

Review by Prof. Dr. Peer C Zalm

Eindhoven, Netherlands.

***A note on the appraisal of J.F. Prins' book
"Superconduction at Room Temperature without Cooper pairs"***

Although unusual, let me start by briefly introducing myself because this is pertinent to the appraisal of my statement below.

My name is Peter Cornelis Zalm. I am a physicist, working for Philips Research in Eindhoven [NL] since 1978 and Visiting Professor at Salford University in Manchester [UK] since 1991. My professional expertise focuses on ion-solid interactions and surface analysis. I (co-) authored well over 100 papers and refereed many times that number for leading international scientific journals. Of the latter I rejected some 60% and not all of those were strictly in my field. But if and when I could demonstrate an experiment or theory to be demonstrably flawed I considered myself qualified for the job. Also, since 1992 I always waive the reviewer's right to remain anonymous but hitherto this has not provoked any of the rejected authors to challenge my verdict.

I bought Johan Prins' book because I know him and his work on ion implantation into diamond well and was intrigued by the results reported in his papers in Semiconductor Science & Technology Vol.18 supplement (2003). After carefully studying the theories presented in the book and email contact with Johan to clarify certain details, I could not find any obvious mistakes, errors or shortcomings. Of course this may imply that I am simply not sufficiently skilled in this area and can easily be deceived. Therefore I gave the book to a befriended theorist who owed me a favour and asked him to read it critically and comment on it. His reply, several weeks later, was highly unsatisfactory in that he suggested something along the lines of "... the author should read the basic textbooks of so-and-so on superconductivity and study the seminal work of X in this area ..." But what I did not get was a clear answer of what was wrong (or indeed even if anything was wrong) with the proposed model and mechanism.

The attitude of my theorist friend seems to be widespread in our scientific community. If and when somebody challenges a well-established concept he is branded a lunatic and not taken seriously. Now I do agree that we can not correct the weird ideas from all the crackpots of our world. But at the same time most progress has been made through propositions that were highly unconventional at the time. So if and when somebody with the track record of Johan produces a disturbing experimental outcome and a novel type of explanation for that he deserves examination of his results by appropriate experts. Unfortunately there seems to be a certain unwillingness to do so, partly because there is little scientific honour to be gained by confirming someone else's theory or demonstrating the untenability of it. Yet I urge anyone with the capability to do either instead of persevering in denial or mockery. I have a purely selfish reason for this because I either want to know where I goofed or I want to be proud of being able to call one of the true innovators of our field my friend!

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