

Introduction: Remembering Phil*

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This issue of the *Journal of the History of Biology* is dedicated to the memory of Philip Joseph Pauly, who died from complications of lymphoma on April 2, 2008, at the age of 57. Phil was a professor in the History Department at Rutgers University, where he had been a faculty member since 1981. In this collection of reminiscences and articles we wish to honor him as a scholar, colleague, friend, mentor to students, and supporter of our discipline.

The essays following the reminiscences reflect in various ways on the ideas in Phil's three books and related articles. Their themes reveal the diverse directions in which Phil's ideas could be taken, showing how his work inspired and challenged us on many fronts. His revised dissertation on Jacques Loeb, completed in 1981 at the Johns Hopkins University, was published in 1987 as *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology*. There followed a superbly crafted and insightful study of American biology, with emphasis on the Progressive Era, *Biologists and the Promise of American Life; from Meriwether Lewis to Alfred Kinsey*, published in 2000. Phil's third book, *Fruits and Plains: The Horticultural Transformation of America* (2007) developed the themes of the second book but explored in a bolder way a synthetic history that aimed to "see the past as a whole" by combining histories of the environment,

* With contributions from Peter Mickulas, Keith R. Benson, Garland Allen, and Jonathan Harwood.

agriculture, science, art, and national development. If there is a single overarching goal or theme to Phil's scholarly work, it might be described as linking narratives from different areas of history and showing how they could be combined into a single story. Phil approached this pursuit with zest and imagination, and he always had something interesting and unexpected to show us.

As a mentor to younger scholars, Phil was generous in his support and encouragement. Peter Mickulas, now Senior Research Associate at the New Jersey Historical Commission, wrote his dissertation on the New York Botanical Garden as a student at Rutgers University. Mickulas worked with another advisor but relied a great deal on Phil's mentoring and his excellent practical advice. This advice included encouragement to stay the course on his project and not be put off because more senior scholars were getting interested in the same subject. The advice paid off when the dissertation became a book, *Britton's Botanical Empire*, published in 2008 by the New York Botanical Garden Press. Mickulas remembered the way Phil's students and colleagues appreciated his insight, intelligence, and wit. Phil was producing groundbreaking work and had the ability to present the work with enthusiasm and a touch of subversive humor, as Mickulas recalled: "Somehow he managed to mix images of strawberries, livestock, 19th-century explorers and horticulturists, and 21st-century pop culture into his talks – and it worked." At the fall 2007 meeting of the History of Science Society, Mickulas was grateful that despite the lure of competing sessions with high-profile speakers, Phil chose to attend his session on the history of botany and botanical gardens, offering, as always, astute observations and penetrating questions that enlivened the discussion.

As Keith Benson comments, "When a valued peer and close friend leaves this life far too early, as Phil most certainly did, many unexpressed thoughts and even more important intellectual debts come into sharper focus. Perhaps the painful nostalgia associated with Phil's death provides the clarity needed to appreciate even more fully his influence on our own professional careers and our own lives." Benson, now professor and historian of biology at the University of British Columbia, Canada, first met Phil as a graduate student when he was giving a seminar paper at Johns Hopkins University on Jacques Loeb, the mechanistic biologist who was the focus of Phil's first book. He offers the following reflections on that book's impact:

"As I read *Controlling Life*, I became completely engaged in Phil's approach especially as he raised arguments about Loeb's influence on American biology that I had never conceived or imagined. Up to that

time and despite Loeb's important role in the development of medical chemistry or biochemistry in the United States, there had been scant attention to his work. I had noted this in a paper I had written in 1977 on Loeb and protein chemistry for a class in the history of chemistry. While researching that paper, I was immediately struck by Loeb's suggestion that the non-stoichiometric manner in which proteins operated in vital (in vivo) chemical reactions had potential to explain the apparent 'vitalistic' nature of the organic machine. Since the early-nineteenth century, several chemists recognized organic beings as consisting of the same chemical components that occurred throughout the natural world. But why should these components behave in a seemingly goal-directed manner when they were organized within organisms? Loeb suggested that the apparent 'vitality' of the organic machine might be attributed to the operation of proteins, which were considered as distinctive from other chemical compounds in that their reactions did not seem to follow the simple combinatory rules of chemistry. Instead, there appeared to be other principles that applied to protein chemistry and Loeb suggested that these, if known, might unlock the apparent 'vitality' of the biological world. I was very taken by Loeb's scientific work; at the same time, I was also aware that these suggestions were not too influential within American biology or medicine when Loeb first suggested them. Furthermore, I did not really think about any deeper historical reflections that this latter point might suggest. Indeed, I was too obsessed with the trees of protein chemistry to observe the forest of American biology in my evaluation Loeb's work and contributions.

