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The phytotronist and the phenotype: Plant physiology, Big Science, and a Cold War biology of the whole plant

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ABSTRACT

This paper describes how, from the early twentieth century, and especially in the early Cold War era, the plant physiologists considered their discipline ideally suited among all the plant sciences to study and explain biological functions and processes, and ranked their discipline among the dominant forms of the biological sciences. At their apex in the late-1960s, the plant physiologists laid claim to having discovered nothing less than the “basic laws of physiology.” This paper unwraps that claim, showing that it emerged from the construction of monumental big science laboratories known as phytotrons that gave control over the growing environment. Control meant that plant physiologists claimed to be able to produce a standard phenotype valid for experimental biology. Invoking the standards of the physical sciences, the plant physiologists heralded basic biological science from the phytotronic produced phenotype. In the context of the Cold War era, the ability to pursue basic science represented the highest pinnacle of standing within the scientific community. More broadly, I suggest that by recovering the history of an underappreciated discipline, plant physiology, and by establishing the centrality of the story of the plant sciences in the history of biology can historians understand the massive changes wrought to biology by the conceptual emergence of the molecular understanding of life, the dominance of the discipline of molecular biology, and the rise of biotechnology in the 1980s.

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1. Introduction

In 1965, volume three of Doubleday's new, glossy *Encyclopedia of the Life Sciences* arrived in the mailboxes of enthusiastic readers of popular science. Volume two had shown them the world of animals, and now the next installment promised remarkable vistas from the world of plants. The book's introduction noted that plants formed the foundation of life on earth because they convert the sun's energy into organic matter, permitting all insect, animal, and human life to exist. Readers learned startling facts of nature like, “cold conditions are necessary to break the dormancy of seeds” in peaches and apples, illustrated by a photograph showing that apple seeds exposed to cold germinated, while ones kept at constant temperature did not (Chouard & Nitsch, 1965, p. 97). Scientists had discovered such facts, readers were told, via remarkable new scientific facilities called phytotrons, climatrons, and biotrons. Around the height of Cold War technological optimism, readers may have been struck by such

evocatively named facilities and, the authors certainly hoped, recognized them as the modern face of plant science. The authors, Pierre Chouard and Jean-Paul Nitsch, believed that these grand laboratories of plant science were at last breaking open the study of the environment's effects on plants. For Chouard and Nitsch, the directors of *le grand phytotron* outside Paris, it was the “reproducible ... experimental conditions” of phytotrons that revealed the “basic laws of the physiology of plants” (Chouard & Nitsch, 1965, p. 103).¹

Beginning with heated greenhouses, a variety of instruments, facilities, and programs gave plant physiologists increasing degrees of control over the growing environment of plants since the late-nineteenth century: one corner of the laboratory revolution

¹ For the French C.N.R.S. phytotron at Gif-sur-Yvette see Chouard (1969), pp. 1–3, and Chouard & de Bilderling (1975). For a brief biography of Chouard, see Champagnat (2012), pp. 61–102. For a view of technological optimism in France, see Bess (1995), pp. 830–862; and Hecht (2009). As Wisnioski (2012) and Wolfe (2014), chap. 7 valuably explore, the technological optimism of the 1950s and 60s was thrown into sharp relief by the cultural crisis of confidence in science of the late-1960s and early 1970s.

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sweeping science (de Chadarevian, 1996). “The use of equipment where external conditions can be controlled in physiological studies is as old as plant physiology itself,” noted the Dutch plant physiologist Theodore Alberda as he surveyed the field in the late 1960s (Alberda, 1970, p. 591). Historians of biology are aware of one famous early controlled environment laboratory, the Vivarium that opened in 1903 in Vienna. As Deborah Coen explored, the Vivarium’s founders, Hans and Karl Przibram, aimed at the “mastery of the environment.” Their laboratory served to concretize their belief that “precision would soon be the driving force in biology” akin to the physical sciences (Coen, 2006, p. 498). Subsequently, many facilities for controlling environments in biological experimentation appeared in guises such as Herman Spoehr’s rudimentary constant-temperature chambers built at the Carnegie Institution’s department of plant biology in the 1930s (Craig, 2005, pp. 62–63).² By the mid-1950s, a variety of chambers, rooms, and facilities to control some array of climatic factors had spread throughout the plant sciences. Otto Frankel, chief of Australia’s major plant research group, the Division of Plant Industry, observed on his grand tour through the United States that “controlled environment facilities are now, at least to some degree, part and parcel of every of every botanical institution.”³

Frankel witnessed, and then helped, a technological revolution take over the plant sciences. Between 1949 and the 1970s, phytotrons emerged as centralized and cybernetic laboratory spaces; another aspect of the broad joining of technology and biology.⁴ The first phytotron, officially named the Earhart Plant Research Laboratory, was the creation of famed plant physiologist Frits Went and opened in 1949 at the California Institute of Technology (Caltech) (Kingsland, 2009; Munns, 1999, 2014). Subsequently, in just under thirty years, over thirty countries eventually built phytotrons, the largest examples being in France, the Soviet Union, and Australia (Evans, Wardlaw, & King, 1985). The Americans built the most, nearly a dozen, including the prominent examples at Duke, Yale, North Carolina State, and Michigan State Universities, as well as the national Biotron at the University of Wisconsin–Madison (Appel, 2000, pp. 183–186). Meanwhile a host of smaller examples occupied large portions of research budgets in Sweden, New Zealand, Canada, Hungary, Germany, the Netherlands, India, and Japan. In all phytotrons, new fluorescent tube lighting, heralding control of light spectrums and intensities, joined with new air-conditioning systems and control over temperature, new systems of humidity control, nutrient standardization, photoperiod control, sterilization protocols, and measurable air-flow. At their center, new computer systems gave control of control (Chouard, 1969; Downs, 1980; Went, 1957a).

Though it must be left to future work to explore, during the 1980s, phytotrons were forgotten like encyclopedia volumes left on coffee tables or shelved in bookcases. But at their height in the late-1960s, phytotrons seemed the modern face of the plant sciences. Alberda described the, to him, commonplace facility: “today,” he reminded his audience, “a number of so called growth rooms and/or conditioned glass houses are often built together to form what is

usually called a phytotron. Such units make it possible to study plant behaviour in its broadest sense under a diversity of climatic conditions where it is possible to vary each factor without appreciably altering the others” (Alberda, 1970, p. 591). Plant scientists generally considered phytotrons the most complete expressions of environmental control and many, like Chouard, readily advertized the fact. Frankel, for instance, returned to Australia from his tour of the United States convinced that antipodean plant science required a phytotron, and had it built by 1962 (Munns, 2010). Also in 1962 Went told a conference audience how a “tool” like his phytotron appealed to numerous “branches of the Plant Sciences” and their quest for the “understanding of the living plant” (Went, 1962, p. 378). A French phytotronist intoned how the phytotron served to “dissect the mechanisms of the plant as the cyclotron had the atom” (Augier, 1972, p. 4). In the future, biologists would one day also need a “marinetron” for water biology, said Donald Griffin, the discoverer of echolocation.⁵ Chouard confidently prophesized that biology was “entering ... a Phytotronic era” (Chouard, 1974, p. 5).

This paper describes a particularly dramatic moment of the technological revolution in biology: the moment when plant physiologists claimed the discovery of the “basic laws of physiology” via phytotrons. As we shall see, that claim was situated and legitimated within a number of interrelated contexts. Firstly, from the early twentieth century, plant physiologists considered their discipline ideally suited to study and explain biological functions and processes, and ranked their discipline among the dominant forms of the biological sciences from the 1920s onwards. Indeed, between 1949 and the mid-1970s, the confidence of many plant physiologists was bolstered by both private industry and public governments’ support for phytotrons, and by the increasing availability of the facilities to the global plant science community.

