Philosophical Implications of Kadanoff's Work on the Renormalization Group

Robert W. Batterman

Journal of Statistical Physics
1

ISSN 0022-4715
Volume 167
Combined 3-4

J Stat Phys (2017) 167:559-574
DOI 10.1007/s10955-016-1659-9

J Stat Phys (2017) 167:559–574
DOI 10.1007/s10955-016-1659-9

Philosophical Implications of Kadanoff's Work on the Renormalization Group

Robert W. Batterman[1]

Received: 30 August 2016 / Accepted: 27 October 2016 / Published online: 7 November 2016

Abstract This paper investigates the consequences for our understanding of physical theories as a result of the development of the renormalization group. Kadanoff's assessment of these consequences is discussed. What he called the "extended singularity theorem" (that phase transitons only can occur in infinite systems) poses serious difficulties for philosophical interpretation of theories. Several responses are discussed. The resolution demands a philosophical rethinking of the role of mathematics in physical theorizing.

Keywords Kadanoff · Renormalization group · Block transformations · Ising model · Minimal models · Universality · Explanation

1 Introduction

I am honored to have been asked to write for this special issue dedicated to the work of Leo Kadanoff. The presence of this paper by a philosopher in such a special issue will no doubt be surprising to some. However, in my opinion, Leo, in addition to being a superb physicist, mathematician, and a wonderful individual, was also a brilliant *natural* philosopher. By this I mean that he was a deep thinker about the nature of reality and about the role mathematics can play to allow us better access to the physical world. In this regard, I think it quite reasonable to compare Leo's work to that of the great nineteenth century theorists like George Gabriel Stokes, George Green, James Clerk Maxwell, and Lord Kelvin, among others.

There are very few natural philosophers anymore. The fields of philosophy and science parted company at the end of that century. Philosophers more and more began to turn toward the disciplines of logic and the analysis of language, and their examination of the enterprise of

This article is dedicated to the memory of Leo Kadanoff.

✉ Robert W. Batterman
rbatterm@pitt.edu

1 Department of Philosophy, University of Pittsburgh, 1001 Cathedral of Learning, Pittsburgh, PA 15260, USA

science began to follow a different, less-engaged-with-scientific-detail, direction. They began to try to determine the logic and structure of scientific theorizing in a way that was much more arm-chair and much less concerned with details about individual theories. The aim was to construct or reconstruct the proper logical structures of scientific explanation, confirmation, and theory choice. The philosophical reconstructions were, by and large, designed to fit all empirical science. For example, an explanation in physics should share the same general (logical) form as explanations in biology, chemistry, or sociology. Scientists themselves, began to specialize more and more. And as a result, the kind of broad understanding of the physical world that Stokes, for example, possessed was distributed in a division of labor.

Leo Kadanoff may have been born in the wrong century. His interests were broad and wide ranging. And, his physical and philosophical intuitions were often prescient. Truesdell has said that "[t]he intuitive physicists are the scouts of natural philosophy." [24, p. 90] Leo was a remarkable scout and a remarkable philosopher.

My personal introduction to him was the result of his finding (on his own) and reading my book, *The Devil in the Details*. I think he was intrigued by the title, especially by the appearance of the word "asymptotic" in the descriptive subtitle. It prompted him to write me an email and ultimately to invite me to Chicago to meet with him. I have known a few physicists in my career and only a handful of them have had much time for philosophers of science. Leo was a special case. Though he might not use the jargon of philosophers of science, much of Leo's writing is deeply philosophical. He had an amazing ability to employ mathematics to study physical phenomena and to use it to better understand the nature of those phenomena.

I have learned a great deal from him and his work. I thank him for his generosity.

What follows is a discussion of the impact on philosophical theorizing provided by the development of the renormalization group (RG). In the next section I examine various possible reactions to what Kadanoff called the "extended singularity theorem." This is a statement to the effect that phase transitions can only occur in systems of infinite extent, with an infinite number of constituents. It is a mathematical fact that the function for the free energy of a finite system cannot display the kind of singularities and discontinuities many have associated with thermodynamic phase transitions. This fact has been the source of much consternation in the philosophy of physics community. In the following sections I argue that those worries are largely misplaced and that Kadanoff, himself, had seriously hinted at different kind of resolution, one I personally think is correct. It is a resolution that requires a rethinking of the role of mathematics in physical theorizing, in that certain features of the mathematics itself play explanatory roles. As such it provides an excellent modern example of the kind of natural philosophy characteristic of physics in the Victorian age.

2 Philosophical Perspectives on Theories

I think it is fair to say that from a philosophy of science point of view, physical theories are supposed to reflect our best attempts to understand nature. Philosophers are also enamored with the idea that theories have a certain logical structure—they can be written down in some kind of axiomatic form from which, given certain inputs, various features of physical systems (future states, e.g.) can be derived using logic and reasonably straightforward mathematics. Furthermore, philosophers often distinguish fundamental from nonfundamental (or "phenomenological") theories. This latter distinction presupposes the idea that fundamental theories are the ones that tell us *really* what nature is actually like at "bottom." These presumably include, quantum theory, quantum field theory, maybe a theory of quantum gravity, etc. Nonfundamental theories such as thermodynamics, continuum mechanics, and fluid dynamics, on the other hand, while pragmatically useful, are in a certain sense (exactly what sense is a matter of serious contention) superfluous. We could, *in principle*, solve problems involving the elastic bending of beams by starting from the fundamental atomic and subatomic theories of the constituents of the beam.

