Infinite Idealization and Contextual Realism

Chuang Liu
Department of Philosophy of Science and Logic, Fudan University
Department of Philosophy, University of Florida

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Abstact

The paper discusses the recent literature on abstraction/idealization in connection with the "paradox of infinite idealization." We use the case of taking thermodynamics limit in dealing with the phenomena of phase transition and critical phenomena to broach the subject. We then argue that the method of infinite idealization is widely used in the practice of science, and not all uses of the method are the same (or evoke the same philosophical problems). We then confront the compatibility problem of infinite idealization with scientific realism. We propose and defend a contextualist position for (local) realism and argue that the cases for infinite idealization appear to be fully compatible with contextual realism.

1 Introduction

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When philosophers of science talk about science today, they are most likely talking about a social activity that comprises a highly disciplined and distinct practice that produces what we regard as the best example of knowledge. This body of knowledge has many disciplines and each discipline at least an experimental component and a theoretical component. The theoretical component contains, *inter alia*, claims that are true (which means what they say correspond to what exists or happens in reality). Philosophers are divided on which sort of claims can obtain truth of this correspondence sort. Scientific realists believe and think they have good reason to believe that highly abstract and theoretical claims are about existing entities and can be true (or false) of their behavior. Anti-realists are skeptical about such a belief and only willing to concede truth to claims about the observable. Among the arguments for scientific realism, the inference-to-the-best-explanation argument (or its variations) stands out. According to Boyd's version of the argument(Boyd, 1983), the instrumental reliability of scientific practice would be a miracle if the theories such practice essentially depends on were not true or approximately true.

[1]First, let me thank Samuel Fletcher and Patricia Palacios, without whom this paper would not have been written. I also thank three anonymous referees for their constructive criticisms.

The qualifying phrase "**approximately true**" is added to accommodate the ubiquitous situation where a scientific theory rarely contains true claims simpliciter. They rather contain true enough claims which later developments can and do improve upon. Instrumental successes in the history of human civilization rarely require truth. Approximate truth that matches the levels or degrees of practical or instrumental discrimination suffices to explain the practical or instrumental successes and therefore provides reason for realism that tolerates approximate truth.

Looking independently at scientific theories, it is difficult to miss one of their crucial features, a feature almost all highly developed and especially highly mathematical theories have in common. And the feature is **idealization**. Idealization is an act in coming up with a scientific representation (e.g. a model) or a theory that **distorts** a chosen aspect or property of a system (the target system) such that the representation or theory becomes manageable. For example, the idealized frictionless plane (assuming no such planes exist in reality) is a distortion of a real plane such that a model of a frictionless block moving uniformly and perpetually on it becomes possible. Idealizaiton may also be used by philosophers of science to refer to the products of the acts of idealization (in our sense). The frictionless plane, the object, may be called the idealization rather than the act of completely smoothing out an actual plane *in imagination* so as to arrive at a mental picture of frictionless plane. We do not regard this as an important issue to settle. The word "idealization" may well be left harmlessly ambiguous as many words in a language are naturally ambiguous. Idealization may also be used to refer to both the act and its products.[2]

Abstraction is also a widely used method in theory construction. Abstraction differs from idealization in that by abstraction one selects some aspects or properties of a system for studying (while neglecting others), while by idealization, such selected aspects or properties often need to be idealized for use. Let us consider a block moving on a plane. While abstraction allows us to select only the mechanical aspects of the block and the plane on which it moves, making the block and the plane frictionless is a further act, and it is a mental act of distortion on the chosen, i.e. mechanical, property. While a model as a result of abstraction alone is a simplified system (as opposed to the target system), it does not necessarily contain distortion or alteration of any properties. Therefore, claims produced from the abstract model can be *literally true*, as long as the claims are about the chosen properties and/or their relations alone. Idealization does imply a distortion and therefore renders claims about the modeled properties literally

[2]This point is for responding to a criticism we received that finds our using "idealization" to refer to the act or practice rather than the product as needing an argument to defend.

false [3] As with idealization, the word "abstraction" as a term of art may also be left ambiguous, referring either to the act of abstracting or the product of abstraction.

Another marked difference between abstraction and idealization is that the former is usually a categorical method, meaning picking out (an abstracted) category or type of objects by selecting a group of properties, while the later is usually quantitative and very often infinitely or infinitesimally quantitative, meaning the distortion of a property is by continuous degrees and the end of the distortion is very often a point of limit. Smoothing out a rough and actual plane in imagination is by degrees and continuously so, and the frictionless plane can be regarded as either infinitesimally close to frictionless or the point of limit as the smoothing act is carried out indefinitely. Therefore, the close connection between idealization and infinite idealization is not just a simple relation between a genus and a species, but more as that the latter is a further articulation of the former. If one insists, one can plausibly argue that all idealizations are infinite idealizations in some form or shape. However, we do not endorse this last move and do not think it is either necessary or beneficial philosophically to hold such an extreme view.

Therefore, there is a natural and close connection between a general discussion of idealization as an essential tool of scientific practice and discussing the issue concerning infinite idealization. Closely connected with the discussion of infinite idealization is the recent literature on the "paradox of phase transitions," in which the necessity and nature of taking the thermodynamic limit is debated. We conduct In Section 4 a survey of the various contributions to the debate and comment on the various moves.

The apparent paradox is created when one has, on the one hand, a thermodynamic account of phase transitions (as such common phenomena as water boiling and ice melting) involves indispensable non-analytic points while, on the other hand, a statistical mechanical account of the same phenomena can only recover such points if the model systems are of infinite sizes (with finite density). The latter is an idealization of molecular thermo-systems that appears to cross over to the impossible. We shall use this as a case-study for the facets of infinite idealization. We then discuss other sorts of infinite idealization and clarify their essential differences.

Section 5 brings out the challenges of these cases to scientific realism. We survey a couple of most prevalent formulations of scientific realism and explain how four challenges that realism so construed are most likely to face. A detailed analysis of why infinity poses a special problem with accommodating realism is then given. For

[3] Here we assume a correspondence theory of truth since we are mostly dealing with issues relating to scientific realism.

instance, we give a detailed study of how an otherwise plausible account of confirmation of idealized theories (namely Laymon's theory) that is arguably compatible with scientific realism encounters problems with infinite idealization.

In section 6, we propose and defend a contextualist position on realism claims. We endorse Dummett's local version of realism that deals with questions of existence or truth of objects or claims of disputed classes. Then with regard to claims under infinite idealization, we investigate the truth conditions for realist claims, such as the existence of phase transitions as taking place in infinite systems and the truth about claims concerning such systems. We then argue that the truth conditions of such (realist) claims depend on the contexts in which such claims are evaluated; and the the contexts are determined by the fundamental or grounding or "anchoring" abstractions and/or idealizations. Scientists may be inclined towards realism, but they are not necessarily realists in the same universal or global context regardless of their areas and disciplines (case in point, many physicists working with quantum theory may be perfect realists except when regarding quantum theory, *per se*). We first discuss the cases of infinite idealization to introduce and illustrate the aspects of contextual realism, and then we articulate and defend it more generally.

Before we get to the main arguments of this paper, we define and defend, in sections 2 and 3, two closely associated distinctions as preliminaries: the distinction between the KT approach and the SM approach; and the distinction between idealization (associated with the KT approach) and abstraction (associated with the SM approach).

2 Two Different Approaches to Thermo-Phenomena

In order to fully appreciate the paradox of phase transition, a closer look at the relationship between thermodynamics (TD) and statistical mechanics (SM) is in order. Mainwood (2005) is perceptive when he made in his article a distinction between a "reduction problem" and an "idealization problem." The reduction problem with respect to such phenomena as phase transition and critical phenomena is a problem of how a TD account is related to a SM account such that one or another conception of inter-theoretical reduction is exemplified. What we want to point out at the outset of our discussion, however, is that the reduction of TD to SM (or the recovery of TD from SM) cannot be simply understood as a reduction of a phenomenological account (as the TD account) to a mechanical account in terms of a detailed account of the molecular motion of the same system. An example of such a reduction would be Boyle's account of temperature, pressure, and the like of an ideal gas. The motion of molecules are highly idealized, and yet the derivations are fully mechanical in that temperature and pressure, for instance, are no more than algebraic aggregates of idealized mechanical accounts of individual molecular motion. Boltzmann's kinetic theory of gases may be regarded as another example of the same sort of reduction. The SM reduction of TD is not such a reduction. SM is not a mechanical theory of molecular motion because it does not tell us anything about the details of the molecular motion inside a gas or liquid. It is a much more *abstract* account than a mechanical one.

In the study of thermo-systems (systems that comprise large number of much smaller components), one must make a choice of two different approaches, which may not even be compatible. The two approaches we discuss here are conceptual rather than historically actual (although they do have distinct historical origins), and we refer to them as the *KT (kinetic-theoretic) approach* and the *SM (statistical-mechanical) approach*. One approach is to make drastic and entirely unrealistic assumptions about the molecular motion, and the other is to make little or no assumptions about them in particular. If we may regard Boyle's ideal gas model as an exemplar of the former approach, which is a highly idealized model in dealing with a thermo-system, i.e., diluted gas, we must call Gibbs's SM treatment/approach of the same system "non-idealized." The point of SM is to make as little idealized (therefore obviously false) assumptions about the thermo-system as possible, and still rigorously recover the TD account of the system's thermo-behavior. This is why when modifying the end products (theories or models) of these two approaches, radically different conceptions have to be employed.[4]

Boyle's model can be replaced by van der Waals's model by removing some of the idealizations, such as zero-size of all molecules and no interactions among molecules. When an estimate (also idealized) of size and interaction as simple coefficients or constants is added to the van der Waals equation, an improvement of the model/theory is made.[5] On the other hand, no improvement of this sort can be introduced to made a SM account better. The only window through which any microscopic details of the studied system can be glimpsed is the form of the Hamiltonian for the system. Giving up obviously false idealizations about the components means to make the Hamiltonian

[4] There are numerous excellent standard texts of statistical mechanics in physics, but the work we rely on in this paper, a work in which an account of how the use of probability allows people such as Boltzmann and Gibbs to come up with an abstract study of thermo-systems, is Guttmann (1999). Our discussion of SM depends heavily on this work.

[5] I thank a referee for pointing out that in (Morrison, 2005) this traditional or textbook interpretation of the relationship between Boyle's model and van der Waals's model is challenged. Since the traditional interpretation is adopted here so that the case serves to make a distinction, I do not feel it is necessary to discuss the controversy. Granted, a better case whose interpretation is not subject to any controversies would have better served the purpose.

function as general and lack of particulars as possible. Once the Hamiltonian is determined, the partition function is determined, and all thermo-behavior is determined by the algebraic relations of the various partial derivatives of the partition function. Figuratively speaking, all the information about a thermo-system is packed into the partition function, which carries within it the Hamiltonian of the system. One may choose another coupling constant to represent the nature of interaction between two randomly chosen elements of the system, so as to chose a different Hamiltonian. Other than that, the SM account of system is entirely fixed. No considerations of the size, the variation, the overall structural features, etc. of the system and its components can enter into any attempt to modify or improve upon the SM account/model. Obviously what is said so briefly here is not accurate. A more accurate account comes later when we discuss the details of the SM account of phase transitions.

