[Kakenhi Essay]

April 2014 Issue

That's One Thing, This Is Something Else!

Hisayoshi Yurimoto, Professor
Graduate School of Science, Hokkaido University

My research seeks to shed light on the origins and evolution of the Solar System. My dream is that I may one day be able to experience their entirety when I shut my eyes. Research in this field began around the dawn of the age of modern science in the mid-18th century, and continued to evolve through rigorous application of the scientific method and laws of physics and chemistry that were developed and consolidated in parallel with the advancement of science. In the second half of the 19th century, it was discovered that meteorites that have fallen from time to time from space contain residual signatures of the array of chemical reactions that took place during the formation of the Solar System. The second half of the 20th century marked the start of interplanetary exploration using manmade space vehicles and probes together with astronomical observations of the processes of planetary formation. In recent years, sample-return missions have succeeded in collecting and recovering samples of asteroidal materials from extraterrestrial locations. Comparative analyses of these samples with scientific data on extrasolar planets have fueled speculation that our Solar System may not be a typical planetary system at all. In other words, it seems that we are entering an extremely exciting and fortunate period of history similar to the transformation that supplanted the Ptolemaic (geocentric) view of the world with the Copernican (heliocentric) view. In the field today, scientists have achieved powerful advances in their research through not a Trinity but a Quaternity of methods—theory, experiment, observation, and exploration—and steadily altered our view of the origins of planetary systems through the perpetual and repetitive proposal and verification of bold new hypotheses. I am mainly engaged in the study of the origins and evolution of the Solar System from an experimental standpoint. The discovery of stellar remnants on the basis of laboratory experimentation and new theories about oxygen isotope anomalies has helped open the door to the use of tools from material science for the verification of theories about the history of the presolar age. Thanks to Grants-in-Aid for Scientific Research, it was possible to move forward with that endeavor.

I received my first research grant in 1987 and have benefited from additional research grants on many occasions in the years since. This may seem something of a waste, for as you can imagine, my research involves highly theoretical topics far-removed from everyday reality and of little immediate practical value. However, even I have had frequent opportunities to experience the pure and healthy spirit of intellectual curiosity shared by my compatriots when I was allowed to analyze materials brought back by Hayabusa, the asteroidal sample-return spacecraft launched by Japan. Although I was surprised by the popularity of the Hayabusa program, the public's strong expressions of support and pride in Japan's achievements also gave me the sense that I was sharing my research with everyone. It also left me with the modest impression that I could be of value in some way to everyone, a feeling that made me happy as well as somewhat embarrassed at the same time. By the way, my analytical work on the samples brought back by the Hayabusa craft was funded entirely by the JSPS research grants. That said, it is extremely rare in my case for research of this nature to ever go as well as expected.

Whenever I am preparing application documents for a new research grant, I always try to have a clear picture of my project goals in mind. Although I take pains to thoroughly detail my experimental plans on the belief everything will go as smoothly as I expect it to, I still can't seem to shake the habit of trying to achieve all of my goals within the defined duration of a single project. Maybe this is because I am impatient or simply too confident. To give an example, I was involved in the development of an isotope microscope, a new piece of analytical instrumentation that I utilized during my study of asteroidal samples from the Hayabusa spacecraft. I received a research grant for that development project in 1994 and managed to have the new microscope ready for practical use by 2003, some nine years later. By practical use, I mean that the equipment was basically functional, but it was nowhere close to the level of perfection I would feel proud of. Actually, to take it to that level, I continued to benefit from extensive grant funding for some time. Although I feel I eventually succeeded in making refinements to virtually every aspect of this instrumentation, I am not so sure how well it might have done in performance benchmark tests that are the norm these days.

Let me give another example. In 1991 and 1992, I was engaged in a research project with the title, "Sanso doitaihi o riyoshita taiyokeigai-busshitsu no tansa to sono-kigen" ("Study of Extra-Solar Materials on the View of Oxygen Isotopes"). While simple yet elegant in its design, this project was also a fairly ambitious undertaking (at least from my perspective) and one that had been approved for grant funding. In the end, I was unable to discover any extrasolar matter during the scheduled duration of that project. However, I continued to receive research grants for this purpose and finally achieved success some 13 years later, in 2004. In effect, I had discovered grains of stellar remnants in meteorite samples that came

from a period predating the formation of our Sun. However, the locations and types of matter in which I had discovered these grains of stellar remnants were far removed from my initial expectations. My only salvation was that I had been accurate with my initial focus on comparisons of oxygen isotopes. This was unequivocally a case of reality proving to be stranger than fiction. My research pursuits often leave me puzzled because Nature has a way of just acting weird.

In recent years, grant recipients have faced the requirement to write interim self-evaluations of their own research progress during grant-funded projects. Given that we are expected to objectively evaluate our own performance, I am able to at least superficially put up a good front in that respect. So far, I have managed to pass muster without submitting reports that are overly critical because I feel confident I am actually making progress with my work. That said, if I were expected to submit only a subjective evaluation of my progress, I am less certain what the outcome would be. The thing is, for me, preparing a self-evaluation typically becomes a drawn-out process of soul-searching. This stems from the frustration I sometimes experience from gaps between reality and my initial expectations toward myself at the grant application stage, or from a sense of powerlessness I feel whenever Nature does not behave the way I anticipated. However, these are exactly the things that make this field so interesting and at the same time the reason why I still pin my hopes on the ideas and insights that may come my way in the future.

At present, I am engaged in a grant-backed research project of my own that will attempt to quantitatively determine how much more intense our primordial Sun of 4.6 billion years ago was than it is now. I am also involved in a grant-funded undertaking in group-based research that is looking at molecular evolution during the presolar period. These are research themes that no one has ever pursued before. If my own sometimes-unreliable expectations prove accurate, the findings of either of these undertakings can be expected to shake up our understanding of the evolutionary path followed by our Solar System during its formation and provide fresh momentum to the quest to perfect a general theory of the origin of planetary systems. Well, I for one am extremely excited to see how things turn out!