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There's plenty of room in the middle: The unsung revolution of the renormalization group

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The remarkable technical contributions of Michael E. Fisher to statistical physics and the development of the renormalization group are widely known and deeply influential. But less well known is his early and profound appreciation of the way in which renormalization group created a revolution in our understanding of how physics — in fact, all science — is practiced and the concomitant adjustment that needs to be made to our conception of the purpose and philosophy of science. In this chapter, I attempt to redress this imbalance, with examples from Fisher's writings and my own work. It is my hope that this tribute will help remove some of the confusion that surrounds the scientific usage of minimal models and renormalization group concepts, as well as their limitations, in the ongoing effort to understand emergence in complex systems.

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1. Introduction

In 1988, Michael E. Fisher authored what must surely be a candidate for his least cited paper. Entitled "Condensed Matter Physics: Does Quantum Mechanics Matter?"[1,2] and bestowed with all of nine citations (according to Google Scholar), it poses an outrageous question that in Fisher's hands is developed and answered with his characteristic brilliance, clarity and originality. Fisher was writing at the behest of Herman Feshbach to review the state of condensed matter physics as it stood on the 100th anniversary of the birth of Niels Bohr, with a special remit to comment on the overlap with Bohr's ideas on the fundamentals of quantum mechanics. The timing could not have been more propitious. The previous two years had seen the discovery of the high-temperature superconductors LBCO[3] and YBCO[4]; the discovery of quasicrystals had been made four years previously[5]; and the integer[6] and fractional quantum hall effects[7] had been discovered and largely explained during the previous eight years.[8] Five major discoveries in condensed matter physics in less than a decade, ultimately garnering five Nobel Prizes and each a splendid manifestation of quantum mechanics.

Nevertheless, Fisher evidently did not regard his role as to provide a self-congratulatory pat on the back to condensed matter physics. Instead, he took it upon himself to explain his thoughts on "... what condensed matter physics does, or should be doing, and what defines condensed matter physics, and thence to approach the question 'Does quantum mechanics matter?"' The viewpoint that he expounded was at the time still rather unorthodox in many areas of physics and science, in particular his perspective that "the question of connecting the models with fundamental principles is *not* a very relevant issue or central enterprise." This would come as news to those in the physics community engaged in the reductionist program, including to some extent condensed matter physicists but to a far greater extent those working in what at the time was sometimes called elementary particle physics or fundamental physics and what is now generally called high-energy physics, or in some areas, with less hubris and reflecting the progress in accelerator physics and budget, medium-energy physics. This dichotomy within physics was certainly pervasive at the time and important enough that Fisher chose to make it the focus of his article.

Fisher undoubtedly hoped to use the opportunity provided by Feshbach to pop the bubble of the reductionist approach to condensed matter physics, and this he did with gusto, and one assumes, with the twinkle in his eye and suave[9] cravat by which I will always remember him.

Over the last 35 years since Fisher's article, physics and its philosophical roots have undergone a significant shift. This shift influences the way we do physics, the way we interpret what we do, and the way we use physics to explore interdisciplinary scientific questions. Accordingly, I have chosen for my title a deliberately ambiguous and evocative phrase. "There is plenty of room in the middle" is of course a parody of Feynman's famous lecture "There is plenty of room at the bottom," which is often credited with foreseeing the nanotechnology revolution.[10] In my case, I am drawing attention away from the microscopic scale to focus instead on the middle scale or level of description, which depending on the problem might be the scale intermediate between the lattice spacing and the correlation length (in the case of critical phenomena) or the scale larger than the dissipation scale and the scale of energy input (in the case of fluid turbulence). In the first of these, one can make this scale arbitrarily large by tuning the temperature close to the critical temperature, and in the second, by making the Reynolds number large. This middle scale was where many of Fisher's interests lay, understanding the universal, cooperative phenomena that arise there. And of course that is why he chose his provocative theme for his article.

But I have a second motivation for my title. On more than one occasion, I remember Michael saying, "You can't have interdisciplinary research without first having disciplines" (for my personal comments, I will lapse into first name terms).

Thus, the “middle” in my title also refers to the space between disciplines, where interdisciplinary research happens and transdisciplinary research begins. I happen to believe that this is where the greatest scientific excitement can often be generated. Fisher himself was one of the shining examples of an interdisciplinary scientist, holding for many years at Cornell University the Horace White Professorship of Chemistry, Physics, and Mathematics, each of which he illuminated with his scholarship and unique contributions. Thus, I propose to use this chapter to pay tribute to Fisher’s deep understanding of the way in which his most important work on renormalization group theory led to a transformation in our understanding of physics and its explanatory purpose and to extend the scope of his examples and comments, especially with regard to non-equilibrium statistical physics. I will spend very little time on critical phenomena and exponents because I believe that although this was the problem that Fisher and the statistical mechanics community set out to solve, the impact of the renormalization group is equally important away from criticality. One of the ways that I will argue this is through the connections between universality, renormalization group, levels of description and asymptotics. I also want to argue that the perspective he espoused is especially relevant today to applications outside of physics, where there has arguably been less self-consciousness about the way we choose to construct theories.

