Erin used this (mostly)

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Recorded on tape 21 January 1964.

Ida Rose: I am going to try to get Tracy Hall to tell the story of his man-made diamonds. He has the flue today and is home from work. That is why his voice will sound hoarse.

Tracy: Ida Rose's statement was made yesterday. Today's the 21st, and I think she has cornered me and I might have to do what she wants me to do.

In October of 1948, I accepted a position as research associate at the General Electric Company in Schenectady, New York. During September I had considered several job opportunities , had traveled to Rochester, New York, to talk to the Eastman Kodak people, had talked to Hercules Powder Company, Allegheny Ballistics lab in Cumberland Maryland, and hd talked with DuPont people, in Virginia, New Jersey and Wilmington and had also talked to people in the Bakelite, Boundbrook, New Jersey.

Peculiar thing - all these people wanted to give me a job, but General Electric didn't. As a matter of fact when I had written to General Electric, they told me there was no point in coming to Schenectady for an interview. But I wrote to them and told them I was passing by anyway and would it be all right if I stopped off. They reluctantly said O>K>, so I did stop off and I apparently didn;'t make too good an impression with the man who headed the Chemistry Department, Abraham Lincoln Marshall, and with other officials, but did make a real good impression with a fellow by the name of William Cass who at that time seemed to be up and coming in the Chemistry Department and was assisting in a lot of their recruiting activities. Case told me at the time he didn't think that the management was giving me a fair shake but that he was sure going to try to convince them that they ought to hire me.

I had wanted to work for General Electric all my life. I can remember when I was a grade school student in approximately the fourth grade in the Marriott School in Weber County, Utah, my teacher at that time, Miss Beulah Stallings, who taught grades three, four, and five all in the same classroom, had asked\ the students what they wanted to be when they grew up. I told her I wanted to be an Electrical Engineer and work for General Electric. Well, it turned out that although I dabbled in areas related to Electrical Engineering, and actually spent time as a radar officer in the navy, having had training at Bowdoin College, MIT, Harvard, and other places, in electronics. It turned out that when I went back to school after World War II I took Chemistry and obtained a PhD in that field.

Well, after this interviewing trip, I returned back to salt Lake City and waited for offers. I pretty soon had offers from all the companies except General Electric. I kept waiting and hoping to hear from G.E. Finally I wrote to them or called them and they said they were still considering things. Finally, a phone call to the Chemistry Department came to me from Schenectady, New York, at the time I happened to be in the Library, and the call somehow was transferred over there. Glen Giddings, who at that time was the recruiter for the research Lab of General Electric Company was on the phone and told me that they were offering me a job at $6200.00 a year. I later learned that my counterparts who were being hired at this same time who were graduating from Princeton, Harvard, MIT, and Yale were being offered $5700.00 a year. I attribute the differential to the fact that I was a few years older, had served in the war, and also, had received a tremendously good recommendation from Henry Eyring. I had been interested in Inorganic systems having done part of my thesis work on the isomers of Chromic Salts, and wanted to continue to pursue some work in this field. General Electric, in spite of the fact that they had said a fellow could do anything that he wanted, on coming to the research laboratory, soon was trying to push me into areas of practical interest to the company. I contended that there might be some interest in inorganic polymers of chromious or aluminum that we might work on. They definitely weren't interested in having me pursue this sort of thing. It was interesting, however, that as years went by there was a point where they asked me if I wouldn't['t like to work in the inorganic polymer area, but I had become interested in other things and, of course, at that point did not want to.

The first work of significance that I did in the Research Laboratory was in connection with Poly triflorochloroethylene materials. these materials now days are known by the trade name Kelleff. The company was interested in finding a solvent for these particular polymers in order that they might be coated on wire by evaporation of the solvent and for other reasons, and also, there was way to measure the molecular weight of these materials because there was no solvent for them.

I was able to make a study of the soluability of these polymers from a theoretical point of view after having read a book on soluability by Hildebrand. Also, I discovered tha the quantity known as the soluability parameter was a thing that would deterimine solvents for this material and that materials that had soluability parameters around those of carbon tetrachloride would act as solvents. Now it turned out that the polymer could only be dissolved at an elevated temperature. It would not dissolve in carbon tetrachloride at any temperature to which carbon tet could be subjected at room pressure, but I discovered that by putting carbon tetrachloride under pressure I could get ot a high enough temperature to readily dissolve the polymer, and by this means I was able to select from the available organic solvents, the other materials that would dissolve the polymer. I wrote this article up for one of the scientific journals. It happened to be reviewed by Hildebrand before publication and he wrote a nice letter to me commending me on a fine piece of scientific research involved that was of practical interest. This buoyed my ego a great deal at that point in my life.

I continued to work on various aspects of this polymer. Measured its molecular weight by a light scattering technique at elevated temperatues under moderate pressure, and I also did other things. There was a point where they were interested in the cut-through of wire that were coated with this particular polymer.

Along in 1951 Chauncey Guy Suits, the Director of Research for the company decided that it was time for the research laboratory to take a look at a synthesis of diamonds. T. Zay Jefferies of the Carboloy Company had before World War II been interested in this problem and he and others in the company had solicted the help of Percy Bridgeman in trying to synthesize diamonds. This was a joint vbenture with the Carborundom Comany, the Norton Company, as I understand it, and $100,000 was spent without success.

A lot of money was spent for a special hydraulic press that later ended up in the Norton Company, where they proceeded to continue their research. The problem was placed in the mechanical investigation section of the Chemistry Department. A. L. Marshall was head of the chemistry department. J (Tony) Nerad was manager of the Mechanical Investigations Department. At this time I was in the Physical Chemistry Department under Lievhavsky. Herman Lievhavsky. Two of the physicists in the Mechanical Investigatins section, Herbert M. Strong, and Francis P. Bundy, were asked to survey the literature to see what others had done. They took approximately a year in conducting this survey. At this point they issued a report to the company managment of what had been done and outlined some procedures that might be followed in some new attempts based on the ue of some of Bridgeman's high pressure equipment.

At this point company officials decided they should have two chemists working on the project. The physical chemists and organic chemists in the company were called into a meeting where the proposed probem was discussed and a question was asked who among these chemists might be interested in working on the project. I was the only one that was interested. The rest of the men considered working on diamonds to be a "crack-pot" venture in view of the long crack-pot history of scientists working on diamonds for over a hundred years.

Since I was the only one interested, I got the job. At this point I thought I was still working for Liebhavsky and Liebhavsky didn't know where I was working and later on I learned that Tony Nerad figured I was working for him. But anyway, for some period of time there was some uncertainty as to whether I was continuing to work in the Physical Chemistry section or the Mechanical Investigation section. Later on, I believe about a year later, the second chemist, Robert Wentorf, who had just graduated from the Unviersity of Wisconsin, was also hired to work on the project.

Now duirng the early days of the project I spent my time reading the scientific literature on what people had claimed to have done on diamonds and doing some additional thinking about the problem. I say additional thinking because diamonds had interested me from the time I had worked on my master's degree at the University of Utah. At that time I had read considerably in the old scientific literature about diamonds and had thought quite a bit about making diamonds or about making boron -- a solid crystalline boron I thought might be as hard as diamond. This was in the days when it was not certain that anyone had ever prepared pure boron. Certainly no one had prepared it in anything but an amorphous format that time.

The work seemed to divide intself into two natural categories. It was obvious that there was no high-pressure, high temperature equipment available that would reach pressures and temperatues of the magnitude that might be necessary to snythesise diamonds. Also, it was apparant that it might not be possible to transform diamond directly or transform graphite directly to diamond at the pressures and temperatures that we might be able to achieve. It might be possible to use a carbon containing compound to produce diamond plus other products. Such a reaction might require a lower pressure and lower temperature than would be required for the direct conversion from graphite.

Also there were questions about the rate of the reaction. Geologists had said that thousands or millions of years were needed to make diamonds in nature and of course if we were going to make them in the laboratory we would only be interested in making them in a very rapid period. At that time we thought maybe if you could grow a diamond in a couple of weeks that might be all right, so one of the objectives was to develop high-presure, high-temperature equipment that might get somewhere in the neighborhood of 35,000 atmospheres and maybe a temperature of a thousand degrees C or so, then this would be a great step forward and it might be possible to synthesise diamonds under these conditions.

