first approaches to reduction. I then introduce the essential elements of BSM and GSM in section 2. In sections 3 and 4, I argue that BSM, and especially Boltzmannian criticisms of GSM, tacitly assume and rely on the ontology-first approach. In section 5, I argue that GSM would be immune to these criticisms if it were to take the theory-first approach as an assumption.

1. Ontology-first versus theory-first approach to reduction

1.1. Introduction

Reduction is a relation. What are the relata of this relation? There is no univocal answer to this question.[2] Sometimes reduction is taken to be a relation between two scientific *theories*, for example, thermodynamics and SM. Reduction of this kind is called *inter-theoretic reduction*. (I use the term '*an* inter-theoretical relation of reduction' for an *instance* of inter-theoretic reduction.) Sometimes reduction is taken to be a relation between *objects*—"real concrete things that exist here in our material world, things like quarks, or mice, or genes"[3]—at two different levels.[4] For example, a box of chlorine gas is composed of molecular chlorine; that is a reduction relation between the greenish-yellow stuff in the box and chlorine molecules. Reduction of this kind is called *ontological reduction*. (I use the term '*an* ontological relation of reduction' for an *instance* of ontological reduction. Moreover, I intend to use the term 'ontological' in a broad way: to include reduction not just between objects but also between their respective states, properties, quantities, and so forth. Crucially, though, ontological reduction relates properties that are not themselves especially theory-laden; that is, these properties can be understood independently of the relevant theories.[5])

How are these two kinds of reduction—ontological and inter-theoretic—related to one another? Is one more primary, on which the other depends? It seems natural to think that if objects at two levels bear a reduction relation (say, a composition relation between chlorine gas and molecular chlorine), then, as a consequence, the

[2] For a general review, see van Riel and van Gulick (2019).
[3] Cartwright (1983, 55).
[4] See, for example, Smart (1959, 143) and Ney (2013).
[5] The primary examples in this article will be spatial locations of particles.

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theories of the objects at each level should bear a reduction relation as well; that is, inter-theoretic reduction follows from, and is dependent on, ontological reduction. But does inter-theoretic reduction necessarily follow from ontological reduction? And what about the other way around? Despite the fact that both ontological and inter-theoretic reduction are commonly employed and discussed in various fields of philosophy and science, there has not been much explicit discussion of how these two kinds of reduction are, or should be, related.[6]

This article offers a starting point to consider the relation between ontological and inter-theoretic reduction. It identifies two possible ways to understand their relation: the ontology-first *approach* and the theory-first *approach* to reduction. These two approaches, in particular, are concerned with which kind of reduction is *prior to* the other.

1.2. An account of reduction: Ontology-first or theory-first?

To clarify, neither the ontology-first approach nor the theory-first approach is meant to provide *an account of reduction*, which concerns what reduction is. Usually, such an account forthrightly specifies what kind of reduction it is an account of (for instance, whether its relata are objects or theories). It then identifies necessary and sufficient conditions that a successful reduction satisfies. Nagel's (1961) account, one of the most prominent accounts of reduction, takes the relata of reduction to be scientific theories. According to this account, one theory is reduced to another theory if (roughly speaking) the former can be derived from the latter. Different accounts of reduction may identify different kinds of relata of reduction. Smart (1959, 143), to consider another example, offers a tentative account that takes *entities* to be the relata of reduction.

The two *approaches to reduction*, in contrast, concern the priority relation between ontological and inter-theoretic reduction and can be conceived of as a way of classifying various accounts of reduction. By specifying what the relata of reduction are—whether they are objects or theories, a particular account of reduction takes reduction to be either *primarily* or *exclusively* an ontological relation (or an inter-theoretic relation). We thus can ask: For any given specific account of reduction, does it follow the ontology-first approach or the theory-first approach?

To answer this question, we need to identify whether that account takes reduction to be *primarily* (or *exclusively*) a relation between objects or *primarily* (or *exclusively*) a relation between theories. For example, Smart's account takes reduction to be primarily about entities; hence, it is classified as following the ontology-first approach. Nagel's account, prima facie, may be seen as following the theory-first approach, since it takes reduction primarily to be a relation between theories.[7] If an account admits more than one correct way to understand reduction, we need to identify whether that account takes ontological reduction to be prior (or primary) and inter-theoretic reduction to be derivative (or secondary), or the other way around.

What does it mean that ontological reduction is *prior to* inter-theoretic reduction (or the other way around)? Various senses of priority are adequate to flesh out the

[6] For exceptions, see van Riel and van Gulick (2019, section 4).

[7] Having said which, the status of bridge laws makes things more complicated. If they are conceived of as stating identities or relations between the extensions of terms in the reducing and reduced theories, Nagel's inter-theoretic reduction "incorporates essential reference to the theories' ontologies" (van Riel and van Gulick 2019, 2.2.3), and thus it requires ontological reduction. But this would not affect my point.

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relation between these two kinds of reduction (and accordingly, the distinction between the two approaches to reduction). For instance, x is prior to y if y is dependent on, derived from, a consequence of, grounded by, justified by, or explained by x. These different senses of priority are not mutually exclusive but could be complementary.

An account of reduction follows the ontology-first approach if, for instance, what it is to be inter-theoretic reduction relies on ontological reduction, or understanding inter-theoretic reduction requires understanding ontological reduction to begin with. For example, Oppenheim and Putnam's account of micro-reduction, which concerns reducing one theory to another (or reducing a branch of science by another branch), requires that the ontology of a branch of science "possess a decomposition into proper parts" of the ontology of another branch (1958, 6). Since they take this ontological reduction to be "the essential feature of a micro-reduction," their account follows the ontology-first approach. In contrast, an account of reduction follows the theory-first approach if ontological reduction is only a consequence of, and depends on, inter-theoretic reduction. New Wave Reduction is an account that most explicitly commits to the theory-first approach: it takes reduction to be primarily a relation between theories; more importantly, it is essential to this account that "the ontological consequences of a given reduction [that is, ontological reduction relations] *are secondary to and dependent upon* the nature of the theory reduction relation" (Bickle 1996, 65, 74).

An account of reduction can be classified as ontology-first or theory-first, but committing to an account of reduction is not the only way for one to follow the ontology-first or the theory-first approach.

