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# The Influence of Vitaly Ginzburg on a Young Scientist

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In some sense the BCS theory was so successful in the 1960's, that people thought it would be the basis of understanding the experimental work on superconducting alloys and compounds. Looking back, there was much controversy at the time and there were some persuasive arguments that there was a limit to the transition temperature of alloy systems. However, there were some thoughtful people, including Vitaly Ginzburg, who championed the possibility of higher $T_c$'s.

In my introduction to superconductivity, I can say that Vitaly Ginzburg's thoughts on mechanisms of high-temperature superconductors played a crucial role in my first 15 years of superconductivity research. My first real introduction to superconductors occurred during my years at MIT Lincoln Laboratory after my thesis work at Yale University. My thesis project was about the superfluid transition in liquid $He^3$, so even here there was some relation to superconductivity. The actual discovery of the superfluid transition occurred decades later and involved D. D. Osheroff, R. C. Richardson, and D. M. Lee.

At Lincoln Laboratory (an electronics and radar laboratory that is run by MIT for the Air Force) I began my real career in superconductivity and I worked with Emanuel Maxwell, one of the co-discoverers of the isotope effect in superconductors. It was there that I really started learning about superconductivity and, of course, the Ginzburg–Landau theory. At this time Robert Meservey, another former Yale graduate student, was involved in tunneling experiments with David Douglass, a Prof. at MIT, and they explored some of the consequences of the new BCS theory as well as the Ginzburg–Landau theory. Maxwell and I were using a low-frequency mutual inductance bridge to study superconducting alloys. We were trying to see whether the measurements looked consistent with the Abrikosov theory of Type II superconductivity, which was based on Ginzburg–Landau theory. In less than 2 years after my arrival it appeared that the administration at Lincoln Lab began to wonder why there was a low-temperature physics effort in the Radar Group. Of course, we were always there for advice on cryogenic issues, but I have the feeling that we were not really critical to the Radar program. Eventually, they learned how to deal with liquid helium and we were given a choice of working on Radars or leaving. Maxwell and Meservey wound up at the Magnet Lab at MIT, and I came to Brookhaven.

Within a few months after arriving at Brookhaven, I became involved in an interdepartmental co-operative superconductivity program. I represented the physics department. Bill Sampson, one of the first magnet experts, represented the accelerator department and Arthur Paskin, from the metallurgy department, was the head.

The first problem we tackled was the surface layer in Type II and some Type I superconductors. It was an exciting time because there were many interesting phenomena to understand, such as how the field penetrates into superconductors, the vortex lattice in Type II superconductors, and other issues involving the magnetic properties of superconductors. During these studies, David Douglass who was a summer guest came across an obscure paper by Ginzburg [1] published in 1958, which had already dealt with various issues such as "supercooling" and "superheating" with rigorous solutions of the Ginzburg–Landau theory. This paper had

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predicted the superheating field from the G–L theory and it gave more accurate results than surface barrier calculations. For some reason it was unknown, but it dealt with some of the problems people were studying, and probably in a more rigorous way. Douglass and I were going to publish a note about this paper, but we never did. At any rate this was clear evidence that the "master" thought about many of the problems we were interested in those years, and he was always in the background.

This short introduction gets me to the real point of the great role Ginzburg had in shaping my career. In some sense the situation in the early and mid-1960s was very different from the present time. As far as I am concerned, at the present time, there is no conclusive theory of high-temperature superconductors. In the mid-1960s the situation was the opposite. With Ginzburg–Landau theory and the microscopic BCS theory some people even felt the field was coming to end and only details remained. Every kind of experiment on superconductors seemed to agree with the theory and relatively new discoveries like the Josephson effect and related problems seemed to be understood within the framework of the BCS theory. With Abrikosov's extension of G–L theory, the properties of Type II superconductors were also beginning to be understood. To summarize the euphoria of this time I have included a quote by a famous scientist who summarized the Colgate Conference [2] in 1963:

> The success is so remarkable that I almost believe you would forgive me if I say there remain no problems in superconductivity.

