

*Hamlet: There's never a villain dwelling in all Denmark
But he's an arrant knave.*

*Horatio: There needs no ghost, my lord, come from the grave
To tell us that.*

Shakespeare, *Hamlet*.

1. Introduction. To do thermo-statistical physics (TSP) is in a large part to devise microscopic models for bulk matter and to use them to reconstruct, and thus to explain, its macroscopic behavior. When the bulk system in question is in a single phase and in equilibrium and of classical parts¹, the task is rather simple and straightforward. Complications arise if these three conditions -- single phase, equilibrium, and classical parts -- no longer hold. If only one of the conditions fails, the complication is usually manageable, and in this paper I examine some foundational problems in one of these cases -- multiple-phase systems in equilibrium with classical (or essentially classical) parts. The theoretical difficulties lie in the microscopic description and explanation of the transitions among, and transitional regions of, phases.

Another phenomenon which has long caused philosophical puzzlement and conceptual difficulties in TSP is the irreversibility of thermal processes. I shall show in this paper that there is a close connection between the treatment of phase transitions and that of irreversibility, and how the difference between the two shed light on the justification for such treatments. Besides this methodological connection of the two phenomena, one should note that they are the two major and basic *cooperative phenomena* in nature. They are cooperative because they involve processes which are more than a collection of random mechanical processes of microscopic parts and are not accountable simply by mechanics and statistics. We shall see how statistical mechanics of finite systems are incapable in principle of rigorously account for either phenomenon and radical measures have to be introduced to provide such an account. And further, they are basic because they involve the simplest *ordered* states in nature. Analogous but much more complex are such

phenomena as the transition from organic to inorganic matter and from life to death of an organism. If one is serious about reductionism in the belief that all phenomena are eventually reducible to physics of microscopic particles, one should learn one's lesson well with the reduction of phase transitions/critical phenomena and irreversibility.

In section 2, I give a brief summary of the chief methods by which a what is known as the rigorous micro-reconstruction of phase transitions (PT) and critical phenomena (CP) is given by the mathematical physicists. Such methods include taking the thermodynamic limit (TL) to recover PT and in using renormalization (semi) group approach (RG) to explain the universality of critical exponents. Then, I examine in section 3 various possible justifications for taking TL on physically finite systems. Section 4 discusses the legitimacy of applying TL to the problem of irreversibility and assesses the repercussion on its legitimacy on its home turf.

2. Phase Transitions and the Renormalization Group. The experimental observations and the purely thermodynamic (TD) -- i.e. phenomenological -- account of PT are unproblematic. For a system of fluid, which may be in the phase of solid, liquid, or gas (cf. Figure 1), its thermodynamic states are determined by three variables, p (pressure), V (volume), and T (temperature). The equation, $f(p, V, T) = 0$, defines a surface in the phase space whose projections onto two-dimensional spaces are shown in Figure 1. Very similarly, for a system of magnet, its thermodynamic state are determined by three analogous variables: B (external field), M (magnetization), and T (temperature). A similar equation defines similar surface on its phase space (cf. Liu 1999). From Figure 1 (a) and (b), one can read off PTs as regions in the phase space which are bounded by singular points, such as A and B and a critical point as the terminating singular point of a phase boundary line. The points are singular in the sense that derivatives such as, $\partial p / \partial V$, are discontinuous function there. For instance, when a system reaches the region bounded by

A and B, an increase of volume at constant temperature will not correspond to a change of pressure, which appears to be a singular behavior in TSP.

Idealizations are involved both in plotting the data into a mostly smooth isotherms and in deriving them from the laws of TD, the most important of which is that bulk matter are *dense* so that *no fluctuation* occurs, and therefore the isotherms are mostly smooth. Such an idealization is necessary because no experiments, no matter how finely tuned, can ever determine whether the 'corners' which bound PT regions, such as A and B in Figure 1(b), are sharp or round, and the theoretical reason for preferring sharp corners (and hence singular points) is quite simple: because otherwise, the isotherm would curve back up above A after passing it downward before curving back down towards B, forming an horizontal 'S' shape which entails such absurd conclusions as that $\partial p / \partial V > 0$ at some points, namely, it is possible to increase a system's pressure by expanding its volume at a constant temperature.

Behavior around the critical point constitutes CP and is thermodynamically accounted for by a set of power laws, which show how various bulk variables of a system in the critical region vary with $t = (T - T_c) / T_c$. For fluids, they can be summarized as follows.

