**The Antiphysical Review**

Founded and Edited by M. Apostol **18** (1999)

ISSN 1453-4436

# Kopietz, Springer and Bosonization

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## Abstract

**P. Kopietz,** *Bosonization of Interacting Fermions in Arbitrary Dimensions*, **Springer Lecture Notes in Physics M48, 1997**

Recently, Kopietz wrote and Springer published the book named above. I was intrigued by the title and, since my resources are scarce, I asked the author and the publisher to send me a copy; which they have kindly done. Now, I am in a delicate position: to publish or not to publish my critical opinion about this book? Because I do not like it at all, and consider it a very bad book.

On the one hand, I think that everyone should be free (and welcome) to be interested in science; but, on the other hand, it is a fact that only a few succeed. As natural as this distinction is, it is, however, almost never made. I have never yet heard someone say "I wrote a bad book (or article) but I will nevertheless have it published, sorry folks". Such an illogical sentence is perhaps still deemed to be too rude and too aggressive, and it is not said yet. But I prefer to believe that most people do not know about, or are not able to distinguish between correctness and incorrectness, or, rather, they do not believe that there could exist such things as correct and incorrect, good and bad. Since I know, since I am able to make the aforementioned distinction, and since science and pseudo-science do exist, I decided to publish my criticism here. After all (and in subsidiary), science is an exercise of critical reasoning too.

Apparently, the book aims at treating the Fermi liquid by a bosonization method. It is hard to see the reasonability of such an enterprise. The theory of the Fermi liquid was done many years ago, and it answers beautifully all the pertinent questions related to interacting fermions. It is a perturbational theory in its essence, and shows that interacting fermions behave as fermionic quasi-particles. Any theory, of course, has its own limits, and the knowledge of these limits is most valuable. Just so, it has been discovered subsequently that interacting fermions in one dimension behave differently, they are not perturbational, and can not possibly be treated as a Fermi liquid. Instead, their excited states consist of bosons, and a bosonization theory has been developed for them. In order to distinguish them from the Fermi liquid, the bosonized fermions in one dimension are usually called the "Luttinger liquid". So, then, why would one try to bosonize the Fermi liquid, why look for bosons there where we know that they do not exist?

Unfortunately, such an enterprise is not unique. In the course of time there have been other similar attempts, some quoted in this book. When free of errors (which is rare), all of these attempts make essential, arbitrary assumptions, which, in fact, spoil the problem by introducing artificial, strange objects, distinct of those stated naturally by the original problem. I have nothing against the artificial objects; but there is given no reason at all as to the necessity of introducing such new objects, and, further, they are not even defined, in fact! These assumptions are actually self-contradictory, if not, at least, by name contamination.

Without exception, all of these unintelligent endeavours, like the present book, do not reproduce known results, nor raise - not to speak of solving - new problems. One is then compelled, with good reason, to ask oneself "What are all these strange new objects called books, (or scientific papers)?". Of course, one may be replied that this is something new, and those who do not understand do not understand, but others do understand, and that is it. Only that I understand, and say that all this is pseudo-science.

One can hardly imagine that a book may begin with a wrong statement; Kopietz' and Springer's book discussed here does. In Introduction, first line of the text, one may read: "The long-wavelength... behaviour of the single-particle Green's function $G(\mathbf{k}, \omega)$...". Everybody knows that "long-wavelength" refers to small wavevectors $\mathbf{k}$, but the author refers to finite wavevectors, of course, especially those on the Fermi surface; which he even states explicitly on the next page.

Further on, and amusingly enough, the author seems to assert that the perturbation series of the interacting fermions can not be proven convergent due to the singularities of the one-particle Green functions. I wonder why, from such a premise, does he not infere that, in fact, the perturbation series is divergent? But instead he appraises Dyson, who, according to him, indicated a way out of this difficulty; namely that instead of working with the Green function $G$ one should work with its reciprocal $G^{-1}$. Hence, according to him, the importance of Dyson's equations. These statements are wrong. One may prefer to pose such a formal problem, or pseudo-problem, as the convergence of the perturbation series, if only because we live in a free world; but there is no need to solve it, as shown both by the Fermi liquid theory and by bosonization, at least; not to mention the quantum electrodynamics.

