Part Seven. Renormalization group

18. What is fundamental physics? A renormalization group perspective

DAVID NELSON

The renormalization group, as embodied in the Callan–Symanzik equation, has had a profound impact on field theory and particle physics. You've heard from Ramamurti Shankar and Michael about some of its impact on condensed matter physics. I'd like to tell you my view of what the renormalization group has meant to practising condensed matter physicists.

What have we learned from renormalization theory? We learned that the detailed physics of matter at microscopic length scales and high energies is *irrelevant* for critical phenomena. Many different microscopic theories lead to exactly the same physical laws at a critical point. As Michael Fisher explained, one can even make precise quantitative predictions about certain 'universal' critical exponents without getting the microscopic physics right in detail. What is important is symmetry, conservation laws, the range of interactions, and the dimensionality of space. The physics of the diverging fluctuations at a critical point, which take place on scales of a micron or more, that's 10^4 angstrom, is largely 'decoupled' from the physics at angstrom length scales.

This story about scaling laws at a critical point tells us something about the meaning of a 'fundamental physics'. Fundamental physics is *not* necessarily the physics of smaller and smaller length scales, to the extent that these length scales decouple from the physics that we're interested in at the moment. To elaborate on this point, I'd like to refer to a short paper, which influenced me a lot as a graduate student, that Ken Wilson wrote in 1972. He argued that Landau's hydrodynamic treatment of magnets in the 1930s was not about critical phenomena, but just about magnets in general at any temperature, possibly quite far from the Curie temperature, the critical point. He argued that hydrodynamic theory was itself representative of a renormalization group fixed point. A rather simple one that prompted me to think a little more in detail about generic classes of theories away from critical points, and how they might be viewed in the context of renormalization theory. You can make similar statements about the hydrodynamic laws derived for fluids in the nineteenth century.

The idea of nineteenth century hydrodynamic physics was that, upon systematically integrating out the high frequency, short wavelength modes associated with atoms and molecules, one ought to be able to arrive at a universal long wavelength theory of fluids, say, the Navier–Stokes equations, regardless of whether the fluid was composed of argon, water, toluene, benzene.

One does *not* have to be at a critical point in order to have universal physical laws, which are insensitive to the microscopic details. We now have many concrete calculations, well away from critical points, which support this point of view. Ignorance about microscopic details is typically packaged into a few phenomenological parameters characterizing the 'fixed point', such as the density and viscosity of an incompressible fluid like water. The extreme insensitivity of the hydrodynamics of fluids to the precise physics at higher energies and shorter distances is highlighted when we remember that the Navier–Stokes equations were actually derived in the early nineteenth century. They were completed in about 1845, at a time when the discrete atomistic nature of matter was still in doubt. The same equations would have resulted had matter been continuous at all length scales. The existence of atoms and molecules is irrelevant to the profound and, some might say, even fundamental problem of understanding the Navier–Stokes equations at high Reynold's numbers. We would face almost identical problems in constructing a theory of turbulence if quantum mechanics did not exist, or if matter first became discrete at length scales of Fermis instead of Angstroms.

Many problems in condensed matter physics, by which I mean the study of matter at everyday length or energy scales, do, of course, depend crucially on quantum mechanics and particulate nature of matter: we can't begin to understand phonons in solids, the specific heat of metals, localization in semiconductors, the quantum Hall effect, and high temperature superconductors without knowing about quantum mechanics of protons, neutrons, electrons, and occasionally even muons and positrons. There comes a point, however, when conventional 'fundamental' particle physics burrows down to such short length scales and high energies that its conclusions, however beautiful, become largely irrelevant to the physics of the world around us. That is why many of my colleagues in condensed matter are not aiming to discover 'fundamental' laws at the smallest accessible length scales. High energy physics, while remaining a noble intellectual enterprise, is now virtually 'decoupled' from physics at Angstrom scales just as atomic physics is decoupled from the Navier–Stokes equations. New particles discovered in high energy physics will *not* help us understand turbulence, or how itinerant magnetism arises from the Hubbard model, nor will they unravel the complexities of repetition dynamics in entangled polymer melts. Completing the 'periodic table of quarks' by discovering a sixth top quark in a particle accelerator was exciting, but so were the discoveries of the exotic and short-lived atoms berkelium, californium, einsteinium, fermium, mendelevium, nobelium and lawrencium in their day. Nowadays, it's hard to get excited about the discovery of element number 119. David Mermin has compared the experiments at the new generation of particle accelerators to an archeological dig into the remote early history of the universe. The connection with cosmology is thrilling, but the results are about as relevant to the way matter behaves today as newly discovered shards of ancient Sumerian pottery would be to the next presidential election.

I don't want to overstate the case I am trying to make here. Research into particle physics has great intellectual value, and many condensed matter physicists, including myself, shamelessly exported its beautiful mathematical ideas into our own research. There's a wonderful cross-fertilization between the two disciplines which dates back at least to Ken Wilson and continues to this day.

I have only tried to argue that the physics at the length scales of the more exotic quarks and leptons, despite its intrinsic interest, is decoupled from physics at Angstrom length scales, just as atomic and molecular physics are decoupled from hard physics problems, like turbulence, which occur at even larger distances. The precise nature of physics at even shorter length scales is unlikely to have a profound impact on deep unresolved problems at much larger scales.