When ($t \rightarrow 0^\pm$),

a. compressibility at constant T	$\infty \begin{matrix} t^{-\gamma} \\ (-t)^{-\gamma'} \end{matrix}$	$\begin{matrix} (t > 0) \\ (t < 0) \end{matrix}$
b. density difference at PT	$\infty (-t)^\beta$	$(t < 0)$
c. pressure-density ratio:	$ p - p_c \propto \rho - \rho_c ^\delta$	$(t = 0)$
d. specific heat at constant V	$\infty \begin{matrix} t^{-\alpha} \\ (-t)^{-\alpha'} \end{matrix}$	$\begin{matrix} (t > 0) \\ (t < 0) \end{matrix}$
c. correlation length	$\infty \begin{matrix} t ^{-\nu} \\ t ^{-\nu'} \end{matrix}$	$\begin{matrix} (t > 0) \\ (t < 0) \end{matrix}$

where $f(t) \propto t^x$ means $x = \lim_{t \rightarrow 0} (\ln f(t) / \ln t)$. There is a similar set of power laws for ferromagnets with the same set of exponents (cf. Stanley 1974). These exponents, $\alpha(\approx \alpha')$, β , δ , $\gamma(\approx \gamma')$, $\nu(\approx \nu')$, are called *critical exponents*, and the fact that whole classes of radically different systems and phenomena shared the same critical exponents is known as the *universality*, which has no explanation in TD. The systems and phenomena can be as different as between fluids for the transition from the one-phase state of gas to the two-phase state of gas and liquid and magnets for the transition from the paramagnetic to the ferromagnetic phase. Therefore, the universality seems to say not only that radically different physical systems behave similarly near criticality but also that liquidation is no different from spontaneous magnetization! (Try to imagine your being told that there is a certain state of the earth at which not only all human beings necessarily walk (or run, etc.) at roughly the same pace and all dauphins necessarily swim at roughly the same speed but the pace of human's walking (or running, etc.) is also very likely to be necessarily the same as the speed of dauphin's swimming.)

To recover, and improve upon, the above results in statistical mechanics (SM) via Hamiltonian mechanics -- plus some statistical assumptions -- a bulk system must be modeled to comprise parts of one or a few kinds that possess their properties independently of where they are in the system -- i.e. they are *transitional invariant*. Typical examples include the van de Waals models for gas and the Ising-type models for ferromagnets and lattice gases (cf. Goldenfeld 1992). This is an essential idealization without which almost nothing can be done in SM, as I shall now briefly show. Given such a model, all thermodynamical magnitudes, except V, N, T , are obtainable through a reductional scheme of which the following is a typical example².

‡ *The reconstruction* $H \Rightarrow \langle H \rangle$: from the microscopic total energy to the average energy.

1. From mechanics: $H(p, q, s)$,
2. via the distribution: $\rho = (1/Z)e^{-\beta H}$

$$\text{(or } \rho = C \text{ or } \rho = (z^N / Z_G) e^{-\beta H}, (\beta = 1/kT)$$

3. to thermodynamics: (i) $Z = \text{Tr}[e^{-\beta H}]$
(ii) let, $Z \equiv e^{-\beta F}$
(iii) $-\partial_\beta Z = F e^{-\beta F} = \text{Tr}[H e^{-\beta H}]$
(iv) $F = Z^{-1} \text{Tr}[H e^{-\beta H}] = \langle H \rangle$.

In step 1, we have mechanics -- classical, semi-classical, or quantum -- which gives the total Hamiltonian of the microscopic parts and the Hamiltonian laws of motion that the parts obey. Step 2 introduces the statistical elements of SM in the form of three distribution functions: canonical distribution for systems which exchange only energy with its environment, microcanonical distribution for isolated systems, and grand canonical systems for systems which exchange energy and particles with its environment, respectively. In step 3, one calculates the partition function with the *Trace* ($\text{Tr}\{\bullet\}$) operation over all parts of the system. Here $\text{Tr}\{\bullet\} = \text{Tr}\{\bullet\}$ (for a quantum system) or $= \bullet(\text{¥})$ (for a discrete system) or $= \int(\bullet)ds$ (for a continuous system). The rest is a simple derivation. This derivational scheme shows why the idealization is essential; for otherwise -- if each part has a distinct Hamiltonian -- neither H nor the Trace operation would be manageable.

This scheme of SM works exceedingly well, within the acceptable limit of approximation, for single-phased system at equilibrium. One may reasonably expect that it also work well for multiple phases at equilibrium, but it does not! To explain this let us begin with a system in the far right region of Figure 1(a) (or far top of Figure 1(b)) and suppose we have written out its partition function, which, as shown above, is a function of T . Then, let T drop pass the critical temperature. If our theory is right, the partition function should have in it the phase boundary between liquid and gas and, particularly, it should contain the two singular points, such as A and B in Figure 1(b) -- the two ports, if you like, of a PT region. Similarly, we should in principle be able to reproduce, with

some negligible give and take, from the partition function the power laws of CP and an estimation of the critical exponents.