2. Condensed Matter Physics: Does Quantum Mechanics Matter?

To appreciate Fisher’s subversive intentions, it is worth reminding the reader of the intellectual background to his article. It is no exaggeration to say that physics underwent a remarkable transformation sometime around the middle of the 20th century. During the previous decades, the reductionist program had been profoundly successful, with the discovery of the electron, neutron, proton; the theoretical predictions of the neutrino (by Pauli[11] in 1930) and the meson (by Yukawa[12] in 1934); the detection of the former by Cowan, Reines, Harrison, Kruse and McGuire[13] in 1956 and of the latter in 1936 by Neddermeyer and Anderson[14] — although they actually discovered what we now call the muon, a lepton whose mass was close to Yukawa’s prediction for the nuclear force carrier mass; the actual discovery in 1947 of Yukawa’s particle, the pion, in cosmic rays[15]; and accelerating with the discovery of a multitude of mesons during the 1950s and the subsequent decades. Yet, with the birth of solid-state physics around 1940 (reflected by the establishment of the American Physical Society Division of Solid State Physics (DSSP) in 1947), the initial focus on one-electron properties of metals and semiconductors turned in the 1950s to the collective properties of matter through many-body theory and the application of non-relativistic quantum field theory. In due course, the field became known as condensed matter physics, reflecting the increasing focus on all matter, not just solids, and especially on collective phenomena[16] (and marked by the American Physical Society renaming the Division of Solid State Physics as the Division of Condensed Matter Physics in 1978).

It quickly became clear that novel macroscopic phenomena transcended the additive properties of single degrees of freedom, and a renewed focus on collective properties was seen by many as the defining aspect of condensed matter physics, particularly through the field's visionary leadership by P.W. Anderson.[17,18] Indeed, Fisher is very clear where he stands on this point[1,2]: "The basic problem which underlies the subject is to understand the many, varied manifestations of ordinary matter in its condensed states and to elucidate the ways in which the properties of the 'units' affect the overall, many-variable systems." In other words, condensed matter physics is about interactions primarily.

For present purposes, I would argue that the pivotal developments in the field of condensed matter theory were two iconically quantum mechanical theories: Bogoliubov's 1947 theory of superfluidity for the weakly interacting Bose gas,[19] and the Bardeen–Cooper–Schrieffer 1957 theory of superconductivity.[20,21] I choose these problems because they are relevant to Fisher's conception of the task of condensed matter physics. Again, he is very explicit about the questions that animate the task of addressing the basic problem underlying condensed matter physics: "What are the states of matter?... What is their nature?... How do the various states transform into one another?" As we will see, inextricably linked with these questions are two concepts that are subtle and took a long time to be appreciated: the notions of "minimal models" and "levels of description". In fact, there are several features of superfluidity and superconductivity theory to which I especially want to draw attention.

2.1. *Minimal models*

First, their starting point. Both these works showed the surprising explanatory power of a ludicrously simple model of interacting bosons or electrons, respectively, that is clearly a brutal idealization of the real complexities of atomic or electron–phonon interactions. Bogoliubov assumed that the interactions between bosons were weak in the sense that "corresponds to a neglection of the finiteness of molecular radius, since we do not take into account the intensive increase of [the potential] for small r, which causes the impenetrability of molecules."[19] At the end of his paper, he argued that the approximation could also be extended to include the finite "molecular" radius by replacing the Fourier transform of the potential by the amplitude of the binary collision probability in the Born scattering approximation (an observation for which he thanks Landau). This amounts to replacing the real interaction with a contact interaction $U\delta(\mathbf{r})$. Bogoliubov's explicit recognition of the fact that his approximation was valid at long wavelengths where the atomic (molecular) size was far smaller than the mean particle separation was prescient, but it took several decades for this insight to be translated into a renormalization group description.[22–25] Bardeen, Cooper and Schrieffer (BCS) took the Bardeen–Pines Hamiltonian for electron–phonon interactions including Coulomb effects[26] and replaced the complex interaction with one in which "each state is connected to n other states by the same matrix element $-V$."[20] They explained that this idealization applied to a subset of states that were paired with equal and opposite spin and momentum and that this was sufficient to capture the formation of the condensed state for arbitrary weak matrix element/potential V.

The models that both Bogoliubov and BCS used in their work are examples of what are sometimes known as "minimal models,"[27–31] "model[s] that most economically caricature the essential physics."[28] However, what this actually means is rather subtle and sometimes misunderstood; in order to elaborate, we need to discuss the role of asymptotics, in particular the process by which the minimal models were analyzed.

2.2. *Asymptotics and universality*

One reason that both Bogoliubov and BCS were successful where others had failed was because they used non-perturbative methods to extract their physical conclusions. BCS used a variational method, while Bogoliubov invented a canonical transformation applied after the naïve perturbation theory in the weak potential had been performed. Shortly after the publication of the BCS letter announcing their results,[20] Bogoliubov's method was applied to the BCS model.[32,33] I emphasize the non-perturbative nature of the theories because in both cases, the results are non-analytic; for example, the depletion of the condensate in the Bose gas problem at zero temperature due to interactions is proportional to $U^{3/2}$, while in the BCS problem, the energy gap depends on the matrix element V and the density of states at the Fermi level $N(0)$ through an essential singularity of the form $\exp\left(-1/N(0)V\right)$. Such results are, of course, beyond the scope of simple, regular, finite order perturbation theory and, due to the way they are obtained, are believed to be valid asymptotically as leading-order approximations for $U, V \to 0$.

