TAKING IT PERSONALLY

Sure, chemical research is fun—but the issues we study must be important and consequential.

With some trepidation and reluctance, I responded affirmatively to Chemical & Engineering News’ invitation to write an essay on the future challenges of chemistry and my field within chemistry. I am no essayist, and I generally resist telling others what I think are important areas for the future. With this in mind, I have chosen to present a small personal challenge that is related to the larger challenges we face as researchers.

First, some history. I took Maitland Jones’s freshman chemistry course at Princeton University because it was required for engineering. After six weeks of molecular geometries, molecular orbital theory, and chirality, I went home for fall break having decided on a career in chemistry. I spent hour after hour in “Jones Alley” conducting undergraduate research. At the Jones group gas chromatograph, I learned of the explosion of the Space Shuttle Challenger; read novels for Japanese lit, and learned about physical organic chemistry. I was thrilled to achieve baseline separation of the cis and trans adducts of dibromo-carbene with trans-cyclooctene—although it was hardly a breakthrough for the field of chemistry.

In other words, I became a chemist to solve puzzles by making things, not because of some vision I had for the future of science or because I felt that one area or another of chemistry was poised to be important in the future. Instead, I could solve puzzles by obtaining nuclear magnetic resonance spectra with sharp signals and smooth baselines, by growing single crystals, or by watching a steady drip from a distillation column. This stuff was fun! To add to the challenge of chemical puzzles, the objects under study could not even be seen. The mysteries of how molecules react led me to study reaction mechanisms.

These motivations led to a general and constant personal challenge. Science is expensive, and scientists are paid a comfortable living to do it. We are working in a period of increasing accountability, and in my opinion, our universities, the government funding agencies, and the U.S. taxpayers do not owe us support just because we like to ponder puzzles and solve puzzles by making things, not because they are fundamental or applied, physical, or biological, or just purely chemical.

Therefore, my personal challenge every day is to identify problems that will lead to results with broad impact while using the thoughts and actions that led me to become a chemist.

How, then, do we decide what is consequential, important, and thus worth paying for? We generally claim that the U.S. is full of creative people, and if this is true, then our creativity is derived from the unique diversity of available experience. Different cultures, languages, family histories, and motivations provide us with a broad spectrum of ideas. When extrapolated to creativity in science, it is the breadth of research backgrounds, approaches to a problem, and intellectual interests that foster a creative environment. Efforts to channel these ideas to certain, so-called important, problems will limit our scientific culture and, in its extreme, make us all think the same way. Research is an exam for which we write our own questions. If we take a limited number of exams, we will generate few answers to even fewer questions. Therefore, as researchers, our challenge is to ask questions that have not been conceived previously and, once they are posed, to make people curious for the answer.

Without a doubt, it is important to explain our future potential to nonchemists. However, within chemistry, I believe that it is counterproductive to emphasize “important” areas for the future. All of us should ask our own questions that identify ambitious goals, that are currently unanswerable, and that generate new problems. To me, these are the characteristics of an important problem. If we ask such questions, then others will want to know the answers. If we choose problems that generate in us the raw passion and excitement with which we approached our first research problems, we will be more likely to find the solutions. Therefore, we should applaud, support, and appreciate those who pose and solve such unanswered questions—whether they are inside or outside our area, and whether they are fundamental or applied, physical or biological, or just purely chemical.

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CULTURAL CREATIVES Breadth of research backgrounds, approaches to solving problems, and intellectual interest work together to foster a creative environment, says Hartwig (seated, left), who is pictured with research group members (left to right) Jing Zhao, James Stambuli, Bjoern Schlummer, Doris Kunz, and Mark Hooper.