Collections and Causes

Reading the Shape of Nature. Comparative Zoology at the Agassiz Museum. MARY P. WINSOR. University of Chicago Press, Chicago, IL, 1991. xviii, 324 pp., illus. $49.95; cloth, $21.95. Science and Its Conceptual Foundations.

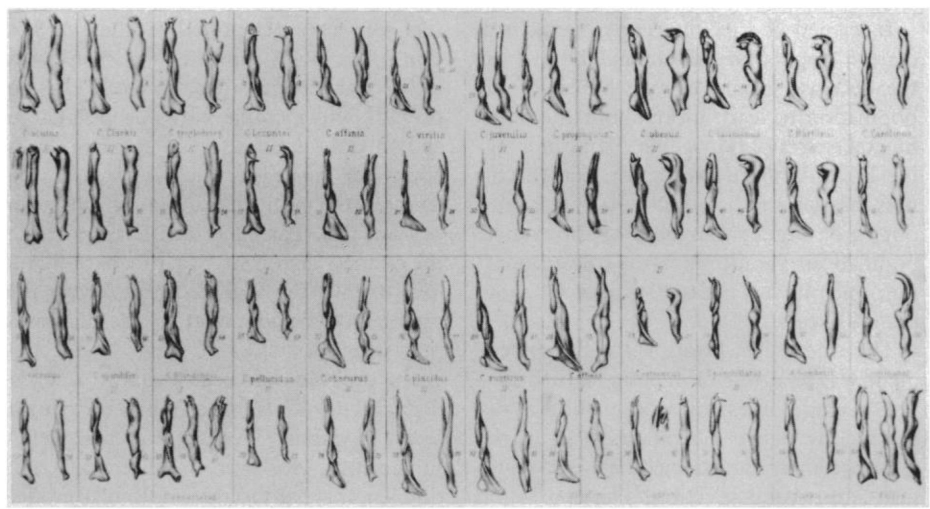
The history of taxonomy, or of natural history, or of museums is not a trendy subject. Yet even readers who think they do not care about any of these matters will find in this account of Harvard's Museum of Comparative Zoology in its early years an entrancing story of scientific practice done by real people with real problems and real disagreements, as well as successes.

Louis Agassiz dominated the MCZ as he dominates the history of 19th-century American science. Larger than life, Agassiz and his enthusiasms have long attracted the attention of historians. Here Winsor examines his role as a museum builder. At first Agassiz attracted student-assistants who worked with him as a "happy family." His anti-evolutionary *Essay on Classification* of 1859 guided interpretations, even when the students did not agree with their leader. Problems arose, for the parties had to operate without, as Winsor puts it, an "appropriate social structure" and had to "try to invent the graduate student." Lines demarcating student training, apprenticeship, and independent scholarship were not clearly drawn. The museum context raised further questions: To whom did the specimens belong, especially those acquired on an expedition in which Agassiz and students collected side by side but with money Agassiz had raised? Who counted as a collaborator and who was to remain just a subordinate helper? In recounting the resulting debates about plagiarism and proprietorship, Winsor shows that previous judgments of Agassiz as arrogant, demanding, and as having trouble with his subordinates as a result are as incomplete as the view that he was a saint. He was much more a visionary than a patient laborer, but many factors worked together to bring his successes and failures.

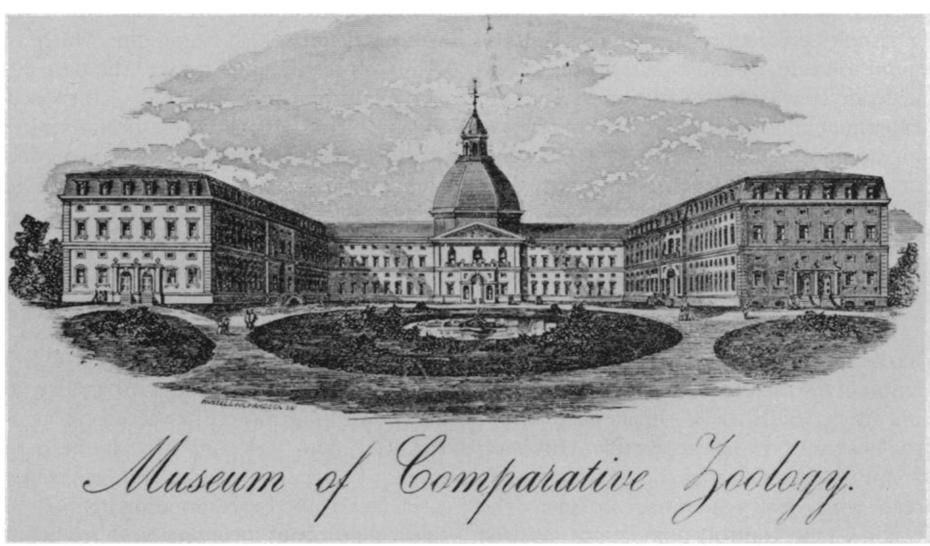
As Winsor explains, the museum's collection never really had a single purpose on which everyone agreed. Agassiz acquired material eagerly, even avariciously. Yet he never had enough staff to process it adequately, and specimens piled up, often becoming separated from related specimens before the collection was ever properly labeled. Agassiz tried to have it all ways: to convince supporters that the museum would offer public displays to edify and entertain, to persuade scientists that the collection was for preserving specimens and for taxonomic research, and to lure students and assistants into doing the tedious work of identifying and classifying. He did not succeed in all things, and, as Winsor charmingly puts it, "death released him" from the burden of trying.