The necessity of using non-perturbative methods to obtain non-analytic results that describe the emergent states of superfluidity and superconductivity is not a result of any "exotic" character of these states. Phase boundaries are by definition the loci where the partition function is non-analytic, and the emergent states are physically not simple perturbations around the normal state, possessing generalized rigidity and elasticity as a result of the symmetry breaking associated with the transition.[18,28] The non-analytic nature of phase transitions means that any mathematical expansion about the transition point cannot be convergent, and so, our results are at best asymptotic in the minimal model interaction parameter. As a result, there will be sub-leading corrections to our predictions, in addition to other regular terms that may arise from the details ignored in constructing the minimal model in the first place. I also want to stress that in both the examples given here, the non-analytic results arise even at mean-field level, and proper treatment of the interaction of fluctuations adds another level of non-analyticity.

For example, in the case of the Bogoliubov Bose gas, the minimal model itself is constructed to be an asymptotic long-wavelength limit when the potential range is negligible compared to the mean separation of the particles. Corrections to this approximation arising from including a finite potential range or a non-vanishing particle density will decorate the leading-order asymptotic result in a complicated way that needs careful consideration of the various approximations. Nevertheless, the notion of a minimal model as being an economical caricature of the essential physics must be understood in this asymptotic context. The two cannot be separated. Thus, it is not simply that a minimal model is an inaccurate representation of a physical system, a poor approximation that can be improved by embellishment. Instead, the point is that the explanatory requirement of a minimal model is to account for the phases and phase transitions of matter, just as Fisher emphasized, and also the mathematical structure of the description of the latter, which necessarily involves asymptotic methods. Gratuitously realistic details of the starting model will not only complicate the technical task of extracting the asymptotic structure, but also will not impact the leading order behavior. Thus, a minimal model represents a universality class of models; whether or not one uses the minimal model or a decoration of it, the asymptotic outcome of a non-perturbative calculation will not be different.

Overall, we are led inexorably to the conclusion that there is a strong connection between asymptotics and universality. Indeed, in my own work, I have shown how renormalization group methods can be used to treat singular mathematical problems, such as those arising in differential equation theory[28,34] and low Reynolds number fluid dynamics.[35] In these problems, there are no fluctuations at all: The non-analyticity arises at mean field level.

2.3. *Minimal models in action: The case of superconductivity*

This narrative of the nature of minimal model explanation is not a subjective philosophical interpretation that is open to debate, although it has been challenged[36] and defended[37] in the philosophical literature.[29,31,38] It is what physicists actually do. In the case of BCS theory, we have a very important demonstration of the validity and utility of this perspective. The BCS minimal model of superconductivity leaves out a number of embellishments that are desirable to include if one wants to calculate accurately observables such as the transition temperature or energy gap at zero temperature for materials where details of the actual electron–phonon interactions are measurable. Such an elaboration of the BCS theory was initiated by Eliashberg,[39] who accounted for the detailed form of the electron–phonon frequency spectrum even in the strong coupling limit, using methods developed by Migdal for the normal state,[40] and by McMillan,[41] who provided a synthesis that included electron–electron interactions and band structure effects. In Eliashberg's theory, the effects of phonons lead to integral equations accounting for the phonon excitation spectrum and so the theory becomes intrinsically non-local. In fact, the interaction between electrons, mediated by phonons, is inherently a retarded interaction because of the mass difference between the lattice ions contributing to the phonon modes and the electrons themselves. Distortions of the crystal lattice, described by the phonons, inevitably relax on a much slower time scale than that of the electrons, and basically, it is the fact that electrons experience the local deformations that causes them to interact and be attracted to one another, in terms of their kinematics — hence the BCS coupling of electrons together in time-reversed momentum states. In McMillan's main result[41] (Eq. (18)), the structure of the BCS formula, with its essential singularity is modified to $\exp\left(-1.04(1+\lambda)/(\lambda-\mu^*(1+0.62\lambda))\right)$, where λ and μ^* are constants that are, respectively, calculable from the Eliashberg theory and the effects of Coulomb interactions.[42] For small values of the parameter λ (i.e., the weak coupling limit), the formula reduces to the BCS formula, with $\lambda - \mu^*$ being identified as $N(0)V$. Thus, we see explicitly how the minimal model can be embellished by including greater levels of realism in order to compare with experimental details, but staying within its universality class and mathematical structure.

2.4. *Levels of description and emergence*

Bogoliubov's work showed how an interacting system can, within a first approximation, be treated as a collection of non-interacting quasiparticles, each of which involves all the original "real" atoms in the system. The quasiparticles were recognized right at the outset as forming an explicitly long-wavelength description of the system, the details of which were later developed in 1957 by Lee and Yang, and others.[22–25,43–45] The many-body ground state and the excitations of the interacting Bose system depend on a single parameter U characterizing the strength of interactions of the original, "real" bosons; both the ground state and the excitations are collective properties of the system. In the case of superconductivity, the additional complication is the formation of composite bosons — Cooper pairs — which simultaneously undergo a similar ordering as in the Bogoliubov gas. The upshot of these developments is that for many purposes, a superfluid or superconductor has a universal description that effectively hides certain details of the underlying constituents and their interactions. In the case of the weakly interacting bosons, the interaction parameter U depends on the s-wave scattering length derived from the collision of two bosons, and the theory is valid when this length is much shorter than the mean separation of the bosons. Thus, it can be said that U relates two different *levels of description*; the microscopic world inhabited by the minimal model of interacting bosons and the macroscopic world of the condensate with its superfluid properties and excitation spectrum for the quasiparticles that have emerged. It would be possible in principle to infer the existence of the quasiparticles phenomenologically from thermodynamic measurements, as Landau famously did,[46] without knowledge of the properties, or even the existence, of the atomic level of description.