Secondly, phytotrons were as much experimental as cultural spaces. Phytotrons invoked the cyclotrons of high-energy physics as an expensive and interdisciplinary style of science centered on massive instruments. Using phytotrons, plant physiologists constructed the object of biological study itself: the phenotype, via big science. The meaning and experimental form of the phenotype was shaped by both the phytotron as instrument and the community of plant scientists assembled in the phytotron’s controlled spaces. That community, including agriculturalists, botanists, foresters, horticulturalists, and especially the plant physiologists, all accepted that the phenotype could be controlled and made, as Chouard said, a “reproducible” and “experimental” object. Across the plant sciences, the phenotype was generally understood as the sum of an organism’s genes and environment.⁶ A phytotron permitted both of

⁵ ‘Biotron Conference,’ Dec 10–12, 1959. Biotron Papers, Series 06/80, Box 1, file ‘Biotron Conference’ Archives, University of Wisconsin–Madison. p. 35.

⁶ This broad conceptual statement appears ubiquitously. For Went’s co-author, Kenneth Thimann, stated the principle in 1957 as “Hereditary potentialities” joined with “Environmental Factors” to create the “Internal Physiological and Biochemical Processes and Conditions” which only then would become expressed as “Plant Growth and Development.” Thimann argued that physiologists well knew that plants not only grew at radically different rates in various climates but that the internal processes of plants were often just as significantly affected. See ‘Thimann Report,’ attached to Thimann to the Secretary of the AIBS, March 13, 1957. In Phytotron Records, box 2. Duke University Archives. p. 4. Also in France, the later deputy director of *le grand phytotron*, N. de Bilderling offered the concept as a mathematical product expression, “Phenotype = genotype x environment” de Bilderling (1974), p. 16, I have chosen to follow the more common usage of a sum expression following the use of Jan Zeevaart, namely “GENOTYPE + ENVIRONMENT = PHENOTYPE”, in Zeevaart (2009), p. 4. Zeevaart was a colleague of Anton Lang, the successor of Frits Went as the director of the Caltech phytotron. Zeevaart and Lang moved to the Michigan State University to found the Plant Research Laboratory in 1965 on the back of A.E.C funding. As major plant research institution since then, the facility has recently begun expanding climate controlled chambers.

² Other disciplines like biochemistry also stressed more stable environments at constant temperatures in which to run new ultracentrifuges and electrophoresis apparatuses; on the eve of the second world war adjustable controlled chambers stabilized the “best-equipped biochemical research facilities in Germany and the world” said the director of the Kaiser Wilhelm Institute for Biochemistry in Berlin Rheinberger (2010), p. 131. At the same time, physiological ecology was developing (or at least dreaming of) controlled environment laboratories. See Kingsland (2009), pp. 293–299.

³ O. H. Frankel, ‘Report on a visit to the USA May 3–Aug 3, 1955, under the auspices of a grant from the Carnegie Corporation of New York.’ Copy in James Bonner papers, file ‘Australia.’ Archives. California Institute of Technology. p. 34.

⁴ Of especial relevance and resonance here is the story of computing and biology. See November (2012); Garcia-Sancho (2012), and also Rasmussen (1997a). More broadly see Creager & Landecker (2009).

those variables to be experimentally controlled: as Went argued, “an organism is the product of its genetic constitution and its environment ... no matter how uniform plants are genotypically, they cannot be phenotypically uniform or reproducible, unless they have developed under strictly uniform conditions” (Went, 1957a, p. 2). Consequently, while the plant physiologists had long advocated and attempted experimental work in fully controlled environments to establish direct causation between particular environmental conditions and specific growth and physiological features of plants, after 1949 they and the larger community of phytotronists gained and utilized ever-greater technological control over growing environments to investigate the complex relationships between phenomena like fruiting, flowering, and growth and specific environmental conditions.

Thirdly, the production of phenotypically standard plants via the control afforded by the phytotron allowed the plant scientists working in phytotrons to claim their work as basic science, a status regarded as distinctly above applied science. When plant physiologists like Chouard and Nitsch laid claim to the discovery of the “basic laws of the physiology of plants” from new laboratory spaces, it was part of a wider struggle over status. In recalling an incident that clearly stuck with him for the rest of his long career, Went’s colleague at Caltech, James Bonner said that to most people in the 1930s, “Biology was just a bunch of facts and no science; nothing rigorous about it.” “Biology?,” Bonner remembered the physicist Willie Fowler said to him when they were both graduate students, “how are you ever going to make a science out of that?”⁷ To Fowler, and probably then afterwards also to Bonner, the basic science assumed an exalted position above applied science. It was a cultural standard that many scientists felt they struggled to measure up to, and one only heightened by the triumph of the physicists during the second world war and the rise of big science (Weinberg, 1967, pp. 85–100). Postwar, the plant physiologists answered that better-controlled environments permitted experimentally valid phenotypes, which in turn opened the path towards basic science. In fact, Chouard and Nitsch largely followed Went, who had already unabashedly claimed that his phytotron was revealing the “universal” factors of growth and flowering and was producing a “general understanding” of the development of a plant (Went, 1957a, p. 97), before prophesying that the continued pursuit of such basic science would permit no less than a “Theoretical Botany” comparable to a generally accepted “Theoretical Physics” to appear at last (Went, 1957a, p. 319).⁸

⁷ James Bonner. Interview by Graham Berry. Pasadena, California, March 13–14, 1980. Oral History Project, California Institute of Technology Archives. http://resolver.caltech.edu/CaltechOH:OH_Bonner_J, pp. 9–10. Accessed Jan 5, 2014.

⁸ Went’s prophetic visions resonate strongly with the earlier claims of the Prizbram brothers in their Vivarium, as Coen has engagingly outlined (Coen (2006), p. 499. Around mid-century, the image of the physicists loomed larger than life in the phytotronist community. At the opening of the Australian phytotron in 1962, for example, Went perhaps spoke for much for the biological community when he noted privately “the awe in which biologists hold physicists.” From Frits Went Diary, Sept 1, 1962. Frits Went papers. Record Group 3/2/6, box 22. Archives. Missouri Botanical Garden. Analogies from physics are also everywhere. The later director of the Duke University phytotron, Paul Kramer, and his co-author and Boyer offered an illuminating example of the “Ohm’s Law Analogy.” Popular in the 1960s they said, the analogy described the water flow through the soil–plant–atmosphere continuum as like the “flow of electricity” in a circuit. By the 1980s it had fallen out of favor because it assumed steady-state flow and constant resistance, which while valid for circuits was rarely seen in biological systems. As late as 1995, though, Kramer and Boyer thought the analogy “too useful to be abandoned” (Kramer & Boyer (1995), p. 9. On the other hand, Went’s friend and colleague Sterling Hendricks (who co-authored an article with Went advocating a national phytotron, the Biotron) would say in his memoirs that “the benign efforts of so many in research, even including something so distant as my work on plants, has origins in ... two simple expressions”: “the photoelectric effect ... $E = \phi N h \nu$ ” and “the expression for the equivalence of mass and energy ... $E = mc^2$ ” (Hendricks (1970), p. 1).

This paper argues that the phytotronists’ claims to experimental control over the phenotype was as much about the establishing the standard plant in a controlled environment as it was about establishing an identity as the basic scientist in the Cold War era. Historians have long appreciated the various changes wrought on the biological sciences in the wake of the “physicists’ war,” the second world war (Creager, 2013; Rasmussen, 1997b; Zallen, 1993, p. 77). In this case, like their heroes the physicists, the phytotronists gained substantial patronage for grander phytotrons on the basis that the facilities were increasingly concerned with basic rather than merely applied science. This struggle over the basic/applied divide is another example of the formation of a “hybrid culture” at the borderlands between the field and the laboratory. Sharon Kingsland valuably drew historians’ attention to the phytotron as an exemplary case to explore the borderland. Concluding that the field also shaped the laboratory in the case of Went’s Caltech phytotron, Kingsland stressed the relevance of Went’s phytotron to the field sciences, especially ecology, in particular that the new laboratory was “well suited for the study of practical problems,” with a famed successes in identifying damage to living plants from smog (Kingsland, 2009, p. 292). This paper explores another feature of the borderland, which Kohler also drew historians’ attention to: the class dimensions marked by the distinction between scientists work on applied versus fundamental science. Kohler’s introduction to the boundaries between the field and the laboratory noted that those in the field often felt and were portrayed as second-class by their laboratory colleagues (Kohler, 2002, p. 1). Went and the phytotronists, certainly, were never satisfied with only practical work, and consequently the demarcation line established by a laboratory like a phytotron signaled, and confronted, the keenly felt class distinctions of being known as basic versus applied scientists. In one of the most overt statements, Lloyd Evans, once a post-doctoral student of Frits Went at Caltech in 1954–55, and, after Frankel, the director of the Australian phytotron, and later still the president of the Australian Academy of Science, believed that the choice for a young scientist between “pure or applied” always remained “that old intellectual class distinction” (Evans, 1998, p. 222).

