The Long-Reaching Influence of Arthur von Hippel: Interdisciplinarity and Semiconductors

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Abstract

While Arthur von Hippel did not work directly in semiconductors, his interdisciplinary style introduced a way of doing science in universities that was applied to semiconductors and now is applicable to nanotechnology as well as many other technologically important areas. This article covers the major developments and players in semiconductors from their early discovery and understanding, in the context of von Hippel's seminal development of the interdisciplinary approach to research and its impact on the field.

Keywords: *Arthur von Hippel, semiconductors.*

While the connection between Arthur von Hippel and the field of semiconductors is at best tenuous, what does seem to be true is that he provided an early model for interdisciplinary research that has proved relevant for technologies far beyond his own scientific endeavors. Such mixtures of training in laboratories is essential if universities are to partake in the most exciting new science and technology, and if students are to be trained for the complex technological world of the present and the probably even more complex world of the future.

Even though von Hippel did not contribute directly to the field of semiconductors, that field still owes him a debt of gratitude for his foresight in seeing the need for academic laboratories that combine the skills of different disciplines. Arthur von Hippel's seminal development of the interdisciplinary approach has had a lasting impact on the very practice of scientific research itself. This article will demonstrate that impact by examining the major developments and players in the field of semiconductors from its early discovery and understanding.

There was a reason that Arthur von Hippel was not in semiconductors. He came from a German tradition that at the time largely regarded semiconductors as intractable to scientific inquiry. Witness the famous comment of Wolfgang Pauli in 1931: "One shouldn't work on semiconductors, that is a filthy mess; who knows whether they really exist?"[1] The heaviest emphasis was on defects in solids, led by Robert W. Pohl. It was in this atmosphere that von Hippel began his work and made his early contributions.

The use of the word "semiconductor" dates back at least to 1826 in a book by Ivan A. Dvigubsky, in which he mentions that Cavendish had observed that water conducts, but much less than metals. There were contributions by Michael Faraday (decrease in conductivity with decreasing temperature, 1833), Ferdinand Braun (the cat's whisker diode, 1874–1877), A. Schuster (rectification of copper-copper oxide, 1874), Willoughby Smith (photoconductivity in selenium, 1873), and Edwin H. Hall (the Hall effect, 1878). It took roughly 50 years before practical rectifiers were developed. By 1900, "semiconductor," or *halbleiter,* was in common usage as a term to describe materials with intermediate conductivities. By 1922, Grüneisen had defined semiconductors by the property of having a minimum in the conductivity.

There is a fine review of the history of the field in a paper by Ernest Braun in *Out of the Crystal Maze*, edited by L. Hoddeson, E. Braun, J. Teichmann, and S. Weart,[1] as well as in books by Frederick Seitz and Norman G. Einspruch[2] and others, including memoirs by some of the participants.

Of course, there were those in the Germany of von Hippel's pre-emigration days who did make significant contributions to the field, notably Johann Königsberger, Karl Baedeker, and especially Walter Schottky. In England, such distinguished scientists as Neville Mott and A.H. Wilson made important theoretical contributions. When Arthur von Hippel came to the United States in 1936 to join the faculty of MIT, there was practically no scientific work on semiconductors in the U.S. The indices of the *Physical Review* for 1935 and 1936 show only four references under "photovoltaic." "Semiconductors" does not appear as a topic until 1940. Work during World War II was largely classified, so it was not until 1946 that a large number of papers on semiconductors began to appear. A feeling for the field in 1940 is shown by the roughly 25 pages that Seitz gives it in his famous book (of 680 pages), *Modern Theory of Solids*.[3] Most of the examples are on metal oxides. While tellurium is listed, selenium is not, although it had been used commercially for more than 10 years. The most important high-voltage material used in rectifiers, copper oxide, is mentioned but not discussed. Nor is germanium. Silicon, the engine of the semiconductor industry, is the only major semiconductor still in large use that Seitz mentioned—and then only to state that it was impure. Indeed, there had been an active debate as to whether silicon was a metal or a semiconductor.

The Second World War stimulated interest in semiconductors, as it did in many areas of electronics. The reason was that silicon point-contact diodes (first observed by G.W. Pickard) proved to be of vital importance in Allied radar systems.

