DOI: 10.1002/anie.201310269

Not All Research Is Equal

*Adam Heller**

Adam Heller, Research Professor at the University of Texas in Austin, received the 2007 US National Medal of Technology and Innovation

Throughout my years in industry, ventures, and academia, I have advised colleagues, students, and postdocs that they should aim, if they can, at uncovering a truth of nature; or if they can not, at creating a people-serving product.

Topmost in my research hierarchy has always been research aimed at uncovering a truth of nature. Examples of truths are Louis E. Brus' model of size quantization in semiconductor particle colloids and the existence of new forms of carbon, namely the fullerenes (Harold Kroto, Robert Curl, and Richard Smalley) and nanotubes (Sumio Iijima). Because of its sheer beauty and elegance, the uncovering of truths requires no justification. If justification were, however, required, I would point out that most of the people-serving and life-extending products, processes, and services that were introduced in my lifetime are based on truths uncovered in the past two centuries. Most, but not all, of these truths were uncovered at modest cost to taxpayers, often by individuals or small teams; only in a few fields, like particle physics and astrophysics, did important discoveries require very large groups. Review of authorship of the publications by the great scientists at the Kaiser Wilhelm Institute/Fritz Haber Institute or of pioneering researchers at Bell Laboratories shows few co-authors. At Bell Laboratories, where I worked from 1963–1964 and 1975–1988, researchers were rarely assisted by more than a single associate, even after being elected to one of the US National Academies, or being awarded prestigious prizes and medals, including Nobel Prizes. It was recognized that the uncovering of truths is more often than not thought-limited rather than labor-limited; thus large teams were rarely needed. When an important observation did require a larger team, researchers of Bell Laboratories partnered with their peers instead of relying on the labor of less-qualified co-workers. At the same time, it was profoundly appreciated that creating a product, process, or service does require a large multidisciplinary team. For this reason, when 28000 people worked at Bell Laboratories, about 27000 developed, engineered, improved on, or lowered the cost of products and services. Only about 500 researchers supported by 500 associates covered all of the physical and engineering sciences, computer and information science, mathematics, economics, and cognitive science. Their studies were rarely redundant.

Most great truths were uncovered often by individuals or small teams, not by large groups

Next in my research hierarchy is applied research aimed at people-serving products, processes, and services. Examples of processes include the purification of silicon by zone melting by William Gardner Pfann, an enabling technology of the age of silicon, and stereoregular polymerization of α-olefins by Karl Ziegler and Giulio Natta. Creators of important processes and products are just as rare as discoverers of truths. Many of the great innovators who created the bases of the world's most successful products and enterprises innovated already early on in their career, then continued lifelong quests for meeting societal needs. They rarely favored a tool that they themselves had developed. Instead, they recognized a need and chose the best available tool from a large toolbox to meet it. They switched tools, methods, processes, materials, and components whenever they learned of the existence of superior ones. Using the best available tools, they routed themselves to the best of the many possible paths to their product, process, or service. In contrast, applied researchers who aim to prove that their tool, compound, material, or method is the best, restrict the number of available paths and create, at best, noncompetitive products, losing out to the open-minded innovators.

Next in my hierarchy is research for the purpose of teaching. Both fundamental and applied research critically depend on the shaping and training of young minds. It is inconceivable that the rapid progress of our times could continue without teaching younger scientists how to carry out research. While it may not uncover a truth, nor lead to a successful product, process, or service, research for the purpose of teaching trains future researchers who might uncover truths and create products or processes. The graduate student or postdoctoral trainee considering an advisor might, nevertheless, check whether the advisor has actually uncovered a truth, created a useful product or process, or added a tool or method of value to fellow researchers.

Data-, method-, and tool-providing fundamental research is equally important as it does provide the necessary

[*] Prof. A. Heller
McKetta Department of Chemical Engineering
The University of Texas MC0400
Austin, TX 78712 (USA)
E-mail: heller@che.utexas.edu

means to those who uncover truths and to those who innovate. Truths could not be uncovered and better products could not be created without utilizing newly added data, new instruments, and novel methods. An example of a widely used tool is Heinz Gerischer's electronic potentiostat. Kary Mullis' DNA amplification by the polymerase chain reaction is an example of an important method.

The bottom rung of my research hierarchy is reserved for taxpayer-funded, chronically unsuccessful applied research. It is often pursued by large groups of laborers, headed by a fund-raising leader. Typically, after a decade or more, these groups have yet to significantly contribute to a people-serving product, process, or service. Their leaders have honed their expertise on proposal writing and report writing. They manage to have their projects judged not by the product, process, or service they produce but by publications, often reviewed and cited by equally unsuccessful peers. The number of the resulting publications is massive and their co-authors are many. An experienced fundraiser may co-author annually 20 or more publications on a currently fashionable, politically correct, subject. Leaders of these groups are often experts in a narrow field, attempting to prove that their favorite tool (a method, a material, a reagent, etc.) is exactly the right one to address the societal need. I once listened to a lecture by a heavily funded researcher telling that the favored tool provided not only for the development of superior lithium batteries, but also for solar cells and for sensors. No product or process of utility ever resulted.

Unfortunately, because the standing of researchers within universities, research institutes, and national laboratories increasingly depends on the funds they raise, scientists gravitate to politically expedient areas, away from critical, independent thinking.

Technologically naïve political leaders often appoint technologically naïve scientist-administrators

Why is nonproductive applied research persistently funded? Technologically naïve opinion leaders, particularly political leaders, are often assisted by technologically naïve scientist turned into administrators. They join political leaders in selling to the technologically least sophisticated public expensive and difficult-to-achieve ideals. These scientist-administrators rarely demand addressing first the least understood and most difficult project components, constituting the greatest risk. Nor do they demand the simultaneous addressing of the ensemble of all necessary parameters, including cost, dependability, mean time to failure, safety, throughput, and size. Public-funded applied research would be better managed by directors appointed for their proven ability to distinguish between innovative applied research and chronically nonproductive applied research. These directors should be delegated authority over, assume personal responsibility for, and held personally accountable for the outcomes of the projects they manage. They must not shift their responsibilities to outside advisors or reviewers. While they should demand the documenting of applied research results through their publication, publications and citations should be by-products of successful applied research, and not its essence.

So I am advising the political leader and foundation director to continue to appoint the fine scientist-directors who so successfully manage the fundamental research of our times. I am proposing, however, that they appoint seasoned creators of products, processes, and services to manage applied research.