Sociological problems of high-energy physics

from Andrew R. Pickering and W. Peter Trower

Large-scale collaborative projects between experimental high-energy physicists have produced sociological problems that will need thought to resolve.

TEAM research entered the study of elementary particles in the immediate aftermath of the Second World War. The collaborative methods which had borne fruit in the Manhattan Project were carried over into the new speciality of high-energy physics. The most visible early manifestation of this organizational transfer was at the Radiation Laboratory of the University of California, Berkeley, where E.O. Lawrence's pre-war development of the cyclotron had led to a central role in atomic weapons research[1]. As R.R. Wilson, the first director of the Fermi National Accelerator Laboratory, recalled in 1970, "A word was even coined — Berkeleitis — to describe the syndrome that existed there"[2].

Despite the misgivings of physicists who harked back to the pre-war days of individualistic research, Berkeleitis soon became endemic to high-energy physics, growing more rampant with the passing years[3]. Until the mid-1960s it was still possible for a small group of, say, less than ten physicists to make a significant contribution to high-energy physics; by the late 1970s the complement of a typical collaboration had grown to around 50. Today, groups of around 200 physicists are assembling in preparation for experiments at Europe's next new big machine — the Large Electron–Positron Collider (LEP), due to come into operation at CERN, Geneva, in 1987.

As collaboration size has grown, so too has the duration of typical experiments. In the 1960s an experiment could be mounted and data analysed within a period of months, while current experiments can span more than a decade from first conception to final publication of results. Thus, in terms of both personnel and duration, high-energy physics experiments have moved ever further from the pre-war stereotype of the lone researcher at the laboratory bench, and have come more to resemble large-scale engineering projects.

Trends

The underlying trend towards larger collaborations and extended timescales in experimental high-energy physics is not hard to understand. The relatively generous funding which high-energy physics has enjoyed throughout its history has made possible the construction of a succession of particle accelerators of ever-increasing size, and sophisticated experimental techniques have been developed to exploit the high-energy beams thus made available. Instead of a single detector, a modern experiment deploys a complex multi-element array, costing millions of dollars and requiring an army of specialists to design, build and run. Considerable resources are also required for subsequent data analysis.

Neither is it hard to appreciate the benefits of this form of research. The discoveries at CERN of the electroweak intermediate vector bosons, $W^{\pm}$ and Z^0, made by two groups totalling nearly 200 physicists, were the culmination of a decade of rapid progress in understanding elementary-particle interactions. But such benefits have been bought at a price. Our aim here is to outline the sociological problems which are the symptoms of Berkeleitis. Our discussion is organized around twin themes: the frustration of individual initiative and creativity within large collaborations, and the consequent tendency to conservatism and orthodoxy in communal practice. (The frustrations inherent in team research have been impressed upon us in informal communication with many experimenters. It seems clear that the present trend towards non-accelerator experiments (for example, searches for free quarks, magnetic monopoles, proton-decay) is, in part, a manifestation of these frustrations.) We will show that the problems arising within individual experiments spill over into programmes of experiment. They are reinforced by the institutional structure of experimental high-energy physics, and have been further exacerbated by conceptual developments in the field over the past decade. Finally, we ask whether anything can be done to check current trends to gigantism and orthodoxy in high-energy physics research.

The rationale for team research is the division of labour. A large collaboration comprises several groups of physicists, each drawn from a single university or laboratory. In setting up and running an experiment, each group takes on responsibility for particular elements of the overall apparatus. Data analysis is likewise broken down into component tasks allotted to different groups. Thus, along with the division of labour goes a degree of specialization beyond that normally associated with scientific research.

This is where sociological problems begin to arise, particularly for junior physicists — postgraduate students and young postdoctoral researchers. Even within a single group of experimenters, research tasks are subdivided. And junior physicists are usually those responsible for the basic items of hardware and software. They thus acquire detailed knowledge of only one small part of an enormous and diversified project, and their chances of making a significant contribution to the overall course of the experiment outside their area of expertise are slight.

Managers

Senior physicists encounter a similar problem, but for different reasons. They oversee and coordinate the work of a group or groups, and are thus obliged to take on administrative responsibilities more familiar to managers of large technological enterprises. Another function of senior physicists is to secure the material resources necessary for the success of the experiment, and to negotiate with laboratory managements for beam-time, workspace and so on. Here they function as entrepreneurs. Again, even at the most senior level, the role of creative scientist can be submerged for long periods of time (if not forever) by the multifaceted organizational demands of a large collaboration.

