Making Biology Tropical:

American Science in the Caribbean, 1898-1963

By

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Abstract

This dissertation traces the role of research stations in the emergence of the interdisciplinary field of tropical biology in the United States during the early twentieth century. My study centers on the community of tropical biologists that formed around two stations administered by the Harvard zoologist Thomas Barbour from the 1920s through 1940s: Harvard's laboratory and garden at "Soledad," an American-owned sugar plantation in Cienfuegos, Cuba, and Barro Colorado Island, a field station and nature reserve in Gatun Lake in the Panama Canal Zone. My analysis of these sites of scientific practice illuminates how the experience of living and working in Caribbean environments transformed American ideas about tropical nature.

Historians of science have proven the importance of stations in shaping the discipline of biology within the US. By following American biology into the tropics, however, I also raise important issues in the history of science and empire not confronted by the existing historiography. The colonial and neocolonial context of twentieth-century tropical fieldwork aligned US biologists with expanding US economic, military, and political networks in the Caribbean. This alignment had material effects not only on the places where biologists chose to work, but also on the patronage structures available to scientists and the organization of station labor. Fieldwork at tropical stations, however, did not simply serve corporate interests, but also raised unique intellectual questions for biologists. Americans had long imagined the tropics as luxuriant and rich in species, but their knowledge was limited to expeditionary science and taxonomic collections. Station science transformed understandings of tropical nature by allowing long-term, place-based investigations. This new approach reoriented research toward the

ecological and evolutionary causes of tropical species richness. Both in its contributions to the concept of species diversity and in its approach to diversity as a resource, tropical biology has also played an under-appreciated role in the formation of the "biodiversity" discourse.

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At the end of the long process of writing a dissertation, it can be difficult not to feel isolated. In looking back to acknowledge all those who have helped me along the way, however, I am happily reminded that I have not been alone at all. I am thankful for having found a field and a research topic that has allowed me to meet so many generous and thoughtful people. This dissertation has indebted me to so many that I am sure I will forget to name some of you here. For this reason, and for this work's many other flaws, I am solely at fault.

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This dissertation would be much poorer without the benefit of feedback from several remarkable communities of scholars. I have been wildly fortunate in finding no less than three scholarly homes at the University of Wisconsin-Madison. First, it has been a great pleasure to be a part of such a collegial, functional, and easy-going department as History of Science. History of Science gave me the grounding I needed to see this project through to the end. Fellow grads, I am already sorely missing our Friday Happy Hours. The Center for Culture, History, and Environment (CHE) of the Nelson Institute for Environmental Studies has been my second home. At CHE's regular gatherings, community events, and place-based workshops, I have found a lively intellectual forum and a supportive group of friends. Last but not least, I want to show my appreciation for the extraordinary opportunities provided to me by the faculty and graduate students of the CHANGE-IGERT. CHANGE gave me a space to confront the challenge of interdisciplinarity head-on.

I am grateful for many helpful questions and comments from audience members at presentations I have given at the History of Science Society and American Society for Environmental History annual meetings, the Columbia History of Science Group, CHE graduate student colloquia, history of science brown bags and colloquium over the years. The participants of a seminar with Lynn Nyhart on the History of Biogeography, a seminar with Gregg Mitman and Florence Hsia on Scientific Expeditions, and the History and Philosophy of Biology reading group also helped to hammer my work into much better shape. I must express my gratitude to the participants of the 2012 ASU-MBL History of Biology Seminar "From Linnaeus to the Encyclopedia of Life: Tracking Diversity in the Natural World" for much fruitful cross-fertilization of ideas. The University of Wisconsin-Madison Writing Center Dissertation Camp also found me at a key moment, allowing me to work through a block and understand myself better as a writer. I have been extremely lucky for the constructive criticism and support of two dissertator groups. Thank you Meridith Beck Sayre, Andrew Case, Jennifer Holland, Charles

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Introduction

What is Tropical Biology?

"It is far easier to define tropical biologists than to define tropical biology," writes ecologist Robin L. Chazdon in her introduction to a collection of the field's foundational writings, "Tropical biology is, simply, what biologists choose to study in the tropics."[1] Like their subject, tropical biologists' interests are diverse. From one point of view, tropical biology is geographic specialization within biology, but it can also be thought of as an interdisciplinary field, or interdiscipline, spanning the full range of biological subdisciplines and bringing them together around the study of the global tropics.[2] Tropical biologists may focus on particular groups of plants or animals. Their approach may emphasize taxonomy, behavior, physiology, evolution, or ecology—although ecology becomes the touchstone when confronting the complexity of tropical ecosystems.[3] Tropical biologists also have a range of overlapping motivations, from a curiosity about the puzzles of theoretical biology raised by diverse tropical species and communities, to a

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1. The title refers to "tropical forest biology," but the book is meant to be "an anthology of classic and foundational works in the field of tropical biology, with an emphasis on tropical forests" and "tropical biology" is used throughout. Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology: Classic Papers With Commentaries* (Chicago, IL: University of Chicago Press, 2002), 1, 2.

2. I have chosen the term "interdiscipline" because "subdiscipline" implies a degree of specialization and boundary work separating tropical biology from other areas of biology than accurately describes the field. For other's use of the term, see Jerry A. Jacobs, and Scott Frickel, "Interdisciplinarity: A Critical Assessment," *Annual Review of Sociology* 35 (2009): 43-65; Scott Frickel, "Building an Interdiscipline: Collective Action Framing and the Rise of Genetic Toxicology," *Social Problems* 51, no. 2 (2004): 269-87; J. Mayone Stycos, "Demography as an Interdiscipline," *Sociological Forum* 2, no. 4 (1987): 616-18. Erik Ellis does not use this term, but the case of marine biology is an interesting comparison, Erik Ellis, "What is Marine Biology?: Defining a Science in the United States in the Mid 20th Century," *History and Philosophy of the Life Sciences* 29, no. 4 (2007): 469-93.

3. Indeed, a measure of how central ecology is to the study of biology in the tropics is the way that "tropical ecology" is used almost interchangeably with "tropical biology" in synthetic publications and professional contexts. For simplicity I have used "tropical biology" to refer to the general field of study and "tropical ecology" only when referring specifically to ecological work.

love of fieldwork in the tropics, to a drive to find ways to conserve and sustainably use tropical biodiversity.

Amid their own diversity, however, tropical biologists are united by place. Place unites the field in two senses. First, at its simplest, tropical biology is the study of life in the equatorial region of the globe. As one textbook author has it, the question of what tropical biology or ecology is "may seem akin to asking questions such as, Who is buried in Grant's tomb?"[4] In fact, tropical biology is more than just a geographical pigeonhole. As we will see, studying tropical biology raises the question of what makes life in the tropics unique, and alternatively, what the study of tropical life might be able to tell scientists about life everywhere. As Chazdon explains, tropical biology is also fundamentally "about understanding the evolutionary and ecological processes in the tropics that generate such bewildering diversity of life on earth."[5] But there is, I argue, a second way that tropical biology is united by place: through its history. The emergence of "tropical biology" is inseparable from the history of the particular places where biologists studied the tropics and came to formulate their questions about the biological uniqueness of the tropics. This dissertation traces the emergence of tropical biology as an area of study for American scientists by examining the history of the places—biological field stations in the Caribbean—where American scientists first encountered the tropics in the first half of the twentieth century.

The first professional associations, university programs, and specialty journals in tropical biology did not appear until the 1960s, and if one judges by the explosion of scientific

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4. John C. Kricher, *Tropical Ecology* (Princeton, NJ: Princeton University Press, 2011), 1.

5. Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology*, 2.

publications and increasing reference to the subject within popular discourse about global environmental conservation and economic development, the field took off in the 1970s and 1980s (see Appendix 1). Nevertheless, this dissertation begins at the turn of the twentieth century, focusing especially on the 1920s and 1930s, because the emergence of tropical biology as an intellectually and institutionally coherent field at mid-century fundamentally depended on the much earlier establishment of stations for basic tropical research. The oldest continuously operating tropical research station in existence today is Barro Colorado Island (BCI), Panama, founded in 1923 (see Appendix 2). BCI played a key role in building a community of Americans biologists by providing them with an easily accessible location for basic research in the tropics. BCI itself built on even earlier efforts to establish American stations in the tropics, beginning at the turn of the twentieth century, as American biologists sought to engage and compete with Europeans in the study of tropical organisms and, moreover, as the attention of the broader American public turned toward Southward economic and political expansion. This dissertation begins with the first movement among American botanists to establish tropical research institutions in the 1890s, but focuses especially on BCI and its lesser-known sister station, Harvard's garden and biological laboratory at Soledad, Cienfuegos, Cuba. Both stations were managed by the Harvard zoologist Thomas Barbour from the 1920s through 1940s. They were not only among the longest-lived and most visited American-run stations in the early twentieth century, but they also set a precedent by embodying Barbour's broad vision tropical biology, which included and valued agriculture and medicine, but placed basic biological research at the core. After Barbour's death, BCI in particular became the primary model for the rapidly increasing numbers of biological field stations established in the tropics from the 1960s through the twenty-first century.

If tropical biology is global in scope, why focus on American scientists? Not only were these key institutions in the development of tropical biology run by Americans, researchers from the United States continue to be predominant within tropical biology today. Although the representation of scientists from tropical nations has greatly increased in recent years, the number of publications in tropical biology authored by American researchers far exceeds those contributed by scientists from any other nation.[6] The field's largest professional society, the Association for Tropical Biology and Conservation, also has a majority of members from the US.[7] During the twentieth century, US researchers broke into a geographical area previously dominated by Europeans, whose countries maintained large tropical empires. Along with a growing number of Asian scientists, Europeans today continue to produce a major component of work in the former colonies of the Old World tropics.[8] Yet research in Central America, completed overwhelmingly by scientists from the US, maintains a disproportionate representation—even as tropical studies in general remain strongly underrepresented.[9]

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6. Stocks et al. found that scientists from the US made up 35-45% of authors of papers on tropical ecology. All other countries were represented by 12% or fewer authors. They also pointed out that, among countries with high rates of publication, only in Brazil and Mexico do resident scientists make up a majority of publishing authors. The number of authors from European and South American countries were about equal. Elizabeth Braker, "The Changing Face of Tropical Biology?," *Tropinet* 11, no. 1 (2000): 1-2; Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology*, 3; Gabriela Stocks, et al., "The Geographical and Institutional Distribution of Ecological Research in the Tropics," *Biotropica* 40, no. 4 (2008): 397-404.

7. Elizabeth Braker, "The Changing Face of Tropical Biology?", 1.

8. Ibid.; Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology*, 3.

9. Although Americans are over-represented in scientific publishing more generally, this specific geographic pattern can only be explained by examining the history of the emergence of tropical biology within the American scientific community. Antonio Gálvez, et al., "Scientific Publication Trends and the Developing World: What Can the Volume and Authorship of Scientific Articles Tell US About Scientific Progress in Various Regions?," *American Scientist* 88, no. 6 (2000): 526-33.On the underrepresentation of tropical research, see for example Laura J

Examining the history of American tropical biology explicitly *as* an American scientific community, then, allows us to address the question of why and how the emerging community of tropical biologists largely excluded Latin American researchers until the 1960s. What opportunities for international cooperation were available? How were they lost? Tropical biologists today recognize a need to increase the national, geographic, and racial diversity of their ranks if their work is to have a broader impact in the places they study. This history provides important context for understanding the problem, as well as how it finally came to be recognized as a problem, rather than simply a matter of course. Certainly, future work is needed to fully explore the history of tropical sciences from Latin American and Caribbean points of view.[10] Nevertheless, the history of the emergence of an American community of tropical biologists also provides a critical corrective to the existing historiography of American science, which has largely been confined within national borders and defined by exceptionalist narratives. Following American biology to its tropical outposts—sites more readily associated with European colonial science—troubles such narratives.

A Place-based Science

A central irony of tropical biology, then, is that it is place-based science that has historically been engaged in primarily by people from outside of that place. For Americans and Europeans, to

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Martin, et al., "Mapping Where Ecologists Work: Biases in the Global Distribution of Terrestrial Ecological Observations," *Frontiers in Ecology and the Environment* 10 (2012): 195–201; Daniel H. Janzen, "The Impact of Tropical Studies on Ecology," in *Changing Scenes in Natural Sciences, 1776-1976*, ed. Clyde E. Goulden (Philadelphia, PA: Academy of Natural Sciences, 1977).

10. Excellent models of this much-needed approach include Leida Fernández Prieto, *Cuba Agrícola: Mito y Tradición, 1878-1920* (Madrid: Consejo Superior de Investigaciones Científicas, 2005); Stuart McCook, *States of Nature: Science, Agriculture, and Environment in the Spanish Caribbean, 1760-1940* (Austin, TX: University of Texas Press, 2002); Julyan G. Peard, *Race, Place, and Medicine: The Idea of the Tropics in Nineteenth Century Brazilian Medicine* (Durham, NC: Duke University Press, 1999).

study tropical life meant traveling to the tropics. Scientific expeditions, coinciding with the era of European empire building, brought collections of specimens from around the world into the metropolitan centers of Europe, allowing studies of the classification and geographical distribution of living things to become global. As tropical biologists today are quick to point out, tropical travels sparked many of biology's greatest insights in evolution and biogeography during the nineteenth century, from the voyages of Alexander von Humboldt to those of Charles Darwin and Alfred Russel Wallace.[11] Some have even pointed to the tropical roots of ecology in Eugenius Warming's sojourn in Brazil.[12]

While observations made in the tropics have long been integral to natural history, the study of life in the tropics did not become a coherent field of research until the twentieth century with the establishment of permanent biological field stations. Though largely overlooked in the historiography of American science, plans to establish US tropical research stations began as early as the 1890s. The American movement to establish research station in the tropics was part of a larger groundswell in biology supporting the establishment of research stations. Among the most influential were the Naples Biological Station, Italy, founded in 1872, and the Marine Biological Laboratory at Woods Hole, Massachusetts, founded in 1888, whose institutional structure served as model for several US stations in the Caribbean. As we will see, the Treub Laboratory at the Buitenzorg Gardens, Java, established in 1884, also stood out as an exemplar of the possibilities of tropical research for American scientists.

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11. See for example textbooks and anthologies such as Carl F. Jordan, *Tropical Ecology* (Stroudsburg, PA: Hutchinson Ross Pub. Co. Distributed world wide by Academic Press, 1981); Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology*; John C. Kricher, *Tropical Ecology*.

12. R. J. Goodland, "The Tropical Origin of Ecology: Eugen Warming's Jubilee," *Oikos* 26, no. 2 (1975): 240-45.

Stations gave scientists from the temperate zone an institutional foothold in the tropics, allowing a much larger number and broader array of biologists to work in the region. Tropical research stations greatly increased the number of American researchers who could work in the tropics, by removing much of the hardship of organizing an expedition and traveling far from home into territories full of real and imagined tropical dangers. Thus, research stations went a long way toward building up a population of American researchers with experience in tropical fieldwork. Moreover, even when no formal research program was articulated and visiting researchers collaborated only minimally, working or having worked at the same site was integral to fostering a nascent community of tropical biologists.

Not only did research stations offer the ability to access tropical organisms and environments while living in comfort, health, and safety, but they also allowed a wider and qualitatively different set of research practices. A field station can be as primitive as a shack or a tent in the forest, but a permanent biological research station may incorporate a variety of scientific spaces. At Soledad, due to its origins as an agricultural experiment station, there were experimental plots, botanical gardens, and greenhouses, as well as a biological laboratory, dormitory, and access to uncultivated land within a day's journey. BCI was set on a forested island nature reserve, and its buildings housed not only a laboratory but also a herbarium and library. Stations thus can flexibly suit many different kinds of biological research and allow the integration of work across different kinds of scientific spaces.

Importantly, whereas expeditions and surveys are best-suited toward the collection of specimens and measurements, permanent stations orient work toward the study of living organisms *in situ* over longer periods of time. Field stations in the tropics opened up studies in ecology, physiology, and behavior, whereas previously only taxonomic and biogeographic work had been possible. But this is not a story of a shift from "old" natural history approaches to "new" biological ones. By concentrating research in a small area, stations continued to encourage taxonomic and distribution studies, but at a much more intensive level—particularly important in the tropics where the numbers of species and especially endemic species (those limited to very small geographic ranges) can be extraordinarily high. A single station sacrifices the claims to geographically extensive knowledge available to expeditionary and survey science—except with recourse to claims of the regional or environmental representativeness of a particular site or through access to a wide range of nearby environments.[13] They make up for this, however, by the intensity and longevity of research possible at a station. Permanent stations also permit biologists to make the long-term observations and experiments necessary for ecological research—indeed, historian of science Sharon Kingsland has argued for the importance of field stations in the establishment of ecology as a discipline.[14] Finally, the permanence of research stations encourages the accumulation of related—however tightly or loosely—studies. Biologists themselves have commented on this quality of station work. Mary V. Price and Ian Billick note that "place-based ecological research" is often conducted "by loose, often interdisciplinary consortia."[15] Similarly, BCI's Egbert Leigh argues for "the importance of

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13. Mountain stations are a prime example of the latter. Jeremy Vetter, "Rocky Mountain High Science," in *Knowing Global Environments: New Historical Perspectives on the Field Sciences*, ed. Jeremy Vetter (New Brunswick: Rutgers University Press, 2011). For a discussion of survey science, see Robert E. Kohler, *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950* (Princeton, NJ: Princeton University Press, 2006).

14. Sharon Kingsland, "The Role of Place in the History of Ecology," in *The Ecology of Place: Contributions of Place-Based Research to Ecological Understanding*, ed. Ian Billick, and Mary V. Price (Chicago, IL: University of Chicago Press, 2010). See also Sharon E. Kingsland, *The Evolution of American Ecology, 1890-2000* (Baltimore, MD: Johns Hopkins University Press, 2005).

15. Ian Billick, and Mary V. Price, *The Ecology of Place: Contributions of Place-Based Research to Ecological*

'unity of place'" which allows "many different kinds of study, done in one small area [to] cohere into a unified picture."[16] These qualities—multiple scientific spaces, focus on living organisms in their environment, intensive and long-term research—are representative of permanent biological field stations in general. Nevertheless, they were were crucial for the development of tropical biology, both because the organisms and environments of the tropics were very little studied in comparison to those of the temperate zone and because of the much greater numbers and variety of species living in the tropics.

Tropical Biology and American Empire

But the permanence of research stations is not guaranteed. As we will see, BCI and Soledad were exceptional in their long life spans—both survive today, although in forms that might be surprising to their founders. No other stations for basic tropical research achieved the same level of stability during the first half of the twentieth century. Some institutions that we will also encounter, such as the tropical botanical station at Cinchona, Jamaica, or the stations begun by William Beebe of the New York Zoological Garden in British Guiana (now Guyana), were begun with high hopes but now lie abandoned, known almost exclusively by local hikers and foreign ecotourists. To achieve institutional stability, station founders and administrators had to maintain connections to streams of funding, transportation infrastructure, and steady sources of visiting researchers. Even more fundamentally, they had to ensure long-term access to suitable land in the tropics.

Understanding (Chicago, IL: University of Chicago Press, 2010).

16. Egbert Giles Leigh Jr., "Barro Colorado," in *Encyclopedia of Islands*, ed. Rosemary G. Gillespie, and D. A. Clague (Berkeley, CA: University of California Press, 2009).

Thus, in the twentieth century, to work in the tropics necessarily entangled American biologists in the networks of empire—whether this meant working within European colonies or their own growing avenues of economic, political, and military influence in the Caribbean and Pacific. Through the nineteenth century, the US ideology of manifest destiny had promoted the vision of a hemispheric empire, validating efforts to expand to the South as well as West.[17] The turn of the twentieth century saw the US acquire a scattered tropical empire, as Spain ceded Cuba, Puerto Rico, the Philippines, and Guam following the 1898 Spanish-American War and the 1903 Hay–Bunau-Varilla Treaty granted the US control of the Panama Canal Zone. In the decades that followed, a policy of "dollar diplomacy" supplanted the acquisition of outright colonies, but military interventions remained a continued threat to the sovereignty of Caribbean nations.[18] American agribusinesses—most notoriously the United Fruit Company—became major landholders throughout the Caribbean and Latin America and exerted increasing influence on the economies, politics, and landscapes of the region.[19]

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17. Matthew Pratt Guterl, *American Mediterranean: Southern Slaveholders in the Age of Emancipation* (Cambridge, MA: Harvard University Press, 2008), 12-46; Louis A. Pérez, *Cuba in the American Imagination: Metaphor and the Imperial Ethos* (Chapel Hill, NC: University of North Carolina Press, 2008), 27-32; Shelley Streeby, *American Sensations: Class, Empire, and the Production of Popular Culture*, American Crossroads (Berkeley, CA: University of California Press, 2002), 20-22.

