

Introduction: Recollections on the Beginnings of *The Journal of Cell Biology*

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"In fame of learning, the flight will be slow without some feathers of ostentation."

Francis Bacon, *ESSAYS*, "Of Vain-glory," 1625

If, by cell biology, one means an integrative and interdisciplinary approach, utilizing techniques and concepts of anatomy, physiology, biochemistry, biophysics, genetics, zoology, botany, virology, and microbiology to seek comprehensions of the nature of living cells, one finds that neither the approach nor the term are new, both dating back more than a century. Integrative approaches are clearly embodied in Henle's *Allgemeine Anatomie* of 1841, in Kölliker's *Handbuch der Gewebelehre des Menschen* (1852), in Kühne's *Untersuchungen über das Protoplasma und die Contractilität*, published in 1864, and in Carnoy's *La Biologie Cellulaire, Étude Comparée de la Cellule dans les Deux Règnes*, of 1884. In Carnoy's introduction, he informs us that the first laboratory of cell biology was established at the Catholic University of Louvain, Belgium, in 1876, and that "Depuis trois ans, près de deux cents étudiants, Belges et étrangers, se pressent autour de nous, avides de science et ardents au travail." With such an honorable and diligent history, it is surprising that the term "cell biology" was still available for the journal title in 1962.

Some of the conversations which led ultimately to this Journal took place in the summer of 1951, when Porter, then at The Rockefeller Institute for Medical Research, went to Seattle to collaborate with Bennett in electron microscopy of thin sections of muscle. The endoplasmic reticulum had been recognized in culture cells and noted in sectioned material sufficiently to support the suspicion that it was a system generally present in all eukaryotic cells. The time seemed ripe to seek the endoplasmic reticulum of striated muscle cells. The anatomy department in Seattle had a brand-new, well-functioning RCA EMU-2 electron microscope, a recent gift from The Rockefeller Foundation. George E. Palade had found that

buffered solutions of osmium tetroxide preserved very adequately the morphology of cells in tissues. S. B. Newman, E. Borysko, and M. Swerdlow had shown that tissues embedded in polymerized acrylic resins could be sectioned, and Daniel Pease and Richard Baker had demonstrated that sections thin enough for electron microscopy could be cut with a manually operated, simply modified microtome designed originally for light-microscope sections. For what they say about cell biology of these early years, selected events of that summer are recalled as follows:

We prepared and examined many sections of chicken heart muscle. We even, in a whimsical mood, took a piece of cooked ham and prepared sections from it for electron microscopy, but without happy issue. We found internal membranes in the chicken muscle, thought of them as representatives of the endoplasmic reticulum of striated muscle fibers, and considered designating this system of membranes the "sarcoplasmic reticulum." In choosing micrographs for our illustrations, we pridefully thought them to be of superior quality, although by today's standards the sections were thick, the preservation imperfect, and the resolution modest. When it came time to select a journal for this masterpiece of electron microscopy, we lamented the shortcomings of the halftones in the journals of the day and wished for something better. We decided to try a Wistar journal, perhaps swayed by Bennett's distant kinship with Isaac Wistar. In any event, our manuscript went to the *American Journal of Anatomy*, accompanied by an appeal for very special attention to the quality of the halftone engravings.

The preparation of our manuscript occupied more time than we had anticipated. We were familiar with the entire, though meager, literature dealing with electron microscopy of muscle when we started the study. However, this proved to be insufficient, for we soon discovered an extensive series of papers based on light-microscope observations, extending back over half a century, dealing with the sarcoplasmic reticulum and mitochondria of muscle as seen by Kölliker, Retzius, Cajal, Veratti, and others. We learned that we were not the first to discover the sarcoplasmic reticulum, nor were we the first to use the term. Retzius, in 1881, had even suggested that the sarcoplasmic reticulum was admirably disposed to conduct an excitatory impulse from the surface membrane of a muscle

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fiber to the myofibrils in the interior. Surprisingly, intelligent speculation is not a 20th-century invention. Our paper did establish that the sarcoplasmic reticulum is membranous, though, at that time, we did not recognize the distinction between the sarcoplasmic reticulum and the transverse "T tubules," nor did Retzius, Veratti, or others before us.

When the muscle paper appeared in the *American Journal of Anatomy* (93:61, 1953), we were disappointed in the quality of the halftones representing our electron micrographs, and talk of other outlets of publication was revived.

Soon after Porter's return to New York in the early autumn of 1951, Don W. Fawcett joined his laboratory at The Rockefeller Institute and undertook with Porter an electron-microscope study of the structure of cilia. The study prospered and described the "9+2" arrangement of pairs of tubule-like structures seen in cross sections of cilia and also of the filamentous nature of ciliary rootlets and the associated cytoplasmic matrix. These were observations that foreshadowed recognition of the "cytoskeleton," which now figures prominently in the cell biology literature. In spite of such important ingredients, the manuscript was rejected by the editors of *The Journal of Experimental Medicine* in the fall of 1953 on the grounds that the study involved little that was experimental, that it was not medical, and, moreover, that the editors did not wish to publish morphological papers. Yet this paper reported the discovery of the ingredients of a system for motility not based on actin and myosin, and provided essential background for an understanding of Kartegener's syndrome. Normal things have to be discovered and described before their pathology can be recognized.

Vincent Dole, a member of the Board of Editors of *The Journal of Experimental Medicine*, agreed to convey the bad news to Porter. Returning to his laboratory from his visit with Dole, Porter encountered Herbert Gasser, then Director of The Rockefeller Institute. Gasser stopped him with remarks to the effect that "I hear the J.E.M. is not willing to accept your paper with Fawcett." After commenting that such an action was ridiculous, Gasser queried, "Porter, why don't you start a new journal?" adding that The Institute could undertake to cover the costs. Since Porter and Fawcett could not wait until a new journal was started, they submitted their paper to a *Wistar* journal. It appeared in the *Journal of Morphology* (94:221, 1954), with halftones that were as disappointing in quality as had been those published a year earlier in Bennett's and Porter's paper in the *American Journal of Anatomy*.