"In retrospect, I did not need to worry about my own limitations, since Phil, as an excellent forester, saw the larger perspective. As he noted, it was not necessarily the mechanistic or materialistic orientation of Loeb's work that was most important. In fact, these aspects of Loeb's medical chemistry probably were received originally with great skepticism in the United States. But it was, in Phil's scholarly way of depicting his historical subject, the engineering ideal of Loeb that captured the imagination of many American life scientists. One might even argue that the engineering motif fit the new age in American science at the beginning of the twentieth century. Certainly the growing importance of engineering programs, now including biology, at MIT and a few years later, at Caltech, can be understood within Phil's framework. As I read on, I realized how much of American biology could be meaningfully understood within this model. Questions of how to manage the forests, wildlife, human populations, disease, and many other biological concerns were addressed in a framework that clearly resembled an

engineering approach. These were problems that needed to be managed by critically understanding the cause of the problem and then to address that problem in a linear and causal manner to arrive at a solution. Cogently, Phil saw this new attitude as emerging from Loeb's work. As I noted in my review of his book, Phil provided a more sophisticated analysis of American biology, beyond simply the traditional arguments involving holism versus mechanism.¹

"At the same time, it is important to point out that Phil's interpretation of Loeb as a biological engineer needs much more attention. More investigation needs to be done on the early years of the Rockefeller Institute for Medical Research and its influence, in large part through the direction pioneered by Loeb, on the new research missions in universities and medical schools throughout the United States. Loeb figured prominently in Robert Kohler's book *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (1982), but further study of the 'internal' or scientific history, especially in the interwar period, would be valuable. That chemists began to understand the organic machine in more materialistic and mechanistic terms is hardly questionable; but these two notions were not popular in the United States, while the engineering ideal was. How did other researchers deal with this orientation? In what sense did Loeb set the methodological agenda for biological and medical research between 1910 and 1940? Phil's suggestive ideas about Loeb's critical role in twentieth-century life sciences could use much more work."

Benson also offers reflections on Phil's interpretation of the Marine Biological Laboratory (MBL), a subject he took up around the time that his book was completed. Benson and Jane Maienschein had become interested in the MBL since it was about to celebrate its centennial (1888–1988), and Benson was also interested in other marine stations that had emerged in the U.S. at the end of the nineteenth century and during the early years of the twentieth century. At that time, there was little interest among historians in the general area of marine biology. Benson continues:

"In the mid-1980s several of us had received a commission from the American Society of Zoologists to write a multi-authored book on the history of American zoology. We invited Phil to contribute, thinking of his work on Loeb and the engineering ideal in American biology. But Phil responded to our invitation by suggesting another topic; perhaps because Loeb had often worked at the MBL, Phil wanted to write about

¹ Benson, 1990.

the important summertime field station. When I first read his paper, eventually published as ‘Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882–1925,’ I considered it to be seriously misguided and even wrong.² After all, Woods Hole, like the other summer stations, was a serious research enterprise. To describe the experience at the MBL as a ‘summer resort’ bordered on trivializing the work that was done there, I thought. But a second reading of his paper, followed by in-depth discussions with Phil at Friday Harbor Laboratories, led me not just to re-conceptualize my own views about Woods Hole and the other new coastal laboratories, but also to consider other questions, none of which I would have entertained initially. What Phil sought to emphasize was not necessarily the MBL as a ‘summer resort’ functioning to allow biologists to vacation with their families or to escape their normal academic routines. Instead, Phil used the notion of the summer resort to describe a new place where a sense of a biological community was initially built and then maintained. Furthermore, this new place helped to shape the very character of American biology. As he stated in his paper, ‘Lillie, W.M. Wheeler, W.E. Ritter, and L.J. Henderson joined H.S. Jennings’s effort to set out the nature and boundaries of a unified, autonomous science of biology.’³ The community-building involved, according to Phil, the adoption of certain developmental metaphors, much like the research that was done, as well as an holistic understanding for the organization of biology, again perhaps borrowing from their scientific work. But buried within Phil’s chapter was another suggestion of which I have only recently become aware and to appreciate. Phil rhetorically raised the question, ‘Were the problems and concepts of the MBL scientists shaped by their resort environment?’ Indeed, and not just in implicit terms, Phil suggested the importance of investigating the role the local setting of the MBL played in understanding the science at Woods Hole. That is, place needed to be considered as an active agent in science, not just a passive location. Unfortunately, and unlike my many discussions with Phil about Loeb, his death prevented us from sharing our ideas about place, one more common aspect of our professional careers. Or, better expressed, Phil’s death presented me from acknowledging one more intellectual debt clearly owed to him.

