

1/2 hr phone call  
29 Nov 1976

Test Data. X# 106

on assignment former

approach -

then quality by different

Final. 40004668

---

Kopka - Kopka Program

former Director

have been working on for  
some time

XTal growing SAE case,  
Agreement

Pres "Overland"

former Wm Anderson

*William A. Harshaw, II*

*6020 Deer Run Drive*

*Chagrin Falls O. 44022*

*216-247-4487*

May 11, 1976

Dr. H. Tracy Hall  
1711 North Lambert Lane  
Provo, Utah 84601

Dear Dr. Hall:

First, I wish to thank you for your courtesy in taking the time to spend an hour on the phone with a total stranger. I feel more than awkward in suggesting an alternate means of accomplishing a result on which so many people have spent so many millions of dollars and to which you have devoted a major part of your career with eminent success.

However, feeling awkward has seldom prevented me from diving into "black holes". Once in a while I have even been guilty of "having started with the wrong premise, he proceeds with impeccable logic until he finally arrives at the grand fallacy" (Disraeli ?). In this case I have spent quite a number of years trying to avoid starting with the wrong premise, but with the help of several people (more technically educated than I in physical chemistry) as sounding boards, or editors, for an unconventional notion for producing large gem size diamonds I am, at last, sufficiently encouraged to present for your critique an apparently novel approach. Inasmuch as it involves the presumption that suitably constructed isostatic presses can achieve by means of phase changes in suitable materials, pressures far in excess of those reported in the literature, it is probably impossible to prejudge a practical result, because even though all of the parameters of the phase diagram for diamond formation will have been met, there is not enough known about the kinetics at lower temperatures to predict the time factor.

At this point I must acknowledge the help and "feedback" of the three gentlemen who have been most helpful in evolving my premise.

49004589

The first is Mr. William S. Foulks, who has done all the literature research on this, and several other subjects on which we have been working jointly. His conception of a disassemblable isostatic press is included in the attached and could make production runs more feasible. His experience includes several years with the Harshaw Crystal Division on pressed optics at moderate pressures. He headed the Pressed Optics Section.

The others with whom I have discussed the most current version of the proposal are Drs. Carl F. Swinehart and E. Baldwin Sawyer, Jr., both of this city. Dr. Swinehart has been the technical director of the crystal department of the Harshaw Chemical Co. (Now a division of Kewanee Industries) for about the last eighteen years. Because the majority of optical and scintillation crystals are alkali halides, he is used to single crystals in as much as tons rather than carats. Methods used by Harshaw include "Stockbarger" diaphragm furnace, "Czochralski pullers" and solution growth. Also, recently, pressing, extruding and forging have become common for the more malleable salts for many optical uses. Dr. Swinehart is primarily an inorganic physical chemist with a great deal of practical engineering experience. He has reached retirement age, but continues as a full time consultant.

Dr. Sawyer was once with Bell Telephone Labs and joined Sawyer Research Labs some fifteen years ago. Originally, Sawyer Labs was started by his father, as an offshoot of Brush Development Corp. since at that time Brush was not interested in quartz crystals and he was. Ironically, after several complex realignments, Brush-Wellman has reacquired Sawyer Research, but not Dr. Sawyer. As you know, quartz crystal growing is a hydrothermal process, and Sawyer are the leading suppliers of single crystal material. Also Dr. Sawyer has had experience with sapphire, both hydrothermal and meltpulling. Again, a very knowledgeable materials scientist.

In any case, the evolution of some potentially useful ideas on the subject of diamonds have evolved in some informal discussions with the above gentlemen. This is not to infer in any way that any of them do not have

the normal reservations of any prudent person when presented with something which apparently has never been tried. The significant thing is that for the first time there is no "hard core" theoretical objection.

I am enclosing a write up on two forms of isostatic press without great detail as to the technology of electroforming, heating the core and cooling the press, and several other niceties that are necessary but solvable with "state of the art" techniques. This was presented to Dr. H. M. Strong through Kaplan, Lazare as a separate disclosure from the discussion of diamond formation, which I have abandoned. No one has yet demurred about the ability of such devices to contain pressures higher than anything achieved in the laboratory. At the time, the entire discussion dealt with  $UF_6$  simply because I was familiar with its properties. Dr. Swinehart has pointed out that alkali halides (notably cesium iodide) have almost as much expansion on melting but at temperatures of 600-700°C, which I believe would have to be exceeded in any case, and are more convenient to handle in assembling the "core" of the press.

Essentially, what I now propose is to start with amorphous carbon, which might be assumed to have interatomic spacing similar to the interlayer spacing of alpha graphite. In this situation the application of pressure is all in the direction of condensing volume in the direction of diamond. If the temperature is kept below that where graphite forms spontaneously, one should not have to go two directions at the same time by stretching the graphite double bonds, while compressing the whole unit to a lower specific volume.