There was intensive work on this topic at the Radiation Laboratory at MIT, close to von Hippel's laboratory. There was also work at Bell Telephone Laboratories and, of course, in England earlier. Early German work was aborted.

Much of the U.S. effort depended on work by C. Marcus Olson's group at DuPont on purifying silicon. At Purdue, Karl Lark-Horowitz and a large group were studying an alternative—germanium. He and his group, which included Paul Bray, Vivian Johnson, Louise Roth, W.E. Taylor, D. Navon, H.Y. Fan, and others, were instrumental in developing germanium technology and opening the scientific study of that material. The development of sensitive, reproducible detector diodes was essential to radar and is detailed in one of the Rad Lab series books, *Crystal Rectifiers,* by Henry C. Torrey and Charles A. Whitmer.[4]

As for so many other sciences, technologies, and institutions, the world was never the same for semiconductors after the war ended in 1945. Even so, universities were slow to become involved in semiconductor work in the next five years. Lark-Horowitz's lab stood out as an exception. Park Miller did some key experiments at the University of Pennsylvania that showed that the heights of energy barriers between different metals and semiconductors (Schottky barriers) were independent of their metal work functions. This led John Bardeen to the conclusion that the Fermi levels of the metals were "pinned" at surface states and started a 50-year controversy over proper models, largely fueled by experimental artifacts. At the University of Rochester, Esther Conwell wrote her MS thesis with Weisskopf on impurity scattering. Work in semiconductors at Columbia was confined to adjunct professors at the IBM Watson Laboratory (Robert Gunther-Mohr, Seymour Koenig, and Peter Price), and even they did not really get started until the mid-1950s.

The major efforts were in industry and, in particular, at Bell Telephone Laboratories in Manhattan. There, William Shockley had been given orders to find a solid-state switch or amplifier to replace tubes. He assembled a brilliant team of experimentalists (Walter Brattain, Gerald L. Pearson, J.R. Haynes, and others), theorists (John Bardeen, Conyers Herring), and "materials scientists," as they are now called (then, they were metallurgists, ceramists, or chemists). Outstanding were William Pfann and Gordon Teal. Pfann invented zone-refining, allowing pure germanium to be made; Teal developed the Czochralski method to grow single crystals and to make grown junction transistors. This innovative materials work provided Bell with a huge advantage over its chief rivals, Purdue University and a French group.

Arthur von Hippel's seminal development of the interdisciplinary approach has had a lasting impact on the very practice of scientific research itself.

At first, an attempt was made to fabricate germanium field-effect transistors. The idea of the field-effect transistor was old, going back in thin metal films to about 1900, when Charles F. Mott, the father of Neville Mott and a student of J.J. Thomson at Cambridge, using thin metal films, failed to see an effect and did not finish a doctorate at Cambridge. Attempts at creating a field-effect transistor in semiconductors date to their invention by J.E. Lilienfeld, an immigrant to the United States, in 1933. (There is also a British patent of O. Heil in 1935.) There had been no reduction to practice. The idea of the field-effect transistor was reinvented for germanium by Shockley. The original insulator in the germanium devices was Mylar. Because of Bardeen's surface states, Bell's germanium field-effect transistors did not work well, either. It was in 1947, while studying the surface states that limited transconductance, that Brattain and Bardeen discovered the point-contact bipolar transistor, which Bardeen immediately explained as being due to minority carrier injection. Shortly afterward, Shockley invented the junction transistor. The floodgates opened. Semiconductors suddenly became the hottest new technology.

Soon, every electronic tube company was in the chase. General Electric, RCA, Raytheon, Philco, Sperry, Westinghouse, and Sylvania had major efforts, partly financed by government money—and that mostly from the military. They were joined by new companies, or companies new to making electronic components, such as Transitron, Texas Instruments, IBM, Fairchild Semiconductors, Pacific Semiconductors, Hughes, Sprague, and many others. It was a time comparable to the high-tech boom of the late 1990s. Along the Route 128 corridor around Boston, new semiconductor companies seemed to pop out of the ground like mushrooms in the spring. The government labs became heavily involved, notably the Naval Research Laboratory, Fort Monmouth, and the new Lincoln Laboratory at MIT, organized in the early 1950s to develop the DEW (distant early warning) line of radars. Most transistor work at that time was on alloy transistors. This technology was largely developed at Bell Labs, RCA, and GE, although it is unclear to me who was first. An insight into work of this era may be had by consulting a special issue of the *Proceedings of the IRE* published in 1952.[5] Of some 60 papers, only three were from universities, including two from Purdue.