Although much of the impact of the Bogoliubov and especially the BCS theories derived from their ability to bridge levels of description and enable quantitative comparisons of the predictions of minimal models with experiment at the microscopic level of description (e.g., the work of McMillan[41] already mentioned), Fisher is emphatic that this is not their main importance. We already cited his views about connecting "the models with fundamental principles," and elsewhere in the same article, he writes, "The basic problem which underlies the subject is to understand the many, varied manifestations of ordinary matter in its condensed states... an important role is, and, indeed, should be played by various special 'models.' ...One might as a theorist argue that more attention should be paid to the connection between models and the fundamental principles as embodied in quantum mechanics."[1,2] However, he argues, this is misguided for the following reason: "I stress again, that in any science worth the name, the important point is to gain understanding. The language in which the understanding is best expressed cannot be dictated ahead of time, but must, rather, be determined by the subject as it develops. Accordingly, it really would not be a 'great success' to derive the Ising model from atomic theory or quantum mechanics. Indeed, for which physical system would one be 'deriving' it?"[1,2] His point is that a minimal model such as the Ising model is a minimal model for a ferromagnet, a liquid–gas system, an alloy, a ferroelectric material and, I would add, many other systems, including examples in biology and ecology, ranging from the neurobiology of retinal ganglion cells[47] to the annual nut distribution of pistachio trees.[48] Thus, although "...one does want to understand what the important variables actually are... in many ways, it is more important to understand how these variables interact together and what their 'cooperative' results will be."[1,2]

To close this important section on levels of description, I want to comment again, in Fisher's prescient words, of the important role played by asymptotics: "Personally, I find the elucidation of the connections between various disciplinary levels a topic of great interest. Typically, delicate matters of understanding appropriate asymptotic limits, both physical and mathematical, are entailed. The issues involved are often very subtle; nevertheless, one must admit, they seldom add significantly to one's understanding of either the 'fundamental' starting theory or of the target discipline. Sad but true!"[1,2]

To this, I would add a point that is not made frequently enough: technical derivations of higher levels of description from more microscopic ones have a limited regime of validity. For example, it is possible to derive the Navier–Stokes equations from Boltzmann's kinetic equation. This is a long and complicated calculation that I often show the physics students in my statistical mechanics classes. After I have done that, I show them a phenomenological motivation that of course is very brief and closer in spirit to the derivations usually given in fluid mechanics texts. Most students, when asked, find the former derivation more convincing. They are surprised to learn that I do not share this view and are somewhat shocked to realize that the seemingly more careful, technical derivation is only valid at very low density, whereas we know that the Navier–Stokes equations are an excellent description of fluid phenomena up to very high densities.

2.5. *Universal and asymptotic scaling laws in non-critical matter*

Fisher structured his article first by describing his perspective on the nature of condensed matter physics and second by giving multiple examples of forms of matter to illustrate his main thesis. The first form of condensed matter that he chose was polymeric matter, focusing first of all on the excluded volume problem for a single polymer,[49,50] and then the problem of the statistical thermodynamics of dilute polymer solutions.[27,51] Fisher chose this example and not the obvious example of critical phenomena scaling. Why?

I conjecture that he wanted to make two points. First, that there are other ways in which matter can enter asymptotic realms other than just being close to a critical point. Fisher was intimately acquainted with asymptotic methods and understood that an example of scaling which has nothing to do with a critical point would be consciousness-raising for some of his audience. This is elaborated on with numerous "critical phase" examples in Chapter 30 by Leo Radzihovsky. Second, that he wanted to show the relevance of condensed matter physics to a discipline other than physics. He writes,[1,2] "...other challenging problems remain beyond the size of a single isolated molecule. One might say, 'Well, aren't polymers for chemists?' The answer is that many of their properties have proved too difficult for traditionally trained chemists!" At this time, Fisher was in fact a Professor of Chemistry — among other things — and had in the early part of his career at King's College, London, worked extensively on the statistics of self-avoiding walks and Ising models, developing methods to enumerate configurations, extract exponents using series expansions, Padé approximants and so on. In an especially interesting paper from this period,[52] he and Gaunt estimated the behavior of self-avoiding walks and Ising models in dimensions $d > 4$, finding that the deviations from mean-field theory seemed to become very small above four dimensions, a result that must have been on his mind during the development with Wilson of the $\epsilon = 4 - d$ expansion.[53] One of the earliest applications of renormalization group theory, and in particular the ϵ-expansion, was to the conformations of polymers.

In 1965, Edwards had[50] developed a field-theoretic approach to the statistics of a self-avoiding polymer chain, based on Bogoliubov's idea of the pseudopotential,[19] and used a functional integral approach to derive a self-consistent theory that showed how in the limit of an asymptotically long chain, the radius of gyration of a single polymer scaled with length L as $L^{3/5}$. This theory, like Bogoliubov's, had a regime of validity associated with a description at long wavelengths compared to the range of interparticle forces, in this case being that L was much greater than the monomer scale. Edwards' result is, like Bogoliubov's, a mean-field theory approximation of course and ignores fluctuations which add still another level of non-analyticity to the problem. The much more complicated problem of semi-dilute polymer solutions was treated the following year by the same methods[54] subject to the same limitations of course.