1.3. The ontology-first versus theory-first approach to reduction: Further explication

The ontology-first approach takes it as given that there is a reduction relation between higher-level objects O_H and lower-level objects O_L, and *if there is a reduction relation between the theory of* O_H *and the theory of* O_L, then this inter-theoretic reduction relation is a consequence of the ontological reduction relation. In short, the ontology-first approach takes ontological reduction to be *prior to* inter-theoretic reduction. This direction of priority is illustrated by the arrow in the middle in Figure 1.

In contrast, the theory-first approach takes it as given that there is a reduction relation between two scientific theories T_H and T_L. Once T_H and T_L are each interpreted with an ontology, the approach states: if there exists a reduction relation between the ontology of T_H and the ontology of T_L, then this ontological reduction relation is a consequence of the inter-theoretic reduction relation. In short, the theory-first approach takes inter-theoretic reduction to be *prior to* ontological reduction. This direction of priority is illustrated by the arrow in the middle in Figure 2.

Stating these two approaches precisely requires specifying what ontological reduction and inter-theoretic reduction are, which requires specifying an account of reduction that takes objects as relata and another that takes theories as relata. However, neither the ontology-first approach nor the theory-first approach relies on any particular account of ontological reduction or inter-theoretic reduction. For our purposes, it suffices to get an intuitive idea of ontological reduction by thinking of, say, a mereological relation. An example of such a relation is the composition relation

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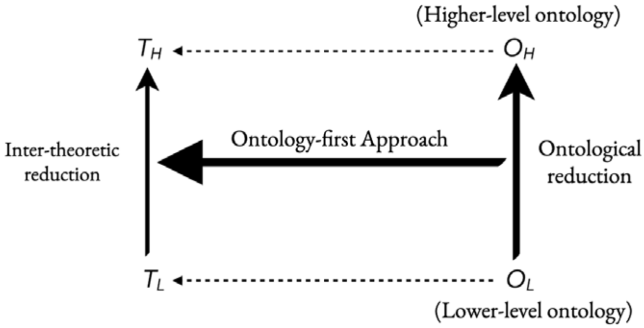

Figure 1. The ontology-first approach starts with the ontological reduction between O_H and O_L, illustrated by the arrow on the right. Once O_H and O_L are specified, scientific theories are then meant to describe, explain, and make predictions about O_H and O_L. The arrow in the middle depicts the core of the ontology-first approach: inter-theoretic reduction (illustrated by the arrow on the left) follows from ontological reduction as a consequence.

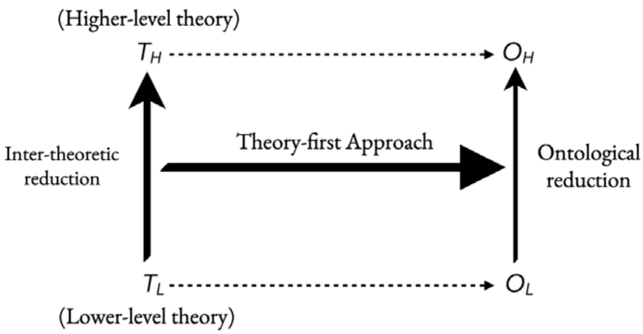

Figure 2. The theory-first approach starts with the inter-theoretic reduction between T_H and T_L, illustrated by the arrow on the left. Given a theory, we can then interpret it with an ontology. The arrow in the middle depicts the core of the theory-first approach: ontological reduction (illustrated by the arrow on the right) follows from inter-theoretic reduction as a consequence.

between chlorine gas and chlorine molecules. Ontological reduction can also be understood in terms of supervenience, identity, realization, or elimination (van Gulick 2001, 4–9). For instance, the states of chlorine gas supervene on the states of chlorine molecules. The general idea of ontological reduction is, as Schaffer (2008, 83) puts it, "[w]hat reduces is grounded in, based on, existent in virtue of, and nothing over and above, what it reduces to" and, metaphorically, "to create what reduces, God would only need to create what it reduces to."

Crudely and tentatively, one can take inter-theoretic reduction to mean something like the following: T_H can be reduced to T_L if and only if T_H can be fully explained by, or derived from, T_L. Consider, as a simplified example, reduction as *derivation*. In this case, what the theory-first approach takes as given are T_H, T_L, and a derivation of T_H from T_L.

The main motivation behind the ontology-first approach is that ontological reduction is about what the world is like, and what the world is like is independent of,

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and prior to, how we theorize about the world. In other words, "[o]ntological reduction is independent of how we conceptualize entities, or theorize about them. Ontological reduction is a thesis about mind-and-theory-independent reality" (Schaffer 2008, 83). Meanwhile, our scientific theories and any relations between them, including inter-theoretic reduction, *depend on* what the world is like. Because ontological reduction means that objects O_H are "nothing over and above" O_L, the theories of O_H and O_L should also bear some kind of reduction relation—it would be deeply puzzling if they didn't. Altogether, it suggests that inter-theoretic reduction depends on and follows from ontological reduction, not the other way around. This relation between ontological and inter-theoretic reduction is explicitly characterized by, for example, Fodor (1974, 97): "the assumption that the subject-matter of psychology is part of the subject-matter of physics is taken to imply that psychological theories must reduce to physical theories."

One (but not the only) way for ontological reduction to be prior to inter-theoretic reduction, or more generally, how we theorize about objects, is if an ontology of a certain domain is taken to be prior to its theory. This way suggests two sufficient but not necessary conditions for an ontology-first approach.

First, scientific theories are primarily about objects. That is, given the objects from a certain domain, a scientific theory is meant to provide descriptions, predictions, and explanations of these objects. Hence, a theory, especially a physical theory, should forthrightly specify or postulate its ontology. Once what the ontology is has been made clear, only then does the theory say what the ontology does, how it behaves, or what its dynamics is.[8] A physical theory is thus necessarily attributed with an ontology. An uninterpreted mathematical formalism, even if it is successful at making novel predictions, does not count as a physical theory unless it is interpreted with an appropriate ontology.

Second, we can have some kind of grasp of what an ontology is like prior to its theory. We may not know exactly what the ontology consists of or what specific properties it possesses. Rather, what we can grasp are pre-theoretical or metaphysical constraints on what the ontology is like. That is to say, what the ontology of a theory is like is not only constrained by what is said by the theory but also by pre-theoretical or metaphysical considerations. In particular, ontological reduction can be one of these considerations.

