

25 years of elementary particles: The cultural context

Constructing Quarks: A Sociological History of Particle Physics

Andrew Pickering

479 pp. Univ. Chicago Press, Chicago, 1984. \$30.00

Reviewed by Hugh N. Pendleton

Constructing Quarks presents an admirable history of the last 25 years of the physics of elementary particles. Andrew Pickering defines two traditions in elementary-particle physics that emerged since 1960: the "old physics," dealing primarily with relatively common processes involving strongly interacting particles (hadrons) at relatively low energies; and the "new physics," dealing with relatively rare processes involving hadrons, usually at very high energies, or involving leptons, which do not engage in the so-called strong interactions at all. His story is the story of the displacement of the old physics by the new, and it emphasizes the growth to dominance of the theory of quantum gauge fields acting upon quarks and leptons and of the experimental practice symbiotic with that theory.

As a narrative account of the recent history of elementary-particle physics, Pickering's tale is clear, comprehensive and compelling. Most of the 5000 or so physicists who participated in the scientific work that is Pickering's subject are around to comment on his presentation; I expect them to enjoy reading about their achievement and to find, with some surprise, that Pickering has "got the physics right." To do that, Pickering has to provide more detail than the general reader might expect about the physics, both experimental and theoretical, at each significant point in his tale. Because his account is so detailed and so accurate, and because it makes clear why the physicists did what they did, it is eminently suited to be required reading for all young physicists entering or contemplating entering the practice of

elementary-particle physics. Even those of us who have lived through the events of his tale are likely to be surprised by some of the physics we learn from it. Physicists who are not elementary-particle specialists should be able to follow the main lines of Pickering's account without difficulty, but readers without scientific training will probably have trouble doing so, even though Pickering clearly intends to interest such readers.

In his first and last chapters, Pickering presents something approximating the following arguments:

► There are always many different theoretical accounts for any given set of data, each of which is equally good in matching the data.

► The theories that we prefer to keep in our heads influence the observations we make: The collection of data is not theory-neutral.

► Our theoretical preferences and our experimental (data-taking) practices reinforce each other symbiotically, making it difficult, if not impossible, for discordant data or unfashionable theories to get a fair hearing, or even to be noticed.

► There are sound sociological and philosophical reasons for the situation described above; it would be naive for us to expect otherwise.

► Nevertheless, the typical account given by scientists of their activities does not mention these points, but instead treats theories as randomly generated hypotheses (Einstein's free creations of the scientific imagination), which are winnowed out by confrontation with facts produced by experiments capable of demonstrating unequivocally that a given hypothesis is untenable. That some theories survive this winnowing process is ascribed by scientists to their being a reasonably accurate description of "reality."

► What scientists call "reality" is what accords with those theories and experimental practices that have defeated other theories and other experimental practices in historical and sociological contests. These contests are not usually described by scientists. (The careful description of such contests is the

central feature of Pickering's book.)

I agree with all the preceding points, noting that many readers may justifiably do otherwise. Pickering is concerned with the central role that the judgment of scientists plays in deciding which theories are worth further theoretical development and expensive experimental tests, which experimental practices are fruitful enough to be improved and continued, and which shall be allowed to fade away. It appears to be his view that scientific judgment, because it is necessarily embedded in the cultural context provided by contemporary science and is usually dominated by the norms of the scientist's special field (elementary-particle physics, in this case), must be thought of only within that cultural context—any attempt to evaluate that judgment from a broader perspective, and particularly one that refers to an underlying physical reality, is to be regarded as unsound historical methodology and as philosophically questionable. It seems to me, however, that to encourage such a restriction on considerations of principles justifying and illuminating scientific judgment is to carry cultural relativism to an unfruitful extreme. For example, Pickering mentions that his work supports the notion that different scientific traditions (in this case, those of the old and the new physics) are incommensurable—probably completely and surely partially. If the new physics is most appropriate for uncovering those phenomena that reveal the elementary substructure of things like protons, thus encouraging the reductionists among us to continue our quest to link all the pieces of science together, the old physics is most appropriate for gaining a phenomenological grasp of the complexities of the exclusive production of pions with small transverse momenta at high energies. I believe we should esteem both approaches, for the insight they yield about phenomena can be moderately well described by either theory—such as the spectrum of hadronic excitations. I suppose that the reason both of these theories are useful is that they each reflect some-

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thing of reality, and not just a socially useful set of conventions propounded by a self-perpetuating group of highly trained but epistemologically backward scientists. Nevertheless, I consider *Constructing Quarks* an important and fascinating book.

