On the phenomenon of scientific disciplines

In this note we regard scientific disciplines as our modules of human knowledge and abilities, because this metaphor seems the simplest way of convincing the computing scientist that the whole of human knowledge and abilities should be appropriately partioned.

For a scientific discipline to be viable, the area it covers must meet a number of constraints. It must not be too big: the core of its knowledge must fit into a single person's head and - our abilities requiring the maintenance of daily exercise - the required abilities should not be more than we can acquire and maintain. It should not be too small either: the area should be rich enough to keep us happily engaged for more than a lifetime.

To the experienced system designer, this sounds all very familiar: too large a module is too hard to manage, and too small a module is not worth creating as a separate entity.

Besides those quantitative constraints, there are qualitative ones. There is the internal requirement of coherence: the abilities should enable us to improve our knowledge, the knowledge should enable us to perfect our abilities. There is the more external requirement of isolation: a scientific discipline should be a world unto itself, as independent as possible from the rest. The degree to which a topic can be studied in isolation is a major test for how appropriately the partitioning has been chosen.

To the experienced system designer, this too sounds very familiar. He recognizes "information hiding", alias "abstraction", and furthermore knows full well that an inappropriate modularization can drastically complicate life at both sides of the fence. This phenomenon of mutual complification is (regrettably) also only too familiar to the ordinary citizen, who has to comply with all the rules and regulations of the various "services" of his government: first the citizen has a hard time filling out all the forms, and then the service has a hard time checking all the data.

A new class of considerations enters the picture when we take into account that viable scientific disciplines exist for long periods of time, during which they first emerge and then evolve. (Some of them we have seen disappear for lack of further interest in the area.) Notice that these long periods are intrinsic: no substantial body of knowledge was ever concocted on a rainy afternoon and the scientific enterprise is intrinsically a very long-range one.

Consequently, this restricts the area of a viable scientific discipline to those in which the proverbial "lasting contribution" can exist, and those concerned with the evolution of a viable scientific discipline don't waste their efforts on anything the obsolescence of which is in sight.

These days, one often encounters the opinion that "scientific progress is so fast that a well-trained scientist's half-life is of the order of 5 years". What nonsense! If, indeed, a scientist's half-life is only 5 years, that should not be taken as a symptom of the speed of scientific progress, but of the deterioration of an educational process that crams the poor graduate's head with perfect ignorabilia. It is a symptom of teaching divorced from a scientific discipline, of which it is a major purpose to prevent such waste of effort.

Before continuing this discussion of the phenomenon of scientific disciplines, I must acquaint you with the "Buxton Index", named after John Buxton, who discovered that a - if not the - most relevant one-dimensional scale along which to place and compare individuals, organizations, industries, and movements is to how far in the future their planning extends; the Buxton Index expresses the length of that period, measured in years. Its significance is that cooperation between persons or groups with very different Buxton Indices leads to mutual (moral) reproaches as long as the partners are unaware of the difference: the partner with the small Buxton Index is accused of short-sightedness and opportunism, the one with the large Buxton Index is accused of hobbyism and a lacking sense of responsibility. The Buxton Index being just a number without moral values attached to it, knowledge of it can greatly contribute to clearing up the sky.

I mentioned the Buxton Index because it gives me a way of discussing in a detached manner the love-hate relation between the world of science and the world "out there", and the closely related schism between "pure" and "applied" science. (In some societies that schism is stronger than in others.)

The relation is often uncomfortable. The world "out there" presents problems to which - if they are solvable at all - only science can offer an effective solution. This dependence on science is quite often resented. (Over and over again one will hear unsupported claims that an unscientific shortcut will do the job just as well or even better. These claims, and the ready audiences that can be found for them, are symptoms of this resentment.) When, in response to such urgent challenges, science seems to ignore them and to go quietly its own way, this may be interpreted as adding insult to injury and further fuel the resentment.

Science, from its side, is not attracted to problems it is not ready for, is repelled by a customer base that prefers to listen to the quacks, and, in exasperation, will point out that many "real-world" problems only still exist because its teachings have been ignored. In short, a fertile ground for drama and conflict.