For example, Poidevin (2005) argues for the principle of recombination as a constraint on what chemical elements are physically possible, that is, on what the ontology of a chemical theory could be. Elements in the periodic table (such as potassium [with atomic number 19] and calcium [20]), which form a discrete series, are *physically possible*. In contrast, anything with atomic number between 19 and 20 (say, 19.356), which forms a continuous series, is merely *logically possible*. According to the principle of recombination, the physical possibility of being an element is constituted by a recombination of actual instances of electron distributions (Poidevin 2005, 129–130). Because there isn't any intermediate position between, say, having two electrons in one orbit around the nucleus and having only one, anything with atomic number between 19 and 20 cannot be the result of a recombination of actual electron distributions and thus is ruled out as a physical possibility by the principle of recombination. This principle identifies a reduction relation between the higher-level objects, elements, and the lower-level objects, electrons, and it is this ontological

[8] See, for example, Allori and Zanghi (2004, 1744) and Maudlin (2016, 318).

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reduction that determines what elements are physically possible and what are merely logically possible. Particularly, Poidevin (2005, 131) emphasizes that the property of being a chemical element is theory neutral. Hence, the reduction relation involved is indeed ontological rather than inter-theoretic.

Consider another example in which ontological reduction acts as a constraint on what the ontology of a theory could be and, consequently, on the theory itself. The primitive-ontology version of Bohmian mechanics has been defended by arguing that a fundamental physical theory (such as quantum mechanics) without a primitive ontology should be avoided. Primitive ontology was introduced as "the basic kinds of entities that are to be the building blocks of everything else" (Dürr et al. 1992, 850). Its role, as Allori (2015, 110) puts it, is to "ground a scheme of explanation" in which the behavior of the primitive ontology determines the properties of macroscopic physical objects. That means: introducing the primitive ontology secures an ontological reduction relation between the fundamental ontology and familiar higher-level objects (like tables, chairs, and measurement pointers). Given this relation between the fundamental ontology and familiar macroscopic objects and the latter being local and three-dimensional, the former needs to have these properties as well. It is in this way that ontological reduction imposes a constraint on what the ontology of the fundamental theory could be like; consequently, whatever the fundamental quantum theory turns out to be like, its ontology needs to contain the primitive ontology, or else it would not be the right theory.[9] (This argument would not work under the theory-first approach, for neither the higher-level theory that describes macroscopic objects like tables and pointers nor its inter-theoretic reduction relation [with quantum mechanics] even comes up. Thus, inter-theoretic reduction is not primary in this argument.)[10]

In contrast to the ontology-first approach, the theory-first approach does not require that each theory be attributed with an ontology. In other words, it is not necessary for a theory to forthrightly specify or postulate an appropriate ontology in order to be physical or carry any physical significance (instead of merely being a mathematical tool). How does a theory establish its status as a physical theory, then? Via its usefulness or efficiency at describing patterns, making predictions, and providing explanations and practical applications. In physics, this is usually achieved by offering a new *robust and autonomous* dynamics (e.g., Maxwell's equations offered such a dynamics for electromagnetic phenomena). Accordingly, a physical theory can be a mathematical formalism that is only partially interpreted, as long as it can be tested empirically, make novel predictions, and provide explanations.[11]

Nevertheless, the fact that a theory is not necessarily attributed with an ontology does not imply that we cannot subsequently interpret the theory with an ontology. It's just that such an ontology does not play a primitive role in the theory. Anything that can be known about the ontology is given by the theory and how it's used. Whether or not a theory is physical, or what its ontology is like, is not constrained by

[9] Or at least a fundamental theory with a primitive ontology should be preferred over those without one.

[10] McCoy (2020, 4, 10) makes similar observations.

[11] One can think of the relation between the mathematical formalism (that is, a theory without an ontology) and the empirical world in terms of structural realism: the mathematical structure of the theory directly represents the world. Such a radical move, nonetheless, is not required by the theory-first approach.

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any metaphysical preconceptions about the ontology or the ontological reduction relation. Rather, the ontology is only taken to be secondary or derivative to its theory, especially to its dynamics. (This is easier to see with physical theories like quantum theories, less so with, say, biological theories.) A supplement on how we can attribute an ontology to a theory might be needed—for instance, something along the lines of functionalism or Dennett's (1991) pattern theory. The theory-first approach may demand a metaphysical picture that is radically different from what we are accustomed to: one no longer centered around objects with intrinsic properties moving in spacetime.

The ontology-first and theory-first approaches are not meant to exhaust all possible views on the relation between ontological and inter-theoretic reduction. They are better thought of as representing families of views by two ends of a spectrum. Another view on this spectrum: ontological and inter-theoretic reduction are interdependent—there is no ground to prioritize one over the other, and they are on par. This view assumes that ontological and inter-theoretic reduction always come together, which is contentious. For instance, inter-theoretic reduction may not follow from ontological reduction because it is computationally intractable to derive T_H from T_L, even though the ontology of T_H is reduced to the ontology of T_L.[12]

The ontology-first and theory-first approaches are not necessarily subject to such challenges. Recall: the ontology-first approach has a conditional, which leaves open the possibility that there isn't any inter-theoretic reduction following from ontological reduction. Thus, the view that ontological reduction is the only correct way to understand reduction or the only kind of reduction that holds in certain cases still counts as ontology-first. Nonreductive physicalism is one such example: it is reductionist only about ontological reduction but not inter-theoretic reduction. Similarly, the view that there is only inter-theoretic reduction and no ontological reduction still counts as theory-first.

Moreover, drawing the distinction between the ontology-first and the theory-first approach is not necessarily incompatible with the view that ontological and inter-theoretic reduction are interdependent. Because this view might not specify exactly how, or in what sense, they are interdependent on each other, the ontology-first and theory-first approaches together can be seen as a way to further explicate this view.

