has been, on the whole, very positive. It has provided excellent observational facilities and has shown that added value can come from the resulting interaction among partners. This success is no news for particle physics, a field in which cost has essentially driven the most complex experiments to a single global site.

It is the conflict between shared facilities and "national" science that may itself generate a problem. Does each nation try to use its shared facilities to steal a scientific advance on its partners? As the sheer scale of frontier experiment and observation increases, and time on such facilities becomes ever more expensive, we need a new approach. We must share the science, too.

In astronomy, the next generation of large telescopes—the Atacama Large Millimeter Array and a proposed extremely large 50- to 100-meter optical/infrared telescope—must be global projects. Both the US and ESO are heavily involved, and that involvement necessitates stronger ties. But to capitalize on the great investment involved, will we be able to share the glory of the inevitable discoveries? Will the *New York Times* headlines declare "World Science Team Discovers . . . " or will it be "US Worried As UK/European 'Boffins' Scoop Discovery"?

Achieving a global aspect to science programs may well be difficult, even in good international partnerships. Satisfying reasonable national aspirations for observation time and still running major joint programs is not trivial. The US has experienced such problems in the rather uneasy atmosphere generated by variable community access to national and privately-funded observatories. Large joint international observing programs have a reputation for inefficient use of telescope time, and innovative thinking is needed to allocate this scarce resource effectively and equitably.

Perhaps it's just a shift of credit that will be needed. We scientists (and the funding governments) need to be prepared to acknowledge and accept success for the global project itself, an accolade for all the partners rather than individuals. In big projects we will need to share, rather than grab, success. Science itself knows no regional or national barriers. Neither should our pursuit of it.

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Renormalized Relations in Condensed Matter

In his article "Brainwashed by Feynman?" (PHYSICS TODAY, February 2000, page 11), Philip W. Anderson says that a generation of "field theoretically trained young theorists" now performs essentially irrelevant studies of the interacting fermions in condensed matter: "The obvious assumption is that if one is able, by dint of very hard work, to sum up all the Feynman diagrams, one must arrive at the right answer. The problem is that no one has ever been able, over four decades, even to arrive at the right interaction that way." Anderson invites these theorists to imitate their particle physics colleagues, who "have long abandoned straightforward diagrams, in favor of a much more varied toolkit of concepts and techniques."

This view, from a leader of theoretical condensed matter physics, calls for a debate. As a field theoretically trained (no longer young) theorist, I attack the problem of interacting fermions by resumming "all the diagrams." I agree with Anderson that the phenomenology of the interactions in "borderline materials" is still poorly understood, and that the description of many phenomena in condensed matter may require tools other than straightforward perturbation theory. However, I would not dismiss perturbation theory, for instance, by believing that it is limited to the analysis of essentially boring weakly coupled theories.

The study of interacting fermions in condensed matter by resumming diagrams has at least two scientific godfathers: Richard Feynman and Kenneth Wilson. In the late 1980s, a group of mathematical physicists, Giuseppe Benfatto, Joel Feldman, Giovanni Gallavotti, and Eugene Trubowitz, also made important conceptual progress: They realized that Wilson's renormalization group should be adapted in a nontrivial way to condensed matter. Indeed the long-range behavior of a system of interacting fermions is governed by an extended singularity, the Fermi surface. As a result, the corresponding scaling analysis and the underlying dynamical flow of effective interactions is much richer than in the ordinary Wilsonian case. No simple analog exists of the "block spin" and rescaling concepts. More important, there is an infinite set of relevant operators, or of coupling constants. Feynman diagrams are essential to analyze the corresponding flows.

Organizing perturbation theory based on the renormalization group around the Fermi surface is therefore not only conceptual progress, it is probably (even numerically[1]) the best tool to understand which among all these couplings diverges first and dominates the long-range physics. I also do not think that the analogy with the confinement problem for hadrons is relevant; we know that, because of the extended nature of the Fermi surface in more than one dimension, nonperturbative phenomena in condensed matter physics (particularly the formation of bound states such as Cooper pairs) can actually be controlled by analytic methods because of their similarity to large-component vector models.[2] Despite many efforts, this is not yet the case for hadrons, because matrix models are involved in their formation.

But is it the use of Feynman graphs or the pretension to "sum them all" that Anderson considers irrelevant?

If the latter is the case, here is a brief defense of traditional, perhaps old-fashioned,[3] mathematical physics, which consists in proving mathematical theorems about idealized systems inspired by physics. I view it as an indispensable complement to theoretical physics in the long run.

When Lars Onsager proved that the two-dimensional Ising model has a phase transition, or when John Imbrie solved a controversy about the nature of the ground state of the random-field Ising model in three dimensions,[4] they certainly did not believe any real material to be an exact Ising model. Nevertheless, each of their results acquires a particular value because it is mathematically rigorous. Even more than diamonds, mathematical theorems are forever; they are precious strongholds among all the uncertainties of an ever-changing scientific landscape.

