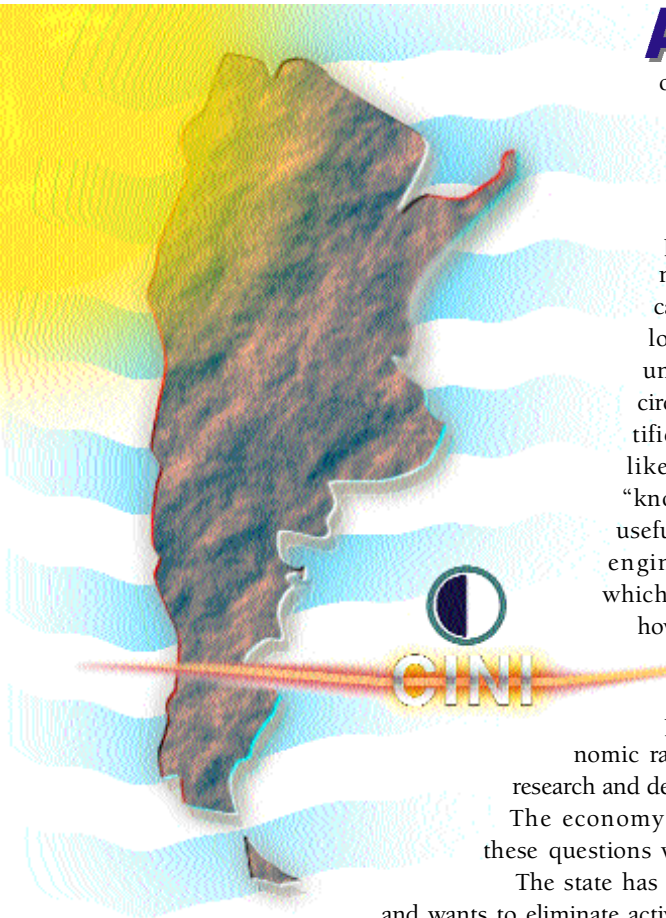


A Theoretical Physicist in Argentine Industry

FEATURE

by Alberto Pignotti



Although I began my career as a theoretical particle physicist, I have spent the past 22 years working for one of the major industrial corporations of Argentina. In a sense my career has been a prolonged attempt to understand under what circumstances the scientific approach, which I like to say emphasizes “know-why,” provides a useful supplement to the engineering approach, which emphasizes “know-how,” and to learn how to choose areas of research that provide a sound economic rationale for sustained research and development.

The economy of Argentina poses these questions with particular force.

The state has run out of resources, and wants to eliminate activities or, where possible, limit state participation in activities that can be undertaken by private parties. Support for applied science must inevitably come from the industrial sector. At the same time, the marked trend to open up the economy to competition from abroad provides local manufacturers with a strong motivation to achieve international standards of quality and competitiveness. Doing so will require deep structural changes, but also, I would argue, the adoption of an approach to manufacturing based not just on “know-how” but also on “know-why.” My own work experience illustrates some of the important issues involved.

Entering the industrial arena

After working for many years as particle physicists, a colleague and I switched overnight from the academic world to the industrial one: we went to work for Techint (Buenos Aires, Argentina), a corporation whose main areas of expertise are engineering and the steel industry.

At the time, Techint was involved in the construction

of two huge railroad bridges across branches of the Paraná River, which flows from the southern tip of Brazil south-southwest into the Río de la Plata in Argentina. Our first assignment concerned buckling and elasticity problems that emerged during the design of steel plates critical to the bridges’ structural integrity. Because of the large loads involved (not only were the bridges to carry cars, trucks, and trains; the span between pillars was 300 meters), these problems fell outside the relevant tables in the engineering handbook, so that it was not sufficient to know how to use the tables judiciously. Instead one had to know how the tables had been generated so as to be able to extend the results to the case at hand. This is an excellent example of the difference between the pragmatic “know-how” approach to problem solving and the harder but more powerful “know-why” approach.

Of course, when we began, we didn’t have the faintest idea how the tables had been generated. To be truthful, we didn’t even know the technical meaning of the word “buckling.” However, it did not take us long to find out that buckling is an eigenvalue problem, and, because eigenvalues are the bread and butter of quantum mechanics, we ended up solving the problem with quantum mechanical techniques. One surprise from this, my first encounter with applied science, was the discovery that, even before the problem was submitted to us, it had been “solved” by the design engineer, by the efficient method of applying a safety factor of two.