Nonfundamental theories don't get nature right. Steel beams are not *really* the continua whose bending behaviors are described by the Navier–Cauchy equations. Gases are not continuous blobs of stuff. The important theories, according to many philosophers and, I believe, according to many physicists, are those that get the ontology right. In part, the (often unarticulated) reason for preferring fundamental theories over phenomenological theories is a realist presupposition that physical theories must accurately describe the world the way the world really is. Phenomenological theories are often good for calculating, but they don't accurately describe the world and so must, in a sense, play second fiddle to their fundamental partners.

Of course, none of this is to deny that historically we often come to consider fundamental theories only after we have constructed and tested sophisticated phenomenological theories.[1] Thanks to work by such luminaries as Clausius, van der Waals, and Maxwell, we had quite well developed theories of matter in the nineteenth century. In particular, these theories were able to provide reasonably accurate accounts of observed behavior of matter in (and in transition between) various qualitatively distinct states or phases.

With the acceptance of work by Boltzmann and Maxwell on kinetic theory, and with data coming in about the existence of atoms—the (actual = real) discrete nature of the constituents of everyday matter— thermodynamics began to lose its status as a fundamental theory [6, 22]. Theorists continued to develop statistical techniques for describing and (hopefully) for explaining the successes of thermodynamics in terms of averages over behaviors of the atomic constituents of the systems described by thermodynamics. Thus, there was a shift to a more fundamental (statistical mechanical) understanding of the various phases of matter. Certain questions became pressing: Can one understand the existence of distinct phases of matter in terms of the statistical analysis of the behaviors of large collections of atoms and molecules? Can one understand the transitions between these distinct phases of matter in statistical mechanical terms?

Statistical mechanics tells us that if we know the kinds of particles involved (e.g., what are their internal degrees of freedom) then we should be able to write down the so-called partition function for the collection of particles that make up our system. Once we have the partition function, we can derive equations that are analogs of the continuum thermodynamic equations of our phenomenological theories.[2]

It is natural in statistical mechanical discussions of various real systems (real fluids, real magnets) to employ simple or "minimal" models.[3] The simplest model is the Ising model for a region of spins, Ω, on a d-dimensional lattice. There are $N(\Omega)$ lattice sites/spins and since the spins σ take values $\sigma_i = \pm 1$, there are a total of $2^{N(\Omega)}$ possible states. The Hamiltonian

[1] In fact, we most often don't recognize the theories as phenomenological until long after they become well-established.

[2] Much of interest, philosophically, concerns the nature of these analogical derivations. Gibbs, for one, was very cautious about claiming to have found identities that would allow one to claim that the thermodynamic relations have been reduced to relations of statistical mechanics. See [4] for a discussion.

[3] I will have more to say about the justification of the use of such models below. For the term "minimal model" and discussions concerning their use see [5, 11, 12].

for this system can be written as

$$\mathscr{H}_\Omega = -J \sum_{<ij>} \sigma_i \sigma_j - h \sum_{i \in \Omega} \sigma_i, \tag{1}$$

where $< ij >$ signals nearest neighbor pairs on the lattice, J characterizes the spin-spin coupling, and h is a (uniform) magnetic field.

One can define the free energy for this region of spins as follows:

$$F_\Omega(T, h, J) = -k_B T \log \sum_{n=1}^{2^{N(\Omega)}} e^{-\beta \mathscr{H}_\Omega}, \tag{2}$$

with $\beta = 1/k_B T$.

Given the Hamiltonian (1) one can write down the partition function

$$Z_\Omega = \sum_{n=1}^{2^{N(\Omega)}} e^{-\beta E_n[\mathbf{K}]}, \tag{3}$$

where E_n is the energy of the nth state and is a linear combination of the coupling constants $[\mathbf{K}]$ (related to J).

The partition function is a finite sum of $2^{N(\Omega)}$ terms each of which is an exponential and is positive. Hence the sums in both the free energy F_Ω and the partition function Z_Ω are also analytic functions. This mathematical fact is at the center of what Kadanoff calls the "extended singularity theorem." [17, pp. 154–156.] The interpretation of this "theorem" has been the subject of much philosophical controversy. In what follows, I intend to discuss some of this controversy and some further implications of Kadanoff's work on understanding of the consequences of this mathematical fact for the interpretation of theories of phases of matter.

2.1 Idealism?

In phenomenological thermodynamics, phase transitions are identified with singularities in the derivatives of the free energy. They are places where those derivatives exhibit nonanalytic behavior. If one accepts the idea that phase transitions are to be identified with such singularities, and if one accepts that something like Eq. (2) "corresponds to" or is "analogous to" the free energy of thermodynamics, then we have a fundamental problem: Since the free energy cannot display nonanalytic behavior for finite N, there can be no phase transition according to the statistical mechanics of finite systems. In his text *Statistical Physics: Statics, Dynamics and Renormalization* Kadanoff puts the point most forcefully:

> The existence of a phase transition requires an infinite system. No phase transitions occur in systems with a finite number of degrees of freedom. [15, p. 238]

An *infinite* sum of analytic functions *can* yield a nonanalytic function. To understand the existence and the nature of distinct phases and the transitions between them, therefore requires that we consider infinite systems.

Kadanoff notes that

> [m]atter exists in different thermodynamic "phases", which are different states of aggregation with qualitatively different properties. These phases provoked studies that are instructive to the history of science. The phases themselves are interesting to modern

> physics, and are provocative to modern philosophy. For example, the philosopher might wish to note that strictly speaking, no phase transition can ever occur in a finite system. .... [16, p. 778]

A realist philosopher will be dismayed by this conclusion. After all we see systems undergo phase transitions (water in a tea kettle boils) as the temperature changes. And, we correctly believe that the number of water molecules in the kettle is finite. How can matter both "exist in different thermodynamic 'phases' " as Kadanoff says, and yet it be the case that transitions between the different phases cannot occur in actual(=real) *finite* systems?