In short, seeking to distinguish themselves, the plant physiologists claimed the pursuit of basic research towards the discovery of the basic laws of plants via a methodology to produce a standard phenotype. Structurally, that goal generated both a conception and practice of the experimental phenotype, and permitted phytotrons, biotrons, and the Climatron to expand in parallel with cyclotrons, bevatrons, and the Tevatron over nearly three decades.

2. The maturity of plant physiology

For much of the twentieth century, plant physiologists considered themselves in an ideal position to study and explain biological functions and processes. The American Society of Plant Physiologists (ASPP) broke from the Botanical Society in 1923 “to give plant physiology identity and recognition as a distinct branch of plant science” (Hanson, 1989, p. 57). For the plant physiologists, their field in fact lay “at the heart of botany,”⁹ ASPP founder Charles Shultz believed because the discipline dealt with understanding and explaining processes in living plants. As the two dominant personalities behind the Duke University phytotron and the University of Wisconsin Biotron, Paul Kramer and Theodore Kozlowski, later described in one of their textbooks, the “role of plant physiology is to explain how plants grow and respond to environmental factors” and diagrammed how processes like plant respiration,

⁹ Quote from Hanson (1989), p. 1. For an analysis of the broader disciplinary changes in botany, see Smocovitis (1992a).

photosynthesis, and nitrogen metabolism—the “internal processes and conditions” of organisms—derived equally from both “hereditary potentialities” and “Environmental Factors” (Kramer & Kozłowski, 1979, p. 3). Never one to undersell his science, Went thought plant physiology the study of life itself: “physiology is the analysis and synthesis of life phenomena in terms of individual reactions or processes” (Went, 1957a, p. 106).

To reveal those individual reaction and processes that formed life, plant physiologists defended a methodology of experimentation. “Plant physiology is an experimental science and is, therefore, based on experiments,” insisted the Dutch plant physiologist Theodore Weevers his early comprehensive history of plant physiology (Weevers, 1949, p. 4). Weevers’ circular protest seems an implicit acknowledgement of the plant physiologists’ struggles to precisely define their experimental science; throughout the 1930s the search for satisfactory experimental approaches had dominated the journal of the Society of Plant Physiologists, which counted “techniques, methods, analyses” in the top three most published areas, along with “environmental responses, stress” (Hanson, 1989, p. 45). Experimental work on distinct and repeatable environments remained notoriously difficult for the plant physiologists, and Weevers’ emphasis on experimentation sought to partition an explicitly experimental science from other more descriptive practices. Meanwhile Went’s concern with the living phenomena and processes perhaps sought to subsume biochemistry, and Kramer’s explicit emphasis on the environment declared genetics only one half of a larger science.¹⁰ When the ASPP became a charter member the American Institute of Biological Sciences (AIBS) in 1948, AIBS reiterated that “physiology, biochemistry, and biophysics [were] uppermost” in the science of biology by the mid 1950s, replacing older fields like taxonomy, embryology, cytology, and experimental biology.¹¹

It was with a focus on the complex processes of living organisms, a commitment to experimentation, and being conscious of their role in integrating several disciplines of biological science that underscored the plant physiologists’ self-image as the leading edge of plant science by the early Cold War. By 1953, the long-serving Executive Secretary of the ASPP, J. Fisher Stanfield, considered “plant Physiology [the] major undergraduate subject and the most mature.” From his pedagogical perspective, “zoology had always dominated the biology departments,” but physiology would triumph because it embraced the “complexity and the physiochemical approach born of technological advances.”¹² Similarly,

and moreover writing as one of the major figures of plant physiology in the 1950s and 60s, Went expected that “plant physiologists will become an important part of every botanical garden,” in the bicentenary history of the Royal Botanic Gardens. Plant physiologists, Went said, had turned the study of plants away from early “display gardens” of “largely horticultural interest” towards a modern “experimental science.” Utilizing the “priceless materials present in gardens” directed through “experimental greenhouses,” plant physiologists had revealed “new knowledge about plant nutrition, plant hormones, [and] plant responses to their environment” (Went, 1959, p. 519). Thus, well before the molecular wars of the 1960s, plant physiologists maneuvered for prominence and legitimacy by emphasizing their commitment to experimental science and technology. Their new, grand facilities for controlled environment experimentation would, they were confident, move their discipline towards becoming the center of biology.

In her history of the rise of molecular biology, Lily Kay noted that Caltech’s plant physiologists vied to dominate Caltech biology in the immediate postwar years (Kay, 1993, p. 185). What she did not mention, however, was what the biology division’s catalogue lionized in 1950: “the newly completed Earhart Plant Research Laboratory,” “a unique instrument for the study of plant growth under complete weather control. ... The old and the new research laboratories offer the opportunity to study plants under different synthetic climatic conditions, yet with complete reproducibility of experimental results.”¹³ The Earhart Laboratory soon became better known as the phytotron. Went both designed the laboratory and amassed the substantial patronage necessary to build it via his considerable stature within the biological community. He was a rising star in the 1920s and a major plant scientist until the 1960s. Went remains best known to biology undergraduates for discovering a class of plant growth hormones—the auxins (de Chadarevian, 1996). Subsequently recruited by one of the major figures of genetics, Thomas Hunt Morgan, to Caltech in 1932, Went rose in the division of biology under A. H. Sturtevant and worked in his phytotron during most of the tenure of George Beadle in a period of dramatic expansion at Caltech and the emergence of molecular biology (Kay, 1993; Kingsland, 2009). At least within plant science circles, Went was celebrated for his “great discovery” that tomato fruit only set “over a limited and experimentally determinable range of night temperatures” after conducting early experiments in rudimentary air-conditioned greenhouses at Caltech between 1943 and 1945 (Bonner, 1962, p. 215). To those who knew him best, Went, “remained primarily a plant physiologist.” “To him this meant using any and all means to understand plant growth, development, and interactions with the environment” (Galston & Sharkey, 1998, p. 359).

3. Plant physiologists, basic science, and big biology

Went’s new phytotron began a “laboratory movement” in plant physiology and the plant sciences.¹⁴ It was a movement both in size and kind. On the one hand, the new laboratory was the first of Caltech’s many expansions of scientific facilities and their entry into the world of big science. On the other, the laboratory was a conscious effort to shift plant physiologists away from applied research and into basic research. The pursuit of basic science

¹⁰ The Australian physiologist Rutherford Robertson explained in his introductory lectures, physiological “methods of investigation are essentially chemical and physical, but have regard to the complexity of living systems. Physiologists studied the function of organisms. Processes investigated are the integrated results of complex organization of cell, tissue, organ or organism. Physiology thus differs from biochemistry, though there is no sharp dividing line.” ‘Plant physiology. Notes for lectures. Sydney.’ 1954. Papers of R. N. Robertson. Basser Library, Australian Academy of Sciences. MS117/7/8. In their undergraduate textbook, Bonner and Galston made much the same claim, “the developments of modern biology have tended more and more to obscure whatever dividing line may once have existed between physiology and biochemistry” Bonner & Galston (1952), p. v. At Johns Hopkins, the same comments were made about biochemistry Long (2009), p. 781.

¹¹ ‘Report of the Committee of the American Institute of Biological Sciences set up to Study and Advise the Biological and Medical Division of the National Science Foundation with Respect to Policies to be Followed in Support of Biological Stations,’ Dec 8, 1955. In Phytotron Records, box 2, file ‘Controlled Environments-discussions/committees before 1962.’ Duke University Archives. A nice comparison with physiology at Caltech is offered through biochemistry at Johns Hopkins, where, as Tully Long has revealed, a re-energized biology department under Benjamin Willier embraced “the application of physio-chemical methods” to underpin new multiple appointments in physiology, genetics, and biochemistry Long (2009), p. 781.

¹² Letter from J. Fisher Stanfield to James Bonner, July 16, 1953. James Bonner papers, file 23.9. Archives. California Institute of Technology.