Here comes the dramatic twist: one can tell with or without carrying out the Trace operation that the partition function of a finite system -- the interaction among whose parts is stable and strongly tempered -- can never harbor any singularities! One can see this either in the algebraic properties of any such partition functions or in the solution of a partition function for a special system, i.e. a normal 2-dimensional Ising system (the latter is an anachronism, see the paragraph after the next). In other words, if a partition function accurately describes all isotherms in Figure 1(b), then whatever shape it gives to an isotherm below T_c , the isotherm will not have the two 'kinks', such as the ones at A and B; nor does it show that when the temperature changes across the critical point there is a discontinuous change from a one-phased system to a two-phased one (or *vice versa*)

To give a very brief sketch of this simple argument (for more details, see Liu 1999), let us suppose that we have a system modeled as follows: it comprises a finite number N of identical parts in a finite volume V and satisfies two conditions for the interactions among its parts: it is *stable*, which means that only a finite number of parts can be crammed into V , and it is *strongly tempered*, which roughly means that a system of large N won't explode.³ The partition function of such a system is obtained by two steps: first we formulate a partition function for each number N of parts and then we sum over all possible number of parts, from zero to the maximum N_{max} . The first function is $Z_N = Tr_N \{ e^{-\beta H} \}$, (i.e. the canonical partition function) and the second $Z_G = \sum_{N=0}^{N_{max}} z^N Z_N$, where $z = \exp(\mu / kT)$ is the term caused by the exchange of parts with the system's environment. Since z is positive, Z_G is a polynomial with a positive coefficient for every term. According to SM, the only way for there to be singularities is if $Z_G = 0$; and the roots, if there are any, of this equation, $z_c = \exp(\mu_c / kT_c)$, should give us the critical temperature. But the theory of polynomials tells us that there exists no real positive roots for $Z_G = 0$, which implies that there is no temperature at which multiple

phases appear or disappear.⁴ In other words, we are stopped at the gate and no simplifications or short-cuts along this line can even get us off the ground.

One may find this a perfect case for anti-reductionism or emergent properties. The impossibility of rigorously reconstructing PT and CP from micro-models may well be taken by some as the nail which could finally seal the coffin of reductionism. For it appears to have deprived reductionists of their prize possession -- a clear-cut case of reduction of TD to SM. Indeed, this seems to involve a failure of type-type identification across theories (Fodor 1974) that renders impossible a reduction as domain replacement (Nickles 1973). There is no distinct type of micro-states in SM which can be identified with the type that TD singles out as PT or critical points (i.e. singularities).⁵

However, mathematical physicists thought differently, and some of them pursued and eventually proved that PT as a type can be rigorously accounted for by SM (see Yang 1972; Griffiths 1972, I-VI). The key idea is both simple and radical: take the volume and number of parts of a bulk system to infinity, while keeping the density finite -- a maneuver now known as 'taking the thermodynamic limit (TL).' If the limits for the major bulk variables exist -- they exist for all systems whose internal interactions are stable and strongly tempered, singularities are proven to emerge again from partition functions (see Liu 1999 for more details). For a simple model -- the 2-d Ising model -- whose partition function can be exactly solved, one obtains both its value of T_c and its critical point under TL (Onsager 1944; Schultz et al. 1964). TL is generally justified as an *idealization* of bulk systems whose size and number of parts are so large that compared to their individual parts they are 'approximately' infinite.

The next natural step is to come up with a micro-explanation of CP: to derive the power laws and recover the values of the critical exponents. This is accomplished by the methods of *scaling* and *renormalization*. By scaling, a 'master equation' for the free energy density is obtained, from which various equality relations among different critical exponents are derivable, and such relations effectively reduce the number of independent

exponents from half a dozen to only three. Then the *renormalization (semi-) group* (RG) theory is discovered to derive the master equation -- or the *scaling hypothesis* -- from the microscopic models and to determine the values of the independent exponents. The RG theory is perhaps better understood as an approach or method, of which only the most general scheme and its application to special cases are worked out. The central idea of RG is coarse-graining by lumping a large number of degrees of freedom together and 'decimate' them (cf. Fisher 1982, 1998; Goldenfeld 1992). Let us see, on a conceptual level, first why this is needed and then how it can be done.

For the success of SM, which uses Trace operations to bridge the macro and the micro, the micro-models must be made as such that the description of a bulk systems is insensitive to the level or scale of description; or more figuratively speaking, when one descends (or ascends) from the macro- (or the micro-) scale of a system, the general configuration in one's 'view-finder', so to speak, stays very much the same. For the single-phased system, this requirement is already satisfied by the standard models, such as the ideal gas and the Ising model -- the random distribution of values for its micro-variables is the same (to a good approximation) at whichever level one focuses one's 'view-finder'. This is why the straightforward application of Trace operations over the Hamiltonians of micro-parts gives us the right macro-properties. However, this is no longer true when a system is near its critical point, where *long-rang orders* -- stable correlations -- among parts form; and what a region of such a system 'looks' like at a micro-level usually does not resemble what it appears at a, say, mesoscopic level. In other words, if we insist on trying to carry out the normal Trace: $Z_N = Tr_N \{ e^{-\beta H} \}$, as if values are still randomly distributed all the way down, we will get a distorted picture of the system's thermodynamic behavior near the criticality.