There are several responses one can make in the face of this conundrum. Kadanoff, at least in one publication, opted for an interpretation that philosophers might call "idealistic."[4] He expresses this in the very next sentence from the passage quoted above: "Thus in some sense, phase transitions are not exactly embedded in the finite world but, rather, are productions of the human imagination."

Many would find this conclusion to be a hard pill to swallow.[5] [16, p. 778] To claim that a full understanding of phase transitions and the qualitatively different states of matter that we experience on a daily basis, requires appeal to the human imagination goes against many well-entrenched (realist) beliefs.

2.2 "Real" Phase Transitions Aren't Sharp?

Many philosophers have taken a different position. They challenge the idea that the proper understanding of a phase transition requires appeal to any mathematical singularities or non-analytic behavior whatsoever.[6] These philosophers reject the idea that to properly characterize the world—a world that genuinely exhibits phase transitions—requires that one make reference to infinities or singularities. On their view, mathematical theories that require (either implicitly or explicitly) the use of infinite limits are simply convenient computational tools.[7] Such theories are short hand for detailed mathematical derivations involving "true" partition functions with *finitely many* summands. This fits quite well with the view that phenomenological, nonfundamental theories are in some sense dispensable. They don't get the ontology right in that they (apparently) assert that real systems like water in a tea kettle contain an infinite number of molecules. Fundamental theories, again, are the ones that describe the world accurately, the way it really is. Thus, from this point of view, a proper understanding of a real phase transition comes from examining the behavior of a system with *finitely* many (10^{23}) molecules. And, of course, it follows from this that thermodynamics does not provide a proper account of real phase transitions. This is just another indication that thermodynamics is nonfundamental.

---

4 Roughly, this involves a claim to the effect that what there is in the world, or perhaps what we can know about the world, is a product or function of human thought.

5 Actually, I'm not completely sure this was his view. In private conversation, the first time I met him, Leo told me he was a Platonist. This reflects a kind of idealism, but not one that particularly fits well with a simple reading of this sentence. And, as we will see below, he later argued for what seems to me, a very different understanding of the consequences of the extended singularity theorem.

6 See [9] for an argument that defining phase transitions in terms of nonanalyticities is evidence that we are "taking thermodynamics too seriously." See [2] for a response. See also [10].

7 A prominent proponent of this viewpoint is Jeremy Butterfield [7,8] (see also, [21]) . On his view, the use of infinite limits receives a "straightforward justification":

> The use of the infinite limit ...is justified, despite N being actually finite, by its being mathematically convenient and empirically correct (up to the required accuracy). [7, p. 1077]

I believe that there is another alternative. It is a point of view that takes the asymptotic mathematics apparently required by the extended singularity theorem seriously without at the same time committing one to believing the false claim that the contents of our tea kettles contain an infinite number of water molecules. I think that this position fits nicely with the general argument Kadanoff presents in his contribution to the *The Oxford Handbook of Philosophy of Physics* and it represents a rather radical departure from the philosophers' usual way of thinking about physical theories. Kadanoff goes so far as to suggest that the modern theory of critical phenomena represents a *new* theory. Furthermore, he asserts that, in the sense of Thomas Kuhn, the development of this theory of critical phenomena provides an example of a scientific revolution [19]. In the next section, I explore rather superficially how this new theory arose. In Sect. 4 I discuss just how different the kind of coarse-graining or upscaling provided by the new (RG) theory is in comparison to the kind of ensemble averaging characteristic of orthodox statistical mechanics and mean field theory.

3 The "New" Theory of the Critical Region

The history of theories of critical phenomena provides an interesting example of the kind of conservatism with respect to accepted theory that Kuhn called "normal science." Physicists, starting really with van der Waals, employed what is now called "mean field theory" in the attempt to describe phase diagrams exhibiting behavior of the type shown in Fig. 1 [17, pp. 147–149]. Van der Waals' equation of state corrected the ideal gas law by including parameters relating to average effects of hypothesized molecules. As Kadanoff points out, the van der Waals' equation of state fails to predict the boiling phase transition [17, pp. 149–150]. Maxwell corrected this and was able to construct the qualitative portrait displayed in Fig. 1 with the "flat" isotherms in the boiling region. We can also plot this boiling transition as a coexistence curve displaying the density values for the vapor and liquid phases in the kettle as a function of temperature. This is represented in Fig. 2.

One can define an order parameter to characterize the behavior in this region as the difference between the liquid and vapor densities:

$$|\rho_{vap} - \rho_{liq}| \propto |t|^{\beta}, \tag{4}$$

where

$$t = \frac{T - T_c}{T_c}.$$

Mean field theory predicts that β takes on the value 1/2 but experiments on many different fluids yield values closer to .35. It is a remarkable fact that different fluids with different molecular structures exhibit the same scaling behavior in the neighborhood of their respective critical points. A famous figure from E. A. Guggenheim's 1945 paper entitled "The Principle of Corresponding States" illustrates this *universal* behavior. When plotted in reduced coordinates, the coexistence curves for eight different fluids all collapse onto the same curve. See Fig. 3.