¹³ California Institute of Technology, *Catalogue 1950–51* (California Institute of Technology, 1950), p. 88.

¹⁴ Kingsland first offered this interpretation, following on from Robert Kohler’s study of the lab/field borderlands Kingsland (2009), p. 292. Because of the global array of phytotrons, the distinct lineage from phytotron to Biotron to Ecotron, and the international and interdisciplinary community of phytotonists, I argue that the Caltech phytotron was indeed the spur for a laboratory movement.

appeared from the very earliest days of the first phytotron. While at Caltech in 1952, famed plant physiologists William Hiesey¹⁵ collaborated with Went and Marcella Juhren and concluded that plant physiology required “a new instrument” like the Earhart Laboratory because basic physiology is “so complex that to date satisfactory experimental approaches have not yet been developed.” For the trio, a new basic science of plant physiology could only emerge from the new interdisciplinary approaches afforded by the ability to control conditions: “basic physiological studies of necessity embraces an appraisal and integration of genetical, physiological, and ecological factors,” they said (Juhren, Hiesey, & Went, 1953, p. 288). In other words, the collaboration admitted that the complexity of controlled environment studies of whole plants necessitated expensive and technologically complex facilities.

Basic science was the expected and necessary future of plant science, but plant physiologists realized that basic science needed big science. Revealingly, after leaving Caltech, Hiesey visited his longtime friend and collaborator David Keck at the New York Botanical Gardens before heading off to Europe. Writing a few days later from the steamship *Liberté* in May 1952 to solve Keck’s evident block of a topic for an upcoming talk, Hiesey offered up the subject “The Golden Age in American Botany.” Hiesey told Keck that botany’s golden age “is the age that is now ahead of us.” To Hiesey, “U.S. botanists” had passed the “rugged exploratory stages,” “inherited rich collections of material, facilities and libraries” and had “new tools available for further real advances.” Though admittedly worked up by the “French wine” on board, he believed that plant scientists were “for the first time” in a “strong position to evolve a mature, well-balanced, integrated development of the plant sciences embodying all the results of the efforts from different special fields.”¹⁶ Facilities like phytotrons represented interdisciplinary efforts aimed at uniting genetics, physiology, and ecology, which in turn encompassed the parts of the plant, the whole plant, and whole communities of whole plants. And finally, such facilities must aim, Hiesey said, to discovery “real advances,” or, as Chouard would say later, “the basic laws of the physiology of plants.” In short, as Hiesey sailed to Europe, plant physiology sailed towards big science, embracing interdisciplinary and monumental laboratory facilities, like Hiesey did in Went’s phytotron, in pursuit of basic science.

As historians of science have explored, big science saw centralized, expensive, usually state-sponsored, technoscientific complexes emerge throughout the Cold War. Especially after 1945, big science displayed scientists’, and their federal patrons’, modernist convictions that even the largest social problems could and would be solved by science.¹⁷ Objects like phytotrons, large particle accelerators, radio telescopes, and oceanographic ships were iconic to big science. At the apex of phytotrons, the Biotron at

the University of Wisconsin–Madison was funded by the largest facilities grant awarded by the National Science Foundation up to 1959 (Hendricks & Went, 1958). Like the better-known cases of MIT or Caltech, Madison expanded courtesy of new, deep federal coffers along the intersection of biology and technology. By 1963, the university celebrated several new buildings including a cancer research laboratory (2.8 million), a primate laboratory (1.9 million), a molecular biology and biophysics laboratory (2.2 million), as well as the new Biotron (4.2 million).¹⁸ A big science facility like the Biotron represented a widespread conviction among scientists, universities, and their government patrons that major scientific facilities had out-grown any individual university’s financial capabilities: only consortiums or groups of universities could now hope to amass the funds necessary for the construction of big science.¹⁹ It is important to note that this exact argument was appropriated directly from other similar “national” scientific facilities, such as Brookhaven National Labs for nuclear research and the National Radio Astronomy Observatory at Greenbank for radio astronomy.

As much in the physical as the biological sciences, big science served to legitimate a new status for disciplines claiming synthetic unity. For example, their successes with photoperiodism allowed plant physiologists to stress their science’s unique ability to derive general knowledge of whole plants in nature. As Went’s colleague and later biographer, Arthur Galston, wrote years later, the discovery of photoperiodism “not only subsumed much information within the confines of a single generalization, but also provided an experimental basis for further explorations of physiology” (Galston, 1974, p. 427). For a plant physiologist like Galston, his field possessed an ability to generate an experimental program around, as well as synthesize and unify the facts from, phenomena like photoperiodism. Synthesis served to legitimate plant physiology among the plant sciences, and, as Went, noted, other varieties of plant sciences offered no such benefits. Characteristically blunt in explaining the role of physiology in the biological sciences in no less than the flagship journal *American Journal of Botany* celebrating fifty years of plant physiology in America, Went said that only once “a reaction or process is isolated from the behavior of the organism as a whole, [can] the biochemist or biophysicist further identify the individual process. Without a proper physiological analysis, however, biochemical studies are meaningless” (Went, 1957b, p. 106).²⁰

Went’s long-time ally, Sterling Hendricks was even more direct about the potential of physiology to synthesize as he penned his memoirs in 1970. “In truth,” Hendricks wrote,

physiology is really an integration of the parts rather than a cataloguing of them. Movement towards integration is inchoate

¹⁵ The collaboration of taxonomists Jens Clausen and David Keck with Hiesey in the 1930s and 40s is a famous one, credited, as Patricia Craig outlines, with demonstrating the gradual evolutionary changes of species as they diverge geographically and climatically, resulting in a new species when two ecotypes of a plant can no longer interbreed to produce a viable hybrid (Craig, 2005, p. 108). In 1952, Hiesey had worked in the Caltech phytotron for some months, (Hiesey, 1953) and later, as Sharon Kingsland has pointed out, Hiesey even contributed a chapter to Went’s foundational book *The Experimental Control of Plant Growth* (Kingsland (2009), p. 317.

¹⁶ Letter from William Hiesey to David Keck, May 8, 1952. David Keck Papers, Box 4, folder ‘Correspondence – William Hiesey’. Archives of the New York Botanical Garden.

¹⁷ Here I am thinking of the “techno-centric” campaigns against infectious diseases throughout the 1950s and the Green Revolution in India in Amrith (2006). On big science in general see, Pickering (1989); Caphsew & Rader (1992); and Galison & Hevly, 1992.

¹⁸ Memo from Van R. Potter, Jun 18, 1963, attached to letter from Willard to Alberty, June 27, 1963. Biotron Papers, Series 06/80, Box 1, file ‘UW Graduate School Correspondence’. Archives, University of Wisconsin–Madison.

¹⁹ A “minimum effective facility is too expensive to be constructed and operated by the great majority of our universities” read the ‘Report on Meeting for Consideration of Controlled Environmental Facility,’ Oak Ridge Tennessee, July 22–23, 1956, p. 7. In Phytotron Records, box 2, file ‘Controlled Environments-discussions/committees before 1962.’ Duke University Archives. This rationale parallels that of the National Nuclear Laboratory at Brookhaven. As Technocrat Lloyd Berkner explained, “Brookhaven provides able scientists the opportunity to carry on the most advanced research requiring great and expensive facilities, without loading the academic staff of any single university with the over-burdening task of utilizing such an expensive facility to the capacity that its cost requires.” Lloyd Berkner to Raymond Seeger, Nov 8, 1954. Tuve Papers, Library of Congress, Box 326; For the definitive history of Brookhaven, see Westwick (2003). For the National Radio Astronomy Observatory, see Munns (2013), chap. 5.

²⁰ Of course, molecular biologists sought function too. In the first issue of the *Journal of Molecular Biology*, John Kendrew advertised that it would publish papers “on the nature, production and replication of biological structures at the molecular level and its relation to function” Zallen (1993), p. 82.

in biochemistry as a natural subject. ... Biochemistry is only one of the basic aspects of physiology, and agriculture only one application. A real sensing of regulation in functioning of the whole organism is still very much for the future in physiology (Hendricks, 1970, pp. 10–11).

