Gerasim (Sima) Eliashberg memorial session

It’s an honor to be here -- and thank you for giving me the opportunity to speak about Sima Eliashberg. He was one of the giants of theoretical physics of our time and one of the founding fathers of the field of superconductivity. He was also a gentle person and a truly inspiring teacher and mentor. I am truly grateful that, as a student, I was fortunate to interact with him and learn from him.

There are many ways Sima impacted how we all think about physics. Some of his ideas, such as Eliashberg’s theory of superconductivity, have left a big footprint and are widely acknowledged. His other contributions for a long time have been considered obscure, and gained prominence only very recently. One of these, which is particularly dear to my heart, is his vision that low symmetry can generate new types of electrical responses. In the past few years I found myself coming back to it often, so let me speak about it here.

It all started in the early 80’s when he published a visionary paper predicting that in the absence of inversion symmetry a macroscopic electric current can arise in response to a magnetic field in thermodynamic equilibrium. His observation was simply that, since a response of this type is allowed by symmetry, it should occur. I remember vividly him quoting Hegel’s dictum that "the rational alone is real" and saying that, despite the microscopic details being messy, symmetry will provide guidance and enable the effect.

But as Yuli and I have found while trying to sort it out, the real story wasn’t quite as simple. Despite Hegel’s wisdom something apparently didn’t work. Very frustrating. The puzzle was solved and explained to us, as I recall, by Vladimir Mineev through subtle effects due to Onsager’s reciprocity and gauge invariance. This cleared the ground and allowed us to understand exactly how Sima’s effect could occur. It was essential that the magnetic field was applied to a system driven out of equilibrium. Once the solid is driven out of equilibrium, Sima’s effect works like a charm. We published these results and moved on to do other things.

Later, charge currents generated by a magnetic field in systems lacking inversion symmetry, such as Dirac fermions with chirality imbalance, came to be known as the “chiral magnetic effect”. This effect was rediscovered by high-energy physicists, confirmed experimentally at Brookhaven National Lab, and then generalized to various condensed matter systems and recently observed in Dirac and Weyl semimetals. Nowadays it is a large and active field: perhaps half of my high-energy friends are working on it, many of them completely unaware that they are developing the ideas introduced by Sima more than 30 years ago.

Another story that is interesting to relay today are the events of 2020, when several friends and colleagues decided to nominate Sima for the Nobel prize for his work on superconductivity. As for myself, I came to think about it while being involved in preparing a 90-th birthday volume of invited articles with Annals of Physics, together with Yuli Nazarov and Victor Galitski. Usually, as many here know, birthday volumes are a tough game. But Sima’s volume was an entirely different story. We got an amazing and super enthusiastic response, getting almost 3 times as many articles as we initially hoped. Furthermore, most of the contributors were young people, making it very clear that Eliashberg theory is highly relevant for current research.

After seeing this, a number of friends (Igor Mazin and a few others who are in the audience today) got together on zoom and quickly agreed that a Nobel prize conspiracy must be formed. We reached out to a wider circle of friends, some of whom I am happy to see in this conference, got an amazing heart-warming supporting response and started laying out the case for the nomination.

No less than five Nobel Prizes have been awarded for superconductivity research, in 1913, 1972, 1973, 1987 and 2003. Isn't that enough for one field? Not at all! Eliashberg's contribution goes above and beyond solely superconductivity, it is a breakthrough that came decades ahead of its time. Indeed, while Bardeen, Cooper and Schrieffer produced one of the most elegant theories in condensed matter physics that was a conceptual breakthrough, by itself it was a bare concept, an artificial model stripped of any disturbance of the real world. It is like the Ising model for magnetism: while it captures the essential physics, it can hardly ever be applied quantitatively to any real material.

Eliashberg's work was fundamentally different in that regard. It was one of the very few theories, especially at that time, which related in a quantitative fashion, without any uncontrollable approximations, a multitude of fundamental experimental quantities (such as T_c, Delta(T), etc) with well-defined microscopic characteristics. An impressive achievement that gives Eliashberg formalism a unique position in condensed matter theory.

Published soon after the paper by Bardeen, Cooper and Schrieffer (BCS) 1957 that explained SC by a two-body attraction between electrons, Eliashberg's paper provided a first explanation of the attraction postulated by BCS. As such it was instantly recognized as a big achievement. For the first few years of its existence was viewed as an explanation of why the BCS ideas, controversial at the time, were actually OK.

I quote a friend, who made a remarkable comparison. He said that Eliashberg's theory did to BCS in the 1960's what the new quantum theory (by Schroedinger and Dirac) did in the 1920's to the old quantum theory (by Bohr and Sommerfeld). Preserving all the ideas and spirit the new quantum theory had provided an appropriate language and a structure. This is exactly what Gorkov and Eliashberg papers did for superconductivity. Indeed, if you read John Bardeeen's Nobel prize acceptance lecture you'll see that in a good half of it he talks about Eliashberg's theory.

Another friend pointed out that for many decades the superconducting critical temperature was considered to be one of the most difficult characteristics to predict theoretically, because of its exponential dependence on the material parameters. In fact, it was often believed that while computational predictions of other material properties were just around the corner, superconductivity theory would never advance to this stage.

The irony is that what happened in recent years was exactly the opposite. Nowadays, Eliashberg's theory is a cornerstone of computational materials design, a wide effort that culminated recently in theoretical predictions and experimental confirmation of the decades-long quest for room-temperature superconductors. Predicted from Eliashberg's theory it was observed in SH_3 under high pressure at about -70 ^{o}C, a success soon followed by other superhydrates with critical temperatures as high as, arguably, the actual room temperature. Thanks to the Migdal theorem, this may be the one and only example of a condensed matter theory of such breadth that does not use any uncontrollable approximation, is fully solvable, and quantitatively accurate for real materials.

A number of really strong nominations, based on these points and a few others, have been submitted in December 2020 and January 2021, before the sad news came of Sima's passing. Participating in this effort was an unforgettable heart-warming experience for everyone involved, and I am happy to have this opportunity to relay some of it to you.

By now I am probably running out of time. Let me end this with a quote. As other speakers mentioned, Sima's career spanned some exciting but also difficult periods of his country's history. As a wise man said, we don't decide on what times we live in, "all we have to decide is what to do with the time that is given to us." Which is what Sima did flawlessly. He is remembered fondly for the multiple ways he shaped the academic community, for his visionary work, his teaching, his mentorship and unique attitude towards physics.

Leonid Levitov
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