Some caveats have to be observed before we can understand and properly defend the above.[6] First of all, if the KT approach is not much more than what is historically found in the kinetic theory of gases, it could not serve as a counterpart of the SM approach, which is essential the actual approach currently used in statistical mechanics. For example, it would be of no use in discussing several important cases (which we will later come to) of phase transitions and critical phenomena, such as the transition from para- to ferro-magnetism. The concept of the KT approach, in contrast to the SM approach, is not supposed to be an actual historical approach. It is rather a historically based conceptual category that emphasizes the kinetic nature of molecular movements (including small vibrations around a fixed point). We can imagine a rigorously implemented microscopically detailed mechanical account of the molecular arrangements and movements of all molecules in a body, where the body could be a gas or a liquid, or a solid. By properly idealizing the appropriate properties of these molecules, we build a reasonably workable model of the body. A recovery of the body's TD properties and the relations among the properties, such as entropy, pressure, and heat capacity, can be carried out with minor help from the theory of probability. It is this approach that is diagonally opposed to the SM approach. Second, it is certainly not true that the SM approach, though heavy in abstraction, does not contain any idealization. We will not list here all the hidden idealizations that go with the SM treatment of a thermo-system, but obviously the assumption that all molecules/elements in a system are alike and interact randomly and the interaction can be characterized by a single (or a few) Hamiltonian(s) is an idealization. The actual system is presumably more heterogeneous. These idealizations, as we argue extensively later, in connection with

[6]We thank a referee for raising the point that is dealt with in this paragraph.

our defense of contextual realism, are special idealizations. We call them anchoring or grounding idealizations. They are idealizations that isolate a discipline in science within which claims about what entities exist and what entities do not are determined. They are different from regular idealizations that scientists use within a discipline or area of inquiry.

It is not entirely unreasonable to separate these two ways of reduction with respect to the thermodynamic and the mechanical relationship. I shall call the former the *idealization approach* and the latter the *abstraction approach*. Given what we have said about the distinction between the two, here are some further reasons why this is a good idea.

3 The Distinction between Abstraction and Idealization

In an excellent general analysis of the two notions: idealization and abstraction, Godfrey-Smith (2009) (see also, Jones (2005),Woods and Rosales (2010)[7], Knox (2016)) begins with an analysis of the phenomena in the practice of science rather than how the two terms are discussed in the philosophical literature. Godfrey-Smith comes up with the following distinguishing features of the two types of activities. Idealization is usually associated with "treating things as having features they clearly do not have[,]" while abstraction is an act "leaving things out, while still giving a *literally true* description (p.1, my italics)." Further, abstraction is said to result in a simplified but still literally true or faithful representation of the target system, while idealization is a product of imagination that usually results in a fictional system that are literally false but often approximately true of the target system.

Godfrey-Smith's insight that "abstraction" should be reserved for denoting a practice that results in simplified yet true/faithful representation, while idealization necessarily brings in **distortion** rings true. However, it still does not solve the problem of when to call a model an idealized one and when to call it an abstraction. Trying to tell which is fictional and which is not does not seem to help. Here is our amendment of Godfrey-Smith's account. First, one should notice that idealization and abstraction are usually associated with different products. Models are idealized but equations and statements, especially mathematical statements, are abstract. This is of course not always true, for models can be abstract as well. But the difference is that a model

[7](Jones, 2005) is a discussion of the distinction that predates Godfrey-Smith (2009) but is of a more limited scope, dealing mostly with the distinction between idealization and abstraction in modeling practice. (Woods and Rosales, 2010) is a wonderful study of different sorts of distortion in model-building including abstraction without emphasizing the distinction.

is often an object that must have sufficient details while statements can be made on one or another aspect or part of the model system. Idealized models are often used for scientists to make abstract claims about how the modeled system behave. And if only abstraction is essential to such claims while idealization not essential, the claims can be literally true even if the model is highly idealized. A claim about the pressure of a diluted gas can be entirely true, not approximately true, when it is derived from Boyle's ideal gas model. The model is highly idealized but since the equation for the gas pressure is abstract enough to be insensitive to the idealization, the claims can be literally true and agreeing with the experimental result within the margin of error.

This way of looking at the distinction, namely, idealization builds models while abstraction concerns the claims that can be generated from the models that are nevertheless literally true, makes further sense when we realize that models are not always concrete systems that are the results of imagination. A set of equations can be regarded as a model. In that case, idealization may not even enter in the "equations." The equations are claims that are true, although they are highly abstract. When scientists pick properties from a complex target systems and try to find true claims to say about them, while not worrying about what the target system is like, abstraction is the act. It is not idealization that is at work there. But when a concrete and complete system is "build" from the target system, a model in "flesh-n-blood," distortion is usually necessary, and the result is usually a simplified but concrete system, often a geometric or structural system.

Based on the above amendment of Godfrey-Smith's account of the distinction, which is an account of the acts in the practice of science, not one for the semantics of the terms, we may say the crucial difference is this.

- Idealization is an act that distorts chosen features in order to make them simpler in a model representation.

- Abstraction is an act that formulates claims about chosen features as they are, as if in isolation.

Abstraction is therefore more common and less problematic. More common because ordinary language use is full of abstract claims, and less problematic because abstract claims are either true or false, much like claims that do not involve abstraction. Idealization is less common-place but also exists outside of scientific practice. It often involves fictional imagination and the results are not regarded as capable of being literally true. Idealization usually result in concrete models that differ from what they are used to represent, abstraction is more often used to directly make claims about a target system only in selected aspects. Again, abstract claims can be made without the mediation of modeling, while idealization is primarily a method of model construction.[8]

Idealization is made possible in model construction by the fact that idealized models are often resembling in some sense the target system. Abstraction is made possible by the modularity of nature. A ball may be red AND big or yellow AND big. If that is impossible in general, (namely, all properties are holistically correlated such that there can only be red and big balls or yellow and small balls; and that's true for all n-tuples of properties,) then abstraction is impossible as well.

We now return to our discussion of phase transition and infinite idealization with greater depth, now that we are clear about the distinction between idealization and abstraction. The difference we introduced at the beginning, the difference between the two different "reducers" of thermodynamics, should now appear in a better light. The KT account of gases and the like depends heavily on models as the result of idealization of one sort or another. This is what we called the idealization approach. The SM account is more "abstract" than that, and hence the abstraction approach. The account is mostly systems of equations centered around the partition function of a certain ensembles (micro or canonical or grand ensembles) that has a specific Hamiltonian. The SM account only chooses those magnitudes that can be treated statistically and come up with averages that cross over to TD magnitudes. Therefore, it is more applicable to use the notion of abstraction rather than idealization. And as a result, no concrete or semi-concrete imaginary systems of molecules are necessary for coming up with SM results that are true to the TD counterparts.

The above discussion yields results that differ from John Norton's analysis of similar cases, although there are many points of agreements (Norton, 2014, 2012; Batterman, 2005). In his attempt to clear the confusion in the Reductionism debate concerning condensed matter physics, Norton (2012) proposed a distinction between two different senses of reductional levels. One sense is what he calls the "Molecular-Thermodynamic" reductional levels. This is the standard level separation between TD and SM. The other is what he calls the "Few Components-Many Components" reductional levels. Both levels are in the molecular level, and both are treated mechanically. The many-component system is said to be reducible to the few or single component system. The example Norton uses to illustrate this second sense of reductional levels is the

[8] A very similar point is made by Norton (2014, 2012), as we shall see in our discussion of his works below.

ideal gas law. Obviously, the kinetic treatments of thermo-systems of Maxwell and Boltzmann that we have discussed before also belong to this sense. Norton (2014) (see also Norton (2012)) has argued for a different conception of the pair: idealization and approximation. In the context of models and modeling, we can safely characterize Norton's two notions as follows. Idealization is a method that allows scientist to imagine fictional (or real) systems that differ from the target systems in points of idealization. Approximations are propositions (or statements) scientists are able to make regarding the behavior of the (idealized) models. Norton's illustrative example for the two is that the ideal gas law is an approximation while the ideal gas model is an idealization. Further, in his discussion of the infinite idealization cases, Norton uses this distinction to argue that some acts of taking to the infinity limit, such as the infinite time limit for reversible thermal processes of heat transfer, involves no idealization. They are no more than statements of approximation. The indefinitely long time limit that allows nonzero heat transfer between equal temperatures is an approximation of a sequence of indefinitely many steps, at each of which an infinitesimal amount of heat is transferred. And many well known so-called idealization cases, such as a motion on a frictionless surface, Norton argues, are actually cases of approximation that do not necessarily involve idealization.

Norton (2012) also talks about the difference, in the context of infinite idealization, between taking the model system to an infinite size and taking some infinite limit on properties as a mathematical operation. Norton regards the former as idealization because it always results in fictional systems as models. The latter does not necessarily involve idealization or fictional systems. Taking infinite limits on properties usually result in approximation, that is in terms of Norton's formulation of the two terms. Norton also consider cases where the idealization sort of infinite limit taking often produces ill-defined results, while the approximation sort usually does not.

We share with Norton's insight that two different senses of reductional levels ought to be distinguished. But as we argued above, the distinction is not cut in the same manner. Our two senses would be, were we to follow Norton's strategy, 1 SM-TD; 2 KT-TD, where the second is the "kinetic theoretical - thermodynamic" level. And we believe that our introduction of the aspect of abstraction (as amended from Godfrey-Smith account) and the distinction between the idealization and the abstraction approach adds a necessary ingredient to the study of the paradox of phase transitions. Therefore, we believe that idealized models are widespread and essential for the KT-TD reductional study of thermo-phenomena, while in the SM-TD case, abstraction is the main method in handling the molecular systems. This way of looking at these cases naturally yields the result that while most lawlike claims in the KT-TD realm are only approximately true, those in the realm of SM-TD are true. And the fact that the structure of reductional relationship in the SM-TD relation is barely altered when one moves into quantum statistical mechanics testifies to our idea that abstraction is often not sensitive to the mechanical details of the models. And the results of abstraction, but not of idealization, often keep their truth-values when transferred from classical to quantum theories.

Although, as we have emphasized above, our distinction between the KT approach and the SM approach is made conceptually in a hypothetical space of possible approaches or models, and we emphasize the speculative or theoretical nature of the KT approach, there is good historical reason to think that the hypothesis is not pulled out of thin air. Historically, statistical mechanics arose as the need to find a less "makeshift," or more rigorous, method in studying thermo-phenomena from a molecular point of view. Part of this process is to make less unrealistic (here we could understand it as less idealized) assumptions about the complex internal details of the studied systems. Eventually, when SM in the hands of Gibbs emerged, we saw an abstract mathematical theory that assumes little of the internal details except the Hamiltonian characterizing the nature of interaction among the components and some global constraints of the systems, such as the homogeneity and randomness of the interactions. *The less the theory says about the internal details, the less it needs from the help of idealization, and more likely that it provides literally true claims about the systems' behavior.* The claims may be terribly abstract, but they are true in the same sense that a claim such as "it is red" may well be literally true of a system that is incredibly complex in other aspects such as its shape, its structure, and its chemical composition.

SM in the guise of Gibbs is able to achieve such a level of abstraction and produce literally true claims about thermo-systems of a much diverse variety also because it "tucks" away a fundamental justificatory element in the assumption of ergodicity. The intuitive assumption about the equivalence of phase average and time average that guarantees the use of statistical mechanical laws is notoriously difficult to prove, as the history of ergodicity testifies. And yet, the use of the assumption relieves scientists from using individual mechanical idealization conditions to come up with molecular models for thermo-phenomena, as in the cases of the KT-TD relationship. The three different types of ensembles, the micro, the canonical, and the grand canonical ensemble, are only distinguished from each other by the total energy conditions of the systems under investigation. As to the specification of the Hamiltonian, either entirely realistic interactional models are used or a coupling constant is put in by hand whose value can be determined by empirical means.