In a variety of ways, then, many physicists find their creative ambitions and aspirations frustrated in the course of high-energy physics experiments. And, all things being equal, opportunities for innovation decrease in proportion to the degree of specialization and the associated administrative load — that is, in proportion to collaboration size and the duration of the experiment.

Only in a field where long experiments were commonplace, though, would one even look for creative opportunities within the development of a single experiment. The classic locus for creativity in experimental science lies in the gaps between experiments: in the conception of new techniques or new problems to tackle. But here too Berkeleitis has a deadening effect. One can discern at least two sources of conservatism in experimental design in high-energy physics. The first is that specialization is self-reproducing. As discussed above, in his research training, the young physicist acquires a narrow, specialized competence in some limited aspect of experimental high-energy physics. This competence then becomes his most precious asset in the pursuit of a research career, so valuable (to himself and to others) that opportunities for future variety in research practice are highly circumscribed. And the upshot of this is an inhibition of technical innovation within high-energy physics experiment as a whole.

Since techniques are usually appropriate to the exploration of a limited class of phenomena, technological conservatism itself implies conservatism in the choice of research problems, and both forms of conservatism are reinforced by a second source: the fear of failure. The senior physicist, or physicists, who propose to conduct a contemporary high-energy physics experiment risk not only their personal reputations, but also the efforts and careers of the many less senior physicists who will be involved in the project. If the experiment fails — if it does not produce interesting data or, indeed, any data at all — hundreds, possibly thousands, of man-years of effort and millions of dollars will have been wasted. Not surprisingly, therefore, there is a tendency to conservatism in experimental programmes, with new experiments aiming to investigate phenomena of well-established interest using well-established techniques.

Rewards

So far we have outlined the obstacles to initiative and creativity implicit in collaborative research of long duration: the twin requirements of specialization (with its self-reproducing character) and administration, and the ever-present risk of failure. In themselves, the impact of these factors can be overestimated. Against them must be set the potential rewards for successful innovation — symbolic rewards like Nobel prizes, and material rewards such as career advancement. There are, however, institutional factors in high-energy physics which serve to structure even successful innovations, to foster a limited set of technological developments or problem choices at the expense of others. This, too, is a form of conservatism, which again acts to stifle individual initiative.

The origins of institutional conservatism in high-energy physics lie in the centralization of research resources. Despite generous funding, the expense of building and running particle accelerators has long been beyond the resources available to individual universities. Over the history of the field, the facilities for high-energy physics experiments have been gathered together into a handful of regional, national and international laboratories at which collaborations assemble to perform their experiments before departing to their home universities for data analysis.

Together with the centralization of research resources has gone a proliferation of committees. Access to experimental beams and funding for experiments are controlled by committee, likewise planning and funding for new facilities — new accelerators or new major items of experimental equipment. Thus any potential innovator is faced with an institutional hurdle. The typical disposition of committees towards conservatism is well known, and the experimenter who does not propose to tackle an established problem with established techniques is likely to find his proposal rejected.

As Nobel Laureate Luis Alvarez put it: "Our present scheduling procedures almost guarantee that nothing unexpected can be found"[4]. As it happens, there is a twofold irony to this quotation. In his leadership of the 72-inch bubble chamber programme at Lawrence's Radiation Laboratory, Alvarez did more than any other to demonstrate the benefits of Berkeleitis in experimental high-energy physics[5]. (For a discussion of the Alvarez group, see ref. 6.) And at the time when he wrote, 1973, high-energy physics was characterized by a pluralism of research strategies that was soon to vanish.

The 1970s saw the rapid development of the 'new physics' world-view in high-energy physics, in which the world was seen to be built from quarks and leptons interacting according to the dictates of twin gauge theories — quantum chromodynamics (for the strong interaction) and the Weinberg–Salam–Glashow unified electroweak theory. And the sociological correlate of the 'new physics' was the 'new orthodoxy' (as it was christened by high-energy physics theorist James Bjorken)[7]. Within the new orthodoxy, communual research practice in high-energy physics became almost exclusively organized around the new-physics world-view. The effect was most striking in the experimental fields, where the institutional committee structure reflected the majority view and effectively enforced the new-physics dominance of research. Since the late 1970s it has been difficult, if not impossible, to mount a high-energy physics experiment which does not promise to engage directly with the interests of gauge theorists.

