Response to report on BG14421/Hirsch

I would like to thank the reviewer for his/her time and effort in reviewing my paper. I would like to ask for reconsideration on the basis that from the reviewer's comments I see that the key issues in my paper were not understood.

1) The reviewer says:
"If true it would constitute a major problem, not only for theory, but for numerous experimental results as well."

There is actually nothing in my paper that constitutes a problem for experimental results. I am not questioning experimental results. I am saying explanations of some experimental results are different from the BCS explanation. For other experimental results the BCS explanation is correct. Yes it would constitute a major problem for theory. Yes I claim BCS theory is not completely correct. I hope the reviewer is open to that possibility. If not, we are talking about religion and there is no point in arguing further.

Assuming that's not the case, let me address the technical points.

2) The reviewer says:
"First, it is not clearly spelled out what the consequences of the claimed inconsistency would be. In other words, which experiment (or theoretical study) would detect the inconsistency unequivocally."
The inconsistency is clearly spelled out, particularly in sects. VIII and XII. Experiments that detect the inconsistency unequivocally are the following: experimentalists will cool a type I superconductor in a magnetic field from temperature T1 to T2, and find that the final state of the system, as well as the amount of heat that was extracted from the system, are identical independent of the speed at which the cooling was done. And this is of course also an assumption of BCS theory.

What my paper is saying is that BCS theory cannot be reconciled with that experimental fact and the laws of thermodynamics, hence it is inconsistent. My paper is the theoretical study that detects this inconsistency unequivocally.

The reason, in a nutshell is:
(i) BCS predicts that the London penetration depth changes with temperature.
(ii) Therefore, in going from T1 to T2, the magnetic flux through a region of the superconductor changes.
(iii) Maxwell predicts that when magnetic flux changes, a Faraday electric field is generated.
(iv) BCS predicts that at finite temperature there is normal fluid (Bogoliubov quasiparticles), that in the presence of an electric field give rise to a normal current that dissipates Joule heat when it flows.
(v) The total amount of Joule heat generated depends on the speed of the process. In particular, if the process is infinitely slow, the total amount of Joule heat is zero. If the process is at a finite rate, it is not zero.

(vi) The final state of the system, and the final state of the environment (reservoir), are both uniquely defined, independent of the speed of the process. Therefore, their respective change in energy is uniquely defined, and their respective change in entropy is uniquely defined, independent of the speed of the process. This is because energy and entropy are functions of state according to thermodynamics.
(vii) The existence of Joule heat Q_J, and associated generation of Joule entropy Q_J/T, in varying amounts depending on the speed of the process, is incompatible with (vi).
(viii) Therefore, unless we want to give up on thermodynamics or on Maxwell's equations, we have to conclude that BCS is inconsistent with basic physical laws.

Specifically, the inconsistency is that BCS tells us that:
(a) The final state of the superconductor doesn't depend on the process.
(b) The London penetration depth changes with temperature.
(c) An electric field in a superconductor gives rise to normal current that dissipates Joule heat.
My paper shows that (a), (b) and (c) cannot be simultaneously true if thermodynamics and Maxwell's equations are valid. At least one of them is invalid. Therefore, BCS is internally inconsistent because it assumes that all (a), (b) and (c)) are valid.

I believe it is (c) that is invalid. Note that I am not saying that an electric field never generates normal current and Joule heat in a superconductor. It does, if the field is generated from outside, e.g with an incident electromagnetic wave or circulating an ac supercurrent. It is well known that in those cases dissipation does occur. I am saying it is different when the electric field is generated through a change in temperature.

The inconsistency would also be resolved if either (a) or (b) are invalid, I don't believe that's the case.

3) The reviewer says:
"The question posed in the paper is whether the final state is an equilibrium state. It is, of course, an equilibrium state if the change is performed adiabatically, meaning infinitely slowly."
That is not the question posed in the paper. I am assuming both the initial and final states are equilibrium states. If the process is infinitely slow, the intermediate states are also equilibrium states. If the process is at a finite rate, the intermediate states are non-equilibrium states. But that is immaterial to the inconsistency I am pointing out, I don't assume the intermediate states are equilibrium states.

"Experiments are, however, performed at finite rate of change."
I certainly agree.

"The essential conclusion is that the normal component will undergo dissipative processes, which contribute to the entropy balance. The entropy of the final state will therefore differ from that of the equilibrium state."
Yes that is precisely what I am saying, and where the inconsistency is. The system will reach the same final state but with the wrong entropy.

"To describe the cooling process at finite cooling rate in detail is not a simple matter. The thermal reservoir has to be modeled in some way, e.g. as a source of phonons in equilibrium at temperature T (dropping from T1 to T2) and a sink for any phonons emitted from the superconductor."
It is true it is not a simple matter, however I don't have to model any of those details to prove the inconsistency. The beauty of thermodynamics is that it imposes constraints that are independent of details. E.g. energy has to be conserved, even if we don't understand all the detailed processes that make sure energy is conserved. If we forget one process or miscalculate it, energy will not be conserved, then we know we did something wrong. Similarly with entropy, thermodynamics tells us it is a function of state. Those general principles is what I need to use in my proof, they are true independent of the details the referee mentions.

"The state of the superconductor during the cooling process is out of equilibrium, which is to say that the Bogoliubov quasiparticle distribution is not the Fermi distribution, but changes with time in a complicated way."
True. But we don't need to know the details. All we need to know is that there is a normal current carried by those quasiparticles during the cooling process, and that the flow of that current dissipates heat. And that the total heat dissipated depends on the time the process took. I address those points in the paper.

"Whatever the details will be, any increase of entropy caused by dissipation taking place in the system, will not contribute to the final entropy balance of the system, but will be absorbed by the heat reservoir."
The problem with what the referee is saying is, that entropy cannot be "absorbed by the heat reservoir". As argued above, the final state of the reservoir is uniquely defined, therefore so is it's entropy. It cannot absorb extra entropy.

"Suppose the cooling process starts at time t1 (temperature of the reservoir T1) and ends at time t2 (T2), then the system will be still out of equilibrium at time t2, but will equilibrate at temperature T2 after some microscopic relaxation time. The final state will be an equilibrium state, as enforced by the contact to the thermal reservoir."
I don't understand this comment. I am assuming the system starts in its equilibrium state at temperature T1, not in contact with the reservoir at T2. Then, the system is put in thermal contact with the reservoir. Then, at the end of the process both the system and the reservoir are in equilibrium states at temperature T2.

To conclude, I am not convinced that the paper presents sufficient evidence for the existence of a fundamental thermodynamic inconsistency of the conventional theory of superconductivity. The paper should not be published in its present form.
As argued above and in the paper, I believe the paper presents clear indisputable evidence for an inconsistency. I don't understand what the referee means by "sufficient evidence". Either my arguments are wrong or they are right. If they are wrong, somebody should be able to point out precisely where they are wrong. If they are right, they prove the fundamental inconsistency of the conventional theory of superconductivity. I have

thought about this very carefully and am convinced they are right.

I hope the referee will consider the above remarks and reconsider his/her recommendation. Thank you for the time and effort spent on this.

Jorge E. Hirsch