Of course I do not wish to imply that the correct short distance theory is useless. A first principle calculation of the viscosity and density of water, for example, would require an atomic or molecular starting point. Deriving hydrodynamics from an atomistic framework is the task of kinetic theory, which has made significant progress in the past century, at least for weakly interacting gases. We are all delighted that lattice gauge theories of QCD may now be fulfilling their promise, and correctly predicting the mass ratios of pions, protons, and neutrons, just as we are thrilled, at least condensed matter people are, when *ab initio* band structure experts are able to predict the lattice constant and correct crystal structure of silicon simply by solving Schrödinger's equation. The point is that there are always important problems, such as turbulence in fluids or the stability of quasi-crystals, which a 'constructionist' approach based on a more fundamental microscopic theory is unlikely to resolve. It may even be that we are even now far from truly 'fundamental' theory, with new particles and fields, such as preons inside quarks, appearing as we probe ever more deeply into nature. *All* current physics would then be phenomenological, with our ignorance about the detailed physics at smaller length scales packaged into a small number of phenomenological coupling constants.

Discussions

Fisher: Thank you, David, for a beautiful demonstration of what I was preaching yesterday, namely, that the renormalization group within condensed matter physics is an approach of enormous richness. Part of that richness arises because there are many, many different systems we can look at, and many, many puzzles to sort out concerning them!

It's also important for the philosophers to realize that these field theories, like Navier–Stokes equation, many of them are very old and were found in easy ways. Generally these are the ones we characterize as trivial these days, and this latest example I think is particularly striking because of the instability of the von Carmen fixed point.

I'm going to stress one other thing, which I know David will agree with. But if you had doubts that he had done everything he should have done here, you could say, well, what about the such and such term that you would have wanted to put in that he left out. He's probably put it in and checked that it's irrelevant. But if not, it's a calculation that you can do. So although he has largely put down what are sometimes called the minimal theories, you can check that the things that are really there need to be there.

Audience: I would like to hear you elaborate on what you said last night after Steven Weinberg made his remarks about renormalization. In effect, you challenged him to come and listen to a refutation.

Fisher: Yes, that is correct. The point is that Steven Weinberg was addressing the topic of the symposium and, as I pointed out in my own introductory remarks, one can hold that the renormalization group does not matter much for quantum field theory in a way that is really interesting. Thus, in the context of quantum field theory, Weinberg said: 'Well, look, we had the Gell-Mann–Low equation and that basically enabled us to study logarithmic factors and what happens when we change the scale on which we study the physics.' What was it, from that viewpoint, that Wilson added that was really new? The answer is: the integration out of the less important degrees of freedom. That was the point that I felt had to be made. I had a brief word with Weinberg after his talk and I believe he does not disagree.

On the other hand, David Gross took pains to actually say, admittedly in one fast sentence, that the essence of the revolution which Wilson inaugurated that has changed the whole way we look at condensed matter theory was to introduce the *space*, if you will, *of all possible Hamiltonians* and *flows* induced within that space by the action of the renormalization group.

I also wanted to emphasize that if, say, Professor Treiman did not happen to like what David Nelson wrote down for his basic Hamiltonian, he could add his own chosen terms. Then the renormalization group approach provides a philosophy, a conceptual way of calculating and testing to see if the new terms are relevant or irrelevant for the behavior under study.

So when David Nelson first started thinking about membranes and tethered surfaces, he told me: 'This theory of elastic shells is a wonderful thing and it is already in the books.' But now he has told you, in effect, that even if he had not got the von Carmen theory and did not know of this particular, corresponding fixed point, it would not matter anyway since, on the long length scales of interest, it is unstable. And so, to put it another way, it is not relevant for real fluctuating surfaces or networks. Now, unless you have this perspective of stable and unstable fixed points, relevance and irrelevance, marginality, and so on, you are really lost. Hence, from the viewpoint of condensed matter physics, the crucial point made by Wilson was that universality and other fundamental, novel concepts arise naturally once you allow yourself a big enough space of Hamiltonians in which to follow the renormalization group flows.

Now from a practical angle, as David Nelson brought out, one can often expect, optimistically, that there are only three or four parameters that you really need to know about in the appropriate effective field theory. If you are Landau or someone with comparable talents, you may succeed in guessing the correct effective theory. It might even be a more or less trivial theory that turns out, mainly for reasons of 'luck', to be the correct one. Then, if only a few parameters are relevant, a practical theoretical physicist (who wants to explain some experiments) would like an efficient method of calculating with those essential parameters. If there is only one variable, she might use the Gell-Mann–Low type of formulation. If there are only a couple of parameters, then the Callan–Symanzik equation or similar approaches provide efficient ways of computing explicit results. But if, as often happens in condensed matter physics, one does not really know the answer (and if proof by Shankar's method of using as small parameter the inverse ego of the investigator fails to go through perhaps because of inadequate hero worship in the field) then you need some calculational approach and some basic philosophy in which you can have a reasoned argument about the issues, about certain terms being left out, about the difference between tethered and nontethered membranes, and so on. Thanks to Wilson, such discussions have waxed fast and furious and for very good reasons.