The two approaches are competing if they are *both* taken to be *metaphysical* (see section 1.3, especially the end). But that's not the only way to understand these two approaches. Instead of metaphysical priority, one can understand the two approaches, for instance, in terms of *explanatory priority*. The ontology-first approach then states: ontological reduction *explains* inter-theoretic reduction; that is, the fact that objects O_H reduce to O_L (say, chlorine molecules are composed of chlorine atoms, etc.) explains the fact that the theory of O_H reduces to the theory of O_L (say, a chemical theory reduces to atomic physics). Similarly, the theory-first approach states that inter-theoretic reduction *explains* ontological reduction. Alternatively, the ontology-first approach can be understood in terms of metaphysical priority and the theory-first approach in terms of epistemic priority. In either case, the ontology-first and the theory-first approach do not oppose each other but can be seen as complementary: each spells out a particular aspect of the relation between ontological and inter-theoretic reduction.

[12] For more arguments, see, for example, Fodor (1974) and List (2019).

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2. Boltzmannian and Gibbsian statistical mechanics

I use *BSM* and *GSM* to refer to two clearly distinguishable positions,[13] the former endorsed by, for example, Albert (2000) and Goldstein (2001), and the latter by, for example, Maroney (2008). This Boltzmannian/Gibbsian dichotomy, though undoubtedly at the center in the philosophy of physics literature, can nonetheless be challenged. The labels of 'Boltzmannian' and 'Gibbsian' could be misleading, because BSM and GSM do not actually track the complicated and nuanced views of Boltzmann or Gibbs (Myrvold 2021a, chap. 7). Moreover, BSM and GSM can be compatible in the sense that Gibbsians, who take their framework to be the more general framework for SM, accept BSM as a special case (Wallace 2020), and Boltzmannians, who take their framework to be conceptually unproblematic or a fundamental theory for SM, recognize GSM as an effective theory (Frigg and Werndl 2019) or at least calculationally useful (e.g., Callender 1999).

The dichotomy, nevertheless, reflects genuine disagreements between Boltzmannian and Gibbsian advocates, and examining these disagreements can shed light on our understanding of SM. I thus organize this article so as to respond to the literature, even though I don't intend to defend the long-term value of treating BSM and GSM as competing (and indeed, the distinction between the ontology-first and the theory-first approach can explain why they are treated as competing).

Let's now introduce BSM and GSM. Consider again a box of chlorine gas, composed of N chlorine molecules. A complete description of the microstate of the system at each time specifies the position q and momentum p of each molecule at that time.[14] The microstate can be represented by a point $(q_1, q_2, \ldots, q_N, p_1, p_2, \ldots, p_N)$ in the $6N$-dimensional phase space. This way of describing the system at the microscopic level is shared by BSM and GSM. They differ in what concepts are employed to describe or represent[15] the system *at the statistical-mechanical level*. And there is no obvious way to translate the concepts of one framework to the concepts of the other (Frigg and Werndl 2019, 424).

2.1. The Boltzmannian framework

BSM uses the concept of *macrostate* to describe the system at the statistical-mechanical level. A macrostate is characterized by macroscopic parameters, such as local pressure and local density of regions that are large enough to contain many molecules but small compared to the size of the box. It is related to the micro-description of the system, namely, a microstate, as follows: the system in a particular macrostate could be in one of many different microstates, whereas the system in a particular microstate is in a unique macrostate. This is because what the macrostate

[13] See, for example, Frigg (2008).

[14] For simplicity, we assume the system is classical and ignore the internal degrees of freedom of the chlorine molecules.

[15] Although what the term *describe* or *represent* means is relatively clear in BSM, it is contested in GSM depending on one's interpretation of probability. For instance, Wallace takes the probability distributions in classical GSM to be understood as classical limits of quantum states; in that case, probability distributions *represent* systems in the same way as in BSM (Wallace 2020). Myrvold (2021a), in contrast, understands probabilities as epistemic chances; thus, *describe* is at most a locution for being "appropriate for" certain physical situations (Myrvold 2021b).

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of a system is is fully determined by the state of its microscopic constituents, but not vice versa. If we slightly change the location or velocity of just one particle in the system, it would no longer be in the same microstate, whereas its macrostate would not be affected. Mathematically, the phase space can be partitioned into regions such that the microstates in each region correspond to the same macrostate—a macrostate is identified with one of those regions. Regardless of whether it is the macrostate or the microstate that is under consideration, what is taken to be the object of study for BSM is clearly, its advocates emphasize, *an individual system* (e.g., Goldstein 2019).

Given the concept of *macrostate*, entropy and equilibrium are defined: the Boltzmann entropy of a system with macrostate M is

$$S_B \equiv k_B \ln \mu_M, \tag{1}$$

where k_B is the Boltzmann constant, and μ_M is the phase-space volume of M. For any given energy, there will be some macrostate that has the maximal Boltzmann entropy among all the macrostates with that energy. This state is designated as the equilibrium state in BSM. As it turns out, the phase-space volume of the equilibrium state of a system at a given energy is overwhelmingly larger than any other macrostates with the same energy. This feature of equilibrium is key to the Boltzmannian characterization of how systems approach equilibrium (e.g., how gas that is initially confined in a corner of a box will uniformly spread out to the whole box later) and their explanation of the prima facie inconsistency between the time-irreversibility of thermodynamics and the time-reversibility of its underlying micro-dynamics (i.e., classical mechanics). Many Boltzmannian advocates take the primary task of SM to be to provide a microphysical description and a justification of thermodynamics and, in particular, to explain the time-irreversibility of thermodynamics.[16]

2.2. The criticized Gibbsian framework

Compared to BSM, recent philosophy of physics has paid less attention to developing GSM into a systematic framework, despite the fact that it is the standard tool in practical applications of SM (Wallace 2020) and widely used among working physicists (Frigg and Werndl 2020). Consequently, it is not clear what exactly GSM is (Frigg and Werndl 2020). For this reason and to demonstrate the disputes between Boltzmannians and Gibbsians more sharply, I first present the version of GSM that has been criticized by Boltzmannians. In sections 4 and 5, I discuss possible conceptual modifications that can be made to GSM to respond to those criticisms.

In contrast to the object of study being individual systems in BSM, the core object of study for GSM is commonly taken to be ensembles (e.g., Frigg 2008; Pathria and Beale 2011, xxiii) or probability distributions (Wallace 2020). An ensemble is usually understood as an infinite collection of systems of the same kind, which only differ in their configuration and velocities at a time point.[17] The state of an ensemble at time t is represented by a probability density function $\rho(q, p; t)$ over the phase space. The time evolution of ρ is given by Liouville's equation:

[16] See, for example, Callender (1999).