What is needed is a systematic -- e.g. a re-iterative -- set of transformations which *coarse-grain out* the degrees of 'freedom' that are tied up by the long-range order. For an Ising-type lattice model, blocks of spin sites are clumped together and considered as

single sites, and the old interaction parameters among the original sites are *transformed* into new ones for the coarser set of sites. Then, the lattice is rescaled -- which may be seen as switching levels of description -- so that it 'looks' the same as the original system except with fewer degrees of freedom (cf. Figure 2). Its end result in terms of physics is to preserve all the thermodynamic properties of the system as one descends to the micro-levels. Such are the *renormalization transformations* which form a *semi-group* and which decimate the 'fake' degrees of freedom in $\{s^\infty\}$. Hence, the original set of micro-variables is divided into two: $\{s\} = \{s'\} + \{s^\infty\}$, where $\{s'\}$ contains the remaining degrees of (true) freedom after the decimation. Once the degrees in $\{s^\infty\}$ are collapsed, we only have those normal variables as if there has never been any long-range order. To these variables the Trace operation is applicable again, and we shall have, instead of the original partition function,

$$Z_{N'} = Tr_{N'}\{exp \tilde{H}'(s')\},$$

where for the sake of convenience we have $\tilde{H} = -\beta H$. Now, because of the decimation, we have $N' = N/l^d$ and $\tilde{H}'(s') = \mathbf{R}_l \tilde{H}(s)$, where \mathbf{R}_l ($l > 1$) is an element of RG, l the number of sites that \mathbf{R}_l decimates, and d the number of dimensions of the system. RG, thus, allows us to partially trace out the expression, $exp \tilde{H}$, so that we have: $exp \tilde{H}' = Tr_{(N-N')} [exp \tilde{H}]$, where $N - N'$ is the number of 'fake' degrees. By 'shrinking' the system $N \rightarrow N/l^d$, one also shortens the correlation length: $\xi \rightarrow \xi/l$, so that when one descends toward the critical point, one may keep the correlation length in one's 'viewer' manageable by the reiterations of \mathbf{R}_l . All these coarse-graining transformations must guarantee the unitarity of \mathbf{R}_l , namely, the partition function must be invariant under the transformations: $Z_{N'}[\tilde{H}'] = Z_N[\tilde{H}]$, which simply means that the TD of the system in question remains invariant under \mathbf{R}_l . Moreover, we have, $f(\tilde{H}') = l^d f(\tilde{H})$.⁶

Besides a few highly simplified cases, such as the 1-d Ising model, the RG transformations are usually too complex to obtain exactly. But is it usually not too difficult to estimate the global features of RG and to obtain linearized local behavior at

some special regions. Figure 2 gives an illustration of what the global features of the RG actions may look like. It shows a submanifold of the total Hamiltonian space, H (usually infinitely dimensional), spanned by thermodynamic fields: t, h, g . Different types of systems with different Hamiltonians initially occupy different regions in this space and are then driven, as *flows*, by RG transformations to other regions, the most interesting of which are the *fixed points* and their *critical manifolds*. The former are defined as those points which satisfy the condition:

$$\tilde{H}^* = R_l \tilde{H}^* \text{ (for any } l > 1\text{)}.$$

These are singular points -- somewhat like the blackholes in Einsteinian spacetime -- into which once a system 'flows' it can never flow out. And the latter consist of all those initial points in H which eventually flow into the respective fixed points: $\tilde{H}^* = \lim_{l \rightarrow \infty} R_l \tilde{H}$ (see Figure 2). Fixed points represent different thermodynamical states of a system, some stable and some unstable. One type of fixed points are known as *sinks*, which represent the state when $T \rightarrow 0$ or $T \rightarrow \infty$, where a system once there not only will never flow out on its own but also cannot be brought out by any manipulations of other thermodynamic fields. Another type are the *discontinuity fixed points* which represent the PT regions such as those at $T < T_c$ in Figure 1(b); and most importantly, there are the *critical fixed points*, which separates the single-multiple phase regions, such as the one at T_c in Figure 1(a). These global features of H tells us several important things: (i) once a system is found or put on the critical manifold of, say, a critical fixed point, then the values of its thermodynamic fields mark the critical point in the physical phase-diagram; (ii) the CP of that system at that critical point can be obtained by investigating the properties of the system's \tilde{H} in a linear approximation on the critical manifold.⁷ Lastly and most importantly, the universality of critical exponents can be read off H and its flows as the fact that a whole class of many different submanifolds flow into the same fixed point.

3. Interpreting TL and RG. Even though TL grounds the entire rigorous approach to SM, we all know that actual systems in which PT and CP take place are finite. Hence, what justifies the taking of TL?

There is a seemingly straightforward answer (for details, see Liu 1999): PT appears in TD as singularities because of a special idealization I emphasized earlier: bulk systems are considered dense with their parts (or without distinct parts) and so without fluctuations. The idealization is a good approximation because molecules in any bulk system are so small and so numerous that the system is practically continuous. This idealization is inapplicable in SM because given bulk systems are stable, as I mentioned before, only a finite number of molecules can be crammed inside a finite volume. To compensate, we take the size and the number of molecules to infinity while keeping its density finite⁸. TL effectively kills the random fluctuations in a system so that the isotherms may again appear mostly smooth and PT as singularities.