“Over the past two or three years, I have gradually understood that I have overlooked the local setting as an actor in the development of

² Pauly, 1988.

³ Pauly, p. 142.

marine laboratories. Despite Phil's clear and convincing argument about the 'summer resort' nature of marine stations, I simply did not apply this notion to the west coast. Maybe the Woods Hole community's long time role as a resort setting colored my perspective when looking at the new settings on the western shores, frequently constructed in a more casual manner than the MBL. However, once I began to examine these other stations with a similar lens to the one used by Phil, many new insights for my own work came into focus. Indeed, I began by asking the same question; that is, did the local environment shape the scientific ideas of the biologists working on the west coast? And it soon occurred that there is a major argument to respond to the question in the affirmative. The distinctive west coast tradition in marine biology to examine intertidal ecology is in large part influenced by the fact that the littoral biota is markedly stratified and suggestively determined by a combination of factors, now known to include both biotic and abiotic considerations. Certainly, I had been aware of the predominance of ecological work on the west coast, while the MBL emphasized developmental and cytological work. But to connect this meaningfully to the environmental setting did not occur to me until I fully incorporated Phil's suggestive ideas.

"Here, again, are suggestions for much more historical work. There has already been considerable attention to the importance of the move of biology into the laboratory and the creation of laboratories as new spaces for biology. Bruno Latour, Steve Woolgar, Robert Kohler, David Livingstone, and others have made substantial contributions in this arena, but their attention has largely focused upon the built environment.⁴ We know little about the role of the actual physical landscape or field (i.e., intertidal zone) as an active agent in shaping the scientific enterprise of biologists. This omission becomes even more critical when one reads of the aesthetic judgments biologists exercised in selecting either study sites or sites to construct field laboratories or research laboratories. After visiting many of these locations, one might ask, 'Why are marine laboratories always located in pristine or aesthetically pleasing settings?' Furthermore, in what sense does the interaction of the biologists and the field help to shape perceptions, attitudes, or ideas? Do biologists 'see' differently as a result of becoming familiar with specific sites? Does the experience of a long-term study of or an association with a specific site become reflected in the actual science that is done? These and other related questions may contribute to the richness

⁴ Kohler, 2002; Latour and Woolgar, 1979; Livingstone, 2003.

of historical queries afforded by the study of the environmental setting of biology, an idea suggested by Phil over two decades ago.

“To isolate only these two aspects of Phil’s work and my debt to him is not intended merely to provide personal reflections, although that is part of my motivation. Instead, it is to suggest to others how rich, important, and influential Phil’s work has been on almost anyone working in the history of American biology. Apart from his many other contributions as a tireless promoter of the history of American science, we owe many debts to Phil as a scholar. Unfortunately, it is often only after one loses a close colleague that these reflections cause us to understand just how critical the influence has been. At the same time and to personalize these remarks more explicitly, I am extremely grateful for the opportunity to have known Phil, to have been able to work closely with him, and to think that maybe we enjoyed an intellectual reciprocity. However, what is most abundantly clear to me is the tremendous debt I owe to Phil and I will continue to owe to him as I pursue our common interests. For this, I can only express my profound appreciation while, at the same time, acknowledging what many of us realize – our collective sense of loss in bidding farewell to our friend and colleague.”

Garland Allan, professor of biology and historian of science at Washington University in St. Louis, and former editor of the *Journal of the History of Biology*, remembers Phil as follows:

“Phil always impressed me as both a scholar and a person. He was clearly one of the most creative historians of twentieth-century life sciences we have had amongst us, and I always found it stimulating as well as fun to talk with him. We always had areas of considerable overlap to discuss, and I learned a great deal from him – among other things, how to think about historical relationship in new ways, ‘outside the box’ so to speak. I had read a good deal of Jacques Loeb’s work and written some things on him, but never thought to cast him as ‘engineering life’ – but that is exactly the aspect that Phil saw and it made so much more sense of Loeb’s lifelong work. I found his book on Loeb marvelous in so many ways. Among other attributes, he was a good and clear writer. His essays and books were not encumbered by heavy theoretical overlays of post-modernist theory. Yet he captured a lot of what that trend has had to offer in a simpler, less self-conscious, but equally (or more so) insightful way.