4 The “Paradox” of Phase Transitions and Critical Phenomena

We shall use the so-called “paradox of phase transitions” as a special case-study to analyze in detail the issues raised in the above discussion. The subject was broached in the 1990s in a series of philosophical/foundational studies of phase transitions and critical phenomena (Callender, 2001; Liu, 1999, 2001; Batterman, 2005). The issues raised in that discussion are recently elevated to the status of “paradox” in an effort to put the ghost to rest once for all (Bangu, 2009, 2015; Mainwood, 2005; Shech, 2013). The conclusion, if there is any conclusion, of the recent investigation appears to be that since we are all clear about what is actually going on with respect to the physical details of the physical process of phase transitions and critical phenomena, the opposing philosophical positions or puzzles over whether infinite idealization is necessary is really a “matter of academics.” We argue that this is not so.

Since there is very little variation or dispute about the accounts of the paradox of phase transition, we shall not engage in a detailed recount. The following sketch, which follows a recent rendition by Bangu (2009) and Shech (2013), should suffice. The TD account of systems comprising a large number of components, such as molecules, is a mathematical treatment of carefully collected results of well-constructed and repeated experiments. They are expressed in mathematical relationships among TD variables, such as temperature, pressure, and entropy. The most illustrative and yet rigorous display of the relationships are functional co-variation relations. Phase transitions are common phenomena that people recognize in daily life without any help of science. The transitions among the three states of a liquid, such as water, is perhaps the most commonly observed and known phase transitions. Less known are transitions between phases of magnetization in metals, such as a transition from paramagnetism (i.e. removing the source of magnetization removes the effect of magnetization) to ferromagnetism (i.e. some effect of magnetization remains after the removal of the source). The following diagram, Figure 1, shows the phase structure of a liquid.

This is a pressure versus temperature (with constant volume) diagram, showing how water in a container change its phases. If we concentrate on the boulder of liquid and gas and come up with a pressure versus volume diagram of isothermal curves, namely, each PV curve plots the change of P over V at the same temperature, then we have the diagram as shown in Figure 2.

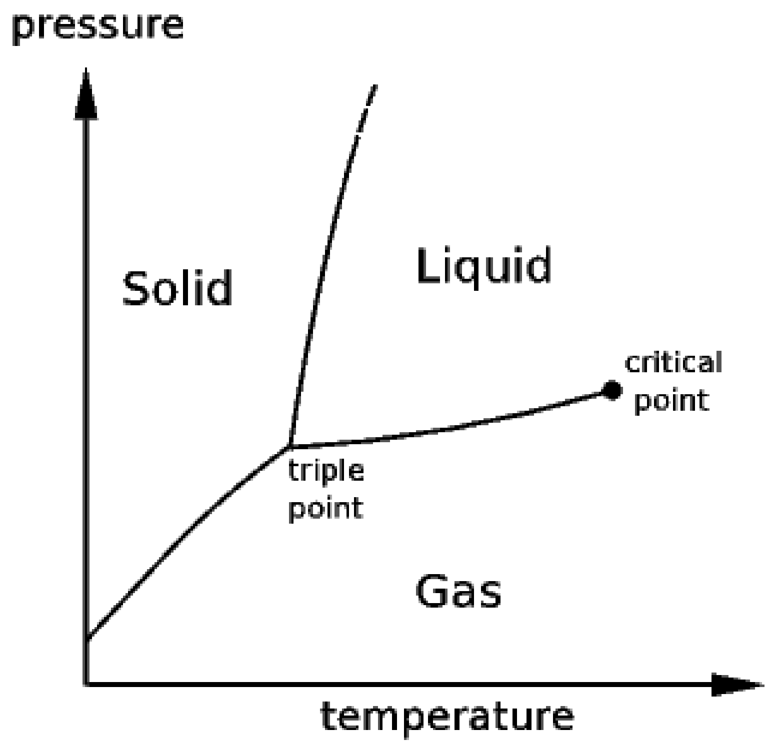

Figure 1: The phase diagram of water

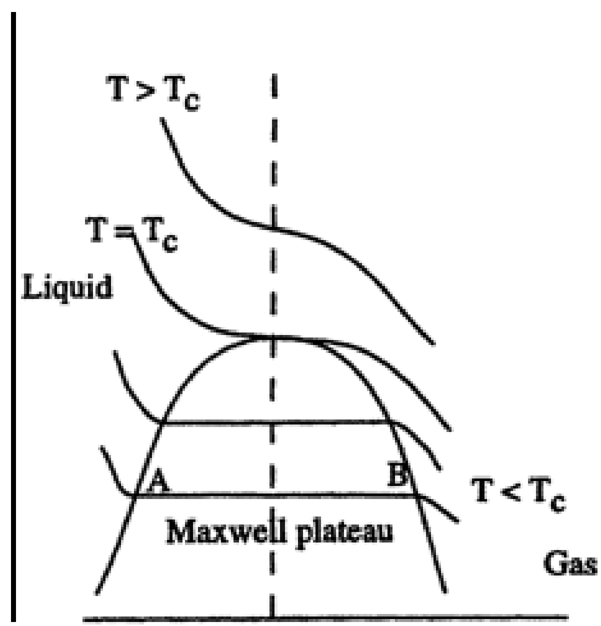

Figure 2: The phase transition between liquid and vapor

The isotherms below the temperature T_c have discontinuous points at the $A's$ and $B's$, namely, the rate of changes as the tangent lines along these curves do not have the same value as they approach these points as limits from left and from right. The physical interpretation runs roughly as follows. As the water in the liquid phase approaches, say, point A, at the same temperature, the molecules becomes less dense and the structure of liquid begins to loosen, so much so that until point B, the pressure stops decreasing even when the volume increases. And this is the region where liquid and steam coexists with varying proportions.

As Bangu (2009) tells the story, we can find the SM rendering of the same TD variables. Once we select the correct ensemble for the system, whose selection depends on global parameters such as whether there is exchange of energy with the environment or of both energy and matter, the probabilistic distribution is determined. With it the density function/operator can be found, which we use to calculate TD quantities as the ensemble average of the appropriate mechanical magnitudes. In such a function there is an all important function that is called the partition function Z, which mathematically contains all the information that SM needs to recover TD in the sense that thermodynamic variables are partial derivatives of the partition function or a combination of such partial derivatives. In the most frequently used functions in such a scheme of "recovery," two main functions stand out. One is the Helmholtz free energy, A, and the other the Gibbs free energy, F. They are defined exclusively by the partition function as we see in the following equations.

$$A = -k_B T \ln Z \tag{1}$$

where,

$$Z = \sum_i exp(-\beta H_i), \tag{2}$$

where the summation goes through all the micro-states with energy H_i of the system in question. And the Gibbs free energy is defined as $F = A + PV$. And through these, the $P's$ and $V's$ and all other thermodynamic variables are defined ultimately by the partition function of the system. For example,

$$P = -(\partial A/\partial V)_T \tag{3}$$

and,

$$V = (\partial F/\partial P)_T. \tag{4}$$

Again, according to Bangu and Shech, the "paradox of phase transition" arises simply because that the partition function of water or magnet is an analytic function, above, below, or at the critical temperature T_c, to any degree of differentiation. The TD function, such as the pressure or volume, which are the first-order partial differentiation of Z could not possibly harbor the desired points where phase transitions are exhibited. A "no-go" theorem is actually proven by Yang and Lee (1952) in their work on the phase transition between the paramagnetic phase and the ferromagnetic phase (see also, Mainwood (2005)). Yang and Lee have further proven in the same work that when thermodynamic limit is taken on the system, the non-analyticity *reappears* in the partition function (see also, Blythe and Evans (2003)). And the thermodynamic limit is the widely used method of taking the volume and the number of particles of the modeled system to infinity while letting the density remain finite.

In Figure 3, we see the behavior of the famous Yang-Lee zeros that represent the effect of taking the TD limit. Yang and Lee (1952) use the Ising model for a magnet to study the possibility of obtaining a rigorous recovery of phase transition and critical phenomena as described by the standard TD account. The three diagrams in the Figure show three different stages, a to b to c, of the value distribution of x on the unit circle in a complex plane. The complex function $x = e^{-\beta\epsilon}$, where the energy levels of the magnet is defined as $E = n\epsilon$ and $n = 0, 1, 2, ..., M$, is a function in the partition function for the magnet as placed in a heat bath, namely,

$$Z(\beta) = \sum_{n=0}^{n=M} g(n)exp(-\beta n\epsilon) \tag{5}$$

The similarity between equation (2) and (5) should help convey the meaning of equation (5). Theoretically, phase transition can only occur when x has a physically meaningful solution (i.e. Im$x = 0$), or the unit circle that represents x touches the positive Rex axis.

Yang and Lee show that without the TD limit, the mathematical situation are as in diagram a and b, where the circles do not close on the Rex axis). Only when that limit is taken could we see the closing of the circle as in diagram c, which means a physical solution exists that corresponds to the existence of ferromagnetism below the critical temperature. The latter is no less confirmed for magnets than the occurrence of boiling for water. As Blythe and Evans (2003) explain, the Yang-Lee result obtained

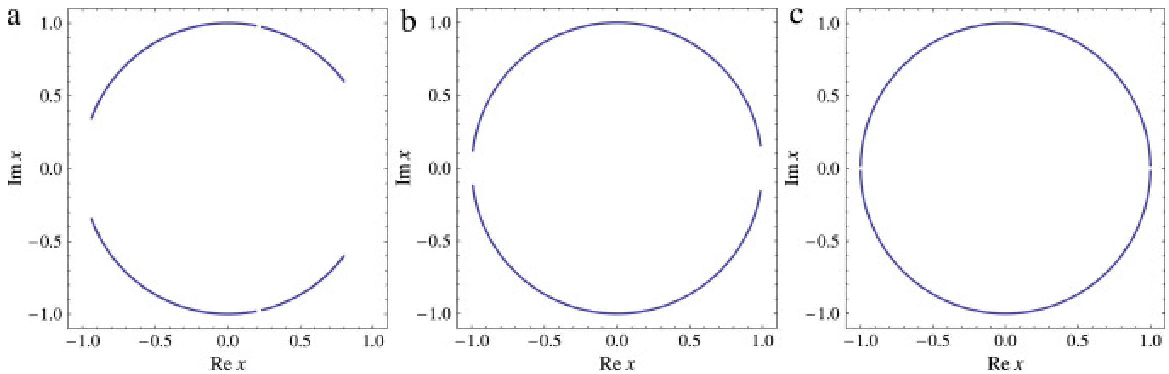

Figure 3: A diagram showing the behavior of the Yang-Lee zeros regarding TD limit

from the Ising Model of ferromagnetism is entirely generalizable to other cases of phase transitions and critical pheomena.

The so-called "paradox of phase transition" as a philosophical problem may be understood in two different versions.

- No finite model of a system, which is actually going through a phase transition, exhibits such a transition according to the standard SM account of the system's thermo-behavior.
- A rigorous exhibition of the phase transition appears in the theory only when a TD limit is taken (which produces an infinite physical system in the model).

The second version renders the model irreconcilable with the actual system it represents. The irreconcilability is pronounced because, as Norton (2014) points out, taking the TD limit, which creates in our imagination an actual infinitely large system, is essentially different from the more widely used taking limit technique, which only takes this or that property of a system to the indefinitely large. The latter limit-taking procedure usually does not come with its own "no-go" theorem, whereas the TD limit is proceeded with a distinct "no-go" theorem.

