P. W. Bridgman and High-Pressure Physics

P. W. Bridgman, Hollis Professor of Mathematics and Natural Philosophy for many years at Harvard, Nobel laureate, and practically single handedly the creator of the field of high-pressure experimentation, was born in 1882 and died in 1961. The first of the 199 scientific papers reprinted in this seven-volume collection was published in 1909, the last in 1963, after his death. Out of his life of 79 years, 52 years represented an active scientific career of publication. These volumes—**Collected Experimental Papers** (Harvard University Press, Cambridge, Mass., 1964. 4721 pp., $100), edited by Harvey Brooks, Francis Birch, Gerald Holton, and William Paul, were planned before Bridgman's death, and he lived long enough to choose and arrange the papers, which are printed chronologically, with a minimum of editing; the papers are reproduced photographically from the original journal articles. There are short introductions and valuable indexes (including unique indexes of substances and of apparatus). But in the main we simply have his scientific output, omitting a number of papers mainly of theoretical or philosophical interest, and of course omitting his several books. Since many of the original papers, particularly those giving extensive experimental results, appeared in the *Proceedings of the American Academy of Arts and Sciences*, a periodical not always available in libraries, this collection is particularly valuable as a complete record of the extraordinary scientific output of a very great scientist.

It is a fascinating lesson in the steps involved in the development of a new field of science to go through some of these papers, starting at the beginning. Here was a new field, in which earlier work had been almost insignificant compared to what Bridgman was preparing to contribute. First he had to produce apparatus. How could one contain a liquid in a cylinder, compress it with a piston, enclose experimental apparatus in it, get measuring devices to make outside measurements of what went on inside, find ways of measuring the pressure, and above all achieve the most elementary, but completely compelling requirement, namely keep the apparatus from leaking? The anti-leak invention was a key to Bridgman's whole career: a most ingenious method, described as the principle of the unsupported area, which resulted in having the pressure within the packing of the piston always a fixed ratio greater than the pressure in the cylinder, so that the liquid could never leak out. How could one measure pressure? A valuable secondary standard was the change of resistance of a manganin wire, but a primary standard had to be worked out. He loaded a known weight on top of a piston, reduced the friction to negligible amounts by an ingenious scheme, leading to a direct computation of the pressure, which was used to calibrate the secondary, manganin gage. How would one get electrical leads into and out of the pressure chamber? Nonleaking electrical leads had to be devised, going through from the high-pressure area to atmospheric pressure. Every step required trying out many different materials until a combination was found which would stand the enormous stresses. How, in fact, would one make cylinders and pistons that did not break? Only by trying out many different types of steel, finding not only the ones that would hold up, but finding how to season or heat treat them so that they would have strength far beyond their normal value.

These are some of the things found in the early papers. Anyone who reads them will realize that Bridgman was a builder of apparatus without peer. I had the priceless privilege of being one of his first assistants and thesis students, and so of seeing how he worked. Bridgman properly realized that building the equipment was the major part of the job, and he personally spent many hours in the shop, at the lathe or drill press, making the more delicate parts of the apparatus, and leaving the taking of readings to his assistant. But nothing could be more wrong than to feel that he was merely a glorified instrument maker. His students, not merely in the laboratory but even more in his lecture courses in electromagnetic theory, thermodynamics, and other topics, realized that he was one of the most original thinkers, and one of the greatest masters of theoretical as well as experimental physics, of his age. He never felt completely at home with atomic theory; but in the electrical and thermodynamic fields that were required for a proper understanding of his experiments, he was without an equal. He had a straightforwardness and directness that are seldom met in theorists, and yet a profundity that many of his contemporaries never understood.

We have in these seven volumes a record of a life and a field. Obviously, having worked out a technique, one must use it. Bridgman realized that pressure is just as important an independent variable in studying properties of matter as temperature. The changes in properties which he could produce in the pressure ranges that he used were just as great as, or often much greater than, the changes produced in the available temperature range. The only difference was that for many years a multitude of physicists had been studying temperature dependences of physical properties, and during most of his career, Bridgman stood essentially alone as a student of the pressure effects. Every property that could be measured was waiting for him to measure it. Polymorphic transitions were particularly easy to study; he found a far richer polymorphism in almost every form of solid than anyone had suspected. Compressibilities, changes of other mechanical properties, and the effect of pressure on resistance, on thermoelectric properties, on thermal conductivity, and on the viscosity of liquids were only a few of the sorts of properties to which he returned again and again. When he measured a property, he would obtain every substance of which he could get acceptable samples, and measure every one. Some of the results were more interesting than others, but they are all here, in these seven volumes. But here again we have not merely a contribution to the tables of physical properties. Bridgman was constantly concerned with the theoretical meaning of the properties that he had been studying. There are a good many papers on the theoretical implications of what he had found, going back as far as his paper in the *Reviews of Modern Physics* in 1935, "Theoretically interesting aspects of high pressure phenomena," and extending on to much later papers. These general review articles would amply repay study by any young theorist of the solid state; we are very far from having explained all the phenomena Bridgman felt were interesting and significant.