“I found Phil’s essay on the Japanese cherry trees as ‘invasive immigrants’ highly suggestive in the current work I am doing on the relationship between the eugenics movement and the burgeoning

conservationist movement in the early decades of the twentieth century.⁵ Phil's cherry tree case alerted me to the striking parallels between the metaphors used to describe 'foreign' organisms and 'foreign' humans. Once I had read Phil's essay I realized that the language and symbolic rhetoric of the eugenicists who initiated and promoted both the 'Save the Redwoods' and the 'Immigration Restriction' Leagues, were based on a common philosophy, one grounded in Progressive ideology of efficiency, scientific management, and 'social control,' but mixed with metaphors of 'native vs foreign species,' 'the noblest of a noble race' [redwoods], and the like. Phil was way ahead in pointing out the interwoven ideologies of xenophobia, immigration restriction and environmental conservation.

"Over dinner at the 2007 meeting of the International Society for the History, Philosophy and Social Studies of Biology in Exeter, we had a wonderful discussion as Phil indulged in his usual animated, but dry, humor (in his cherry tree paper he referred to the burning of the infected trees on the Washington mall as a 'horticultural auto-da-fé'). He was frank about the continuing treatment of his cancer and remarked that the value of the therapy would be determined by whether we continued this conversation at the next ISHPSSB meeting in Brisbane. Unfortunately, it was not to be. We have all lost a great resource and friend."

Jonathan Harwood, professor of the history of science and technology at Manchester University, U.K., offers an appraisal of Phil's third book that expresses the feeling that many of us had about it:

"It is a wonderful good fortune – for Phil as well as for us – that his book, *Fruits and Plains*, appeared at all. Since we work in related fields, I had heard from him about this project, but I also knew that he had been struggling with poor health for many years. It will, therefore, have been a tremendous physical effort for him to finish the book, but what struck me whenever I heard him give papers in recent years was his enthusiasm for this topic. This shines through in the book, too, where his use of language is playful and often witty. However much he suffered physically while writing it, he seems also to have been having fun.

"The book ranges widely and is illuminating on many fronts. It contributes, needless to say, to the history of horticulture as a discipline, placing particular emphasis upon the enormous role of amateurs throughout the 19th century. And what it has to say about plant-introductions will be of much interest to historians of agronomy and

⁵ Pauly, 1996.

plant-breeding. But the most striking feature of the book is its insistence that horticulture should be understood in social context. The contexts of interest, of course, include economic history; one chapter on Florida, for example, tells the story of numerous development schemes dreamt up by one obsessive visionary after another, beginning already in the early 19th century. But more unexpected for me is the fact that in its discussion of 18th-century discussions of plant introduction, the book gives a rich and physically tangible sense of what colonization entailed. Against the backdrop of the substantial differences in climate and ecology between North America and Western Europe, for example, one begins to understand why Jefferson and various contemporaries were so concerned about the processes of adaptation and degeneration, in plants as in humans. As Phil puts it, American independence was, among other things, ‘a biohistorical event.’

“Finally, *Fruits and Plains* is especially attentive to the cultural dimension, drawing upon language as a bridge between the wider society and the circle of horticulturists. For some time historians of animal-breeding and of entomology have noted experts’ use of terms drawn from the social realm (e.g. noble vs. common breeds, declaring war on pests, etc.), but until now this has never been done for the plant sciences. The omission is surprising since, as Phil points out, ‘culture’ in 19th century English (as well as in Latin) referred to a process whereby not just humans but also plants and animals were improved. ‘High culture’ meant opera and literature, but it also meant intensive cultivation techniques. It was to be expected, therefore, that horticulturists often referred - sometimes ironically but at others no doubt unconsciously - to ‘native’ versus ‘alien’ plants and to ‘immigrants’, ‘quarantine’, and ‘naturalisation’. This *cultural* history of science seems to me the book’s most novel feature and the one most likely to secure it a lasting impact.”

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