



REPLY

Reply to the Comment by Jacob Szeftel et al.

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Reply

Reply to the Comment by Jacob Szeftel et al.

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In the introduction, the authors make three assertions about my *EPL* paper:

- 1) “The main assumption, regarding the Meissner effect, which the whole argument relies upon, is disproved”.
- 2) “The subsequent analysis misconstrues an original view of the Meissner effect by other authors [2]”.
- 3) “The discussion of the Joule effect turns out to violate the first law of thermodynamics”.

I argue that this comment is wrong in its entirety.

The three assertions are discussed in the three following sections referring to those in the Comment. I will address them one by one.

Section “An incorrect assumption”. – Here the authors point out that I am assuming that the final state of a system undergoing the Meissner effect is independent of the transient regime. That is correct. Then they say that “this mainstream view has been disproved [10]”, where [10] is a paper by the same authors. And they state: “Since Hirsch’s main argument [1] has been thereby rebutted, we could end our review at that point”.

The assumption that the equilibrium state of a type-I superconductor in a magnetic field is uniquely determined by the temperature and magnetic field is *not* a “long-standing fallacy”, as these authors claim. The final state in the Meissner effect does *not* depend on the whole transient, as these authors claim. The fact that the equilibrium state of a type-I superconductor in a magnetic field is unique and independent of history has been established experimentally by the original work of Meissner in 1933 and repeatedly confirmed experimentally thereafter. It was established theoretically by the work of Gorter and Casimir on thermodynamics of superconductors in 1933 and by the work of the London brothers in 1935 and thereafter on electrodynamics of superconductors, as described clearly in London’s 1950 book cited by the authors as ref. [7]. It is an integral part of BCS theory, and of all other theories of superconductivity I am aware of. And, it is part of my theory. I am not deviating from the conventional

understanding on this in the paper being commented on by these authors, nor in any of my other work on superconductivity.

These authors think that this well-established fact of superconductivity is wrong. It is up to them to convince the scientific community that they are right, but this is clearly not the place to do it.

In addition, the first sentence in this section is very confusing to the reader: it reads “Hirsch rephrases therein [1] a discussion which had been previously published by himself [3–5] and further rebutted [6] by us”. Now ref. [1] is my published *EPL* paper, ref. [4] is the preprint (arXiv) version of that paper, refs. [3] and [5] are two other preprints by me in arXiv dealing with related topics. Reference [6] is the arXiv version of this same comment by the authors. So I am not “rephrasing” anything, nor is ref. [6] a “further” rebuttal.

Section “Meissner effect”. – The section starts by stating: “Although Hirsch [1] has long favored an interpretation of the Meissner effect, based on quantum pressure [15], he suddenly embraces quite an unrelated explanation [2,8,10]”. References [2], [8] and [10] are papers by the authors of this Comment in 2018 and 2019.

I do not “apply” any of the authors’ arguments exposed in those references, as the Comment claims. In the authors’ references, two different lengths are introduced, λ_L and λ_M , and physical arguments are made with which I completely disagree. My paper is completely unrelated to those references, my paper continues to develop the same approach to the Meissner effect that I started to develop more than 15 years ago.

For example, the authors state: “In this novel view, the Meissner effect is ascribed to the susceptibility χ , going from paramagnetic ($\chi_n > 0$) in the normal ($T > T_c$) state to diamagnetic ($\chi_s < 0$) in the superconducting ($T < T_c$) state (T_c stands for the critical temperature)”. In fact, the susceptibility for diamagnetic metals in the normal state is also negative, but much smaller in magnitude than in

the superconducting state. I explain how to understand this within my point of view in a way that is completely unrelated to any of the authors' papers, see, for example, my papers [1,2] or [3].

Section “Joule effect”. – All statements in this section are also incorrect. For example,

1) “Hirsch ascribes [1] the whole Joule heat released during the transient regime to eddy currents, carried by normal electrons. However their contribution is negligible because the *ac* conductivity of superconducting electrons can be larger than the normal one by five orders of magnitude”.

The whole Joule heat released during the transient regime is indeed due to eddy currents carried by normal electrons. There is no other contribution.

2) “The work, performed by the Faraday field and giving rise thereby to the eddy current, typical of the Meissner effect, has been overlooked”.

Not so, the work performed by the Faraday field is taken into account.

3) “Only the heat, exchanged with an external reservoir, is considered [1], whereas that released through the Joule effect has been completely disregarded”.

Not so, the heat released through the Joule effect is transferred to the reservoir and is not disregarded.

4) “As a matter of fact, the Joule power [...] released in a superconductor, has been shown elsewhere [18] to comprise two contributions...”.

The authors are advocating their own unconventional point of view which is not accepted by anybody, including myself.

5) “It must be recalled that the specific heat of a superconductor depends [19] upon the current flowing through it, because the current modifies the respective concentrations of normal and superconducting electrons”. “A simple experiment has been proposed [18] to bring evidence for the anomalous Joule effect and to validate thereby a novel explanation of the persistent currents”.

Again, I believe the authors' “novel explanation” is incorrect. It is certainly not generally accepted.

In summary, I argue that this comment is wrong in its entirety and does not call into question the validity of my paper.

REFERENCES

- [1] HIRSCH J. E., *J. Phys.: Condens. Matter*, **20** (2008) 235233.
- [2] HIRSCH J. E., *Int. J. Mod. Phys. B*, **25** (2011) 1173.
- [3] HIRSCH J. E., *Phys. Scr.*, **85** (2012) 035704.