The Beginning of the Semiconductor World

General Electric's Role

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The semiconductor world started in the 19th century with the discovery of the diode. There was little or no use for the device until the advent of radio. The diode became very important but there was little understanding of the physics of the device. Wilson's "Theory of Metals" in 1934 gave a scientific basis for semiconductor theory. World War II and the advent of radar created a new demand for semiconductor diodes.

In 1942 Harper North, at General Electric's Research Laboratory, was searching for better and more reliable radar diodes. He developed a much more reliable and reproducible diode. Both germanium and silicon had been studied but germanium became the preferred material due to difficulties with silicon. Others, for example, Sylvania, Raytheon and Bell also had developed usable diodes.

The work Harper North had done lead to the creation of a germanium diode department at GE. By 1946 this was definitely a viable business.

GE opened Electronics Park in Syracuse N.Y. in January – February of 1948. Electronics Laboratory moved in February and the author reported for work in March. The laboratory was quite small with about sixteen scientific investigators. The author was hired by J.P. Jordan and the two of us worked on nuclear instruments. About August 1948 a chemist Dr. Frederich

Pingert was added to the group. In late 1948 the germanium diode people came to us and asked for help. Eagle Picher Co. was the only supplier of germanium (at a very high price) and you had to take what they offered. This was supplied in ingots about 3 ½ inches long by about ¾ inch diameter. There was a very large deviance in diode quality and yield from one ingot to another. Fred Pingert suggested that we could purify the material by progressive freezing. This is of course the Bridgeman method of crystal growth.

Fred was a brilliant chemist but was totally inept when it came to things mechanical. As a consequence it fell to the author to design and build the furnace. Fortunately GE was one of the world's premier suppliers of ultra pure carbon so we had an excellent boat material. The furnace was completed and was a rousing success. By cropping the last 20% of the ingot and using the first 80% an immense improvement in yield and manufacturability of diodes was achieved. This was particularly important as GE's diode business was growing by leaps and bounds.

About this time a physicist Raymond Szupillo was added to the group with the responsibility of maintaining a supply of good germanium for the group and for the factory.

About the end of 1949 R.N. Hall and W. Crawford Dunlap of GE's Research Laboratory came up with the alloy/diffused junction using antimony and indium for contacts. (The Hall-Dunlap patent). We at Electronics Laboratory picked this up immediately and started to make diodes. It very quickly became apparent that we were going to need single crystal germanium for a successful product. The author was given this responsibility and after one abortive effort we had a working crystal furnace in about five weeks. Afterwards we found that RN Hall at the research laboratory and Teal & Little at Bell Labs had also grown single crystals. Apparently these three events occurred in about a two week time slot and we never did know who was first. That crystal pulling furnace was duplicated for the factory and for several years a number of identical furnaces supplied all of their germanium.

Unfortunately Dr Pringert died about this time (early 1950).

I would like to digress and relate an anecdote about WRG Baker, GE vice president, and head of the Electronics Division. Management of Electronics Laboratory came to JP Jordan and the author and told us that we had to stop playing around with this physics nonsense and start working on good electronics (circuitry) or we would be discharged. Paul Jordan went to Baker and stated the problem. Dr Baker solved the problem in a typical manner, he wrote on a 3x5 note sheet; "you have $10,000 WRGB". Needless to say there was no one at GE who had the nerve to challenge that. Dr. Baker did that twice more for a total of $30,000 and by that time the semiconductor world was off and running. I must say that I never met anyone who could see the future better than WRG Baker.

To return to the diode world, GE was making about 100,000 diodes per day by the end of 1950. In hindsight it is now obvious that GE's whisker diode was really a fused junction device. This made the diode reproducible and rugged. The market responded and grew.

The alloy junction device was progressing rapidly but tooling, fixtures, packaging and process all had to be developed. For example the now familiar "top hat" design for diode packaging took six months because everything had to be developed and built. The diode itself was in good shape and the potential demand was ferocious. As a result an interim package was developed consisting of two copper cups, one of which fit inside the other. The diode was placed inside and a paper washer was used to insulate one cup from the other. Then a rubber O ring was placed between the cups and the device was crimped with the O ring serving as a seal. The market was so demanding that about 50,000 of these devices were made and sold at about $5.00 each. At this point the top hat design came on line and the famous IN93 was marketed and GE had moved its manufacturing to a new plant in Clyde N.Y.