18. See for example, Mary A. Renda, *Taking Haiti: Military Occupation and the Culture of US Imperialism, 1915-1940* (Chapel Hill, NC: University of North Carolina Press, 2001); Emily S. Rosenberg, *Financial Missionaries to the World: The Politics and Culture of Dollar Diplomacy, 1900-1930* (Durham, NC: Duke University Press, 2003). On the cultural history of American imperialism, see Amy Kaplan, and Donald E. Pease, eds. *Cultures of United States Imperialism* (Durham, NC: Duke University Press, 1993); G. M. Joseph, Catherine LeGrand, and Ricardo Donato Salvatore, eds. *Close Encounters of Empire: Writing the Cultural History of US-Latin American Relations* (Durham, NC: Duke University Press, 1998); Pennee Bender, and Yvonne Lassalle, eds. *Islands in History: Perspectives on US Imperialism and the Legacies of 1898*, Radical History Review 73 (Durham, NC: Duke University Press, 1999); Ann Laura Stoler, *Haunted by Empire: Geographies of Intimacy in North American History*, American Encounters/Global Interactions (Durham, NC: Duke University Press, 2006); James T. Guterl Matthew Pratt Campbell, and Robert G. Lee, *Race, Nation, and Empire in American History* (Chapel Hill, NC: University of North Carolina Press, 2007); Shelley Streeby, "Empire," in *Keywords for American Cultural Studies*, ed. Bruce Burgett, and Glenn Hendler (New York, NY: New York University Press, 2007).

19. Jason M. Colby, *The Business of Empire: United Fruit, Race, and US Expansion in Central America* (Ithaca,

The connection of American science to such colonial and neocolonial ventures is most obvious in the realm of tropical medicine and agriculture. The entomologists, engineers, and medical doctors who worked to control disease-bearing mosquitoes in Cuba and Panama were as effective as military forces in consolidating US control over the island and Canal Zone respectively. More than that, recent scholarship has shown how such work helped to provide humanitarian justifications for invasion.[20] The protection of vast banana, sugar, and rubber monocultures from pests and disease are also examples of how scientific agriculture, chemistry, economic botany and entomology, have been essential to the practices of US corporations throughout the Caribbean and Central America in the twentieth century.[21] These examples are relatively well-known, although the role of science, medicine, and agriculture in the growth of

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NY: Cornell University Press, 2011); Marcelo Bucheli, *Bananas and Business: The United Fruit Company in Colombia, 1899-2000* (New York, NY: New York University Press, 2005); John Soluri, *Banana Cultures: Agriculture, Consumption, and Environmental Change in Honduras and the United States* (Austin, TX: University of Texas Press, 2005); Richard P. Tucker, *Insatiable Appetite: The United States and the Ecological Degradation of the Tropical World* (Berkeley, CA: University of California Press, 2000); Steve Striffler, *In the Shadows of State and Capital: The United Fruit Company, Popular Struggle, and Agrarian Restructuring in Ecuador, 1900-1995* (Durham, NC: Duke University Press, 2002); Michelle Harrison, *King Sugar: Jamaica, the Caribbean, and the World Sugar Industry* (New York, NY: New York University Press, 2001); César J. Ayala, *American Sugar Kingdom: The Plantation Economy of the Spanish Caribbean, 1898-1934* (Chapel Hill, NC: University of North Carolina Press, 1999); Catherine LeGrand, "Living in Macondo: Economy and Culture in a United Fruit Company Banana Enclave in Colombia," in *Close Encounters of Empire: Writing the Cultural History of US-Latin American Relations*, ed. G. M. Joseph, Catherine LeGrand, and Ricardo Donato Salvatore (Durham, NC: Duke University Press, 1998).

20. Mariola Espinosa, *Epidemic Invasions: Yellow Fever and the Limits of Cuban Independence, 1878-1930* (Chicago, IL: University of Chicago Press, 2009); Paul Sutter, "Nature's Agents or Agents of Empire? Entomological Workers and Environmental Change During the Construction of the Panama Canal," *Isis* 98, no. 4 (2007): 724-54. See also John Robert. McNeill, *Mosquito Empires: Ecology and War in the Greater Caribbean, 1620-1914* (New York, NY: Cambridge University Press, 2010).

21. John Soluri, *Banana Cultures*; Richard A. Overfield, "Science Follows the Flag: The Office of Experiment Stations and American Expansion," *Agricultural History* 64, no. 2 (1990): 31-40; Greg Bankoff, "Breaking New Ground? Gifford Pinchot and the Birth of 'Empire Forestry' in the Philippines, 1900-1905," *Environment and History* 15, no. 3 (2009): 369-93; Deborah Fitzgerald, "Exporting American Agriculture: The Rockefeller Foundation in Mexico, 1943-53," *Social Studies of Science* 16, no. 3 (1986): 457-83; Gregg Mitman, and Paul Erickson, "Latex and Blood: Science, Markets, and American Empire," *Radical History Review* 107 (2010): 45-73. See also Greg Grandin, *Fordlandia: The Rise and Fall of Henry Ford's Forgotten Jungle City* (New York, NY: Metropolitan Books, 2009).

American hegemony in the Caribbean has not yet received nearly the intense level of attention that has been focused on science in the European empires.

As we will see through this dissertation, however, even the rise of a community of "pure" or "basic" tropical biologists cannot be separated from the expanding networks of US corporate and government influence in the Greater Caribbean. Certainly, permanent sites for agricultural or medical research—agricultural experiment stations, plant introduction gardens, hospitals and medical laboratories—exploded in this context, while institutions for research without immediate or direct economic applications had much more difficulty succeeding in the long run. Those that did were closely aligned with the broader networks of US interests in the region. Barbour's involvement with both of the longest lasting stations, BCI and Soledad, is no coincidence—his high level connections in the US business community and government worked to ensure outside support for basic tropical research. Biological stations like Soledad and BCI drew American biologists who sought to catalogue the vast flora and fauna of the tropics, who wanted to observe the behavior and intricate co-adaptations of ecological communities that so differed from the Northern forests of their childhood, and who, ultimately, wanted to understand the causes behind these differences and an explanation for the tropics' great diversity of species. These scientists' pursuits cannot be reduced solely to the economic interests of US companies and government agencies. Yet, neither can they be separated from the context that allowed the US biological community—a community lacking tropical environments at home—to become dominant in the field of tropical biology by the second half of the twentieth century. Barbour helped to create space for tropical biology not by "carving out" space for basic research, but by linking scientific spaces into closer connection with the broader structures of US hegemony in the region.

Caribbean Encounters: From Tropicality to Biodiversity

This dissertation explores not only the early twentieth institutional foundations and political context of tropical biology, but also examines how American biologists' ideas about the tropics were altered by their experiences working at particular sites in the Caribbean. From a straightforward geographic standpoint the tropics are defined as the area of the globe around the equator between 23° 26′ 16″ North and South latitudes, characterized by a lack of frost except at high elevations and the predominance of seasonal fluctuations in rainfall rather than temperature. The tropics are nevertheless a diverse and contested region, containing not only a multiplicity of environments and peoples, but also loaded (perhaps burdened) with powerful meanings and imagery to outsiders.

Like the history of science and empire, the cultural and intellectual history of ideas of the tropics has focused tightly on European examples. Although we do not have a comprehensive treatment of the tropics in the American imagination, Americans drew on similar cultural resources to imagine the tropical "other."[22] A discourse of "tropicality" pictured the equatorial regions as a region of unbounded abundance and luxuriance—a place both attractive and dangerous, at once a tropical "paradise" and a "green hell." In European and American popular imagination the tropics were, and to an overwhelming degree still are, seen as rich, fecund—sometimes dangerously so—and full of as yet untapped potential. Within this discourse, tropical

22. Some recent headway has been made however. See Candice Millard, *River of Doubt: Theodore Roosevelt's Darkest Journey* (New York, NY: Doubleday, 2005); Wade Davis, *One River: Explorations and Discoveries in the Amazon Rain Forest* (New York, NY: Simon & Schuster, 1996). On American racial science and tropicality, see Warwick Anderson, *Colonial Pathologies: American Tropical Medicine, Race, and Hygiene in the Philippines* (Durham, NC: Duke University Press, 2006); Paul A. Kramer, *The Blood of Government: Race, Empire, the United States, & the Philippines* (Chapel Hill, NC: University of North Carolina Press, 2006).

people appeared as backward, lazy, and spoiled by the abundance offered by tropical nature, unable to efficiently develop the resources of their own lands. Like Orientalist discourses, the exoticized imaginary geography of the equatorial regions functioned to justify the colonization and exploitation of its people and environments.[23]

Scientists' views certainly ran in parallel with broader cultural understandings, and, as we will see, applied science in the tropics enacted narratives about the need for outsiders to rationally develop and civilize tropical countries. At the same time, to biologists, the tropics have also been a place that raises fundamental theoretical questions. Questions about the biological differences of the tropics—Why are there so many species? What is the effect of constant day lengths and high humidity on tropical organisms?—remain, however, deeply connected to the tropicality discourse. It is worth considering why the idea of "temperate biology" may seem an odd category, while "tropical biology" likely conjures images of lush rainforests and brightly colored birds. The formulation of "tropical biology" required a standpoint that made tropical nature the "other" and nature in the North temperate zone a familiar biological norm, an unmarked category. If this were not the case, the question would be why there are fewer species in temperate climates (indeed these are just the sort of questions that tropical biologists eventually came to ask). This is not to say that biological questions about the tropics did not

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23. David Arnold, "'Illusory Riches': Representations of the Tropical World, 1840-1950," *Singapore Journal of Tropical Geography* 21, no. 1 (2000): 6-18; Nancy Stepan, *Picturing Tropical Nature* (Ithaca, NY: Cornell University Press, 2001); Felix Driver, "Imagining the Tropics: Views and Visions of the Tropical World," *Singapore Journal of Tropical Geography* 25, no. 1 (2004): 1-17; Felix Driver, and Luciana Martins, *Tropical Visions in an Age of Empire* (Chicago, IL: University of Chicago Press, 2005); Beth Fowkes Tobin, *Colonizing Nature: The Tropics in British Arts and Letters, 1760-1820* (Philadelphia, PA: University of Pennsylvania Press, 2005); Krista A. Thompson, *An Eye for the Tropics: Tourism, Photography, and Framing the Caribbean Picturesque* (Durham, NC: Duke University Press, 2006); Mimi Sheller, *Consuming the Caribbean: From Arawaks to Zombies* (New York, NY: Routledge, 2003).

change in content through the twentieth century. As we will see through the course of this dissertation, the questions posed by life in the tropics were shaped by the long-term experience of work at research stations, in dialog with broader trends in the history of biological thought. At the turn of the twentieth century, many botanists interpreted the large numbers of species and other floristic differences of the tropics as a question of physiological adaptation to physical factors of light and humidity. By the 1930s, the problems of complex ecological communities came to the fore. By the 1960s, the question of how trophic dynamics might control tropical species diversity was a vital area of research.

Although tropical biologists were never large in number, their work has in turn fed into broader discourses about tropical nature and biodiversity. The term "biodiversity" was not coined until 1980 and, as David Takacs has shown, is not merely synonymous with species richness, but also connotes a wide range of environmental values—indeed it has broad and flexible enough meanings to bring together a diverse set of stakeholders.[24] Timothy Farnham points out that "biological diversity" was in use as early as the 1960s—although perhaps without the political valences of the contraction. Meanwhile, Libby Robin convincingly argues for an equally deep history for the political currency of the idea in conservation efforts outside of North America.[25] Historians and philosophers of science have devoted much less attention to the intellectual history of "species diversity" or "species richness," which formed a core intellectual precursor to biological diversity, and which, I argue, tropical studies played a key role in

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24. David Takacs, *The Idea of Biodiversity: Philosophies of Paradise* (Baltimore, MD: Johns Hopkins University Press, 1996).

25. Timothy J. Farnham, *Saving Nature's Legacy: Origins of the Idea of Biological Diversity* (New Haven, CT: Yale University Press, 2007); Libby Robin, "The Rise of the Idea of Biodiversity: Crises, Responses and Expertise," *Quaderni* 76 (2011): 25-37.

articulating.[26] This dissertation begins to explore how biologists shifted from a language of tropicality, which saw species diversity as an exotic attribute of the tropics, to one of biodiversity, which casts it as a measurable global phenomenon.

American biologists discussed the need for research in the "tropics" rather than in the "Caribbean." Nevertheless, I have purposefully chosen to focus on US biology in the Caribbean in this dissertation, rather than on biology in the tropical territories of the US more generally. While the Philippines and Hawaii were also important sites for US tropical research, concentrating on the Caribbean shifts attention from science in formal colonial settings, to a much wider variety of imperial sites (i.e. a range of informal colonies, the colonies of European nations) and their interrelations.[27] Following the practices of Caribbean historians, I take a broad view of the region and include both the islands of the West Indies and a broad swath of Central

26. See for example Daniel P. Faith, "Biodiversity," in *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta (http://plato.stanford.edu/archives/fall2008/entries/biodiversity/, 2008); Sahotra Sarkar, *Biodiversity and Environmental Philosophy: An Introduction*, Cambridge Studies in Philosophy and Biology (Cambridge: Cambridge University Press, 2005); Markku Oksanen, and Juhani Pietarinen, *Philosophy and Biodiversity*, Cambridge Studies in Philosophy and Biology (Cambridge: Cambridge University Press, 2004); Irama Núñez, et al., "La Biodiversidad: Histora y Contexto de Un Concepto," *Interciencia* 28, no. 7 (2003): 387-93. Species diversity was in use by ecologists by the late 1940s, and species richness, while defined first in 1910 by C. Raunkaier, came into broader use in English around the same time. Robert P. McIntosh, *The Background of Ecology: Concept and Theory* (Cambridge: Cambridge University Press, 1985). (McIntosh himself played a role in clarifying the relationships among these terms in 1967: Robert P. McIntosh, "An Index of Diversity and the Relation of Certain Concepts to Diversity," *Ecology* 48, no. 3 (1967): 392-404.)

27. On tropical science, especially medicine, racial science, forestry, and agriculture, in Hawaii and the Philippines, see Greg Bankoff, "First Impressions: Diarists, Scientists, Imperialists and the Management of the Environment in the American Pacific, 1899–1902," *The Journal of Pacific History* 44, no. 3 (2009): 261-80; Greg Bankoff, "Breaking New Ground? Gifford Pinchot and the Birth of 'Empire Forestry' in the Philippines, 1900-1905."; Warwick Anderson, *Colonial Pathologies: American Tropical Medicine, Race, and Hygiene in the Philippines*; Warwick Anderson, "Going Through the Motions: American Public Health and Colonial 'Mimicry'," *American literary history* 14, no. 4 (2002): 686-719; Paul A. Kramer, *The Blood of Government*; Richard A. Overfield, "The Agricultural Experiment Station and Americanization: The Hawaiian Experience, 1900-1910," *Agricultural History* 60, no. 2 (1986): 256-66; Christine Leah Manganaro, "Assimilating Hawai'i: Racial Research in a Colonial 'Laboratory,' 1919-1939" (Dissertation, University of Minnesota, 2012).

and South American land bordering the Caribbean sea—the region also known as the Greater Caribbean or Caribbean Basin.[28]

The Caribbean offers analytical as well as geographical advantages. Geographically, US tropical stations tended to actually be within the Greater Caribbean rather than the deep continental areas of Latin America, for reasons of accessibility by shipping routes and the overall pattern of US presence in the region. Thus, although the Amazon rainforest has long been prominent in the American imaginary of the tropics, as well as a destination for exploration, a lack of long-term research sites in the early twentieth century leads to its absence from this study.[29] Analytically, Caribbean historians have emphasized how the category of the Caribbean demands attention to movements across national boundaries and to broad shifts in colonial history. As a region, the Caribbean has been described as "culturally and ethnically perhaps the most heterogeneous for its size in the whole world," yet one united by a shared history of colonialism and plantation slavery.[30] Caribbean historians define the region not by sharp boundaries, but by material and cultural circulations. Thus, although the Panama Canal Zone is not always thought of as Caribbean, its role as a focal point of US strategic interest, transportation networks, and West Indian labor migration roots it firmly within the region in the

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28. For example, David Barry Gaspar, and David Patrick Geggus, *A Turbulent Time: The French Revolution and the Greater Caribbean* (Bloomington: Indiana University Press, 1997); Graeme Mount, *The Caribbean Basin: An International History* (Routlage, 1998); Sidney Wilfred Mintz, and Sally Price, *Caribbean Contours* (Baltimore, MD: Johns Hopkins University Press, 1985); Franklin W. Knight, and Colin A. Palmer, *The Modern Caribbean* (Chapel Hill, NC: University of North Carolina Press, 1989); Richard S. Hillman, and Thomas J. D'Agostino, *Understanding the Contemporary Caribbean* (Kingston, Jamaica: L. Rienner, 2003).

29. Candice Millard, *River of Doubt*; Wade Davis, *One River: Explorations and Discoveries in the Amazon Rain Forest*.

30. Sidney Wilfred Mintz, and Sally Price, *Caribbean Contours*, 5. See also Ruben S. Gowricharn, *Caribbean Transnationalism: Migration, Pluralization, and Social Cohesion* (Lanham, MD: Lexington Books, 2006).

twentieth century. Although the actor's category of the "American tropics" is also available, using the Caribbean avoids the slippage in meaning between the US nation and the Western hemisphere inherent in the word "American." Beyond the strict geographic denotation of the tropical part of the Western Hemisphere, biologists' use of the term the "American tropics" carried connotations of colonial possession.[31]

Moreover, biologists never simply went to "the tropics," but rather worked in distinct Caribbean localities, which they framed as "tropical" and through which they reshaped their ideas about tropical environments and organisms. These sites—their environments, societies, and institutions—shaped the work that biologists were able to do in particular ways. The particularities of experiences at BCI and Soledad significantly shaped the history of ideas about nature in the tropics. Local conditions shaped what science was done and how it was done. In fact, the view of "tropical nature" each site presented was quite different: Soledad with its agricultural landscape, BCI with its apparently pristine rainforest. At these stations, American researchers encountered ecosystems very different from those with which they were familiar. For example, whereas temperate forests in the US might be dominated by a single species of tree, just under six square miles of forest on BCI contained more than three hundred tree species. American biologists drew on an older discourse of tropicality to understand this experience of difference, but they also added new understandings of the ecological causes of geographical variation in the diversity of life. BCI became a living laboratory for such study. Transformed

31. Caribbeanists and Latin Americanists, as well as US diplomatic historians have long discussed this problem. Historian of science Philip Pauly has also specifically discussed its expansionist use in nineteenth-century natural history. Samuel Flagg Bemis, "'America' and Americans," *The Yale Review* 57, no. 3 (1968); Mary A. Renda, *Taking Haiti: Military Occupation and the Culture of U.S. Imperialism, 1915-1940* (Chapel Hill: University of North Carolina Press, 2001), xvii; Philip J. Pauly, *Biologists and the Promise of American Life : From Meriwether Lewis to Alfred Kinsey* (Princeton University Press, 2000), 7.

from an exotic “other” into a biological norm, today BCI is a standard model for understanding ecological and environmental controls on diversity. Furthermore, as biologists re-imagined the tropics as a coherent field of research, they began to consider how to conserve its diversity while simultaneously developing its resources. Thus, this dissertation adds to both the history of biology and the rapidly expanding field of Latin American environmental history by examining the formation of the ideas about tropical nature that would shape policies of both natural resource exploitation and conservation in the 20th century.

