More on Early Diamond Work

To the Editor:

The article, "Diamond Synthesis: The Russian Connection," *MRS Bulletin*, p. 65 (February, 1996) gives an interesting account of Russian accomplishments in the long history of diamond synthesis. The description of the development of the chemical vapor deposition (CVD) of diamond is in agreement with my recollections. Your readers might also be interested in one or two events, not mentioned in the article, that in my experience significantly influenced developments in metastable diamond growth.

The unfortunate polywater episode had a significant impact on events in the United States. The realization that Deryagin's claim of a new phase of water was erroneous cast an unwarranted shadow on other efforts from the Deryagin laboratory, even on programs such as diamond that were unrelated to the polywater work. This, together with the widespread skepticism that diamond could be grown under conditions in which it was the metastable phase, led to a sharp curtailment in funding of diamond research in this country by the mid-1970s. However, the low growth rates achievable at that time also must have been a factor in funding decisions.

The history of diamond synthesis would have been a great deal different if the Soviet group had been willing (or perhaps able) to share their findings in the normally accepted manner of scientific discourse. The publishing of papers, including a major review article in *Scientific American* in 1975 [B.V. Deryagin and D.V. Fedoseev, November **233**: 102], without mentioning the use of atomic hydrogen, for whatever reason, had the result of further confusing an already confused experimental situation. In retrospect, the use of atomic hydrogen now seems so obvious as to be self-evident. At the time, nothing was that clear. There were a multitude of directions to pursue, and too little available time and resources to pursue them. The Japanese team at NIRIM [National Institute for Research in Inorganic Materials, Tsukuba, Japan]—Yoichiro Sato, Mutsukasu Kamo, and Seiichiro Matsumoto—under the far-sighted direction of Nobuo Setaka had to rediscover much of what others had already learned in one form or another.

One can trace the evolution of the use of atomic hydrogen in diamond synthesis back one step further. Professor Nelson C. Gardner, a co-worker with me in our early diamond work, learned how to generate atomic hydrogen with a heated tungsten filament for the purpose of cleaning carbonaceous residues from field emission tips during his graduate studies at Iowa State University. Based on this knowledge, we had the idea of using atomic hydrogen as a more effective alternative to molecular hydrogen or atomic oxygen for removing co-deposited graphite. Atomic hydrogen worked quite well in a cyclic growth-cleaning process and this fact was reported, as the article indicates, at a Soviet diamond meeting in Kiev in 1970.

Finally, although it is true, as the authors state, that there was widespread skepticism about the possibility of growing diamond at low pressures, these doubts were by no means universal. Percy Bridgman, in a 1955 *Scientific American* [November **193**: 42] article describing the successful synthesis by GE [General Electric] at high pressures, made a point of explaining that diamond synthesis as a metastable phase at low pressures was also possible. J.J. Lander and J. Morrison at Bell Labs clearly stated the case for diamond CVD in 1966 and, in fact, pointed out the role of chemisorbed hydrogen in maintaining the bulk-terminated diamond surface structure. To this participant, it seemed that the better the scientists, the more receptive they were to novel ideas.

Perhaps some lessons can to be learned from these events. They surely provide support for open scientific communication. They may also indicate that, as we reinvent ways of funding research, we recognize the need for a broad spectrum of funding sources so that truly novel ideas can find at least one receptive audience.

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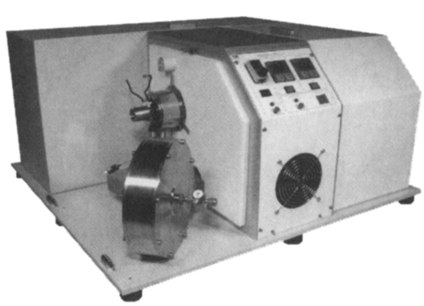

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