Still, there was relatively little effort in the universities. It built slowly. The cyclotron mass was measured by Kip and Kittel at UC–Berkeley in the early 1950s. In the mid-1950s, Luttinger and Kohn did their work on hydrogenic levels at Carnegie Tech. Work started at Harvard under Harvey Brooks and William Paul, beginning in 1953. Park Miller continued with an effort at the University of Pennsylvania; later, Eli Burstein joined Penn from the Naval Research Laboratory. There was early work at the University of Illinois, especially after Bardeen and Paul Handler arrived. At the University of Chicago, Andy Lawson and Ed Adams started experimental and theoretical work, respectively. There was work at Stanford, also led by industrial lab veterans. And, of course, the Lark-Horowitz group continued its strong effort at Purdue. Part of the slowness may have been attributable to the inertia inevitable in universities because of the hiring process and often because of innate conservatism. Old professors sometimes tend to continue at what they know and what made them famous (or at least famous within a narrow circle, if not in a league with baseball players or rock stars). Often, new blood was slow in coming, possibly because industrial laboratories were competing for the limited trained talent. Many physicists left other fields, especially atomic physics, to become semiconductor scientists. Many were hired, not by physics departments, but rather by electrical engineering departments.

There are other reasons for the slow entry of semiconductors into universities. Semiconductor research is intimately tied to materials and technology. That implies a cross-disciplinary effort. With very few exceptions, the universities had little experience with such efforts, although many on the faculty had served in wartime laboratories. Two major exceptions in the 1950–1955 time frame were the Lark-Horowitz group at Purdue and Arthur von Hippel's Laboratory for Insulation Research at MIT. To maintain a broad semiconductor program, a significant infrastructure is also required; a variety of analytic tools are often too expensive and require too much skill and maintenance for a one-professor effort. In the 1950s, most universities emphasized individual effort on the part of their professors (and many recalcitrant universities still do). Furthermore, the principal purpose of a university was still supposed to be education. Crossover theses didn't quite fit into neat departmental packages.

There were a lot of exceptions, of course. In the 1960s, some universities lashed together cross-departmental laboratories. Examples were the University of Illinois, Stanford, and MIT. Arthur von Hippel played a major role in MIT's efforts to create a cross-disciplinary materials laboratory. He suggested in 1956 that a department, bridging departments, devote itself to the study of "molecular engineering."

Semiconductor science students in the universities which eschewed cooperative laboratories were greatly limited in the subjects that they could choose to study. Typically, they would have to get samples from friends in industry. Often, they would look into areas at the edges of semiconductor research.

That does not mean that major contributions did not come from universities. For instance, a large part of the early work on amorphous semiconductors was done at various university laboratories, both in the United States and abroad. In fact, the first amorphous silicon thin-film transistors were made at the University of Dundee in Scotland; they are now one of the bases of liquid-crystal displays. The work was possible because the amorphous silicon technology was relatively simple. High-pressure studies were another area where universities led, notably in William Paul's group at Harvard and Harry Drickamer's at Illinois. Early ultrahigh-vacuum surface studies of semiconductor surfaces were strongly represented in universities, notably at Brown University. Aldert van der Ziel turned his attention to noise in semiconductor devices, first at the University of British Columbia and then at the University of Minnesota, and the explanation for $1/f$ noise (where f is frequency) came from A.L. McWhorter at MIT and Lincoln Lab.

