Acceptance speech

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Helmut Schwarz, awardee in the Basic Sciences category (17th edition)

For someone who was brought up in a non-academic environment in a small village, it sounds like a miracle to be receiving tonight – together with eminent colleagues – this prestigious award.

After all, as a young man I didn't dream of becoming a scientist; rather I wanted to make a career as a football star only to realize, after having broken my leg twice, that I was simply not smart enough to succeed in this business.

So, what to do? After finishing school, for a number of years I worked as a laboratory technician in a big company with the job of analyzing chemical explosives. Clearly, there were some alternatives to live a more satisfying life.

Off to the university, where, however, I was torn between a career in the sciences or in law, or joining a theatre group; the latter would hold the opportunity to merge reality with the imaginary. In the end, I chose chemistry. Why chemistry? Among the natural sciences chemistry is, in my view, the most fascinating one, as its subject is not only to discover what is already there, but also to create new forms of matter and to address quite diverse and fundamental problems about, for example, the origin of life or how to cope with climate change or to feed billions of mouths. Moreover, chemistry is closely related to the arts and the engineering sciences, and I would bet anything that if Leonardo da Vinci were alive today, he would be fascinated by chemistry, as I was as a student.

During my formative post-doctoral time at MIT, ETH Zurich and Cambridge University, however, I recognized that traditional, mainstream organic chemistry wouldn't satisfy my desire to enter truly uncharted research territories.

Following Robert Frost's poem which reads "Two roads diverged in a wood and I took the one less traveled by – and that has made all the difference," I embarked on a scientific journey which most of my peers and fellow colleagues regarded as much too risky for an independent academic career. In retrospect, I have never regretted having stepped beyond well-trodden paths and having had the courage to try out entirely new things.

What scientific problems has my research team tackled? Let me briefly mention just one project which, I believe, has formed the basis for bestowing on me a BBVA Foundation Award in the Basic Sciences.

In a chemical process being catalyzed by an enzyme or occurring on a surface, usually millions of atoms are involved. Yet the actual breaking and making of chemical bonds are local affairs in which individual atoms, the so-called "aristocratic atoms" (Taylor, Schwab 1924) do the miraculous work. And a burning question for decades centered around the problem as to whether one can unambiguously identify these atoms and perform "catalysis with single atoms," and by doing so dissect the chemical process at a strictly atomic level.

To achieve this challenging goal, we modified mass spectrometers, coupled them with advanced laser instrumentation and complemented the cutting-edge experiments with high-level electronic structure calculations.

Performing experiments in the absence of obscuring, ill-defined environment effects in the "smallest test tube in the world," as a fellow colleague once described our rather unorthodox approach, has indeed provided unprecedented microscopic insight into numerous chemical processes, some of which bear quite some practical importance.

One example may suffice as illustration.

Each year millions of metric tons of methane are released to the atmosphere and a similarly large amount is flared to produce another greenhouse gas, carbon dioxide. Is there no better use for this major component of natural gas or way to valorize this precious gift of nature? We identified three main obstacles associated with the selective activation of this otherwise highly unreactive hydrocarbon, and suggested scenarios for converting methane to value-added products.

"What is your publicly funded research on electricity and magnetism good for?"

"My Lord, one day you will tax it!" Michael Faraday's famous answer to the question of the British Prime Minister of the day holds true even today.

We must not forget that practically everything we take for granted in daily life can be traced back to basic research: No GPS without Einstein's esoteric theory of general relativity; and Paul Dirac's prediction of anti-matter was in his time regarded as an entirely useless oddity. Now, decades later, almost every major hospital uses PET (positron-emission tomography) for the early diagnosis of cancer. Likewise, roughly 20% of the world economy is related in some way or the other to chemical catalysis, an initially purely academic question on how bonds are broken and made. The list of examples is endless.

So let us be in no doubt: blue sky research that, sooner or later, enables further

discoveries or inventions, and spawns applications that benefit society at large, is a public good whose funding must not be subjected to politically opportunistic considerations.

In my own academic career, our research has always been guided by a statement Max Planck made amidst the deeply depressing atmosphere hovering over Europe in the wake of the First World War. He said: "Knowledge ought to precede application." How true indeed!