Now it seemed that the logical men to work on the high-pressure equipment were the two physicists, Strong and Bundy, and they were given this assignment. Strong and Bundy had been working on what was known as the P-O project. This was a project to use vacuum as refrigerator insulation. Strong continued on this project for awhile as Bundy went off onto the diamond project thinking about apparatus. Bundy came up with a device that was a modified version of a hollowed-out anvil that Bridgeman had tried. Now Bridgeman had worked with these flat anvils and in trying to increase his volume, had hollowed out the anvils but had left the angle sharp. Now Bundy in a logical fashion rounded these sharp angles so that the stresses would be much more uniformly distributed. Bundy called this thing the saucer apparatus. Bundy has never published any details of this saucer device, but I described it in my General Electric Research Laboratory Report #R11064, which was published in March 1954. This particular report was rigedly limited in its distribution in the General Electric Company. It was a class 4 report which was their highest secrecy catagory. This report which was entitled the "Belt + High-pressure, High-temperature Apparatus" was finally released for publication in a journal in 1960.

Well, Bundy's saucer, was capable of reaching pressures of the order of 35,000 atmospheres simultaneously with temperatures of about 2500 degrees C. The working volume was extremely tiny. There was some indication that pressures and temperatures should be high enough to convert graphite to diamond, but in practice this did not occur. Now, Bundy and Strong together with their laboratory assistant Jim Cheney (Wentorf and I did not have an assistant -- Strong and Bundy also had a second assistant Hal Bovenkirk who was working on the P-O project and later worked on the diamond project.)

Strong and Bundy on failing to make graphite go directly to diamond in the saucer apparatus began to fiddle around with various chemical systems, which was supposed to be our assignment. After some time they had no success with this and became discouraged and at this point it was possible for Wentorf and I to start using this equipment. Now each of us who had been fooling around with various chemical systems, tried these chemical systems under pressure without any success and after awhile we too became discouraged. At this point it seemed like equipment that would reach still higher pressures and temperatures had to somehow be developed. Strong and Bundy had for a long time had an eye on the double acting piston. I shouldn't say double acting, I should say the two stage piston and cylinder device that Bridgeman had used at pressures to 100,000 atmospheres. Now the inner piston in this device of Bridgeman's was only 1/16th of an inch in diameter. But at least it had been to these tremendous pressures and Strong and Bundy thought that it wouldn't be too big a job to scale up the size of this apparatus so that the inside piston were say something like 1/4 inch in diameter and then heat the interior of this device in some manner and thus get to higher pressures in a reasonable large system. Actually the saucer system had much too tiny a volume in addition to not getting to the desired pressures.

Well now, this two stage piston device of Bridgeman's would require what is known as a double-acting press for its operation and it would require other large press tonage wise in order that the inner piston might have a diameter of the order of a 1'4 inch or so. Well, this was talked about, and it was decided to buy such a double acting press. The press was to cost something in the neighborhood of $150,000 and this was without Bridgeman's two stage piston and cylinder device which had not yet been designed in the larger size. Well, permission had to be obtained, at this time anyway from the president of the ompany, to buy a piece of capital equipment for the research laboratory that cost more than $25,000. Well, permission was granted and the die was set and things were to go this way. While the press was uner construction, which was going to take a year and a half or two, Strong and Bundy were to design this two-stage piston and cylinder apparatus to be placed in the press at the the press arrived.

This made a long wait ahead of us and so we each went off working on other things. I know that Bundy was working on turbine bearings, as well as going back to addional work on the P-O project. I believed I continued some additional work on the polytetraflorethylene cut-through of insulation on wirs. I believe, I also worked on a humidity control for basement dehumidifiers that were becoming poipular at that time, due to the humid conditions you have in basements in the eastern part of the country.

In December of 1952, while pondering the problem of an apparatus that might have a fairly large volume and get to higher pressures than Bundy's saucer apparatus, and trying to think of something tht we might get into peration right away without waiting all this long period of time for this double acting press, I came up with an idea for a device that was later known as the half-belt. We did have a hydraulic press available -- it was an old Watson-Stillman press that utilized water as the hydraulic fluid. This had been around the company for at least thirty years and was used in compressing powdered tungsten for the experiments that were being done in learning how to draw tungsten into wires. This particular press had a side ram on it as well as a regular ram that operated downward from above. This press was in terrible condition - leaked water all over from the pullback rams were located up on top and wverwhere water just ran out from everwhere. But this was the press that was being used to operate Bundy's saucer apparatus. Now since I didn't have the calling to design apparatus, I approached Bundy with my idea. He was semi-enthused, at least he was willing to help me get it constructed. I did not have a shop order on which apparatus could be constructed and you had to have a shop order to get work done in the G.E. machine shop. So this device, which I have described orginally in this G.E. Report #RL 1064 of which I have a copy and which I have already mentioned was later published in the Review of Scientific Instruments, in 1960. Well, this device

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and talked to the fellows down there that I knew real well by this time, and just ask them if they'd build it. I didn't give them a shop order, I just asked them if they wouldn't build it when they had slack time and weren't working on the things that Strong and Bundy wanted built. Well, this they did. Now it took a long time - it took about six months as I remember it for them to complete my first belt apparatus made out of steel.

Ida Rose sitting here, ask me when they completed it. I don't know whether I have any record of that or not - I probably do in my G.E. books, but of course the G.E. Company has those in their possession. I have some 35 MM copies of the notebook, but they've deteriorated through the years, and I don't know whether they're any good at this point or not. At any rate, as I remember it, it was about six months, which would have placed this sometime around June or July, of 1953, that I received the Belt apparatus from the shop. Well, during the time in which the Belt was being constructed, I obtained additional ideas as to how to increase its effectiveness. One of these ideas was to use a sandwich-gasket. This is also described in my various publications. Now I had broken the components to the half-belt at this point, and so in order to try my sandwich gasket idea I first tried it out in Bundy's saucer. This increased the effectiveness of the saucer considerably, and proved the point that this would work in any kind of device, using compressible gaskets.

When I first told my associates about this sandwich gasket idea, they pooh-poohed it, as a matter of fact, Herb Strong said, "It's no damn good!" Just like that. I paid no attention to them, however, and tried it out in Bundy's saucer apparatus, where it proved itself. So when I finally got my belt from the shop I immediately incorporated the sandwich gasket idea into what later came to be called the flower-pot gasket. Now I was convinced right off that the belt apparatus was a good device. I immediately set about to find out the pressures that it would achieve by getting various transitons in the device that Bridgeman had discovered in his flat anvils. Now Bridgeman's flat anvils worked on an extremely minute sample, could not be heated, but could obtain paperthin sample of about 3/8 of an inch in diameter pressures of 100,000 atmospheres and in this minute apparatus, Bridgeman had measured the electrical resistance of almost all the metals and had found sharp changes in electrical resistance in these things occuring at given pressures and I was using these to try and find how high a pressure my apparatus would go to. I ran the bismuth transition, the thalium transition, and so on. Now I was quite convinced that in my steel apparatus I could get pressures somewhere around the order of 50,000 atmospheres - possibly higher, and, of course, the volume was much larger than the volume in the saucer. And with the principles wich I had at work here, if I could only build the tapered pistons and the chamber out of tungsten carbide I could get to the goal of 100,000 atmospheres in an apparatus that would be much simpler than Bridgeman's two stage apparatus that Bundy and Strong were still thinking about and had not yet designed in the scaled up version for the press that was under construction.

Although my collegues and the boss were not willing to admit that the apparatus that I had at this point was any good, you see they were definitely commited to this $150,000 press and this two stage device of Bridgeman's - seeing me work kind of started giving them ideas, too, and here at one point Herb Strong decided - became interested in seeing if he couldn't use a piston and cylinder in a two stage device, but in a somewhat simpler way than Bridgeman had used it. Later on he strated to put a collar around the top of his piston that was composed of a sandwich gasket. It was a stack of pyrophylite rings alternating with steel rings. Well, this was exactly the sandwich gasket system that he had told me earlier was "no damn good," but he never did refer to this as the sandwich gasket. He always called it a collar, and whever any of us jumped him about this - and there were others besides myself, it always seemed to make him mad. No it wasn't at all like Hall's sandwich gasket, this was a collar!