Cross-disciplinary groups were much more natural to industrial laboratories than to any other research institution. Most industrial laboratories had both discipline-oriented and task-oriented groups. The task-oriented groups usually included a spectrum of scientists and engineers. Even the discipline-oriented groups were expected to consult and to pitch in when needed. This was certainly true at the time of my experience, starting in 1952; it was apparently true at Bell Labs during the period of the invention of the transistor. One may be sure that all the early great industrial labs—General Electric, Westinghouse, and RCA, as well as Bell—combined disciplines in their efforts. Cooperation among disciplines was far more a part of the industrial (and also the government) laboratory culture than that of universities at the time. Arthur von Hippel's efforts at MIT seem to have been a very rare early exception.

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One of von Hippel's major fields of interest was insulators. Thin-film insulators are vital to the semiconductor industry. Silicon dioxide has been an essential material, especially in metal oxide field-effect transistors (MOSFETs), for about 45 years. It was the material that served as a passivating layer and as a diffusion mask in planar technology. It is native to silicon surfaces and can easily be grown thermally or laid down by chemical vapor deposition (CVD).

Planar technology has allowed the development of large-scale integration. It is the insulator that has made the MOSFET the dominant electronic device, because of its high barrier height (which reduces carrier injection), its low defect density, and its low surface-state density when grown or deposited on silicon and annealed in hydrogen. It has only recently reached its limits as the films have shrunk to the order of 1 nm in thickness. There is no obvious replacement for it. The best source on the history of the MOSFET is *To the Digital Age*.[6] This is one of the few books that treat the history of semiconductors after 1965.

Thin-film silicon dioxide technology was developed in many laboratories, notably at Bell in the mid to late 1950s and at Fairchild, IBM, RCA, and other industrial laboratories in the 1960s. There were a few academic contributions, notably from Sah at Illinois and the groups at Lehigh. The von Hippel laboratory was not involved in the development of thin-film silicon dioxide, although early work on defects had been of interest. Partly this is because silicon dioxide has a relatively low dielectric constant and is not of high interest for capacitors.

Of more relevance is work on various transition metal oxides with high dielectric constants. There has been active work in the silicon industry on such insulators as hafnium oxide as a substitute for silicon dioxide, because the silicon oxide films are now so thin that tunneling is a problem. After early work in the 1970s—and recently, more than 10 years of intensive effort in many laboratories—there is still no assurance that there is any viable substitute for silicon dioxide, at least not one that will be useful for more than a very few generations. By about 1990, thin-film silicon dioxide became probably the most studied and best understood insulator with adequate theories to explain breakdown and trapping. In this area, the experimental work of D.J. DiMaria is preeminent.

The other important insulators for semiconductors are polymers such as polyimides, which are used to insulate higher levels of wiring. They have even lower dielectric constants than silicon dioxide and so could not have been of much interest in capacitors. Silicon nitride also has played a role, although many people contributed, starting in the late 1950s.

Ternary compounds and III–V semiconductors became important in the 1950s and have been principally used for light-emitting diodes and injection lasers. Again, their invention and development required an interdisciplinary effort—materials scientists to provide the materials, technologists to build the structures, and physicists to study them (and then to get credit for inventing them). Whether acknowledged or not, materials work was the basis of both electronics technology and most experimental (and therefore theoretical) semiconductor physics.

Few ideas were studied at only one laboratory. The GaAs injection laser was invented almost simultaneously at GE and IBM, with Lincoln Lab only a short time behind, in groups led by Robert Hall, Marshall Nathan, and Robert Rediker, respectively. The history of heterojunctions is similarly murky. Probably, Herbert Kroemer was the first to discuss them, but how many know who first made and measured them? (My guess is John Marinace and Richard Anderson in Ge-GaAs.) They have become useful mainly for light-emission and some elegant physics. Leo

Esaki, Leroy Chang, Webster Howard, and Ray Tsu started the first work intended to build a wholly artificial structure by using molecular-beam epitaxy—the layered superlattice. The MBE technology had been developed by John Arthur and Al Cho at Bell. Since then, artificially engineered structures have multiplied.

