PERSPECTIVES

INTERNATIONAL MICROBIOLOGY (2005) 8:137-140
www.im.microbios.org

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Fundamental things apply: the case of *Dehalococcoides ethenogenes*

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You must remember this, / a kiss is still a kiss, / a sigh is just a sigh; / the fundamental things apply, / as time goes by.
Herman Hupfeld (1894-1951), *As time goes by* (1931; made famous in the 1942 film *Casablanca*)

L'essentiel est invisible pour les yeux (Chapter 21).
Antoine de Saint-Exupéry (1900-1944), *Le petit prince* (1943)

Searching for words

Most successful applications of science, at least at some stage in their development, are the products of basic research, much of which takes place behind the scenes but is nonetheless essential for the final result. Of course, there may be examples to the contrary. George Porter (1920-2002), Nobel Laureate in Chemistry 1967, said: "Thermodynamics owes more to the steam engine than the steam engine owes to science." But the reality is that applicable results of science are generally achieved from many fragments of information. This involves a series of gradual scientific successes, which at the time the work was completed their degree of recognition by the scientific community may not have been comparable to the effort invested. Despite this lack of recognition, the research may later become fundamental to a specific application. How many published results or techniques have been forgotten for years only to be rediscovered and used later by the private sector? While purely practical, applied science may be commercially attractive, financially rewarding, and possibly essential, in the long run this approach alone will not guarantee success. It is basic science, leading to fundamental results, that makes applied research feasible and which therefore must be nurtured.

What are exactly "fundamental results"? *Merriam Webster's Collegiate Dictionary* defines "fundamental" as something "of central importance", "serving as an original or generating source", and "dealing with general principles rather than practical application".

This is, in fact, the job (or the aspiration) of basic science. The importance of a given "fundamental" contribution will depend, among other factors, on the priorities set by society and by its view of life. The value assigned to a particular field of study or the scope extended to a specific project derive from global concerns, which, in turn, determine the potential worth of basic research findings. While this may be true, it is also true that present-day priorities cannot predict either the future importance of such findings or their relevance to researchers in other fields of study, who may already be able to appreciate the potential applications of a seemingly trivial result. An early example of this was described by J.J. Thomson (discoverer of the electron), in a speech delivered in 1916 [quoted on p. 198 of *The Life of Sir J.J. Thomson*, Lord Rayleigh, Cambridge University Press, 1942]: "I will give just one example of the 'utility' of […] (basic) research, one that has been brought into great prominence by the War—I mean the use of X-rays in surgery. Now how was this method discovered? It was not the result of a research in applied science starting to find an improved method of locating bullet wounds. […] No, this method is due to an investigation in pure science, made with the object of discovering what is the nature of Electricity." Also, as C.H. Llewellyn Smith wrote: "the reasons we have practical computers now, and did not have them 100 years ago, […] is because of discoveries in fundamental physics which underwrite modern electronics, developments in mathematical logic, and the need of nuclear physicists in the 1930s to develop ways of counting particles" [What's the use of basic science? by C.H. Llewellyn Smith, former Director-General of the CERN. At: http://public.web.cern.ch]. Such examples are countless.

So, basic research leads to fundamental results that may later be used in developing and applying technological improvements (Fig. 1). However, results that are or will become

