[Kakenhi Essay] November 2013 Issue

Support Research Yet to Bud

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During the period I was assigned to a laboratory to complete my graduation research, my academic supervisor instilled in me a mental preparedness for experimental research. One of the points he made that I remember to this day and always have in mind whenever I am about to embark on a new research undertaking is this: Research is performed not by equipment, but by people. Back in 1965, our lab did not have any sophisticated or expensive equipment, but it was filled with many devices that had been made by hand to meet the conditions absolutely essential to the achievement of research objectives.

We were trained to view the advancement of learning and knowledge as a process that involved the creation of experimental tools from scratch with the objective of opening up new fields. Furthermore, we had been converted to the belief that commercially available equipment facilitated research that anyone could perform, but could not open the door to as-yet uncharted fields. My own research involved the observation of structural defects in large perfect crystals, a task that had not been performed in our laboratory up to that point in time. So, to assemble the tools I needed, I took over some high-voltage transformers, an optical goniometer, and other discarded equipment from the physics laboratory, gathered together some other electrical parts, and began with the task of building an X-ray generator. I devoted myself to reviews of the background research literature, the mastery of experimentation techniques, and the design and development of observational tools. Working together with engineers from a local factory, I designed and built a new type of experimental apparatus (an X-ray diffraction microscope). That achievement gave me a firsthand taste of the strong surge in motivation one derives from creating something from nothing. These successes led to the acquisition of grant funding for a research project led by my academic supervisor and as an outgrowth of that, we developed an ultra-high vacuum microfocus X-ray generator through joint research with a large corporation. I also proposed a new growth model for copper whisker crystals, noting that such crystals still grow even if they do not incorporate axial screw dislocations. Additionally, I prepared serial photographic footage of the proliferation of dislocations during copper whisker crystal transformation and unraveled the mechanism of transformation itself. Furthermore, exploiting the fact that copper whisker crystals are perfect crystalline structures on their surfaces and internally, I conclusively identified copper's x-ray atomic scattering factor, f220. This series of research achievements culminated with the publication of my graduate thesis.

Eventually, I attained research assistant status, which put me in a position to apply for Grants-in-Aid for Scientific Research. After returning from a period of foreign exchange study in France in 1974, I received my first grant—for research on super-elastic phase transformation that demonstrated the shape memory effect. Fortunately, I continued to receive grant assistance as my research evolved to other, related research themes. Ultimately, in 1997, I received a Grant-in-Aid for Scientific Research on Priority Areas (A) for a three-year program. In that undertaking, I served as a team leader for research projects on several themes and in the process learned that having appropriate frameworks for collaboration is vital to the goal of boosting the accomplishments of research in general.

Considering that most of the students in my laboratory would be employed by companies in the electrical and electronics industries after they graduated, around 1982 I launched a new program of research on the manufacture of magnetic metal thin films. The number of students from China increased and I worked with them to start the fabrication of film deposition equipment. The investigative documentation that I was required to submit with the application for grants-in-aid asked for detailed information on accomplishments made by the applicant to date on the proposed research theme as well as specific and expected results of the proposed research. It was not easy entering satisfactory details on my research accomplishments with the inadequate, handmade equipment that I had used. Fortunately, the application referees were impressed by the originality of my research objectives, a high-speed deposition type direct-current resistant sputter device and the unique designs of devices for sample analysis. As a result, I received two grants around the same time, with a duration of three years. After graduating and returning to their home countries, some of my students have harnessed their experiences with device fabrication, developed new film deposition equipment, and continued to engage in research to this day. When applying for new research grants, I gained the impression that proposed research themes will be more highly favored if they specifically detail research objectives deemed likely to guarantee fresh accomplishments that outstrip the accomplishments other researchers have made up to that point. Needless to say, it will be essential in any event for applicants to prepare appealing proposals that highlight the differences distinguishing their proposed research themes with themes pursued by other researchers to date, carefully describe the prospects for new findings in terms that are verifiable, and endeavor to make the content of their proposed research easy to understand.

By the way, I consider it necessary that university research frameworks put emphasis on basic research and ensure a research climate that facilitates the sustained exploration of themes that do not always immediately generate tangible results. Nonetheless, as a bare minimum, it is essential to have a budget for basic research because no project can be realistically launched on the basis of ideas alone. To maintain their status as world-class research institutions, some universities may be selective about those areas of science and technology where they want to concentrate their resources and encourage progress. However, that approach will not be sufficient to enable them to broaden the horizons of the fundamental fields of science nor will it breed in young researchers a spirit of challenge. In reality, our policies on science and technology today tend to narrow or limit the horizons of fundamental science while putting heavy emphasis on the more advanced, cutting-edge challenges of big science. As a national policy that has a better chance of producing more Nobel laureates, I think it would be more effective if, rather than limiting the selection and focus of research assistance, we extend its scope of inclusiveness more broadly to young researchers and allocate a certain measure of assistance to research in fields that are diverse and consequently have unknown potential. Starting research on a new challenge demands more than knowledge and infrastructure; it demands motivation—namely, the motivation to be the first to pursue research on that theme. Ensuring the value of human endeavor demands that we seek a balance between freedom and restraint. If we limit our scope only to structured or established fields of research, we may lose an opportunity to cultivate the motivation to take on new challenges. In that sense, why not conversely think of failure to obtain a grant as a period of hunger and an effective opportunity to nurture new ideas? What we strongly yearn for now is a shift in the orientation of core policy toward the creation of an environment that enables young researchers to constantly take on new research challenges.