But one important fact is that, in the intervening 20 years no similar bridges have been built in Argentina. It did not make sense for our company to develop expertise in basic bridge design in order to meet the demands of a market of one bridge every 20 years. Indeed, what the company normally does in ventures of this type is to hire a top-level foreign consultant with expertise in the required field. This is a sensible approach and to fight it is to fight a losing battle—except, perhaps, if the emergence of a common market changes the scale of the market to which Argentinean companies have access.

Attempting applied research

After this first experience, the course of events involved me in projects that could hardly be called scientific. Rather I was required to develop software tools for nuts-and-bolts engineering design. This lasted for a few years, and I found it personally rewarding, both because I was doing something useful and because I was exercising some of my skills. This was a legitimate use of

my training, and many physicists have managed to prosper by exporting to other fields their ability to use computers. However, since I would like to emphasize here the application of physics to industrial activities, I will not elaborate on this phase of my career.

Some time later, again for circumstantial reasons, I drifted into the field of heat-exchanger design. In this area I again put to work my background as a theoretical physicist, and it turned out that I was able to generate original results, based essentially on symmetry arguments and formal similarities between heat exchange and the scattering of elementary particles. (Improbable as it might seem, the performance of a car radiator is not unrelated to the scattering of pions by protons.)

Eventually, I became an expert in basic heat-exchanger design and published several papers on this topic in international journals—after some initial skirmishes with referees. I was even invited to be a keynote lecturer at the 8th International Conference on Heat Transfer, held in 1986 in San Francisco, the only such lecturer from Latin America. Thus, my work on heat exchangers looks like a textbook example of cross-fertilization between different disciplines, a good example of the sort of problem that allows a physicist to thrive in an engineering context.

A more realistic assessment of this experience, however, would acknowledge that by the time I had turned into a heat-exchanger expert, my work had become so specialized that it was totally irrelevant to my company's business. Not one heat exchanger was designed in Argentina using my findings. At some point the Heat Transfer Research Institute of Alhambra, California, became interested in my results, and I even worked out a formula for heat-exchanger effectiveness for them. This was a novel experience for a former particle physicist: nobody had ever purchased a formula from me before. But for my company, for which a nine-figure contract was not unusual, a four-figure formula was not a particularly significant landmark.

Center for Industrial Research

So far, I could summarize my experience as a physicist in a leading Argentine engineering company by saying that I had succeeded both in doing useful work that was not research and research that could hardly be justified on the basis of its industrial impact. This should not be taken to mean that I don't think it is possible for physicists to perform significant industrial work. My point is that there are pitfalls, and that the choice of an

area of research is a delicate one that should be carefully studied.

With this point in mind, about 10 years ago, my colleagues and I began to promote the value of research for industrial projects that meet the following three criteria:

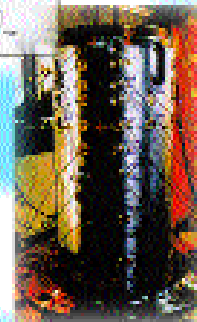
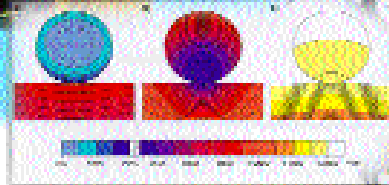
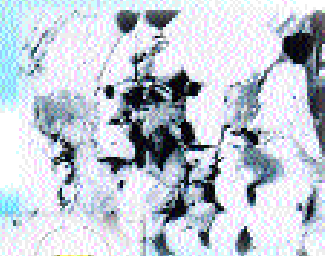
- They require modern technology
- They address sizable markets
- The markets are competitive—that is, companies compete on the basis of quality and price.

I would argue that all three factors are needed to ensure a sound basis for sustained R&D activity. For example, even if there is a sizable market for a technological product, if the market is not competitive because of protectionist policies, it will be difficult to convince industrial management to devote resources to research and development. If a profit can easily be made, “know-how” is enough, and there is no need for “know-why.” And with no support from the industrial sector, no R&D program stands on firm ground.