Hendricks classed the sciences on their ability to integrate knowledge, and by a measure of their applicability. Agriculture was lower than biochemistry on the latter score as were sciences concerned only with cataloguing, while biochemistry's "inchoate" attempts at synthesis also rendered it below physiology. Though dwelt upon in scientists' memoirs, these measures reveal the hidden thoughts behind their experimental and instrumental choices, namely that while the study of whole organisms required multiple sciences to elucidate the multiple reactions simultaneously existing within any life form, the experimental pursuit of function and the ambition to offer causal significance between phenomena elevated plant physiology.

Among the technological innovations in the middle of the twentieth century, plant physiologists embraced and developed increasingly elaborate controlled environments as a way to produce standard phenotypes to gain basic knowledge. Nicolas Rasmussen has demonstrated the plant physiologists' desire for growth control with the example of George Avery, who said in 1942 that it was a "familiar phrase" that "once normal growth is better understood it should be possible to control abnormal growth" (Rasmussen, 2001, p. 295). Many plant physiologists and other plant scientists believed this had been achieved in controlled environments, especially phytotrons, by the 1960s, and knowledge of growth control travelled rapidly. Undergraduates, for example, learned courtesy of Went's colleague at Caltech, Galston, how, "with any given genotype, tremendous control over growth may be exerted by obvious influences in the environment" such as light and temperature (Galston, 1961, p. 63). Starring in a television documentary, the French phytotronist Jean Paul Nitsch (and student of Went) advertised that France's great phytotron was establishing nothing short of human control over nature's processes by determining "which factors of the environment were crucial for plant growth and to devise means to control these factors at will" (Nitsch, 1972, p. 33). Creating a standard methodology to standardize experimental organisms legitimated such grand claims (and the grander costs of their facilities), but also legitimated big science as the path towards unity for the plant sciences. Throughout the 1950s and 1960s, the increasing availability of phytotrons gave plant physiologists unprecedented experimental control over the environment and that control generated new knowledge for plant physiologists, botanists, horticulturalists, ecologists, foresters, and agriculturalists. New knowledge flourished about the reaction of various plants to specific climatic conditions, or sets of conditions, and about photoperiodism, nutrient uptake and transport, germination and flowering, and photosynthesis (Evans et al., 1985; Galston, 1974; Murneek, 1948; Pennazio, 2005; Somerville, 2000).

The heart of phytotronists' experimental practice was the production of reproducible phenotypes. Phytotrons allowed plant physiologists to confront the immense problem of the complexity of "the living organism" multiplied by the "complex physical system [of] climate," Went told his classes.²¹ The only way forward, he advised his students, was to work with "genetically

homogeneous material analyzing one factor at a time: light, temp [erature]."²² An experiment in 1955 in the Caltech phytotron gives an illustration of this production. Bonner and his student Mary Lou Whaling, the wife of Caltech physicist Ward Whaling,²³ generated standard stocks of experimental organisms to test various concentrations of growth hormones like DCA and 2,4,6-T under the controlled conditions of the phytotron. At the outset, however, the standard, reproducible plants had to be produced. Bonner and Whaling achieved that by taking genetically uniform *Avena* (oat) seeds grown for 96 h in the phytotron's 'red room' at 25 °C, and then selecting only those plants that had grown between 2.75 and 3.25 mm in length from the first node to the tip of the shoot. They discarded the rest. Crucially, the temperature of 25 °C was not chosen arbitrarily. According to Bonner and Whaling's results, it represented the temperature at which the least variation in growth rate occurred. In other words, the goal of limiting variation in the experimental organism was the foundation of the entire practice: on the first page of their experimental notebook a chart displayed how the temperature of 30 °C gave growth anywhere between 0.22 and 0.32 mm/h growth rates. Likewise at 15 °C, the pair saw growth rates of between 0.12 and 0.14 mm/h. At 25 °C, however, the shoots grew the most uniformly between 0.22 and 0.23 mm/h.²⁴

This procedure was replicated throughout the experiments in the Caltech phytotron, the Australian phytotron, and Chouard's *le grand phytotron*. Phytotronists demonstrated that every organism showed a range of growth and development under discrete environmental conditions, and that those conditions could be optimized to produce uniform phenotypes. As Went summarized, "genotypic uniformity can be translated into phenotypic uniformity by subjecting all plants to exactly the same conditions of temperature, light, air movement, watering and nutrition, as closely as possible to their optimal conditions" (Went, 1957a, p. 198). This was not natural, evidently, but optimal; not producing a range of varying organisms but exact duplicates of an organism. Bonner and Whaling's choice of *Avena* as a model organism, for example, was based on its relative dependence on temperature for its growth rate.

In short, Bonner and Whaling's notebook makes it clear that by the mid-1950s, plant physiologists regarded the production of a standard plant under experimentally determined optimal environment as a straightforward step in plant research. Years afterwards, several Australian phytotronists believed that phytotrons had made it possible "to accelerate and make more reproducible many kinds of research on plants at all levels of organization from the sub-cellular to the community" (Evans et al., 1985, p. 207).²⁵ Of course, plant research remained significantly applied: the phytotronists used environmental control for applications from growing orchids, to evaluating the effects of smog, to developing better tomatoes, lettuces, grasses, and tobacco. The Caltech phytotronists, for example, saw their facility as saving valuable time and money because plants up to the F₄ generation could be tested in the controlled conditions of a phytotron, and all without the risk, Bonner quipped, of not the right "kind of summers" ruining everything (Bonner, 1960, p. 73). Practical applications, such as

²¹ Handwritten notes. n.d. (~1951). Frits Went papers. Record Group 3/2/6/1, box 11, folder 38. Archives. Missouri Botanical Garden.

²² Handwritten notes. n.d. (~1951). Frits Went papers. Record Group 3/2/6/1, box 11, folder 38. Archives. Missouri Botanical Garden.

²³ Mary Lou Whaling (nee Slichter) was the wife of Caltech physicist Ward Whaling. See Whaling, Ward. Interview by Shelley Erwin. Pasadena, California, April–May, 1999. Oral History Project, California Institute of Technology Archives. http://resolver.caltech.edu/CaltechOH:OH_Whaling_W. Accessed March 23, 2011.

²⁴ See Bonner and Whaling, Laboratory Notebook: 'Avena Growth (at various temperatures) book iv, 1955–56.' In James Bonner papers, box 42. Archives. California Institute of Technology.

²⁵ The dream of acceleration through efficiency haunted those involved with technologies of genetic modification as well, see Curry (2012), p. 13.

accelerating the discovery of new plant varieties, drove both technological innovation in plant science as well as secured the funding necessary to establish scientific environmental control. The Swedish Royal College of Forestry declared their phytotron to be a boon to their forest industry barely a year after the facility opened, the facility having “made it possible to determine the various photo- and thermoperiodic systems controlling the growth of different provenances of European conifers” (von Wettstein, 1967, p. 7).

At the same time, the new phytotonists assured their patrons that their facilities were also significantly engaged in basic science, leading to dramatically sweeping claims. The plant physiologists in New Zealand, for instance, regarded the early results from their phytotron as “completely re-orienting basic thought on factors determining the optimum growth of plants.”²⁶ Outside Paris, the never understated Chouard declared “phytonics” to be “the methodological key in plant research,” “to which phytotrons ... are the necessary logistics” (Chouard, 1974, p. 4). Over the previous decade, he had asked French government and the CNRS for over two *milliard* francs to build that logistical key.²⁷

4. Making the phenotype an experimental object

As the discipline of plant physiology vied for dominance, the study of the environmental as well as the genetic components of organisms rose to prominence. Having worked in the Caltech phytotron for only at most two years, Bonner and Galston noted in their undergraduate plant physiology textbook how genetic as well as environmental control made it possible to gain an “understanding of the functioning plant” (Bonner & Galston, 1952, p. v). Some of the greatest moments for the plant physiologists came about mid-century when they enrolled other biological disciplines and used phytotrons as the venue where uniform genotypes met uniform environments to produce standard phenotypes. In other words, the creation of a style of science (controlled environments) created the phenotype as an experimental object for biology.