Well, I'm getting a little bit ahead of the story, but later on Strong decided that this wouldn't work quite right either, so instead of having a straight circular cylinder, the cylinder tapered just like the tapered piston that I had originally used in the half belt. Now, when he had a tapered piston, then he had to make his gasket flower-pot shape just like mine, and again it had alternating layers of steel and pyrophylite. It was the sandwich gasket, but still Strong called it a collar.

Well, he now had arrived at the point of development of my half belt, but I had long since had the full belt working. Well, as he moved along, he experienced the same troubles with his belt that I experienced with my half belt. He had cracking at the bottom of the chamber and the only way to eliminate this craking is to make a full belt out of it. Well, Strong didn't want to go quite all the way - he always seemed to be trying to follow what I was doing, yet trying to keep it different enough that he wouldn't exactly be duplicating what I was doing. So he finally made a belt - a full section belt chamber, but plugged up one end of it so that he only worked on it from one side like my half-belt so that he wouldn't have the troubles of splitting at the bottom of the cylinder. Well, later on, to my amazement, Strong was ble to get patents independent of my own patent on various versions of the half-belt which was obviously just a take-off on the principles which I had developed many, many months before.

Well, to get back to my story. I wanted to have the chamber and the pistons of the belt made in tungsten-carbide - now this would have cost about one thousand dollars - somewhat in that neighborhood - for the chamber and a couple of sets of pistons. Well, I wasn't getting anywhere - I couldn't convince my collegues - at least, they acted unconvinced - that I had anything - I always felt that they were trying to protect their own skins - they had committed this vast sum of money to this double acting press, and they weren't going to admit that a guy had figured out a way not to have to use that big double acting press. So I talked to my former boss, Libhavsky, who was running a seminar among the chemists in the physical chemistry group and asked him if I could give a talk on some of my high pressure developments.

Theoretically, I shouldn't have done this. This project was very secret. Other people working in other sections, other groups, divisions with the research labor were not supposed to know about this. Well, I talked to Libhavsky and his group, they all enjoyed my talk immensely and Libhavsky was himself very impressed, thought that I had a first class development here and so he talked to Nerad and others, including Suits, and convinced them that I had something here that ought to be allowed to pursue. Well, subsequently, I did produce these components in Tungsten Carbide. Tungsten Carbide having a higher compressive strength than steel and being only 1/3 as compressible will always allow you to obtain a higher pressure no matter what the apparatus is if you construct the vital components from this material.

Well, I immediately was able to show that I could get now to truly high pressures. I obtained the cesium trnasition, no doubt being the first man to obtain this transition after Bridman had succeeded in discovering it. I obtained the barium transition, kwhich, at that time, was thought to occur at 77,400 atmosphers. We now know that it comes at 59,000, but this earlier pressure was the pressure that Bridgeman put on it as he had discovered it on his flat anvil apparatus. And so on.

Extrapolating upward, I had been to pressures of easily 100,000 atmospheres. Now I was very cautious at this time in life. I had been raised as a poor boy and never had any money and was extremely conscientous with the regard to the cost of things. I'd see others going forward pell-mell spending company money all over the place, but I was always very cautious and this sort of thing, beside the fact that I was quite reticent and not forward, had delayed kind of experiments that I would do. But I did subject graphite in this apparatus to a pressure of one point - let's see, I probably have a date here, the date of my report is Feb. 27, 1954. Memo report C-54-51, Chemistry Research Department. Title of report was "Attempt to synthesize diamond at 78,000 atmospheres and 2400 C. Well, it was kind of discouraging, of course, to be able to reach these high pressures -- I could have gone about 100,000 atmospheres if I'd wanted to risk my tungsten carbide, but I was afraid they wouldn't buy me any ore. But, anyway, at 78,000 atmospheres we now know wasn't this high, but even so - well, let's see it would have been somewhere around 60,000 atmospheres and I could have gone much higher , as I said, if I had had plenty of tungsten carbide to break. Well, at this point, of course, the thing to do was to go back to chemical systems and see wht one might do in the say of finding a catalyst or starting with a material other than graphite to make the diamonds.

Now I tried hundreds of chemical systems as time went on - all sorts of reactions trying to make ldiamonds. It might be intersting to mention at this time, that I had learned on a visit to the University of Utah that Loring Coes at the Norton Company had synthesized a new silica mineral which later came to be known as Coesite, at a pressure of around 35,000 atmospheres at 700 C or so. On returning back to - I'm probably mixed up on the date of this - I think I learned this in 1953 - now that I look back. It's when we came out west to the ACS meetings in California and vacation to Utah in 1953 in talking to Eyring and Kisttler at the University of Utah I learned about this Coesite. I immediately went back to the G.E. lab and put some sodium silicate solution in Bundy's saucer apparatus. It must have been at this point that my half-belt was broken and my full belt was not yet constructed, but I made some beautiful crystals of coesite and, no doubt, was the first man after Coes first discovered them to make these particular crystals. And in searching for things to put in my device to give me some ideas of the pressures I was achieving, I was always eagerly looking for these - I had been given the data that coesite required something like 32,000 atmospheres and 700 C and I used this also as a calibrate point on my apparatus.

It might be well to point out that I was by far the most active person on this diamond project from the time that it was decided that the $150,000 press with the two stage apparatus was going to be built. The project pretty much fell apart. And it was only because of these ideas that I was pursuing an apparatus that wouldn't require this press that things were kept somewhat active. Bundy was off working on turbine bearings, and Strong was working PO and so was Bundy working on this at times. I've already mentioned some of the things that the others of us were doing, but I was the one that was carrying the ball on the diamond project.

Well, Wentorf was interested in using my device, the Belt, and did use it extensively, for some period of time, trying out chemical systems, trying to synthesise diamonds. Bundy and Strong never did express any interest in the device other than the fact that Strong seemed to follow along closely behind me in building devices that were similar that he wanted to declare that he invented independently and were not as similar as they actually were.

Sometime along in what I would gess was late summer of 1954 this gigantic 1,000 ton double acting press was delivered - it was a monsterous thing. There had to be a special pit built for it in the floor and it continued on up for a couple of stories above that floor. Not, it's interesting that Strong and Bundy never did get around to designing that scaled up version of Bridgeman's two-stage piston apparatus, but along in the fall Strong had built a half-belt that my flower pot connical sandwich gasket arrangement and he was beginning to experiment with this in this huge press that had been delivered. Now, of course this did not require the double acting principle and could just have well have been used in the old Watson Stillman press that I had been using all the time. This device - this half-belt that Strong was using - was constructed of steel. And he was experimenting with various systems, trying to make diamonds.

Now along in the first part of December, Strong had put a mixture of materials known as carborizing compound - it was some particular brand name of a carborizing compound that did contain carbon - but what else was in it no one would ever have known without a chemical analysis. It was just a trademarked carborizing material used for hardening steel. He had put this in a chamber of this half-belt apparatus, at a temperature that he thought was of the order of 1200 C and had let it stay there overnight in a 16 hour run, as I remember, it was a 13 or 16 hour run. Now he had put some seed diamond in this particular device. Now he had opened his chamber and taken the contents out of this thing and then had put them on his desk in an open little pitre dish that we used to use for holding things and had let this thing sit there for a week or two open on his desk.

Now a long some time - he must have made the run about the 1st part of December, along a week or two later, he decided to analyse this to see if he had made any diamonds, well - he took the thing to a lab to have it polished so he could examine it under a microscope and the polishing wheel had appaently struck something hard and on fooling around they found a couple of diamonds in this device over and above, Strong says, the diamond that he had put in it. Now he was going to have these diamonds X-rayed and Strong's story is that they lost one of the diamonds, but he still had one of them left. Now this diamond looked in physical size and general appearance and characteristics exactly like some diamond chips that we had obtained on a trip that I had made in New York City for use in our expeiments. Well, here was a diamond that Strong thought he had synthesized because he claims that he didn't put any of that kind of diamond in it as a seed. He claimed he had put little tiny octahedra in as seeds. Now you might wonder about the reason for putting seeds in. Well, whenever you grow any crystals, they need to somehow have to be nucleated, that is they have to start to grow somewhere and very often a new diamond might start to grow on old diamonds so that was the reason for putting in diamond seed. Well, I was very skeptical of this whole business, particularly as the fragment that he had there looked identical to these diamond chips.