I am sure no one would say this today; 43 years later. In some sense the major problem was, what materials can be superconducting and what was the maximum $T_c$. The BCS theory gave an expression for the transition temperature in terms of the phonon frequencies, the density of states, and the net electron–phonon interaction and by 1968 these results were extended into the strong coupling regime by McMillan [3]. On the basis of these ideas some imaginative people proposed that new things were possible. William Little at Stanford had proposed high $T_c$s due to an excitonic mechanism [4] in one-dimensional conductors, and Ginzburg and Kirzhnits [5] proposed the possibilities of high $T_c$ in the surface regime in a letter to JETP and then Ginzburg [6] submitted a *Physics Letter* in November 1964, which outlined phenomena that could happen at surfaces. These papers discussed the two-dimensional aspects along with the effect of overlayers on the surfaces and the possibility of excitonic superconductivity. These short papers opened up an exciting world and I immediately started to set up an elaborate apparatus to achieve ultrathin layers and measure them in situ. Ultimately, it would also be used to measure sandwiched metals and multilayers. At this time, as mentioned previously, Bill Little at Stanford had already proposed the excitonic mechanism in one-dimensional systems and then Ginzburg in the *Physics Letter* above also mentioned the possibility of an excitonic mechanism at surfaces, and later this suggestion was followed by the more elaborate work of Allender *et al.* [7].

I assume people reading this are wondering what really happened with the thin film and surface experiments and I have put in some selected figures of early work. Figure 1 illustrates how $T_c$ is indeed raised, but when the film is thin enough and the resistance is near $h/4e^2$ ($\sim$6500 Ω) localization starts to destroy superconductivity. This is shown better in Fig. 2, which shows the first measure-

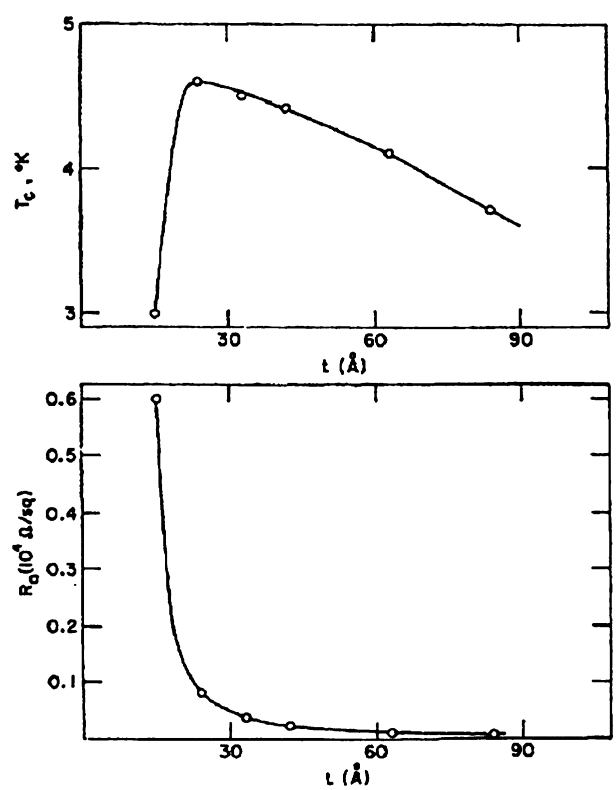


**Fig. 1.** $T_c$ vs. thickness for Al films deposited on previously deposited SiO. Lower graph shows sharply increased resistance at small thicknesses.