New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory

David Kite Allison

228 pp. Naval Research Laboratory, Washington, D.C., 1981. \$13.00

Radar Development in Canada: The Radio Branch of the National Research Council of Canada 1939–1946

W. E. Knowles Middleton

147 pp. Wilfrid Laurier Univ. Press, Waterloo, Ontario, 1981. \$9.75

Each of these books is concerned with an aspect of the development of radar before and during World War II. *New Eye for the Navy* deals mostly with the work at the United States Naval Research Laboratory from its beginning in the early twenties until the entry of the US into World War II in 1941. The other book describes the work of the National Research Council of Canada, which was an important part of that country's role in World War II. My own involvement with radar development began late in 1940, initially with the Submarine Signal Company in Boston and, from the Summer of 1941, with the Radiation Laboratory at the Massachusetts Institute of Technology, where I played a part in the advance of microwave technology.

The first of these books is based on a doctoral dissertation in the history of science presented to Princeton University in 1980. In it David Allison uses the pioneering work at the Naval Research Laboratory, on what later came to be called radar, as a theme around which he presents the story of the establishment and growth of the Naval Laboratory itself in the years between the two World Wars. He relates the establishment of NRL, in 1923, to the appearance not very many years earlier of research and development laboratories in corporate industry. He tells the story with the support of much documentation—of both formal and informal character.

The pioneering discoveries and developments of Albert Hoyt Taylor, Leo C. Young and Robert Morris Page are described in some detail. The story is conveyed in part through extracts from recollections recorded by Young in

1953. As early as 1922, Taylor and Young interpreted a "slow fading in and out" of the 60-MHz signal, sent from a transmitter at the Naval Air Station in Anacostia, D.C. and received at a point across the Potomac River, as an interference effect caused by a ship moving up the river. Taylor foresaw several possible applications and sought further support to pursue them, but he did not get it. No further work was done until a similar discovery relating to the detection of aircraft was made in 1930 at the Naval Research Laboratory, which had been established soon after the original observation.

For Young and Taylor, this new discovery marked the beginning of several years of work, although at a slow pace, on an apparatus capable of exploring this "bistatic" form of continuous wave, or "Doppler" radar. In the course of the year 1934, drawing parallels to the well-developed pulse-echo underwater sound-detection equipment (later called sonar), the team went over to the use of pulses of transmitted rf power. Success with pulsed-echo detection of a small airplane was achieved in December of 1934. More intensive efforts to improve on that initial apparatus led to a working experimental set demonstrated on several occasions in May and June of 1936, with the result that the furtherance of the project toward actual shipboard installation became a priority item at NRL. Allison's story continues with the development of a prototype shipboard radar, called the XAF, operating at 200 MHz, which was put into production by RCA and code-named the CXAM. An appendix in the book indicates that six of these production units were being installed in ships of the fleet in October, 1940.

Later chapters of the book describe the relationship of the projects at NRL to those of industrial laboratories, those of the US Army and, most importantly, those of the British. The latter had gone to war in 1939 and, according to the same appendix, already had an impressive assortment of radar devices in operation on land, at sea and in the air by the time of the visit to Canada and the US of a mission headed by Henry T. (later Sir Henry) Tizard in September and October of 1940. The Tizard Committee had been created in Britain in 1935 and had initiated very soon thereafter, at the recommendation of Robert Watson-Watt, broad development work on radar in response to the sensed imminence of war. The mission led to a major change in the organization and an intensification of US work on radar development resulted from that exchange. The civilian Radiation Laboratory at MIT was established especially to exploit the possi-

bilities of microwaves opened up by the pulsed multicavity magnetron shown to the US representatives by the British mission. The magnetron was immediately recognized as opening up entirely new standards of performance by radar. (See the article by James E. Brittain on page 60.)

Throughout his book, David Allison makes frequent reference to other documents and many sources that support and expand his exposition. The book is a fascinating story of an activity and a period in the development of science and technology that made an enormous contribution not only to the war effort, but also to the way "big science" and technology has developed since that time.

The second book, also written by a historian of science, W. E. Knowles Middleton, is concerned with the administrative structure, the personalities and the technical details of the contribution by the Canadian National Research Council to the development of radar during the years of World War II. The Tizard mission in September of 1940 brought about a considerable expansion of the Canadian effort, a development much encouraged by the British. This book also describes the background of the Tizard mission and the efforts made by Archibald V. Hill, a British physiologist, to bring about such an exchange. This book devotes much space to descriptions of the several radar systems developed by the Canadian group as the needs of the war evolved. Many of these projects were related to parallel or similar projects in the US, and the continuous liaison between the two groups is made apparent. Middleton's book is also interesting to read, and especially in connection with Allison's book. In the shadow of the much-larger-scale efforts of both the British and, after 1940, the US, the important Canadian work has been given little attention.

Both books make frequent reference to the still unpublished work of Henry Guerlac, *Radar in World War II*, the history of Division 14 of the wartime National Defense Research Committee, under which the MIT Radiation Laboratory operated. Very soon after it was established—as a result of the Tizard mission—that laboratory became a major participant in radar development. Its technical accomplishments were preserved in the 28-volume "Radiation Laboratory Series," but little of the political, administrative or human drama of those years is conveyed in those volumes. The books reviewed here provide some of that—especially for their subject institutions. I hope Guerlac's book, too, will soon become available.

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