By the way, I have written a memo on my interesting trip to New York City to buy these particular diamond fragments that we were using as seeds - you might want to read that sometime. It's in my Spring-bound notebooks where I have my various published papers and other bits of information concerning patents and what-not as well as some of the GE reports. When this diamond of Strong's was X-rayed, it turned out to be a single crystal.

Well, it was hard for me to understand how a single crystal diamond would grow in the irregular-shaped splinter--that if you put it with the splinterers that we had purchased for seeds, it would look just exactly like these. Besides, there was some uncertainty in the sitution because Strong had let the thing sit out on his desk for such a long period of time. Now we had had the problem of friends introducing "seeds" into the experiments we were doing -- all sorts of hanky-panky of that nature--it had apparently gone on since time immemorial in situations where people have been trying to synthesize diamonds. If you read the literature, you'll find out that there are claims that when Moisson thought that he had made diamonds, some of his assistants had been playing pranks on him by putting seeds in the mixture that he was using while trying to make diamonds. Well, we had these seeds here that we were using for the purpose of seeds that others had access to, and I had cases myself where my friends had put diamonds in the material that I had grown.

Now in Strong's mind, he had synthesized diamonds -- and Bundy and Bovenkirk and Keeny and Nerad and others in the lab all quickly grabbed on to this as being finally a synthesis of diamond. Now, Strong immediately set about to try and duplicte this run, without success. Now, just about one week later--it was Strong's decision that he had made diamonds--I made diamonds. Now the date that I made these diaonds was the 16th of December of 1954. Now, I had been experimenting all this time--in fact, as I have mentioned, I was the one who was working the entire problem. Bundy, still had not come back to working on the diamond project at this point--he was still off working on turbine bearings, and other problems.

Well, on this day, I had taken a clue from nature in another approach to trying to make diamonds. In my all-carboloy belt-apparatus, I had a little graphite cylinder which was being used as a heating element and also to supply the graphite to make diamonds. Inside this cylinder of graphite, which was plugged at the end with graphite, I had placed iron sulphide. This is known as the mineral Troilite. I had also placed a little octahedron diamond seed near the center just below the center--of this tube. Now I happened to have it up to just the right pressure and just the right temperature and just the right timing sequence--I'd had it to a pressure of which at that time was considered to be 95,000 atmospheres. I had held the thing for about 3 minutes and then slowly lowered the temperature and pressure, trying to conserve my anvils to keep them from breaking. I described all the details of my experiment in the journal of chemical education, so you might want to refer to that paper. But I opened the cell on this day, and the cell just broke open where I caught the glitter of quite a number of little, tiny diamonds--and I could see with my naked eye their little tringular faces. Now I just knew at this instant that I had made diamonds--I was real skeptical that Strong had made diamonds--he had been working frantically--he and his assistants during this week--trying to duplicate that run, without success.

I might mention at this point that they had continued night and day, trying to duplicate that run--and before they were through, they completed over 200 runs, in trying to duplicate Strong's claim--without success. In other words, that run was never duplicated--not even to this day. Yet, the entire management continued to regard that as a synthesis of diamond--and as the first synthesis of diamond that had ever been made. Now out of the other side of their mouth, later on, the company kept emphasising that in order to be sure that diamonds were synthesized, you had to be able to duplicate it.

Well, to get back to my own run--my own reactions, at this point, were probably the most unusual that I've ever had in my life. I can hardly describe them. But I just knew, inside, that I had made diamonds. My hand began to tremble, and my knees became weak, and I just had to sit down to regain my composure. Now, when I had regained my composure--and I judge it was ten minutes before I did--I went upstairs to the microscope and looked at these beautiful little crystals under a microscope--and they had all the facial charactersitcs of diamonds. Now I might mention that these didn't look at all like the diamond that Strong had said that he had synthesized. You couldn't see a triangular face, which is characteristic of diamond crystal, anywhere on Strong's diamond. It looked like a diamond splinter--identical to these things that we had been using as diamond seeds, of which I had purchased some carats some time before. In this connection, it's interesting to note that later on, many pictures of Strong's diamond, alongside a diamond phonograph needle were shown. This was shown in the orriginal publicity and what-not. Now I had a fellow tell me that Christianson Diamond Tool Company--I became acquainted with these fellows a short time after I came to Brigham Young University--I ran into this man who was very familiar with diamonds and their characteristics--when he first saw this photograph of Strong's diamond--his synthetic diamond that he had made as shown beside this phonograph needle--he said "If they grew that diamond, I'll eat my hat. All that diamond is is a chip--a splinter from a bigger diaond!"

Well, to get back to the diamonds that I had grown: The diamonds that I grew were polycrystaline and it was intergrown crystals of octahedral diamond. In character, they didn't look at all like Strong's diamonds. I had grown them in three minutes. Strong had said that it had taken at least thirteen hours to grow his diamonds. Well, I quickly went to work and duplicated my experiment. I left the diamond seed out -- found I could grow diamonds without the diamond seed -- wondered if sulphur had been responsible for growing the diamonds -- whether it was iron or had to be iron sulfide -- so I tried growing diamonds with iron alone and was able to grow them with iron alone. I could not grow them with sulphur alone. The ______ seemed to be playing a role -- it seemed to be helping the _______, the initial growth of the diamond. The diamonds always grew at the end of the tube against the ____________. I wondered also if it was a matter of the temperature being hotter or cooler in this region. At any rate, I continued my experiments for about a month, making somewheres I remembered, around 27 runs -- and I have this written down in some of my published works. And as I remember it, I made diamonds in about 12 out of those 27 runs. Of course, I was varying conditions and this, that, and the other trying to find out what was going on.

Well, none of my colleagues had expressed interest over this full month of time-- in particularly in what I was doing. They were still trying to duplicate this run of Herb Strong's. Well, Herb eventually came to say, "Well, it undoubtedly did not take that full 13 or 16 hours or whatever it was to grow diamond, they undoubtedly grew in two or three minutes, just as Hall has found out now. And as time went on Strong tried to twist things so that my work represented a duplication of his work. Of course, the system that I was using was entirely different. I was working with much higher pressure and temperature, and I had made the amazing discovery that you could grow diamonds in high yield in just two minutes time -- was all the time it would take before the diamond growth was complete. So, immediately I had the basis for commercial synthesis. Also, Strong was working in a steel half-belt which, we're pretty sure, could not reach the required pressure that we now know is required to have made the diamonds.

Well, now, the one thing that Nerad and Marshall were trying to do -- it became apparent and obvious to me -- this is a very difficult time of my life -- I was heart-broken at times. I was made at times, and I had every sort of reaction because of the way I was being treated. They were just doing their best to show that Strong had really made diamonds first. Even though this run to this date has never been duplicated. I came to realize that they wanted the first diamond to be made in this $150,000 double-acting press. They weren['t goijg to tell the management that it wasn't strictly being used as a double acting press. They tried both rams together, but they wanted the diamonds to be made in that press to save their own skins -- although to me there was no point in saving their skins -- who cares if

$150,000 have been wasted, if after 125 years, someone in the company had somehow made diamonds, even if they made them in an apparatus that only cost a couple of thousand dollars.

Well, now, when Suits, the director of the lab, came down to see about these diamonds I was making, the first question he asked me was, "Tracy, did uyou make them in the big press?' Well, I had to tell him, "No." Well, he was disappointed, but also, shomehow, he seemed to be having to sve his skin -- to tell the management that these diamonds were made in the big press, so tha they wouldn't have wasted their mony on the thing. They must have had some difficulty in convincing the management in the first place that this thing was needed.

Well, there's some other interesting things here. I might not ge them in in the order in which they occurred, but maybe I can try and get them down. Shortly before I had made these diamonds, I don't remember how long it was before I made them, the company was really becoming disinterested in the whole project. And I heard rumors that they wanted to drop the thing. Originally, they had said tha they would be willing to spend a million dollars on this project. I know at this point, they had only spent somewhere around $300,000 -- everything included, and yet they were thinking -- there was strong talk of dropping the whole thing. I don't know whether I said it or not, but Bundy wasn't even working on the thing at this time -- until, as soon as the diamonds had been made, he came back to help Strong and see if he couldn't duplicate his run, and they worked like dogs to do that.