These two different versions of the paradox point to different ways of finding a resolution. Suppose the question is, how come a finite SM model does not exhibit any phase transition while the TD account of the transition is clear and rigorous? This is to take the first version given above. A solution may come in the form of "try not to take the TD account too seriously." And this is essentially how Callender (2001) takes and resolves the paradox (see also Bangu (2009, 2015)). Closely associated with the Callender resolution (Liu, 1999; Bangu, 2009) is the analysis of what actually must be the case in the plotted curves of the TD variables that actually come from the labs. The corners where the non-analyticity appears in the TD curves must be fuzzy and unsharp, which might just be what a finite SM model of the actual system could reproduce. Even if a finite SM model could not exactly reproduce the plotted lab results, it is at least true that the "no-go" theorem can no longer be defended. That, in a minimal sense, resolves the paradox.

Bangu (2009) (also Bangu (2015)) provides a stronger argument and a more robust resolution than the above. By enlisting a "well-known distinction between *data* and *phenomena*," a general conception of scientific methodology in the works of Bogen and Woodward (1988), Bangu is able to defend a position that takes the non-analyticity in TD seriously (unlike what Callender advised) and yet understands the physical reality of phase transitions. While conceding the fact that plotted data graphs from the labs do not contain any non-analytic "corners," (as Liu (1999) has pointed out, implying the corresponding TD non-analytic points are artifacts in the theory), Bangu argues that that is only in the realm of *data*, or *data models*. One cannot solely depend on data models to conduct scientific investigations. One has to come up with the *phenomenon*, or *phenomenal models*, before rigorous mathematical representation of the phenomenon can begin. The TD theory in terms of continuous variables and partial differential equations are inconceivable if one stays with the data models. And the non-analyticities are part of the bargain for the phenomenal models.

We want to further defend Bangu's view by making the following observation in view of our discussion of Mainwood's point of view at the beginning of section 2. As Mainwood (2005) points out, there are two problems in dealing with the SM recovery of TD phenomena (i.e. thermo-phenomena recognized and theorized in TD). One is the "reduction problem" and the other is the "idealization problem." The paradox of phrase transition is not really a problem of physical ontology; not simply a problem of finding out what actually happens in nature when a phase transition is taking place. For if it were such a problem, well-known metaphysical positions would have to be paraded before any sensible conclusion was drawn. For example, in that context, it would no longer be an innocent claim to say, "of course, the system in question is made of small particles sometimes bound and sometimes free in their movements." Nor is it conclusive to assume that whatever shapes the plotted lab data curves assume, the real manners of phase transitions cannot possibly be any different. One has to get into the debate about natural kinds, and argue whether or not phase transition is a natural kind. The position that Callender and Liu take appears to imply that it could not be taken as a natural kind (and therefore, should not be taken too seriously). Any yet, is there anything currently categorized in physics or science ultimately a natural kind, a question Quine can be taken to have raised in serious doubt in his eponymous article (Quine, 1969)?

Therefore, as a problem of reduction (or in our terms, it is "recovery"), we naturally have to consider the difference between the SM-TD relationship and the KT-TD relationship, as we have broached earlier in discussing Mainwood's work. The points made there are two. First, the SM-TD relationship is indeed a strictly reductional relationship. It is intended and pursued by scientists as such, and therefore, TD must be taken seriously. Otherwise, the SM-TD relationship would not have survived. Those of us who engage in the debate about reduction or recovery must remember that there is a fundamental difference between "reduction" and "elimination." To borrow a familiar example in philosophy of mind. The mind-body relationship may be a reductional relation; but it does not follow from that that it is also an eliminative relation. Reductiionism is not eliminativism, and the difference, among many differences, is that the former recognizes the reality of mind while the latter denies it. If we may use the language of natural kind, we could say that reductionists believe that mind is a natural kind, while the eliminativists deny that. Analogously, those who regard the relationship between TD and SM as one of reduction or recovery must hold TD categories, such as phase transitions and critical phenomena, as mathematical singularities; they must recognize the legitimacy of such categories in reality as no less real than, say, the velocity of every molecule in the body that is undergoing phrase transition.

In this reductional relationship, the SM handling of the molecular systems is primarily by way of abstraction. It is not primarily by way of idealization. The latter belongs to the KT-TD relationship, which is much loser than the SM-TD relation; and this is the second point. In the kinetic theory of matter, a huge variety of idealized systems could be imagined (which Norton calls fictional systems). Since the molecular details in the KT models, albeit highly idealized, are taken seriously in the study, deviations from the TD accounts abounds. We agree with Norton to think that such studies produce highly approximate claims about the behavior of the target systems, and yet when properly improved upon, they give much more realistic accounts of the thermophenomena than either TD or SM accounts of the same. These considerations belong to Mainwood's idealization problem. The problem of course is that the possibility of improvement is very limited.

Shech (2013) provides a different way to resolve the "paradox of phase transition." The idea, if I understood correctly, appears to be this. Suppose we consider such claims as there are phase transitions in finite systems in nature and yet SM says that no such things exist in finite systems because the partition functions of such systems are all analytic at any temperature. Are such claims contradictory or problematic? But these claims are ambiguous in that the central terms such as “phase transition” or “partition function” are often semantically ambiguous. In some contexts, they are used to refer to concrete systems and actual events in them. Partition functions may also denote the quantity of the concrete systems. However, they are also used to referred to mathematical objects, such as model systems and mathematical functions (say, in mathematical physics rather than ordinary theoretical physics). By a disambiguation, Shech believes we can dissolve the paradox. If we stick to the former semantic function of these terms, there are no non-analyticities but phase transitions. The two do not even have anything to do with each other from a physical point of view. So, no paradox there. If we stick to the latter meaning of the terms, there are no phase transitions in finite systems (models) because there are no non-analyticities in those systems. The “no-go” theorem holds and there is no problem, nor is there any problem that phase transitions do not exist in finite (models) systems in SM, because they exist in infinite (model) systems. The situation is just as simple as that and the paradox is dissolved.

We disagree with some of Shech’s analysis of the philosophical situation involved in the paradox of phase transition, but we recognize that a lot of insights are shared between the two approaches discussed above. The idea that realistically there does not seem to be a place for phase transitions as non-analyticities or singularities seems to be a shared conviction; and so is the view that the “no-go” theorem is an artifact of SM; a very important artifact, perhaps, but an artifact nonetheless. In reality, phase transitions take place in finite physical systems, whether their mathematically rigorous accounts contain non-analyticities or not. Such artifacts as non-analyticities or singularities do not refer to anything physically real and should not be thought of capable of causing philosophical paradoxes or conundrums. Notice, this view does not necessarily apply to other areas in science, where singularities are routinely used to represent genuine physical phenomena and do not produce “paradoxes.” For instance, the rigorous mathematical representation of black holes uses non-analyticity and yet few people would say: after all, black holes are in fact not singular; they are no different ontologically from non-singular entities in a spacetime continuum; and singularity is just an artifact in our model representation of black holes. We reserve our disagreement with Shech about whether it is no more than a matter of semantic ambiguities until later when we defend contextual realism.

Let us now turned to the other version of the paradox. It amounts to the question of how we can justify the taking of the TD limit as necessary for providing a rigorous solution to the non-analyticity problem. As Norton (2012) notes, idealizing a system into a physical impossibility is different from many other forms of infinite idealization. Making a process of heat transfer a reversible process by taking time to infinity, for instance, is a different sort of infinite idealization.[9] When the time is *arbitrarily long enough*, the temperature difference for each heat transfer could be made *arbitrarily small* such that the summation remains the same. The limit is no more than an approximation of the very large, and the desired result approaches the limit asymptotically. There is no "no-go" theorem in this type of limit taking. The almost reversible matches the almost infinitely long or slow of the process. The same applies to, say, the case of motion on a frictionless plane.

Therefore, unlike the other non-problematic infinite idealization cases, taking thermodynamic limit that results in infinity systems does seem to raise the prospective of a paradox. This is the problem of "Essential" or "Indispensable" Idealization that Shech (2013) discussed at length in his article, (see also Batterman (2005)). This type of idealization threatens to always cause paradoxes or conundrums or under-girt emergent property claims. A lot of ink has been spilled on this issue, and many what we regard as sensible or only sensible things have been said about it (Batterman, 2005, 2009; Liu, 1999, 2001; Norton, 2014, 2012). Moreover, since we are going to connect this issue of essential/indispensable idealization with whether or not scientific realism is thereby threatened, we shall postpone a detailed discussion until later. For now, we mention two related points.

One may wonder with good reason why it is not possible to come up with a finite-sized model system to account for phase transitions (and critical phenomena). If as we said earlier, we know in an ontological sense what is going on in a phase transition, such as happening in a tank of water or piece of magnet, why can't we directly model the thing and come up with a mathematical account of the phenomena? The first point is that there are indeed some efforts to do just that, but not in as straightforwardly a manner as one might suspect. Finite-scaling technique is an attempt to remove the need for taking the TD limit and still recover phase transitions as non-analyticities. We shall not engage in any explanation of what this technique is all about, but it is an effort to rescue, so to speak, our common sense of what happens in reality (see Menon and Callender (2011), Bangu (2009)).

The related second point is more philosophical. We have discussed at some length the difference between recovering TD accounts with the SM approach versus recovering

[9] To distinguish it from the former kind, Norton calls it "approximation."

that with the KT approach. Given our above-given analysis of the difference between these two types of "recovery," we have the following conclusion. The SM accounts or models try to avoid excessive idealizations about the material and structural details of the target system, whereas such idealizations are mandatory and freely engaged in the KT accounts. The kinetic theory of gases, as the simplest example of the KT approach of condensed matter systems, exhibits all the features of attempts of idealization as well as attempts of reducing it or relaxing it so that more realistic models result. From a methodological point of view, if the KT approach is taken seriously in the recovery effort of the TD account of phase transition and critical phenomena, it is quite possible that finite-size modeling succeeds eventually. But this is only possible when the full array of material and structural details are at the modelers' disposal and when manageable mathematical treatment of such details are available. Some of the examples surveyed in Menon and Callender (2011) sufficiently indicate that the KT approach handling of phase transition is no more than in principle possible. The actual practice of coming up with finite-sized models for phase transitions is in fact along the line of using the SM approach with specific structural restrictions put in.[10]

The situation with the SM approach and its models is very different. As we have argued earlier, abstraction, not idealization, is the main method for building an account that recovers the TD account of phase transitions. Unlike idealization, whose relaxation is conceptually unproblematic, albeit often difficult in practice, abstraction is not something that can be "improved" upon in the same manner. In fact, it is problematic to think of abstraction and concretization as a matter of more or less in the same way that idealization and its relaxation are. While idealization is a distortion of a chosen property or magnitude, abstraction is the ignoring of properties that do not effect the truth of the abstract claims. Successful abstractions do not need "improvement" in order to produce true statements, as seen in earlier discussion about Godfrey-Smith's work. Therefore, the SM account of TD phenomena is not subject to slight adjustment for improved representation as in the case of the van der Waals gas model improving on Boyle's ideal gas model. When the SM account of phase transitions encounters the difficulty of the "no-go" theorem, a solution is not to be found by adding more real details of the target system, because one cannot add details of properties that the original abstraction ignores. For example, an abstract claim about a red wooden ball might be "the thing is red." If for some reason this statement is somehow in need of

[10]I thank a referee for pointing out that not all cases surveyed in (Menon and Callender, 2011) are in the KT approach camp. The conceptual point is that the KT approach encourages a study of the complex details of a finite system in order to recover the TD account of macroscopic phenomena.

repair, the improvement cannot be gained by adding the information of what sort of wood the ball is made of. The statement does not even mention that the thing is made of wood. Similarly, in a SM account of, say, ferromegnetism, the size and movement of the components of the magnet in question is not even relevant. They do not figure in the Hamiltonian. Adding more realistic estimates about those properties is obviously not going to help, very much unlike the cases in a KT approach. Given the nature of a SM account of phases transitions in general, taking the thermodynamic limit seems to be an indispensable/essential idealization that solves the paradox and yet preserves the integrity of the SM abstraction scheme for a condensed matter system. As far as we know, taking the TD limit appears to be the only way of preserving SM and accounting for phase transitions at the same time. One necessary idealization that saves a branch of physics that do not use any other idealizations essentially.[11]

5 Challenges to Scientific Realism

Scientific realism has had a long history. More than one variations have been introduced during that long period in which it faced challenges from many directions. An evaluation of the implications of infinite idealization on all variations of realism is beyond the scope of this paper. We leave the work of discussing the more recent version of scientific realism and their compatibility with the above analysis of infinite idealization to another occasion.[12] A complete job of sorting out whether infinite idealization poses problems for every variety of scientific realism is perhaps best left for a monograph.