Therefore, the singularities which represent PT and CP should be regarded as mere artifacts or fictions of TD, which do not exist in reality. PT are just places where rather dramatic changes of thermo-variables take place so that their derivatives make sharp, but not discontinuous, changes. If this is right, one should retire both idealizations and treat PT and CP as what they really are. Hence, there shouldn't have been any 'crisis' of the 'impossibility of PT' or any 'no-go' theorems, and the effort to introduce TL seems a consequence of the failure to notice the simple truth that a crucial idealization in TD should no longer be honored in SM.

Even though I believe the account of the two idealizations given above is right, the conclusion may sound outrageous. It implies that the rigorous methods (including RG) are a waste of time, which has the implication that PT and CP do not form distinct kinds -- qualitatively different from other states of a bulk system. Should we not then extend the conclusion to such conjectures as that none of the other kinds, such organic vs. inorganic or life vs. death, are distinct kinds, but rather they are merely quantitatively

different configurations of molecules in a system? One can think of a number of ways to get around this conclusion, if one wants to; and here I can only discuss a few. One approach is not to deny the main thrust of the conclusion but to appeal to the pragmatic virtues of TL for its defense. Another is to fully embrace special sciences, such as thermodynamics and biology and regard their kind predicates as of genuine natural kinds despite the micro-structures' showing otherwise. Yet another is to separate mathematical predicates from the physical properties they are used to pick out. The predicates may be exactly defined and related, but in their applications a certain range of approximation is introduced so that the predicates can pick out those objects whose properties they refer to fall within the range. I shall discuss them briefly in turn.

First, we should not forget that there are 'approximate' models, such as the mean field models, which reproduce PT and CP with finite systems (cf. Wannier 1966; Goldenfeld 1992). The fact that they make wrong predictions does not mean that they are not improvable. The 'no-go' theorem does not necessarily apply to mean field models because they do not assume the stability and strong temperedness conditions for the interactions. Onsager's success with a 2-d Ising lattice at TL and $B = 0$ certainly lends strong support for TL, but it is such a special case which is unlikely extendible to any other more realistic situations (not even for the same model at $B \neq 0$). The strongest practical virtue of TL is that it makes the RG approach possible, and RG is such a brilliant approach that it is not only recognized by a Nobel Prize but also has applications in quantum field theories.

Even if one discounts such 'external' merits of RG, there is the alleged success in giving an *explanation* of the *universality* of critical exponents, which in TD is merely a curious coincidence (Fisher 1998; Batterman 1998). Looking at Figure 2 again, we see that all systems whose physical criticalities are eventually driven by an infinite iterations of RG transformations into a fixed point belong to the same universal class and share the same set of values for their critical exponents. From the critical manifold we also learn

which thermodynamic fields are relevant and which not. The irrelevant ones have no effect on a system's critical behavior while the relevant ones do. But what kind of explanation is this? The question is: why should CP as different as the opalescence of fluid and the spontaneous magnetization of magnets behave the same near their own critical points? The answer from the RG approach is: because their physical critical points flow (driven by RG transformations) into the same fixed point! I would think that the right kind of answer to the question should say something about how the composition and structure of a bulk system do not matter or make little difference near a critical point, but what we get from RG does not even begin to tell us that. In fact, isn't it still a mystery as far as understanding the physics of it goes that a fluid behaves the same way at the opalescence as a ferromagnet at the onset of spontaneous magnetization? As Balashov (1997) rightly pointed out that an explanation has to come from a specification of the physical conditions rather than the 'mathematical' or 'topological' facts in a Hamiltonian space. And the physical condition, as Balashov again pointed out, is the cooperative behavior, the long-range order, near the critical point; but we don't need RG to tell us that. What RG does not provide, and which is what an explanation of universality needs, is the exact physical conditions near criticality -- presumably, the irrelevance of which *kinds of compositional* and *structural* features -- that are responsible for diverse systems' having the same critical exponents. Otherwise, it is a bit like explaining why all planets revolve around the sun by telling us that the geometric shapes of their orbits all belong to a set of conic sections. Suppose that besides this geometric fact we also know that the planets are constrained by some kind of force (analogous to our knowing that long-range order is responsible for CP), but that we don't know the exact nature of it and how it affects the motion of the planets. Do we have an explanation?⁹

There is a great deal of physics that we don't yet understand about CP; for instance, we know that some thermodynamic fields are relevant and some are not, and

that the relevant ones may drive a system off the critical manifold and the irrelevant ones cannot. But why? What makes some fields relevant and others not? What is the physics behind this? RG has no answer. However, universality is indeed no longer a mere coincidence in RG as it is in TD.