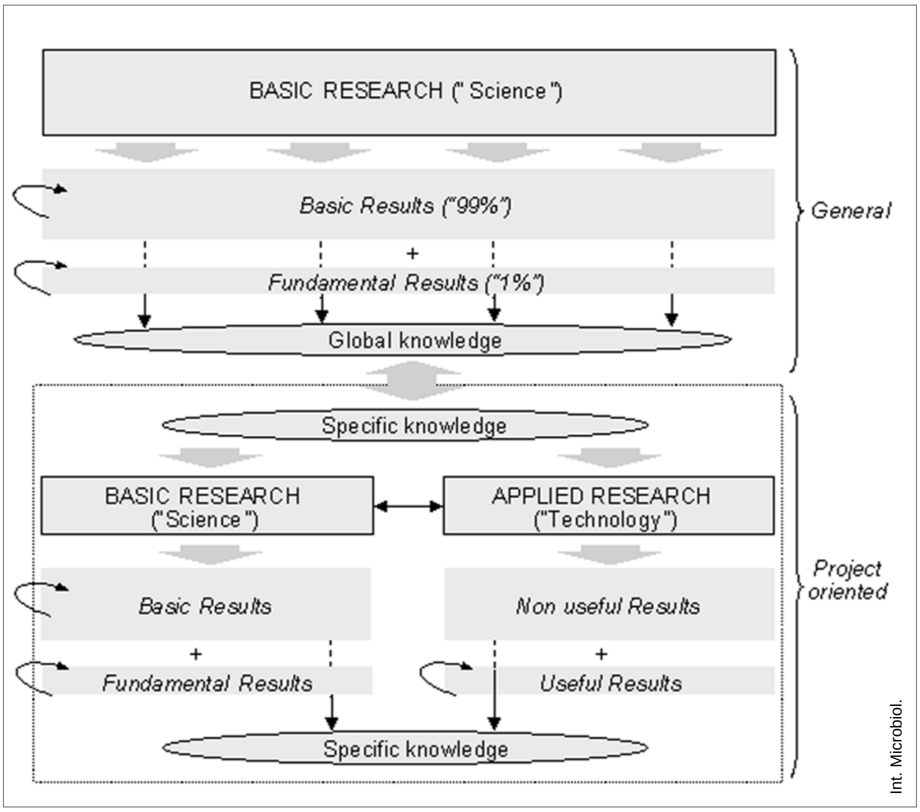

Fig. 1. Roadmap to the application of basic science.

fundamental are not usually predictable. Might Watson and Crick heve predictal the cloning of dinosaur DNA? Did Rutherford foresee nuclear power? Since this was obviously not the case, then why do institutions controlling research funds exert such powerful control over deciding what should be investigated? A few lines of applied research will clearly deserve right of passage, but by no means should the bulk of basic research funding be obliterated by a short-term, financially or commercially derived vision of knowledge. The constant four-dimensional (vertical, horizontal, in depth, and across time) transfer of scientific information rules out the prospect of a world researched only by applied scientists to the detriment of basic-research scientists, as postulated by some. Carl Sagan (1934-1996), in a meeting at Cornell University celebrating his 60th birthday, two years before his early death, conveyed a clear message: nothing applied may be pursued successfully without its fundamental base being well-developed. Basic research is the key to obtaining fundamental results that, at some point in time, will become essential to progress in applied research and to the well-being of society. As a consequence, public institutions should pursuit a merit-based approach to supporting basic research (fundamental results do not yield immediate revenues), which means that long-term investments with an assumed long payback must be made using tax revenues, with the payback occurring later in the form of either a financial return on the initial investment or an application that improves society's well-being. If this strategy were followed, both basic science and applied research would thrive.

A case report

Within my previous field of study (microbial ecology), results compiled from decades of basic research have been fundamental to a wide variety of applications, including gold biomining (*Thiobacillus ferrooxidans*), the production of biodegradable plastics (polyhydroxyalkanoates-producing bacteria), the improvement of rumen and soil-plant-microbe symbiotic interactions, sewage clean-up, and bioremediation of recalcitrant toxic compounds in contaminated sites.

On January 7, 2005, a paper was published in *Science* [7] reporting the genome sequence of *Dehalococcoides ethenogenes* strain 195 (Fig. 2), the only bacterium known that is capable of reductively dechlorinating tetrachloroethene (PCE) and trichloroethene (TCE)—two cancer-producing groundwater pollutants—to the non-toxic form ethene. Deciphering the *D. ethenogenes* genome sequence contributes to a better understanding of the complex nutrient requirements, respiratory processes, phylogenic relation-