We begin our discussion with the earlier formulations of scientific realism: one from a critic and the other from one of its best early defenders. The formulation of scientific realism by van Fraassen in the early 1980s serves as a benchmark (Van Fraassen, 1980). According to van Fraassen, scientific realism comprises essentially two theses. the essential terms used in a scientific theories genuinely refer, and to embrace a theory is to have good reason to believe what it says about what those terms refer to are true. And the conception of truth is the correspondence theory or its variation. Van

[11]If there is still any lingering doubt about how real the metaphysical implications are with the case of taking the TD limit to resolve the paradox of phase transition, the case concerning quantum measurement should supply more reason for serious concerns. It turns out that if one embraces the decoherence approach in general as a hopeful resolution to the quantum measurement problem, one has to inevitably face the use of thermodynamic limit on finite target systems in which quantum measurements take place. An infinite quantum system is the only type of systems in which unitarily inequivalent representations of algebraic quantum states appear that realize the measurement results. We shall reserve a discussion of the quantum measurement problem to another occasion, but see (Ruetsche, 2011; Liu and Emch, 2005)

[12]For more recent discussions, see Psillos (2005); Chakravartty (2011).

Fraassen's empiricist alternative view, namely, constructive empiricism, also adheres to the correspondence theory of truth and the requirement for reference. It objects to the realist demand that to accept a theory is to believe that it is true (or approximately true). Accepting a theory, according to constructive empiricism, should only require empirical adequacy. Richard Boyd's version of scientific realism, as mentioned earlier, has all the elements of van Fraassen's characterization with the addition that theories that are well confirmed are true or approximately true, and there is a historical progress of science towards more approximately true theories.

The main argument for scientific realism, almost by unanimous consent, is the inference-to-the-best-explanation argument, and there have been many different versions of this argument (Boyd, 1983). Boyd's contribution is to precisify the notion of success in science as the increase of reliability of scientific methodology (which includes the theory-laden designs of experimental procedures and apparatuses). The increase in the history of science of the reliability of scientific methodology would be a miracle if the scientific theories that the methodology relies on were not true or approximately true and the terms used in them did not genuinely refer.

Suppose, as we have argued above, that infinite idealization is an indispensable scientific method in the construction of scientific theories. Does it conflict with the notion of scientific realism articulated by van Fraassen or Richard Boyd among others?

First off, do terms such as "phase transitions" and "critical phenomena" as used in thermodynamics, genuinely refer? Do they refer as terms used in statistical mechanics? More generally, are all those terms associated in theory as non-analytical points genuinely refer?

Then, are infinitely idealized theories, such as the theory of phase transition and critical phenomena as a rigorous solution, fit for the sort of confirmation processes that scientific realism sanctions? Given the discrepancy between the TD account and the SM account of the phenomena, how can the confirmation of the two accounts cohere with each other?

Thirdly, how are we to understand the notion of truth or approximate truth with respect to infinitely idealized theories? With a model of an infinite system, are claims about the behavior of such a system possibly true or approximately true? If they are approximately true, what notion of approximate truth are we using to account for the infinite system?

Finally, if we know that finite systems are the real systems and science aims at representing reality as closely and adequately as possible, why is it unproblematic to use a method that appears to be deliberately deviating from the realist spirit? To see the importance of this question, let us consider a similar question. Suppose we know for sure that a human being is nothing beyond a body and its functions. Isn't it problematic if a mind-body dualist method turns out to be indispensable for our best theory of human mind?

Mainwood (2005) discusses at length in section 4 of his article the question of how one justifies using quantities under TD limit to define phase transitions in actually finite systems, as such common phenomena as water boiling in a kettle. The challenge Mainwood poses in that section is how one could justify such a definition as, "[p]hase transitions occur for a finite system in state S if and only if $\mathcal{F}_\infty(S)$ has a singularity" (Mainwood, 2005).[13] Mainwood's strategy in defending the definition is to allay two worries about using $\mathcal{F}_\infty(S)$ to precisely account for a physical system whose free energy "ought to" be expressed by $\mathcal{F}_N(S)$, where N is the number of components in the system. Part of this challenge, as we see it, is surely about the reference of terms. How does "phase transition" defined in terms of $\mathcal{F}_\infty(S)$ refer to phase transitions in nature? Does that term genuinely refer, as van Fraassen would ask, or is it just a piece of a theory that aims at no more than "saving the phenomena?"

One may think that a simple answer can already be found given what we have discussed so far. Bangu (2009), as we mentioned earlier, has already pointed out, in lieu to arguments from Callender (2001) and Liu (1999), that we know all about what actually is going on ontologically with phase transitions. Taking the TD limit may be seen as a purely theoretical move to seamlessly connect SM with TD. $\mathcal{F}_\infty(S)$ simply refers to the actual phase transition taking place in a finite physical system. This is unproblematic because there are no other phenomena that $\mathcal{F}_\infty(S)$ refers to instead. There is no ambiguity and there is no problem either.

However, an answer of this sort seems too simplistic. Bangu mentions the contrast between data and phenomena, where the former is closer to the collection of observations and the latter the theoretical understanding. The connection between the two is not usually characterizable by a simple notion of reference. The freedom of theoretical considerations in coming up with models for phenomena may create alternative representations for the same set or cluster of experimental findings from the labs. Consider the following hypothetical situation. Here we assume what we have defended earlier of the conceptually possible KT approach, where a detailed idealized model of the exact molecular positions and movements over time are given. And we assume that at least one strategy of coming up with a finite-system account for phase transitions is feasible, as discussed in Menon and Callender (2011). Suppose besides our $\mathcal{F}_\infty(S)$ description

[13] Here, $\mathcal{F}_\infty(S)$ is the Gibbs free energy of the system in state S.

of phase transitions (e.g. boiling water or ferromagnetism) we also had a KT model for them that does not involve taking the TD limit. Call the latter a $\mathcal{K}_N(S)$ account. We may now ask: which term, $\mathcal{F}_\infty(S)$ or $\mathcal{K}_N(S)$, refers to phase transitions? A further question may also be asked. If we had the $\mathcal{K}_N(S)$ account, which let us assume is more realistic, would we be forced to give up the $\mathcal{F}_\infty(S)$ account? The answer is neither clear nor simple. For someone who embraces, say, van Fraassen's constructive empiricism, it might be more reasonable to argue that each term refers to something real but is radically different from the other. So the literal stories using one or the other term are incompatible; and yet they both save the phenomena [14] of phase transitions as observed in the labs and in daily life.

Inconclusive as the above, we can at least say this: to say simply that we know all about what phase transitions are ontologically in a finite physical system is a bit too hasty. Therefore, to try to figure out what $\mathcal{F}_\infty(S)$ or $\mathcal{K}_N(S)$ says or refers to by assuming that they are no more than theoretical artifacts is premature philosophically. In Section 4, we argue for a position that avoids having to make such claims about what reality is like ontologically in order to defend scientific realism.

Laymon (1985) has developed a theory of confirmation that is specifically designed to deal with idealized theories. In slogan form, it says that an idealized theory is confirmed if and only if it monotonically approbates approximation when the idealization conditions are gradually relaxed and/or removed. And idealized theories are no problem for realism if they monotonically approbate approximation because it would be a miracle if they did such a thing and yet laws and such that the idealized theories possess were not true and terms did not genuinely refer. Space prevents us from discussing Laymon's theory in detail, but see Liu (2004, 2007). We only want to point out that when applied to the method of infinite idealization, there are definite complications. We have seen when discussing Norton's work on infinite idealization that there are two radically different types of infinite idealization. The more common ones are those that do not require an imagining of real infinite systems in order to theoretically represent phenomena in finite systems. These are cases such as frictionless planes and adiabatic heat transfers. In these cases of infinite idealization, incrementally smaller and smaller effects are taken ad infinitum. The value at the limit differs from any next closest values only infinitesimally such that Laymon's idea of monotonically approbating approximation is precisely applicable in these cases.

In addition, Laymon (1989) argued that in most cases of idealization, the laws as the

[14] In our context, here it should be the "data model."

result of the idealization are true, not approximately true, while the models in which the laws are exhibited do not actually exist. For example, the law of inertia in Newtonian mechanics can only be displayed in the model of frictionless systems. The law is true but the model does not exist. Such are what Norton calls fictional or fictitious models, but they approximate the real system and the degrees of approximation can in many cases be estimated numerically. Again, Laymon argues that precisely because the laws are true, the relaxation of the ideal conditions for the models results in the models asymptotically approach the experimentally obtained data. Otherwise, the relaxation would not work.

This felicitous situation, we argue, is not true for the other type of infinite idealization, the type that contains the results of taking the TD limit. These are the cases where a "no-go" theorem is accompanied when the system is finite or indefinitely large. This is the type that Norton regards as involving genuine (infinite) idealization; (the other type involves approximation only). Further, no laws of nature are exhibited in such models. On the contrary, the phenomena, such as boiling and ferromagnetism, are fully observable and real. The TD limit is taken so that the theory of SM says the right things about what we can readily observe. Without infinite systems (which by the way are real infinities, much unlike the infinitesimals in the previous type of cases), there are no phase transitions. Any slight relaxation of this idealization results in nothing comprehensible or useful in SM with respect to making judgments about the phenomena. Whether or not Laymon's theory of confirmation can cover this case and other similar cases and still honor scientific realism remains an unmet challenge.

Theories of approximate truth have been closely associated with notions of approximation. When it can be proven that solutions for certain equations exist but no method of solving them exactly are in sight, methods of approximation are often introduced in the form of, say, Taylor expanding a supposed solution and obtain an actual solution by truncating the expansion, abandoning the higher order terms which are very small. Hilpinen (1976) has developed a general theory of approximate truth that says a proposition p is approximately true within the threshold of ε if and only if there is a possible world that is no farther away from the actual world in the measure of comparison, ε, in which p is true. Again, we can see without spelling out the details of the reason that this notion of approximate truth fits the first type of infinite idealization but not the second type, the type that is causing the trouble.

And given all these difficulties with the second type of infinite idealization, it seems entirely reasonable to ask whether infinite idealization such as taking the thermodynamic limit in order to recover a rigorous account of phase transitions and critical phenomena is at all compatible with scientific realism.

Before we develop the view of contextual realism, which we think helps to meet the challenge, let us emphasize the fact that we are forced philosophically into this option by the specialness of the type of infinite idealization that is exemplified by taking the TD limit. Although most idealizations are infinite idealizations, such as making the discrete into continuous quantities, the special challenge does not arise from them.

6 Contextual Realism

6.1 Cases

We begin in this section to sketch a philosophical position, which we shall call *Contextual Realism* or scientific contextualism.[15] We argue for such a position in a limited fashion by only using the case of the infinite idealization of the more problematic type as a testing ground. We shall see that enlisting contextual realism helps us to maintain that we know what is going on with phase transitions in finite physical systems and that a realist view is compatible with a substantive use of taking the TD limit. For a full development of the position we leave for another occasion, but in the next subsection, we give a sketch of it.