Mean field theory was able to predict the existence of phase transitions and in particular the existence of power law scaling behavior near criticality. But, as noted, it failed to match experimental evidence. It worked very well in regions of the phase diagram (Fig. 1) that were far from the critical region, but misrepresented the behavior of systems near criticality. However, mean field theory did (theoretically) lead to the expectation that different systems

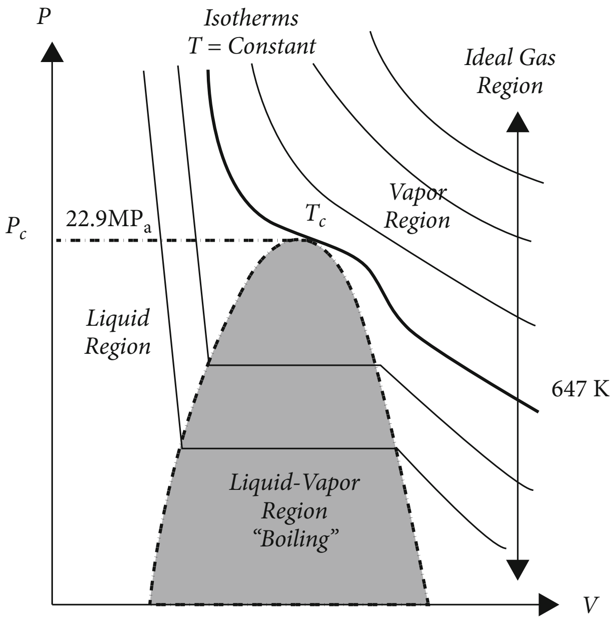


Fig. 1 Cartoon PVT diagram for water [17, p. 148]

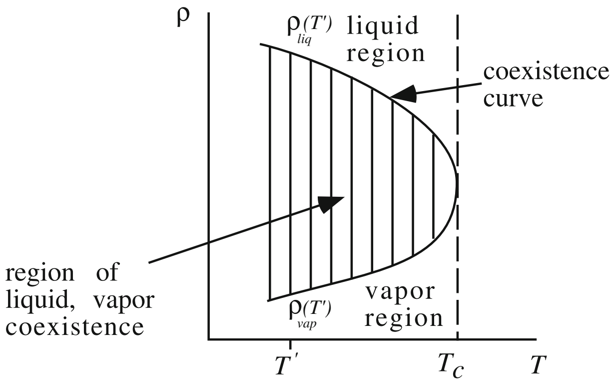


Fig. 2 Cartoon coexistence curve

near criticality should display similar (scaling) behavior. Despite its misrepresentation of near critical behavior, theorists to some extent carried on using the mean field approach.[8]

Interestingly though, as investigators began to focus on (non-mean field theory) behaviors near criticality, they noticed similarities in the between different systems.[9] That a ferromagnet

[8] This is a hallmark of Kuhn's characterization of "normal science."

[9] Kadanoff notes that similarities between different systems were well known to early investigators [14, p. 103]. The idea that the "details of the system undergoing the phase transition might be irrelevant and that many different phase transitions problems might be essentially identical" was already noted in Landau and Lifshitz in the context of mean field theory. I believe that this mean field understanding of universality also contributed to that theory's entrenchment in the scientific community.

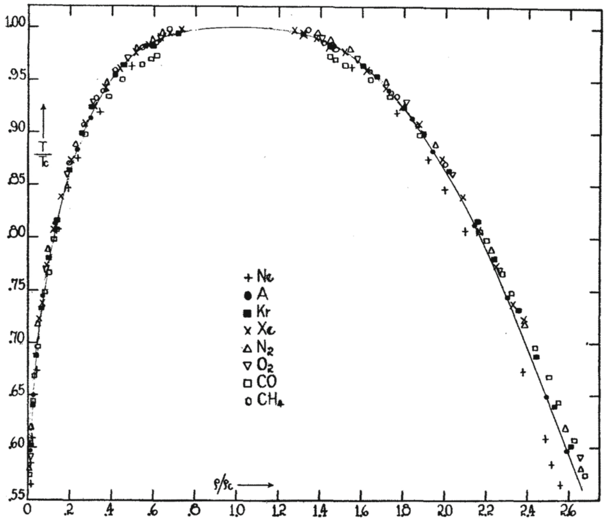


Fig. 3 Universality of critical phenomena [13]

with order parameter defined as the net magnetization displays the same behavior in the critical region as does a liquid/gas system with the order parameter given in Eq. (4) was a remarkable fact. Kadanoff stated the *hypothesis of universality* in the following way:

> All phase transition problems can be divided into a small number of different classes depending upon the dimensionality of the system and the symmetries of the order state. Within each class, all phase transitions have identical behaviour in the critical region, only the name of the variables are changed. [14, p. 103]

Given the rather large discrepancy between the mean field theory predictions of behavior near the critical point and experiment, many experimentalists tried for more accurate assessments of the values of the critical exponents or critical indices, e.g. β. Kadanoff asserts that this work (sometimes subject to mild ridicule) was well worth the effort:

> During the many years in which critical behavior has been a subject of scientific study, many human-years of scientific effort has been devoted to the accurate determination of these indicies. Sometimes scientists complained that this effort was misplaced. After all, there is little insight to be obtained from the statement that β (the index that describes the jump in the liquid–gas phase transition) has the value of 0.31 versus 0.35, or 0.125 or 0.5. But these various values can be obtained from theories that give a direct calculation of critical quantities or related them to one another. Finding the index-values then gave an opportunity to check the theory and see whether the underlying ideas were sound. Thus, the small industry of evaluating critical indices supports the basic effort devoted to understanding critical phenomena. [17, p. 160]

Most importantly there was a shift of emphasis to study the region of the phase diagrams in the neighborhood of the critical points. The other regions (reasonably well understood in mean field theoretic terms) were put to the side. This led to an intense focus on universality and scaling. It also led to what Kadanoff takes to be a *new specialized* theory "intended to apply mostly to the region near the critical point." [17, p. 161]