This experience with the J.E.M., amplified by similar encounters Palade had had, made us realize that we were faced with two kinds of publication problems: one related to the quality of reproduction of electron micrographs; the other related to the editorial policies of existing journals, whose boards of editors could not always recognize the significance of new discoveries or of concepts appearing in manuscripts parading before their eyes. Unwittingly, then, or possibly wittingly, the editors of the J.E.M., played a prominent role in starting the new journal.

The incident relating to Fawcett's and Porter's article came near the end of Gasser's term as Director. Detlev Bronk was appointed his successor. Additional conversations on the journal problem took place in late 1953 and early 1954, especially between Palade and Porter. The Rockefeller conspirators realized that the problem should be presented to Bronk in a persuasive manner. Therefore, upon Porter's suggestion, Bennett, in early 1954, wrote a letter to Bronk that presented

forcefully the need for a journal with editorial policies and technical capabilities appropriate for the developing field of cell biology, and asked if The Rockefeller Institute would consider adding a journal of this character to its distinguished list, then headed by *The Journal of Experimental Medicine*. We do not know if Bronk received other letters of this character, but whatever the case, his reply was encouraging. As a consequence, he assembled a group of interested persons to meet with him at lunch in Atlantic City during the April, 1954, meetings of the Federation of American Societies for Experimental Biology. In addition to Dr. Bronk and the authors of these "Recollections," the following persons attended the lunch and participated in the discussions: Francis O. Schmitt and Richard S. Bear of the Biology Department, Massachusetts Institute of Technology; Albert L. Lehninger of the Biochemistry Department, The Johns Hopkins University School of Medicine; and George E. Palade of The Rockefeller Institute for Medical Research. From the beginning of the conversations around the lunch table, Bronk seemed to have concluded that a new journal was needed and should be sponsored by The Rockefeller Institute. Most of the ensuing conversation related then to the name of the proposed journal and the types of manuscripts it should attract. All agreed that it should be interdisciplinary. Bennett proposed that it be called *Journal of Cytology*. This was quickly and enthusiastically rejected as too old-fashioned and restrictive to reflect the desired interdisciplinary nature of the new journal. No one proposed the name *The Journal of Cell Biology*. At times, disagreements about the title seemed sharp. Realizing that we had not agreed on a title, Bronk threateningly said that if that issue stood in the way, it might be that a new journal was not needed, and left the table. Thereafter the tone of the meeting became more harmonious, and after further conversation, the qualifying adjectives "biophysical" and "biochemical" were suggested, to specify and to dignify the general topic, "cytology," and to convey to all that the context of cytology, as used by the new journal, was not restricted to studies of chromosomes. By the end of the lunch, all agreed that *The Journal of Biophysical and Biochemical Cytology* was the most appropriate name that the assembly could think of. It was then recommended that most of those gathered around the luncheon table be appointed by Dr. Bronk to the founding Board of Editors of the new journal and a few persons not present were suggested as additional members.

The first issue of *The Journal of Biophysical and Biochemical Cytology* appeared less than a year after the memorable luncheon. It was dated January 25, 1955, and carried, along with other papers, the first full description of ribosomes (Palade, "A Small Particulate Component of the Cytoplasm," pp. 59-68); the first full paper distinguishing clearly between "rough" and "smooth" endoplasmic reticulum (S. Palay and Palade, "The Fine Structure of Neurons," pp. 69-88), and the first full description of synaptic vesicles and the inter-membranous synaptic spacing (E. D. P. DeRobertis and Bennett, "Some Features of the Submicroscopic Morphology of Synapses in Frog and Earthworm," pp. 47-58). Thus, the new journal got off to a good start, with Porter functioning as the first Managing Editor.

These recollections would not be complete without nostalgic comment on the spirit of goodwill, friendship, and cooperation which dominated the personal relations between those closely associated with developments of the field as presented in the Journal. Besides the authors of these "Recollections," this congenial and collegial group included Palade, Fawcett, M. J.

Moses, W. Bernhard, De Robertis, M. H. Burgos, Hugh Huxley, E. Yamada, K. Hama, Palay, Pease, and others. A new world was opening for exploration; a new information gusher had been uncorked. Excitement of discovery and community of purpose brought us together. The friendships and mutual respect engendered in those exciting days have endured and have fortified the field of cell biology.

This same spirit of cooperation was immediately expressed in the organization and participation of an international group in the first Conference on Tissue Fine Structure.* It was held in January, 1956, at Arden House in Harriman, New York, and was attended by about 100 enthusiasts, the great majority of whom presented papers. These were assembled and pub-

* The conference was organized under the auspices of the Morphology and Genetics Study Section, Division of Research Grants, NIH, Ernest M. Allen, Chief.

lished in 1956 as a supplement to Volume 2 of the Journal. It was the kind of volume to attract attention and to record forever the beginnings of modern cell biology. The journal profited from immediate recognition and a pronounced increase in subscribers.

The growth of interest in and use of the electron microscope, supplemented by the applications of cell fractionation, was rapid, and by 1962 the number of manuscripts submitted to *The Journal of Biophysical and Biochemical Cytology* greatly exceeded the number it could accept and publish. The result was an increasing interest in the publication of additional journals with purposes similar to those of the JBBC. It seemed probable that one of these would preempt what was obviously a most appropriate name and one that Carnoy evidently coined. Hence the editors acted quickly and changed the name to *The Journal of Cell Biology*, under which title it will doubtless survive and prosper for many decades.