[17] Recall that we are working with the example of a box of gas for illustrative purposes.

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$$\frac{\partial \rho}{\partial t} = -\{\rho, H\}, \tag{2}$$

where H is the Hamiltonian, and $\{\ ,\ \}$ is the Poisson bracket.

The Gibbs fine-grained entropy is defined as

$$S_G(\rho) \equiv -k_B \int_\Gamma \rho \ln(\rho) d\Gamma, \tag{3}$$

where Γ is the phase space, and $d\Gamma$ is the standard Lebesgue measure. It is invariant over time, as a consequence of Liouville's equation. Since thermodynamic entropy increases when the system evolves from a nonequilibrium state toward an equilibrium state, the Gibbs fine-grained entropy is inadequate to be the microphysical counterpart of thermodynamic entropy (this is almost universally recognized).

The Gibbs coarse-grained entropy, in contrast, is not invariant in time. Abstractly, coarse-graining is a procedure of averaging over details of the system that are irrelevant to its description at a higher level. We can represent such a procedure by a projection operator J, which is a map on the space of probability distributions such that $J^2 = J$ (i.e., the result of coarse-graining twice is the same as coarse-graining once). J acts on the original probability density ρ, yielding the coarse-grained density:[18]

$$\bar{\rho} = J\rho. \tag{4}$$

One particularly important way, at least conceptually, to think of coarse-graining is as partitioning the phase space into small cells. We define $\bar{\rho}$ such that it is uniform over each cell and assigns the same probability to a cell as ρ. $\bar{\rho}$ is coarse-grained in the sense that the details of ρ within each cell are disregarded. The Gibbs coarse-grained entropy $\bar{S}_G$ has the same form as equation (3) but substitutes ρ with $\bar{\rho}$:

$$\bar{S}_G(\rho) \equiv S_G(\bar{\rho}) = -k_B \int_\Gamma \bar{\rho} \ln(\bar{\rho}) d\Gamma. \tag{5}$$

In GSM, the microphysical counterpart of thermodynamic entropy is $\bar{S}_G$.

Accordingly, equilibrium is defined as a state for which $\bar{\rho}$ is invariant in time.[19] GSM characterizes how systems approach equilibrium in terms of the increase of $\bar{S}_G$. To describe and make quantitative predictions about thermodynamic systems at equilibrium, GSM associates each macroscopic parameter with a phase function $f : \Gamma \to \mathbb{R}$. The phase average $\langle f \rangle$ of f,

$$\langle f \rangle = \int_\Gamma f(q, p) \rho(q, p; t) d\Gamma, \tag{6}$$

gives the values of these macroscopic parameters.[20] Precisely because the macroscopic parameters are insensitive to coarse-graining, we in fact attain the same value for $\langle f \rangle$ whether we use ρ or $\bar{\rho}$.

[18] See, for example, Zwanzig (1966) and Wallace (2015).

[19] Additionally, it is the state that systems tend to approach. One may prefer to define equilibrium by building in this feature of being an attractor state. See, for example, Sklar (1993).

[20] This is the standard way to calculate equilibrium thermodynamic values. Such values, in BSM, are just macroscopic values that specify macrostates (Wallace 2020).

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To summarize, BSM and GSM offer different descriptions of the same physical system at the statistical-mechanical level; in particular, they differ in whether such descriptions should involve probability. For GSM, probability or ensemble is indispensable to describe the system and define key notions like entropy and equilibrium. For BSM, it's not.

3. The ontology-first approach and BSM

In this section, I argue that BSM assumes and relies on the ontology-first approach to reduction. To clarify, I do not mean to argue that Boltzmannians just happen to hold the ontology-first approach. Nor do I mean to argue that BSM is entailed by this approach. What I mean is something conceptual: in order to make sense of BSM, we need to assume the ontology-first approach.

I'll first show how BSM directly appeals to ontological reduction, more specifically, an ontological relation of reduction that holds between thermodynamic and statistical-mechanical systems. If it were the case that BSM instead assumed the theory-first approach, the ontological relation of reduction would be secondary or derivative and thus would not appear directly in BSM.

Recall how the key concept in BSM, *macrostate*, is related to *microstate*: a microstate corresponds to a unique macrostate, while a macrostate is compatible with many different microstates. How is this relation justified? The obvious justification appeals to ontological reduction. It is because of the ontological relation of reduction (say, the composition relation between chlorine gas and chlorine molecules) that a microstate of the molecules and the corresponding macrostate of the gas are just two descriptions of the same system and these two descriptions are related in this particular way. If the ontological relation of reduction were not assumed, the fact that there is a relation between macrostate and microstate would not be natural and obvious, and we would request some other justification as to why macrostate and microstate are related in this particular way. But no such request has been made.

Instead, Boltzmannian advocates are explicit that ontological reduction is taken to be an assumption in their discussions of inter-theoretic reduction between thermodynamics and SM. For example, Callender (1999, 366) claims:

> We know that … the actual gas has a microstate *X*. We also know that *X*, whatever it is, gives rise to the macrostate *M* we see before us. These are merely the assumptions we make when we say thermodynamics is in some sense reducible to mechanics. They are completely uncontroversial. Surely, the gas has a microstate, and surely whatever microstate it occupies corresponds to the macrostate we see.

Moreover, Callender distinguishes ontological reduction from inter-theoretic reduction—only the latter poses a real problem for reducing thermodynamics to SM, whereas it is an uncontroversial assumption that thermodynamic systems are "ontologically reduced" to mechanical systems (1999, 351):

> Thermodynamic systems—like chairs, tables, and similar systems picked out by our common object language—are nothing more than complicated

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> arrangements of physical properties. Very few would disagree with this . . . . In this weak sense, thermodynamics is *already* "ontologically reduced" to mechanics.

Frigg (2008, 104), to take another example, points out that reduction between a macrostate and a microstate is an assumption in BSM and characterizes this ontological reduction relation in terms of supervenience:

> It is one of the basic posits of the Boltzmann approach that a system's macro-state supervenes on its fine-grained micro-state, meaning that a change in the macro-state must be accompanied by a change in the fine-grained micro-state.