One consequence, then, of the rise of the new orthodoxy has been a close circumscription of opportunities for initiative in the choice of topics for experimental investigation. Along with this restriction on problem choice have gone restrictions on acceptable forms of technical innovation. As currently elaborated, gauge theory offers an analysis of only a limited range of rare phenomena. Investigation of those phenomena (in the presence of an overwhelming background of 'uninteresting' processes) requires large, complex experiments and highly sophisticated electronics — precisely the experiments which call for large collaborations working together over a period of years. Larger and larger teams have assembled to man the ever-larger experiments expected to probe the fine details of already rare phenomena, leading to the projection of 200 member teams for LEP.

Thus, more than any other factor, it has been the new physics (via the new orthodoxy and the institutional structure of high-energy physics) which has driven the trend to gigantism in experimental physics throughout the 1970s and into the 1980s. And, as an unintended consequence, it has multiplied all of the sociological problems discussed above. The degree of specialization in the experimental physics community, the extent of administrative responsibilities and the risks attached to failure have all increased with the coming of the new physics. Opportunities for initiative and creativity have decreased in proportion.

The new physics, then, through the grip of the new orthodoxy on experimental high-energy physics, is experienced by many physicists as making the life of the researcher more frustrating and less rewarding. What of the future? At present, the new orthodoxy, with its attendant drive to gigantism and the multiplication of sociological problems, is set to reproduce itself indefinitely. Through the institutional structure of high-energy physics, the future as well as the present of experimental research has been given over to the new physics. The next generation of big machines — at which the next generation of experimenters will learn their trade — are explicitly conceived as new-physics facilities. LEP, for example, is intended to operate as an intermediate vector boson factory (see, for example, ref. 8).

The state of high-energy physics is therefore not entirely satisfactory, despite the conceptual triumphs of the new physics. The existence of a self-perpetuating orthodoxy is anathema to many physicists, especially an orthodoxy which multiplies pre-existing sociological problems by further stifling opportunities for individual initiative. The question arises: can anything be done to ameliorate this situation? Clearly, a return to the pre-war days of the lone researcher is impossible — Berkeleitis is intrinsic to the technical and institutional fabric of experimental high-energy physics — but can the slide into gigantism be checked (for those who wish it)?

Here there is a straightforward suggestion. The institutions of high-energy physics should be used to relax rather than to enforce the stranglehold of the new physics upon experiment. A fraction of the available resources should be set aside for those who would, for one reason or another, follow a heterodox path. In this way, the oppressive aspect of the current orthodoxy could be *de facto* eradicated. And, since it is the new-physics emphasis on rare phenomena which is driving the present increase in collaboration size and experiment duration, the route would be open for individualistically inclined researchers to move towards their chosen form of practice. Unconventional paths of inquiry could be followed, novel small-scale experimental techniques could be developed and old techniques rescued from oblivion, all with unpredictable but possibly major consequences for future patterns of research.

It is important to stress that the argument is not that the new-physics world-view should be abandoned — its virtues are too well established for that — but simply that some opportunities should be left for research outside the orthodoxy. (It is worth recalling that it was precisely such heterodox research that led to the 1974 discovery of the J-psi particle and the establishment of charm — an episode that marked a watershed in the development of the new physics.) Of course, in comparison with a well-established orthodoxy, heterodox experimental proposals inevitably appear lightweight — perhaps frivolous or even incomprehensible — and committees find it correspondingly hard to support them. But this point has already been adequately dealt with by the mathematical physicist Freeman Dyson, who suggests that funding agencies should allot somewhere in the region of 10–25 per cent of their resources to heterodox research. (The figure of 10 per cent is also suggested by Muller. Alvarez argues that proposals in experimental high-energy physics should be assessed solely on the basis of the experimenters' past performance, and without regard to consensually perceived theoretical significance.)[9]

It should be noted that although this article has focused upon sociological problems arising in experimental high-energy physics, problems also exist in theory. Here again institutional structures presently act to discourage practice outside the gauge-theory orthodoxy. Dyson's article is, in fact, concerned with encouraging theoretical rather than experimental diversity. His conclusion is, however, relevant to both theoretical and experimental practice: "We should not be afraid of looking foolish or even crazy. We should not be afraid of supporting risky ventures which may fail totally . . . Organizations which only support research where there is no chance of mistakes will in the end support only mediocrity. If we proceed with good sense and courage to support unfashionable people doing things that orthodox opinion considers irrelevant or crazy, there is a good chance that we shall rescue for science . . . people whose ideas will still be famous long after all our contemporary fashionable excitements are forgotten".

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Andrew R. Pickering is at the Department of Sociology and Program in Science, Technology and Society, University of Illinios, Urbana, IL 61801, USA, and W. Peter Trower is at the Department of Physics, Virginia Polytechnic Institute and State University, Blacksburg, Virginia 24061, USA.

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