In the early 1980s, Ohta and Oono[27,51] developed a conformation-space renormalization group method which was not based on de Gennes' analogy to the n-component magnet[55] and thus was able to account for arbitrary molecular weight distributions of the polymers. Ohta and Oono used their method to go far beyond computing simple scaling laws, not only correcting the Edwards mean-field results to include fluctuations but also calculating the universal scaling functions for the statistical thermodynamics of polymer solutions as a function of concentration. Fisher could not hide his excitement in reporting their results, especially the complete comparison with experiment[56] over several decades for the functional form of the osmotic compressibility and other variables. In particular, Fisher enthused: "Thus we see in polymeric matter new, subtle and universal behavior which we have succeeded in understanding theoretically. But quantum mechanics has had essentially nothing to say about the problem! Indeed, one feels that if some of the giants of the past, like Boltzmann or Gibbs or Rayleigh, were able to rejoin us today, they would be able to engage in research at the cutting edges of condensed matter physics without taking time off to study quantum mechanics first!"[1,2]

3. Turbulence: Does Fluid Mechanics Matter?

I want to turn now to a topic that did not play a large role in Fisher's article or his research interests: systems far from equilibrium. At the time of his article, condensed matter physicists were beginning to address problems in non-equilibrium pattern formation, phase transition kinetics, and there is a long story to tell there about asymptotic scaling laws, since this is the field where I started my own career. That story is for a future occasion. Here, I want to mention briefly two vignettes from my own research on turbulent fluids. The first of these I know he enjoyed because I got to tell him about it during a visit to Cornell. Unfortunately, I do not remember if I had the chance to tell him about the second one.

3.1. *A turbulent analogue of Widom scaling*

The classic problem of fluid dynamics is that fluids appear to be scale-invariant when strongly turbulent.[57,58] Specifically, in a fluid geometry with characteristic scale D (which might be the diameter of a pipe), characteristic velocity U (which might be the average mean flow velocity along a pipe) and kinematic viscosity ν, we define the Reynolds number as $Re = UD/\nu$. In addition, there is the energy dissipation rate ϵ, which is in steady state equal to the energy input rate. The so-called energy spectrum — the kinetic energy per unit mass of fluid per unit wavenumber k — was argued by Kolmogorov,[59] in a paper universally known as K41, to vary as $E(k) = \epsilon^{2/3}k^{-5/3}$ at large Re, and this is broadly in agreement with experimental data going back more than 50 years.[60] The classical measurements and theories only concerned themselves with the lowest-order moments of the velocity fluctuation probability density, but today, there is strong evidence for multifractal scaling of higher-order moments.[58] Kolmogorov further showed that the range in k-space over which this scaling occurred was intermediate between the scale of forcing D and the Kolmogorov scale of dissipation $\eta_K = (\nu^3/\epsilon)^{1/4}$, in other words $2\pi/D \ll k \ll 2\pi/\eta_K$. As $Re \to \infty$, this range of scaling increases because the Kolmogorov scale gets smaller and smaller, but can be shown to always be above the mean free path, so that the fluid is always in the hydrodynamic limit. In fact, K41 is not quite correct, even for the second-order correlation function, and there is a so-called large-scale intermittency correction, η, which changes the scaling result to $E(k) \sim k^{-5/3+\eta}$.[61,62]

To a statistical mechanic, this scaling behavior is reminiscent of what happens in a magnet near a critical point. In general, correlation functions decay as a power law within a range of wavenumber that is intermediate between the lattice spacing a and the correlation length ξ. As the temperature T approaches its critical value T_c, the correlation length diverges to infinity, and at any scale accessible to experiment, the correlations will be in the power-law scaling range, ultimately scaling as $G(k) \sim k^{-2+\eta}$ at the critical temperature itself. Here, η is a correction to the mean-field result, sometimes called Fisher's exponent, and now understood to be the anomalous scaling dimension of the magnetization M, which grows with a power law β as $T \to T_c^-$.

Statistical mechanicians also know that these results are only valid when there is no external field H. When $H \neq 0$, there are more complicated scaling laws. For example, in general, we expect that the magnetization is proportional to the external field, but this linear response law breaks down exactly at the critical temperature: $M(H, T_c) \sim H^{1/\delta}$, where δ is another critical exponent that can of course be calculated by renormalization group. In the critical region $H \sim 0$ and $t \to 0$ (where $t \equiv (T - T_c)/T_c$), Widom showed[63] that the behavior of the magnetization, ostensibly a function of both H and t is actually a function of a combined variable, predicting a data collapse of $M(H, T)$ when plotted appropriately, similar to the data collapse that we talked about with polymer solution theory. This scaling was independently discovered by Kadanoff and explained in a famous paper the following year,[64] and this discovery by Widom and Kadanoff was instrumental in the development of the renormalization group.

Thus, it is a natural question to ask: Is there a counterpart to the Widom scaling in turbulence?