One potential concern: even if BSM assumes ontological reduction, it does not mean that BSM assumes the ontology-first approach. That the theory-first approach takes inter-theoretic reduction to be prior to ontological reduction does not mean the approach is incompatible with there being an ontological reduction relation. It may well be the case that (a) BSM assumes both the theory-first approach and ontological reduction, which is just secondary to, or derivative from, inter-theoretical reduction, or (b) BSM assumes both ontological and inter-theoretic reduction and takes them to be on par (that is, neither is prior to the other).

(a) and (b) are possible but not plausible. If (a) were true, the role of ontological reduction in BSM could thus be fulfilled by some kind of inter-theoretic reduction. That is to say, BSM would be presented or at least could be reformulated in a way that does not directly appeal to ontological reduction; a more straightforward justification for the microstate–macrostate relation would appeal to, say, how the *dynamics* at the macro-level is related to the *dynamics* at the micro-level. However, this is not how BSM is presented, and it is unclear, or at least not obvious, how this can be done.[21] If anything, it goes the other way around: the Boltzmannian justifications or derivations of the second law or the dynamical equations of thermodynamics assume ontological reduction between microstate and macrostate. Inter-theoretic reduction in BSM is thus not primitive but something derived. Consequently, it is—contra (b)—not on a par with ontological reduction, given that the latter is assumed as primitive in BSM.

4. The ontology-first approach and Boltzmannian criticisms of GSM

The role of the ontology-first approach is even more explicit in Boltzmannian criticisms of GSM.

4.1. Problems of ensemble and probability

First of all, Boltzmannians criticize GSM for taking "ensembles of infinitely many systems" as its core object of study, in particular, for using ensembles to represent *actual individual* systems (e.g., Callender 1999; Goldstein et al. 2020). In fact, some Boltzmannians describe GSM as "ensemblist," in contrast to their own framework

[21] One such justification may be as follows: a macrostate is chosen because carving up the phase space this way gives rise to a robust and autonomous dynamics. But it's unclear what such a dynamics might be in BSM, or it may unavoidably involve probabilities.

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being "individualist" (Goldstein 2019). The criticism goes as follows. SM should be about actual individual physical systems. An ensemble, which is a collection of infinitely many systems, is neither actual nor individual. More specifically, equilibrium and entropy are supposed to be properties of an individual system. But if they are defined in terms of probability distributions over ensembles, an individual system can no longer be said to be in equilibrium or have certain entropy. Moreover, we cannot infer the behavior of an individual system from the behavior of an ensemble (Frigg 2008, 174). Thus, actual individual systems cannot be represented by ensembles.

Gibbsians have an immediate response to this criticism: an ensemble is only a fictitious set of all possible microstates of the system. It is introduced merely for convenience or as a heuristic. GSM can be presented without mentioning 'ensemble': statistical-mechanical systems are represented directly by probability distributions over phase space (Wallace 2020).

But this is criticized by Boltzmannians as well. They argue that *actual*, *individual* statistical-mechanical systems cannot be represented by probability distributions. For example:

> The problem is not the use of ensembles ... The problem is instead thinking that one is *explaining* the thermal behaviour of *individual real systems* by appealing to the monotonic feature of some function, be it [of] ensembles or not, that is not a function of the dynamical variables of real individual systems.
>
> It is impossible to calculate the intellectual cost this mistake has had on the foundations of statistical mechanics. (Callender 2001, 544; emphasis in original)

For Callender, any function that is not "a function of the dynamical variables of real individual systems" is inadequate to be a part of the explanation for the thermal behaviors of actual individual systems, and probability is one such function.

Why can't probability distributions represent "individual real systems"? A probability distribution describes how likely it is for a system to be at one of the many possible microstates. But at any given time, there is only one definite microstate at which the actual system can be. Goldstein (2019, 443) thus asks:

> What, after all, does *the* probability distribution μ_t of our system at a given time refer to? What in fact is its actual probability distribution? I'm aware of no plausible answer to this question.

By pointing out that a nontrivial probability distribution over many possible microstates does not refer to anything actual, Goldstein is effectively arguing that it is problematic to use probability to represent an actual system.

(One may argue that an actual individual system can be represented by probability, if probability is interpreted as subjective in the sense that it measures how much we know about the system. In that case, Gibbs entropy, which is defined in terms of probability, would be subjective as well. However, the reason why thermodynamic entropy of an isolated system does not decrease cannot be subjective, because that fact holds regardless of how much we know about the system. Thus, Gibbs entropy as a

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subjective notion is not adequate to capture this objective fact about the thermodynamic entropy (e.g., Albert 2000). Accordingly, interpreting probability as subjective is not a viable solution to the problem that probability can't represent actual individual systems.)

The key to the Boltzmannian criticisms of the Gibbsian use of ensemble and probability lies in their claim that SM should be about actual individual systems. If a framework of SM is not about individual systems for whatever reason, it is plainly a drawback of that framework. Here's Callender (1999, 357; emphasis added):

> Thermodynamics states that once an isolated system achieves equilibrium, it stays in equilibrium forever ... Boltzmannian SM ... abandons the idea that equilibrium is stationary in time. The Boltzmann approach balances this affront to thermodynamics by retaining the idea that equilibrium and entropy are properties of *individual systems*. The Gibbs approach *pays for* its strict agreement with the thermodynamic laws by relinquishing the idea that entropy and equilibrium are properties of *individual* systems.

For Albert (2000, 70), it is just "sheer madness" that entropy, equilibrium, and the laws of thermodynamics are associated not with individual systems but with ensembles or probability.

Why is SM supposed to be about actual individual systems? We can answer this if we take scientific theories to be primarily about objects (section 1.3). Then it makes sense to think that appealing to objects and their actual states is the only admissible way to characterize a given physical system, and appealing to "some abstract entity," such as probability (Maudlin 1995, 147), is not. Accordingly, the core object of study of a physical theory cannot be a fictitious collection of many possible states. Because taking theories to be primarily about objects is a sufficient condition for the ontology-first approach (section 1.3), this approach needs to be assumed as a consequence. In sum, to justify their claim that SM is supposed to be about actual individual systems in this way, Boltzmannians assume the ontology-first approach.

Maudlin (1995, 147) gives a slightly different explanation, which arguably appeals to ontological reduction:

> Since phenomenological thermodynamics originally was about such individual boxes [of gas], about their pressures and volumes and temperatures, 'saving' it by making it be about probability distributions over ensembles seems a Pyrrhic victory.

