March 13, 2020

Response to report of Reviewer #1 of paper "Thermodynamic inconsistency of the conventional theory of superconductivity"

I am grateful to the referee for the time and effort spent in reviewing my paper. In what follows I give a point by point response.

(1) The referee starts the review by stating *"I counted a dozen of the author's claims about inconsistency of the conventional theory of superconductivity, and that the theory is incompatible with the laws of physics before the author explained the essence of the matter in Section VIII."*
Instead, a quick search of the letters *"inconsis"* in the pdf file reviewed by the referee shows that in the introduction the paper states "*We will conclude that the conventional theory is inconsistent...*", and the next time *"inconsis"* appears in the paper is in the title of Section VIII. In other words, it does not appear even once in Sects. II to VII. So the "dozen" "counted" by the referee are not real but imaginary numbers, and reveal the referee's bias.

(2) Second paragraph of the referee:
"As it turned out, everything is not so bad: if the cooling process of the superconductor is very slow, then these claims of the author have no basis in fact (page 10 of the paper). If this process is carried out in a finite time, then, of course, the observables will differ from the predictions of the conventional theory. But, very important, this process is nonequilibrium one, and temperature cannot be the same in different areas of the superconductor."
The first sentence makes no sense. The author (me) claims that an inconsistency arises when the process occurs in a finite time, not when the cooling is infinitely slow.
The second sentence makes no sense. The conventional theory has a prediction for what the initial and final states should be, which does not depend on the speed of the process. I am arguing that this prediction is inconsistent with the thermodynamic analysis. So it makes no sense to say that " *If this process is carried out in a finite time, then, of course, the observables will differ from the predictions of the conventional theory."*
The third sentence makes no sense. We can assume that the temperature is arbitrarily close to being the same in different areas of the superconductor, by simply assuming that the thermal conductivity of the system is sufficently large, for a given speed of the process, so that any temperature gradient is compensated in a timescale much faster than the given speed of the process, so the temperature is essentially homogeneous throughout the system. So the objection "*temperature cannot be the same in different areas..."* is not relevant. I have added a discussion of this point in a new Sect. IV.

(3) Third paragraph of the referee says:
"... One should not require a description of nonequilibrium processes from the conventional theory developed for equilibrium states"
That is not so. The conventional theory of course has implications for how the system responds to probes where the system is taken out of equilibrium. For example, when an

electromagnetic field is applied or an ac current flows. In particular, it predicts that when an electric field is applied to a superconductor at finite temperature, the normal component of the system responds by generating a normal current which dissipates Joule heat. The referee should read for example Section 2.5 of Tinkham's book to learn about this fact. For example, Tinkham says *"a time-varying supercurrent requires an electric field E to accelerate and decelerate the superconducting electrons. This electric field also acts on the so-called "normal" electrons (really thermal excitations from the superconducting ground state, as we shall see in Chap. 3), which scatter from impurities, and can be described by Ohm's law"*. Tinkham furthermore says *"Although this model is, of course, an oversimplification, it is the standard working approximation for understanding electrical losses in superconductors"*. Instead, the referee claims it is impossible to understand electrical losses in superconductors within the conventional theory, so he/she is saying Tinkham is wrong. I hope the editor will side with Tinkham and not with the referee.

(4) Fourth paragraph of the referee says:
"The author claims that "the physical mechanism by which these changes in momenta happen in the process of normal electrons condensing into the superconducting state… has never been discussed in the superconductivity literature". This is completely wrong since the author does not use the microscopic theory at all. This is explained by the condensate wave function. The latter defines the phase of the single boson wave function. This phase, which determines the momentum distribution for bosons, is the same for all condensed particles."
What I say is entirely true. The physical mechanism by which normal electrons condensing into the superconducting state acquire the momentum of the supercurrent, and how this does not violate momentum conservation, has never been discussed in the superconductivity literature. To state as the referee does "*This is explained by the condensate wave function"* explains nothing because it violates momentum conservation. The normal electrons do not carry net momentum, the electrons in the condensate do.

(5) Fifth paragraph of the referee says:
"At certain cooling rates and the superconductor sizes, a time-dependent temperature field must appear into the superconductor. The London approach cannot be used to describe such nonequilibrium state. It should be investigated using microscopic theory (see, for example, Kopnin N.B. Theory of Nonequilibrium Superconductivity). Cooling rates necessary to avoid this do not calculated and not discussed by the author."
As explained in (2) above, we can simply assume that the thermal conductivity of the system is large enough that such complications don't arise. The paper already stated at the beginning of Sect. V "We consider a process where the temperature changes gradually so the system is always in equilibrium". I have now expanded this into a longer discussion to take into account the concern of the referee, in the new Section IV.

(6) Sixth paragraph of the referee says:
"Second, Eq. (26) at the beginning of Section V and, since it is crucial for calculation of the vortex electric field, all of the following formulas are very doubtful to me. Really, the author just take the formula for the magnetic field distribution in the cylinder for the

equilibrium state and use it for the time-varying state, assuming $\lambda_{L}(t)$. Since the cooling process is not specified by the necessary estimates, the vortex electric field must, at least, be found the complete system of Maxwell-Lorentz equations."
The referee objects to the assumption that the magnetic field is given by Eq. (26) with a time-varying $\lambda_{L}(t)$. I argue that it is obviously a reasonable description for a sufficiently slow process, and it will break down when the process is very fast. By "slow" and "fast" I mean compared to other timescales in the problem. That is all I need to prove the point the paper makes about inconsistency. This is related to the point in (5). I have added a discussion of this point in the new Sect. IV. I am using "*complete system of Maxwell-Lorentz equations"* except for the displacement current, which is utterly negligible. I clarify that explicitly now in Sect. VI.

(7) The referee does not recommend publication because he/she has not understood the significance, importance and validity of the results presented in my paper.

In summary, I have carefully considered the referee's points and have added a Section IV to address the referee's and potentially some reader's concerns, as well as an explanation for why the Maxwell term can be ignored in Sect. VI.