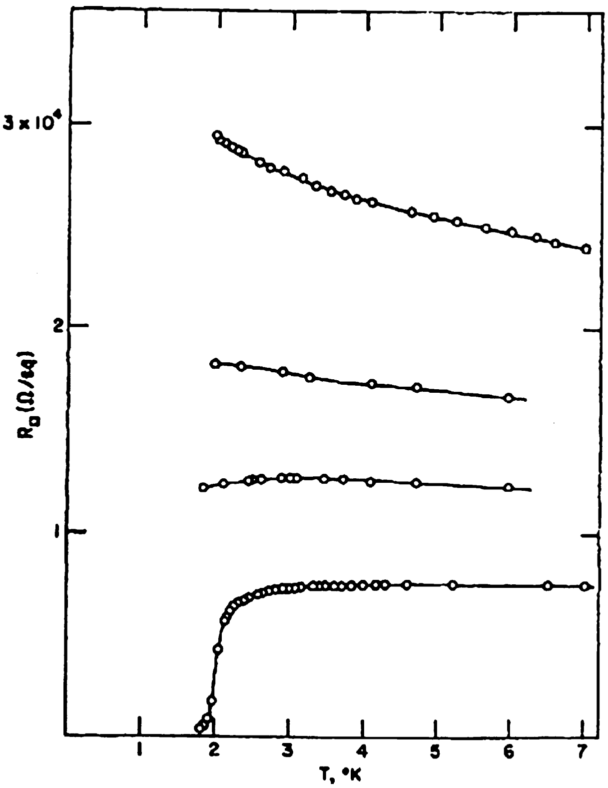


**Fig. 2.** The first measurement of the superconductor/insulator transition. This figure shows the resistive behavior as Pb is built up. First stages show nonmetallic conduction where the film resistance increases as the temperature decreases. As more metal is deposited the resistance decreases and metallic behavior and superconductivity appear. In the middle two curves there is nonmetallic conduction in the normal state and the beginning of a superconducting transition. When the full transition appears the film thickness is of the order of 1 nm.

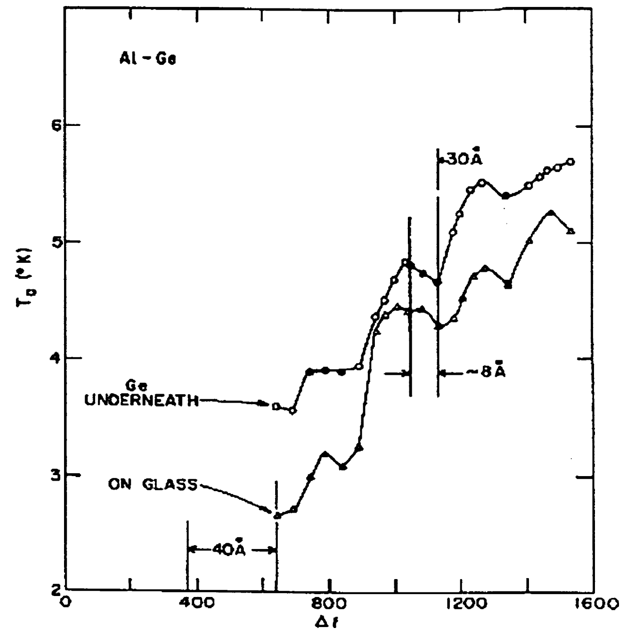


**Fig. 3.** Plot of $T_c$ vs. thickness for alternate layers of Al and Ge. The thickness scale is in units of frequency change in a quartz crystal oscillator thickness monitor. Some thicknesses are given in the figure.

ment of the insulator to superconductor transition. You always lose when the film resistance is of the order of 6500 Ω. In Fig. 3, I show how the $T_c$ of Al [$T_c$(bulk) $\sim 1.2$ K] can be raised to about 6 K in alternating layers of Al and Ge deposited onto substrates held near 4.2 K. This problem is still not understood and the rise in $T_c$ is attributed to phonon softening at the interface. It is interesting that making alternating layers of Al and Cu also yields higher $T_c$s than the bulk value when Al deposited at low temperature. So lots of things went on, but we never reached high-temperature superconductivity or even raised anything to near 10 K. In some sense the most important result of this exercise was to define the limits of superconductivity with disorder, which overshadowed anything else. Ultimately, the superconductor/insulator transition has become an exciting branch of physics. The important point was that this was a new regime to study as far as superconductivity was concerned and although there was no high $T_c$ there were other fundamental discoveries. It is still not clear how disorder and inhomogeneities affect the properties of the new high-temperature superconductors and this is still an active area in both high $T_c$ superconductors and regular superconductors.