First, we clarify and dispose of some potentially misleading ideas about contextualism in general and contextual realism in particular. The most familiar contextualism would be epistemic contextualism (Lewis, 1996; DeRose, 2009). Contextualism about epistemic claims is not about the context-dependent nature of which beliefs may be counted as true or justified. That would be conflating contextualism with relativism. Contextualism is about the context-dependency of knowledge attribution, namely, when or under what conditions an attribution of knowledge to an agent is true is a context-dependent matter. The same claim that "S knows that p" may turn out to be true in one context but may not in another. It all depends on the standard of knowledge attribution or of sufficient justification; and one standard in one context or with one community may not be so in another. The context in which the Cartesian doubt of any piece of knowledge is taken seriously must be quite different from the context in which the most secure set of common-sense beliefs are regarded as knowledge. The opposing

[15] As far as I know, there isn't any known territory in philosophy that bears the name of Contextual Realism. The best known example of its mentioning is in Fine (1991) when referring to a position espoused by Richard Miller (Miller, 1987). Fine's own name for the position is called "Piecemeal Realism." It is a different position from the one we defend here; and we intend to reserve the discussion of its relationship with our position for another occasion.

view to contextualism is invariantism, where knowledge claims are invariant across all contexts.

Similarly, contextual realism is not about the context dependency of the truth-makers of scientific claims. It is rather about when or under what conditions a realist claim about a scientific practice or theory is true. And the notion of realism here may be any of the traditional versions of scientific realism, such as van Fraassen's or Boyd's version as we discussed before. The strategy here is diametrically opposite to, say, Putnam's maneuver towards internal realism (Putnam, 1981). Putnam's strategy is to move away from the correspondence theory of truth and the accumulative approach towards more and more approximately true theories in science. Our strategy is to retain all that and modify the expectations of claims for realism.

To begin our sketch of contextual realism, let us consider some illustrative examples. As Dummett (1982) reminds us, the question concerning realism is really a collection of questions concerning the existence of this or that disputed class of objects and the truth of the *existence claims* about such objects. The collection may well be heterogeneous: whether the past is real or exist is not the same question about realism as whether numbers are real or exist. And both are very different from whether or not electrons or quarks are real. And in the current case, the disputed class of objects are rather peculiar to say the least. What we are asking is whether such phenomena as phase transitions as defined in SM for infinite systems exist in a sense that is compatible to realism. And if we follow Mainwood (2005), this is a question of how it may be true that "[p]hase transitions occur for a finite system in state S if and only if $\mathcal{F}_\infty(S)$ has a singularity."

We now consider three examples. The first is Boyle's ideal gas model and Boyle's law for ideal gas. Then it is the model of Carnot engine or cycle in which adiabatic processes take place. The third is our SM model for phase transitions and critical phenomena in which TD limit must be taken in order for the phenomena to be possible. As a case for contrast, we also consider a case of elementary particles, for example, the question of the existence of quarks. We shall take it as obvious that the ideal gas (as modeled by Boyle) does not exist or is not real, nor is the Boyle law for that gas a law of nature; nor is it even approximately true of nature. And it is entirely compatible with realism to deny the existence of ideal gas. We can think of the model as a pure fiction, as Norton (2014, 2012) has thought so.

Here we see one of the features of contextual realism. The contextual elements that make us reject the ideal gas as real are of the following sort. The assumption about the components of the target system, e.g. tanks of diluted gas, is perhaps vague but entirely realistic. The ideal gas model in which highly idealized assumptions (namely, assumptions of distortion), such as the zero size of, and no interaction among, the moving molecules, are made is taken as a highly sketchy representation of the target system, i.e. the diluted gas. The model itself and the ideal gas law can only be taken as a rough "map" of the thermo-behavior of the system. In another place, we call such laws as the Boyle's law a "law-map," not a law. It and the like are not intended by the scientists themselves to represent laws of nature (not even in approximate degrees of accuracy). They are rather given as guides to the discovery of laws. Hence, a realistic but vague idea about the components and their behavior of the gas and a highly idealized model intended to represent the gas in a rough-and-ready way, these are the contextual beliefs that ground the claim that Boyle's model is fictitious or fictional.

Notice, the above situation is very different from the contextual beliefs scientists have or would have regarding a model of quarks and the lawlike claims concerning their behavior. There is no contextual belief that assume that quarks and their behavior are radically different (because of idealization) from what our theory of quarks say about them. There is also no explicit contextual belief that our current theory is no more than a rough-and-ready sketch of quarks. There may be, instead, beliefs about the abstractness of our theory; but as we have argued earlier, abstraction is a very different method than idealization, and claims under abstraction are often literally true rather than approximately true. So, a realist claim about the existence of quarks can be in serious dispute, rather unlike in the case of ideal gas, where the model and laws are clearly not real.

Also note, there is no impossibility for the existence of ideal gas. In a possible world that obeys the same laws of nature as the actual one, systems of ideal gas could well exist and function without any apparent problem. It is entirely possible as a matter of fact that no molecules in any tanks of gas ever interact with each other. As to the zero-size assumption, it can be replaced by the idea of indefinitely small sizes. Therefore, as in literary fiction, if we suspend our disbelief and forget the actual world, the fictional world might feel as real as the actual one.

Another important point to notice is that this analysis of a realist reading of the situation agrees with Laymon's account of how idealized models and laws that are exhibited in them square with realism. Recall that a necessary condition for lawlike claims to be laws of nature in an idealized model is for them to monotonically approbate approximation. Boyle's law of ideal gas cannot do that because to relax the idealization in this case, namely, going into a model of gas such as the van der Waals model, the gas law has to be changed (into the van der Waals law of gas). This is another reason for regarding the Boyle's ideal gas model and law as purely fictional.

The second example is a case of pure thermodynamics, which we have discussed earlier in connection with the works of Norton (2014, 2012). Norton has argued that there is no idealization but only approximation in this case of taking the infinite time limit that ensures reversibility. What he means, as we have interpreted him earlier, is that no imaginary model of the components of the target system is built in this case, and what is done is to take an infinite time limit to approximate the actual results of near reversibility. As we have argued then, this case certainly involves a TD model and the model is the reversible Carnot cycle, where the efficiency is at maximum and heat can be transferred without a temperature differential. However, the model is not made by idealizing the micro-components of the target system; rather the entire compositional details of the system are abstracted away. The main contextual assumption or belief in this case is therefore the absence of micro-components of the system. As in the cases for abstraction, many claims from the abstract TD model of the Carnot cycle are literally true in the context (namely, by disregarding those very unlikely large fluctuations). Under contextual realism, thermodynamic terms and claims in general are genuinely referring terms and literally true or false, even though there is another context within which the reference relations are shaky and claims are only approximately true or even flatly false. As we make explicit in the next section, the *anchoring* or *grounding* assumption that defines the context of this case is that *condensed matter is continuous*.

But what about reversible processes? Do they exist in nature or do such terms genuinely refer? Given that taking time to the limit of infinity is impossible, a special problem for realism arises. The first thing to notice is that the necessity for taking the time limit is occasioned by the context-defining abstraction that no compositional details are to be considered or condensed matter is continuous. For otherwise, if we go beyond TD and look at the molecular level of the system, reversibility is more fundamental than irreversibility! Between two subsystems at equilibrium in this context, heat transfers in the form of molecules moving in-between take place all the time. Summing over the process in a long but finite time, it is entirely possible a certain quantity of heat is transferred from one side to the other, or vice versa, without any appreciable temperature differentials. But our anchoring belief in the context of TD systems forbids such considerations. As in the case of phase transitions and critical phenomena under the TD limit, here we are also faced with the following choice. We may adhere to the abstraction that thermodynamic modeling requires and account for reversibility in terms of the infinite time limit. Let us call this choice the t_∞ option. Or we could go with what we have just described as the molecular approach (or micro-kinetic approach), which we may call the K_N option. If in the context of the t_∞ option, there is no possible alternative account for reversible heat transfer, especially there is no account that does not require taking the infinite time limit, then we say, as a contextual realist would, that reversible heat transfer as depicted by the model of reversible Carnot cycle is a thing; it exists. The fact that there is also a K_N option in a different context does not render the above realist claim false or problematic.

Again, this case is somewhat different from the quark case. There is no clear-cut stipulation of abstraction that insulate high-energy physicists from looking further into the details of quarks. And although quark models are surely abstract, but in what way it is abstract could never be made clear. That is because we do not have a set of beliefs about quarks analogous to the beliefs we have about the molecules in a thermo-system such that we know what we have abstracted away from quarks. Therefore, questions about quarks or electrons or any other elementary particles/waves may be regarded as analogous to knowledge attribution claims that require the highest standard of justification (where Cartesian skepticism is generated). There is no "insulation" by way of abstraction for elementary particle physicists. If there is a science that is no holds barred, that is elementary particle physics or unified field theory because they are by scientists' stipulation about the ultimate furniture of reality. This is surely right because if scientific realism holds for science at all, it must hold for the theory of the ultimate furniture of reality. In other words, if contextual realism holds at all, it must hold for realism simpliciter, namely, there must be a default or ultimate context that needs no contextual assumptions. In other words, contextual realism is first and foremost a realist position, which simply says that the ultimate furniture of reality exists independently of any possibility of being known by any cognitive agents.

Again, Laymon (1985)'s version of realism and the asymptotic idea of idealization as approximation approbation apply to case of reversibility without much problem. Relaxing the time limit and making the model more realistic would not eliminate the effect of reversibility. It is similar to the case of frictionless plane. By adding back infinitesimally the effect of friction, the behavior of the system will be gradually recovered to what can be experimentally reproduced. Laymon's account covers all idealization cases of this type and realism is safe in the context of condensed matter systems, where abstracting microscopic details away from consideration anchors or grounds the discipline.

The SM account of phase transitions by taking the TD limit can be handled in a similar manner. What is different from the pure TD account of reversible processes is that we are now considering molecular models of thermo-systems. We shall not repeat the actual analysis of the case here. For that we refer our reader back to our detailed discussion in section 4, where two different versions of the "paradox of phase transition" are thoroughly investigated. Here we sketch an argument for why that analysis fits well with contextual realism so that phenomena such as phrase transitions and critical phenomena in infinite systems do exit.

If our analysis in section 4 is correct, whether one takes the first or the second version of the paradox of phase transition, the central issue is whether one is considering the SM-TD relationship or the KT-TD relationship. Here we further argue that the realist concerns about referring terms and approximate truth are different in these two relationships; or the two relationships *exhibit* two different contexts. If one takes the SM-TD relationship, the grounding assumption for the context comprises the abstractions that are taken to come up with SM models that involve as little distortions (i.e. idealizations) as possible. The KT-TD relationship is the opposite. It assumes a great deal about the compositional details and introduces many distortions in order to tidy up the details so that aggregate properties that parallel TD properties can be had. It is quite possible that the finite-system KT accounts with highly idealized components could "almost" recover the TD properties in question. As we have emphasized before, this is a possibility claim that should be treated as for the purpose of clarifying concepts rather than establishing facts.

This is a case that doesn't fit Laymon's realist position for idealized theories. The "no-go" theorem for finite systems in such a case makes it more reasonable to consider infinite systems with finite density more as fictional systems than systems that exist. However, when we use a contextual realist viewpoint to look at this case, the situation becomes quite different. The great and single idealization of the TD limit is introduced in the SM-TD relationship precisely because, unlike in the case of the KT-TD relationship, there is little or no "essential idealizations" involved in most of the recovery of the true TD statements by SM accounts. The lack of competing alternative SM accounts for phase transitions also contributes to the evaluation that the realist claim that "phase transitions as the non-analyticities in infinite systems exist" is true.