I approached this question by asking what are the analogous quantities to t and H in turbulence. With Greg Eyink, I had long ago argued that it is natural for $1/t$ to be analogous to Re because both control the size of the intermediate regime in k space where there is power-law scaling as $1/t$ and Re, respectively, go to infinity.[65] But what is the turbulent analogue of H? My reasoning was that H is a variable that couples to and induces magnetization. In a pipe, it turns out that the laminar flow is linearly stable, but if it has rough walls, the roughness will excite turbulence. Thus, my guess was that the roughness scale s could be analogous to H. Using the same sort of scaling arguments that Kadanoff and Widom had used, and assuming that turbulence was in fact a non-equilibrium steady state with its own critical point at $Re \to \infty$, $s \to 0$, it was possible to predict the analogue of Widom's scaling law.[66]

Fortunately, there were experimental data on this very question! In 1933, Nikuradse had measured the friction experienced by a turbulent fluid as it transited a pipe with rough walls.[67] Importantly, Nikuradse had systematically varied both Re and the roughness scale s. His data collapsed satisfactorily when plotted according to the formula.[66] However, I had only used the mean-field exponents in my collapse, the ones found in K41. Mehrafarin and Pourtolami extended my calculation to include the intermittency correction.[68] Even though the correction is very small for the second-order correlation function, they were able to show convincingly that they could extract it by improving the data collapse fit. Their result was consistent with known estimates for the intermittency exponent obtained by direct measurements of velocity fluctuations.

This result is extraordinary for the following reason. In phase transition theory, it is known that there is a connection between purely thermodynamic critical exponents and those associated with spatial correlations.[28] It turns out that it is possible to deduce Fisher's exponent for correlations, the anomalous dimension of the order parameter, directly from the thermodynamic exponent δ that we mentioned above quantifies the breakdown of linear response theory at the critical point. In my treatment of the turbulent scaling problem, the analogue of the thermodynamic exponent is one that concerns the scaling of the pressure drop along the pipe with Re. So, using the data collapse scaling law, if it had been known in 1933, Nikuradse could have measured indirectly the intermittency exponent of turbulence characterizing the strong spatial fluctuations! In other words, the fluctuations of turbulence are directly related to the pressure drop along a pipe, i.e., the dissipation experienced. This implies that there are non-equilibrium fluctuation–dissipation relations in turbulence, but at the present time, we do not know how to extract them.

There were other developments that are important to mention. My Illinois colleagues Pinaki Chakraborty and Gustavo Gioia came up with an ingenious heuristic mean-field argument that predicted the various exponents that are visible in different regimes of the Nikuradse data,[69,70] at least at mean-field level.[71] With Nicholas Guttenberg, I worked out the scaling laws and simulated the flow in a rough two-dimensional pipe (i.e., a soap film suspended between two wires),[72] where the point was that in two dimensions, a different scaling of the velocity correlations is possible than those arising from the K41 theory for the energy cascade, because of the flow of angular momentum fluctuations (enstrophy). This fact meant that we could in principle construct flows with different velocity correlation scaling from the K41 one, and observe the effect on the pressure drop, i.e., the friction, and thus test the fluctuation–dissipation relation that had been discovered. Our predictions were fully confirmed in experiments that we performed with Walter Goldburg at Pittsburgh and Hamid Kellay in Bordeaux.[73–75]

Overall, these findings suggest that fully developed turbulence is controlled by a non-equilibrium critical point, with strong connections to a far from equilibrium statistical mechanics through an unknown fluctuation–dissipation relation.

However, this was not the only surprise in treating turbulence as a problem in statistical mechanics.[76]

3.2. *Fluids become turbulent through a non-equilibrium phase transition*

Up to now, we have talked about the large Re behavior of turbulence. But how do fluids transition from predictable, smooth, laminar flows to unpredictable, fluctuating turbulent flows? In certain shear flows where the laminar–turbulence transition is sub-critical, including pipe flow, we now have compelling theoretical,[77–83] computational[84,85] and experimental[86–89] evidence that this transition is a non-equilibrium phase transition in the universality class of directed percolation.[90] A synthesis is beginning to emerge,[76,91,92] and most workers in this small field tend to agree that the one-dimensional problem is understood in varying levels of detail, but the problem of the transition to turbulence in two dimensions is still not understood theoretically, even though the experimental data are very compelling.[88]

I believe that Fisher would have been excited by these developments because it is fascinating to see phase transition physics, which he calls "my own love," emerge unexpectedly in a field such as fluid dynamics, where there is no explicit stochasticity, no partition function, and no obvious connection to directed percolation. This is an intriguing example of universality, to be sure. But I think Fisher would have been equally delighted by the way that this prediction was made. When my students and I set out to tackle this problem, our perspective was that the worst starting point for understanding the transition to turbulence was the Navier–Stokes equations. Instead, our goal was to construct the level of description that corresponded to what would be Landau theory for an equilibrium transition. To this end, we used direct numerical simulations to identify the weak long-wavelength modes that are relevant at a putative continuous laminar–turbulence transition, and once we had identified these, constructed the generic description, which turned out to be a stochastic predator–prey or activator–inhibitor model.[81] In other words, we wanted to identify the variables that defined the universality class of the laminar–turbulence transition. This model led to directed percolation in a straightforward way using known techniques,[93] and as we showed was able to reproduce the universal aspects of the phenomenology at the laminar–turbulence transition.[76,81,83]

We did not set out to derive the directed percolation transition from the Navier–Stokes equations because we already knew that this sort of derivation from low levels of description cannot be done systematically, let alone rigorously, even for the simplest systems, such as equilibrium magnets. We also did not attempt to predict the critical Reynolds number of the laminar–turbulence transition because we know from the renormalization group in equilibrium statistical mechanics that this is not universal.