Even if the pragmatic answer succeeds, one may still find the main thrust of the conclusion unacceptable. By accepting it one would have in fact abolished PT and CP -- so far as they are distinct thermodynamic kinds -- as genuine physical states or properties, and all of our talks of PT and CP in terms of TL and RG are talks about some artifacts or fictions. Moreover, this will have to apply to all the other special sciences, such as chemistry and biology. Therefore, the second response begins with TD and takes points of PT and CP as singularities (or whatever distinct kinds) to be about genuine properties of bulk matter. Then, whatever our hypothesis of the micro-structure of reality is, it will have to be made compatible with the above by recovering at least PT and CP. Therefore, the 'idealization' in TD -- if it is at all an idealization -- is not to be removed simply because we realize that bulk matter consists of discrete parts. If this is right, when we realize that finite systems with the posited micro-structure, such as the Ising-type models, forbid PT and critical points, either we come up with a good idealization or the models are thrown out. TL appears to be the only idealization by which all microscopic models are saved with regard to PT and CP. Therefore, TL is not an idealization to compensate or 'cancel out' the 'idealization' in TD concerning PT and CP; but rather it is one to compensate the artificiality of such micro-models as the Ising-type models or of such conditions as the stability and strong temperedness of inter-molecular interactions. To argue for this response, one may invoke the spirit, albeit not the actual theory, of logical-positivism and first affirm the primacy of TD, since it is closer to the observables. One then looks at the lattice structures in the Ising-type models and the two stipulations for the interactions between lattice sites as equally theoretical as the theoretical size of a system which may harbor PT and CP. Then, TL

may be seen as the necessary theoretical element in a consistent and an optimal theoretical reconstruction of PT and CP. All the elements -- at least the ones mentioned above -- in this theory are equally idealized and may be subject to the same kind of philosophical criticism as TL is. But the fact that they together recover the thermodynamic description of PT and CP gives them justification as an integrated whole. It would be inappropriate and unfruitful if we single out TL for such a criticism.

This would be acceptable if TL is a normal kind of idealization. Idealizations are acts in theory construction by which conditions are said to obtain when they actually do not. Normal idealizations specify at least physically possible conditions; but TL seems to be flat out contradictory: a finite system is of infinite size!¹⁰ Even though it is, I believe, a problem for this approach, one may have a way out, as some discussions in the mathematics literature seem to suggest (Griffiths 1972). Even though PT and CP do in fact happen in finite systems, what really takes place there is entirely independent of the systems' sizes (given the densities remain the same). It means that where the boundary of the system is has no relevance to the behavior in question.¹¹ This can be seen in the fact that PT and CP are described entirely by intensive quantities, such as the free energy density, when the TL of these quantities exist. Therefore, even though finite and infinite systems are logically incompatible, it doesn't really matter because the physical properties of PT and CP are identical in both kinds of systems. But there is a subtle shift in this argument which may ultimately defeat it. What are the same are the thermodynamic phenomena of PT and CP in a finite system and the SM account of them in a corresponding infinite system. What we do not and cannot know is whether some (or any) SM account of PT and CP in a finite system is the same as in a corresponding infinite system. If we know that is true, then we are indeed justified to use the results in an infinite system to account for the thermodynamic phenomena of PT in a finite system. But if we know that, we wouldn't be taking the trouble of going TL.

The third response is probably the closest to the truth for our case. Predicates we used to describe PT and CP in TD or infinite systems in SM are mathematical ones which result from an accentuation or exaggeration of the corresponding physical properties by neglecting or filling out negligible differences. With such predicates, scientists must demand strict exactness among their relations, whether they are lawlike or reductional statements. But when such predicates are applied to actual physical systems, estimates of approximation are brought in so that the right kind of systems are pick out by the predicates. For instance, being a critical point as a mathematical predicate is defined by a singular point on an isotherm; but when using it to pick out a physical critical point a certain range of approximation to the singular point should be understood so that it picks out the right set of systems. This is similar to our use of most exact magnitudes, such as 'weighing 100 kg'. We are justified to use it to pick out objects whose weight is not exactly 100 kg but very close to it. Here, unlike in the first response, most mathematical predicates we use in physics or other scientific disciplines whose precision cannot always be assumed in their applications do pick out genuine properties, just as 'weighing 100 kg' does, even though a certain degree of approximation is implied in their meanings. Unless there is a danger that approximation of any kind, when introduced to the SM predicates of PT and CP, may compromise them so much so that it is impossible to use it to pick out the right systems (which are picked out by the corresponding predicates in TD), we do not seem to have a problem here. If this is right, infinite systems provide the extension for a mathematical predicate which when applied to real physical systems picks out finite but very large systems. It is not difficult to imagine that this is how all mathematical predicates which depend on limits or convergences, such as being differentiable, being continuous, and being smooth, should be understood. One should note that this response may not apply to all kind-predicates in special sciences; it may only apply to those quantitative ones which are related by precise mathematical relations.