Now, I'm not one to oh, particularly, share the glory -- no, I didn't say that right. Usually I'm pretty generous in sharing the glory and pretty generous with people as a rule; but the thing that was getting me down was the fact that I was being left out of the picture altogether. Now, immediately the company had to make decisions as to how they were going to publicize this -- or if they were going to publicise it -- and what they would tell, and how much and so on. And very early, Strong, who was obbviously extremely concerned that he should get the major honor and glory for this achievement began cozying up to the management and the man who had been assigned to write a story. Strong immediately took him out to dinner and gave him all the dope and what-not, and I just kind of sat back, not being particularly a fighter, at least at that time I wouldn't fight for much. I just sat back and kind of felt bad about it. Well, when that first report came out, that had been written after Strong had gone to dinnner with this writer and coached him properly, this report talked about the big accomplishment of Strong and Bundy in synthesizing diamonds and then, down in a little two-sentence paragraph, it made the statement that Hall and Wentorff had also succeeded, after Strong, in synthesizing diamonds.

Well, now Wentorff got on this thing about a month later, by working in the belt, under the same approximate pressures and temperatures that I had found efficacious for making diamonds -- yeah, as a matter of fact, Wentorff was on vacation in Wisconsin at the time all this diamond-synthesis I was doing was going on, and as soon as he came back, of course, he immediately jumped on to it, and he just substituted the other metals around iron, just as anyone would have done, if I had had assistants that I had had working for me, I would have had them making all of these substitutions, but he quickly found that most any transition metal would synthesize the diamonds, as long as you had the correct high-pressure and temperature. And another thing that was amazing to me was that the amont of credit that the management and everybvody wanted to give to Wentorff for this simple role that he played in substituting other metals for the things that I had substitutred in the apparatus that I developed and what-not. They really tended to play it up--and I began to wonder if I wasn't being discriminated against because I was a Mormon or something.

Now there's one thing I should fit in here before I forget. About a week or so after I had made the diamonds a few times, I decided that it was time that I had someone else duplicate it. Well, I had a friend all these years, a Jewish friend by the name of Ed Brady, who worked in the same lab with me a good part of the time. I went into the lab with him when I first went to General Electric. So I asked Ed to come down one day and gave him the directions and what-not to make diamonds with the iron sulfide. Well, he made a run and didn't make them. As I remember it, on a second day, I had him try it-- and diamonds still were not made. Well, this is to be expected. I wasn't being 100% successful -- I've already mentioned, I believe in about 27 runs, I had made diamonds 12 times.

Well, a little later, on -- I believe, as a matter of fact, the last day of December in 1954, I had a friend of mine, Hugh Woodbury, come in to the lab and showed him how to do it and had him do a run, and he did make the diamond. So, to our knowledge, then, this is the first time, that a man had been able to duplicate the diamond-synbthesis claim of another. Well, now, the company management became concerned about duplicaton -- they didn't want to put anything out in the way of publicity until they were sure this thing could be duplicated. So, they had Richard Oriani of the metallurgy department come and make three runs, and they also asked Hugh Woodbury to make three runs, but they didn't want me to supervise the runs. They didn't want to trust me -- I might have been sneaking diamonds from underneath my fingernails or something, but, of course, I had taught Wentorff by this time how to make the diamonds, and they had Wentorff supervise these two guys and watch and see that they did everything without any chicanery. And they made three runs apiece, with the iron-sulfide system, and did duplicate the process for making diamonds, and they were successful in all three runs -- and each of them made three, a total of six runs.

I breathed a sigh of relief after this. I couldn't be anywhere around. They didn't want me there. This was kind of a legal test that the lawyers had dreamed up to prove that there were no shinaniguns (good grief!) going on and that diamonds could be made. Later on in the company publicity, they made a great point of this -- that many others had claimed to snythesize diamonds, but that no-one else had been able to duplicate it. So you couldn't say that anyone had really synthesized diamonds unless someone had the name of the duplicator. Yet, in the same publicity, they always named first Herb Strong's synthesis of diamonds and yet, here this one had never been duplicated. Well, this has always been bothersome to me.

Ida Rose just handed me a letter that I wrote to the Director of the Research Laboratory, Chauncy Suits on January 10, 1955, which she thins I ought to read on to this record:

Dear Dr. Suits:

Herb Nichols press release on diamond synthesis indicates that there is not a clear understanding of my contributions to the super-pressure program. This disturbe me considerably, as I feel that my contribution should rank as the outstanding scientific achievements in recent years.

My contributions are: 1) the design of equipment which delivers proven pressures up to 100,000 atmosphers and extrapolated pressure to 130 atmospheres at temperatures in excess of 3,000 C. for hours on end. This apparatus which I have called the "belt" was designed over two years ago. No other person on this program has made any basic contribution to high pressure, high-temperature work since the belt was designed. The belt stands as the only 100,000 atmosphere, high-temperature apparatus in existence. The closest approach to it, is an apparatus of Dr. Strong's, which has a proven pressure of 53,000 atmospheres. I wish to point out, however, that this design achieves this pressure only because it uses the three basic principles of the belt design: it differs from the belt only in minor detail. (I should have been stronger than that. I'm not quoting my letter now. Herb just seemed to follow along. Just as closely as well, he was slow at first. He was many months behind, at first, and then as he say I was having more and more success with my apparatus, he kept changing the design of his. His originally started out to be a piston and cylinder device, to copy the half-belt and the belt. Now to return to the letter.)

The three basic principles of the belt are:

1) the all-important sandwich-gasket of pyrophelite and metal,
2) conocal pistons and chamber,
3) double ending to increase symmetry and eliminate stress concentration points.

Items one and two are clearly my inventions. Item 3 is an obvious extension of the principle used by Bridgeman. Without the sandwich gasket, we would be limited to a top pressure of only 40,000 atmospheres and very small sized reaction chambers. This gasket allows increased relative motion between piston and chamber, giving larger-size reaction chambers for attainment of higher pressures. This sandwich gasket also provides particularly in combination with the conical piston and chamber, a multi-staging effect which more than doubles the load that can be placed on the carboloid and also eliminates the necessity for a double-acting press. The conical piston with 60 degree angle as used in the belt makes it possible to double the thickness of the pyrphelite gasket for that which can be used between flat-faced surfaces. I have given a detailed discussion of these principles in Research Laboratory Report No. 1064.

2) The first synthesis of diamonds by a process that can be duplicated. It is obvious that the company is able to make a public announcement of diamond synthesis because of my reproducible work. Yet, the publicity emphasis as presently planned is on Dr. Strong's as yet unduplicated work. In view of the many unduplicated claims that have been made in the last 100 years, this emphasis seems unwise.

3) Studies and measurements at pressures to 100,000 atmospheres at high temperatures. Five papers have been prepared on this work. They are:

a) Ultra-high pressure, high-temperature equipment, the belt.
b) The melting point of germanium to 100,000 atmospheres.
c) Studies on sodium-oxide, silican oxide, water-system at pressures to 100,000 atmosphers and temperatures to 2300 degrees centigrade.
d) Attempts to snythesize diamonds by direct transformation of graphite at pressures to 100,000 atmospheres and temperatures ranging to over 3,000 degrees C.
e) Some histories of properties of stone at pressure to 100,000 atmospheres.

The first two have been submitted for General Electric approval. the last three will be submitted shortly. Much additonal work has also been done to form the basis for future papers. I request that these papers be released for publication in view of the pending announcement of diamond synthesis. They are of such scope that they will give back-bone to the diamond announcement, bring additional prestige to the laboratory, and bring delayed recognition to the author. Because of my pioneering 100,000 atmosphere, high-temperature work had not received adequate recognition, I planned to leave the research laboratory last Spring; however, a token recognition in the form of financial compensation persuaded me to stay on. If, at the present time, I am not to receive the recognition, financial and otherwise that my work merits, I will feel no incentive for continuing at the laboratory.