The new theory necessarily must part from the orthodox statistical mechanics that is employed in mean field approaches. The reason, as we have already noted, has to do with the fact expressed by the extended singularity theorem. In order to describe the behavior near the critical point one needs an infinite system.[10] Statistical mechanical averages ignore fluctuations and near critical points fluctuations dominate. Kadanoff describes the chaos at an (in)famous 1937 meeting celebrating van der Waals' 100th birthday. At that meeting the participants debated whether a (finite) partition function could or could not explain sharp phase transitions. In effect they wondered about the applicability of statistical mechanics. With hindsight Kadanoff argues that the participants were right

> to be disquieted by the applicability of statistical mechanics. But they focused upon the wrong part of the phase diagram. The liquid region is described correctly by statistical mechanics. But this theory does not work well in the two-phase, "boiling", region of Fig. [1]. Here the fluctuations entirely dominate and the system sloshes between the two phases. The behavior of the interface that separates the phases is determined by delicate effects of dynamics and previous history, and by hydrodynamic effects including gravity, surface tension, and the behavior of droplets. Hence, the direct application of statistical mechanics is fraught with difficulty precisely in the midst of the phase transition. Thus the extended singularity theorem suggests that a new theory is required to treat all the fluctuations appearing near singularities. [17, pp. 163–164]

Mean field theory fails in the critical region because it cannot determine the effect of (spatial) fluctuations at all length scales on the average behavior of the relevant order parameter. Somehow this hard mathematical problem needed to be tamed. At this point, I am going to stop following Kadanoff's historically oriented discussion leading to the development of the block scaling techniques that allowed for the construction of effective Hamiltonians, etc. The ideas are well-known and Kadanoff's contributions are crucial for the final development of the RG transformation.

I want now to focus on several important ideas that support Kadanoff's claim that the development of the RG really did provide a new theory of critical phenomena and potentially a new reason for philosophers to rethink their overly simplistic understanding of the role of mathematics in the formulation or construction of physical theories.

4 The RG Revolution

Reconsider the view that statistical mechanics is a more fundamental theory than thermodynamics. Since it (together with the quantum theory) correctly characterizes the finitely many constituents of fluids and gases, one should expect it to be able to explain the behavior of such systems throughout the various regions of the phase diagrams. Statistical mechanics is inherently a means for averaging. And the averaging techniques that it engenders allow one to "upscale", "coarse-grain", or to find statistical correlates of thermodynamic functions only in certain regimes. A different kind of coarse-graining or upscaling is needed for a theory of critical phenomena. And it is a kind of upscaling that requires a different set of mathematical tools than are present in orthodox Gibbsian statistical mechanics.[11] As we've seen the

[10] One needs this for first order transitions as well, but the singularities are weaker.

[11] It was in part Gibbs' recognition that his ensembles failed to provide an account of phase transitions that he exercised extreme caution in claiming to have identified statistical mechanical analogs of thermodynamic properties like temperature and entropy. See [4] for a discussion of Gibbs' caution and his awareness that

orthodox theory considers the partition function for the actual finite system. The new theory needs to employ mathematical asymptotics and, as the extended singularity theorem asserts, must work with infinite systems.[12]

4.1 Universality as Explanans

We have seen that one aspect of the theoretical focus on the critical regions was a recognition of the importance of universality. Similar scaling behavior by different systems is an empirical fact about the world. And, in much of Kadanoff's work, and the work of others, it was something to be appealed to in various explanatory contexts. I will describe what I mean by this in a moment. Ultimately, however, I aim to consider the following question:

– *From a theoretical point of view*, how is universal behavior possible and how can it be explained?

This takes the existence of universality to be the *explanandum* (or thing to be explained) rather than the *explanans* (or thing(s) doing the explaining). Kadanoff definitely had ideas about what, physically, was responsible for universality and so, he had a good idea of how one might provide the explanation.[13] But I think the asymptotic techniques employed in the Wilson refinement of the block transformation arguments provide the key to the explanation.

Before addressing the question of how universality can be explained, let me provide an example of how universality has been appealed to in order to explain other things. Kadanoff shows how "[u]niversality describes the relationship among different phase transitions problems." [14, p. 105] To this end, imagine that another field is inserted into the equation for the free energy for a ferromagnet. Call that field λ and let U be the operator that is its thermodynamic conjugate. In other words, we redefine the problem by adding λ and U to the free energy:

$$F = F(\epsilon, h, \lambda),$$

with differential

$$dF =< M > dh+ < H > d\epsilon+ < U > d\lambda. \tag{5}$$

Footnote 11 continued
phase transitions would pose theoretical problems for the statistical mechanical theory he developed. See [22, Chap. 3] for a discussion of Gibbs' thermodynamic analogies.

12 As an interesting aside, Clifford Truesdell, in a lecture entitled "The Ergodic Problem in Classical Statistical Mechanics," discusses Khinchin's use of asymptotics to try to justify the identification of infinite time averages of certain functions with phase (or ensemble) averages. The last paragraph reads as follows:

> I should like to be able to say that statistics is unnecessary, that the name "statistical mechanics" is a misnomer for "asymptotic mechanics", as far as equilibrium is concerned. This is almost true, but not quite so. [23, p. 82]

On my understanding, Truesdell is stressing the importance of appealing $N \rightarrow \infty$ limits in justifying certain claims about finite systems whose observable properties are represented by sum-functions. Actually, I believe it is possible to tell an RG story to justify the use of the microcanonical probability distribution for calculating equilibrium values for functions of this form. See [4] and [18, pp. 139–142] for details.