It was left to his son Alexander to carry on his father's work. Having made his fortune in copper mining, Alexander was a better businessman than his father. He took up the museum work loyally but with considerable ambivalence. Winsor's sympathetic development of the influential but poorly understood Alexander goes far to explain what happened to the MCZ after Louis died and why Harvard failed to retain the preeminence in zoology that it might have had at the turn of the century.

Alexander Agassiz's uneasy dominance at the Newport, Rhode Island, laboratory that he established ensured that the students could not be equals, would not feel part of a research community as they were beginning to do elsewhere, and would have trouble establishing their own research agendas, as it was becoming important to do. Alexander had different taxonomic views from his fa-



"The first abdominal limb of male cambarid crayfish, lithographed by Paul Roetter" for a monograph by Hermann Hagen. "Having discovered a taxonomically effective feature, Hagen made careful drawings, front and side view, for each of twenty-nine species, two forms of male per species. He offered no explanation for the special value of this character, but later specialists agree with his choice." Roetter and Hagen were among several Europeans Louis Agassiz induced to join the Museum of Comparative Zoology. [From *Reading the Shape of Nature*; Museum of Comparative Zoology, Harvard University]



"Vision of the M.C.Z. of the future. . . . This drawing . . . must have been made between 1872 and 1876, for it shows the [added] fifth storey . . . but does not reflect the actual shape of the Peabody Museum of Archaeology and Ethnology, which in 1876 filled the site of the left end of this imaginary building. It probably predates 1875, when Alexander Agassiz included in his *Annual Report* a slightly different version, with the same dome, but with the main entrance on Oxford Street, as it would later be constructed. The dome remained a fantasy." [From *Reading the Shape of Nature*; by permission of the Museum of Comparative Zoology Archives, Harvard University] ther's, but he remained committed to taxonomy and to classification generally. In addition, though, the MCZ was expected to take on a larger teaching role.

Teaching what? was the question. The museum remained committed to comparative zoology. Yet Harvard was joining the rest of the United States and Europe in pursuing biological research, meaning increasingly technical microscopic work that would carry research in quite different directions from the natural history and taxonomy. This attracted some and lost other students. Winsor quotes Theodore Roosevelt's lament that Harvard had quite forsaken natural history and concentrated on "the study of minute forms of marine life, or else [on] section-cutting and the study of the tissues of the higher organisms under the microscope" (p. 176). Roosevelt found this appalling and gave up his hopes of becoming a naturalist who would roam outdoors and observe nature. The MCZ was in an awkward position of housing the materials for the old science while also attracting the faculty and students to pursue the new. As Winsor says, "It was as though cytology had forced out the taxonomic portion of biology, like the cuckoo chick expelling the nest builder's own young. And why does that chick, scrawny and blind, behave so heartlessly? It acts unconsciously and without malice, but consistently and vigorously, because only with a monopoly on the food can it grow strong" (pp. 194–95). Thus did Harvard, the MCZ, and Alexander Agassiz try to grow—sometimes at cross-purposes and to the frustration of all parties.

Alexander's temper and his ability to hold grudges undoubtedly aggravated the situation. He clearly interfered in ways that, for example, confused sea urchin taxonomy for some time. It was not until later in his life that he bitterly realized that he had taken on his father's causes and that he had not succeeded because they were not his own. "It is rather late to wake up, as I have done at 48, to the lesson that nobody should undertake another man's work if he has any he can do himself" (p. 146). Winsor helps us see how both Agassizes dominated and shaped the MCZ and biology at Harvard.

She carries the story to the next generation as well, in which Thomas Barbour took over as director and put the museum on a much more even course. Yet essentially a museum remains awkward by its very nature. It has no necessary connection to science and can remain immune to scientific change. Winsor concludes that a museum is an "eminently respectable piece of scientific equipment," a useful tool, but unwieldy and conservative.

Representing the best of modern scholar-

Vignettes from Physics

The behavior of famous scientists often amuses or bemuses their associates, engendering an informal literature of anecdotes. Here are some examples from current works about two renowned physicists.