Overview of chapters

The five chapters of this dissertation are roughly chronological, beginning at the turn of the twentieth century with the earliest movement to establish American stations for basic tropical research in the and ending with the emergence of tropical biology as a coherent field of research, an interdiscipline with its own professional organizations and institutions in the 1960s. American biologists expressed intellectual concerns about the puzzles of the ecology and physiological adaptation of organisms to tropical climates even before the acquisition of tropical territories in 1898. Their interests could not be pursued, however, without access to institutions in the tropics, for they required not only temporary expeditions, but also facilities for laboratory work and long-term observations and experiments. Chapter 1, “An American Tropical Laboratory,” examines how American botanists emulated European models in their initial movement to establish a tropical research station. In the 1890s, the rise of the "new botany" in the US, with its emphasis on physiological and ecological approaches, gave impetus to the study of tropical plants in their natural environments. Botanists raised concerns about the temperate-zone bias of the majority of experimental botany and pointed to the progress being made by European scientists in tropical colonies. Like many Americans in the period around the Spanish American War, botanists' eyes turned to the south. American botanists emulated German scientific approaches and British and Dutch institutions to create a borrowed version of colonial science at Cinchona, Jamaica.

While researchers at Cinchona had the freedom to pursue an innovative mix of laboratory and field approaches, its focus on pure botany and lack of strong ties to the growing US corporate and government interests in the region left it vulnerable. The Cinchona station collapsed by the 1920s, and ongoing field research by Americans in Jamaica would center on seasonal expeditions to the expanding holdings of the United Fruit Company rather than at any long-term station. As Chapter 2, "The Harvard Station: The Intersection of Academic and Business Interests at Soledad, Cuba," will show, tropical research institutions were more stable when American biologists made alliances with rapidly expanding US business and government interests in the region. In the case of the station at Soledad, Cuba, Edwin Atkins of the E. Atkins Sugar and Company reached out to Harvard, seeking expert knowledge for the improvement of sugar cane as early as 1899. Harvard scientists responded to Atkins' interests in economic botany, but over the years they expanded their mission. The university and company cooperated to improve sugar cane crops and amass a world-class tropical botanical garden. By the 1920s, Thomas Barbour, director of Harvard's Museum of Comparative Zoology, became involved with the station. He worked to broaden it into a destination for researchers from across the breadth of biology. Under Barbour's increasing influence in the 1920s, it became an endowed tropical biological station, with a laboratory, dormitory, and scholarships for students. Collaboration at Soledad served both corporate and academic interests.

Perhaps no single person had a more lasting influence on American biological institutions in the tropics than Thomas Barbour during the first half of the twentieth century. Even as Barbour was working to expand Soledad, he helped to secure BCI, an island in the middle of the Panama Canal, as a new tropical station and nature reserve in 1923. In Chapter 3, "Organizing Biology in Tropical America: Imperial Ambitions and the Assembly of a Scientific Network" we will examine the history of the Institute for Research in Tropical America (IRTA). IRTA's correspondence and meetings provide insight into American biologists diverse views about the future of biological research in the tropics and in the years immediately following World War I. They also lead, circuitously, to the foundation of BCI. As a member of IRTA Barbour helped to push the organization out of a stage of endless debate and into action through the formation of alliances in the Panama Canal Zone. Barbour was able to establish and keep BCI afloat as a station for basic tropical field research through his connections in business and government, as well as the USDA entomologist James Zetek. Both BCI and Soledad grew rapidly in the 1920s, owing largely to Barbour's connections with the US and Canal Zone Governments, universities, and corporations like the United Fruit Company. Barbour persuaded diverse interests to support basic science at BCI. Unlike Soledad and other stations in the region, BCI fostered little economic botany—work focused on ecology, ethnology, and zoology. As a result, BCI built a reputation as a scientific destination by the 1930s.

Chapter 4, "'Jungle Island': Taming Tropical Biology on Barro Colorado Island," zooms in to examine life at the research station on BCI during its first two decades of operation. From its founding, BCI has been touted as a "natural laboratory" for the study of a pristine tropical forest—in spite of its areas of second-growth and "unnatural" origins as an island created by the flooding of the Panama Canal. Yet, while BCI was set aside to preserve of a sample of tropical nature for scientific study, the project of making BCI a place for science also significantly altered the natural and social space of the island. To access and observe tropical nature, biologists themselves became part of the islands ecosystem—developing a network of trails, building facilities to keep scientists fed and sheltered, incorporating environmental monitoring equipment into the landscape. The islands' shifting populations of wildlife, scientists, tourists, and Panamanian laborers were intensely managed to maintain the station for scientific research. In effect, BCI was not merely a backdrop for biological research but an ever-evolving system that integrated people, technologies, and organisms on the island to produce scientific knowledge. In many ways, BCI's scientific infrastructure transformed it into a laboratory-like space, with access controlled and ever more natural variables accounted for. Yet, while most laboratories are constructed to simplify nature, BCI's unique scientific system allowed ecologists to develop new practices for studying the tropical forest's complexity.

Not only was BCI instrumental in the institutionalization of tropical biology, but research there has strongly shaped ecological understandings of the origins of the high species diversity of tropical forests—and thus has been a significant contribution to the emergence of the idea of biodiversity. Chapter 5, "A Full Scale Program in Tropical Ecology," explores how research at BCI fit into broader developments in biological ideas about tropical species diversity by mid-century. In the years before World War II, BCI was the primary research site for American biologists interested in the behavior and ecology of tropical animals. Over time, the empirical basis of knowledge about the island's environment and organisms grew, producing a picture of the complex interactions and changing composition of species on BCI and making it a key site for testing emerging ideas about the evolutionary and ecological controls on tropical species diversity by the 1950s and 1960s.

Throughout the early 20th century, US biologists had aligned with US corporate and government interests in the development of Caribbean natural resources. Their contributions to tropical agriculture and medicine were integral to US hegemony in the region. Nevertheless, they had begun to create spaces for basic research, particularly at field stations where biologists' professional and intellectual concerns were not wholly dominated by state and business interests. Yet, this nascent community of "tropical biologists" remained bound to US power structures—a fact made visible when these were shaken by the 1959 Cuban Revolution and the global post-colonial moment of the 1960s. As Harvard's station at Soledad was lost to American biologists, demands for the return of the Canal Zone to the Republic of Panama also grew louder. BCI would face a new turning point perhaps more serious than the one it had confronted at the end of World War II. With the vulnerability of American tropical biological institutions exposed, American biologists would have to form new alliances if they hoped to survive these political transformations and solidify the emerging interdiscipline of tropical biology.

Chapter 1

An American Tropical Laboratory

Fielding the Laboratory

In November of 1896, an editorial appeared in the Botanical Gazette calling for the foundation of a tropical research laboratory for American botanists. It began with a radical statement. In drawing attention to the geographically circumscribed nature of existing botanical research, it offered an outright challenge to the objectivity of knowledge thus far produced in the laboratories of the United States and Europe:

> The time has come for the establishment of a laboratory in the American tropics. The science of botany in general, so far as morphology, physiology, and ecology are concerned, rests largely upon the results of researches carried on in the north temperate zone, in gardens and laboratories situated between the parallels of 40º and 55º. While taxonomy has had world-wide material for its superficial diagnoses, and has reached a fair measure of knowledge concerning the general relationships of plants, knowledge of these other great divisions of the science has been derived from a study of the plants indigenous to a strip of territory fifteen degrees in width and extending across one continent and partly across another, and of those introduced from other regions and growing under abnormal conditions of climate or substratum in the open air and in conservatories. That many of the conclusions reached under such circumstances are not capable of general application is becoming more and more apparent.[32]

Here, in the foremost journal of botany in the United States, appeared an article questioning the general applicability of laboratory knowledge. Today, we tend to imagine laboratories as "placeless places," that is, sites where nature's variables are controlled and universal knowledge can be produced.[33] As the historian of science Robert Kohler has written, "Laboratories are

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32. "Editorials: An American Tropical Laboratory," *Botanical Gazette* 22, no. 5 (1896): 415-16. A note on technical language in this quote: by "superficial diagnoses" the author is not implying that taxonomists' work is shallow, but is referring to a provisional description (diagnosis) of a species based on an external (superficial) examination—say of a dried specimen only.

33. Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago, IL:

meant to seem universal... the reason why we trust experiments... is that they turn out the same wherever they are performed."[34] According to this editorial, however, existing laboratories were not universal, but in fact merely *temperate zone* laboratories. They were capable of making regional truth-claims, but not global generalizations. American and European laboratories had failed to base their research on a truly global and representative set of species, and they had been unable to overcome the complications inherent in growing tropical organisms under unnatural, northern conditions. Taxonomic botany might be able to rely on the global specimen collections housed in metropolitan centers, remote from the sites where plants actually lived and grew. Morphology, physiology, and ecology could not. A truly general botany, encompassing all branches of the discipline, needed stations throughout the globe for long-term observation and experimentation on living plants. Only a laboratory within the tropics could hope to comprehend tropical nature.

This critique of laboratory science came from what may initially appear to be a surprising source. Though published as an unattributed editorial in John Merle Coulter's *Botanical Gazette*, the text was in fact submitted by the up-and-coming plant physiologist Daniel Trembly

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University of Chicago Press, 2002), 9; Robert E. Kohler, "Labscapes: Naturalizing the Field," *History of Science* 60): 473-501. Historians and sociologists of science have thoroughly explored the great effort that has gone into constructing laboratories as "placeless." For perspectives on the production of knowledge within laboratory spaces, see "Focus: Laboratory History" in Isis 99, no. 4 (2008): 761-802; David N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago, IL: University of Chicago Press, 2003); Crosbie. Smith, Jon. Agar, and Gerald. Schmidt, eds. *Making Space for Science: Territorial Themes in the Shaping of Knowledge*, Science, Technology, and Medicine in Modern History (New York, NY: St. Martin's Press in association with Centre for the History of Science, Technology, and Medicine, University of Manchester, 1998); Peter Galison, and Emily Thompson, eds. *The Architecture of Science* (Cambridge, Mass.: MIT Press, 1999); Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago, IL: University of Chicago Press, 1994); Bruno Latour, "Give Me a Laboratory and I Will Raise the World," in *Science Observed*, ed. K. Knorr-Cetina, and M. Mulkay (Beverly Hills: Sage Publications, 1983); Bruno Latour, and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, Sage Library of Social Research (Beverly Hills: Sage Publications, 1979).

34. Robert E. Kohler, *Landscapes and Labscapes*, 7.

MacDougal.[35] MacDougal is best known as a crusader for experimental methods in biology, attacking the ability of natural historical methods to answer evolutionary questions. The episode set off by this editorial, however, shows that he likewise had sharp criticism for his fellow experimentalists. While Sharon Kingsland and David Magnus have focused on MacDougal's promotion of the mutation theory and his divisive assault on the practices of naturalists, MacDougal was equally responsible for pointing out American experimental botanists' parochial biases.[36] MacDougal was as much at home in the field as in the lab. At times he chided those naturalists who were insufficiently experimental, but in this case he was equally ready to criticize laboratory researchers who had no experience with plants growing in their natural habitats. Moreover, rather than driving botanists apart, he largely unified botanists in his call for a tropical laboratory, initiating a groundswell of support for tropical research across a wide swath of the American botanical community. MacDougal's push for tropical botany appealed to botanists interested in a range of research areas, from physiology and ecology to basic taxonomic work. His contention that research in the tropics held the promise of unifying biology was compelling

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35. Coulter privately expressed his hope that MacDougal had understood his submission was to be published in this fashion: Coulter to Macdougal, 12 October 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG. MacDougal also used nearly identical language to advocate a tropical laboratory in another article published a month later: Daniel T. MacDougal, "Botanic Gardens I," *Popular Science Monthy* 50, no. December (1896), 185-86.

36. Sharon E. Kingsland, "The Battling Botanist: Daniel Trembly MacDougal, Mutation Theory, and the Rise of Experimental Evolutionary Biology in America, 1900-1912," *Isis* 82, no. 3 (1991): 479-509; David Magnus, "Down the Primrose Path: Competing Epistemologies in Early Twentieth-Century Biology," in *Biology and Epistemology*, ed. Richard Creath, and Jane Maienschein (Cambridge: Cambridge University Press, 1999). Historiographically, MacDougal's endorsement of experimentalism has often lead him to be placed in opposition to "the naturalists" and fieldwork. This both simplifies his own broader views on biological methods and probably overemphasizes the divisions between these groups, particularly within botany. See Robert E. Kohler, *Landscapes and Labscapes*, 88-89; Sharon E. Kingsland, *The Evolution of American Ecology*, 70-77, 81-83; Juan Ilerbaig, "'The View-Point of a Naturalist': American Field Zoologists and the Evolutionary Synthesis, 1900-1945," in *Descended From Darwin: Insights Into the History of Evolutionary Studies, 1900-1970*, ed. Joe Cain, Michael Ruse, and Frederick Burkhardt (Philadelphia: American Philosophical Society, 2009), 29.

to many. Moreover, he played on national pride. MacDougal made a strong case that the American botanical community needed to catch up with European countries in the field of tropical research, where access to colonial possessions had given them a distinct advantage. Indeed, the natural proximity of the Caribbean might even allow America to surpass Europe as a leader in international science.

MacDougal's campaign for an American tropical laboratory would ultimately foster the establishment of a small station at Cinchona, Jamaica, used on and off by US researchers between 1903 and 1921. As US national interests shifted toward the Spanish Caribbean and US-British relations in the region adjusted, however, Cinchona would be largely overshadowed. As we will see in Chapter 2, by the time Cinchona officially opened as an American station, several other tropical and subtropical stations were in the works in Cuba, Puerto Rico, Bermuda, Bahamas, and the Dry Tortugas. In 1906 MacDougal himself would terminate involvement when he became director of the Desert Botanical Laboratory at Tuscon, Arizona.[37] Nevertheless, the movement that resulted in Cinchona was an important first attempt by Americans to establish a permanent, year-round biological laboratory in the tropics, and one with predominantly "pure" research goals. It marks the emergence of US involvement in long-term, place-based research in the tropics and the beginnings of a community of biologists with a shared interest in tropical problems.

In a way, Cinchona was a laboratory in a double sense. Historians of empire, sometimes frame colonies as "laboratories," places where new policies and social relations could be

37. He held this position until his retirement in 1928.

experimented with in ways that were not permissible within the metropole.[38] Cinchona was likewise a "laboratory" for the social and methodological relations of American science in the tropics. To begin with, the initial movement for tropical research within the American botanical community reveals a much more fluid relationship between lab and field, experimentalism and natural history, than is sometimes assumed to be the case during this period.[39] At the same time, it was a "laboratory" for American colonial science, where Americans could experiment with existing European research models and infrastructure in the tropics. American botanists

38. Colonies as "laboratories of modernity" emerged first in the work of Ann Laura Stoler, Gwendolyn Wright, and Paul Rabinow: Paul Rabinow, *French Modern: Norms and Forms of the Social Environment* (Cambridge, MA: MIT Press, 1989); Gwendolyn Wright, *The Politics of Design in French Colonial Urbanism* (Chicago: University of Chicago Press, 1991); Ann Laura Stoler, *Race and the Education of Desire: Foucault's History of Sexuality and the Colonial Order of Things* (Durham, NC: Duke University Press, 1995). The concept of colonial "laboratories" has since become a widespread theoretical tool within colonial and postcolonial studies. It has been useful for breaking down artificial boundaries between national and colonial histories, but has also been critiqued for the overly "rational" picture of colonial government that the laboratory metaphor may imply. Examples of its broad application include Frederick Cooper, and Ann Laura Stoler, *Tensions of Empire: Colonial Cultures in a Bourgeois World* (Berkeley, CA: University of California Press, 1997); Jorge Cañizares Esguerra, "New World, New Stars: Patriotic Astrology and the Invention of Indian and Creole Bodies in Colonial Spanish America, 1600-1650," *The American Historical Review* 104, no. 1 (1999): 33-68; Ann Laura Stoler, "Tense and Tender Ties: The Politics of Comparison in North American History and (Post) Colonial Studies," *The Journal of American History* 88, no. 3 (2001): 829-65; Laura Briggs, *Reproducing Empire: Race, Sex, Science, and US Imperialism in Puerto Rico*, American Crossroads (Berkeley, CA: University of California Press, 2002); Nancy Christie, *Transatlantic Subjects: Ideas, Institutions, and Social Experience in Post-Revolutionary British North America* (Montreal: McGill-Queen's University Press, 2008); Alfred W. McCoy, and Francisco A. Scarano, eds. *Colonial Crucible: Empire in the Making of the Modern American State* (Madison, WI: University of Wisconsin Press, 2009), 5-6; Robert Dixon, *Prosthetic Gods: Travel, Representation, and Colonial Governance*, vol. UQP Australian studies (St. Lucia, Qld.: University of Queensland Press in association with the API Network, 2001); Sebastian Conrad, *German Colonialism: A Short History* (Cambridge: Cambridge University Press, 2011), 142-45; Christine Leah Manganaro, "Assimilating Hawai'i: Racial Research in a Colonial 'Laboratory,' 1919-1939."

39. Works that have complicated the relationship between natural history and biology (where the "old" natural history was supposed to be associated with the museum, fieldwork, and observational methods and the "new" biology with the laboratory and experimentalism) include Lynn Nyhart, "Natural History and the 'New' Biology," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996); Philip J. Pauly, *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey* (Princeton University Press, 2000); Keith R. Benson, "From Museum Research to Laboratory Research: The Transformation of Natural History Into Academic Biology," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia, PA: University of Pennsylvania Press, 1988). In reality, these epistemological approaches are neither mutually exclusive nor do they neatly map onto particular scientific spaces. For a look at how "natural history" approaches have often gone unnoticed within twentieth century laboratory biology see Bruno Strasser, "The Experimenter's Museum: Genbank, Natural History, and the Moral Economies of Biomedicine," *Isis* 102, no. 1 (2011): 60-96.

borrowed German scientific approaches and British and Dutch institutions to create a their own version of "colonial science" at Cinchona.

The sensitive plant

MacDougal was first drawn to the tropics by his work on *Mimosa pudica*, the "sensitive plant." His interest in the movements of *Mimosa* at first appears strictly physiological and laboratory-based. A closer look illustrates the inseparability of physiology and ecology at the turn of the century, and explains how questions formulated at a lab bench in Minnesota could lead to a rainforest in Jamaica. *Mimosa pudica* is well known for its remarkable reaction to touch. The leaflets will fold up and droop dramatically if pressed or shaken, lending it the evocative name "shameweed" in the West Indies. The leaves also exhibit striking periodic behavior, closing up tightly during the night. Native to Brazil, *Mimosa pudica* is a troublesome weed in pastures throughout the tropics.[40] It is but one of many species of sensitive plants which increase in diversity as one approaches the equator. Long been grown as a novelty, its curiously animal-like irritability has made it an object of especial scientific interest since the eighteenth century.[41] By the nineteenth century, physiologists were bringing *Mimosa* into the laboratory setting and subjecting it to a wide range of stimuli. Especially after 1880, when Charles and Francis Darwin published *The Power of Movement in Plants*, German researchers were eager to use the plant put their theories through rigorous tests.[42] The influential Julius Sachs and Wilhelm

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40. As David Fairchild noted of its invasive qualities, "Though highly interesting and valuable in a botanical laboratory, *Mimosa pudica* is quite useless on a tropical estate," David Fairchild, "The Sensitive Plant as a Weed in the Tropics," *Botanical Gazette* 34, no. 3 (1902), 230.

41. See Lynnette Regouby's dissertation-in-progress on eighteenth-century constructions of plant and human bodies.

42. Soraya De Chadarevian, "Laboratory Science Versus Country-House Experiments. The Controversy Between

Pfeffer used *Mimosa* in investigations of osmosis and plant irritability.[43] *Mimosa*'s motion was interesting now, not as a strange exception to the general immobility of the plant kingdom, but rather as an especially rapid and easy to study example of the general phenomenon of plant movement.