The silicon industry has, of course, continued to rely on an interdisciplinary spectrum of engineers, physicists, and materials scientists to expand the size of the industry, mostly by shrinking the size of components. It is not always easy to assign names to some of the developments. The bipolar transistor, which started it all, was invented by Brattain and Bardeen; the junction transistor was invented by Shockley. The MOSFET was invented by Dawon Kahng at Bell. Integrated circuits are usually attributed to Jack Kilby at Texas Instruments and Robert Noyce at Fairchild, but early work was done by J. Torkel Wallmark at RCA; in addition, Kurt Lehovec at Sprague made major contributions. CMOS logic circuits were invented by Frank Wanlass at Fairchild, who has received too little attention from prize committees. Complementary bipolar circuits were studied earlier at RCA. Dynamic random-access memory was invented by Bob Dennard at IBM. Silicon gates were developed first at Bell Labs.

Planar technology is usually attributed to Jean Hoerni at Fairchild, but there was similar work reported by many other industrial labs at the same time in nonconfidential government contract reports from other labs, including Pacific Semiconductors and Raytheon (planar technology used silicon oxide for diffusion masking and passivation). This supports a theme of Bassett's,[6] that not only were the laboratories multidisciplinary, but the whole community interacted and labs fed on each other. There was a surprisingly open atmosphere, especially at the annual Device Research Conferences, and even more at cocktail parties and dinners.

With this kind of interdisciplinary collaboration, the semiconductor industry has grown to a height that few would have anticipated in 1950. Even if, as some believe, the exponential improvements have ended (the end of Moore's law), there are still important improvements being made and interesting challenges to explore. As ever, the field requires scientists and engineers of vastly different skills and training to attack them.

Despite these challenges, the interdisciplinary model provided by Arthur von Hippel has become the rule rather than the exception in scientific research, and the climate for interdisciplinarity seems more amenable now than ever.

Conclusion

The model of interdisciplinary laboratories in universities, pioneered by Arthur von Hippel, is relevant as technologies become more complex. Large amounts of money are now being spent on so-called nanotechnologies. At present, most of the commercially useful things on the "brag list" for these technologies are old (zeolites) or have resulted from natural extensions of silicon technology. Many of the more interesting nanotechnology problems involve not just physics, chemistry, and materials science, but also biology and bioengineering. It is clear that progress will most likely come by following the model of Arthur von Hippel and will fuse together these various disciplines, sometimes in the same person, but usually in cross-disciplinary laboratories. Nanotechnology centers may well evolve into a new discipline, just as the laboratories combining metallurgy, ceramics, physics, and chemistry led to materials science as an academic discipline. That led to the Materials Research Society and to this publication.

The pressures in some universities against such a fusion are obvious. The most important is the sanctity of department lines. There are, of course, real dangers in students working in cross-disciplinary laboratories. The major one is that their education and training will be shallow and that they will become jacks of all trades and masters of none. Another danger is that supervision by professors will be diffuse. Many universities are still seeking ways to break down those barriers to creating viable interdisciplinary laboratories; others seem to have succeeded. Despite these challenges, the interdisciplinary model provided by Arthur von Hippel has become the rule rather than the exception in scientific research, and the climate for interdisciplinarity seems more amenable now than ever.

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Finally, I would also like to make a disclaimer. This paper is not totally authoritative nor in any way the work of a professional historian. Where I have had to rely on an increasingly less than perfect memory alone, I may be less than reliable. I am sure that contemporaries of mine who worked in the same time frame might remember or evaluate the same period differently from what I have. I am certain that they might emphasize other work and workers. To any that I have slighted, *mea culpa.*

References

1. L. Hoddeson, E. Braun, J. Teichmann, and S. Weart, eds., *Out of the Crystal Maze: Chapters from The History of Solid State Physics* (Oxford University Press, New York, 1992) p. 121.
2. F. Seitz and N.G. Einspruch, *Electronic Genie: The Tangled History of Silicon* (University of Illinois Press, Champaign, Ill., 1998).
3. F. Seitz, *Modern Theory of Solids* (McGraw-Hill, New York, 1940).
4. H.C. Torrey and C.A. Whitmer, *Crystal Rectifiers* (McGraw-Hill, New York, 1948).
5. *Proc. IRE* **40** (11) (Institute of Radio Engineers, 1952).
6. R.K. Bassett, *To the Digital Age: Research Labs, Startup Companies, and the Rise of MOS Technology* (Johns Hopkins University Press, Baltimore, Md., 2002). ☐

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