In a later paper reviewing the statistical mechanics approach to turbulence,[76] we did give a heuristic motivation for the predator–prey description, at least at mean-field level, but I believe that it is fair to say that the majority of fluid mechanicians do not yet share Fisher's perspective that it is more important to understand the cooperative interactions of the emergent degrees of freedom at the appropriate level of description than to systematically derive these degrees of freedom. The reason for his perspective, and the reason why it is so counterintuitive, is that the renormalization group approach relies on a heuristic calculation or guess for the emergent level of description, and then calculates the relevant degrees of freedom at the appropriate fixed points. This approach obviates the need to calculate quantities that will neither affect the universal properties nor the final goal of qualitiative understanding.

4. Concluding Remarks

It is an unavoidable temptation to imagine the reaction when Fisher's manuscript arrived on Feshbach's desk. Not only does its very title threaten to undermine the rationale for the whole enterprise, but Fisher doubles down, arguing at one point that "...it is a mistake to view complementarity as merely a two-terminal black box ... a fully reductionist philosophy, while tenable purely as philosophy, is the wrong way to practice real science!" For many editors and organizers of a symposium on the legacy of Niels Bohr, Fisher's article might have seemed like an unwarranted provocation. But I suspect that in Feshbach's case, he appreciated the article as a masterly contribution that both recalled the extraordinary collision between physics and philosophy of a previous age, and heralded the dawn of a new era where this confrontation would be renewed, but on fresh ground, whose boundaries had been demarcated by Fisher's warning shot across the bow.

Surprisingly, it would be over a decade later before the first skirmishes took place. I would place their date to be the 2001 publication of a book by R.W. Batterman, entitled *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*.[29] Drawing on sources such as the works of Michael Berry on caustics and asymptotics,[94,95] Leo Kadanoff,[64] Michael Fisher[96] and Ken Wilson[97] on critical phenomena and renormalization group, and my own work on asymptotics and renormalization group,[98,99] as summarized especially in Ref. 28, Batterman weaves together the concepts of minimal models, asymptotics, renormalization group theory and emergence in a thesis that explicitly recognizes how the modern way of doing science is neither Popperian nor Kuhnian, partly because of a more sophisticated notion of falsifiability entailed by concepts such as universality. This work and its extensions[31,38] have been increasingly influential during the last 20 years, and the battles have been fought mostly by philosophers of science. Most recently, Batterman has used hydrodynamic linear response theory as an example to expound further on these ideas, echoing many of the themes that Fisher advocated and which I have described here.[38]

I believe that the philosophical debates about minimal models are especially important for complex systems such as biology and turbulent fluid mechanics, because the choices one makes in modeling of these fields are so much more difficult than in physics. I discussed above how the minimal model and renormalization group approach to modeling at the appropriate level of description was successful in understanding quantitatively the laminar–turbulence transition in certain shear flows. The idea of making effective models of turbulence is, however, not new to fluid mechanics. Indeed, there is a long tradition of making approximate models, and these are discussed in the textbooks, especially Pope's excellent monograph on turbulent flow, see Section 8.3 in Ref. 57. These are not minimal models because the "ultimate objective is to obtain a tractable quantitative theory or model that can be used to calculate quantities of interest and practical relevance."[57] This means using models as approximations for computer simulations or simple analytical calculations whose utility is assessed by factors such as cost and ease of use, range of applicability and accuracy.[57] In general, it is not the goal to devise universal scaling functions, for example, in the way that minimal modeling was used in the semi-dilute polymer problem.[27,51]

That being said, it must not be forgotten that Kolmogorov's program of analyzing turbulence began with two similarity hypotheses about universality and this is to my knowledge the first example of where the renormalization group perspective was articulated clearly[59] and followed up in many seminal works by Barenblatt (Kolmogorov's last student!).[100,101] The first of these is that the energy spectrum can be written in the middle or inertial range of scales as $E(k) = (\nu^2/\eta_K)F(k\eta_K)$, where the limit $kL \to \infty$ has been assumed to exist and been taken, and the scaling function $F(z)$ must be the same for all cases of locally isotropic turbulence.[59] This hypothesis does *not* require the large Reynolds number limit to have been taken, and indeed, it has been verified to a good approximation even for transitional turbulent flows.[102] Only when a second similarity hypothesis is taken, that the $Re \to \infty$ limit exists, does the K41 scaling emerge. In fact, the connection with critical phenomena is very close: the first similarity hypothesis is not quite correct. The $kL \to \infty$ limit does not exist, and the asymptotics exhibit incomplete similarity[100,101] leading to the existence of a new exponent, the intermittency exponent that we described earlier.