(Unfortunately, the reading of this book has moved me to imagine strange, nonsensical situations, of a metaphysical nature it must be said; and I apologize for becoming a philosopher for a moment. But why, if someone poses the problem of the convergence of the perturbation series, would that person not pose too, for instance, the problem of the finite velocity of moving bodies. Because, from the definition $\mathbf{v} = d\mathbf{r}/dt$ of the velocity, it does not follow that velocity is finite - it may very well be infinite; and why should we not try to "prove" under what circumstances the velocity would be finite, and under what others would it be infinite? This is ridiculous of course, since how can one possibly "prove" a convention, or a proposition? One can only accept it or not, but when not accepting the science conventions one should be aware of the risk of doing pseudo-science. Science is a set of useful conventions about the empirical, or natural, world; our basic working material is these conventions. A warranty that they may be useful is their clarity, consistency, and mathematical nature. Their usefulness is proved by their ability to describe and classify such a large variety of empirical situations. The conventional character of science makes science both possible and tentative - and that is almost all there is to it, I think. How one could get to these useful conventions is hard to say, but anyone can form a personal idea on this matter by studying science; my own idea, if matters, is that the way to science is the pure contemplation of the science. I apologize again for this excursion, divagation or digression.)

There are many errors in this book, probably an infinite number, because every explicit error entails many other implicit ones. I could not possibly analyze all of them, but let me point out that the author does not even know the proper language of the subject he is treating in this book. For instance, a few lines down on the first page, one may read "Because the qualitative features

... are usually determined by certain universal parameters such as dimensionality, symmetries and conservation laws...". If dimensionality could perhaps be termed a parameter, it is hard to do so with symmetries and conservation laws. The latter are not parameters, they are what they are, *i.e.* symmetries and conservation laws. Playing words is not science.

Embarrassingly enough, even the author's grammar is incorrect. Paragraph 1.2 on p.5 begins with the following sentence: "The discovery of the high-temperature superconductors and Anderson and coworkers suggestion...has revived the interest...". The predicate here must be in the plural, *i.e.* "have revived the interest...", because the subject is so, namely "the discovery..." and "Anderson and..."; the latter are quite distinct, in the sense that one can hardly make a connection between "the discovery of the high-temperature superconductors" and "Anderson and coworkers", and this is why this subject is in the plural. I am sorry for indicating this error.

There is a strange sentence which may be read on p. 6. After telling that the perturbation theory in dimensions higher than one is consistent for "regular interactions" the author announces that, "Nevertheless, consistency of perturbation theory does not imply that the perturbative result must be correct." How could a thing possibly be consistent and still incorrect? Such a statement must be qualified; as, for instance, a thing may be consistent in itself but yet be politically incorrect. Which means that the thing works, but we do not like it, it is against our, or others', interests. But, again, this is not science, this is politics (or policy), and the author seems not to know what he is talking about.

Speaking of politics, or policy, the author pays a heavy tribute to Haldane, who, we are told, "added the grain of salt" and had a "crucial insight" (p.6) by dividing the Fermi surface into boxes, and neglecting the momentum transfer between boxes, after Luther's similar work (with "tubes") "has not received much attention".

Actually, there is a funny story about these boxes, sectors or "patches" of the Fermi surface; let me tell it briefly here. If one assumes that the interaction transfers only small momenta, then these boxes are practically distinct objects, provided that their size is much greater than the interaction cut-off. The interaction part of the Hamiltonian can then be written in terms of the density fluctuations in each box, and these operators satisfy approximate boson-like commutation relations. Now, limiting the momentum transfer to small values only means a certain type of long-range interactions (or "forward scattering" as Kopietz likes to say), and this is a very unrealistic restriction. So, this can only be accepted as a model; and let us hope that it might teach us something. Unfortunately, the author' s approach is inconsistent, because a small momentum transfer requires a uniform coupling between the boxes, but yet he still includes a non-uniform one. This is a major drawback, and it is hard to see the basic reasons for doing such a contradictory thing.

But let us pass over this, and accept for the moment that the size of the boxes should be large in comparison with the interaction cut-off. However, large box sizes are inconvenient for the "bosonization" of the kinetic part of the Hamiltonian, due to the motion along the Fermi surface. Since the author still seeks "bosonization", he feels the need of "renormalizing" his statements. Consequently, on p.27 we read the ridiculous statement that the size of the boxes "should not be chosen too small, but also not too large"; but what then is the proper quantitative choice? "It depends", we are told, and that is almost all.

I say that, as long as such vague assumptions are not made clear, the computations presented in a publication, or a book like this one, should not be followed, and the book should not be read.

But since the author and publisher had been so kind as to send me a free copy, I shall continue to read further.

Since, after a long interval, the issue comes up again in the discussion in paragraph 4.3.4 on p.65, entitled "The hidden small parameter". However, in four full pages of text and equations the reader is not once told what this parameter is. We are left to guess that it would probably be the one given in eq. (4.115). Is this parameter so well hidden that not even the author himself can find it? Or is it so small that the author is ashamed of it? Anyway, we are left with the idea that the box sizes should now be small. But, if they are too small, there will again be problems with the momentum transfer of the interaction, and, in fact, we lose in this case the bosonic character of the density fluctuations (just as it is also lost for a very large box). The author, like many others who deal with this problem, fails to give a clear statement about the size of these boxes. What, then, could one learn from such an undefined, arbitrary model?