Plant physiologists’ quest to understand the living plant required control over the phenotype. The phenotype, in turn, as Went noted, required control over an organism’s genes and its environments. The later-director of the Duke University phytotron, Paul Kramer, traced the established truism of the plant sciences that a phenotype is the sum of a genotype and an environment back to German physiologist Georg Klebs. Klebs had established the principle that “hereditary potentials” and “environmental factors” combined to produced a plant’s “processes and conditions” that dictated the “quality and quantity of growth” (Kramer & Boyer, 1995, pp. 9–10). For historians, how scientists explored relationships between genotype and phenotype, and what they took to be the nature of that interaction, can help explain the concepts, practices, disciplines, and institutions of the plant sciences in the

twentieth century. The complementary set of papers in this journal issue offer many entry points towards deeper explanations of how plant scientists understood the relationship between genes and environments.²⁸

From the point of view of the plant physiologists, one half of that story is already well developed. As Helen Curry recently concluded, the history of biology has been largely “the story of geneticists’ great successes in the twentieth century” (Curry, 2012, p. 426; Kohler, 1994; Smocovitis, 2009). The geneticists pursued the genotype half of the story of the phenotype, conceived of by the Danish plant physiologist Wilhelm Johannsen as “whatever remained identical in living beings through generations and was therefore amenable to experimentation, just like the molecules in chemistry and the atoms in physics.” (Müller-Wille & Rheinberger, 2012, p. 140). As Peter Bowler noted, classical Mendelian geneticists simplified their work through the assumption that the phenotype approximately expressed the genotype (i.e. genotype $\equiv$ phenotype). Indeed, the great unspoken assumption remained that experimental organisms were considered *identical enough*—not actually identical just sufficiently similar regardless of the environmental conditions of their development (Sapp, 2003, p. 136).²⁹ In practice, Mendelian geneticists assumed that the range of phenotypic variation across a single population was irrelevant because any environmental differences operated on genetically identical organisms (Bowler, 2005, p. 22). Deborah Coen also noted the assumption by the Przibram brothers even as they worked in their controlled environment Vivarium. The brothers muted the category “environment” to seek genetic mutations, not adaptive changes from environmental conditions (Coen, 2006, p. 498). As the century went on, the geneticists’ view of a population as a range of variations took hold in many other fields of biology, especially in evolutionary biology. Surveying his field nearer the end of the twentieth century, Ernst Mayr noted how “most naturalists ... stressed that variability is a normal attribute of populations and

²⁸ As Nicoglou notes in this issue, Anthony Bradshaw challenged the prevailing understanding of most geneticists, who considered that the environment should be removed from the analysis rather than a factor whose effects might be crucial. To them, the environment was just “noise” in the signal. She quotes Conrad Waddington, for instance, explicitly considered that environmental effects were minimal during early development of organisms (Nicoglou, 2015). In contrast, as Erick Peirson explores in detail in this volume, Bradshaw the geneticist “suggested that particular responses to specific environments in individual traits could be under direct genetic control, and thus natural selection could therefore act directly to shape those responses” (Peirson, 2015). Indeed, all the cases of this journal issue tease apart the same shared problem confronting plant scientists in the Cold War era, how to conceptualize and control the stability of plants in the face of environmental variation. In my own case, groups of plant physiologists in phytotrons took the novel step of defiantly aiming to control the environment with stable genotypes, in contrast to the geneticists who accepted the fluctuating environments and sought to understand the genetic basis for stability. Consequently, while those in phytotrons did not engage with the subject of evolution, for the geneticists it formed the ultimate goal of their work.

²⁹ Sapp (2003), p. 136; Peter Bowler stresses this key assumption for work following on from the early Mendelians Bowler (2005). Similar examples abound: Angela Creager noted that early proponents of using radioisotopes as tracers, for instance, often “did not reckon with the biological effects of the radiation they put into their systems,” and even claimed that “low-level amounts of radiation did not disturb fundamental living processes” Creager (2013), p. 223. Likewise, the philosopher of biology, F. S. Bodenheimer, wanted to show how the “environment” was often been the culprit in confounding scientists, and so he described at length a “series of experiments” made under “apparently ‘equal’ condition” by E. Roubaud showed how experimental practices undermined the “assumption of an equal temperature” because thermostats were turned off and on, and cultures removed at length from one environment for cleaning Bodenheimer (1957), p. 66. A wonderful example in the physical sciences, Andrew Pickering showed, was how Joule’s measurement of specific heat was undermined, among other detailed factors, by the body temperature of more than one experimenter in the room with the apparatus, from Pickering (1995), pp. 104–109.

²⁶ Memo to [New Zealand] Minister in Charge of Scientific and Industrial Research from W. M. Hamilton, Secretary, N.Z. D.S.I.R., July 18, 1958. Research Phytotron (Controlled Climate Facilities) 1955–1973, F 1 W3129 (Box 247) 41/7. Archives New Zealand. “For instance, it was previously thought that adequate soil moisture would maintain good growth of ryegrass during the summer months. Work in the Grasslands Unit, however, quickly showed that the optimum temperature for growth of ryegrass was much below the summer soil temperature. Expensive irrigation installations would therefore be a waste of money in hotter parts of New Zealand until plants adapted to these temperatures were available.”

²⁷ Actually, 2,178,032,243 francs. See J. Marechal & L. Barberon, ‘Etat Recapitulatif ... concernant ... le Phytotron et ses Annexes,’ p. 11. Article 870258 Solo-dai, dossier ‘Correspondance générale (3).’ Dépôt des Archives du CNRS (Gif-sur-Yvette).

that what characterizes populations is indeed the kind and amount of variability. Natural selection is meaningless if there is no variation on which it can work" (Mayr, 1980, p. 128).

In contrast, Went and the phytotronists considered variation quite differently from other biologists working on the mechanisms of heredity and evolution, or even earlier controlled environment scientists like the Przibram brothers who had sought to "study the causes of deviance in individual organisms" (Coen, 2006, p. 501). Resisting and actively disputing that assumption recovers one axis of a larger tension among the styles of biology in the twentieth century. The phytotronists understood variation not as a normal attribute of experimental organisms but more as a lack of standardized control over the experimental subject. Went declared, for instance, that phytotronic experiments demonstrated that "at least 80% of phenotypic variability may be due to uncontrolled environment" (Went, 1956, p. 383). In his other publications of the mid-1950s, Went offered photographs (see [Illustration 1](#)) to prove that the example of peas, were "among all easily available seeds ... are genotypically most uniform and can be grown to perfect phenotypic uniformity" (Went, 1957a, pp. 116–117, plate XX). The phytotronists saw variation as mostly an effect of the environment. Writing in *American Scientist* in 1956, the year before he left Caltech for the Missouri Botanical Garden where he built the Climatratron, Went argued that "one of the most important results obtained in these air-conditioned greenhouses and growing rooms is the extent to which biological variability can be reduced." It was variability, Went explained, that was "usually the greatest handicap in biological experimentation and it reduces the reliability of conclusions based on quantitative responses" (Went, 1956, p. 383).

But for Went, neither the conception of variation nor its investigation was in fact the heart of the issue. Rather, the previous failure to control the production of organisms for biological experimentation rendered biology a second-class science. To Went, it was exactly "the uncertainty in conclusions reached in biological experimentation [that] has led physicists and chemists to distinguish themselves as working in the 'exact' sciences" (Went, 1956, p. 383). Went was sensitive to physical sciences' appropriation of the label "exact" as a measure of the status of their science, especially since Went believed that environmental

control resolved the uncertainty. Indeed, for Went, control over the environment served to not only correct a misunderstanding about the nature of variability in organisms, but fulfilled the larger purpose of claiming that the experimental methods in the plant sciences were now on a par with those of chemistry and physics, an especially common theme throughout the life sciences in the twentieth century.³⁰

5. Conclusion

Back in the sunshine of the 1950s, the first phytotron shone like a technological Eden. The Director of the Hungarian phytotron at Martonvásár, Sándor Rajki called phytotrons "the grand experiment" of modern biology.³¹ Phytotrons were the hand of man finally taking hold of capricious nature through controlled environments, at least so indicated a provocative image ([Illustration 2](#)), taken from the booklet promoting the Australian phytotron in the mid-1950s. The computer-like square product was an icon of the phytotronist's conviction that through modernist technological science, nature would be regular, controlled, and reproducible. An Edenic vision, identical heads of wheat would stabilize humanity the phytotronists assured their patrons, when stable genes were united with control over the environment. Nothing assured or promoted that vision better than the computer control panel at the entrance to Climatratron, and at the heart of every phytotron and the Biotron. All epitomized Gilles Deleuze's observation about modern "societies of control" whose social norms are both controlled by and have created "computers" (Deleuze, 1992, p. 6). Desirous of stability and afraid of unpredictability, the Cold War generation understood and responded to their world through trons, including major facilities like phytotrons, Biotrons, and the Climatratron, but also in myriad small ways such as the little known eggatron, assimitron, evapotron, dasotron, and rhizotron, and even the speculative marinatron.