The question of whether perturbation theory can be summed up or not goes back at least to Henri Poincaré. Even a negative result, such as his famous observation that the Lindstedt perturbation series of classical mechanics diverges, is a scientific landmark, a starting point for advances such as the convergence theorems of the Kolmogorov-Arnold-Moser type and the investigation of chaos. Therefore, a mathematical physicist educated in the Poincaré spirit, when learning that first-order perturbation theory makes sense in a particular physical context, worries whether adding "all" the diagrams does not change the picture.

To return to condensed matter, Manfred Salmhofer recently formalized a precise mathematical criterion to distinguish Fermi liquids above the Bardeen-Cooper-Schrieffer temperature from, for example, Luttinger liquids.[5] Summing up "all the diagrams," Margherita Disertori and I proved that an isotropic jellium with a small, short-range interaction in two dimensions is indeed a Fermi liquid in this sense.[6] Certainly, at least for simple models, interacting Fermi liquid theory is mathematically consistent in two dimensions; it is also, one would hope, a step toward rigorously settling the phase diagram of more complicated and realistic models, such as the Hubbard model near half-filling, and towards the rigorous analysis of the two-dimensional Anderson model of free or weakly interacting electrons in a random potential.

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ANDERSON REPLIES: The question Vincent Rivasseau raises is the subject of a recent paper of mine,[1] which I believe explains the situation satisfactorily. It appears that there is actually no mathematical contradiction between our two results.

Rivasseau and Margherita Disertori explicitly point out in Rivasseau's reference 6 that their perturbative renormalization group procedure eventually fails due to lack of convergence, and is only valid above a certain explicitly derived temperature. I would agree completely with that statement. My arguments stem from a second rigorous approach to the many-body problem, the method of Kerson Huang, T.-D. Lee, and C. N. Yang, and of Viktor Galitskii, which was applied to the two-dimensional case by Paul Bloom in 1975.[2] Bloom also claimed to have proved rigorously that the system is a simple Fermi liquid, in a different limit, near $T = 0$ and for arbitrarily strong interactions, but for low density. I show that Bloom's calculation is in error, but the difference between Bloom's and my results would only be visible below Rivasseau's limiting temperature—so I agree that the electron gas would look like a Fermi liquid above that temperature. I think it must be significant that the temperature limits derived from two completely different approaches are the same. But it is worth noting that for real physical interactions the Rivasseau limitation excludes any T appreciably below the Fermi temperature.

Of course, I cannot be accused of too much opposition to the use of the renormalization group in the many-body problem, since I invented it. Rivasseau's thumbnail history does not mention that I was the first to use the renormalization group on a many-body problem, with Gideon Yuval, in a paper submitted in 1969 on the Kondo Hamiltonian.[3] We predate Kenneth Wilson by some months, though the paper was delayed by an eminent but unperceptive referee. I may also have been the first to suggest using it on the Fermi liquid.[4] Thus, the subsequent work by Rivasseau and others is not "an adaptation of Wilson's renormalization group" but of mine, though of course I make no claim to its more important use in statistical mechanics.

Some of the above resulted from valuable discussions with Manfred Salmhofer at the Institute for Theoretical Physics in Santa Barbara, California.

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Mystery Error in Gamow's *Tompkins* Reappears

The review by Daniel M. Greenberger of Russell Stannard's *The New World of Mr. Tompkins* (PHYSICS TODAY, June 2000, page 57) intrigued me sufficiently that I obtained a copy of this revised and extended version of George Gamow's (now dated) three classic works for the general reader. I agree with much of what Greenberger writes. Stannard has "done a remarkable job of preserving the mood and feeling of the original," and I hope that at least some of the few small slips will be edited out before this commendable book is reprinted.

One error, however, struck me rather forcibly. In the diagram on page 159, the professor is lecturing to his evening audience, which contains the (as usual) dozing Mr. Tompkins. A slide of the Bohr–Sommerfeld orbits for principal quantum numbers $n = 2,3$ has been projected onto the screen. Unfortunately, the orbits with highest azimuthal quantum number (orbital angular momentum quantum number l) are shown as ellipses with the highest eccentricity, while the s-orbits ($l = 0$) are shown as circles. This confusion of "penetrating" with "nonpenetrating" orbits is common, occasioned perhaps by the recollection that in the wave-mechanical picture, the s-orbitals are spherically symmetrical, albeit with the important property that their probability densities peak at the origin. This property is of vital importance, for example, for the Lamb shift and the Fermi contact interaction in hyperfine structure, quite apart from the fact that the quantum defects are therefore largest for the s-orbitals in many-electron atoms. For comparison purposes, excellent diagrams of these two distinctly different forms of representation may be found in reference 1.

How did this unfortunate mistake occur in the new work? On turning to its predecessor,[2] one finds on page 132 a picture containing the same Bohr–Sommerfeld orbits and, in addition, the quantization rules in the "old quantum theory" and formulas for the energies. (A keen-eyed reader will spot that the electronic charge e is raised to the wrong power in two places.) Moreover, in this diagram by Gamow himself, the orbitals are labeled in exactly the