As a young Assistant Professor in Plant Physiology at the University of Minnesota, MacDougal was granted a leave of absence to study with Pfeffer at Leipzig from 1895 to 1896. He also spent time in Sachs' laboratory, and with other influential experimentalists in Germany, Holland, and England. When MacDougal returned to the US, he followed up on his mentor's work on *Mimosa*. He hoped to elucidate the mechanism by which *Mimosa* produced its distinctive movements and show how impulses were transmitted through the plant. Like the German students he had met during his travels, however, MacDougal's interests went beyond a narrow study of the life cycles and physiological processes of plants, toward a broader concern with the physiological adaptation of plants to their natural environments.[44] In fact, as we will see in the next section, MacDougal was particularly impressed by the fact that many German students made a pilgrimage to the laboratory and garden at Buitenzorg, Java, for the express purpose of gaining a familiarity with the life processes of plants in the tropics.[45] His time in

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Julius Sachs and Charles Darwin," *The British Journal for the History of Science* 29, no. 1 (1996): 17-41.

43. Erwin Bünning, *Ahead of His Time: Wilhelm Pfeffer, Early Advances in Plant Biology* (Ottawa: Carleton University Press, 1989), 27-36. For Sach's own views of the history of this research see his chapter on "The History of the Doctrine of the Movements of Plants" in Julius Sachs, *History of Botany (1530-1860),* trans. H.E.F. Garnsey (Oxford: Clarendon press, 1890), 535-63.

44. Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900* (New York, NY: Cambridge University Press, 1990), 24-25. See also the introductory chapter of Lynn K. Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago, IL: University of Chicago Press, 2009).

45. For example Eugene Cittadino, *Nature as the Laboratory*, 1-2,62,74,79,92,110,135.

Europe helped to make MacDougal a staunch supporter of the "new botany" in the US, as promoted by men like Charles Bessey, George Lincoln Goodale, John Merle Coulter, and his own advisor at Purdue, J.C. Arthur.[46] Fostered by increased exposure to German botany and the development of greater professional opportunities for botanists in the US Department of Agriculture and land-grant colleges, the "new botany" was a movement that advocated the study of living plants, rather than the dried herbarium specimens that had long dominated American botany. This meant bringing a new experimental attitude toward botany. Botany would now have a much broader scope, not merely to classify species, but to interrogate their functions, internal structures, and adaptations to the environment. The experimental turn of the new botany did not mean, however, a reductionist laboratory orientation, but rather an emphasis on the living plant as a whole—in the laboratory and in the wild.

MacDougal expressed frustration at the lack of attention to *Mimosa*'s natural habitat in the existing literature, which he felt was symptomatic of a blind spot in the study of tropical organisms in general. Researchers boldly conjectured that the plant's behavior was a defensive adaptation against hailstorms or grazing, without regard to whether such conditions were prevalent where *Mimosa* had evolved.[47] Research into the plant's irritability had up to that point been little more than the description of anatomical details and chemical properties, producing "'working theories'...and metaphysical explanations," but "futile in real results."[48] The subject of

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46. On the "new botany," see Richard A. Overfield, *Science With Practice: Charles e. Bessey and the Maturing of American Botany* (Ames, IA: Iowa State University Press, 1993), 72-99; Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890-1905," *Studies in the History of Biology* 4 (1980), 175-77. On

47. Daniel T. MacDougal, "Mimosa: A Typical Sensitive Plant," in *Living Plants and Their Properties: A Collection of Essays*, ed. Joseph Charles Arthur, and Daniel T. MacDougal (New York: Baker and Tayler, 1898), 61.

48. Daniel T. MacDougal, "The Mechanism of Movement and Transmission of Impulses in Mimosa and Other

the transmission of impulses in *Mimosa* had received "no attention except that given it in the north temperate zone, in warm houses, and for the greater part under highly artificial conditions. In such manner it has been the object of numerous series of experiments and of many highly ingenious speculations."[49] Measurements taken under artificial conditions were misleading, and conflicted with those made in the tropics, MacDougal argued. In tropical climates, *Mimosa* grew prostrate, acting as a low-growing woody ground-cover. Under the insufficient light conditions of northern greenhouses, however, the plant grew upright and spindly, appearing "'drawn' in the language of the gardener."[50] According to MacDougal, *Mimosa*'s sensitivity to environmental differences (not just to touch) caused changes in form and physiology that complicated attempts to infer its adaptations. The plants' physiology and behavior could only be understood in relation to the environmental conditions under which it had evolved. The conditions could not be sufficiently reproduced in a northern laboratory—there was neither the technical capability nor the basic knowledge of the plants' original habitat. "The entire problem," he argued, "together with that of the developmental history of such highly specialized forms of 'sensitiveness' as those exhibited by *Mimosa*, must be followed to their solution in the tropical habitats of the plants."[51] *Mimosa* could not come to the laboratory. The laboratory would have to go to *Mimosa*.

MacDougal was quick, however, to point out that the need for experimental work in the tropics went deeper than the study of a single species of plant; it was fundamental to the whole

"Sensitive" Plants: A Review With Some Additional Experiments," *Botanical Gazette* 22, no. 4 (1896), 293.

49. Ibid.

50. Daniel T. MacDougal, "Mimosa: A Typical Sensitive Plant," 49.

51. Daniel T. MacDougal, "The Mechanism of Movement and Transmission of Impulses in Mimosa and Other "Sensitive" Plants: A Review With Some Additional Experiments."

success of the "new botany." MacDougal's argument echoed the botanists he had encountered while studying abroad. As historian Eugene Cittadino has explained, German botanists considered research in the tropics particularly important for three reasons.[52] First, physiological botanists were interested in plants living under conditions of extreme moisture and temperature—like deserts, the arctic, or the tropics—where environmental adaptations were supposed to be more evident and easily understood. Influential ecologists like Andreas Schimper, Eugen Warming, and Henry Cowles all advocated the establishment of stations in extreme environments, and included the tropics. Second, and more significantly, although the tropics might be considered an extreme climate with regard to humidity, tropical conditions were in general more favorable to plant growth. The widely accepted view was that in a uniformly warm and moist climate, as Gottlieb Habberlandt expressed it, "the external conditions for growth and nourishment are favorable throughout the entire year, [and] the plant world can develop and grow with a freedom which our native flora is denied."[53] Tropical plants, unconstrained by northern winters, were thus able to grow in a more "typical fashion." Finally, a third and closely related reason that the tropics were of particular interest was their role as the evolutionary home of the plant kingdom. The belt of the tropics was seen as a kind of Eden for the plant world. Plant taxa were conspicuously more diverse near the equator, suggesting that this was their point of origin. Some of the most primitive and ancient forms, like the cryptogams, continued to flourish there. At the same time, the highest plant forms, flowering plants, were thought to attain their greatest development. According to MacDougal, in the tropics, there was "such rapidity of

52. Eugene Cittadino, *Nature as the Laboratory*, 138-43.

53. Gottlieb quoted in translation, Ibid., 140.

growth and metabolism that the adaptive possibilities of the organism reach their highest expression."[54]

For these reasons, tropical plants could be considered more representative of plants in general. If anything, it was the physiology of temperate-zone plants, living in harsh climates far from their evolutionary birthplace, that was aberrant. That the science of living plants—physiology and ecology—had a strong temperate-zone bias, then, was especially problematic; "Before a permanent foundation for the science can be laid," MacDougal argued, "research along all lines must be extended to include the most highly developed forms, in the primitive habitat of the plant kingdom, in the tropics."[55] In this realm, the "new botany" in fact struggled to catch up with traditional taxonomic botany. The systematic relationships of all the world's plants were much better known than their physiological processes or environmental interactions; "In the herbaria it has been possible to study normal specimens of prepared plants from the equator to the poles," MacDougal noted. [56] But living plants' sensitivity to the environment, their embeddedness in a whole set of ecological relationships, meant that the "new botanist" must go to the plant in its natural habitat. Thus, MacDougal made his case to the botanical community:

> "In order to come in contact with the problems that are now pressing, the botanist must establish his laboratory in the midst of the normal conditions, and no condition is so vitally needed by botanical research, and so overwhelmingly lacking, as that furnished by the tropics."[57]

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54. Daniel T. MacDougal, "Botanic Gardens I," 185-86.

55. Ibid.

56. Ibid.

57. "Editorials: An American Tropical Laboratory," 415.

The way to stimulate tropical botany, MacDougal believed, was a the creation of a permanent research station in the tropics, "It is impossible to overestimate the value of such an institution, not only to American botany, but to the science in general."[58]

"A mecca for the botanists of the world"

Convinced that both the success of his own *Mimosa* research and the cutting edge of botany in general lay in the tropics, MacDougal contacted John Merle Coulter at the University of Chicago, whose *Botanical Gazette* was the leading journal promoting the physiological perspective. MacDougal proposed the organization of a new laboratory in the tropics in the Americas, where American botanists could gain experience with tropical plants. Preparations should begin with a tour of possible locations in the West Indies. Coulter responded favorably. He offered the Gazette as a forum for the issue and asked MacDougal to lead a commission to evaluate potential laboratory sites. He was confident that the movement would find broad support. Though the proposed laboratory would have no specific institutional affiliation, its selection by a respected committee of American botanists would lend it legitimacy and support. "I believe we can push the thing through," he told MacDougal.[59]

Interestingly, MacDougal made his case by arguing that a laboratory in the Caribbean could be for American botanists what the Dutch station at Buitenzorg, Java, was for the Europeans. While studying under Wilhelm Pfeffer, MacDougal encountered many researchers who had worked there, and their experiences apparently made a strong impression on him.

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58. Ibid., 416.

59. Coulter to MacDougal, Oct 12 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

Buitenzorg, or the Government Botanical Garden (*'s Lands-Plantentuin*), was originally established in 1817, on the estate of the governor's palace.[60] It was long used as an introduction garden for economic plants from other parts of the tropics, as well as for its aesthetic charm. Under the direction of Melchior Treub, beginning in 1880, basic physiological research began to flourish alongside the agricultural research that was of more direct interest to the Dutch colonial government.[61] Treub built a visitor's laboratory on the grounds of the botanical garden, which also housed a herbarium, library, and museum. Within a relatively short distance were substations in the mountains, at Tjibodas, and on the sea shore. Thus, Buitenzorg was a research station with access to living tropical plants in a variety of natural conditions, in addition to the controlled spaces of the lab and garden.

As as tropical botanical station, the first of its kind and one with an explicitly physiological orientation, Buitenzorg proved to be much more direct model for American botanists than, for example, the Marine Biological Laboratory (MBL) at Woods Hole.[62] Several

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60. Today it is known as the Bogor Botanical Gardens. For its significance in the history of, respectively, Indonesian, German, and Dutch science, see Andrew Goss, *The Floracrats: State-Sponsored Science and the Failure of the Enlightenment in Indonesia* (Madison, WI: University of Wisconsin Press, 2011); Eugene Cittadino, *Nature as the Laboratory*; Robert-Jan Wille, "Melchior Treub, the Botanical Station at Buitenzorg and the Role of the Station's Visitors in the Dutch Culture of Science and Science Polity, 1870-1930" (Dissertation-in-progress, Radboud University Nijmegen).

61. Treub eventually ran into trouble with the colonial officials for the attention he gave to less "practical" lines of research, Suzanne Moon, *Technology and Ethical Idealism: A History of Development in the Netherlands East Indies* (Leiden: CNWS Publications, 2007), 29, 32-34.

62. MBL played an important role in the disciplinary formation of biology in the US. It likewise has an outsized place in the historiography of American biology: Jane Maienschein, "Agassiz, Hyatt, Whitman, and the Birth of the Marine Biological Laboratory," *Biological Bulletin* 168 (1985): 26-34; Philip J. Pauly, "Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882-1925," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia, PA: University of Pennsylvania Press, 1988); Jane Maienschein, *100 Years Exploring Life, 1888-1988: The Marine Biological Laboratory at Woods Hole* (Boston: Jones and Bartlett Publishers, 1989); Dean C. Allard, "The Fish Commision Laboratory and Its Influence on the Founding of the Marine Biological Laboratory," *Journal of the History of Biology* 23 (1990): 251-70; Keith R. Benson, "Summer Camp, Seaside Station, and Marine Laboratory: Marine Biology and Its Institutional Identity," *Historical Studies in the Physical and Biological Sciences* 32 (2001): 11-18.

tropical station proponents, including MacDougal, were in fact regular MBL attendees, but the MBL was known mainly for zoological work.[63] For researchers interested in terrestrial plants, Buitenzorg was a more immediately relevant prototype. After all, American botanists were in the midst of the campaign for the "new botany" and were looking to German models. Treub's laboratory had "become a sort of Mecca to the botanists of the world," attracting German, Dutch, and other European researchers—among them influential physiologists and ecologists like Ernst Haeckel, Andreas Schimper, and Karl von Goebel.[64] Visitors to Buitenzorg worked on physiological problems in the laboratory, observed nearby forest and shoreline plant communities, and collected material for later study and teaching.[65] Through publications and contact with German botanists, the station had gained a strong reputation among American botanists by the mid-1890s.[66]

Indeed, by seeking to emulate Buitenzorg, MacDougal and other American botanists participated in a form of "modular" scientific colonialism.[67] Historians Frederick Cooper and

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63. For example, Jane Maienschein, *100 Years Exploring Life, 1888-1988: The Marine Biological Laboratory at Woods Hole*, 129-30. In addition, the innovative aspect of MBL's institutional structure—the integration of teaching and research—did not arise in discussions of an American tropical station. Buitenzorg had a strong reputation for research, and this emphasis was something proponents of the tropical laboratory wanted to emulate, despite the strong emphasis on education at existing stations in the US: Keith R. Benson, "Why American Marine Stations?: The Teaching Argument," *American Zoologist* 28, no. 1 (1988): 7-14. Teaching would not become a priority at American tropical stations until the mid-twentieth century, as we will see in Chapters 4 and 5.

64. "Editorials: An American Tropical Laboratory," 416. Although they were overwhelmingly botanists, the broad purview of the station's physiological and ecological focus meant that some zoologists—particularly entomologists interested in plant-animal interactions and agricultural pests—also worked there.

65. K. W. Dammerman, "A History of the Visitors' Laboratory (Treub Laboratorium) of the Botanic Gardens, Buitenzorg, 1884-1934," in *Science and Scientists in the Netherlands Indies*, ed. Pieter Honig, and Frans Verdoorn.

66. For example, Beverly Thomas Galloway, "The Buitenzorg Gardens," *Botanical Gazette* 22, no. 6 (1896): 496-97. See also David Fairchild, "Notes of Travel VI," *Botanical Gazette* 31, no. 6 (1901): 423-25.

67. On "modular colonialism," see Frederick Cooper, and Ann Laura Stoler, "Between Metropole and Colony: Rethinking a Research Agenda," in *Tensions of Empire: Colonial Cultures in a Bourgeois World*, ed. Frederick Cooper, and Ann Laura Stoler (Berkeley, CA: University of California Press, 1997), 13. The idea draws from Benedict Anderson's concept of "modular nationalism," Benedict Anderson, *Imagined Communities: Reflections on*

Ann Laura Stoler have invoked the modularity of colonial institutional structures and knowledge forms to highlight their transfer across imperial systems, suggesting complex trans-imperial relationships, rather than direct movement from metropole to colony. In this way, the campaign for an American tropical laboratory was not propelled simply by participants' own experiences at stations within US borders, but was deeply informed by existing colonial forms of European science in the tropics. Although the movement for an American tropical laboratory certainly fits within the broader history of experiment stations and environmental laboratories playing out within the US at the time, the conversation around the proposed American tropical laboratory drew heavily on Buitenzorg as an ideal.

"But," MacDougal noted, for American botanists, "a visit to Buitenzorg is equal to a trip around the globe."[68] The trip took at least a month in each direction and could run to $1,200 in expenses—far out of the reach of most university botanists. Indeed, one of only two Americans to work at Treub's laboratory before 1900 was David Fairchild, who (after a serendipitous meeting) was personally financed by the Chicago millionaire Barbour Lathrop.[69] Nevertheless, "the American botanist needs to make no such journey," MacDougal maintained, pointing to the possibilities of research in the American tropics:

> At the distance of a week's travel from almost every important laboratory there lies a tropical region whose teeming flora is but little known, even to taxonomists. At our very

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the Origin and Spread of Nationalism (London: Verso, 1983).

68. "Editorials: An American Tropical Laboratory," 415.

69. Future chapters will show that Fairchild became important in his own right in the development of the stations at Soledad and Barro Colorado Island. And as we will see in Chapter 2, the second American visitor was E. Mead Wilcox, visiting on a Harvard fellowship funded by the American sugar baron Edwin Atkins. For a colorful retelling of Fairchild's first encounter with Lathrop during his journey to the Naples Zoological Station (where he was also the first American to use the Smithsonian table), see Marjory Stoneman Douglas, *Adventures in a Green World: The Story of David Fairchild and Barbour Lathrop* (Coconut Grove, Fla.: Field Research Projects, 1973), 12-17.

> door there lies a vast *terra incognita*, with excessively luxuriant vegetation, and inviting endless research. The duty seems to be laid upon American botanists to establish and maintain an international laboratory in the American tropics.[70]

MacDougal exploited a longstanding language of tropicality; the tropics were rich and prolific, mysterious and inviting. They beckoned American researchers to harness their potential. Like other Americans, he also cast the Caribbean as a "neighbor," "at our very door," drawing on the broader discourse of geographical proximity that charged turn-of-the-century American expansionism.[71] Finally, he tapped into his readers' national pride and desire to become leaders within an international community of botanists; establishing their own tropical laboratory was no less than a responsibility to world science.

"At our very door"

MacDougal's editorial set off a flurry of discussion. The question of the location of the proposed American tropical research station was taken up within the broader botanical community. The response in journals and MacDougal's own correspondence shows interest among even the most eminent botanists, and strong opinions coming from those who already had experience in different parts of the tropical world. Researchers debated the virtues of competing locations and enumerated the qualities they desired in an ideal station.[72] Tracing this

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70. "Editorials: An American Tropical Laboratory," 416.

71. For a thorough exploration of this discourse in the case of Cuba, see Louis A. Pérez, *Cuba in the American Imagination*, 32-38. For broader discussion of historical shifts in the geographical discourse of American empire, see Neil Smith, *American Empire: Roosevelt's Geographer and the Prelude to Globalization*, California Studies in Critical Human Geography (Berkeley, CA: University of California Press, 2003).

72. Although the majority of the evidence for this section is taken from the initial response to MacDougal's commission, I have included a few particularly illustrative quotes from discussion that occurred later, during the events described in the rest of this chapter: the establishment of the Cinchona botanical station and several other tropical marine and agricultural stations up through about 1905. I have only used these in cases where similar views were expressed during the period of analysis.

conversation reveals the contours of the network of American botanical science in the tropics at the end of the nineteenth century, before the transformations of 1898. It also maps out the territory of the tropics in the imaginations of American scientists—both their hopes for the scientific possibilities of a tropical research station, and their broader impressions of the people and environments of "tropical America."

In many ways, the American discourse surrounding the establishment of a station in the Caribbean articulates with earlier nineteenth century European discourse of tropicality.[73] American researchers imagined that they would be inspired by the abundance and variety of tropical flora and fauna, and intrigued by encounters with exotic and picturesque peoples. Their minds were full of the writings of Alexander von Humboldt, Charles Darwin, Alfred Russel Wallace, and Thomas Belt, which celebrated the sublime in the landscapes of the tropics.[74] The tropics were a land of exuberant and primitive nature, promising both scientific discovery and aesthetic experience. A laboratory site should conform to these notions of the tropics; Fairchild made a point, for example, of weighing Jamaica's "beautiful scenery" against Trinidad's "far more luxurious vegetation" in assessing potential laboratory sites.[75]

The idea that the tropics was "at our very door," rather than a distant foreign land, however, tended to moderate the discourse of exoticism. Moreover, this argument of proximity

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73. On tropicality, see especially David Arnold, "'Illusory Riches': Representations of the Tropical World, 1840-1950."; Nancy Stepan, *Picturing Tropical Nature*; Felix Driver, "Imagining the Tropics."; Felix Driver, and Luciana Martins, *Tropical Visions in an Age of Empire*; Beth Fowkes Tobin, *Colonizing Nature*; Krista A. Thompson, *An Eye for the Tropics*; Mimi Sheller, *Consuming the Caribbean*.