6.2 General Conception

What then is Contextual Realism, given the above case-studies of its applications? Here we give an account of what can only be regarded as an initial approach. The inspiration and parallel is, of course, contextualism in epistemology.[16]

One of the challenges for the version of scientific realism that takes reference and truth to be radically independent of any epistemic or philosophical conceptions we have of reality is, of course, how implausible it seems. It implies that however reasonable our current semi-metaphysical beliefs about the subatomic particles or waves or wavicles are, claims about them may well be utterly false and may never be discovered as such by any cognitive agents.

In contrast, Putnam's internal realism and Dummett's anti-realism deny such a radical independence of truth (Dummett, 1982; Putnam, 1981). Truth is intrinsically connected to epistemic justification; and according to internal realism, a claim is true if and only if it is fully justified in an idealized limit of justification. The limit can never be reached and yet truth/falsity is guaranteed there, just as frictionless systems can never be had in the actual world and yet the law of inertia obtains in such systems alone.[17]

Contextual realism also eschews the radical independence implication of realism in its original guises. But we think that the problem lies not in the correspondence theory of truth for the first-order scientific claims. We believe that without that, the connection between our representation and what is represented, the world, which is surely independent of our epistemic status, can never be properly characterized. We see the problem, instead, as a problem of neglecting the many grounding or anchoring metaphysical or semi-metaphysical assumptions or beliefs that laypeople or scientists legitimately make routinely. Realism without contextual considerations is bare realism that only applies to the most fundamental furniture of reality, whatever those are. But such a realism is too dogmatic for any reasonable philosophical allegiance. The reasonable assumptions or beliefs that soften realism are manifested as the *anchoring/grounding abstractions/idealizatins* that different contexts institute for insulating a body of knowledge so that realist claims are only evaluated within the limits of such

[16] I thank a referee of the journal for pointing out to us a recent work by Michael Shaffer, *Counterfactuals and Scientific Realism* (Palgrave 2012), which appears to have relevance to the position of contextual realism we defend here. It is a work that proposes and defends a contextual theory of idealization, which is in turn used to simplify the treatment of counterfactuals so as to relieve the pressure counterfactuals have on scientific realism. Upon viewing the work, we have come to the conclusion that it is not feasible to fully discuss in this paper the many ideas and arguments in Shaffer's work; nor does it at all render the position of contextual realism we defend in our paper redundant or superfluous.

[17] The analogy is actually evoked by Putnam (1981), p. 55.

constraints. Or we may look at the problem this way. One of the tasks for judging the existence claims about a disputed class of objects or the reference of a disputed set of terms is to separate objects into real versus fictional or to separate terms into referring or non-referring. Contextual realism seeks to make such sorting jobs more reasonable and convergent with the scientifically informed common-sense views rather than otherwise. For example, if realism makes the singularities in the TD account as phrase transitions below the critical temperature a fictional rather than a real thing in science, the realism is no good. Contextual realism makes such things real with good reasons.[18]

To some extent, contextual realism and its conception of anchoring assumptions have an affinity with a by now well defended idea that "less is different" or "less is better" or "less can be more explanatory," (see, Butterfield (2011), Batterman and Rice (2014), Morrison (2012)). Roughly, the idea is that many jobs of scientific description/explanation are better accomplished by abstracting away from the physical systems that are full of complexities and difficult to handle either theoretically or experimentally. This is also a standard argument now defending emergence or emergent properties or systems. For us, this is certainly correct because when some special abstractions or idealizations are in place, the simplified or beautified systems may contain "emergent" real objects that are otherwise non-existing. Without anchoring assumptions that ensure a context, cats and dogs will be no different from heaps of molecules, and phase transitions will appear no sharper than any changes of the configurations of heaps of molecules.

Epistemic contextualism also leaves the theory of truth alone. It does not necessarily embrace the correspondence theory, unlike the case of contextual realism, but it is entirely compatible with any theory of truth that makes the truth of our beliefs independent of justification. What is contextually dependent is the standards by which knowledge attribution is made. The different standards could be those of evidential justification, which implies an internalist position; or they could be about the qualities of reliable true-belief production systems, which then would be an externalist position. Either way, the everyday standard of knowledge claim may be radically different from the scientific standard for the same subject. The highest of all standard may well be

[18]There is a *prima facie* resemblance, in analogy, between the anchoring/grounding assumptions for contextual realism and what David Lewis calls '*sotto-voce* proviso' for knowledge claims, see (Lewis, 1996). I thank a referee for suggesting this analogy. The similarity in analogy between the two items is quite striking. Both are needed in their separate domains to dispel possibilities that render the respective beliefs, the truth of a claim or the reality of an entity, suspect. Both are often given, say in textbooks, as passing remarks, as in the case of infinite population, complete randomness, etc.

the Cartesian standard, which produces a version of skepticism that seems impossible to surmount for empirical beliefs.

Contextual realism has a similar structure, but the contexts are for claims about realness or existence, not about knowledge attribution; and the anchoring assumptions that define contexts are about features or facts of reality. As our examples in the previous section demonstrated, realist claims about entities or phenomena in the context of the SM approach could be quite different from such claims in the context of the KT approach. The context of TD is yet another different context. Crossing contexts may render what is a true claim of existence or realness no longer true. Phase transitions as singularities in the contexts of TD and SM is no longer true in the context of KT, although all three have ways to account for the observable phenomena of phase transitions. And parallel to the Cartesian standard for knowledge attribution, there may well be a zeroth level "context," the context for elementary particles or unified fields.

As we have mentioned earlier, the realist claims about the ultimate fundamental elements of nature, if there are such things, need no assumptions about any context for realism. But for many other types of things and the claim that they exist or are real, contexts have to be given, for otherwise they may simply be non-existing or false. The most common such contexts are the ones that ensure that the claims about the realness or existence of ordinary objects may be true. Does a table we see and feel exist when what "actually" exists is a heap of molecules? Many argue against ordinary-object realism precisely because what exists are heaps of molecules (see, Thomasson (2007)). But with contextual realism, ordinary objects can be real or exist if we have good reason to believe that there are anchoring conditions that properly insulate such objects. In fact, we may argue that the anchoring conditions for ordinary-object realism may have been given to us by our sense organs. Other anchoring or grounding conditions or assumptions are more theoretical or even metaphysical. Condensed matter may be regarded as continuous matter rather than being composed of multiple discrete units. Here disregarding the discontinuous nature of bulk matter is an anchoring assumption. The result is the legitimacy of making realist claims about TD qualities or entities. Another way of anchoring a context for realism for bulk (or condensed) matter is to *keep the discrete compositionality but take the TD limit*. It turns out the some of the same realist claims as in the context of continuous bulk matter remain true in this new context.

It is obvious that anchoring or grounding assumptions are essential to contextual realism. Contextual realists believe that realist claims about any types of disputed objects can only be evaluated as true or false within a context, and a context is determined or defined by its anchoring assumptions. But what are the anchoring or grounding assumptions? How are they defined and determined? These and other related questions need to be addressed before one can evaluate the viability of contextual realism. The function of anchoring assumptions is to insulate parts or levels of reality so as to secure reasons for believing of certain objects as real or existing that would otherwise not be believable. The parts or levels are not determined by conventions or language frames or convenience of practice or some such things. They are a feature of reality itself that makes the anchoring assumptions possible. In other words, the reality we live in might not be one that allows for anchoring assumptions to be reasonably made. A possible world in which nothing except a few identical particles move about and never interact in any way would not be a world in which anchoring conditions we have been talking about could be met. In that world, realism is realism simpliciter. In such a world, it does not make sense to claim that over and beyond the existence of those particles, there also exist objects of other types. But in our world/reality, contexts beyond the ultimate level do exist and there are objects that exist only in their own context. Change to another context, the objects "disappear." In a general sense, at the ultimate level of fundamental particles and/or fields, cats and dogs don't exist or are not real because in the context of nothing but individual particles, a cat may be more similar to a dog than to another cat. But it is entirely possible, or even very likely, that there is a level or context within which cats are more like each other than like dogs. Such a context may well be the context within which biology assumes an autonomous statues, and biological kinds, i.e., species are real or exist.

Therefore, contextual realism holds only if there are good reasons to believe in the assumptions that anchor parts or levels of reality. The assumptions may not be about anything in the realities that they anchor, but they are factual, although some may be mathematical facts. In general, they are assumptions of abstraction or idealization; but not any abstractions or idealizations could be anchoring ones. The anchoring abstractions or idealizations are those that locate the separation of parts or levels of reality. Different parts or levels are different realities or different contexts of reality, whichever way one prefers to call it, and the separation conditions are represented by the anchoring assumptions. Once such assumptions are justifiably made, the part or level with the assumptions is secured and claims about the existence of objects of the disputed types can be evaluated within that part or level. As to how anchoring assumptions are determined for a part or context, the question can only be answered by the practicing scientists. This is exactly how taking the TD limit as an anchoring assumption for recovering TD entities, such as phase transitions and critical phenomena, is discovered, i.e., by mathematical physicists.

We eschew in general any formal one-paragraph definition of any "-ism," but we can summarize the above articulation and argument for contextual realism as follows. Contextual realism is a version of scientific realism. The traditional versions of scientific realism may be regarded as about a particular reality of contextual realities. Perhaps that is a realism about the context of the ultimate furniture of reality. Contextual realism is also a version of local realism, which advocates claims about reality for this or that part of scientific discourse. Contextual realism provides a clear conception of a context of reality that is anchored by semi-metaphysical conditions (stated as assumptions) such that existence claims can be evaluated and objects that are referred to in the scientific account of that part of reality so anchored may be separated into "being real" and "being fictional." Thus, contextual realism provides a reasonable way of looking at models constructed for a part of reality that a scientific theory deals with and telling which represent real systems and which fictional ones. Anchoring assumptions are a special sort of abstraction or idealization claims that define a part or level of reality. Existence claims are always context dependent in that they are true, if they can be true, under the anchoring assumptions that define the context. Claims true in one context may be no longer true in another. How contexts are separated objectively is determined by nature; they are not determined by convention, or language frames, or the practice of science. Assumptions of abstraction and/or idealizations make explicit the conditions under which different realities (or parts or levels of Reality) are separated.

Again, contextual realism is different from epistemic contextualism, although they have a great deal in common in spirit.[19] Contextual realism is a position on realism, while epistemic contextualism is a position on knowledge attribution. The challenge that epistemic contextualism aims at meeting is the challenge of skepticism, while contextual realism aims at overcoming the challenge anti-realism poses, namely, how could one believe in the realness of such things as phase transitions and critical phenomena as singularities in infinite systems? Isn't it more reasonable to think of such things as pure theoretical instruments? Contextual realism as we have defined and defended it provides good reasons to believe otherwise. Epistemic contextualism is about knowledge attribution claims, while contextual realism is about reality/existence attribution claims. The contexts in epistemic contextualism are defined by different standards for knowledge claims, while the contexts in contextual realism are defined by special abstraction/idealizations assumptions.

[19] I thank a referee of the journal for urging me to clarify the difference between epistemic contextualism and contextual realism.