4. TL and Irreversibility. If TL is not just a mathematical trick for handling the problems of PT and CP, it ought to apply to other problems concerning bulk matter; it ought to work, for instance, for a system approaching equilibrium from a non-equilibrium initial state. But does it and should it? Here I briefly explore this question which, to my best knowledge, is broached in the philosophy literature by Sklar (1993, 244ff, 277ff).

Boltzmann's H-theorem (without its probabilistic interpretation) says roughly that the H-function of a system (= the negative of the entropy) can never increase through time, given it satisfies the simple assumptions of exclusively binary collisions (a pure mechanical assumption) and of Stosszahlansatz (a statistical assumption). This produces two problems. The *reversibility problem* (Loschmit) says that since all mechanical processes taking place at the microscopic level are reversible, for any process that leads the system towards an equilibrium state there may be an exact reverse of it which leads the system away. And the *recurrence problem* (Zermelo) says that given Poincaré's theorem of recurrence -- which says that an isolated system of finite size, number of parts, and energy will return in finite time to an arbitrarily small neighborhood of almost any state from which it begins -- one should expect that a system will return in finite time to the neighborhood of a non-equilibrium state if it begins there. From either problem, the approach to equilibrium is not monotonic and the equilibrium state not stable.

The similarity with the case of PT and CP is striking: the H-theorem is intended to recover the second law of TD in SM, just as the evaluation of the partition function is for recovering the thermodynamic description of PT and CP; and the reversibility or the recurrence problem gives a kind of 'no-go' result to monotonicity as the 'no-singularity' result does to PT and CP. Several questions arise at this juncture, of which I can only discuss two: Is TL used to solve the problems? And should it be so used?

Introducing TL makes, *prima facie*, a big difference. Poincaré's theorem of recurrence in its current form is no longer true, for it assumes finite system with finite energy. Taking TL throws out at least one of these conditions. Can the theorem be

extended to infinite systems? That is not clear at all. If the energy is made unbounded as well as the size and number of parts infinite, the theorem clearly cannot hold. Even if the energy is finite, the theorem still seems unlikely to hold. If Poincaré's theorem fails, the recurrence objection can no longer be made. However, Loschmit's objection still seems to stand since whether or not the mechanical processes of micro-constituents are reversible has nothing to do with whether or not the system they comprise is finite. It is not even clear whether taking TL helps Boltzmann's probabilistic interpretation of his H-theorem, especially if it is assumed that all micro-states are equally probably, i.e. each 'towards-equilibrium' state is equally probably as its reversal, an 'away-from-equilibrium' state (cf. Sklar 1993, 42). If it helps at all, it may help in getting monotonicity back into a *local evolution* of H from a non-equilibrium state. If for a finite system, the overwhelming majority of states are at or close to equilibrium, for an infinite system the set of states *not* at or close to equilibrium will be of zero measure. But do we really want strict monotonicity? I shall come back to this point.

Returning to the recurrence problem we should realize that TL has been introduced, not to extend Poincaré's theorem but to directly derive the H-theorem from certain models. Kac (1956; also Wannier 1966) devised a 1-dimensional ring model in which taking TL produces a monotonic approach to equilibrium (i.e. no recurrence). But the model is too abstract to be useful even for idealized gases. TL is applied to a model of gas by Lanford (1983) and monotonicity (non-strict) is proven for a short time after any initial states ($1/5$ of the mean-free-time). We begin with a Boltzmann gas model and apply to it the Boltzmann-Grad limit, which sends the volume and the number of particles N to infinity, while the diameter d of the particles goes to zero in such a way that Nd^2 is finite but $Nd^3 \rightarrow 0$ (so the density goes to zero); and thus the gas becomes infinitely diluted with exclusively binary collisions. Even with such a dramatic TL, what is proven for $1/5$ of the mean-free-time is *not* that for any system if it begins ($t = 0$) at a state x near f , $T^t x$ ($t > 0$ and small) is near f_t , (where $t: f \Rightarrow f_t$ is due to the

Boltzmann equation and $T: x \Rightarrow T^t x$ to mechanics of particles), but rather this: the overwhelming majority of those systems at a state x near f end up via T near f_t .¹² No extension of this result to a larger time scale has been produced so far, although reasonable speculations point to a successful extension (and the mean-free-time of such an infinitely diluted gas is not as short as one might assume).

Although Lanford's model does not 'recover' Boltzmann's H-theorem and the second law of TD, it (together with Kac's) points to the necessity of TL in such endeavors. But now we must confront a more serious question, namely, whether H-theorem should be recovered in its strict sense. If it is indeed true that what H-theorem represents is not a law of nature but the result of some widespread initial/boundary conditions of the actual universe, then the reversibility and recurrence problems should no longer be problems and Boltzmann's probabilistic interpretation should be the right answer. Judging from the latest investigations of these issues (cf. Callender 1998), the more sensible attitude towards them seems to be that we should fully accept the statistical nature of irreversibility and reconcile the fact that our current cosmological regime appears to phenomenologically support the strict H-theorem with the set of special initial conditions (low entropy states) of the regime and the smallness of the recurrences within the time span of the regime. Such an attitude seems to re-enforce one of the earlier views of TL in PT and CP, namely, we should fully accept the 'non-singular' nature of PT and reconcile the fact that PT and critical points appear to be singularities with the smallness of fluctuations in bulk matter.