In the renormalization group-informed perspective presented throughout this chapter, minimal models are in the universality class of the transition and thus give a quantitatively accurate account of the transition. They are not approximations in the sense that they have inadequate realism. As we have discussed above, the predictions of minimal models are quantitatively accurate, despite the fact that the minimal models are seemingly lacking in realism. In biology, there are many more levels of description than in physical systems because of the intertwining of molecular sequence, three-dimensional structure, large-scale molecular motions, elasticity, gene expression, metabolism and energy flow, regulation, signaling, cell division, tissue mechanics, electrical activity,..., all the way up to ecology and evolutionary processes. Choosing the right questions and the right levels of description is critically important because there is no other way forward. As Fisher wrote: "Schrodinger's equation really does have *almost no* relevance to what one actually does in vast areas of chemistry."[1,2] I believe that the analysis of biological modeling from the perspective of philosophy of science would be a very interesting project, one which might bring a broader range of topics into the field and which might ultimately help biophysicists and biologists in their efforts to understand these complex systems by being aware of the necessity to choose the right level of description. In my own work, there are a number of examples where this has been attempted, in topics ranging from the evolution of the genetic code,[103] the open-ended growth of biological complexity,[104] to the topological scaling laws that have recently been uncovered in phylogenetic trees.[105]

For physicists, it was all over by the early 1990s, as even elementary particle physicists came to the clear realization that their subject was about effective field theory and not about fundamental physical law. This was a remarkable 180° turn for a community which by and large had regarded renormalizability as a necessary but not sufficient condition for fundamental quantum field theories and which still adhered to the program of first understanding the basic constituents of matter, so that they could then be combined to understand the low-energy world to which we have direct access. But within a few years of Fisher's article, this view was superseded by the general acceptance of effective field theory, the fascinating history of which has been beautifully described from a personal perspective by Weinberg[106] and summarized intellectually by Georgi.[107] I want to end by briefly talking about effective field theory, through the lens of Georgi's review, because it is truly remarkable to see the convergence between the modern perspective of condensed matter, as championed by Fisher, and the modern perspective of high-energy physics, as embodied in effective field theory.

4.1. *High-energy physics: Does high energy matter?*

The starting point of effective field theory is the recognition that the goal has changed. The goal is not to create a theory of everything but, according to Georgi, "to isolate a set of phenomena from all the rest, so that we can describe it without having to understand everything... Fortunately this is often possible. We can divide the parameter space of the world into different regions, in each of which there is a different appropriate description of the important physics."[107] This is the pragmatic rationale for doing effective theory, but historically, this is not how it developed. If we had a theory of everything, it would be awfully unwieldy to calculate something specific and so for convenience you "use the effective theory... it makes calculations easier because you are forced to concentrate on the important physics."[107] In practice, this means doing an expansion in scale, shrinking to zero size the features of the physics that are smaller than the scale of interest. As Georgi writes,[107] "This gives a useful and simple picture of the important physics. The finite size effects that you have ignored are all small and can be included as perturbations." The attentive reader will have noticed that this passage is uncannily reminiscent of the modeling strategy articulated earlier in this article by Bogoliubov in 1947.[19] In high-energy physics, this strategy is equivalent to removing massive particles, but the price paid for this massive oversimplification (sorry, I could not resist!) is that the ultraviolet regularization is now non-trivial because of the way that coupling constants vary with scale. Integrating out the high-energy degrees of freedom results in a non-local theory because of virtual exchange processes with the neglected massive particles. The program of effective theory replaces these non-local interactions with local interactions that are[107] "...constructed to give the same physics at low energies...

Thus the domain of utility of an effective theory is necessarily bounded from above in energy scale." This is of course reminiscent of Bogoliubov (prompted by Landau) connecting his minimal model to reality by replacing his point interaction amplitude with the amplitude of the binary collision probability in the Born scattering approximation and of course to the Eliashberg extension of the BCS model of superconductivity.

I pause to note another important connection to condensed matter physics. The scale dependence of interactions was discovered in the context of critical phenomena by Kadanoff[64] and Patashinski and Pokrovsky[108] in 1966 but identified earlier during the development of the renormalization group,[109,110] and used by the Landau[111] school in the context of quantum electrodynamics and the famous "Moscow Zero"[112–116] — the divergence of the coupling constant that renders the field theory ill defined at high energies. I refer the interested reader to a thoughtful modern perspective on the Moscow Zero in the context of condensed matter physics[117] and the remarkable experimental observation of the scale dependence of the coupling constant in graphene.[118]

The effective field theory is also bounded below in energy. This is because the effective field theory will generate its own massive particles, and on energy scales smaller than these masses, these particles can again be eliminated to generate a new effective theory at a lower-energy scale too. Thus, an effective field theory lives in an intermediate or "middle" scale of energy.

Effective field theory is in practice looked at in a different way because we do not have any information about the high-energy theory that we invoked above. So, effective field theory is used to describe the physics of an energy scale of interest up to a certain level of accuracy, with a small or finite number of parameters that "parameterize our ignorance in a useful way."[107] This is not the same as the old-fashioned view of renormalizability in field theory because it is expected that with increasing energy, the non-renormalizable interactions get replaced with a new effective theory.

Weinberg recounts[106] how his perspective started to change in 1976, when he learned about Wilson's approach to critical phenomena by integrating out the ultraviolet degrees of freedom and using the renormalization group equation to ensure that physical quantities are cutoff independent. He realised that this entails introducing "every possible interaction renormalizable or not, to keep physics strictly cutoff independent. From this point of view, it doesn't make much difference whether the underlying theory is renormalizable or not... Non-renormalizable theories, I realized, are just as renormalizable as renormalizable theories."[106]

Georgi describes this change in perspective in a remarkable comment that sounds the death knell for the fundamental viewpoint of elementary particle physics[107]: "How does this process end? It is possible, I suppose, that at some very large energy scale... the theory is simply renormalizable in the old sense. This seems unlikely...

It may even be possible that there is no end, simply more and more scales as one goes to higher and higher energy. Who knows? Who cares?" Michael Fisher would agree.

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