That is, because thermodynamics is about individual systems, SM is supposed to be about individual systems as well; making SM be about probability distributions, even though it preserves thermodynamics, has the cost of making SM no longer be about individual systems; this cost is so devastating that it is tantamount to defeat. But why is SM supposed to be about individual systems? Simply because thermodynamics is about individual systems? If the ontology-first approach is assumed, then ontological reduction acts as a constraint on what the ontology of a theory could be (section 1.3). Given that there is an ontological relation of reduction between thermodynamic and statistical-mechanical systems and that thermodynamic systems are individual systems, statistical-mechanical systems should be individual systems as well.

4.2. Problems of coarse-graining

Moreover, GSM is criticized by Boltzmannians for its use of coarse-graining without proper justification. For example, Callender (1999, 360) argues that the sole purpose of coarse-graining is to get a notion of Gibbs entropy that monotonically increases in time (i.e., the Gibbs coarse-grained entropy; see eq. 5). That is to say, the coarse-graining projection operator J is chosen opportunistically, and thus the Gibbs coarse-grained entropy is introduced without any justification apart from it matching the increase of thermodynamic entropy in time.

BSM, however, also employs coarse-graining without providing a justification. Although Boltzmannians do not always use the word 'coarse-graining', the idea of partitioning the phase space into finite regions is employed. For example, the definition of Boltzmann entropy appeals to coarse-graining:

> [W]e define the Boltzmann entropy S_B for the actual microstate of an individual system. Consider some microstate X. X corresponds to a macrostate $M(X)$, which, in turn, is compatible with many different microstates. We wish to determine the relative volume in [the phase space] corresponding to all the microstates giving rise to M. To accomplish this, we must partition [the phase space] into compartments such that all of the microstates X in a compartment are macroscopically indistinguishable. (Callender 1999, 355)

Hence, Boltzmannians apply a double standard in criticizing the Gibbsian use of coarse-graining.[22] What justifies this double standard? A plausible answer is that BSM tacitly assumes the ontology-first approach. This assumption licenses BSM to take ontological reduction as given, which justifies its choice of coarse-graining, more specifically, its choice of partitioning the phase space into "macroscopically small but microscopically large cells" (Goldstein 2001, 42). This particular choice of the size of the cells can be justified, as Frigg (2008, 135) points out, if there exists an objective separation of the relevant macroscopic and microscopic scales. A reduction relation between objects at a microscopic and a macroscopic scale provides just such a natural and objective micro–macro separation. It's simply "carving nature at its joints" that there are such objects at such and such scales. The two scales involved in coarse-graining are thus not chosen arbitrarily, but are picked out because they are ontologically significant (i.e., they are associated with the relevant ontologies in the relevant reduction relation). Put another way, the choice of coarse-graining is justified because it gives rise to the right higher-level objects, which are marked off by what is macroscopically indistinguishable and what can be meaningfully measured. Without the assumption of ontological reduction, it would be puzzling why being macroscopically indistinguishable matters to any individual microstate.

GSM cannot appeal to the same kind of justification for coarse-grained probability density. It is unclear in what sense probability distributions are ontological and stand in an ontological reduction relation with individual microstates. Indeed, this is exactly what GSM is criticized for—under the assumption of the ontology-first approach (section 4.1). Therefore, Boltzmannians would argue, there is no relevant

[22] One exception is Frigg (2008, 134–135).

ontological reduction relation for GSM to employ to justify their choice of coarse-graining.[23]

5. The theory-first approach and GSM

It is subtler how GSM is related to the theory-first approach. Unlike the case of, say, consciousness, there is a relatively clear and uncontroversial ontological reduction between thermodynamic and statistical-mechanical systems. (The Boltzmannian-vs.-Gibbsian debate is not about whether, say, gas is composed of molecules.) The ontology-first approach thus appears to be a prevailing assumption in discussions of SM, including those that are more on the side of GSM. For instance, Malament and Zabell (1980, 341) claim:

> Every one of these [thermodynamic parameters], presumably, is uniquely determined by the exact microstate of the gas. That is our fundamental reductionist assumption.

This quote suggests that the ontological reduction relation between thermodynamic and statistical-mechanical systems is assumed even in a Gibbsian discussion (although it is unclear what exact role this assumption plays in their argument). Thus, I do not intend to argue that GSM assumes and relies on the theory-first approach. Rather, I argue that GSM is immune to certain criticisms insofar as it assumes the theory-first approach. In fact, GSM can be vulnerable to those criticisms discussed earlier exactly because the criticisms are taken from the point of view of the ontology-first approach. Hence, for GSM to be a coherent and valid foundation for SM, it should assume the theory-first approach.

The main reason for GSM to adopt the theory-first approach is that it permits the use of probability to represent statistical-mechanical systems. GSM is the standard tool used among working physicists (section 2.2). It is more efficient than, say, classical mechanics at describing patterns, making predictions, and providing explanations in certain domains. If the theory-first approach is assumed, the efficiency and usefulness of GSM warrant its status as a viable and physically significant theory. Accordingly, what its core object of study is, or how a system can be represented, is not constrained by any pre-theoretical or metaphysical considerations (section 1.3). Consequently, GSM cannot be ruled out as a viable physical theory *just because* probability does not fit with the familiar ontological reduction relation between gas and molecules. In other words, without the ontology-first approach, it is unclear why systems cannot be represented by probability distributions (section 4.1).

In particular, if we adopt something along the lines of Dennet's (1991) pattern theory, the representational role of probability can be justified in terms of its function in describing the dynamical patterns picked out by GSM. What Dennett's theory contributes is to explain and justify how a new higher-level ontology (in this case, probability) can emerge in terms of some real patterns,[24] even if such an ontology is

[23] In section 5, I sketch a justification for coarse-graining in GSM following Wallace (2015, 2020) and Robertson (2018), who arguably works with the theory-first approach.

[24] Roughly speaking, there is a real pattern if and only if there is some more efficient way to describe certain phenomena than specifying every single detail.