Abraham Pais, writing in *Niels Bohr's Times* (Oxford University Press), offers the following personal recollections:

■ Bohr said to me...that it would only be profitable to work with him if I understood that he was a dilettante....I thought of his remarks...some years later, when I sat at his side during a colloquium in Princeton. The subject was nuclear isomers. As the speaker went on, Bohr got more and more restless and kept whispering to me that it was all wrong. Finally, he could contain himself no longer and wanted to raise an objection. But after having half-raised himself, he sat down again, looked at me with unhappy bewilderment, and asked, "What is an isomer?

■ Bohr was divinely bad as a public speaker....The main reason was that he was in deep thought as he spoke. I remember how [in a talk at Princeton] he had finished part of the argument, then said "And...and...," then was silent for at most a second, then said "But...," and continued. Between the "and" and the "but" the next point had gone through his mind. However, he simply forgot to say it out loud and went on somewhere further down his road."

Pais also retells a story from the recollections of Georg von Hevesy, who became an associate of Bohr's after studying at the University of Manchester.

■ In his boarding house in Manchester [according to von Hevesy] "the landlady always served the same food all week, and when I suggested as much she said it was not possible—'Every day fresh food is served.' So one day when she wasn't looking I added a dose of radioactive material. And the next day the hash was radioactive!"

Lawrence Bragg has recently been the subject of a volume of "selections and reflections" published under the title *The Legacy of Sir Lawrence Bragg* (John M. Thomas and David Phillips, Eds.; Science Reviews Ltd., Northwood, Middlesex, U.K.). Below are extracts from some of the reflections.

■ Bragg was one of the last of the classical physicists, who never involved himself much with the ideas of quantum theory or of particle physics. Scientifically he possessed in high degree the classical physicist's power of making intuitive order-of-magnitude calculations in his head; the ability to see the likely result without even the traditional back-of-the envelope to help him. And he liked simple methods; he distrusted electronic computers, as I discovered when I was first using the EDSAC Mk I in Cambridge to calculate Fourier synthesis....Bragg came into the laboratory with a drawing on a single sheet of paper and although he was ready to admit that my results *might* be more accurate, his were certainly not much in error.

—John Kendrew, director general of the European Molecular Biology Laboratory

■ Too many of us, young and arrogant that we were, thought of Bragg as a kindly old buffer, well past his prime; and he didn't help his image by getting excited about using rafts of bubbles as models of dislocated crystals. It was only when the eminent Cyril S. Smith of Chicago, passed through and also got excited that we began to concede that there must be something in what the old man was doing; and as the protein work thrived under his aegis we came to acknowledge that the Professor, after all, was a master of his craft.

—Nevill Mott, formerly Cavendish Professor of Physics, Cambridge

■ I remember a winter's evening spent with my father in the study at the Royal Institution. He was measuring and cutting out for me a circular skirt, the fashion of the fifties. The difficulty was, of course, that my waist was elliptical and that I was not symmetrical back and front. It would have been much easier, he pointed out, if I had asked for a handkerchief. But we persevered and I wore the finished creation on my honeymoon.

—Patience Mary Thomson, Bragg's daughter.

■ [Bragg] brought a saucepan to the Workshop for repair....It already had five or six repair washers in the bottom! We decided the only answer to the problem was to buy a new saucepan, and beat the "heck" out of it, which is what we proceeded to do. We then put two repair washers in the bottom, and with judicious use of a very rough emery cloth, made it look suitably "well used." Not a word was said when the "finished article" was returned to Sir Lawrence. But by the twinkle in his eye, he was in no doubt as to what we had done!

—S. Bruce Morris, one-time maintenance and workshop manager, Royal Institution

ship in the history of science, this book joins Ronald Rainger's fine new study of the American Museum of Natural History (*An Agenda for Antiquity;* University of Alabama Press, 1991) to show that careful studies of scientific work at museums can tell us much about science—how it gets done, where, why, by whom, and to what ends.

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Plasmas Close at Hand

Auroral Physics. CHING-I. MENG, MICHAEL J. RYCROFT, and LOUIS A. FRANK, Eds. Cambridge University Press, New York, 1991. xx, 464 pp., illus., + plates. $120. Based on a conference, Cambridge, U.K., July 1988.

The realization that a giant natural plasma physics laboratory was close at hand came 30-some years ago when the advent of satellites allowed discovery of the earth's magnetosphere. Almost simultaneously, analyses of extensive auroral data sets acquired during the International Geophysical Year 1957–58 showed that the visual aurora was one of the best tools for probing the nature of complex processes within the magnetosphere. An often-expressed but accurate analogy holds that the aurora playing on the polar atmosphere reflects dynamical processes operating in the magnetosphere and at the outer magnetospheric boundary in the same way in which the variable image on the phosphor of a cathode tube indicates changing electric and magnetic fields and particle trajectories in the body of the tube. As many of the 34 review papers in this book acknowledge, the state of the global visual aurora is a measure of the state of the magnetosphere and its interaction with the solar wind.