74. On Humboldt's aesthetic and scientific program, see Michael Dettelbach, "Global Physics and Aesthetic Empire: Humboldt's Physical Portrait of the Tropics," in *Visions of Empire: Voyages, Botany, and Representations of Nature*, ed. David Philip Miller, and Peter Hanns Reill (Cambridge: Cambridge University Press, 1996).

75. David Fairchild, "The Tropical Laboratory," *Botanical Gazette* 27, no. 4 (1899): 320-22.

had both moral and practical implications. The nearness of the American tropics connoted an obligation for American botanists to use it to its full scientific potential. As Luis Perez has argued, in the language of nineteenth-century geopolitics proximity and "neighborliness implied duty" and "conferred rights."[76] The same metaphorical language was used to give grounds for US intervention into the affairs of Cuba and other Caribbean countries. At the same time, the need to attract less adventurous scientific travelers meant that it was important to prove that a sojourn to the tropics was not extravagant and unusual, but feasible and "within easy reach of the investigator or graduate student."[77] American botanists, constrained by university employment, needed a station that was within their budget and could be visited "during a summer vacation, or upon a short leave of absence;" merely "a study of the map" suggested the "eastern coast of Mexico or the islands near the Caribbean Sea."[78] Starting in the 1860s, steam transportation had begun to make the region more accessible to travelers, although geared toward commerce rather than tourism. By the 1890s, the fruit trade had expanded and opened even more shipping lines (though they were not yet what they would be by the time of the opening of the Panama Canal).[79] Caribbean ports were only "four or five days from any important city in the country."[80] The Caribbean coast of Central American struck several of MacDougal's correspondents as providing several possible locations due to frequent steamship departures. Or if the station were located

76. Louis A. Pérez, *Cuba in the American Imagination*.

77. Daniel T. MacDougal, "Botanic Gardens I," 186.

78. "Editorials: An American Tropical Laboratory," 416.

79. B. W. Higman, *A Concise History of the Caribbean* (New York, NY: Cambridge University Press, 2011), 226, 237-8.

80. Daniel T. MacDougal, "Botanic Gardens I," 186.

near Miami, one could get "to any part of the West Indies," and there was the possibility of maintaining substations in the Bahamas and on the North coast of Cuba.[81]

A laboratory site ought to have "advantages of a civilized community, such as good mail and telegraph facilities, good markets and satisfactory means of reaching other places."[82] The attraction of a field station was the ability to experience tropical wilderness without sacrificing all the comforts of "civilization." In fact, accessibility had to be weighed against the purely scientific benefits of a location. For example, one USDA scientist commented that an easily accessible lab was preferable "even if located in a field not so rich as one that would …[require] a considerable amount of traveling."[83] He balked at the suggestion of a marine station in the Dry Tortugas; this sounded like "an ideal place for solitary confinement."[84]

As far as the scientific characteristics of an ideal site, researchers sought diversity, both in facilities and environments. The advantage of a permanent research station over an expedition was in the ability to move back and forth between multiple scientific spaces. Botanists agreed that "the laboratory should be placed near a botanical garden," where they could observe a catalogued collection of living plants, including those from other tropical regions.[85] A good herbarium and a library to consult on taxonomic matters were also necessities. Grounds for

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81. Swingle to MacDougal, 19 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

82. Hubert Lyman Clark, "The Proposed Biological Station at the Tortugas," *Science* 17, no. 442 (1903), 979.

83. P. H. Rolfs, and J. Fred Clarke, "The Proposed Biological Laboratory at the Tortugas," *Science* 17, no. 443 (1903), 1009.

84. Ibid.

85. Karl Goebel, quoted in Daniel T. MacDougal, "The Tropical Laboratory Commission," *Botanical Gazette* 23, no. 3 (1897), 207-08.

experimental plantings would be an additional boon for plant breeding and acclimatization investigations. In the minds of botanists, there existed much less a dichotomy of lab versus field than a range of scientific spaces—field, experimental garden, botanical garden, herbarium, library, photographic darkroom, laboratory—with intersecting and non-competing purposes.

Despite MacDougal and Coulter's primary concern with promoting physiology, their contacts and the response they received make it clear that a variety of approaches would be welcome. Proximity between the laboratory and a relatively pristine field area was of importance for taxonomists as well as physiologists. Locating "the laboratory as near as possible to a primitive forest" was "of especial importance in researches upon cryptogams" which required identification under the microscope.[86] For cytological collectors, too, a "compound microscope is an absolute necessity;" a permanent station, wrote one cytologist "seems to be the only solution to the accessibility of such regions."[87] Following in the footsteps of Humboldt, many botanists sought to measure the relation between climate and vegetation.[88] Thus, a station on a tropical mountain, with its regular variations in temperature and humidity, was ideal.[89] One correspondent advocated a laboratory at Orizaba, Mexico, writing "You may think me a crank, but from the coast to the ice may be passed over in a day. There is no other place in the world

86. Goebel, quoted in Ibid.

87. C. H. Farr, quoted in Duncan S. Johnson, et al., "Cinchona as a Tropical Station for American Botanists," *Science* 43, no. 1122 (1916), 919.

88. Michael Dettelbach, "Humboldtian Science," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996).

89. A broader trend of establishing mountain field stations was also occurring within the US at this time, see Jeremy Vetter, "Rocky Mountain High Science."; Jeremy Vetter, "Labs in the Field? Rocky Mountain Biological Stations in the Early Twentieth Century," *Journal of the History of Biology* 45, no. 4 (2012): 1-25.

where you can go from the Tropics to the Polar Circle in a day."[90] The German physiologist Karl Goebel also advised, "If at all possible the main station should be in the highlands, with a subsidiary laboratory in the lowlands or on the seashore for the study of algae, and the vegetation of tropical plains."[91] Interestingly, while stations in the temperate zone might be demarcated as mountain or lake stations, here the tropics (varied though botanists knew they were) was the object of analysis. While departing from some aspects of the discourse of tropicality, botanists never wavered in imaging the tropics as a unified region, a single "tropical world" of natural exuberance, luxuriant vegetation and abundant and varied species.[92]

On the topic of health, however, botanists confronted tropicality's darker side: the image of the tropics as "white-man's-grave" or a "green hell." Tropical diseases were a serious threat, especially for researchers who intended to stay in the region for an extended period of time. By the nineteenth century, the diseases of the tropics were the focus of intense scrutiny by European empires intent on better control of colonial populations, and by Latin American governments seeking national progress.[93] Although the Cuban doctor Carlos Finlay had recently theorized that mosquitos were the vector for yellow fever, little could yet be done to effectively control tropical disease. As one USDA botanist warned MacDougal, "There is liable to be some loss of life no matter where the laboratory is located South of 27º."[94] Especial danger was perceived to lurk in

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90. Jared G. Smith to MacDougal, 7 January 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

91. Goebel, quoted in Daniel T. MacDougal, "The Tropical Laboratory Commission," 207-08.

92. On generalizations about the "tropical world" see David Arnold, "'Illusory Riches': Representations of the Tropical World, 1840-1950," 9.

93. David Arnold, ed. *Warm Climates and Western Medicine: The Emergence of Tropical Medicine, 1500-1900* (Amsterdam: Rodopi, 1996); Julyan G. Peard, *Race, Place, and Medicine*.

94. Jared G. Smith to Macdougal, 11 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T.

the ports of the West Indies, where steamships could pick up disease as they circulated from port to port. One of MacDougal's correspondents, for example, expressed "hope of finding some place in Mexico or Central America which can be reached by rail or by a cruise on the Pacific without danger of yellow fever. This yellow fever is no laughing matter to the new-comer, and if such a thing is possible the station should by all means be located in region entirely free from this malady."[95] Most agreed that the tropics were disease-ridden in general; the key to a successful station was to find as salubrious a site as possible. This might mean a station at high elevation (which had its scientific advantages as discussed above) like Orizaba or Cinchona, or in a country professed to have superior sanitation. The botanist James Humphrey, for example, writing in favor of Jamaica argued, "As compared with many other parts of the tropics, the climate of Jamaica is exceptionally healthful, and it is remarkably free from poisonous animals."[96]

Fear of disease was not the only anxiety that confronted American botanists who wished to work in the tropics long-term. The Caribbean was seen as a region of political unrest and alien, non-white societies. Cuba was in the midst of its War of Independence (1895-1898), a continuation of conflicts with Spain that had erupted intermittently since the middle of the century. Though several correspondents recommended the island's scientific advantages, all agreed "Cuba is at present out of the question."[97] Even assuming a brief conflict, the laboratory

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MacDougal Papers, NYBG.

95. Swingle to MacDougal, 19 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

96. James Ellis Humphrey, "The Tropical Laboratory," *Botanical Gazette* 23, no. 1 (1897), 50.

97. Swingle to MacDougal, 27 December 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

commission could not visit while the revolution was still in progress because of Cubans' prescient suspicions of US intervention; there was a real "danger of being mistaken for filibusterers which might cause a very sudden and tragic end of the expedition."[98] There were concerns about political and criminal disorder in all of countries of the Spanish Caribbean that were contemplated. Considerations often came down to racialized generalizations; one correspondent argued, "The Mexican is not as dangerous as the Cuban or Spanish soldier... The Mexican is not civilized nor is the Cuban—but I would much rather trust the Mexican than the Cuban. Your life and property are safer."[99]

To some, the British Caribbean appeared to be the safer alternative to supposedly backward and turbulent Latin countries. Speaking of Jamaica, James Humphrey wrote, "The island is a British colony, which means that life and property are secure, the roads fine, the language English." To Humphrey, the British colonies offered a haven of civilization in the American tropics:

> We are apt to think, when speaking of American botany and botanists, only of those of the United States and Canada, assuming that our southern neighbors, both continental and insular, have not yet reached that stage of civilization that encourages the cultivation of the sciences. And so far as those regions are concerned which have felt the influence chiefly of Latin civilization, this is measurably true. But some of the neighboring islands have been under Anglo-Saxon rule for two centuries or more, and have felt different influences.[100]

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98. Swingle to MacDougal, 19 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

99. J. T. Scovill To MacDougal, 25 November 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

100. James Ellis Humphrey, "Botany in Jamaica," *Science* 22, no. 550 (1893): 85.

Many agreed, "Jamaica is the most civilized has the most stable government and the best start in science of the West Indies."[101] In Jamaica or Trinidad, they would have the aid of colonial scientists and some of the oldest botanical institutions in the hemisphere, as well as the comfort of a more familiar social order. Jamaica had "a hospitable community" and "the government officials and the officials of the fruit company, which virtually controls communication with the United States, are simply unwearying in their efforts to put the visiting scientist under lasting obligations."[102]

A few, however, could only feel comfortable with a site "within our own borders."[103] In some ways, the reaction foreshadowed the broader American debate that would soon follow the Spanish-American war, with imperialists seeing the possibility of recreating American society in its overseas colonies and anti-imperialists fearing the encounter with majority non-white populations. For some botanists, the idea of working outside of the US triggered racial anxieties and xenophobia. The USDA agrostologist Jared G. Smith fretted about the increasing proportion of blacks to whites in Jamaica, cautioning MacDougal that "there is a danger not to be overlooked of a sudden uprising of this black horde." Any foreign country would present "strange customs" and "trouble with people who are more or less jealous of America and everything and everybody American." In particular, Smith warned of bureaucratic red tape:

> Every place that you visit on your tour of inspection will want to secure the Laboratory, and the authorities will promise anything and everything to secure it for their particular

101. Swingle to MacDougal, 27 December 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

102. Hubert Lyman Clark, "The Proposed Biological Station at the Tortugas," 980.

103. Smith to Macdougal, 11 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

> locality, but there is not a place outside of the States where you will not be amenable to restrictions and vexatious rules and regulations when once the institution is established.[104]

It is not a surprising that a USDA scientist like Smith would advocate South Florida, where his organization could have a larger hand in affairs.

Similarly, Walter T. Swingle, a USDA plant pathologist and a close friend and colleague of David Fairchild, hoped the planned tropical lab might cooperate with the citrus experiment station he had organized in Eustis, Florida. He jokingly noted that Florida had the drawback of an "absence of a picturesque foreign-looking population," but pointed out the financial advantages of a location where:

> No international complications could arise and where money could be much more easily obtained. You see for three months it is the mecca of the millionaires and they are in a receptive condition since they have largely left the cares of business behind them... Your idea that after all it is the tropics we want makes me think Florida fully the equal of Jamaica, and it is certainly much easier to reach.[105]

He suggested coming down in February and meeting Henry "Flagler ($20,000,000)."[106] The influential Charles Bessey also believed the station should not be "too far south," and preferred Florida.[107] However, neither Coulter nor MacDougal seemed convinced of either the fact that Florida was suitably tropical or the need to court support from the government or private philanthropy.

104.Smith to Macdougal, 11 February 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

105.Swingle to MacDougal, 27 December 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

106.Swingle to MacDougal, 27 December 1896. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

107.Charles Bessey to MacDougal, 30 January 1897. Box 1, Folder 1 "Tropical Laboratory Commission," Daniel T. MacDougal Papers, NYBG.

The Tropical Laboratory Commission

MacDougal became convinced of the superiority of Jamaica, though what particular factors persuaded him remain unclear (although, he was assured by many of the abundance of sensitive plants in Jamaica). Coulter agreed, but did not completely rule out South Florida. The planned tour of potential laboratory sites went forward. Neither had personal experience in the tropics; moreover, the venture needed to appear transparent in order to garner the support of botanists from a variety of institutions. This was essential if US botanical institutions and university departments were to stand behind the project, both financially and by sending researchers. In fact, Coulter had an opportunity to use his biology department's endowment to establish the laboratory for the University of Chicago. Coulter, however, remained steadfast in his position that the laboratory should be independent, a concern that would be echoed by future tropical station-builders. He felt strongly that the station should be a cooperative effort and not controlled by a single organization (he must also have been concerned about the financial and administrative burden); "In my notion it is essential that this station be free from the domination of any institution or society. I don't want a trop[ical] station for this Univ[ersity], but I do want this Univ[ersity] to gain with others in establishing one that we can all feel free to use."[108] MacDougal, ambitious and likely already on the look out for more influential positions than his University of Minnesota professorship, had his own reasons for supporting institutional flexibility for the laboratory.

108.Coulter to Farlow, 1897, cited in Andrew Denny Rodgers, *John Merle Coulter: Missionary in Science* (Princeton, NJ: Princeton University Press, 1944), 168.

The Tropical Laboratory Commission was finally composed of MacDougal, Coulter, William Gilson Farlow of Harvard, and Douglas Houghton Campbell of Stanford.[109] With Campbell and Farlow on board, the project now had the support of senior and extremely influential botanists, representing different regions of the country. While the original intention was that the commission would tour through the Caribbean as a group during the summer of 1897, administrative, teaching, and family duties complicated their plans. In the end, only MacDougal and Campbell traveled together, on a much reduced itinerary. Farlow and Coulter promised to make a follow-up inspection during the winter months.

On May 27, 1897, MacDougal and Campbell set sail from Boston to Jamaica. They arrived in Port Antonio in darkness at the end of the sixth day. As morning came, they "were greeted with the sight of cocoa palms, breadfruit trees, bananas, and other evidences of the tropics."[110] Traveling to Kingston by rail, they met the director of Jamaica's Department of Public Gardens and Plantations,William Fawcett, who eagerly guided them through the facilities at Hope and Castleton Gardens, nodes in the vast, global botanical network of Kew Gardens.[111] Later, they visited the gardens at Bath. A century earlier they had been the destination of Sir

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109."Editorials: Tropical Laboratory Commission," *Botanical Gazette* 23, no. 2 (1897): 126-27; Daniel T. MacDougal, "The Tropical Laboratory Commission," *Botanical Gazette* 23, no. 2 (1897): 129. Coulter and MacDougal hoped eventually to add British and German representatives or advisors to the Tropical Laboratory board of directors—giving the project the additional prestige of being international, but keeping the Americans in charge. They aimed high, courting Francis Darwin and Karl Goebel, apparently with some success. See Box 1, Folder 2: Tropical Laboratory Commission, Daniel T. MacDougal Papers, NYBG.

110.Douglas Houghton Campbell, "Botanical Aspects of Jamaica," *The American Naturalist* 32, no. 373 (1898), 34.

111.On Kew and colonial botany, see Lucile Brockway, *Science and Colonial Expansion: The Role of the British Royal Botanic Gardens* (New York, NY: Academic Press, 1979); Richard Harry Drayton, *Nature's Government: Science, Imperial Britain, and the 'Improvement' of the World* (New Haven, CT: Yale University Press, 2000); Londa Schiebinger, and Claudia Swan, *Colonial Botany: Science, Commerce, and Politics in the Early Modern World* (Philadelphia, PA: University of Pennsylvania Press, 2005).

Joseph Banks' infamous breadfruit plants, the paragon of colonial botany, but were now in sad disrepair. MacDougal and Campbell made connections with colonial officials and the local scientific community, all of whom appeared enthusiastic about the possibilities of international collaboration. Macdougal's sensitive plant research was favorably received at the Institute of Jamaica, a small colonial scientific and literary society.[112] Far from British metropolitan scientific circles, and perhaps feeling somewhat neglected, the Jamaicans welcomed American interest in their colony.

During their stay, they also met with the American botanist James Ellis Humphrey, of Johns Hopkins University. Beginning in 1891, the itinerant "Johns Hopkins Marine Laboratory" had made three summer excursions to Jamaica, mainly around Port Henderson.[113] There was some talk of establishing a permanent marine biological laboratory there, and Humphrey agreed to cooperate with the Tropical Laboratory Commission. He thought "it would be an unfortunate mistake to make such an establishment as is proposed exclusively botanical or zoological." The "cooperation of both biological groups" would give it "added strength," and "the very great mutual advantage of the association must be self evident."[114]

With passes provided by the railway company, Campbell and MacDougal continued to explore the botanical resources of the island. As they traveled high into the Blue Mountains, they encountered a "bewildering variety" of ferns and mosses.[115] About two thousand feet from the

112. Daniel T. MacDougal, "Mimosa: A Typical Sensitive Plant."

113. The members included among their ranks a young Thomas Hunt Morgan. W. K. Brooks, "Johns Hopkins Marine Laboratory," *Science* 19, no. 465 (1892): 10-11; William S. Swindle, "Notes From a Marine Biological Laboratory," *Popular Science Monthly* February (1894): 449-58.

114. James Ellis Humphrey, "The Tropical Laboratory," 50.

115. Douglas Houghton Campbell, "Botanical Aspects of Jamaica," 38.

summit, they reached the Hill Garden, known more often simply as "Cinchona." Established in 1868, it was the site of an experimental plantation of cinchona—a tree whose bark is the source of the antimalarial drug quinine, and a linchpin for the consolidation of Britain's tropical empire. The cooler temperatures at five thousand feet made cinchona cultivation successful, but the experiment was abandoned as competition from Ceylon drove prices down in the 1880s.[116] The same microclimates that made the area particularly useful for experimental horticulture also made it extremely diverse in species. The steep mountains had the effect of creating substantial variations in local moisture, light penetration, and temperature, where different kinds of plants found ways to adapt. Though Campbell arrived expecting an abundance of plant species, he still found himself astounded by how "extraordinarily rich and varied" the flora was. "Considering the size of Jamaica the number of indigenous plants is surprising," he wrote.[117] Also impressed, MacDougal tried to put this extraordinary diversity into perspective:

> The equatorial zone is the home of an enormous number of species. The island of Jamaica, with an area of about 6,000 square miles, about that of Connecticut, furnishes approximately four-fifths as many species of flowering plants as are to be found in the United States east of the Mississippi river.[118]

Although Jamaica's system of botanical gardens continued to engage in experimental agriculture and acclimatization projects, Campbell and MacDougal felt that little in the realm of physiology

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116. William Fawcett, "The Public Gardens and Plantations of Jamaica," *Botanical Gazette* 24, no. 5 (1897): 345-69; Roland Wenzlhuemer, *From Coffee to Tea Cultivation in Ceylon, 1880-1900: An Economic and Social History* (Boston: Brill, 2008), 75.