The author seems to think, incorrectly, that one may take the boxes large enough, and compensate the error by computing the corresponding corrections brought about by the non-linear terms in the energy expansion on the Fermi surface. But, in spite of his criticism of the previous works which failed to present such a computation ("The problem of calculating these corrections within the conventional operator approach seems to be very difficult and so far has not been solved.", p.6; which amounts to saying that these authors publish but do not solve), and in spite of his own claims of having done this, such a computation is inconsistent in terms of bosons.

Actually, and to end my story, the construction of such boxes is not only impossible but not even desirable; because the motion, or momentum transfer, along the Fermi surface can not be eliminated from the problem - they are simply there. This is the difference between fermions in one dimension, which accept "bosonization", and fermions in dimensions higher than one. This obvious fact has been pointed out before Haldane's works, which appeared in 1992 and 1994, both by Luther in 1979, and by myself in 1977 (Rev. Roum. Phys. **22** 1045 (1977); the other references may be found in Kopietz' book).

In fact, the author himself seems to make little use of these boxes; though he declares on p.8 that "these geometric constructions are the key to the generalization of the bosonization approach to arbitrary dimensions." Hence one may see that this author does not in fact know what he is doing (or saying). Because, and with this I arrive at the central point of the book, Kopietz employs the Hubbard-Stratonovich functional integration in order to represent the effects of the interaction. As it is known the functional integrals may be used with both fermions and bosons; and when correctly manipulated they will reproduce the known, correct results. They will show, for instance, that the Fermi liquid is described by fermionic quasi-particles. So, what the author is doing (if he would do it correctly) is actually a "fermionization" of the Fermi liquids in more than one dimensions, and a "bosonization" of the fermions in one dimension, just as it ought to be, and which we already know.

Then why is he doing this? The only possible answer after reading this book, in my opinion, is that he is doing this just to have an opportunity to make mistakes. With the self-contradicting assumption of box "bosonization" noted above, for instance, the "bosonized" hamiltonian given in (4.55) and (4.56) on p.57 could have been directly written down with no need of resorting to functional integrals; but the author does not attempt to diagonalize it by employing the usual well-known methods of the one-dimensional bosonization. When this is done, one may convince oneself that this hamiltonian would never reproduce the Fermi-liquid quasi-particles, and, in certain cases, it leads in fact to inconsistent results.

To beat around the bush, the author takes pride in announcing that, by using the Hubbard-Stratonovich functional integration, he is able to include in his computations the non-linear terms in the energy expansion on the Fermi surface, and thereby to take into account the curvature of the Fermi surface in dimensions higher than one. This would justify, in his view, giving up the small-size boxes and working with large boxes only - even with the entire Fermi surface. In his own words on p.28 "...we may...formally identify the entire momentum space with a single sector. Then the around-the-corner processes simply do not exist." All the troubles with the boxes, among which would be these "around-the-corner processes", *i.e.* interaction processes which couple box boundaries, would then be swept away.

This is too simplistic to be true, unfortunately. When properly handled, the kinetic part of the fermionic hamiltonian generates, via functional integrals, effective hamiltonians (or actions) which are often taken as describing boson degrees of freedom. These effective hamiltonians contain interactions between bosons, and perturbation calculations are usually attempted to treat them. For instance, the quadratic terms in the energy expansion leads to a boson interaction which contains the inverse of the mass parameters; so that the latter may naturally be taken as the perturbation parameters. So "speculated" Haldane in one of his works, but Kopietz criticizes this on p.92 as a "naive expansion". The suggestion is not as much "naive" as simply wrong, because the mass parameters are dictated by the Fermi surface, and consequently they may or may not be small. The author's criticism does not seem to refer however to this point, and in fact this criticism is given up in paragraph 5.2.2, where the author suggests on pp.104-105 that such a perturbational parameter would be his "hidden small parameter"; which, however, includes in itself the inverse of the mass parameters. The reader is then brought back to the box problem, to the vague model considerations mentioned above, and, thus, unknown problems are relegated to unknown problems, and so on, and so on... . In particular, all these ill manipulations made by the author would lead to an essential cut-off dependence of the single-fermion properties, which render the "results" both arbitrary and unphysical. It is also worth pointing out in this connection how the author tries to play formally between the "fermionization" of the Fermi liquid and the bosonization of the one-dimensional fermions. As expected, he does this by using very general representations, like Schwinger's ansatz for the Green function in the direct space, and the eikonal approximation for the motion of the bosonic fields. Naturally, by choosing one or another particular case he believes that he is able to talk formally both fermions and bosons.