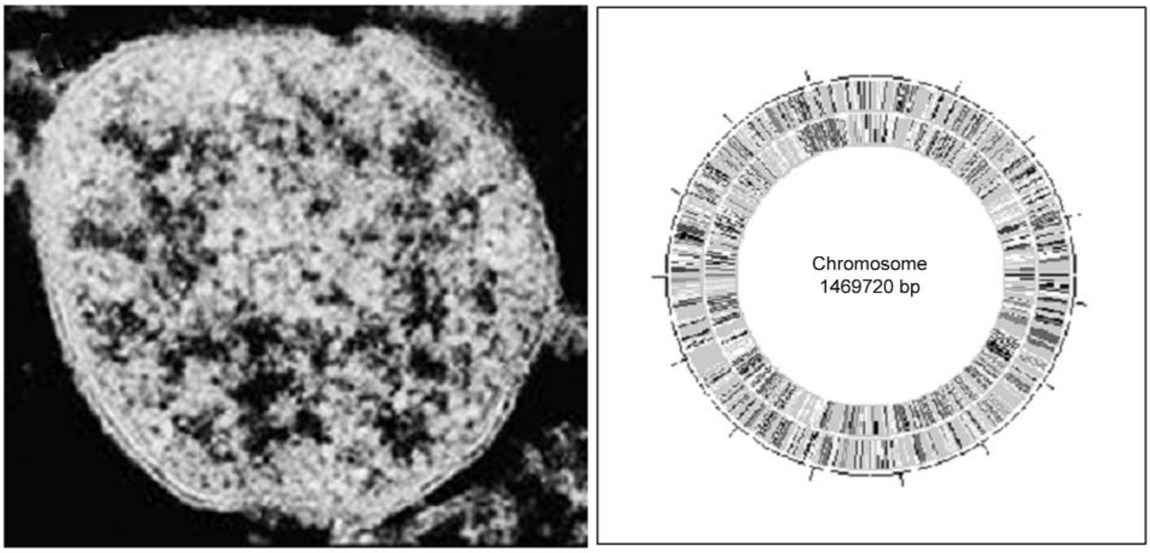

Fig. 2. *Dehalococcoides ethenogenes* strain 195. Mechanism and genes in a schematic genome (See *Science* 7 Jan. 2005, p. 107.)

ships, and enzymatic reactions of this bacterium, and lays the foundations for synthesizing specific probes and for genetic studies. The *Science* article has surely improved our scientific knowledge on how life works at some of its smallest levels, but it will also allow researchers to elucidate the regulatory network involved in the dechlorination of PCE, TCE, both dichloroethene (DCE) isomers, and vinyl chloride (VC) to innocuous compounds. Together with previous studies [1–6] of the growth requirements and other characteristics of *D. ethenogenes* strain 195, the basis for the effective bioremediation of PCE-, TCE- and DCE-polluted environments has been established.

The successful use of *D. ethenogenes* in bioremediation is the result of both basic and applied science, in an essential, intense, and extended team effort that has been going on for more than 15 years. In 1997, I isolated *D. ethenogenes* strain 195 [4], the genus-defining strain, while carrying out doctoral thesis research under the direction of Stephen H. Zinder, at the Department of Microbiology of Cornell University. Our progress would have been slower and less fruitful if it were not for continuous collaboration with the laboratory of James M. Gossett, of the Engineering Department at the same university, more involved in applied science. The professional relationship among the two laboratories made it impossible to divide the work into basic and applied efforts. Working as a multidisciplinary team not only improved the quality of the output, but also—as the published articles show—allowed us to maximally benefit from results, derived either directly or indirectly, of basic research carried out over the previous decades. This earlier research, comprising studies on methanogenic and acetogenic bacteria, anaerobic culture enrichment techniques, and the microbiology and biochemistry of electron donors and acceptors, supplied crucial information, without which neither the remediation of environments polluted with chloroethenes nor the sequencing of the D. *ethenogenes* strain 195 genome would have been possible.

Final thoughts

The *Casablanca* song says, intelligently, "the fundamental things apply, as time goes by". In science, this should not be interpreted as meaning that everything considered fundamental must have a practical application, or that everything resulting from applied research is directly derived from basic research. It does imply, however, that we must strongly support and encourage basic science in order to improve the general social welfare and living conditions on Earth. The fact is that, regretfully, we walk through our days immersed in a society ruled by governments and financial markets that, because of their chronic short-term interests, are only willing to understand fundamental efforts in the light of their applied and thus almost-immediate commercial potential. Needless to say, this is not a good strategy for facing the future.

Public institutions must support basic research regardless of its less than probable immediate returns (and also because of this). However, this is not an opinion likely to win votes, nor does it produce quick economic and financial gains. Nevertheless, the results that society expects from applied science depend on the priorities that society sets for itself. By neglecting or annihilating basic science research, we shut ourselves out of a better world if our priorities, as a whole, were to change someday (and let us hope they will). Should we continue to pursue material prosperity only out of individual interest? Or should we also aim at achieving further cultural and education advances? As Robert Wilson (1914-2000), first director of the Fermilab, stated in front of a Congressional Committee, "my research may not contribute to the defense of our country, but it will make it worth defending" [What's the use of basic science? by C. H. Llewellyn Smith, former Director-General of CERN. At: http://public.web.cern.ch]

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