13 In his 1970 lecture [14] Kadanoff says the following:

> The physical origin of this hypothesis [of universality] is the observation that critical phenomena are dominated by fluctuations in M [the order parameter] and H [the energy] which appear in a special scale which is very long compared to the force range. Then these fluctuations cannot probe all the details of the interatomic potential. Rather they only see certain gross features of the potential .... [14, p. 104]

One then considers a continuous variation from $\lambda = 0$ to $\lambda = 1$ to "…represent the change in the Hamiltonian which takes us from the Ising model to the Heisenberg model, or from Ni to Fe or from a nearest neighbor interaction to a next nearest neighbor interaction." [14, p. 105] In effect, this variation "morphs" the atomic/lower scale details of one system into those of another. Universality, then

> …*implies* that the basic thermodynamic functions and correlation functions only depend on λ via a trivial change of variables. The functional form is the same as at $\lambda = 0$. However, the variables in these functions are changed in that h, ϵ, and M are each multiplied by parameters a, b, c, d which depend upon λ. [14, p. 105, Emphasis added.]

Let

$$\lambda = \frac{J_{n.n.n.}}{J_{n.n}},$$

and consider the variation from nearest-neighbor to next-nearest-neighbor coupling. Let ϵ and h be defined as follows:

$$\epsilon = \frac{(T - T_c(\lambda))}{T_c(\lambda)} \quad \text{and} \quad h = \frac{\mu_\beta H_z}{KT_c}.$$

For $\lambda = 0$ write the order parameter as

$$< M >= m_0(h, \epsilon)$$

and the spin-spin correlation function as

$$< \sigma_z(0)\sigma_z(r) >= g_0(h, \epsilon, r).$$

Then Kadanoff [14, p. 106] asserts that "[u]niversality implies for $\lambda \neq 0$":

$$< M >= am_0(\bar{h}, \bar{\epsilon}), \quad < \sigma_z(0)\sigma_z(r) >= (ab/d^3)g_0(\bar{h}, \bar{\epsilon}, \bar{r}),$$

where

$$\bar{h} = bh, \quad \bar{\epsilon} = c\epsilon, \quad \bar{r} = dr.$$

This means that the functional forms of $< M >$ and $< \sigma_z(0)\sigma_z(r) >$ do not change as λ varies. The point here is that *universality is being assumed* and that it is being *appealed to*, to explain why "the parameters should be materially independent." [14, p. 107]

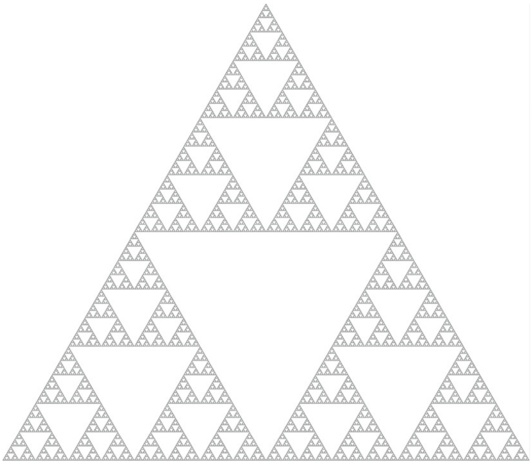
The next section of [14] is entitled "Scaling derived from universality." Here again the empirically recognized fact of universality plays the role of a premise. It is used in arguments aimed to derive theoretical descriptions of observed scaling relations and so is used to explain the existence of scaling.

4.2 Universality as Explanandum

However, what about the *legitimate* question, highlighted above, concerning the very existence of universality. *From a theoretical point of view*, why should we expect the data collapse displayed in Fig. 3? This question has a different form than, say, the question of why the coexistence curve for Xe has the form that it does—a form that fits a scaling law relation

$$|\rho_{vap}(\text{Xe}) - \rho_{liq}(\text{Xe})| = k_{\text{Xe}}|t|^\beta.$$

Fig. 4 Sierpinski triangle



Presumably an answer to this question will very much depend upon the details of the intermolecular potential for Xe. Similarly, an explanation for why methane's order parameter exhibits the behavior

$$|\rho_{vap}(\mathrm{CH_4}) - \rho_{liq}(\mathrm{CH_4})| = k_{\mathrm{CH_4}}|t|^{\beta}$$

will be some detailed derivation from the detailed quantum Hamiltonian for methane. Neither of these individual explanations provide an answer to the question about the general/universal pattern displayed across a host of systems. There are two kinds of "why-questions" that one might ask about a pattern of behavior.

- Type I: One might ask why a particular instance of the pattern was realized.
- Type II: One might ask why the pattern itself exists across its different realizations. [1, Chap. 3].

By way of making the difference between these questions clear, consider the outcome of what some have called the "chaos game." Here one marks three vertices of a triangle A, B, and C on a plane. Choose a point somewhere on the plane and roll a three sided die labled a, b, and c. Go half way to the corresponding vertex and put a point. That's the new starting place. Iterate and one gets a Sierpinski triangle (Fig. 4). Why did the particular instance of the pattern appear? Answer: We started at point p rolled an a, then moved half way toward a, rolled a c, and so on. That provides a complete explanation of the appearance of the pattern known as the Sierpinski triangle. Why, in general, do we get patterns of this type (with this particular fractal dimension, etc.)? The answer provided to the first question is essentially irrelevant for our response to the second question. Answering the second requires an appeal to the strong law of large numbers. In fact, we explain why we should expect this pattern by showing it to have probability one in the space of sequences of As, Bs, and Cs generated by our die rolls. It also follows that we can lop off any finite initial sequence of As, Bs, and Cs from any sequence of rolls without changing the the fact that we can expect, with measure one, that the triangle pattern will emerge. (The details of arbitrarily long finite sequences are irrelevant for the outcome. One can "morph" one sequence into another without affecting the emergent pattern.)