By the mid-1960s, plant physiology was one of the three major disciplines of the plant sciences, while phytotrons were one of the plant sciences' major technological expressions. The state of the plant sciences throughout the United States was described in a comprehensive report from the Panel on the Plant Sciences and sent to President Frederick Seitz of the National Academy of Science in 1966. Compiled from over a thousand questionnaires sent to a representative third of the estimated number of plant scientists active in teaching and research, *The Plant Sciences Now and in the Coming Decade* was a wide-angle snapshot of the plant science community in the middle of the Cold War. As the Panel of the Plant Sciences assessed it, theirs was an era when the vast majority of plant scientists lived and worked in a culture that possessed a heterogeneous mixing of new technological tools and decreasing disciplinary specialization focused on the investigation of "living systems." Though they acknowledged an "understanding of life processes at the molecular level" was "the most important recent development in biology," it was still the living system of the plant, which "extended downward from the whole organism to its parts," that was of paramount importance in agriculture, medicine, civilization, and indeed that "makes life possible" ([Panel for the Plant Sciences, 1966](#), pp. 11, 4). The Panel's ostensive purpose was to argue for a prioritized set of funding goals for the forthcoming decade, most dramatically asking for over 400 million dollars of new funding with a particular emphasis on the support of basic science. The Panel justified such vast new levels of research funding on a "persuasive case for new research

³⁰ As Robert Kohler argued, one of the most influential patron's of science at the Rockefeller Foundation's natural science's division, Warren Weaver, "shared with the physicist-biologists a view of biology as an underdeveloped subject, rich in potential but shackled by unscientific habits and traditions" and consequently identified promising areas of biology to lavishly support, notably biophysics and the early molecular biology, see Kohler (1976), p. 287. Trying to measure up is a theme throughout many life sciences. Sharon Kingsland's study of the American ecologist community, for example, revealed that ecologists throughout the twentieth century also constantly worried about their science's "ability to measure up to other kinds of hypothesis-testing science" Kingsland (2005), p. 3. Ecologists legitimated their science by parsing off applied ecology into conservation (leaving pure science as ecology), creating and defending larger theoretical entities like ecosystems, and appropriating metaphors from the physical sciences, notably Harold Odum's ecosystem circuit diagrams Kingsland (1993), pp. 168–169. Likewise Betty Smocovitis argued that the emergence of the "evolutionary synthesis" in the middle third of the twentieth century "signaled the unification of the biological sciences," on the basis of a coherent theoretical core, and the parsing off of more natural history oriented evolutionary studies for the more rigorous and "successful adoption of experimentation in evolutionary practice through mathematical modeling" Smocovitis (1992b), pp. 1, 3, 18, 20. It was this parsing and adoption which permitted "biology to par with the physical sciences" Smocovitis (1996), pp. 192–193. Finally and most famously in the emergence of molecular biology, Pnina Abir-Am explained "the rise of molecular biology to scientific hegemony by the 1960s" by noting how the molecular biologists retained "their pretences to basic science without the disgrace of entering naturalist 'stamp collecting,' and how "would-be molecular biologists projected themselves as strategic disciples ... of atomic physics" Abir-Am (2003), pp. 502, 505.

³¹ S. Rajki to Paul Kramer, Feb 6, 1976. Paul Kramer papers, box 11, file 'Phytotrons, 1972–76.' Duke University Archives.

Illustration 1. 'Uniformity of Pea Plants.'

From Frits Went, *The Experimental Control of Plant Growth* (Ronald Press Company, 1957), plate XX.

opportunities that have been opened up by new concepts in biochemistry and genetics and by the availability of new technological tools such as computers, controlled environments, and modern physical and chemical instrumentation" (Panel for the Plant Sciences, 1966, pp. xi, iii).

Of course, even garnering such praise from a major national body belied the fact that by the late 1960s, plant physiologists could not claim phytotrons as the route towards the biological unity they hoped to provide. The Panel declared plant science heterogeneous, and in contrast to the grand claims of Went and Chouard, phytotrons were neither the unifying facility for biology nor phytotronics its singular methodology. To Went, phytotrons had long been a facility to facilitate the unification of the biological sciences around the study of the whole plant. As he told a conference of plant scientists in 1962, a "phytotron is designed to serve all of biology, classical as well as molecular, and it should be able to bring all

biological research workers together into a community of common interests, instead of pulling them apart, as the establishment of a separate department of molecular biology tends to do" (Went, 1962, p. 151). But as the Panel on the Plant Sciences' noted, phytotrons remained prominently regarded as only one of the tools available to plant scientists. Similarly, outside the United States, phytotrons occupied a necessary level of biological investigation, a level situated in between the test tube, and the field. This was made explicit during a meeting of the International Biological Program, when a trio from the botanical institute of the university of Würzburg, O. L. Lange, E. D. Schulze, & W. Koch, diagrammed how experiments in phytotrons occurred in the conceptual space between *in vitro* experiments and field trials. They argued that feedback between these levels permitted the "understanding, interpretation, and prediction" in biological science. Moreover, they challenged the emerging molecular understanding of life to argue that every level

Illustration 2. 'What can a Phytotron do for Australia?'

From "What is Needed for an Australian Phytotron?" March 28, 1958. National Archives of Australia, Series A4940, file C2060.

of biological science, whether *in vitro*, in phytotron, or in field, served as interrelated “models” to “explain photosynthetic productivity of plants in the field” (Lange, Schulze, & Koch, 1970, p. 340).³² No one style of experimentation was independently sufficient; the science of the plant required them all.

For reasons that remain understudied, physiology sharply declined in the late-twentieth century (Kremer, 2009, pp. 358–366).³³ Went had exalted back in 1957, “For the future, let us look forward to a further period of steady growth of plant physiology, for the sake of gaining a better understanding of the world around us, and to help the applied botanical disciplines in making plants serve us better” (Went, 1957a, p. 110). But in 1981, the story was radically different. Still at Caltech, Bonner claimed to be the sole remaining plant biologist at Caltech: “the level of competence in plant biology here [at Caltech] is approximately ‘0’ (except for me),” he said.³⁴ In part, the decline of phytotrons was linked to the facility serving to “channel,” in Angela Creager’s formulation, plant physiologists down the path of ever-increasing environmental control that was soon declared unsupportable by the facilities’ major patrons.³⁵ Of course, the molecular view of life also served to channel much of biology towards the molecular; the plant physiologists suffered grievously during what E. O. Wilson later termed the “molecular wars” (Wilson, 1994).³⁶ Bonner noted in 1981 how Caltech’s excellence now lay “in everything concerning molecular biology, genetics, etc.,” though he also pointed out the plant sciences had once more began to attract funding and students interested in genetically engineered crops.³⁷ The truth of the decline is less important than the perception of its causes from the plant physiologists themselves, which tell historians much more about how they viewed their world. More or less following Bonner’s lead, the plant physiologists themselves have laid the blame for the decline of their discipline at the self-interest of public funding of science, which they believed had prioritized applied science over basic science since the 1970s; as the American Society for Plant Physiologists’ president and later historian J. B. Hanson noted, support for “fundamental biology [was] a poorer third” behind “medicine, which received the bulk of the funding” and “agriculture a poor second” (Hanson, 1989, p. 191).

³² The meeting was unfortunately timed, taking place only a year after the Soviet suppression of Czechoslovakia in 1968, severely limiting the involvement of Westerners, especially Americans making Paul Waggoner’s contribution even more remarkable.