To return to the logistics of this new semiconductor world – as stated before everything had to be invented, designed or researched before you could move forward. Even such simple things as a diamond saw suitable for our purposes had to be designed and built. At that time purity levels of chemical reagents were only specified to parts per million and that was not good enough. As a consequence you searched for reagents that were good enough and then hoped that the manufacturer held that quality. Good clean water was a problem and in the laboratory was often solved with distilled water. This was not possible in the factory. As a consequence after considerable work our Clyde N.Y. plant had 10^{13} OHM-CM water piped to every work station by 1952. At that station the water went into a triple cascade where the parts to be washed were first introduced into the bottom tray and then moved up into the cleaner water above. All of these things lead to better process control, higher yields and a better product.

Another example: about 70% of the germanium ended up as saw dust in the dicing process. There was only one supplier, Eagle Picher Corp. and they only offered about $1.00 per pound for germanium scrap. By the end of 1950 the factory had several thousand pounds of germanium saw dust and scrap. They asked us in the laboratory what we could do to help. We hired Vernon Ozarow a graduate student from Penn State who had run out of money. We arranged with Penn State that he could do his thesis research in our laboratory. His assignment was to develop the necessary processes for purifying germanium. Remember that while Eagle Picher was doing this they kept it a secret and there was no literature on the subject. Ozarow was successful in about six months and published his research. The price of scrap germanium immediately rose to about $100.00 per pound. And Dr. Ozarow got his PhD.

With this we want to return to 1948 and Bell's announcement of Bardeen, Brattain and Shockley's invention of the transistor. Many had tried to make a gain device of variations of the diode, but the transistor was real and these scientists really understood what they had done. Immediately several companies started to make transistors, certainly this included GE, Raytheon, Sylvania, Bell and RCA. The whisker transistor was a fragile, uncertain device with rather poor transfer characteristics. Raytheon (and certainly Bell) tried hard to exploit the device. Raytheon was making hearing aids and had developed ultra miniaturized tubes to do this. The whisker transistor did not replace them. It is certain that some people tried to make products, but the author is not aware of any successful commercial product.

With all of the above, by early 1950 we knew we could make a functional transistor but there was no economic incentive to do so. On the contrary the burgeoning diode manufacturing and the constant need for infra-structure made us put the subject aside for the time being. However in late 1950 we hired John Saby with the understanding that his assignment would be to make a junction transistor. He was ill and did not report until February 16, 1951. By March 11th, 1951 he had successfully made a transistor using the existing infra-structure. Interestingly the principle reaction was "so what – it'll never replace the tube". However Richard Shea the leader of a circuits group in Electronics Laboratory was immediately interested and soon he, his group and Saby had worked out the characteristics of the device. In May of 1951 the War Department had taken a delegation of electronics executives to Korea to see the problems there. Upon their return Bell executives asked some of the others to come to Bell Labs where they were shown the NPN grown junction transistor. JP Jordan, JS Saby and the author were invited to a meeting in Dr Baker's office to hear what he had to say. After reporting the Korean visit he told those present about the Bell visit. The Bell announcement was not secret but it was not being publicized at that time. When he got done Saby handed the data sheets and a PNP transistor to him. I do not think I have ever seen a happier man, his $30,000 was justified and we could do no wrong.

By Spring of 1951 we wanted to announce the transistor work. This was done at a joint AIEE and IRE conference on Electron Devices. (That meant tubes in those days). This was held at the University of New Hampshire at Durham in June 1951. The room was tiny and hot and the crowd was large. Saby presented his paper on the PNP junction transistor and back to back (order Unknown) Morgan Sparks and Bill Pietenpol presented their paper on the NPN grown junction transistor.

Oddly enough, except in the semiconductor groups and Richard Shea's circuits group the transistor was greeted with yawns and denial. One of the laboratorie's tube specialists stated flat out that the transistor would never replace the tube.

In late 1950 and 1951 we added a number of people to our group. Donald Cronemayer, a physicist, helped establish us as authorities in parametric definition of semiconductors. David Jillson, a metallurgist, added needed skills and was the author's co-worker for many years. Earle Steel, Jan Engel and Norman Brown all added physics expertise. I. Arnold Lesk also a physicist was the inventor of a number of devices such as the field effect transistor. Fred Keihn was a crystallographer. Dorothy Markham was a truly exceptional laboratory assistant. Shea's group also expanded at this time and everybody was working on transistor characterization and applications.