In asking why we should expect universal critical behavior across a wide range of molecularly distinct systems, we are asking a question of this second type. And the answer is going to be similar in some respects to the appeal to the strong law of large numbers. We will need to appeal to asymptotics. We can explain why we should expect such behavior, if we can demonstrate that the lower scale details that genuinely do distinguish the systems from one another (those molecular details, for example, that are responsible for the system's having the critical temperature and pressure it actually has), are *irrelevant* for the behavior expressed by the scaling law for the order parameter. If we can tell a story about how these lower scale molecular details are irrelevant for the upper scale observed scaling behavior, then we will have an explanation for the universality displayed in Guggenheim's Fig. 3.

4.3 RG: New Problems and New Solutions

An important consequence of the focus on the critical region was a better understanding of the concept of universality as described above in Sect. 3. Kadanoff understood the idea that repeated blocking operations would enable one, in effect, to eliminate irrelevant details about the atomic potentials (irrelevant, that is, for describing behavior in the critical region). He saw the procedure as a means for constructing a sequence of effective Hamiltonians each of which (because of universality) could be assumed to have the same structural form as the previous one. Furthermore, each of new (rescaled) parameters in the Hamiltonian would be proportional to their old values near criticality.

However, he did not recognize that this iterative behavior could be understood in terms of dynamical systems theory. The idea of an iterated flow on an abstract (dynamical) space to a possible fixed point provides the means to answer questions that statistical mechanics with its averaging procedures cannot. Crucial, of course, to finding a fixed point, is the fact that at criticality the correlation length diverges.[14] Unless one has correlations of infinite extent, the iterative blocking procedure will get hung up at a given length scale and one will not find the fixed point.

Most important is the fact that while the dynamical mathematical analysis necessary to reach a fixed point requires infinities and divergences, the topological nature of the RG flow in the neighborhood of the fixed point can tell us much about the behavior of *near* critical systems. That is, the RG is not just a theory of the critical point, but rather it is a *theory of the critical region*. And, this covers large but finite systems. So, contrary to the line of reasoning presented in Sect. 2.2 the explanation of the behavior of real finite systems requires the use of mathematical infinities, but it does not require there to *be* infinite real systems.[15]

Theories are supposed to provide descriptions and explanations. They enable us to solve certain kinds of problems presented to us by the world. Statistical mechanical averaging or mean field theory failed to solve the problem of characterizing the critical region in part because statistical mechanics consider problems to have a certain form. Kadanoff puts this particularly nicely:

---

[14] It is also important to recognize that at the critical point the fluid exhibits fractal-like, self-similarity. It is the recognition of this fact that allowed Kadanoff to introduce the blocking operations and Wilson to think about finding fixed points of the RG transformation.

[15] A referee objects to this way of putting things because according to the point of view in Sect. 2.2 the use of infinities is merely a matter of mathematical convenience and so, the claim I'm making here is completely in line with the claims of those supporting the idea that real phase transitions aren't sharp. In response, it seems to me that if one is going to hold that the use of the infinite limits is a convenience, then one should be able to say how (even if inconveniently) one might go about finding a fixed point of *the RG transformation* without infinite iterations. I have not seen any sketch of how this is to be done. The point is that the fixed point, as just noted, determines the behavior of the flow in its neighborhood. If we want to explain the universal behavior of finite but large systems using the RG, then we need to find a fixed point and, to my knowledge, this requires an infinite system.

> Previous work in statistical physics had emphasized finding the properties of *problems* defined by statistical sums, each sum being based upon a probability distribution defined by particular values of coupling constants like K and h. Such sums would be called *solutions* to the problems in question. In the renormalization group work the emphasis is on connecting problems by saying that different problems could have identical solutions. The method involved finding different values of couplings that would then give identical free energies and other properties. These set of couplings would then form a representation of a universality class. All the interactions that flow into a given fixed point in the course of an infinite number of renormalizations belong to the universality class of that fixed point. [17, p. 179]

The mathematical asymptotics that characterizes the RG allows one to solve different problems. Among other things, it allows one to understand and to some extent quantify the existence and nature of universal patterns realized by radically different systems.

4.4 Justifying Minimal Models

There is another aspect to acknowledging the existence of different kinds of problems that has, to my mind, interesting philosophical consequences. In Sect. 2 I described the Ising model using the term, borrowed from Goldenfeld, of "minimal model." [11, p. 33] Philosophers have long tried to understand the epistemological role of the use of models and idealizations in describing and explaining the behavior of relatively complex physical systems. [3,20] One particularly import question concerns the justification for the use of models (minimal models) that are not (very) representative of the systems they are used to understand. The Ising model is a case in point. A set of discrete spins on a lattice really does not look at all like the liquids in our tea kettles or other vessels. How is it that we can, nevertheless, use the Ising model and others to explain and understand the behavior of real systems in the world?

The RG explanation of the existence of universality provides an interesting answer to this question. [5] The process of renormalization aims to find a fixed point of the renormalization group transformation. If it does, then all (critical) Hamiltonians that flow to that fixed point are in the same universality class. That is to say, the universality class is the set of systems with Hamiltonians in the basin of attraction of the fixed point. By investigating the nature of the RG flow in the neighborhood of that point one can describe the shared, universal macroscopic behavior for all systems in that class. If an Ising Hamiltonian is a member of that class, then we have a justification for using that model to investigate the critical and near critical behaviors of real systems that are also members of that class. Despite the wild idealizations and over simplifications characteristic of the Ising model (and other lattice models), the investigation of their behavior will be guaranteed to tell us about the macroscopic behaviors of actual gases, fluids, and magnets. Thus, from an epistemic point of view, the RG theory answers pressing philosophical questions about the use of idealizations and models in new and interesting ways.