By the time we get to the last volume, we are coming to topics of recent interest. We come to the very high-pressure range, where the pressure was not purely hydrostatic (for there are no liquids that remain liquid at these extreme pressures), and therefore where it is much harder to estimate the pressure, but where nevertheless many interesting phenomena appear. We come to papers on such popularly interesting topics as synthetic diamonds, in which Bridgman made some of the early steps. We learn that he made measurements on the compressibility and phase transitions of plutonium during the wartime Manhattan District, though the results could not be reported until much later. But also we follow up to his last years additional measurements on less exciting topics, but nonetheless topics on which he had been working for many years.

The concluding paper was not intended for this volume by Professor Bridgman. It was his introductory essay, "General outlook on the field of high-pressure research," for the book *Solids Under Pressure*, edited by W. Paul and D. M. Warschauer, two of his former students, and published by McGraw-Hill in 1963. That book is a monograph on high pressure, mostly written by other workers. It is an eloquent demonstration of the fact that after a career of more than 50 years, Bridgman's field of high pressure has at last ceased to be a one-man show, and has come to represent a technique so familiar that now many laboratories are equipped to reach pressures that even Bridgman, early in his career, would have regarded as impossible. It is now a valuable and established field of physics, one for which Bridgman received his Nobel prize, and a field that in all probability would not soon have reached anything like its present mature state if this uniquely able, ingenious, and persistent man had not built it up.

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Paleoclimatology: Controversy, Chaos, and Confrontation

Paleoclimatology today is a snipe hunt, and one is very likely to find himself holding the bag, wondering where all the continents went. At the moment, it is also chaos, as any system without constraints must be, and one is reminded of the traffic jam that results when the red light on the street corner fails to function. The traffic problems caused by drifting continents have seldom been more evident than in **Problems in Paleoclimatology** [Interscience (Wiley), New York, 1965. 705 pp. $18], for though there is far more to paleoclimatology than continental drift, that fact is, in this volume, frequently brushed aside by a wandering pole.

The strong slant of the volume is no failing of its editor, A. E. M. Nairn, who, despite the handicap of extremely heterogenous subject matter, has succeeded in introducing a measure of order. Rather it was inherent in the symposium whose proceedings this book contains. Perhaps it was inevitable because of the necessity of fixing some physical configuration of the continents, ocean basins, and poles before real progress can be expected in paleoclimatology and because the symposium was held in Newcastle-upon-Tyne, the home port of many a drifting continent. In any case, the bias, once recognized, in no way impairs the utility of the volume and, in fact, lends interest for, after all, what is more interesting than the controversial?

Perhaps—and this is a serious problem—the existing chaos in paleoclimatology has come about because the field lacks tangible form. There is no such thing as paleoclimatology in its own right. Almost no one professes to be a paleoclimatologist, though many excellent geologists, biologists, meteorologists, chemists, and physicists profess an interest. Scientists who work in the field often do so on an occasional basis and commonly as a by-product of some other interest. The result is both the strength and the weakness of the subject: strength because paleoclimatology interacts with and draws from such a diversity of flanking disciplines; weakness because few of its practitioners can comprehend its whole structure and because few critical studies designed to solve uniquely paleoclimatic problems are ever made.

Despite shortcomings that this book inevitably shares with many a symposium volume, it carries an underlying tone of excitement. It is, I think, the excitement of an area of science beginning to recognize that it is a focal point for investigators in many fields. A great value of the NATO Paleoclimate Symposium and of this book is that both brought together the ideas of students working in many fields. As a result, one's imagination is stimulated, and one's admiration aroused, by the diverse and clever means by which paleoclimatic problems can be approached. The price paid for this benefit is that few will be able to read the entire book with full