After the 1951 Saby paper Bell Laboratories suggested an exchange of visits. This took place during the summer of 1951. The Bell delegation was about 8 people and included Jack Morton, Bill Pietenpol, Morgan Sparks, Bill Pfann and others that I cannot remember. Our laboratory was still small and it was sort of funny to see this large crowd.

A short time later a group from GE visited Bell. This included JP Jordan, the author, JS Saby, W Crawford Dunlap, RN Hall and several others. The members from Electronics Laboratories were awed by the munificence of Bell's facilities. Neither group had much to learn because technically we were about at par even though we were on slightly different paths.

In 1951 and ’52 we were told to teach some other companies our technology. The first of these was an affiliate BTH (British Thompson Houston). This went very well and the association lasted for several years. We were also told to teach CFTH (Compagnie Francaise Thompson Houston) another affiliate. We did so and they were in business but our association with them was short lived.

An odd exchange occurred when management told us to teach Raytheon the art. This was an exchange of information. Management valued our semiconductor so poorly that they exchanged it for some radar data. It is interesting that when Raytheon was asked about “their invention of the transistor” (about 1998) they responded “we didn’t invent it, GE taught us the art”.

About this same time management told us to teach RCA what we knew. We never knew why but presumed that it was a derivative of the RCA compact of the 1920’s. Interestingly RCA then filed a patent on the PNP transistor and started a legal battle. RCA lost and GE’s patent (Saby) was upheld.

In 1952 Bell had the great sale of semiconductor technology. For $25,000 (a lot of money) a company could send a delegation (of 4?) to a weeks teach-in at Murray Hill and Allentown. The company also got automatic licenses to Bells’ patents. We had no reason to go but at Bell’s invitation we went (at no charge). Several hundred people showed up.

An interesting sidelight to the above was a rather insistent invitation from RCA asking that GE send a delegation to RCA labs the week after Bell’s affair. It was decided that the author and

Crawford Dunlap would go for two days. It was a strained two days because they had nothing to show.

While all of the above was going on the circuits group under Richard Shea had grown and was developing the theory of transistor circuitry. In 1953 they published a book "Principles of Transistor Circuits" Richard Shea, Editor. This was the first book of its type and remained in print for 20 years. It was translated into many languages and was distributed worldwide. It was the longest (or nearly longest) printing of a text book by John Wiley and Sons. When it went out of print in 1973 Wiley brought all of the authors to New York for a party. This was just the start of GE books. Shea edited another more comprehensive book on circuitry in 1955 and then the manufacturing professionals started publishing.

The semiconductor world was off and running. GE's manufacturing grew until it involved multiple plants and thousands of people. The Semiconductor Laboratory of Electronics Lab prospered. We started growing silicon crystals and introduced the technology into the manufacturing system. Devices were invented and problems were solved. D.C. Jillson and the author grew large floating zone crystals by magnetically suspending the molten zone. Another first for GE. The laboratory had a unique identity on into the early 1960's. In 1962 Nick Holonyak made the first visible light injection laser (and the consequent light emitting diode). This was a fitting demarcation for the end of the lab. There had been fourteen years of wonderful events which were unique for most of the participants. Many of the people from the lab and Shea's lab went on to live illustrious lives.

The author has talked about a specific group that was at the focal point of the beginning of the semiconductor world. In doing so he has not meant to slight the outstanding work of others such as RN Hall and others at the GE Research Laboratories. Likewise he would like to bring the reader's attention to the wonderful work of the people in the manufacturing sector. Such names as Ray York, Russ Lyons, Bill Gutzwillerh, Finis Gentry and many others come to mind.

It's worth asking one more question. Why did Bell receive all the accolades and credit while GE received very little? This apparently goes back to the 1920's RCA compact. Bell, GE, Westinghouse (and one other company) decided to pool their electronic skills in RCA. The courts abolished this almost immediately but did not destroy the compact itself. Bell's attitude appeared to be that for the most part they were going to ignore the compact and RCA. GE's attitude as conveyed to us was that we'll hold our cards close to our vest and what RCA doesn't know they can't claim. The result was that Bell had a publicity department that beat the drum whenever it got the opportunity. GE on the other hand severely limited what we could patent or publish.