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not reduced to a lower-level ontology in a way we are familiar with (such as composition or identity). This way for probability (or whatever it represents) to emerge as a higher-level ontology is unsuited for the ontology-first approach, which requires that ontological reduction be taken as primary. If there is any ontological reduction relation between probability and lower-level objects like molecules, it would not be a standard one (like composition), especially because probability distributions are not located in ordinary physical space. Such a new ontological reduction relation needs to be either postulated as primitive or further justified. It's hard to see how the former can be motivated. In the latter case, the ontological reduction relation in which probability stands (if there is any) would not be primary but likely dependent on and secondary to the inter-theoretic reduction relation.

The second reason for GSM to adopt the theory-first approach is that it provides a way for GSM to justify its choice of coarse-graining. Again, it is according to the theory-first approach that GSM establishes its status as a physical theory via its usefulness and efficiency. A typical way for a physical theory to have these virtues is to identify a new *robust and autonomous* dynamics (section 1.3). In our case, an appropriately chosen coarse-graining procedure can give rise to such a dynamics for the relevant degrees of freedom. The coarse-graining projection operator J decomposes the original probability ρ into a relevant part $\bar{\rho} = J\rho$ and an irrelevant part $\bar{\rho}_{\text{irr}} = (1 - J)\rho$. What is special about this decomposition is that there turns out to be autonomous dynamical equations for $\bar{\rho}$ (namely, the coarse-grained probability). In a sense, J throws away the part of ρ that is irrelevant to the new dynamics. The existence of such dynamics thus justifies the particular choice of coarse-graining. Contrary to what Boltzmannian critics think, coarse-graining in GSM is chosen not *just* to match the increase of thermodynamic entropy.

Because the dynamics of $\bar{\rho}$ is obtained from the corresponding theory (i.e., GSM), this justification for coarse-graining appeals to inter-theoretic reduction instead of ontological reduction. As discussed earlier, if there is any ontological reduction in which $\bar{\rho}$ stands, it would be dependent on the inter-theoretic reduction relation. That's why this justification would not work under the ontology-first approach. Worse, the ontology-first approach would question if the dynamics of $\bar{\rho}$ is even physical, since $\bar{\rho}$ does not evolve in ordinary physical space.[25] In contrast, the theory-first approach imposes no constraint on the emergence of a new dynamics (i.e., the time evolution of $\bar{\rho}$, a real pattern) or of a new higher-level ontology picked out by that dynamics.

Lastly, adopting the theory-first approach permits a broader understanding of SM than is conceived of by the ontology-first approach, and this broader understanding is more congenial to GSM than BSM. The primary task of BSM is taken to be to provide a microphysical description and justification of thermodynamics (section 2.1). In contrast, the scope of GSM goes beyond that (Wallace 2015): it contains a collection of techniques that are used to model all kinds of systems (including gases, liquids, solids, magnets, and plasmas) and phenomena (e.g., Brownian motion and black-body radiation), and has a remarkably broad application (e.g., in the theory of neural networks [Bahri et al. 2020]).

[25] Thanks to Valia Allori for this point.

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If the ontology-first approach were assumed, it would be natural to think that the primary task of SM is *just* to provide a microphysical foundation for thermodynamics —since there are microscopic constituents of thermodynamic systems (i.e., there is an ontological reduction relation), there should be a theory of those constituents that can explain, justify, and in principle, make predictions about thermodynamic systems. Such a theory is physically significant *because* it is about the right ontology (i.e., the microscopic constituents of thermodynamic systems) and provides a microphysical foundation for thermodynamics. A theory that does not do so would *not* be physically significant and would be seen only as mathematical or instrumental. If the collection of techniques in GSM is not essentially about the microscopic constituents of thermodynamic systems and their behaviors, one would question if those techniques carried any physical significance. The ontology-first approach, accordingly, does not support a broader understanding of SM, such as given by GSM, that goes beyond providing a microphysical foundation for thermodynamics.

In contrast, if the theory-first approach is assumed, a theory establishes its status as a *physical* theory via its usefulness and efficacy. GSM can thus stand as a successful physical theory on its own, and its physical significance is justified via its usefulness and efficacy (along with providing a microphysical foundation for thermodynamics). Consequently, its broad scope would not be restricted to merely providing a microphysical foundation for thermodynamics.

To clarify, that GSM should assume the theory-first approach does not suggest that GSM conflicts with the presence of any familiar ontological reduction relation, such as the composition relation between chlorine gas and molecular chlorine. Recall: according to the theory-first approach, ontological reduction (if there is any) follows from inter-theoretic reduction. Hence, this approach is compatible with there being an ontological reduction relation; it's just that such ontological reduction should be conceived of as secondary to inter-theoretic reduction.

This also clarifies why my thesis is not that BSM aims for ontological reduction while GSM aims for inter-theoretic reduction, but instead concerns the relation between the two kinds of reduction. Both frameworks aim at providing, or at least accommodating, ontological as well as inter-theoretic reduction. The question is how it is done. BSM accounts for how thermodynamics is reduced to SM based on ontological reduction, whereas GSM appeals to that inter-theoretic reduction to accommodate ontological reduction.

This is compatible with the view that ontological and inter-theoretic reduction are interdependent and complementary. If one holds such a view, they would also think BSM and GSM are not competing but complementary. That being said, it does not undermine the value of drawing the distinction between the ontology-first and the theory-first approach. These two approaches can be seen as spelling out exactly how ontological and inter-theoretic reduction are dependent on each other (section 1.3). Accordingly, my analysis would suggest how and in what sense BSM and GSM complement each other.

6. Concluding remarks

I proposed a distinction between the ontology-first and the theory-first approach to reduction. I demonstrated the significance of this distinction by explaining how it

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plays a role in discussions of BSM and GSM: the disagreements between them essentially arise from the disagreement between the two approaches. A discussion on whether the ontology-first or the theory-first approach is the *correct* approach to reduction, in the context of reducing thermodynamics to SM, can then determine, or at least help us gain more insights into, whether BSM or GSM is the right framework for SM.

The significance of this distinction is not limited to SM or physics. The distinction can, for example, shed light on discussions on the interpretations of quantum mechanics. Arguably, the justification for Bohmian mechanics assumes the ontology-first approach (see brief discussions in section 1.3), whereas the Everettian interpretation needs to assume the theory-first approach. Outside physics, the distinction could be useful for understanding instances of reduction in biology, chemistry, philosophy of mind, and so on.

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