Very truly yours,

H. Tracy Hall

cc: A. L. Marshall
A. J. Nerad

Well, this letter brought immediate reaction from Suits and Marshall. They got me cornered and tried to smooth things over and tell me that I'd get my recognition in due time and I was always skeptical of what they told me. Suits said, "Well, we've got this publicity planned for February 15," he ses, "You go along with it the way we have it planned, and you'll eventually get your recognition, because we'll let you publish your work on the diamond," and so on. Well, it turned out that this never did happen. First, the diamond paper that was published was a release from the publicity department, really, a letter to Nature that was, in effect, excerpts taken from publicity written by Niles Martin and other science writers that G.E. had. I wanted no part of it. I told them that I wouldn't put my name on it. They put it on it anyway, and sent it off to Science. Then future publicity in the way of papers written to Science on diamond had other names added and didn't tell very much, anyway. It kind of burned me up that after the publicity release, I was given a what seemed to me a minor role in describing what had gone on. Of course, the difficulty with the publicity was that the publicity release was held at the laboratory. This was the largest publicity affair that G.E. had ever held up to that point. Nothing was given in detail, there were no descriptions about how it was done, no description of apparatus, just a plain publicity release announcing to the world that G.E. had made diamonds.

I was pretty sure after this letter that G.E. people would do something to try and placate me and, certainly after having synthesized diamonds, a fellow would be entitled to a raise. I had been passed over on raises, as I remember it now, for some four or five years, and most of the fellows were getting a raise every year. And this was really beginning to bother me, and I thought I'd probably better get out and get myself a job somewhere else. Well, after making diamonds and after developing this high-pressure apparatus, I felt that I had finally showed my mettle -- that the company would come around -- but I had, in my own mind, kind of set my own limits on thins. I forget what I was making at the time -- probably somewhere around $10,000, something like that, I had obtained raises, but they had been the automatic variety that was coming along with the union raises that were coming. I think I had only received one official raise that was a raise over and above this; but I had decided in my own mind that if the company didn't -- if when they came round to give me their reward for this diamond business work -- that if my salary wasn't set at $15,000, and I wasn't going to argue with them about it -- this had to be voluntary; if when marshall came to me and said, "We're raising your salary to $15,000, I'll stay with the company. If they don't raise me, anything like that, I'm going to leave.

Well, Marshall came around a few days after this letter, all right. He came around to everybody -- everybody who worked on the project got a raise. Of course, I have no way of knowing what the other fellows received. But, as I remember it, my raise brought me somewhere up around $12 or $13,000, something in that neighborhood. So it was set at a minimum and I had heard from things that I had read that there were a few people in the laboratory who were making as high as $35,000 a year. I had heard that there was at least one in our own physial chemistry area who was making $18,000, so I figured this $15,000 was a minimum, and I had really had hopes that they might do better than that. Well, at that point I decided that I would leave the lab. I felt confident of my own ability and thought I would stand more by leaving than by staying, and because I gauged what they pay you in dollars as a pretty good gauge of what they think of you.

Well, when Marhsall came, I was fairly sullen about the whole thing and let him know that I didn't particularly think that was a very big raise for such an accomplishment and asked him what I could expect for the future, whereupon he told me that would depend entirely upon what I did in the lab subsequently. If I pulled off something good, I'd get a raise again and so on. Well, I thought that a guy ought to be worth a little "ride" for awhile after making diamonds -- after all the interest that had gone into this sort of thing over some 125 years.

Well, I let Henry Eyring know at the University of Utah that I might be interested in another job. I was offered a job at the University of Utah, but didn't like the salary, mainly, as I remember it. Shortly thereafter, the word somehow got to Ernest Wilkinson of BYU. He needed a Director of Research, and I had corresponded with Harvey Fletcher who is Dean of the College of Physical Engineering Sciences and was the Director of Research. In correspondence with Fletcher and Wilkinson, I came out to the "Y" April Conference time in 1955 and, after thinking it over for only a week or a little more, decided that I would come to Brigham Young University as their Director of Research and Professor of Chemistry, and I've been in that position to this day.

Well, I guess I'd better get back to the other story for a little bit. Unfortunately, after getting back to the other story, I pushed the wrong button on this tape recorder, and I've gone off 500 feet of tape here or so, with nothing recorded. I just hope that I can remember what I've said and that I don't duplicate what's already on the tape, prior to this mishap.

I might say one other thing. When Nerad, my immediate bos, became aware that I might be looking for another job -- and, of course, he suspected this, as soon as I asked for permission to make a trip out to Brigham Young University for a couple of days. He got in a huddle with Marshall and came back to see what they could do to keep me around one way or another. They offered me the job of manager of the diamond busines in Detroit. It was only in the planning stage at this point, but the way it was planned -- the manufacturing pilot-plan operation at first, of course, would be set up at detroit, and they offered me the responsibility of this job. Well, I turned that down right off. Later, though, when Nerad got hold of me, he said, "Now look, Tracy, you stay around. Don't you take another job." He says, "We'll get more money for you here."

So I asked him how much and he says, "Well, I can't do it all at once, but he says, "within a very short time, we'll get you $20,000 a year for you." Well, I don't to this day know whether to believe that or not. At the time, I didn't believe it. I just thought that was more just kidding me along and that Nerad nor anyone else would in a short period of time get me up in that $20,000. Well, I don't know, maybe they would have.

Well, now to try and get back to the story again. I'm all mixed up on this point on what I've said and what I haven't said. The company held the largest press release that they've ever held on the 15th of February of 1955. Correspondents from all over the world were invited to Schenectady. They didn't tell them what they were coming for, they just said, "We have something big. Come to Schenectady at our expense, and we'll tell you all about it."

Well, most of these correspondents came into New York City. They were loaded on airplanes and flown to Schenectady. When they were once on the plane on the flight to Schenectady, they were given brochures telling about the synthesis of diamonds. Now, this was supposed to have been the first information that was out on the synthesis, but there had been a leak somewhere because the British Broadcasting Company, we learned later, had broadcast at 6:00 A.M. Eastern Standard Time, this was about 10:00 A.M. when these correspondents were on the plane, the BBC announced that G.E. said they had made diamonds.

I came to the "Y" September 1 of 1955 as their Director of Research and Professor of Chemistry. Shortly after arriving here, I had a phone call from Chauncey Suits, who said that they were going to present the first diamond to the Smithsonian Institution. Well, I asked him, "Which first diamond?" Whereupon he informed me that it was to be Herb Strong's first diamond. This diamond, I may have already mentioned, was given to the President of the General Electric Company, Ralph J. Cordiner. I told Suits I didn't think that would be very wise, in view of the fact that there had never been a duplication of this synthesis of Herb's diamond, and certainly there had been a long history of people claiming to have made diamonds, and no-one had been able to duplicate them. Furthermore, Herb's diamond was atypical. The diamonds that we were now making, based on process discovered by me and variations thereof, were polycrystalline diamonds - - tiny octahedron inner grown, completely different than this diamond of Herb Strong's. Well, I didn't go to Washington, and I told Suits that if he presented this diamond to the Carnegie Institution in Washington, he ought to at least tell them that it had never been possible to duplicate this particular synthesis -- and if he did present it, I was going to write and tell the Smithsonian that this was the case.

Well, I heard no more from him, but I learned later that Strong, Bundy, Wentorff, and Suits, and others had gone to Washington and presented diamonds to the Smithsonian Institution. They didn't present Herb's diamond, but they presented some diamonds that had been synthesized in the laboratory -- a half-thimblefull or something. But the plaque reads: that these were the first diamonds synthesized in the General Electric Laboratory. Well, it wouldn't have been bad if they'd said these were some of the early diamonds made in the G.E. laboratory, but it says these were the first diamonds made in the G.E. laboratory. Well, it's amazing to me that men in Suits' position would be dishonest this way, and it's amazing to me that Strong, Wentorff, and Bundy, whom on casual acquaintance appear to be the nicest Christian gentlemen you can imagine, would go along with things like this.