Sometime around 1970, I found out that John Bardeen was trying to get experiments started at the University of Illinois to look for the possibility of excitonic superconductivity. I immediately called Bardeen and we agreed to pool our resources and a talented student named David Miller came to Brookhaven from Illinois to work on this problem. By this time we had excellent ultra-high vacuum systems with low-energy electron diffraction apparatus and cryostats to study the structure of films along with measuring $T_c$. For a few years we struggled to put epitaxial layers of metal on PbTe. Of course, by this time we had learned that disorder can reduce $T_c$ and we tried to make the lowest resistance, ultrathin films that we could. This, of course, was very difficult and it took a year to get things going. Films were made on the PbTe in ultra-high vacuum, studied with

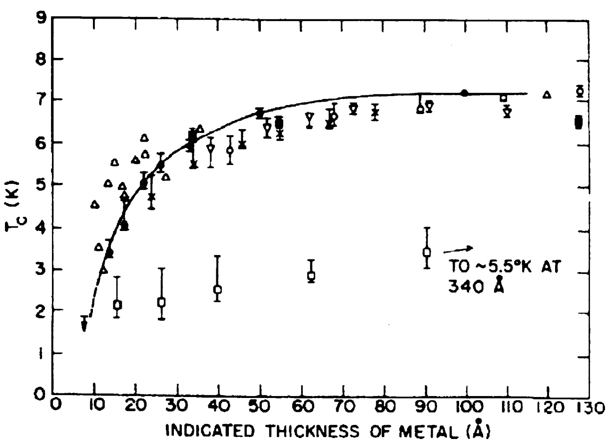


**Fig. 4.** Superconducting transition temperature, measured resistively, as a function of the thickness of metal deposited. (•) Pb deposited on PbTe at 77 K, measured in situ; (○, ×, △) Pb deposited on Te at about 7 K, measured in situ; (△) Pb deposited on PbTe at 77 K, warmed to room temperature and exposed to air before measurement; (■) In deposited on Te at about 7 K, measured in situ; (∼) In deposited on PbTe at about 7 K, measured in situ.

low-energy electron diffraction and ultimately measured down to about 4 K in this chamber. Dave Miller did a remarkable job, but there was no smoking gun for excitonic [8] superconductivity in ultrathin layers on PbTe. Some of this data is shown in Fig. 4. It shows the depression of $T_c$ for small amounts of Pb. This was what we found previously for ultrathin Pb films on amorphous Ge. There were some surprises in the data, and not all of it was followed up, but there was nothing that unambiguously pointed toward excitonic superconductivity.

During this period, I was fortunate to be in a superconductivity delegation in 1971 and we visited both applied superconductivity laboratories and some world famous research laboratories in Russia. Although I was not familiar with the applied laboratories, I, of course, knew of both the Kapitsa Institute and the Lebedev Institute by reputation. As I remember, we spent a day at the Kapitsa Institute and we spent 2 days visiting the Lebedev Institute. Ginzburg was a great host at the Lebedev and one day we had lunch with him. He asked me about what was happening on the excitonic superconductivity front. I told him that we had found nothing so far and most theorists in America felt that it would not work, and because of this, we would probably give up the search. Vitaly said, "Don't listen to theorists—Do the experiments." When I returned we were still doing our experiments and on one of Bardeen's visits I told him what Ginzburg had said, and he replied "I've been around longer than he has and I agree—do the experiments." We tried for a while longer, but although we did not understand everything that happened in the experiments, we gave up and Dave Miller wrote up his thesis.

During this time from, say, 1965 to the early 1970s, other things were also going on and there were reports of high-temperature superconductivity in the organic compound, TTF–TCNQ, and some other materials. There were suggestions of the possibility of superconducting metallic hydrogen at high pressures and high temperatures [9]. Also, extrapolations of the BCS theory seemed to indicate that temperatures near 40 K would be possible [3]. Ultimately, nothing turned out to work. By that time both the experimentalists and the theorists were writing articles about why phonons could not yield transition temperatures above the 25 K barrier and most people were trying to push up the transition temperature in the A-15 series. $Nb_3Ge$ had the highest $T_c$ of 23 K, and it was the least stable of the compounds in the Nb A-15 series. It could not be made by bulk metallurgical techniques, but it could be made by thin film deposition. There were efforts to make $Nb_3Si$, which was even more unstable than $Nb_3Ge$. As far as I know, nobody has succeeded in making $Nb_3Si$. Below is a quote by one of the best "materials physicists" of this time. In 1970, he wrote at the end of a paper,