117. Douglas Houghton Campbell, "Botanical Aspects of Jamaica," 40.

118. Daniel T. MacDougal, "Mimosa: A Typical Sensitive Plant." Comparing the number of species in a given area of the tropics and the temperate zone—often a familiar American state—would become an increasing common trope. over the twentieth century. Interestingly, MacDougal made this comparison two years before Eugen Warming commented on the need to begin to quantify "this curious tropical richness of species by numbers" Eugenius Warming, "On the Vegetation of Tropical America," *Botanical Gazette* 27, no. 1 (1899), 2.

or ecology was being done with the colony's vast native botanical resources—particularly when it came to the question of physiological adaptation to variations in climate. The field was wide open. The two travelers returned home in time to attend the Botanical Society of America meeting in August and share their exciting experience.

Just as they did, however, tragedy struck. James Humphrey was killed by an attack of fever, probably yellow fever, on August 17th. Another member of the Johns Hopkins party, Franklin Story Conant, also perished on arrival in Boston, just shy of his twenty-seventh birthday.[119] The deaths were a difficult blow. Only thirty-six years old, Humphrey's career held much promise. He was well-loved as a lecturer, and was fast gaining a strong reputation in experimental botany. Conant was a very recently minted PhD and a Johns Hopkins Fellow with great prospects ahead. The loss of two young men with such potential was a serious shock to their colleagues. It also slowed action on the establishment of a tropical laboratory to a snail's pace. Further outbreak of yellow fever in Jamaica lead to quarantines of the ports. Fear of the disease was likely amplified by the contemporaneous outbreak in Cuba, which threatened the entire region.[120] Coulter and Farlow quickly decided to postpone their follow-up visit. Paradoxically, Humphrey himself had once assured American botanists of the "exceptionally healthful" climate of Jamaica.[121] Now, there was widespread doubt about the safety of any location in the tropics.

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119. A third, H. L. Clark, caught a mild version and survived, see W. K. Brooks, "Notes From the Biological Laboratory," *Johns Hopkins University Circulars* 17, no. 132 (1897). The uncertainty around the cause of Humphrey and Conant's deaths are likely due to an unwillingness on the part of local officials to declare the disease yellow fever, and thus risk panic and jeopardize commerce. On this phenomenon, see Mariola Espinosa, *Epidemic Invasions*, 25.

120. Ibid., 24-30.

121. James Ellis Humphrey, "The Tropical Laboratory," 50.

Then, within a few months, the slow simmer in Cuba came to a full boil. After three continuous years of war for independence from Spain, the prospect of an independent Cuba left many Americans clamoring for intervention. A free Cuba might mean anarchy, the stoppage of trade, and the irruption of disease. The Spanish-American War lasted barely three months, but it forever altered America's relationship with the world. A hot debate ensued over the fate of the new colonies: Puerto Rico, Cuba, Guam, and the Philippines (though three more bloody years of the Philippine-American war remained until the US had a semblance of control there). For biologists, as for Americans as a whole, the acquisition of an overseas empire fueled uncertainty, as well as a sense of great opportunity. Circumstances had changed. The consideration of a laboratory site would have to start over from scratch. Coulter suggested that it was "a good thing that we did not undertake to settle in the West Indies until after the war."[122] "My thought has been towards Porto Rico, since the war has secured it for us," he told Farlow, "It may be that Cuba would be even more available, but its fate does not seem to be so well fixed as that of Porto Rico."[123] To an impatient MacDougal, he wrote, "Cuba and Porto Rico seem open to us now...[but] the whole thing is so indefinite at present that I cannot say anything positive."[124]

It was at this time that Nathaniel Lord Britton was working to build up his own "botanical empire," expanding the New York Botanical Garden's collections in the US West and the Caribbean. Britton was ambitious to make his garden an American Kew, and was eager to extend a "botanical Monroe doctrine" making American botanical collections from the Western

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122. Coulter to Farlow, 17 May 1898, cited in Andrew Denny Rodgers, *John Merle Coulter*, 170.

123. Coulter to Farlow, 1898, cited in Ibid., 172.

124. Coulter to Macdougal, 22 September 1898. Box 1, Folder 2: Tropical Laboratory Commission, Daniel T. MacDougal Papers, NYBG.

Hemisphere equal or superior to those of Europe.[125] At the close of the Spanish-American War, Britton's eye focused particularly on Puerto Rico, where Coulter had hoped the laboratory might eventually come to rest. With funding from the Vanderbilt family, Britton made his first of many future forays into Puerto Rico.[126] Coulter was somewhat frustrated by this new competition, but felt that Britton did not yet have a total monopoly on science on the island. He told MacDougal that "Britton is fortunate in having a Vanderbilt to back him...[But] they are going at it purely from a taxonomic standpoint, to which they are, as far as I am concerned, entirely welcome. I would wish the tropical laboratory to concern itself chiefly with physiology, morphology, and ecology."[127] MacDougal, whose correspondence with Britton was greatly increasing as the garden's power grew, playfully tested the waters, "Congratulations on your annexation of Porto Rico," he wrote to Britton, "I hope you will not devastate the native flora to such an extent as to render it unfit for a tropical lab."[128]

Soon, however, in 1899, MacDougal was himself scooped up by Britton, hired to become Director of Laboratories for the New York Botanical Garden. With MacDougal's new responsibilities, the tropical laboratory project came into the New York Botanical Garden's domain. In 1902, Britton attempted to secure funding for the tropical laboratory through the

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125. Peter Philip Mickulas, *Britton's Botanical Empire: The New York Botanical Garden and American Botany, 1888-1929*, Memoirs of The New York Botanical Garden (Bronx: New York Botanical Garden, 2007).

126. See Darryl E. Brock's dissertation-in-progress and Inés Sastre-D.J, and Eugenio Santiago-Valentín, "Botanical Explorations of Puerto Rico by N. L. Britton and e. G. Britton: Their Significance in Plant Conservation, Horticulture, and Education," *Brittonia* 48, no. 3 (1996): 322-36.

127. Coulter to Macdougal, 5 October 1898. Box 1, Folder 2: Tropical Laboratory Commission, Daniel T. MacDougal Papers, NYBG.

128. MacDougal to Britton, 7 October 1898, cited in Peter Philip Mickulas, *Britton's Botanical Empire: The New York Botanical Garden and American Botany, 1888-1929*, 129.

newly formed Carnegie Institution, the first private foundation to support ecological research.[129] Under this plan, the tropical laboratory would be part of a much larger project to study the relationship between vegetation and the environment in the US and the Caribbean.[130] The proposal followed in the footsteps of the program environmental laboratories advocated by Schimper and Cowles. The proposal replicated much of the exact language from MacDougal's original *Botanical Gazette* editorial, but added an emphasis on the economic importance of research related to the improvement of tropical agriculture. Nevertheless, the tropical laboratory did not win support from the Carnegie Institution. The Desert Botanical Laboratory, which was estimated to cost nearly half as much, did go through. MacDougal became involved in its establishment, and would be chosen as its director in 1905. Always ready to go where opportunity took him, he finally shifted his attention from tropical to desert plant adaptation. The tropical laboratory project remained in limbo.

Intemperate Ecology: Research at the Cinchona Botanical Station

Then, in 1903, news arrived that the Jamaican colonial government intended to relocate what work was still going on at Cinchona to other plantations, and was interested in renting out the garden's buildings. Britton jumped at the opportunity and immediately sent MacDougal to Jamaica to arrange for a lease. He hoped to gain the wider support of university botanists later.

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129. On the Carnegie Institution's early support of plant ecology and biological laboratories, see Robert P. McIntosh, "Pioneer Support for Ecology," *BioScience* 33, no. 2 (1983): 107-12; James D. Ebert, "Carnegie Instituiton of Washington and Marine Biology: Naples, Woods Hole, and Tortugas," *Biological Bulletin* 168 (1985): 172-82.

130. Beside the tropical laboratory, it included the establishment of a desert station in the American Southwest, the investigation of the effect of forests on climate change, and a large-scale exploration of the flora of the West Indies in relation to climate, historical human usage, and economic potential for the future, see Carnegie Insitution of Washington, *Year Book No. 1 1902* (Washington, DC: Carnegie Insitution of Washington, 1903), 3-10.

Of the several hundred acres of the original plantation, the New York Botanical Garden's ten year lease acquired the use of a small residence ("Bellevue House"), three modest laboratory buildings, two greenhouses, a stable, servants quarters, and ten acres of surrounding grounds.[131] A drying house and a darkroom were later added. Though Cinchona itself was relatively isolated and accessible only by bridle path, American visitors were welcome to access the libraries, herbaria, living plant collections, and other facilities at Castleton and Hope through the cooperation of William Harris, the superintendent of Jamaica's botanical gardens. Cinchona's laboratory housed glassware and common reagents¸ and was equipped with pigeon holes for drying plants. Visitors would have to bring along their own microscope and any other specialized equipment. Jamaican servants, and sometimes researcher's wives, attended to the domestic chores, allowing researchers to be free to pursue their work.[132] Visiting collectors were also advised to hire a "boy"—in reality a black man of any age—to cut trails and carry supplies and specimens. Many collectors over the years also came to rely on David Watt. He was known for his encyclopedic knowledge of the flora of the Blue Mountains, gained through long residence and interaction with both British and American botanists.[133] Indeed, he is one of the only Afro-Jamaicans to be referred to by name in visitors' publications.

131.N. L. Britton, "The Tropical Station at Cinchona, Jamaica," *Journal of the New York Botanical Garden* 5, no. 49 (1904), 251.

132.Janice Emily Bowers, *A Sense of Place: The Life and Work of Forrest Shreve* (Tucson: University of Arizona Press, 1988), 30.

133.Winifred J. Robinson, "A Visit to the Botanical Laboratory at Cinchona, Jamaica," *Journal of the New York Botanical Garden* 5, no. 58 (1904), 193-94; William Seifriz, "The Length of the Life Cycle of a Climbing Bamboo. A Striking Case of Sexual Periodicity in Chusquea Abietifolia Griseb," *American Journal of Botany* 7, no. 3 (1920), 86, 91.

Over the next few years, an average of four or five American botanists visited each year.[134] Travel expenses ranged from $140 to $230. Most were from the New York Botanical Garden or Johns Hopkins University, as well as a variety of small eastern colleges. Nearly a third of the researchers were women, a somewhat surprising fact given the highly restrictive policies affecting female visitors in place at Barro Colorado Island, for example, later in the century.[135] Visiting researchers worked on a variety of problems, including transpiration, the physiology of epiphytes, the embryology and reproduction of ferns and flowering plants. MacDougal himself arranged for the same species to be cultivated at Cinchona, New York, and Arizona in order to test the heritability of environmentally-induced variations.[136] In spite of the strong emphasis on experimentalism in the discussion leading up to Cinchona's founding, a large number of visitors stopped at Cinchona primarily as part of a larger collecting expedition through Jamaica. Britton's own interests, of course, were primarily taxonomic and collections remained the foundation of the New York Botanical Garden's mission. Indeed, Britton was most interested in using Cinchona as a plant introduction garden, growing delicate tropical seeds and cuttings in their

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134. Information on visitors comes from *"Tropical Laboratory," Bulletin of the New York Botanical Garden 4 (1905): 28; Lucien Marcus Underwood, "A Report on the Condition of the Tropical Laboratory," Journal of the New York Botanical Garden 7, no. 83 (1906): 250-55; "Tropical Laboratory," Bulletin of the New York Botanical Garden 5 (1906): 26; Laboratories, vol. 6, Bulletin of the New York Botanical Garden (1908); Bulletin of the New York Botanical Garden 7 (1909): 161-62, 285, 323; Duncan S. Johnson, "Johns Hopkins Botanists at Cinchona," Journal of the New York Botanical Garden 11, no. 132 (1910): 271-72; Laboratories, vol. 8, Bulletin of the New York Botanical Garden (1912); "Proceedings of the New York Entomological Society," Journal of the New York Entomological Society 20, no. 4 (1912): 295-306.*

135. Pamela M. Henson, "Invading Arcadia: Women Scientists in the Field in Latin America, 1900-1950," *Americas* 58 (2002): 577-600.

136. Daniel T. MacDougal, "Discontinuous Variation in Pedigree-Cultures," *Popular Science Monthly* 69 (1906), 214.

natural conditions and transporting them to New York when they were hardy enough to be transplanted into his garden's collections.

Nevertheless, the line between laboratory-based experimentalism and field-based observation was blurry. Given the explosion of interest in the cryptogams around the turn of the century, taxonomic research could look much like experimental work.[137] Identifying plants whose reproductive anatomy—essential for proper classification—was not visible to the naked eye required painstaking morphological investigation under the microscope. At Cinchona, the visitor A. W. Evans noted, there was no difficulty crossing such boundaries; "The study of tropical Bryophytes involves careful work in the field followed by careful study in the laboratory and herbarium, and the facilities offered at Cinchona and Hope Gardens are probably unequalled anywhere else in the tropics except at the botanical garden of Buitenzorg."[138] Indeed, contrary to the caricatured taxonomist who spends all of his time working with dusty, dead specimens that he has never seen growing in nature, taxonomists were becoming increasingly interested in living plants. Although the "new botany" is associated with the experimental turn, taxonomists also sympathized with its call for more attention to living plants. In the tropics especially, existing specimens and data were often fragmentary and questionable, having been collected in earlier centuries when practices of preservation and the recording of locality data were not yet standardized. Evans explained the need for the examination of living tropical plants by taxonomists, "The student of taxonomic problems soon becomes impressed by the imperfection

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137. "Cryptogam" means "hidden marriage," a reference to their lack of visible reproductive organs. The group is particularly diverse in the tropics and includes ferns, mosses, liverworts, lichens, algae, and, at that time, fungi. On the growing interest in cryptogams within the new botany, see Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890-1905," 176.

138. Evans, quoted in Duncan S. Johnson, et al., "Cinchona as a Tropical Station for American Botanists," 918.

of our knowledge of tropical species and by the difficulties of interpreting the older records regarding them. It will, in fact, be a very long time, unless the number of workers becomes much greater, before our knowledge can even approach completeness."[139] Specimens collected in different seasons, life stages, or under different environmental conditions might falsely appear to be completely different species; however, "at Cinchona, one is likely to find all of the stages in the life history of a species on almost any single day."[140] Knowing the range of a species' individual or geographic variation, let alone speculating at the causes of such variation, was difficult without observation in the tropics. Even MacDougal noted the increasing importance to taxonomists of comparing dried specimens with live material, growing under varying environmental conditions.[141]

Indeed, the whole program of tropical research implied a need to bring a strong comparative angle—an approach usually associated with old-fashioned natural history—into laboratory work. While experimentalists would come to depend on just a few model organisms, early in the century they were still searching for species that they could be sure displayed truly general characteristics. As the cytologist Clifford H. Farr explained, "Much light is frequently thrown on perplexing cytological problems by the study of related genera and species; and thus to one whose investigations have been confined to those species growing in the temperate zones, Cinchona furnishes splendid opportunity for the extension of his work to such allied tropical

139. Evans, quoted in Ibid.

140. C. H. Farr, quoted in Ibid.

141. Daniel T. MacDougal, "Discontinuous Variation in Pedigree-Cultures," 214.

species."[142] Gaining experience with tropical organisms could broaden one's views about the morphology and physiology of cells, helping to sort out general phenomena from particular environmental adaptations and rid oneself of temperate-zone biases. At the same time, tropical plants and animals might prove well-adapted to use in the lab for purely practical reasons. For example, Farr noted that "In the plants of a tropical rain-forest... there is much less cutin, fewer hairs, etc., to interfere with the penetration of fixing solutions, and hence there is the probability of better fixation."[143] The push for tropical research was in this way part of a broader interest in locating potential experimental organisms.[144]

In this fluid environment, where experimental and natural historical practices mixed freely, ecology thrived. In fact, the studies that perhaps most approached MacDougal's original hopes for such a station were those of Duncan S. Johnson and Forrest Shreve on rainforest ecology. Johnson's work was somewhat opportunistic. In 1909, unusually heavy rains—18.3 inches in twenty-four hours at Cinchona—caused disastrous flooding and landslides.[145] From the lookout at Cinchona, one could see entire coffee plantations and acres of native vegetation washed away. Johnson had visited the station in 1903 and 1906, and the dramatic change in the landscape that he witnessed made a deep impression on him. In the valley where had once collected Piper and Peperomia amid the dense forest, he now encountered a "gray desert of broken rock and pebbles."[146] He resolved to return to the site over the coming years and decades

142. Farr, quoted in Duncan S. Johnson, et al., "Cinchona as a Tropical Station for American Botanists," 919.

143. Farr, quoted in Ibid.

144. Robert E. Kohler, "Drosophila: A Life in the Laboratory," *Journal of the History of Biology* 26, no. 2 (1993): 281-310.

145. Duncan S. Johnson, "Invasion of Virgin Soil in the Tropics," *Botanical Gazette* 72, no. 5 (1921), 305.

146. Duncan S. Johnson, "An Opportunity to Study the Origin and Development of a Tropical Forest," *Journal of*

in order to observe the process of revegetation. He thought the potential value of such a study ranked with that of the island of Krakatoa.[147] Johnson's work did not directly take advantage of the laboratory itself; it was a "natural experiment."[148] Nevertheless, the expected permanence of the station surely helped him conceive of a the study as a long-term endeavor. Johnson returned in 1919 and 1926. He was surprised to see how slowly the forest recovered. Nearly a decade after the flood, large areas were still covered by little more than bare rock. Though all the conditions of physical environment were in place to produce lush vegetation, the forest had not returned. Perhaps physical factors were not the primary determinant of vegetation, as was widely assumed? Even more interestingly, the pattern of invasion by new plants was just the opposite of what Frederick Clements, Eugen Warming, Arthur Tansley and Thomas Ford Chipp predicted.[149] Research based largely in the temperate zone, and especially in the American Midwest, stated that the "lower plants," algae, mosses, and lichens, would be the first to establish themselves in a forest climate, followed by annual flowering plants, and later by perennial shrubs and trees. Instead, Johnson found the reverse. He argued that this inversion might be a general feature of plant succession in tropical forests, distinguishing them from temperate forests.

Today, Forrest Shreve is best known for his work in the Sonoran desert, foundational to desert ecology. He first visited Cinchona with Duncan Johnson in 1903, as a Johns Hopkins

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the New York Botanical Garden 11, no. 132 (1910), 274.

147. The study of plant succession on Krakatoa after its decimation by volcanic eruption was, coincidentally, begun by Treub, the director of Buitenzorg.

148. See the chapter "Nature's Experiments" in Robert E. Kohler, *Landscapes and Labscapes*, 214-48. See also Jared Diamond, "Laboratory, Field and Natural Experiments," *Nature* 304, no. 18 (1983): 586-87.

149. Duncan S. Johnson, "Invasion of Virgin Soil in the Tropics."; Duncan S. Johnson, "Revegetation of a Denuded Tropical Valley," *Botanical Gazette* 84, no. 3 (1927): 294-306.

graduate student, and again in 1905 with an assistantship at the New York Botanical Garden.[150] These encounters with the tropics left him wishing he could stay longer; Shreve felt he had had "just about enough experience" to identify interesting problems, but "not enough time afterwards to go at them."[151] MacDougal gave him that chance. He hired Shreve at the Desert Botanical Laboratory, promising him a year in Tuscon followed by several months in Jamaica. On July 1, 1909, Shreve was back in Jamaica, this time with his wife, Edith. Shreve was greatly influenced by Schimper's *Plant-Geography Upon Physiological Basis*.[152] Schimper had coined the term "rain-forest" just a few years earlier, and Shreve set out to apply Schimper's methods scrupulously in the rainforests surrounding Cinchona. He amassed the historical rainfall, humidity, air and soil temperature data that had been collected by the British, and began gathering a wide array of environmental variables on his own. Shreve measured the loss of water (transpiration) over the course of the day in eight representative rain-forest species, rigging up a dark room and moist chamber in the lab to measure the effect of light and humidity on this process. By calculating the ratio of transpiration to evaporation in these species, he was able to create a standard that made comparisons possible between "widely separated localities, with a basis of accuracy which removes this subject from the limbo of controversy."[153]

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150. Forrest Shreve, "A Collecting Trip at Cinchona," *Torreya* 6, no. 5 (1906): 81-84; Forrest Shreve, "A Winter at the Tropical Station of the Garden," *Journal of the New York Botanical Garden* 7, no. 80 (1906): 193-96.