One thing I have noticed, though, particularly about Strong and Bundy, is that they are company men, through and through. They go along with the company on anything that the company says. In more recent years, Suits has published a flossy article purporting to be the history of diamond synthesis at the G.E. Company. It's a multi-colored pamphlet, very beautifully done, and they distributed thousands and thousands of copies of this -- Suits presented it originally at a speech before the American Chemical Society at Rochester Section. I started through one of these at one time, marking up all the errors that were in it. Suits does his best, in this, to make a "team effort" out of the thing -- make it a company effort -- historical sequences in the thing are incorrect, and he makes an attempt in that to show that the belt was a joint accomplishment of Strong and myself -- when, in fact, the belt was strictly my own invention and development; and, in general, plays it from all angles, to make it appear tha the thing was -- I had the impression that a great deal of what was done at G.E. was done to save the skins of the bosses.

I can't quite understand this. They had said that they had to have this huge press to do the job, and then when it was not needed because of the belt apparatus, they still wanted the president of the company to feel that this had all been done in the big press that had been purchased, (you already said that) and I think, that all this was done from that standpoint. The other thing was that they deliberately wanted to submerge one individual and make this a company achievement rather than an individual achievement. In the long run, I think the company would have fared a lot better by allowing it to be an individual achievement.

I don't know whether I've already said it, but I'd better say it again to make sure. My synthesis was repeated on the 31st of December. of 1954 by Hugh Woodbury, and later on, under the supervision of company management, and because the patent attorneys felt it was necessary, had Hugh Woodbury make three separate runs and Richard Orioni made three separate runs (you already said that, too) without my being around, in order to make sure that there was no monkey-business. Well, fortunately, each of these men made diamonds in all three runs, using the original process that I had discovered. The iron-sulfide and the praphite tubes in the presence of the tantalium disks.

Now, after I had been making diamonds for about a month, Wentorrf returned from a vacation he'd been on and then, using my apparatus, immediately started making variations of the process (you already said that, too), he substituted .... I think that Herb Strong substituted the nickel later on; but it was eventually found by simple substitution of these transition elements that any of these metals were capable of synthesising diamonds up in this 95,000 atmosphere range in the neighborhood of 100 degrees Centigrade. Now, we know that these pressures today were not as high as we thought they were then, but at least that's what we thought they were at that time. The wonderful thing about the synthesis was that the yields were fairly high, and the diamonds could be synthesized in a couple of minutes time. Pilot plant was soon established in Detroit and by 1958 G.E. was selling diamonds. It is my understanding that they sell more than 3,000 tons of this diamond grit now, using nickel as the catylist or alloys of iron, chrome, and aluminum, and other things as the catylist these days.

Let me make one brief statement at this point. To summarise what I have been saying -- the synthesis of diamond at G.E. hinged very strongly on the development of apparatus. I developed the belt apparatus in the face of opposition from my colleagues and in opposition to the company management. General Electric obtained this apparatus in spite of themselves. Then, after having developed this belt apparatus, I succeeded in synthesising diamonds by a process that could be reproduced by others, and to our knowledge, this was the first time that man had ever synthesized diamonds that met all the scientific tests that needed to be applied to prove that a synthesis was being made. I cannot comprehend to this day how come the G.E. managmeent and my associates reacted in the manner that they did.

Scientists, in general, have very powerful egos and are no different from other men seeking high position. Scientists obtain their reward in recognition and seem as anxious when the chips are down to step on other persons necks that they need to in order to get the recognition they seek, just as do politicians and others.

The days at Greneral Electric, when this was going on were very difficult days for me and for Ida Rose too. It's hard to describe the anguish and the problems that we had, and since I have always been a retiring person that doesn't push (you already told this part before), when I feel that I'm being wronged, it might be that if I had an entirely different kind of personality and forward-fighting, gregarious in there pitching for what I feel are my rights -- the outcome of the whole thing may have been quite different -- I don't know. Sometimes I had the feeling that there was religious discrimination on account of the fact that I was a Mormon (you said that before).

I was, to my knowledge, the first Mormon to ever be employed by the G.E. Research Laboratories. Marshall used to tell some absolutley filthy jokes (you didn't tell that before) about the Mormons right in my presence. I'd been rather reluctant to put this material down on tape. Ida Rose has been after me for years to do it. I keep saying the past is gone, and let's forget it. Let's just look to the future. But she felt that my own knowlege of the diamond synthesis and my own feelings in reaction to this, at least in part, ought to be put down on tape for our children, and possibly future descendants. I have never knocked the General Electric Company since leaving G.E. I have had only good to say about the company and the people. This is the first time that I have ever said anything of my inner feelings concerning the individuals and others with whom I worked, and I think that I would say even today that one of the smartest things I ever did in my life was to kind of force myself upon the General Electric Company. That's the only way I can describe how I got employed -- and possibly, the second smartest thing I ever did was to quit them and come to the B.Y.U. in 1955.

It's interesting to note that the DuPont people in 1948, when I became a Ph.D. and was looking for a job -- the DuPont people were particularly anxious to hire me. When this was all over, and I had left the G.E. Company, the DuPont people, who had tried to recruit me in the first place met me -- I forget the occasion -- remembered me, and told me that I didn't know it, but they, too, had been interested in diamond synthesis -- and if I had synthesized diamonds for the DuPont Company instead of G.E. Company, that even though I had quit the DuPont Company, I would continue to receive financial remuneration for my efforts. (This seems to conflict with what you said above about it being the first smartest thing you did to join G.E.)

DuPont is one of the few companies that has a kind of royalty program where when a fellow has a particularly outstanding achievement, they give him a kind of a royalty that he collects for as many years as this particular achievement is making money for the Dupont Company. Now G.E.'s system of rewards is, of course, based solely on the salary that they pay you, and when you leave the company, of course, that ends.

Of course, a man has troubles in any kind of endeavor, and my troubles didn't end when I left General Electric. They followed me. Because I was able to use my belt apparatus at B.Y.U. for research -- now this was due at first to the company's own particular secrecy-- and, second, to the Department of Commerce secrecy that was put on my patent applications on my belt apparatus. Now, I did try to work out a program whereby I could continually use the apparatus with G.E., but the terms that G.E. wanted was that they would receive and own everything that came out of any experiments I would perform with the belt apparatus. This, in spite of the fact, that Brigham Young University and others might be supplying the money for the experiments. Well, these terms were not tolerable to me--and, of course, I had looked for a way out. Yet I had to get money to conduct my experiments. This all took time, but eventually, I did receive a grant from the Carnegie Institute in connection with the tetrahedral press.

I had made a couple of trips to Washington to talk with people in the department of Commerce about my continuing to use the Belt Apparatus. ~~Now~~ I had kind of felt that I was the father of this ultra-high-pressure, high-temperature research, -- that there was a lot of science in it besides the making of diamonds, and I wanted to use this apparatus to explore phenomena under these very high-pressure, high-temperature conditions. Well, the Department of Commerce people weren't very sympathetic -- as a matter of fact, I know that G.E. personnel were in the wings in other rooms there, at the same time that I was there -- I saw them as I came in. But, it just seemed that there was too much personal business going on there between the Department of Commerce and General Electric Company -- and G.E. was determined -- in fact, I heard it from Hal Bovenkirk, himself, that Abe Marshall and G.E. were out to get me -- to keep me from doing anything in this field.

But, on one of these visits to Washington, one of the Commerce people said, "Hall, we want to hang on to what we've got -- this equipment that we know we can use to make diamonds -- we don't want the Russians to have this. I understand that all this business -- all this secrecy was based on the fact that we didn't want the Russians to know how to make diamonds because the Russians didn't have access to diamonds -- but you could tell from the literature, and what I could learn from geologists that the Russians had all the diamonds they wanted from their own countries. This later proved to be true; although I understand that the testimony of a single woman expert of the Department of Commerce -- this woman was an expert on diamonds -- on her single testimony, all this secrecy was justified -- since she testified that the Russians did not have any diamonds.

Well, as I was saying, this man in Commerce said, "Hall, this secrecy is all based on the patents -- that's how we put it on. Why don't you invent another apparatus, and if you can, it'll be free for you to use, and we won't put secrecy on it?" Well, I did finally devise the Tetrahedral-anvil press-- and it's published in the literature, of course. I was real worried about it all the time. I thought it was clearly, separably patentable from the Belt apparatus, but I didn't know that. But, talking to people about it, they said I was in a position to judge more than anyone else. The real test would probably only come up in a court case. Well, in thinking about it, I decided it certainly was separably patentable over the belt; so, I arranged to have patents applied for and submitted an article to the Review of Scientific Instruments and presented a paper at an American Chemical Society meeting. I believe it was in San Francisco in the Spring of 1958 -- had this all arranged so that the paper would be published about a month after I gave my speech in San Francisco.