> And these instabilities increase as their transition temperature increases until eventually the crystal won't even form in the first place. For temperatures between 22 K and 25 K these metastabilities are still sufficiently long lived to cope with. Therefore, any search for high transition temperatures must concentrate on metallic phases that should never have been formed in the first place. 25 K may be possible—not excitonic, not organic—just a relatively unstable intermetallic compound which is cubic and has an electron concentration in the range from 4.5 to 4.8 electrons per atom.

This statement was made 7 years after the first statement at the Colgate Conference about understanding everything. Obviously some people thought we knew it all. It was not only the above statement, but there were also exchanges at various meetings. This whole period would be an interesting study. Let it suffice if I say that people felt strongly about their views on this subject. I used to get calls from the West Coast; one asked how I could keep going on trying unsuccessfully to get high-temperature superconductors without getting depressed. During this time Vitaly was the champion of the possibility of high $T_c$s and I still remember his inspired talk at the Stanford meeting in 1969.

Clearly, the last 20 years have shown that this point of view of a limit on $T_c$ was incorrect, Ginzburg and Bardeen, and others, were right. High $T_c$ superconductors do exist and we can bet that the story is not over yet. Not only the quasi-two-dimensional cuprates, "high $T_c$s" are here, but also magnesium boride at 39 K. It took so long because almost everyone, including myself, was taken in by the instability arguments. Just think, if somebody put this commercial compound into a cryostat in 1970 we would have had a 39 K superconductor that would probably be in commercial production by now. As a community we basically believed all the pseudo-explanations of why high $T_c$s are not possible.

The last time I saw Vitaly was early in 1990s. He and his wife, Nina, were visiting various places in America and they stayed at Brookhaven a few days and Vitaly gave a couple of talks. During this period my wife and I got to know Ginzburg and his wife as real people. I have talked to Maurice Goldhaber about this period and we both remember Ginzburg mentioning that he had received a medal or prize for an important application of Li. Maurice pointed out in a discussion that at that time he had done some of the relevant nuclear physics with Chadwick at the Cavendish Laboratory in Cambridge in 1934. This was an example of Vitaly contributing ideas and being involved in many areas of physics. He has had many awards, including the Nobel and Wolf prizes, for work in ferroelectrics, Cherenkov radiation, the Ginzburg–Landau theory; he has had 400 publications; he has worked in cosmology, radio astronomy, waves in plasmas, liquid helium, cosmic rays, and I have still left out a lot of things.

In concluding this note, I would like to emphasize again the important role Vitaly played at a time when the dream of high $T_c$s was under attack. Of course, in physics, the truth has always come out, but we need leaders like Vitaly Ginzburg who have the courage and fortitude to realize what is possible and to encourage unpopular views.

## ACKNOWLEDGMENTS

The work described here has involved many friends and colleagues. Prof. Jack Crow, Director of the National Magnet Laboratory, was involved in the early work and his untimely death was a great loss to everyone who knew him. Prof. Ronald Parks initially realized how order parameter fluctuations could play an important role in some of the ultrathin film work and is missed along with Jack Crow. Prof. David Douglass also collaborated on some of the early work, and I owe a special debt to Otto Kammerer who was crucial in the initial stages of making the thin films. Of course, there is Dr. David Miller whose superb work is mentioned in the text. In recent years, I have relied on Prof. Zvi Ovadyahu, a wonderful experimentalist, to discuss what really goes on in these systems. On the theory side, I have already expressed my debt to Prof. Vitaly Ginzburg and Prof. John Bardeen, and I should also add Prof. Richard S. Thompson, who provided much insight at the beginning of this work. I have a special debt to Prof. Yoseph Imry, who really was one of the first theorists who understood what was going on in these disordered systems, and in recent years there were many discussions with Dr. Victor Emery, who tragically passed away some years ago. This work was supported by the U.S. Department of Energy under contract no. DE-AC02-98CH10886.

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