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