151. Shreve to MacDougal, 15 April 1907, cited in Janice Emily Bowers, *A Sense of Place*, 17.

152. Janice Bower's biography has been very helpful to me in explaining and contextualizing Shreve's work; see Ibid., 13.; A. F. W. Schimper, *Plant-Geography Upon a Physiological Basis,* trans. William Rogers Fisher (Oxford: Clarendon press, 1903). Robert Kohler also discusses Shreve's later tenure at the Desert Botanical Laboratory, where he encountered resistance to his combination of laboratory and field work; Robert E. Kohler, *Landscapes and Labscapes*, 205-10.

153. Forrest Shreve, *A Montane Rain-Forest: A Contribution to the Physiological Plant Geography of Jamaica* (Washington, DC: Carnegie Institution of Washington, 1914), 59.

Sheve's work was a true synthesis of field and laboratory work, and his findings presented some serious challenges to prevailing ideas about tropical rainforests. He found that contrary to botanists' assumptions, desert plants had high transpiration rates in comparison to tropical plants; transpiration rates are actually proportional to evaporation rates. Many of Shreve's other finding were also at odds with general botanical opinion. Carefully monitoring phenological changes, Shreve found unexpected seasonal periodicity among some forest plants, and speculated that they were due to "inherited differences in physiological constitution."[154] He also disputed the supposition that the elongate leaf tip morphology of many tropical plants was an adaptation for shedding water in moist climates.[155] He had no doubt that certain structures were climatic adaptations, but insisted that botanists use experimental methods to test their hypotheses. In contrast to many botanists, Shreve's work in Jamaica and Arizona led him to believe that the montane rainforest, like the desert, was actually a harsh and extreme environment, less favorable to the origination of new species than intermediate climates in the tropical lowlands or moist temperate zone.[156]

Finally, like Johnson, Shreve questioned the applicability of the prevalent model of plant succession to the tropical rainforest. Shreve identified a variety of vegetation types on the mountains' windward and leeward slopes, ridges, and ravines, and in the canopy of the forest around Cinchona. He saw these types, however, as continuous rather than discrete. None, he argued, could be seen as better adapted to its own particular conditions than any other, and he

154. Ibid., 107.

155. Forrest Shreve, "The Direct Effects of Rainfall on Hygrophilous Vegetation," *Journal of Ecology* 2, no. 2 (1914): 82-98.

156. Forrest Shreve, *A Montane Rain-Forest*, 110.

saw no possibility that one type could come to dominate the area. "In other words," Shreve argued in strong condemnation of the Clementsian view, "there is no means by which it might be possible to fix upon any one of the five types as representing the so-called 'climax' forest of the Jamaican montane region."[157] No one particular association of plants, but rather diversity and constant change were characteristic of the rainforest. In closing his *A Montane Rain-Forest*, he highlighted the importance of this diversity and the need to fuse laboratory and field techniques in order to understand it:

> There is no type of vegetation in which may be found a wider diversity of life forms than exist side by side or one above the other in a tropical montane rain-forest. Together with the structural diversities, discoverable in the field or at the microscope, are diversities of physiological behavior, discoverable by observation or experiment.[158]

Shreve's work raised questions that tropical biologists would continue to wrestle with as the century went on. Why are there so many species of trees in the rainforest? To what degree do variations in moisture, light, and temperature control plant distribution? What are the vertical relations in plant diversity between the canopy and the forest floor? Shreve followed Schimper in seeing these questions as physiological in nature. He did not make a distinction between physiology and ecology, and characterized the work he did as "physiological plant geography." Moreover, he focused on transpiration rather than photosynthesis as modern ecologists would today, and some of his conclusions did not pan out.[159] Perhaps for this reason, present-day tropical biologists tend to cite Shreve simply for his data.[160] Since the late 1950s (see Chapter 5),

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157. Ibid., 106.

158. Ibid., 109-10.

159. Janice Emily Bowers, *A Sense of Place*, 25-26.

160. Citations of Shreve's work can be found in some foundational texts, however: P. W. Richards, *The Tropical Rain Forest: An Ecological Study* (Cambridge: Cambridge University Press, 1952); P. W. Richards, "What the

tropical biologists have approached questions of the diversity and distribution of species through the complex interactions and trophic structures of ecological communities, rather than primarily through the correlation of distribution with the physical factors of the environment. Nevertheless, this was the first foray of American scientists into the field of tropical ecology, and the findings of Shreve and Johnson demonstrated the potential of tropical research to challenge broader biological assumptions—just as MacDougal had suggested. Moreover, the existence of a station that was intended to be permanent encouraged the pursuit of longer-term studies. More senior scholars who had visited the place, like Johnson and MacDougal, encouraged younger scholars, like Shreve, to return there, forming the very beginning of a community of American tropical researchers.

Conclusion

Cinchona never quite became a "mecca" of American tropical botany, however. As time went on, it was plagued by financial instability and disruption by war and changing international relations. The New York Botanical Garden made several attempts to gain the cooperation of American botanists in the maintenance and improvement of the station. In 1907, for example, after petitioning forty-five universities, gardens and museums, only five of the twenty necessary would donate $100 in order to install a resident investigator. Such staff would have greatly increased the station's stability and usefulness to visitors. As the ten-year lease came to a close, Britton finally decided that the trickle of mostly non-New York Botanical Garden researchers

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Tropics Can Contribute to Ecology," *Journal of Animal Ecology* 33, no. 1 (1964): 1-11; Egbert Giles Leigh, Jr., "Structure and Climate in Tropical Rain Forest," *Annual Review of Ecology and Systematics* 6 (1975): 67-86; R. H. Whittaker, "A Consideration of Climax Theory: The Climax as a Population and Pattern," *Ecological Monographs* 23, no. 1 (1953): 41-78.

could not justify a renewal. After vociferous lobbying organized by Johnson, however, Smithsonian secretary Charles Walcott agreed that his institution could take up a three-year lease of Cinchona beginning in 1916.[161] The Smithsonian performed only an administrative role on behalf of fourteen American botanical institutions that pledged nominal financial support to the station.[162] Although the Great War seemed far away as the Americans took up the new lease, they soon found travel disrupted.[163] Unable to send any investigators during the remainder of the war, the Smithsonian suspended their lease until peace returned.[164]

By 1921, however, the colonial Government of Jamaica decided to terminate the lease and "retain the station for the use of British and Jamaican botanists."[165] The political dimensions of science in the Caribbean were changing. British botanists had initially praised Americans for their "enterprize" and looked forward to "cordial co-operation" in establishing a tropical laboratory.[166] As US hegemony in the region was consolidated, British botanists began to express unease about the rapidity of America's development of botanical institutions:

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161. Lease between the Colonial Secretary of Jamaica and Charles D. Walcott, Secretary of the Smithsonian Institution, 28 December 1916. Box 107, Folder 2: Cinchona Botanical Station, 1915-1916, RU 45, SIA.

162. These included: the University of Chicago, University of Colorado, Columbia University, Harvard University, Grinnell College, Johns Hopkins University, the State University of Iowa, the Missouri Botanical Garden, Princeton University, Stanford University, Wellesley College, Yale University, and, as individual contributors, Britton and MacDougal (who, at $50, gave at least twice as much as any other sponsor). See "List of Subscribers to the Fund for the maintenance of Cinchona," undated (1916?). Box 11, Folder 6: Cinchona Botanical Station Correspondence, 1917-1919, RU 45, SIA.

163. For example, Douglas Houghton Campbell to Frederick Orpen Bower, 8 May 1916. Papers of Frederick Orpen Bower, GUAS.

164. Johnson to Walcott, 18 February 1918. Box 11, Folder 7: Cinchona Botanical Station Correspondence, 1917-1919, RU 45, SIA; Duncan S. Johnson, "The Cinchona Station," *Botanical Gazette* 69, no. 4 (1920): 347-48.

165. Smithsonian Institution Board of Regents, *Annual Report of the Board of Regents of the Smithsonian Institution 1921.* (Washington: US Government Printing Office, 1922), 12. See also Johnson to Walcott, 15 February 1921. Box 11, Folder 8: Cinchona Botanical Station Correspondence, 1920-1923, RU 45, SIA.

166. "Book-Notes, News, Etc," *The Journal of Botany* 34 (1897): 112.

> Vast as is their territory, and numerous as are their experimental stations... our cousins are not yet satisfied. They have invaded British territory, in a most genial and friendly manner it is true, but still they have annexed, with our consent, a portion of the island of Jamaica, and there they have established, at 'Cinchona,' a botanical laboratory and research station... The policy of the 'open door' of the Americans in these matters prevents us from doing anything but acquiesce... We do not question the utility of ironclads and cruisers as protectors of our commerces but it is obvious... that if we do not largely extend our scientific training and induce our wealthy citizens to follow the example of their American brethren in endowing science, the necessity for protection will vanish.[167]

By the 1920s, with British power in the Caribbean on a sharp decline, interest in scientific collaboration waned.[168] Cinchona shifted into the hands of the Jamaican Department of Agriculture.

While Cinchona was occupied by American botanists, it fostered a mixture of experimental and natural historical approaches. It was not only a tropical biological laboratory, but also a "laboratory" for how to organize biology in the tropics. Because the tropics were important as a test of theories about physiological adaptation to the environment, botanists with an ecological bent—those most likely to see laboratory and fieldwork as complementary—were attracted to the stations, and were the most prominent in the larger movement for tropical botanical institutions. Moreover, the basic life-histories and taxonomic relationships of most tropical organisms were poorly known; even the most hardline experimentalist would have to rely on traditional natural historical work to a large extent. Under such circumstances,

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167. This passage appears in both "Laboratories for Botanical Research," *Science New Series* 20, no. 496 (1904): 27-28; "Science in the British West Indies," *Science* 20, no. 518 (1904): 769; "Botanical Laboratories in England and America," *Journal of the New York Botanical Garden* 5, no. 54 (1904): 109-12. Admittedly, British botanists are here using the American tenure at Cinchona for their own rhetorical purposes to gain funding for science.

168. On US-British relations see Warren G. Kneer, *Great Britain and the Caribbean, 1901-1913: A Study in Anglo-American Relations* (East Lansing: Michigan State University Press, 1975).

experimental and natural history approaches could not be at war. Comparative methods had a role in conjunction with experiment: a better understanding of tropical organisms was needed for comparison with temperate organisms. What aspects of physiology were truly general, and not just regional adaptations? What explained these differences in adaptation? The tropics were not just interesting for their difference, but for how that difference might challenge broader biological generalizations.

In order to access tropical environments, however, American botanists at first aligned themselves with the existing networks of European colonial science in the Caribbean. Although the existing literature emphasizes Americans' "indifference to tropical research" before 1898, the idea that American science "followed the flag" into the tropics is only partially true.[169] As we will see in Chapter 2, American scientists would indeed become deeply involved in US government and corporate efforts to manage tropical environments and populations and to develop tropical agriculture as the twentieth century progressed. But the tropical laboratory movement of the 1890s shows that American botanists' ambitions actually anticipated US state intervention. Even after 1898, they formed coalitions among American universities, museums, and gardens and British colonial institutions in order to access the tropics.[170] Their early emphasis on basic research and international cooperation should not be seen as an indication of

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169. For this claim, see Stuart McCook, "'The World Was My Garden': Tropical Botany and Cosmopolitanism in American Science, 1898-1935," in *Colonial Crucible: Empire in the Making of the Modern American State*, ed. Alfred W. McCoy, and Francisco A. Scarano (Madison, WI: University of Wisconsin Press, 2009), 499; Richard A. Overfield, "Science Follows the Flag."

170. Indeed, this pattern continued for years after the Spanish-American War brought the US formal tropical colonies. Many American biologists preferred to work at stations in the British colonies of Bermuda, Jamaica, and British Guiana (now Guyana). They often cited the same factors discussed in the 1890s and 1900s, such as relative political stability and the prevalence of the English language.

political disinterestedness, however. As we saw, American botanists certainly reflected a broader US attitude of nationalism and possessiveness toward the Caribbean. At the same time, they borrowed from and emulated European colonial science and began to imagine and rehearse their own; Cinchona was, again, a “laboratory” for an American form of colonial science. Indeed, by envisioning tropical research as part of a campaign to make botany a global science, with the US at its head, American botanists engaged in their own imperial visions.

Nevertheless, the alliance with a foreign empire and the lack of US corporate or government funding had serious drawbacks. Sparse funding meant that there were no scholarships available to help encourage students to visit. Duncan Johnson and A. W. Evans managed to bring students to Cinchona on their own initiative, but there were never any formal classes like those at the Marine Biological Laboratory or other stations within the US. The spotty, highly fluctuating attendance and a lack of a resident director or investigator meant that no real community of researchers or a central research program was ever formed. The absence of a large donor with a specific agenda allowed individuals to be free to pursue eclectic and boundary-crossing research, but a lack of community meant that a share of interdisciplinary knowledge about the environment around Cinchona was never built up. In fact, while Johnson opportunistically pursued a long-term “natural experiment,” he was the exception. Without steady institutional support, there was no continuity of research. Shreve, for example, returned to Tuscon and desert ecology after his research for *A Montane Rain-forest*. The questions he raised about tropical rainforests would wait decades to be addressed in more depth. Most importantly of all, when the British government decided to terminate the lease, American botanists had no leverage to resist.[171] The vision of an American Buitenzorg never materialized.

171. Johnson continued to take students to Jamaica after the demise of the Cinchona station, but he stayed on United Fruit Company land, taking advantage of the American company's increased penetration of the island. Among these students, for example, was the ornithologist Alexander Skutch. *"A Naturalist in the Rainforest: A Portrait of Alexander Skutch" (VHS, Bullfrog Films, Paul Feyling Productions, 1995).*

Chapter 2

The Harvard Station: The Intersection of Academic and Business Interests at Soledad, Cuba

A Place for "pure botany"?

In a promotional article for the Cinchona Botanical Station, published just as the New York Botanical Garden lease there was lapsing, botanist Duncan Johnson lamented the fact that it was at that time "the only station in the western tropics for the study of pure botany." "In the last decade," he noted "it is true, several other botanical laboratories, all of them primarily for experimental-station work, have been organized in the American tropics...At none of these has much botanical research thus far been carried on, except the economic agricultural work of the regular staff members."[172] Rapid political and economic changes in the Caribbean were fostering a different sort of institution, motivated more by practical concerns than the theoretical conundrums of the "new botany" that had motivated MacDougal and others. In the wake of the Spanish-American War, and with the beginning of the construction of the Panama Canal (1904-1914), American power in the Caribbean grew. As the US military sought authority over the region's key strategic points, US corporations increasingly dominated Caribbean agricultural landscapes and economies.

Leaders in US businesses and at the USDA quickly saw the potential to strengthen their control over the biological resources of the Caribbean through research in economic botany and agriculture.[173] New research stations began to open throughout the region over the first two

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172. Duncan S. Johnson, "The Cinchona Botanical Station," *Popular Science Monthly* 85, no. 36 (1914), 523.

173. For an overview of the role of the USDA establishing institutions for tropical agricultural research, see Stuart

decades of the twentieth century (see Appendix 2).[174] Unlike Cinchona, these stations were geared toward agricultural botany and possessed much more intimate relationships with US corporations and government from the start. As such, they were more directly concerned with research on the exploitation and control of tropical environments than with the natural history of tropical flora and fauna.[175] None began with the broad ecological mission of the "tropical laboratory" originally envisioned by the American botanical community in the 1890s. Nevertheless, while their primary goal was to serve their patron's interests in making tropical agriculture more efficient and profitable, a closer look reveals the existence of more flexible relationships between academic, corporate, and national interests than might at first be expected.

McCook, "”'the World Was My Garden'”," 503-06.

174. Two subtropical marine research stations for "pure" research were, however, started in 1903: Dry Tortugas, funded by the Carnegie Institution, and the Bermuda Biological Station for Research (BBSR), operated through the cooperation of Harvard and New York University. Both were marine stations on the model of Marine Biological Laboratory at Woods Hole. Both also initially operated on a temporary basis. BBSR has greatly expanded and is in existence to this day, while Dry Tortugas closed due to lack of activity. They are not a major focus of this dissertation, first because their focus was strictly marine. Second, and more importantly they were outside of the sphere of US economic interest in the Caribbean. Bermuda is not in a strict geographical sense a Caribbean island at all, though it shares historical commonalities. The Florida Keys are Caribbean, but Dry Tortugas was extremely remote from the transportation and trade networks uniting the region. These stations were far less shaped by the field of commercial and national interests than the Harvard Station in Cuba which this chapter focuses on, though they would be interesting for comparative purposes for this reason. On these stations, see James D. Ebert, "Carnegie Instituiton of Washington and Marine Biology: Naples, Woods Hole, and Tortugas."; Inc. The Bermuda Biological Station for Research, *The First Century: Celebrating 100 Years of Marine Science* (St. George's, Bermuda: The Bermuda Biological Station for Research, Inc., 2003).

175. Usually, research focused on a single commodity—sugar, bananas, pineapples, rubber—and had the effect of reinforcing a colonial dependence on the US export market. Even when USDA station-builders began with talk of supporting crop diversity and "democratic agriculture," they were quickly captured by the interests of growers of one or two export crops: Richard A. Overfield, "Science Follows the Flag."; Richard A. Overfield, "The Agricultural Experiment Station and Americanization." For broader histories of these commodities see Sidney Wilfred Mintz, *Sweetness and Power: The Place of Sugar in Modern History* (New York, NY: Penguin Books, 1986); César J. Ayala, *American Sugar Kingdom*; Robert Thomson, *Green Gold: Bananas and Dependency in the Eastern Caribbean* (London: Latin America Bureau, 1987); John Soluri, *Banana Cultures*; Gary Y. Okihiro, *Pineapple Culture: A History of the Tropical and Temperate Zones* (Berkeley, CA: University of California Press, 2009); Mark R. Finlay, *Growing American Rubber: Strategic Plants and the Politics of National Security* (New Brunswick: Rutgers University Press, 2009).

This chapter will focus on one particular station, the so-called "Harvard Botanical Station" at the Soledad sugar plantation of E. Atkins and Company at Cienfuegos, Cuba (see Appendix 3). Though largely forgotten outside of Cuba today, it was the first US-run agricultural or botanical station in the tropics, established in 1900. The history of the Harvard Station is in many ways illustrative of American science in the Caribbean in the early twentieth century. It was built primarily to serve a US agribusiness, not the needs of local Cubans, and it was part of a broader movement to bring "scientific" agriculture to supposedly "backwards" people in the new dependencies of the US. Yet, while most histories of American science and empire have emphasized the role of the federal government (e.g. the USDA, the US military), the Harvard Station emerged through the collaboration of a private university and a business—specifically through the relationships of the Harvard scientists Thomas Barbour, Oakes Ames, and George Lincoln Goodale, the horticulturalist Robert Grey, and the businessman Edwin Atkins and his family.[176] The Harvard Station was thus shaped by the intersection of both academic and agricultural interests. Over time, the character of the Harvard Station gradually shifted as these intersecting interests realigned. It began as a sugar experiment station, broadened into a botanical garden, and by the 1920s became established as a fully-fledged tropical biological station for researchers with a wide range of interests, many of which were only very remotely related to sugar agriculture. By the 1930s, the Harvard Station, renamed the Atkins Institution, would become one of the premier sites for the training of a generation of US tropical biologists. The trajectory of the station was deeply shaped by both its place on an American-owned sugar

176. On the need to pay closer attention to the role of corporate-academic cooperation in the history of American empire, see Gregg Mitman, and Paul Erickson, "Latex and Blood: Science, Markets, and American Empire."; Simon Baatz, "Imperial Science and Metropolitan Ambition: The Scientific Survey of Puerto Rico, 1913-1934," *Annals of the New York Academy of Sciences* 776, no. 1 (1996): 1-16.

plantation in Cuba and by the maneuvers of the Harvard botanists and zoologists who administered and worked at the station.

