

**THE PHYSICAL REVIEW**  
**AND**  
**PHYSICAL REVIEW LETTERS**

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4 August 1998

Dr. Cronin B. Vining  
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Re: Comment on "Multilayer thermionic refrigeration"

By: Cronin B. Vining

LRK642

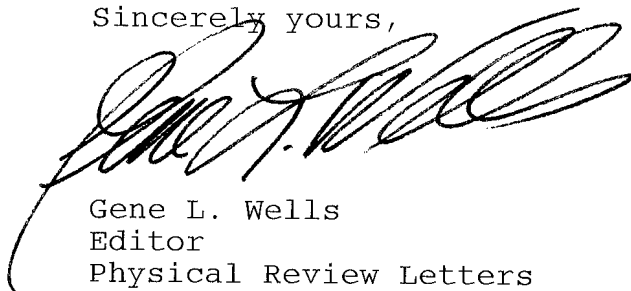
Dear Dr. Vining:

Enclosed are two further referee reports on your Comment, LRK642. Clearly, referee B recommends publication while referee C does not; referee A also recommended against publication.

I recognize many elements of concern about publication of the Letter by Mahan and Woods. However, I do not find that the Comment satisfies our narrow and restrictive criteria. I note that even the favorable referee does not use words that imply our criteria are satisfied.

In accordance with our standard practice (see enclosed memo), this concludes our review of your manuscript.

Sincerely yours,



Gene L. Wells  
Editor  
Physical Review Letters

GW:pm

I have read the comment, the reply, the original paper, and the several letters related to the comment under examination. I understand the referee's hesitations concerning the convenience to publish or not the comment and the reply. In spite of that, I do not share completely the referee's negative conclusion. In my opinion, this brief discussion enlightens a crucial point in the original paper, namely, the issue of thermal resistance. After reading the comment and the reply, one has a clearer understanding of some of the challenges to achieve in practice the proposal in the original paper. Thus, I would recommend publication.

Vining's main point is that the refrigeration scheme put forward in the paper has an efficiency that is primarily due to the assumed high thermal resistivity of the superlattice. He claims that it does not really matter if the device is based on thermionic emission or normal thermoelectric effects, provided a superlattice is used in each case. This is an interesting and important statement. As far as I can tell, Mahan and Woods always compare standard homogeneous thermoelectric devices with a multilayer thermionic device, and the implication of Vining's note is that this is probably misleading and tends to overemphasize the thermionic aspect.

Vining likely has a point when he criticises the statement of Mahan and Woods that - *The results illustrate that ballistic transport is not more efficient than the usual diffusion transport* - and this might be sufficient grounds to publish this as a comment. However, I would encourage him to extend this work to a short paper and publish it in that way. He has made some implicit assumptions that should be justified. In particular he appears to have optimised his solution using bulk semiconductor parameters. The superlattice problem seems to be a bit more complicated than this. I would prefer to see arguments that a superlattice can indeed have the required thermoelectric properties in a direction perpendicular to the plane. The calculations of thermopower by Friedman (J. Phys. C: Solid State v17, 3999 (1984)) might be a start with this. The details of the required structure are also presumably different from that envisioned by Mahan and Woods.

In summary, I think Vining is basically correct and his views should be made known, but probably not as a comment in PRL.