However, one may still ask why such disparity in the attitudes towards the use of TL in the community of physicists and mathematicians; why TL was taken so serious in dealing with the recovery of PT and CP and yet not nearly as seriously in dealing with Boltzmann's H-theorem. There is a dissimilarity in the two cases which may contribute towards an explanation: PT and CP need to be qualitatively distinguished from other states of a bulk system in TD (as distinct kinds) while no such distinction is needed for

processes from non-equilibrium states to equilibrium states. All such processes should obey the H-theorem however it is interpreted; so one does not have the problem that under one interpretation the 'H-processes' are threaten to collapse into another qualitatively distinct thermodynamic kind while in another interpretation they are not.

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¹ I shall in this paper use adjectives such as 'classical', 'quantum', 'purely thermodynamical', etc. to mean the ways in which properties or objects are regarded in the appropriate theories. In this sense, the same object or property can be called classical or quantum or thermodynamical or mechanical, depending on in which theory it is regarded.

² I thank Gerd E. Imre for suggesting this example to me.

³ Conceptually, stability against implosion (stability) and stability against explosion (strong temperedness) implies thermodynamic stability (existence of TL). Mathematically, stability = homogeneity: for certain reasonable functions, $h, h(ax) = ah(x)$; strong temperedness = subadditivity: $h(x + y) \leq h(x) + h(y)$; and thermodynamic stability = convexity: $h(ax + (1 - a)y) \leq ah(x) + (1 - a)h(y)$. (Cf. Wightman 1979)

⁴ Although $z = 0$ is a root, it is not physically meaningful.

⁵ For an extended discussion of issues in multiple realizability and universality which touches on this Fodor and reduction, see Batterman (forthcoming).

⁶ \tilde{H} is usually written as $\tilde{H}(\{s\}, t, h, g, \dots)$, where $\{s\}$ are all the micro-variables and t, h, g, \dots are the macro-variables, often known as the 'thermodynamic fields' because their values determine thermodynamic states; and therefore, they determine where the system begins in H and where it flows to under RG (see Figure 2). Such fields include coupling strength/temperature, external fields, and any other relevant external influences.

⁷ In a nutshell, we Taylor expand the effective Hamiltonian near the fixed point as

$\mathbf{R}_l(\tilde{H}^* + \varepsilon h) = \tilde{H}^* + \varepsilon \mathbf{L}_l h + O(\varepsilon)$, as $\varepsilon \rightarrow 0$. In other words, we linearize \mathbf{R}_l by \mathbf{L}_l , and from \mathbf{L}_l we can set up an eigenvalue equation from which we can obtain some eigenvalues which turn out to be directly related to the critical exponents. Further, from $f(\tilde{H}') = l^d f(\tilde{H})$ we can recover the scaling hypothesis.

⁸ To be more precise, in TD the density of a system is given as either $1/V$ or M/V , where M is the system's mass and V its volume and which is a continuous function, while in SM it is N/V , where N is the total number of parts and which is not a continuous function. When $N \rightarrow \infty$ and $V \rightarrow \infty$, N/V converges to a continuous function if it converges.

⁹ After the completion of this paper I received a copy of Bob Batterman's most recent paper (Batterman forthcoming), which explains in detail in what sense the RG explanation of universality is an explanation (or what kind of an explanation it is). It is quite clear to me -- although I cannot go into the details in this paper -- that the kind of explanation Batterman attributes to the RG explanation of universality is the same kind as I regard it in this paper, namely, it renders universality a non-accidental feature of bulk systems near criticalities. It does, in this case, take some mystery out of the macro-micro relations between kinds in special sciences and their microscopic realizations, but it does not entirely eliminate it. My example of the planets is still accurate in this regard. I leave a detailed discussion of this subject to another occasion.

¹⁰ There are many examples of idealizations in physics which assume non-actual but physically possible conditions. Complete isolation is one example, having all lattice sites of equal distance is another, and even such mechanisms as the famous Cooper-pairs which initially provided an explanation for superconductivity are of this kind.

¹¹ Note that there is a big difference between **where** the boundary is and **what** it is. For a single bulk phase, it doesn't matter where or what the boundary is; but for systems in multiple phases, what the boundary condition is could sometimes be crucial, but it doesn't matter where it is, since the long-range order is supposed to hold even if the boundary is infinitely far away.

¹² One should be careful in interpreting Lanford's result in connection with Boltzmann's probabilistic interpretation of H-theorem. What Lanford proves is an H-monotonicity in a short time for the mechanical evolution of the majority of systems satisfying the Boltzmann-Grad limit, while Boltzmann's probabilistic interpretation gives no monotonicity to any mechanical evolution but only high probabilities for those systems starting at non-equilibrium states to end up at the equilibrium state.