The resolution of the conflict requires the understanding of two things, the first one has to do with the difference in Buxton Index. On the time scale of the scientific enterprise, utility criteria, as dictated by the fads and fashions of the fickle public, lack the necessary stability to be used for long-term guidance: one day, robotics has to save our economy, the next day it is the biggest threat to full employment. The other thing we must understand is why science has so often been so extremely successful, why it can provide answers and solutions that cannot be obtained by any other means. The explanation has always been that science has stuck to its own rules.

No scientific breakthrough has ever been obtained because some benefactor ordered it. Besides hard - sometimes even very hard - work by its most gifted practitioners, a flourishing and vigorous scientific discipline requires a very carefull choice of what to try. The successful scientist is very pragmatic -otherwise he would probably not be so successful-. He knows he should not waste his effort on trivia in the sense that, if he does not do them, someone else can be trusted to do them. He also knows he should not waste his time on trying the impossible. Consequently, after very careful evaluation, he embarks on what he sees as in all probability just possible. It is in a subtle balance between courage and cowardness, between vision and caution that the improbable is achieved. Science is more opportunity-driven than demand-driven. It has to be, because this is one of the corner stones of its success. This is why each successful scientific enterprise always emerges as a so strongly autonomous activity. A society eager to reap the benefits from the science it supports or tolerates should leave to its scientists the choice what to tackle, and it will be rewarded many times over; needless to say, this is anathema to the managerial mind. [E.T. Bell remarks about some European monarchs that "these rulers were clear sighted enough to see that the simplest way of getting mathematics out of a mathematician is to pay his living expenses."]

We have seen the large Burton Index as an intrinsic characteristic of scientific disciplines; this is the place to point out that it does not come without a price: in their course, our scientific disciplines have as little flexibility and as much inertia as intellectual supertankers. Our scientific disciplines evolve, the attention shifts from old topics to new ones, and it is in particular the increasing mastery of the new topics that gives the feeling of progress. But the inertia is enormous, and from a somewhat larger distance the progress quickly looks like "more of the same": the scientific discipline remains steady on its course.

The steadiness was eloquently reflected in an interview with Herbert Robbins, in which he explained why he did not expect mathematics to change dramatically during the next century, computers or no computers. His argument was that mathematicians had always worked at the limits of the capacity of their brains, that the human brain had not changed much over the last 5000 years and that it was a safe prediction that it would not do so over the next 100. Fair enough. But at the same time the argument reveals what would be involved in revolutionizing mathematics: a drastic reduction of the reliance on our brains, e.g. by replacing more and more of our human reasoning by calculation and so letting the symbols do the work.

In the same book – "Mathematical People" – Donald E. Knuth – I quote by heart as someone borrowed my copy – remarked about Computing Science that it had provided a home for otherwise homeless mathematicians (like him) whose interests – and way of working? – deviated too much from what contemporary mathematicians are used to.

At least speculatively, I am willing to go a lot further. We <u>are</u> ready to realize most of Leibniz's Dream, we <u>are</u>, in fact, realizing more and more of it by designing and learning how to use the formal techniques that offer an alternative that, in effectiveness, is far superior to traditional mathematical reasoning. The revolution that Robbins excluded is in full swing.

More exactly, Robbins did not exclude it; he only said not to expect mutiny on his ship, and it looks as if he is right. The speed with which cultures – scientific subcultures not excluded – can absorbe change is, even in the case of progress, strictly limited, the mathematical supertanker proceeds on its steady course as supertankers have to, and it required a new scientific discipline to realize the Dream of Leibniz.

That the job fell to the scientific discipline also known as Computing Science is not surprising: (i) computing scientists are in daily need of the effectiveness only formal techniques can provide, (ii) professionally, computing scientists are quite familiar with the manipulation of uninterpreted formulae, and (iii) theirs is the technology of mechanizing the manipulation (if such mechanization is needed).

From Robbins and his confraters on the supertanker we can expect the protest "But that is not what mathematics is about!". Right! That is precisely why a new ship had to be built. If we honestly believe in our mission we should answer "But it is what mathematics will be about a century from now.".

All the above was written for my own illumination. I needed a satisfactory answer why computing science research is each time drawn to mathematical foundations. The conclusion —drawn on other grounds— that we are in the process of realizing the Dream of Leibniz provides a more than satisfactory answer.

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