5 Conclusion

This paper has aimed to examine the consequences of an RG account of critical phenomena and universal behavior for our philosophical understanding of the nature of physical theories. One of Kadanoff's main conclusions is that the RG constitutes a different kind of theory than orthodox statistical mechanics. It is not simply a new mathematical tool that gets attached to the orthodox theory. Instead, it represents a new, and quite revolutionary, development. New kinds of questions can be formulated and answered using the mathematics of the RG. The content of the theory is informed by the mathematics that it employs.

This represents a rather dramatic departure from philosophical orthodoxy concerning the nature of physical theories. That orthodoxy locates the content of a theory in the axiomatic structure and, typically at least, assigns the use of infinite mathematics a more pragmatically justified role. We employ infinities and continua for computational ease. The theories that use such mathematics are not fundamental.

Kadanoff's understanding of the new RG theory of critical phenomena reflects a different conception of the role of asymptotics and infinities. The kind of upscaling that leads to an understanding of the universal macroscopic behavior of micro-diverse systems is different than the upscaling provided in the ensemble averaging of mean field theory. He describes this new kind of upscaling in the following poetic passage:

> …the extended singularity theorem suggests that phase transitions are triggered by a very elegant mathematical juxtaposition put before us by Nature. On the one hand, the phase transition is connected with a symmetry operation built into the microscopic couplings of the system. For example, the ferromagnetic based upon the breaking of a symmetry in the possible directions of spins. On the other hand, the phase transitions also make use of the extended topology of [a] system that extends over an effectively infinite region of space. This coupling of microscopic with macroscopic has an unexpected and quite breathtaking beauty. [17, p. 183]

Here, Kadanoff displays the hallmark of the natural philosopher. This seems like a good place to end.

Acknowledgements Thanks to Michael Miller for helpful comments.

References

1. Batterman, R.W.: The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence. Oxford Studies in Philosophy of Science. Oxford University Press (2002)
2. Batterman, R.W.: Critical phenomena and breaking drops: Infinite idealizations in physics. Stud. Hist. Philos. Mod. Phys. **36**, 225–244 (2005)
3. Batterman, R.W.: Idealization and modeling. Synthese **169**, 427–446 (2009)
4. Batterman, R.W.: Reduction and renormalization. In: Ernst, G., Hüttemann, A. (eds.) Time, Chance, and Reduction: Philosophical Aspects of Statistical Mechanics, chap. 9, pp. 159–179. Cambridge University Press (2010)
5. Batterman, R.W., Rice, C.C.: Minimal model explanations. Philos. Sci. **81**(3), 349–376 (2014)
6. Brush, S.G.: Statistical Physics and the Atomic Theory of Matter, From Boyle and Newton to Landau and Onsager. Princeton Series in Physics. Princeton University Press, Princeton (1983)
7. Butterfield, J.: Less is different: emergence and reduction reconciled. Found. Phys. **41**(6), 1065–1135 (2011)
8. Butterfield, J., Bouatta, N.: Renormalization for philosophers. In: Bigaj, T., Wüthrich, C. (eds.) Metaphysics in Contemporary Physics, vol. 104, pp. 437–485. Poznan Studies in Philosophy of Science (2015)
9. Callender, C.: Taking thermodynamics too seriously. Stud. Hist. Philos. Mod. Phys. **32**(4), 533–539 (2001)
10. Fraser, J.D.: Spontaneous symmetry breaking in finite systems. Philos. Sci. **83**, 585–605 (2016)
11. Goldenfeld, N.: Lectures on Phase Transitions and the Renormalization Group. Number 85 in Frontiers in Physics. Addison-Wesley, Reading (1992)
12. Goldenfeld, N., Kadanoff, L.P.: Simple lessons from complexity. Science **284**, 87–89 (1999)
13. Guggenheim, E.A.: The principle of corresponding states. J. Chem. Phys. **13**(7), 253–261 (1945)

14. Kadanoff, L.P.: Critical behavior. Universality and scaling. In: Green, M.S. (ed.) Proceedings of the International School of Physics "Enrico Fermi" Course LI, volume Course LI, pp. 100–117, New York. Italian Physical Society, Academic Press (1971)
15. Kadanoff, L.P.: Statistical Physics: Statics, Dynamics, and Renormalization. World Scientific, Singapore (2000)
16. Kadanoff, L.P.: More is the same; phase transitions and mean field theories. J. Stat. Phys. **137**, 777–797 (2009)
17. Kadanoff, L.P.: Theories of matter: infinities and renormalization. In: Batterman, R.W. (ed.) The Oxford Handbook of Philosophy of Physics, chap. 4, pp. 141–188. Oxford University Press (2013)
18. Koralov, L., Sinai, Y.G. (2007) Theory of Probability and Random Processes, 2nd edn. Springer (2007)
19. Kuhn, T.S.: The Structure of Scientific Revolutions, 2nd edn. The Universtity of Chicago Press, Chicago (1970)
20. Morgan, M.S., Morrison, M. (eds.). Models as Mediators: Perspectives on Natural and Social Science. Cambridge University Press (1999)
21. Norton, J.D.: Approximation and idealization: why the difference matters. Philos. Sci. **79**(2), 207–232 (2012)
22. Sklar, L.: Physics and Chance: Philosophical Issues in the Foundations of Statstical Mechanics. Cambridge University Press, Cambridge (1993)
23. Truesdell, C.: The ergodic problem in classical statistical mechanics. In: Six Lectures on Modern Natural Philosophy, chap. 5, pp. 65–82. Springer, New York (1966)
24. Truesdell, C.: Method and taste in natural philosophy. In: Six Lectures on Modern Natural Philosophy, chap. 6, pp. 83–108. Springer (1966)