Within Techint, these conditions were satisfied by Siderca, an integrated mill that manufactures steel pipe, mostly for the oil industry. (Techint is composed of many financially and administratively independent companies.) At the time, Siderca was in the process of switching from an operation oriented to the local market to one aimed at international trade. Since the plant had to compete with manufacturers the world over, this was an ideal setting for the development of a pilot industrial R&D program.

Before they would sponsor full-scale R&D activity, our top management had to be convinced that it was worthwhile. For persuasion to work at all, one must have an enlightened management, which is not easy to find, and even enlightened management needs some means of evaluating the ability of an R&D group to serve its purposes. We were lucky to have a chief executive officer with a Ph.D. from the Massachusetts Institute of Technology, an unusual credential in Argentina, and he helped create an atmosphere receptive to R&D. In addition, we found a far-sighted middle manager, who, on his own responsibility, sponsored an initial program with a modest budget.

For a couple of years, we kept a low profile and man-



aged to achieve moderate success. We were based at the engineering company that had hired me originally and worked on a charge-back basis, mostly on problems of interest to the steel-tube manufacturing plant. We hired expert consultants as needed, often physicists who had been trained abroad in metallurgy, materials science, and other disciplines. Some initial successes were influential in the company's later decisions about our project.

Eventually, our low-profile operation came under close scrutiny. We had to justify our existence by proving that the investment had been worthwhile. We were audited by an external consultant, a distinguished American professor in the field of materials engineering, who was hired to review our research programs and, we heard indirectly, to "quench the boiling spirits" of our unruly group. Oddly enough, given our fear of being perceived as too academic, this evaluation concluded that we were too involved in plant operations and that our Center for Industrial Research was in danger of becoming no more than a sophisticated process-engineering group.

Achieving independence

Eventually, to give the research center some independence from the manufacturing plant, a foundation was created and put in charge of the administration of the center. The original sponsors of the foundation were five different industrial companies, all of which belong to Techint, and the branch of the National Technological University located in the city of Campana, less than 50 miles from Buenos Aires.

The Center for Industrial Research has grown and prospered. Like all human enterprises, it can be criticized and improved, but these days nobody questions its existence. It has an annual budget of more than \$4 million, a permanent staff of five senior research scientists, some junior staff, and a fluctuating population of graduate students. At present our laboratory facilities cover our needs only partially, but lab tests are performed at various institutions in Argentina and, occasionally, abroad. The center focuses on improving manufacturing processes, understanding the relation between operating parameters and product quality, and evaluating the quality of manufactured goods. The techniques used include mathematical modeling, laboratory tests, and plant tests. The work draws on materials science, metallurgy, physics, numerical modeling, and signal processing, among other disciplines.

How can one measure the success of a center of this type? In some cases it is possible to attach a price tag to a process improvement. One example is the replacement of a quench-and-tempered steel by a steel with a different formulation that has similar properties but is made by a cheaper process. In such a case, one can

claim that so many cents or dollars per ton have been saved. In many cases, however, it is difficult to measure success in strictly economic terms. What if the effect of the work is to improve the quality of the product without lowering its cost? How much is this improvement worth? What if it influences the decision of a major oil company to sign a contract with your corporation rather than a foreign competitor?


Although it is genuinely difficult to evaluate a research program, we at the center also recognize that we are usually not too keen on pursuing this type of analysis. We find our satisfaction in the technical improvements and tend to neglect to measure their economic impact. We have to learn to overcome this tendency. We must make a deliberate effort to sell our product, in order to improve our chances of gaining support for future endeavors.

Lessons learned

Again, I would stress that only a sizable and competitive market provides a sound basis for applied research in physics. Although industry in developing countries such as Argentina is not used to sponsoring this type of research, it will be forced to in order to survive in the global markets of the future. But the Argentine case only poses in a particularly clear form problems that also typify applied research in developed countries.

In conclusion, I would like to mention one apparently paradoxical requirement for the success of an applied research program: it must meet the highest standard of academic excellence. Excellence is needed not only to ensure that a team has the technical skills required by complex modern technological problems, but also because it is the best guarantee that these problems will be approached in an open, honest, objective way.

Acknowledgments

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