Subject: AJP: decision on ms 25512
From: AJP Editorial Office <ajp@dickinson.edu>
Date: 2013/04/11 23:12
To: "Daijiro Yoshioka" <daijiro@toki.c.u-tokyo.ac.jp>

Dear Professor Yoshioka,

Attached you will find copies of the reviewers' reports on your manuscript "Meissner effect cannot be explained classically," our manuscript #25512. Before discussing my decision, I need to clarify something. The original reviewer R3 was not contacted to review the revision because, quite frankly, their original review provided too little detail to be useful (I couldn't even tell if the reviewer had read the manuscript). Because of this, original reviewer R4 has been re-labelled as R3 for this round of review.

As you can see, these reviewers point out a number of difficulties with the manuscript and the consensus opinion is against publication. I find this to be a particularly difficult decision because on the one hand, I agree with R1 who states that there are likely many readers who agree with your viewpoint and that a dissenting opinion should probably be heard. On the other hand, given that you already had a Comment rejected on this topic and have now had two versions of this manuscript reviewed and the consensus is still that the manuscript should not be published in its present form, I am unsure whether another round of revision will lead to a useful paper for the readership of AJP.

My own reading of the manuscript leaves me feeling that you believe the original paper is trying to claim that superconductivity can be explained classically. But this is not at all the case. Thus, the entire first portion of your manuscript seems out of place and unnecessary. In addition, there have been numerous publications over the years that make use of classical models to explain various superconducting phenomena, with varying degrees of success. None of these authors suggest that superconductivity can be explained via classical physics, but they do note that classical models can provide a simple way to understand and calculate various aspects of the phenomena. In my view this supports the claims made in the original paper, and leaves the onus on you to demonstrate exactly where the original authors have gone wrong. So far you have been unable to convincingly do so.

Unfortunately, the review process cannot continue indefinitely, and based on this most recent set of reviews I do not believe we should continue editorial consideration of this manuscript. AJP receives many submissions, almost all of which have some value. However, we can only accept a limited number of manuscripts and thus manuscripts must receive enthusiastic support from all reviewers for further consideration. I am writing to inform you, therefore, that we will not pursue publication of this manuscript.

Thank you for your interest in the American Journal of Physics.

Sincerely,

David Jackson

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Attachments:

R1's report
R2's report
R3's report

—AJP MS25512-2 R1's report.txt

The author has a problem with the results of the original paper, which is fine. His opinion is likely shared by many, and a dissenting opinion should probably be heard. However, the author's attempts to explain what is wrong with the original paper boil down to the beginning of his section IV. THE ORIGIN OF THE FLAW. He disputes the statement in the original paper (a result derived in reference 29 by the same authors) that when a sphere expels a pre-existing applied magnetic field from its interior the total magnetic energy is reduced. The author claims that it costs energy to remove/expel this field. The most useful, and convincing, argument that he could make would be to show a flaw in the derivation of reference 29 that argues for a reduction of the total magnetic field energy when expulsion occurs. However, he does not do so. At present the author disagrees with the energetics, and instead of showing where in the derivation there is an error or a mistake, he presents his own interpretations and derivations of the energetics. This approach is less than convincing in its present form.

Aside from this main point, there are still some nagging difficulties. The author appears to mix up classical electrodynamics and classical mechanics (page 2). This should be addressed. On page 3, just above equations (8), he references equation (4), but it appears he actually means to reference equation (6). This should also be addressed. On page 4, after equation (14), he writes that $B^2/mu\_o = W + E\_B$, which seems to be equating an energy density to energies. This appears to be dimensionally incorrect, and should be addressed.

There are some larger issues. Pages 2 - 3, under III Strange Consequences, point 1. Aside from the equation reference issue mentioned above, this argument does not make a convicing point. First there is a derivation which appears to support the conclusions of the original paper. Then, condensation energy is used in a confusing argument where latent heats and entropy equal zero at T=0 are involved at the last. Whatever this argument is, it needs to be developed more carefully.

I presume his point 2 follows from his point 1. Without a more clear point 1 then point 2 is also unclear.

Point 3 is confusing. In the London theory there is a supercurrent density, which the author uses on the previous page. Does this not mean that the metal is in the superconducting state? If so, then how can he state that this is not so?

—AJP MS25512-2 R3's report.txt

I think the manuscript could be feasible for publication later, but is far away from it. The review made by the author of the new manuscript was not carefully and did not respond to questions and suggestions from referees. I think that a reader would not understand why quantum mechanics is needed to explain the effect Meisnner, especially: why the origin of the condensation energy comes from quantum mechanics?, and why in the BCS theory the magnetic field is expelled?. So, I do not recommend the manuscript for publication.

—添付ファイル:

AJP MS25512-2 R1's report.txt	2.5 KB
AJP MS25512-2 R2's report.pdf	22.1 KB

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