It might be argued that the contextual realism we have defended appears to have a great deal in common with the version of "ontological relativism" famously given by Rudolf Carnap (Carnap, 1950).[20] It raises the possibility that contextual realism is no more than Carnap's ontological relativism, or at least, an explanation is owed of why on top of that famous view on ontology, we need to have our version of contextual realism. Despite some surface similarities between Carnap's view on ontology and our view on realism, the two views/positions are completely different. First, let us see why it may appear that there is a strong similarity between the two views. Carnap argues that whether some entities, such as electrons and genes and even phase transitions, exist or are real depends on in which language framework such questions are asked. If in a language framework, claims about electrons are properly connected to claims that can be empirically tested, then electrons exist. Otherwise, electrons do not exist (if the language does not contain such explicitly empirical means of answering such questions). Therefore, ontology is relative to language frames, and the choice of language frames is a matter of pragmatics, namely, is it useful to scientists or not? This is no doubt too rough a characterization of Carnap's position, but we hope it suffices to clarify the issue at hand. If we identify Carnap's language frames with our contexts, isn't it rather obvious, as the objection goes, that our context dependent claims of what is real or exist and what is or does not, are essentially the same as the language frame dependent claims in Carnap?

The first thing we want to point out is that there is no good reason to think that Carnap's ontological relativism is, or could be construed as, a form of realism of any stripe. Carnap's ontological relativism is in fact one of the flagships for an empiricist or anti-realist position. The position we have defended is a realist position, it has no affinity with Carnap's position. But perhaps, it might be argued that our contextual realism is really a sham; it is really a disguised anti-realist position. We do not think there is any reason to support such a claim. In a similar manner, there is no good reason to believe that epistemic contextualism, say of the DeRose version, is in fact a version of skepticism, namely, since the entitlement of knowledge claims varies from context to context, there is no real knowledge at all. So, while formally, Carnap's ontological relativism is a form of anti-realism, just as relativism in epistemology is a form of skepticism, our contextual realism is a position of realism, just as epistemic contextualism is a position that aims at refuting skepticism.

In principle, we have disposed of the suggestion that our position of contextual realism is just another form of Carnap's ontological relativism. But we could go on

[20] I thank a referee of the journal for raising this objection in connection with Carnap's work.

and offer an explanation as to why the two positions appear to be similar and yet are fundamentally different. This goes back to how Carnap's ontological relativism should be properly understood. For Carnap, there is a clear distinction between an internal question and an external question about the existence of something. The internal questions concern the empirically determinable facts in a given language framework. The external questions are empty questions because the possible answers for them go beyond experience into the traditional realm of metaphysics. Such answers are without sense or meaning. A necessary (but not sufficient) condition for answering the internal questions is the selection of a language frame, which itself is a practical matter that is neither true or false but useful or not useful. This is the relativity thesis on ontology. It does not deal with the metaphysical question of whether realism holds or not. Contextual realism is not a relativist position on realist claims. It is first and foremost a realist position. The fundamental furniture of the universe, whatever they turn out to be or whether mankind can last long enough to finally discover what they are, exist independently of human cognition or language frames or any such cognitive fixtures. Secondly, it is a contextual realism in that not only the fundamental furniture but also such things as phase transitions as singularities in physically impossible infinite systems also exist entirely independent of any cognition, if they exist at all. And they exist in a context that is anchored by essential abstractions or idealizations. Anchoring idealizations/abstractions are not strategies of language use or stipulations about meaning (i.e. about the semantics of terms), nor are they a matter of practical utilities. They are claims about facts of the world, and the facts are those that define a part or level of reality or that distinguish one reality from another. Given that our arguments for contextual realism are cogent, realms of real entities are multiple and differ, sometime greatly, from one another.

Now, although we believe that the $\mathcal{F}_\infty$ account of phase transitions in SM admits a realist interpretation under contextualism and we have argued for it, we are not wedded to it. If eventually it becomes clear that such notions can only refer to fictional systems (because, among other reasons, the systems under the TD limit are literally of infinite size), it does not necessarily refute contextual realism. However, whichever way it comes down for the statues of such infinite model systems, whether they end up real or fictional, the reason for either should still be a contextual one.

The idea of contextual realism may also be grafted onto other categories. For example, we may have a contextual theory of natural kinds. We have no space in this paper to fully explain and defend such a theory, but the rough idea is that what counts as a natural kind or whether some kind can be legitimately counted as a natural kind is context dependent. When fully developed, such a view may be used to counter Quine's eliminativist view on natural kind terms (Quine, 1969).

7 Conclusion

The realist claim that the infinite system exists (is real) in which a phase transition as a non-analytic point takes place is true in the context of Statistical Mechanics. The context in this case comprises fundamental metaphysical beliefs about bulk-systems of molecules and the beliefs in the form of abstraction away from the details of the molecules. The TD limit is a necessary idealization that makes a rigorous account of phase transitions possible in the context of SM theories. Changing to a different context, a context of kinetic theory of the same physical system, for example, the realist claim can no longer be regarded as true. The paradox of phase transitions as understood in the literature is therefore, if we are right in this paper, an apparent paradox; it dissolves in the light of contextual realism. Our solution bares resemblances to almost all the attempts at resolving the paradox we discussed in the paper. But we differ from Shech (2013)'s in that we do not regard it as a problem of semantic or meaning of the terms, such as "phase transitions." We differ from Bangu (2009, 2015)'s in that ours is perhaps a more general or more philosophical solution. The data model versus phenomena distinction may be regarded as two different contexts in contextual realism. Mainwood (2005)'s solution is a multipronged approach that we shall not simply connect to contextual realism. And Norton (2014, 2012)'s approach also fits into the contextual realist approach when we realize that the burden of the second type of infinite idealization in Norton's sense differs from his first type precisely because the grounding metaphysical beliefs that define the context for the second type are radically different from those for the first. Abstractions are different from idealization in many aspects. The inattention to their differences in some of the discussions of infinite idealization is among the source of confusion.

The conception of contextual realism needs more clarification; more examples should be studied to explore the richness of this conception. Another important category of cases for infinite idealization consists of the different models for solving the quantum measurement problem (see footnote 7). And one needs to conduct a detailed discussion of Miller (1987)'s initial idea of contextual realism and Fine (1991)'s further development of it in his articulation and criticism of piecemeal realism.

References

Sorin Bangu. Understanding thermodynamic singularities: Phase transitions, data, and phenomena. *Philosophy of Science*, 66:S92–S106, 2009.

Sorin Bangu. Why does water boil? fictions in scientific explanation. In Michel Morange Anouk Barberousse and Thomas Pradeu, editors, *Mapping the Future of Biology: Evolving Concepts and Theories*, pages 319–330. Switzerland: Springer, 2015.

Robert Batterman. Critical phenomena and breaking drops: Infinite idealizations in physics. *Studies in History and Philosophy of Modern Physics*, 36:225244, 2005.

Robert Batterman. Idealization and modeling. *Synthese*, 169:427–446, 2009.

Robert W. Batterman and Collin C. Rice. Minimal model explanations. *Philosophy of Science*, 81:349–376, 2014.

R. A. Blythe and M. R. Evans. The lee-yang theory of equilibrium and nonequilibrium phase transitions. *Brazilian Journal of Physics*, 33:1–17, 2003.

James Bogen and James Woodward. Saving the phenomena. *The Philosophical Review*, 97(3):303–352, 1988.

Richard N Boyd. On the current status of the issue of scientific realism. In *Methodology, epistemology, and philosophy of science*, pages 45–90. Springer, 1983.

Jeremy Butterfield. Less is different: Emergence and reduction reconciled. *Foundations of Physics*, 41:1065–1135, 2011.

Craig Callender. Taking thermodynamics too seriously. *Studies in History and Philosophy of Modern Physics*, 32:539553, 2001.

Rudolf Carnap. Empiricism, semantics, and ontology. *Revue Internationale de Philosophie*, 4:20–40, 1950.

Anjan Chakravartty. Scientific realism and ontological relativity. *The Monist*, 94(2): 157–180, 2011.

Keith DeRose. *The Case for Contextualism*. Oxford: Oxford University Press, 2009.

Michael Dummett. Realism. *Synthese*, 52(1):55–112, 1982.

Arthur Fine. Piecemeal realism. *Philosophical Studies*, 61(1-2):79–96, 1991.

Peter Godfrey-Smith. Abstractions, idealizations, and evolutionary biology. In U. Maki et al, editor, *Recent Developments in the Philosophy of Science: EPSA13*, pages 47–55. Boston: Springer, 2009.

Yair M Guttmann. *The concept of probability in statistical physics*. Cambridge University Press, 1999.

Risto Hilpinen. Approximate truth and truthlikeness. In *Formal methods in the methodology of empirical sciences*, pages 19–42. Springer, 1976.

Martin R. Jones. Idealization and abstraction: A framework. *Poznan Studies in the Philosophy of the Sciences and the Humanities*, 86(1):173–218, 2005.

Eleanor Knox. Abstraction and its limits: Finding space for novel explanation. *Nous*, 50:41–60, 2016.

Ronald Laymon. Idealizations and the testing of theories by experimentation. *Observation, experiment, and hypothesis in modern physical science*, pages 127–146, 1985.

Ronald Laymon. Cartwright and the lying laws of physics. *The Journal of Philosophy*, 86(7):353–372, 1989.

David Lewis. Elusive knowledge. *Australasian Journal of Philosophy*, 74(4):549–567, 1996.

Chuang Liu. Explaining the emergence of cooperative phenomena. *Philosophy of Science*, 76:488–505, 1999.

Chuang Liu. Infinite systems in sm explanations: Thermodynamic limit, renormalization (semi-) groups, and irreversibility. *Philosophy of Science*, 68:S325–S344, 2001.

Chuang Liu. Approximations, idealizations, and models in statistical mechanics. *Erkenntnis*, 60(2):235–263, 2004.

Chuang Liu. Confirming idealized theories and scientific realism. *PhilSci-Archive, Pitt*, 2007.

Chuang Liu and Gerard Emch. Explaining quantum spontaneous symmetry breaking. *Studies in History and Philosophy of Modern Physics*, 36:137–163, 2005.

Paul Mainwood. Phase transitions in finite systems. *PhilSci Archive*, 2005.

Tarun Menon and Craig Callender. Turn and face the strange... ch-ch-changes: Philosophical questions raised by phase transitions. *PhilSci Archive*, 2011.

Richard W Miller. *Fact and method: Explanation, confirmation and reality in the natural and the social sciences*. Princeton University Press, 1987.

Magaret Morrison. Emergent physics and micro-ontology. *Philosophy of Science*, 79: 141–166, 2012.

Margaret Morrison. Approximating the real: The role of idealizations in physical theory. *Poznan Studies in the Philosophy of the Sciences and the Humanities*, 86(1): 145–172, 2005.

John Norton. Approximation and idealization: Why the difference matters. *Philosophy of Science*, 79:207–232, 2012.

John Norton. Infinite idealizations. In *Vienna Circle Institute Yearbook*, pages 197–210. Wien-New York: Springer, 2014.

Stathis Psillos. *Scientific realism: How science tracks truth*. Routledge, 2005.

Hilary Putnam. *Reason, truth and history*, volume 3. Cambridge University Press, 1981.

Willard V Quine. Natural kinds. In *Essays in honor of Carl G. Hempel*, pages 5–23. Springer, 1969.

Laura Ruetsche. *Interpreting quantum theories*. Oxford University Press, 2011.

Elay Shech. What is the paradox of phase transitions? *Philosophy of Science*, 80: 1170–1181, 2013.

Amie L. Thomasson. *Ordinary Objects*. Oxford University Press, 2007.

Bas C Van Fraassen. *The scientific image*. Oxford University Press, 1980.

John Woods and Alirio Rosales. Virtuous distortion. In & C. Pizzi W. Carnielli L. Magnani, editor, *Model-Based Reasoning in Science and Technology*, pages 3–30. 2010.

Chen-Ning Yang and Tsung-Dao Lee. Statistical theory of equations of state and phase transitions. i. theory of condensation. *Physical Review*, 87(3):404, 1952.