Indeed, analysis of auroral behavior soon after the IGY led to several insights that are still important to modern understanding of how energy from the solar wind transfers through the outer magnetosphere to enter the auroral ionospheres and the inner magnetosphere's trapping region, the Van Allen belts. In 1961, noting the strong control that sun-earth geometry exerts on both the visible aurora and the magnetosphere, J. W. Dungey proposed energy transfer into the magnetosphere via magnetic reconnection. It is now believed that 90 percent of the transfer is by this process, which converts kinetic energy of solar wind particles to magnetic energy stored temporarily in the tail of the magnetosphere. Concurrently, and similarly influenced by observed patterns of auroral motion, W. I. Axford and C. O. Hines proposed a viscous-like interaction between the solar wind and the magnetosphere that involves the establishment of a large-scale electric field across the magnetospheric tail and a two-cell convective flow within the outer magnetosphere. That flow maps down through the geomagnetic field to the auroral atmospheres where it is seen in auroral motions. The auroral observations also led S.-I. Akasofu to recognize auroral (or magnetospheric) substorms—repeated impulsive events wherein the rate of energy transfer through the system increases radically.

A number of the papers in *Auroral Physics* demonstrate that parts of the field have matured. The characteristics of trapped and precipitating charged-particle distributions are now fairly well known, as are most details of auroral excitation processes. The interactions and feedback between the lower magnetosphere, the ionosphere, and the thermosphere are becoming increasingly well documented.

However, the cause and nature of the substorm remains a mystery, and many of the papers in this book deal directly or peripherally with this important issue. Substorms, lasting one to several hours, occur in both quiet and disturbed times, with a strength and frequency greatly enhanced when the interplanetary magnetic field is oriented in a way favorable to Dungey's reconnection process. Thus the condition of the solar wind is critical. Yet the substorm appears to be a process largely internal to the magnetosphere, initiated somewhere and somehow by a mechanism not yet understood. Authors in *Auroral Physics* discuss six or more different models of parts or all of the substorm, and others present data that bear on the problem.

Progress on the substorm problem has been slow these past 30 years. Part of the difficulty is uncertainty about where auroras map out into the magnetosphere. This book's first and last papers rightly stress the field's major need: better globally oriented observation of the aurora, the ionosphere, and the magnetosphere. The aurora is the easy part because the technology exists. The application of similar "imaging" techniques to yield global views of magnetospheric particle and current distributions and ionospheric currents is likely to bring significant advances in the years ahead.

This multi-author compendium, better than most of its breed, is well arranged by its editors, and the numerous references made to its contents in recent literature attest to its usefulness to specialists. Its depiction of the struggle to develop a comprehensive understanding of the substorm also is of interest to plasma physicists at large—for what transpires in the earth's magnetosphere has broad application elsewhere, both in the laboratory and in the cosmos.

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Superconducting Devices

SQUIDs, the Josephson Effects and Superconducting Electronics. J. C. GALLOP. Hilger, New York, 1991. x, 232 pp., illus. $90. Adam Hilger Series on Measurement Science and Technology.

Since the discovery of high-temperature superconductivity, the superconducting quantum interference device (SQUID) has been repeatedly touted as one of the first major applications of the new materials. The many times this possibility has been suggested have produced probably an equal number of questioners in search of a book that describes what SQUIDs are, how they work, and how to use them. Here is the book.

A SQUID consists of one or two Josephson junctions joined together with a loop of superconductor, typically 10^{-5} to 10^{-4} meter in diameter. SQUIDs measure magnetic flux but can be configured to measure magnetic fields or magnetic gradients, as well as voltages or currents. *SQUIDs, the Josephson Effects and Superconducting Electronics* starts with the basics of superconductivity, including the modern theories of superconductivity and the Josephson effect, continues on through the principles of operation of the two types of SQUIDs (RF and DC), and finishes with a long description of applications of the SQUID. The discussion of applications is one of the most extensive to date. SQUIDs made from both low- and high-temperature superconductors are discussed. The book could be used as a textbook for a course on SQUIDs that covers all aspects from start to finish.

The first-time user of a SQUID quickly learns that the price of working with one of the world's most sensitive amplifiers is that its exceptional sensitivity to a signal also means exceptional sensitivity to all types of magnetic fluctuations, external noise, and miscellaneous other signals from nearby galaxies. The lore of SQUID use is extensive and must be learned before useful measurements can be made. Here this book is unique; the chapter "A practical guide to