Well, this all happened. It all went off real well. The thing went through the Patent Office on the first office action -- no problems at all -- clearly separable and patentable. Then, a few months after all this, I received in the mail from the Department of Commerce a statement that this was all secret and that I had to inform everybody that knew anything about this apparatus that it was now secret. So they broke their verbal promise to me -- you ought to have everything in writing when you're dealing with a government official, I'm sure. Well, of course it was impossible to write all the 60,000 persons who subscribved to the Review of Scientific Instruments and I didn't know what to do. I did write to everyone that we had sent reprints to, or who had visited our laboratory and said the certain words to them that I had been told had to be said to people. Well, Philip Ableson, who had helped me get the money from the Carnegie Institute in Washington and others took up a little fight for me. I wans't able to follow what happened all the way -- the Research Corporation, who now owned our Tetrahedral patents, took up a fight on this -- and I do not know the details as to what went on in Washington -- I know that the whole secrecy situation on high-pressure, high-temperature apparatus was discussed in the highest of scientific and military circles -- but one day at a meeting, a high official of the Pentagon showed up and pulled rank on the Commerce people, as I understand it -- told them the secrecy was coming off. Now this was about 1959 or 1960 -- 1960, I believe. Well, they said it was not only coming off on my apparatus, but also would be taken off on the G.E. apparatus.

Well, the G.E. people, who had already held it for six years, to get their patent matters straightened out, pled that they hadn't had enough time and they wanted an extension of six months -- which was granted, and so, in effect, G.E. obtained 22 years of patent protection, and it was about six more, on account of this secrecy order. It turned out then that my patent on the tetrahedral-anvil press was issued before my patent on the belt apparatus. In our B.Y.U. deal with the Research Corporation, the Research Corportion is to license people to manufacture our device. Of the royalties received, the Research Corporation gets 42-1/2%, B.Y.U. 42-1/2%, and the inventor 15%.

Unfortunately, the Research Corporation chose to license the Engineering Supervision Company of New York City to manufacture and sell the apparatus. I had urged the Research Corporation to license either McCartney Manufacturing Company of Baxter Springs, Kansas, or NewHall high-pressure outfit in Wallpole, Massachusetts. Now as the years have gone by, the Engineering Supervision Company -- now called the Barogenics, has done an extremely poor manufacturing job -- it has kind of alienated the entire thing. Apparently, in trying to get McCartney licensed, the Research Corporation is still dragging its feet, but because of the very poor engineering and manufacturing job that has been done by Barogenics, the tetrahedral press, which is indeed an extremely fine apparatus, has received kind of a bad name. We haven't made nearly the money on the apparatus that we thought we would make. It's been making a total royalty of somewhere around $9,000 a year for the past three years. But I'm convinced that it could have been making ten times that amount. As of now, the Research Corporation has licensed the McCartney Manufacturing Company to build a tetrahedral X-Ray press such as Dean Barnett and I have recently constructed. I say, recently -- it took four years to design and construct it and get the bugs out of this thing and get it operating. But a duplicate on this press is now being built for the French Atomic Energy Commission in Paris at a cost of $156,000 -- and it is our understanding that they're going to build a second unit which will be ordered sometime in April. This first unit may be completed within the next couple of months -- I'll be going to Baxter Springs to get it assembed, and later on, Dean Barnett will be going to Paris to re-assemble it in the French A.C. lab there, after we are through with the assembly and testing in Baxter Springs.

Regarding the diamond affair at General Electric, I have no regrets. I tell folks that one of the smartest things I ever did in my life was to force myself upon G.E. so that I went to work for them in 1948. The second smartest thing I ever did was to quit them in September of 1955 (you said that before). The years here I've found a great deal of support financially for the work that I wanted to do at over half a million dollars in grants -- could have had a lot more -- particularly in the early days, but felt I couldn't use it. I've had some difficulty in interesting other professors, but as time has gone on, I've got some good ones. Barnett has been particularly good and has really taken hold of the high-pressure research. We've had hundreds and hundreds of visitors through here to see how we do the high-pressure work. In the early days, they were in here constantly, asking questions, phone calls, letters -- I think we have averaged something like 100 visitors a year who come from all over the world since back in 1966-67 when we finally got underway in our high-pressure work again.

There's not a laboratory anywhere that's going that I don't believe hasn't had its personnel here at one time or another to see how we're doing at Brigham Young University. The biggest thing I've lacked for here is the time. Of course, having responsibility as Director of Research -- this program has grown through the years and takes more and more of my time. I have taught occasionally through the years. The biggest problem has been finding the time to continue research. Also, we don't have the facilities and services that we used to have at General Electric - and in all, I feel, however, I've been able to accomplish much more here that I would have been able to had I stayed at General Electric. Also, things have turned out fine financially -- right soon I began to have offers to do consulting -- and I've been getting several thousand dollars worth of consulting each year. It's interesting how I first set my fee. The G.E. people wanted me to consult. I'd already said that I couldn't consult under the terms that they wanted, but in trying to arrive at a consulting fee, they asked me how much I wanted. I was feeling rather smart-alecky, and not too interested, and so I quickly asked them how much they were paying Percy Bridgeman. Percy came on as a consultant after the diamonds had been made. Well, they hummed and hawed a minute and then they said they were paying him $250 a day. I immediately said, "I'll take $300. Well, their jaw dropped a bit and they said, "Well, --and they hummed and hawed a bit and said, well, we'll have to go talk to the top men." -- this was in Detroit. Well, they came back in just a few minutes and said, "O.K., we'll pay you your $300." But the rest of the terms of the agreement were not acceptable to me, so I didn't take it. However, the next people who came along who wanted me to consult, I told them $300 and they readily paid it.

About a year and a half or two ago, I decided to raise it to $450 a day -- and there still seem to be a sufficient number of companies that will pay that rate--this adds to my income substantially. I've had several job offers through the years -- three of them have been as Directors of Research -- Booth, Allen, and Hamilton approached me for Kennecott Copper at one timne for me to be Director of Research in their new lab that they were forming -- said they'd pay anything I wanted. I could set up the lab, hire my own personnel, and even have the lab built the way I wanted it built. Ida Rose was a little provoked that I didn't ever take this one. G.E. came after me later on -- I think '58, '59 -- sometime along there-- and offered me $35,000 to be manager of their diamond lab in Detroit. I.B.M. approached me at one time at a $35,000 a year salary to be director of a new lab that they were building. So, even financially, I think that we made out all right by making the transfer to Brigham Young University. It certainly has been better for our family. The children have access to all the cultural opportunities that the University can provide, and, of course, Sherlene and Tracy Jr. are now attending the University and it costs us a lot less money to be living right next door to one than to be sending them out here from Schenectady.

THIS TAPE WAS PREPARED ON JANUARY 21, 1964. I am appending this note here on February 8, 1965, when I'm again sick in bed with the flu and am preparing another tape which is a duplicate of this tape.

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In retrospect, my generation has been a generation of much progress in almost any field you might mention. It has been of tremendous satisfaction to feel that my work has contributed to that progress.

The ability to make diamond under laboratory conditions has opened the door to new horizons in industry and research. I have watched it germinate from our laboratories at Brigham Young University and develop into what it is today. (figures, etc.)

I forsee that many new advancements and innovations will still be forthcoming. Gem diamonds will surely be one of these. There will be further refinements of equipment and methods in the industrial development of synthetic materials, including diamond grit and compounds and I forsee that basic research in the field of high-pressure-high temperature will yet yield greater understanding of the geological process and formation of yet undiscovered new materials.

Our pioneer forefathers wrested a nation from a wilderness. Our pioneer progenitors will wrestle with the frontier of the mind. I can hardly wait to hear of their accomplishments and discoveries.
: <:\= ™verwhere. But this was the press that was being used to operate Bundy's saucer apparatus. Now since