Formally, because the author does not give any specific, known result from the Fermi liquid theory, or from the bosonization theory of fermions in one dimension, though he writes down many general formulas, describes many long calculations, employs concrete forms of interaction, and talks on various subjects, and various physical systems, like "quasi-one-dimensional metals," electron-phonon interaction", "fermions in a stochastic medium", "transverse gauge fields", "screening and collective modes"; he decries only the lack of time for having not studied also, by his method, the van Hove singularities, "The fact that so far I have not studied this problem by myself... does not necessarily mean that this problem is very difficult ... . I simply have not found the time to work on this problem." (the footnote on p.31). Indeed, I can agree that the problem is not "very difficult", but my advice would have been that, instead of having written this book, the author would better have studied the van Hove singularities (and many other problems too, from the general curriculum of physics); and, by the way, using classical methods first. Anyone interested in this book would have expected to see, for instance, an effective mass for fermions in three dimensions computed, for a given interaction by the author's method, or the zero-sound, etc; or, similarly, the Fermi distribution at the Fermi points in one dimension, etc, such that one would be able to check the author's calculations. None of this, or anything like it, can be found in this book.

The author believes that his approach is non-perturbational; probably because he employs, for instance, the Schwinger ansatz for the single-fermion Green function, and obtains thereby an exponential Debye-Waller factor-structure for the Green function. This would mean, however, that Schwinger's ansatz is non-perturbational, not the author's approach. Indeed, by including non-linear terms in the kinetic energy, the author gets, via Schwinger's ansatz, the eikonal equation for the Green function, which he "solves", however, perturbationally, in spite of some of his claims. In addition, the relevant quantities given by the author for the Green function in (5.152) and (5.153) on p.106 have a strikingly similar structure with the second-order perturbation theory for the Green function. Unfortunately, all these formal manipulations are inconsistent for many reasons. For instance, the effect of the bosonic fields, when properly treated, are quite comparable with the own dynamics of the individual fermions, which would preclude by far a perturbational treatament, and in fact makes useless or impossible the introduction of the bosonic fields (while in one dimension the two aspects coincide). In particular, the author applies his computations to highly singular interactions, and the corresponding perturbation integrals are made convergent by the well-known random-phase-approximation screening. However, as is known, the latter implies formal summations of divergent perturbation integrals, and, as such, it leads to inconsistent results. In this respect, the formal manipulations with singular quantities, either in perturbation theory or in functional integrals, are incorrect.

I can not refrain from indicating in brief a few additional errors in the book, which I consider to be extremely grave. For instance, see the $f$-quantities in the hamiltonian (2.3) on p 11, which are not, I underline, they are not, "the so-called Landau interaction parameters"; considerations on the counter-terms which would eliminate contributions to the chemical potential (p.12), which are pure fantasy, I mean they are wrong, even if Anderson had talked them recently. Speaking of fantasy, I must note also the fancy subscript "mat" of the hamiltonian $H_{mat}$ of a many-fermion ensemble, nowhere explained; could it mean "mathematical"? If so, is it relevant? What then would be the "physical" hamiltonian? Probably it would be made to carry the subscript "phys", so much I begin already to accept; and to care not anymore.

So, I stop here. To summarize, the biggest error of this book is that its author does not succeed in defining the subject he is talking about. Since I have seldom read so bad a book as this one, I shall kindly ask the author and the publisher for permission to keep my free copy as a souvenir. (After all, I make, thereby, my modest contribution to a diminishment of the number of available copies on the market.) In exchange, I give them the (free) advice to no longer write or publish such bad books.

I am perfectly aware, as I am told, that my criticism above could be taken personally, which I did not intend at all. Thereby, I may incur the risk of being excluded, to say so, to some extent, from the scientific community. Yes, I consider myself a member of this community, since I use to endeavour with physics research. Such an exclusion would be pretty harmful to me, because I have no other means of support. We all live, more or less, in a world of power, not of understanding, so such an exclusion would not at all be unlikely. Given this prospect, I may "renormalize" my statements, and admit that the above book has qualities too. For instance, one of its qualities is that it is not singular, there are many other books and publications which contain similar errors. My criticism, so far as is possible, addresses equally these publications too. After all, I can understand this game, some authors write and some others criticize, so, the latter ones too, like myself, may find a job for themselves in the scientific community. If this would seem too unpleasant for some, I can assure them that it will not last too long.