The question is what temperature is enough to allow reasonable atomic mobility to have the kinetics to operate conveniently? There are several reasons to suspect that ~800°C (1073°A) might be the magic area. Firstly, this is the lowest temperature reported in the literature at which diamond was observed to revert to graphite on the surface (pressure not reported but presumably low). Secondly, Dr. Swinehart has observed, from his readings in geology, that diamonds are often associated with jade, which, in turn is chemically similar to the "blue earth" associated with diamonds. He tells me that jade is thought to have originated at depths where the temperature is about 800°C. Finally, the diagram in your patent No. 3,816,085 defines the

lower practical sintering temperature for your matrix at not too far above this temperature, but you are deliberately working at pressures below the stable region for diamond whereas I propose them far in excess.

In short, I propose trying amorphous carbon  $\sim 800^\circ\text{C}$  and upwards of 500 Kb (but who can measure it--the only test I can think of to approximate the pressure is that if the container ought to hold a certain pressure and doesn't, we went too far).

What would one expect to get as a product? Hopefully, large gem-size, clear, diamond-like structures, whether single or polycrystalline. I have read about "white carbon" which seems to form epitaxially in plates like graphite. Could there be such a thing as might be described as amorphous diamond? (I recognize the contradiction in terms.) Anything without continuous cleavage planes should really be hard.

Well, there are other interesting observations which one might consider. The ring spacing similarity between graphite ( $1.421\text{\AA}$ ) and hexagonal boron nitride ( $1.446\text{\AA}$ ) suggests the interlayer spacing may also be similar and might alternate in a dense CBN structure.

Dr. Swinehart worked on electrolytic fluorine cells during WW II. He reports that graphite anodes would swell considerably by adsorbing considerable amounts of fluorine at low electrolyte temperatures, which could be expelled or avoided at higher temperatures, whereas carbon anodes did not swell. This could provide some lattice mobility with pressure, but has numerous drawbacks. Since nitrogen is found in natural diamonds, in as much as 15%, this may be considered as a possible catalyst in the sense of a solid-gas solution for carbon.

Why bother you with all this? The answer is probably transparent. In order to try the first, and basic idea requires money that I don't have. I know where it can be obtained, but not on my technical credulity. Your

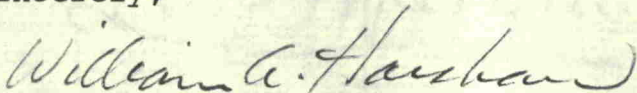
40004671

appraisal, if not negative, would be the best available. The gentlemen I mentioned earlier have a great deal of knowledge in crystal growing, but not diamonds. Perhaps the work could be done at your laboratory, but this is unimportant at this point. In case of any success I would expect you to receive a fair proportion of the proceeds.

My ultimate goal is to establish a development company to work on projects which are not obvious to people too preoccupied with the hassles of everyday business. There is an untapped wealth of viable ideas in the heads of excellent people who can't get a chance to try them out because their jobs are assigned and not chosen. What is needed is a winner to start with as a basis for more.

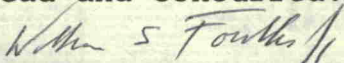
I have not asked for a secrecy agreement because I believe in your case it is understood and unnecessary. I shall be most anxious for your reaction.

Sincerely,



William A. Harshaw II

Read and concurred:



William S. Foulks, Jr.

P. S.: We have not included any of the two or three pounds of literature research done by Mr. Foulks since this would be all too familiar to you.

40004672

*William A. Harshaw, II*

*6029 Deer Run Drive*

*Chagrin Falls O. 44022*

*216-247-4487*

May 12, 1976

Dr. H. Tracy Hall  
1711 North Lambert Lane  
Provo, Utah 84601

Dear Dr. Hall:

Since dictating the attached, I have discovered that among the enclosures I have only half of Mr. Foulks' drawing (No. 2). The other half is in his possession and he is not available at this writing. The missing part is an expanded detail of the core which will be forwarded later.

Basically this press can be considered a drawer-in-drawer arrangement in which each succeeding drawer is oriented at 90° to the preceding, and are all open-ended for assembly and disassembly. These would be interlaced with expendable soft metal shims of preferably cadmium which could be electroplated on the base surface. The drawers could be assembled by force fitting or could even be electroformed with preferably nickel to make the next massive chamber. This would mean that machining would have to be done only on the outer surface. Disassembly could be facilitated by moderate heating to permit the shims to easily release.

Also Mr. Foulks has now delivered to me a copy of your Patent No. 3,913,280 which is of further great interest inasmuch as the example concerning nitrogen ties in with the attached discussion.

Sincerely,

*W. A. Harshaw*

William A. Harshaw, II

WAH:cm

Encls.

60004673