³³ One example of how plant physiologists began to lament the declining status of their style of science came from the same meeting as the discussion of models. One of the few Americans able or willing to attend, Paul Waggoner from the Connecticut Agricultural Experimental Station, bemoaned that, “physiologists have had too little effect upon models of canopy performance,” which ecologist modelers had claimed as their own realm. Of other fields, he wrote “sometimes photosynthesis is simply written as a function of light, ignoring well-known facts of physiology... Ignored are the different effects of light upon photochemistry, respiration and stomata; the different effects of temperature upon light and dark respiration ... instead a black box has been used” Waggoner (1970), p. 585. Waggoner’s longer explanation resonated with themes that the phytotonists had long attempted to bring into general biological usage, especially the interrelated complexity of living organisms in favor of a reductionist philosophy.

³⁴ Letter from Bonner to Ben Burr, BNL, Jan 27, 1981. James Bonner papers, file 21.5. Archives. California Institute of Technology.

³⁵ A well-known example was the employment of radioisotopes in biochemistry, ecology, medicine, and molecular biology which effectively “channeled,” Angela Creager argued, “experimenters down pathways of molecular knowledge about life.” The broader claim is how scientific tools shape knowledge: radioisotopes allowed molecular reactions to be traced through cycles, for example Creager (2013), p. 258.

³⁶ Wilson quoted George Wald, a key ally of the co-discoverer of the structure of DNA, James Watson: “there is only one biology, ... and it is molecular biology” Wilson (1994), p. 222.

³⁷ Letter from Bonner to Ben Burr, BNL, Jan 27, 1981. James Bonner papers, file 21.5. Archives. California Institute of Technology.

Though much remains to be done, some recent historical work has partly born out their observations. The conclusions of the 1966 Panel on the Plant Sciences, namely that “continuing basic research progress in the plant sciences contributes to agriculture and medicine” (Panel for the Plant Sciences, 1966, p. vi), no longer resonated with patrons of biotechnology in the 1970s and 80s. Instead, as Sally Hughes noted, biotechnology companies like Genentech valued commercialization for gene therapies, and consequently “molecular biology ... acquired a patently utilitarian dimension” (Hughes, 2011, p. 63).³⁸ In its name and in the ambitions of their users, phytotrons everywhere had indeed pursued basic productive research, skirting the line between the desires of their scientists to engage with fundamental research and the goals of their patrons to food security. Plant physiologists attempted to navigate the space between basic and applied, *in vitro* and *in vivo*, genotype and phenotype, but dualities prevailed in the funding of 20th century science. The larger change that swept biology was the rise of reductionist thinking, and the domination of the new disciple of molecular biology that then recoded the science of life once more, and instead of a combination of genes and environments, a molecular understanding of life emerged.

6. Coda

At the end of the Cold War, controlled environment facilities have once more appeared, and provide a coherent coda to the story of the phytotrons, and to the plants scientists’ ambitions to render a uniform phenotype a standard scientific object. While older phytotrons like the Australian phytotron have been renovated, others such as the Atomic Energy Commission-funded Plant Research Laboratory at Michigan State University have expanded. At the same time, new phytotrons have appeared, including the facility at the University of Saskatchewan. One specialized controlled environment facility, however, has appeared explicitly to provide fundamental research on the pressing ecological issues surrounding the changing structure, growth, and development of whole communities of plants, insects, and small animals. Beginning in 1989, ecological prowess joined with government initiatives in the shape of the UK’s National Environmental Research Council’s Center (NERC) for Population Biology, which subsequently gained the financial commitment of over £1million from the NERC to build a facility containing sixteen 8-cubic-meter environmental chambers where ranges of environmental variables including light, water, air, airflow, carbon dioxide, humidity, and temperature are maintained and electronically monitored. It is called the Ecotron.³⁹

John Lawton was made the director of the NERC Centre for Population Biology in 1989 at Silwood, and the Ecotron. Situated at Silwood Park outside London, though attached to Imperial College, the Ecotron, Lawton said, “is unique among controlled

³⁸ Scientists are often caught between two realities of their working lives. On the one hand, scientists are often idealists working to discover larger truths about nature. On the other, of course, they are often working people with careers and mortgages who work for practical ends. As Angela Creager notes, the use of radioisotopes exploded after World War Two not least because they were “free of charge for cancer research, diagnosis, and therapy,” provided in abundance by the Atomic Energy Commission as an extension of American domestic policy, and even shipped overseas as part of American foreign policy. Creager (2013), p. 154. That “radioisotopes did not turn out to be the ‘medical bullets’ envisioned” Creager (2013), p. 155 only underscores the extent to which science is often shaped by mundane concerns of careers and funding. Indeed, it appears that the attractiveness of promises of therapies underwrote molecular biology’s expansion both in terms of patrons and disciples. Creager thus astutely points to both “ideology” and “infrastructure” as features of the Cold War context of the widespread use of radioisotopes Creager (2013), p. 9.

³⁹ <http://www3.imperial.ac.uk/cpb/history/theecotron> Accessed June 1st, 2014.

environmental facilities in that it attempts to construct, maintain and manipulate entire model ecosystems and simultaneously monitor population dynamics and ecosystem processes" (Lawton, 1996; Lawton et al., 1993, p. 181). Lawton built his facility at the apex of a remarkable scientific career. Lawton had pursued the ecology of insects and bracken since the 1970s, and came to be admired and consequently promoted by the central figure of British ecology, T. R. E. Southwood, throughout the 1980s. Southwood considered Lawton as possessing "the broadest grasp of ecology" of any British ecologist (Gay, 2013, p. 238). The scientific community agreed, awarding Lawton the gold medal of the British Ecological Society in 1987 and electing him a Fellow of the Royal Society in 1989. Lawton was also a member of the 'Silwood Mob,' which as Hannah Gay argues, was a group of like-minded ecologists who emphasized "environmental and epidemiological issues" and who agreed that "the way forward was to combine experiment and field observation with a mathematically informed theoretical approach" (Gay, 2013, p. 1). As such, the Silwood Mob replicated the goals of the last century of biologists from the Przibram brothers in their Vivarium in Vienna, to the early evolutionary biologists for whom "mathematical models were attractive ... since they lent an air of respectability to biology" (Coen, 2006; Smocovitis, 1988, p. 296).

The Ecotron put the Silwood Mob's style of science into practice. Lawton specifically invoked the contrast between field experiments in ecology and laboratory, or, as he said, "Big Bottle" experiments. Unlike ecological experiments done in the field, the Ecotron offered instead a number of advantages for ecology via valid "laboratory experiments" including building a "biologically realistic bridge between the simplicity of mathematical models ... and the full complexity of the real world." Moreover, Lawton argued that "laboratory experiments speed up research" and that facilities like the Ecotron "give a degree of control and replication that is impossible in the field" (Lawton, 1998, p. 178). Consequently, a series of experiments in the Ecotron unraveled the Gordian problem of the environmental effects on species richness and ecosystem dynamics. And then, Lawton the ecologist took this one step further, expanding Went's original phytotron's focus on singular plants in controlled environments to mesocosms in controlled environments. But both Lawton and Went agreed that controlled environment studies enabled the life sciences to reach for a larger theoretical unity that had allowed physics and chemistry, as Went noted, to claim the galling title of the "exact sciences." In contrast, after the wake of a decade of work in the Ecotron, however, Lawton addressed the, to him, questionable proposition that ecology even possessed "general laws." As he noted, "parts of science, areas of physics for instance, have deep universal laws, and ecology is deeply envious because it does not" (Lawton, 1999, p. 177).⁴⁰ The broader struggle between the field and the laboratory over more than the previous century, then, appeared to some as a debate between rival conceptions of biological nature, the difference between laws and simple patterns, between universals and mere models (Ankeny, 2007).

In conclusion, considering the period from the first phytotron to the Ecotron suggests a larger story about, as Lawton indicated, both the nature of biological science and its object of study. Within that larger narrative, we can follow the label "tron." It leads historians to the broad issues of, firstly, rival disciplines vying to establish a unified methodology across the biological science. Secondly, it speaks to the appropriation by the plant sciences of the ideals of the

physical sciences as well as the construction of optimal, standard organism via technologies of environmental control. Finally, thirdly, it tellingly indicates the struggle to construct the phenotype as a legitimate experimental object.

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⁴⁰ Acknowledgement of the doubt surrounding the existence of "any high-level laws in biology" was made by the editors of the volume that emerged out of a workshop "Model Systems, Cases, and Exemplary Narratives" Creager, Lunbeck, & Wise, (2007), p. 4.

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