Graphene and the 2010 Physics Nobel Prize

This year's award of the Nobel Prize in physics has raised issues of accuracy and fairness. My colleague at Georgia Tech, Walt de Heer, is one scientist who was slighted by the Nobel committee and decided to say something about it. You can read Eugenie Samuel Reich's account in Nature at the URL

http://www.nature.com/news/2010/101124/full/468486a.html

and in it you will find a link to de Heer's letter as well.

I have recounted an error by the Nobel Prize committee for Chemistry in an autobiographical sketch that will be published in a volume of such sketches in 2011. Here I reproduce an excerpt from that essay * :

It did not take me long to rediscover the work of Ilya Prigogine, which I had encountered at Caltech. Uhlenbeck and Lars Onsager had strong opinions, indeed qualms, about his research, but Kac communicated with him and even exchanged a postdoc. In 1971, I moved from Berkeley to Atlanta to become an assistant professor of physics at Georgia Tech. At the time, the Prigogine group in Brussels was promoting the then-new Glansdorff-Prigogine Criterion for nonequilibrium thermodynamic steady states with applications to biology. At first I couldn't understand their theory, but once I did, I found a flaw in the logic—where a result could be sufficient but not necessary. I coauthored a paper with Joel Keizer '64, and later wrote two more, criticizing Prigogine's idea.

At first Prigogine was livid, and he even telephoned Kac from Europe one morning (the middle of the night for Kac) to protest an editorial decision to publish my paper in the *Proceedings of the National Academy of Sciences* (*PNAS*). Regardless, the critique was published, along with a response from Prigogine; time would tell who was right. Some years later, in 1981, Prigogine was the Hitchcock Lecturer at the University of California, Davis, while I was on sabbatical there as a guest of Joel's in the chemistry department. I was eager to meet him face to face. Once I did, we spent many hours together. By then, he was a controversial figure so that the local organizer for the Hitchcock lecture series had difficulty filling his time slots. At one point in our conversation, Ilya asserted in his thick accent, "When you do as many things as I do, you make a few mistakes"—although he never admitted as much publicly.

Publicly, the Glansdorff-Prigogine principle got restated for a while, and then finally Prigogine retired it from his writings altogether. He was a charming and multi-talented man but a little grandiose in his scientific claims, which is what worried Uhlenbeck and Onsager. After our exchange in *PNAS*, his group waged a campaign to promote his (flawed) principle. He won the Nobel Prize in Chemistry in 1977, the year after Onsager died, and the award cited his principle and its putative importance for questions about the origin of life. It was known that Manfred Eigen and Francis Crick were in favor of the award. In print, Eigen misquoted the principle with regard to precisely the issue Joel and I had criticized, necessity and sufficiency, thereby missing the flaw. I wrote Eigen a letter about it. He did not respond. Years later, the scientific layman's perception was that I had done battle with a Nobel laureate (Prigogine, not Eigen) and won. Alas,

that he wasn't yet a laureate when the argument was won was a point people often missed. Indeed, the fight triggered events leading to the awarding of the prize! This story highlights the politics of science at work. To underscore it, the argument that I had won the fight was used, I was told, when I was selected for the title of Regents' Professor of Physics at Georgia Tech in 1991.

My very close friend and research colleague, Joel Keizer had abetted me in my battle with Prigogine. The repercussions for Joel, a member of the chemistry department, were much harsher than for me. Many vocal, well-established physicists were clearly critical of Prigogine so that my colleagues at Georgia Tech and elsewhere were sympathetic to my position. Many chemists, on the other hand, could not admit that a Nobel laureate (in chemistry) could be in error, or unwarrantedly honored. Joel lamented to me about negative effects even several years later.

This shows how politics plays a significant role in the awarding of a major prize. Errors of commission have been made by the Nobel committees but errors of omission are much more frequent. The influence of a man with the stature of Francis Crick is understandable but even he made an error. When he told Joel Keizer his position on Prigogine at an Origin of Life meeting hosted by Sidney Bernhard in Oregon in 1977, Joel quickly set him straight and explained the error. Crick was greatly taken aback by what he had done *vis a vis* the Nobel committee.

*"Discovery's Ecstasy, Friendship's Reward," pp. 147-157 in *Thinking Reed: Centennial Essays by Graduates of Reed College*, ed. Roger Porter and Robert Reynolds (Portland, OR: Reed College, 2011).

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