The story of the Harvard Station's first two decades is important to the larger history of tropical biology not only because it would eventually become an important scientific training ground, but also because it introduces a web of academic, business, and national interests which continued to shape tropical biology as the century progressed. In particular, Thomas Barbour, a Harvard zoologist and major proponent of tropical research, will become an increasingly important figure as this dissertation progresses. Barbour only became involved with the Harvard Station a decade after Ames, Goodale, and Atkins had already established and expanded it into a botanical station. As we will see in this and the following chapters, however, his success at Soledad in merging the varied interests of patrons with American biologists' interests in the development of basic tropical research institutions would carry into other endeavors in the region.

Tropical biology emerged in a colonial context—the twentieth-century Caribbean—that was heavily dominated by funders whose primary interests were in scientific applications that would bolster US economic, military, and political hegemony in the region. Funding for research on the improvement of commodity crops for export was much easier to obtain than funding to study, for example, butterfly mimicry or photosynthesis in tropical trees. Yet, biologists like Barbour did manage to construct space for relatively "pure" research—or rather, for problems that emerged more directly out of the intellectual and professional contexts of their own disciplines than from the agendas of funders.[177] As we will see, the motivations behind both sides

177. The "pure" versus "applied" science distinction is useful in so far as they are actors' categories. From this point of view, "pure science" is defined as science done "for its own sake" or directed at more general or theoretical

of the collaboration between Harvard and E. Atkins and Company were much richer and more complicated than financial interests alone, and allowed a surprising degree of flexibility in research choice over time. Examining the history of the Harvard Station allows us to see not only how this particularly important institution was established, but also to understand the dynamics of the relationship between academic and corporate interests in an American colonial context.

E. Atkins and Company in Cuba

To understand the Harvard Station's position in relation to the aims and interests of Atkins and his business, we must first examine how Atkins' company gained a foothold in Cuba. US research stations, like colonial projects more broadly, rest on a foundation of land dispossession. In the 1870s and 1880s American ownership of Cuban land increased dramatically.[178] It was

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problems. Pure science may have the connotation of being disinterested and "untainted" by economic concerns. Conversely, "applied science," is done with an eye toward the solution of practical problems, often through the application of the theoretical principles of pure science to the specific problems of industry or technology. (In reality, of course, practical work may also lead to theoretical breakthroughs.) A new category of "basic" or "fundamental" science—denoting general scientific research that may or may not produce practical innovations—emerged in the middle of the twentieth century, breaking down some of the historical barriers between pure and applied science. The distinctions and historical relationships among pure, applied, and basic science have been discussed extensively the literature of the history of science and technology for decades. Some helpful examples include: Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought*, Revised and expanded ed. (Baltimore, MD: Johns Hopkins University Press, 1997), 200-10; Charles E. Rosenberg, "Science, Technology, and Economic Growth: The Case of the Agricultural Experiment Station Scientist, 1875-1914," *Agricultural History* 45, no. 1 (1971): 1-20; Ronald Kline, "Construing "Technology" as "Applied Science": Public Rhetoric of Scientists and Engineers in the United States, 1880-1945," *Isis* 86, no. 2 (1995): 194-221; Sheila Slaughter, and Gary Rhoades, "From "Endless Frontier" to "Basic Science for Use": Social Contracts Between Science and Society," *Science, Technology, & Human Values* 30, no. 4 (2005): 536-72; Jane Calvert, "What's Special About Basic Research?," *Science, Technology, & Human Values* 31, no. 2 (2006): 199-220; Paul Lucier, "The Professional and the Scientist in Nineteenth-Century America," *Isis* 100, no. 4 (2009): 699-732; Sabine Clarke, "Pure Science With a Practical Aim: The Meanings of Fundamental Research in Britain, Circa 1916-1950," *Isis* 101, no. 2 (2010): 285-311. In this chapter, however, I will generally avoid using pure and applied science as analytical categories, focusing instead on how the various interests of the actors involved in the Harvard Station shaped the facility and the specific choices of problems researched there. I will discuss these categories, however, when the actors themselves deployed them to further their interests.

178. Louis A. Pérez, *Cuba and the United States: Ties of Singular Intimacy* (Athens, GA: University of Georgia Press, 2003), 56-61.

during this time that a young Edwin Atkins joined his family's firm, E. Atkins & Company, and would soon take over its Cuban operations. The Atkins family had been involved in the sugar import trade since the 1830s and had grown in wealth and stature both in Cuba and back home in Boston.[179] By the 1860s, Elisha Atkins, Edwin's father, had begun to expand the business beyond trade and into offering credit to Cuban planters.[180] During the Ten Years' War with Spain, from 1868-1878, warring factions destroyed many of Cuba's cane fields and mills. In the war's wake came the abolition of slavery and a precipitous drop in sugar prices. Cuban planters had borrowed heavily from American financiers, including Atkins, during the war in order to stay afloat. The economic depression that followed left the creole planter class unable to pay back their loans. A wave of foreclosures ensued, allowing American investors to buy up sugar estates at an astounding rate. By 1883, every mill in Cienfuegos had changed ownership at least once.[181] Atkins himself purchased or acquired long-term leases on at least fifteen estates in the 1880s. By the 1890s, E. Atkins & Company was one of the largest plantation owners in Cuba.[182]

Among the estates Atkins acquired through foreclosure was Soledad, meaning "Solitude," located near the major port city of Cienfuegos on Cuba's southern coast.[183] Soledad

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179. Helen A. Claflin, *A New England Family* (Belmont, MA: H.A. Claflin, 1956), 91-95; Christopher Harris, "Edwin F. Atkins and the Evolution of United States Cuba Policy, 1894-1902," *The New England Quarterly* 78, no. 2 (2005), 203-06.

180. Elisha Atkins was also a director of the Boston Wharf Company and the Union Pacific Railroad at this time.

181. Louis A. Pérez, *Cuba and the United States: Ties of Singular Intimacy*, 57.

182. Benjamin Allen, *A Story of the Growth of e. Atkins & Co. And the Sugar Industry in Cuba* (New York, NY: E. Atkins, 1925), 18-21; Edwin F. Atkins, *Sixty Years in Cuba: Reminiscences* (Cambridge: Riverside Press, 1926), 30-137; Louis A. Pérez, *Cuba and the United States: Ties of Singular Intimacy*, 56-58.

183. Rebecca Scott gives a fascinating account of the changing social and political circumstances in Cienfuegos during the period of this chapter: Rebecca J. Scott, "Race, Labor, and Citizenship in Cuba: A View From the Sugar District of Cienfuegos, 1886-1909," *The Hispanic American Historical Review* 78, no. 4 (1998): 687-728.

became the center of Atkins' sugar operations, and it rapidly grew as he annexed adjoining estates. Developments at Soledad mirrored broader changes in the Cuban economy and agricultural landscape. From the 1890s through the 1920s, land under sugar agriculture expanded as US investments in Cuba skyrocketed.[184] Cuban agriculture transitioned from a patchwork system of estates, each with their own small sugar mills (*ingenios*), to a more centralized and industrial system, whereby a few powerful estates operated much larger mill complexes (*centrales*). The *centrales*, in addition to grinding the cane from their own plantations, contracted cane growing out to other plantations (*colonias*)—run by renters or small landowners who could not afford to modernize their milling equipment. The *centrales*, like Soledad, were increasingly owned by US companies. Moreover, like E. Aktins & Company, which also owned a sugar refinery in Boston, these companies consolidated their power over the Cuban economy as they became increasingly vertically integrated.[185]

With these strong foundations built up, E. Atkins & Company was well-positioned to endure the turbulent period of the Cuban War of Independence from 1895 to 1898. Soledad was out of the way of the heaviest fighting and Edwin Atkins skillfully played both sides of the conflict, secretly providing food and hospitality to both the Cuban and Spanish Armies.[186] Atkins escaped much of the destruction experienced by other planters, and his was apparently the only American company to maintain production throughout the war. One might expect Atkins, a

184. Reinaldo Funes Monzote, *From Rainforest to Cane Field in Cuba: An Environmental History Since 1492* (Chapel Hill, NC: University of North Carolina Press, 2008), 217-62.

185. César J. Ayala, *American Sugar Kingdom*, 74-120.

186. Rebekah E. Pite, "The Force of Food Life on the Atkins Family Sugar Plantation in Cienfuegos, Cuba, 1884–1900," *Massachusetts Historical Review* 5 (2003), 73-79.

conservative Bostonian, to have lobbied for US intervention in this war, as did many Americans who had an interest in lowering import tariffs on Cuban goods.[187] In fact, Atkins originally opposed American involvement, fearing that the Spanish would lose an important incentive for protecting his land. Although he had the ear of both the Cleveland and McKinley administrations, the interventionists in Congress eventually won out with the help of the explosion of the USS Maine, precipitating the four month Spanish-Cuban-American War. Always an opportunist, Atkins quickly became an enthusiastic supporter once the US invasion began. He welcomed the new integration with the US economy that came along with the occupation.[188] With US capital flooding into the island, sugar production began to explode.[189]

The Harvard-Atkins Partnership: The Business of "Improving" Cuba

Atkins saw this boom as an opportunity to invest in the scientific improvement of his business. Since first acquiring Soledad, Atkins had sought to maintain a modern *central*. Atkins' autobiography continuously returns to the theme of rationalizing the supposedly inefficient manufacturing, cultivation, and labor practices of Cuba. Atkins made sure Soledad was among the first mills on the island to implement new, state-of-the-art technologies for boiling and purifying cane juice. He swiftly converted to machinery that allowed the mill to use cane

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187. Louis A. Pérez, *Cuba and the United States: Ties of Singular Intimacy*, 17, 35.

188. The first US occupation of Cuba lasted from 1899 to 1902, but was followed by a second from 1906 to 1909.

189. Tariffs were not dropped immediately. Other US colonial (i.e., Hawaiian) and internal (beet growers in the northern states) sugar interests competed with those in Cuba. Thus, Atkins still found himself at odds with elements of Congress, though this time on the side of increased political intervention and economic integration. For more detail about the complicated causes and consequences of this sugar boom in Cuba, see César J. Ayala, *American Sugar Kingdom*, 200-21; Reinaldo Funes Monzote, *From Rainforest to Cane Field*, 217-62.

byproducts for fuel in a more economical manner.[190] By the start of the war, Atkins had also built twenty-two miles of railroad track in order to get the cane from the colonias to Soledad efficiently.[191] He personally kept a look out for new breakthroughs in the leading international sugar publications and he hired experts, such as the chemical engineer Wilfred Skaife, to improve the sugar manufacturing process. Atkins wrote to his wife, "All my time and attention is devoted to possible savings and improvements in work and arrangements for the future... Mr. Williams [Soledad's manager] devotes his attention to the mechanical improvements, Mr. S[k]aife to the chemical, and I to the business points."[192]

Atkins was not alone. A preoccupation with scientific improvement accompanied growing American influence over the Cuban economy. Americans imagined Cuba to be both scientifically and socially backward.[193] Cubans were supposedly incapable of innovation and unable to govern themselves. According to American popular opinion, innate Cuban racial deficiencies had been exacerbated by Spanish misrule and the overall laxity of Latin culture. Four hundred years under Spain had left the Cuban people weak and dependent. The *Chicago Tribune*, for example, claimed that "Spanish tyranny" had made Cuba "a nation with little science, less progress, no invention, and uninterrupted backwardness and widespread poverty."[194] In this discourse, scientific progress was tightly linked with the entrepreneurial spirit. Americans

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190. Benjamin Allen, *A Story of the Growth of e. Atkins & Co*, 23; Edwin F. Atkins, *Sixty Years in Cuba*, 109.

191. Ibid., 138.

192. Edwin F. Atkins to Katherine Atkins, 25 January 1895, cited in Ibid., 154.

193. On American perceptions of Cuban backwardness at this time see especially chapter 3 of Louis A. Pérez, *Cuba in the American Imagination*.

194. *Chicago Tribune*, 11 January 1899, cited in Ibid., 112.

asserted that Cuban reliance on slave labor had caused industrial innovation to stagnate—an argument that drew from a long-held ideology that had placed Southern slavery in opposition to Northern capitalism and technological progress and tied to discourses of tropical decadence. The Cuban planter's own "lavish" spending and "luxurious life" had brought foreclosure upon himself, argued one hagiographic account of E. Atkins and Company; the Cuban believed he "needed neither reserve capital nor the assistance of science, as his land and slaves had brought him all in the past that he could wish for."[195] This was also the dawn of the era of "scientific management": Since the 1850s the railroad companies had fostered a revolution in management and cost accounting, and by 1890 Frederick Taylor had performed his first time-and-motion study.[196] Industrial research laboratories were also on the rise.[197] American business leaders embarked on a quest for efficiency. They promoted the rationalization of industrial practice—often casting their efforts as the application of scientific principles. Men like Atkins were keen to export these advances abroad. Yankee work ethic and ingenuity would replace the old system in Cuba, steeped as it was in the inefficiencies of slavery.[198] American business, hand-in-hand with

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195. Benjamin Allen, *A Story of the Growth of e. Atkins & Co*, 15, 20.

196. Alfred D. Chandler, *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, MA: Belknap Press, 1977); Robert. Kanigel, *The One Best Way: Frederick Winslow Taylor and the Enigma of Efficiency* (New York, NY: Viking, 1997).

197. Investment in industrial research and the adoption of scientific management often went hand-in-hand. Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science* 17, no. 3 (1987): 479-518. See also Geoffrey C. Bowker, *Science on the Run: Information Management and Industrial Geophysics at Schlumberger, 1920-1940*, vol. Inside technology (Cambridge: MIT Press, 1994), 10-14.

198. Ironically, many of Atkins' methods of labor organization were in fact carried over from the days of slavery. Indeed, Atkins was very likely a slaveholder himself between his acquisition of Soledad in 1883 and the abolition of slavery in Cuba in 1886: Rebecca J. Scott, "A Cuban Connection: Edwin F. Atkins, Charles Francis Adams, Jr., and the Former Slaves of Soledad Plantation," *The Massachusetts Historical Review* 9 (2007): 7-34. See also Rebecca J. Scott, *Slave Emancipation in Cuba: The Transition to Free Labor, 1860-1899* (Pittsburgh, Pa.: University of Pittsburgh Press, 2000).

science, would make the island more productive than ever before—Cubans themselves were seen to have neither the capitalist drive nor the technical curiosity to develop their country to its full potential.

In reality, Cuban landowners and farmers were deeply concerned with implementing scientific agriculture—since the *centrales* controlled the most fertile land, smaller Cuban planters were strongly motivated to organize themselves and seek out more efficient practices.[199] And contrary to the idea of Cuban scientific backwardness, the island had long fostered a small but well-established scientific community.[200] Nevertheless, the myth persisted. Bringing scientific enlightenment became a central element of America's "civilizing mission" in Cuba.[201] American entrepreneurs like Atkins envisioned themselves as implementing progressive business practices abroad, bringing prosperity to themselves while uplifting backward peoples. American efforts to increase agricultural production, eradicate yellow fever, and enact other reforms in Cuba were cast not as undertakings motivated by self-interest, but rather as beneficent acts that justified the US occupation.[202] (This would of course continue under the Platt Amendment (1901-1934), as

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199. Leida Fernández has forcefully challenged the myth of Cuban agricultural stagnation. On the work of the *Círculo de Hacendados* and other home-grown Cuban efforts to scientifically improve agricultural production see: Leida Fernández Prieto, *Espacio de Poder: Ciencia y Agricultura en Cuba: El Círculo de Hacendados, 1878-1917* (Madrid: Consejo Superior de Investigaciones Científicas: Universidad de Sevilla: Diputación de Sevilla, 2008); Leida Fernández Prieto, *Cuba Agrícola: Mito y Tradición, 1878-1920*; Leida Fernández Prieto, "Azúcar y Ciencia en Cuba: 1878-1898," *Tzintzun* 31 (2000): 29-54.

200. Pedro Pruna, *Historia de la Ciencia y la Tecnología en Cuba* (La Habana: Editorial Científico Técnica, 2006); Pedro Pruna, *Ciencia y Científicos en Cuba Colonial: La Real Academia de Ciencias de la Habana, 1861-1898* (La Habana: Editorial Academia, 2001); Arxer Clark, Ismael, "138 Años de la Academia de Ciencias de Cuba: Visión de la Ciencia en el Proceso Histórico Cubano," (1999): 31; Pedro Pruna, "National Science in a Colonial Context: The Royal Academy of Sciences of Havana, 1861-1898," *Isis* 85, no. 3 (1994): 412-26.

201. Cuba was of course not unique in this regard. Americans applied this colonial logic to various projects, in the Caribbean, the Pacific, and, indeed, across the North American continent itself in relations with Native Americans. For example, see Michael Adas, *Dominance by Design: Technological Imperatives and America's Civilizing Mission* (Cambridge, MA: Belknap Press, 2006).

202. On the particular importance of the medical motivations of US imperialism in Cuba, see Mariola Espinosa,

the US maintained, and exercised, the right of military and economic intervention in the nominally independent nation.) Atkins enjoyed casting himself in this role.[203] In a variety of publications, from advertisements for sugar refining equipment to popular books like *Our Cuban Colony*, E. Atkins and Company would be held up as a model of a progressive American business implementing the fruits of science.[204]

Atkins' eye turned from mechanical improvements to the biological. Having read about the development of superior sugar cane through hybridization in the East Indies, Atkins was aware that botanical science could be applied to increase the productivity of Cuban cane. In 1899, with the war barely over, he turned to his Massachusetts connections. Perhaps Harvard could supply a botanist to add to his assembly of experts? Atkins contacted George Lincoln Goodale, director of the Harvard Botanic Garden and a strong proponent of economic botany at the university, to discuss the possibility of cooperation.

The partnership with Atkins was equally important to Goodale. At Harvard, there was an increasing interest in fostering applied science. Donors had long pushed Harvard to promote practical research and education, but with limited success.[205] The Harvard Bussey Institution's

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Epidemic Invasions.

203. In fact, Atkins' "improvement" agenda went beyond agriculture; the Atkins family also began a school for the children of Soledad workers. Education served an important role in colonial contexts and in projects to create disciplined labor forces. On education and US empire more broadly, see Part 3, "Education" in Alfred W. McCoy, and Francisco A. Scarano, *Colonial Crucible*.Solsiree del Moral, "Race, Science, and Nation: The Cultural Politics of Schools in Colonial Puerto Rico, 1917-1938" (Dissertation, The University of Wisconsin-Madison, 2006).

204. One such advertisement is from 1890, cited in Reinaldo Funes Monzote, *From Rainforest to Cane Field*, 193. Books celebrating Atkins' business and his scientific approach began to appear later, throughout the 1920s: Benjamin Allen, *A Story of the Growth of e. Atkins & Co*; Edwin F. Atkins, *Sixty Years in Cuba*; Leland Hamilton Jenks, *Our Cuban Colony, a Study in Sugar* (New York: Vanguard Press, 1928).

205. The teaching of the "applied" or "practical" sciences at Harvard went back at least to 1847 with the founding of the Lawrence Scientific School for "the acquisition, illustration, and dissemination of the practical sciences forever." Implementing this mission proved elusive, however. The existing literature on the dynamics of pure and applied science at Harvard tends to focus on the engineering and physical sciences, rather than biology and

undergraduate school of agriculture foundered, for example, encountering difficulty competing with the State colleges and the associated USDA experiment stations.[206] Indeed, in its institutional structure and academic culture, the university remained firmly committed to the ideal of "pure science." Goodale, however, had a deep interest in economic botany. He considered the study and development of the world's plant resources to be imperative for the future security and prosperity of mankind.[207] Goodale's efforts were key to cementing the place of the "new botany" (see Chapter 1) at Harvard, and he worked tirelessly to connect this work with the world of applied botany. He communicated with horticulturalists outside of academia, and brought economic botany into the Harvard Botanical Museum and the classroom.[208] At Harvard and through his position as president of the American Association for the Advancement of Science, he argued for the coordination of efforts at experiment stations to produce new economic plant varieties (and possibly even new species), as well as for the exploration of the world for new economic plants to introduce into the US.[209] Botanists, he argued, should use their

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agriculture. See: Mary Ann James, "Engineering an Environment for Change: Bigelow, Peirce, and Early Nineteenth-Century Practical Education At Harvard," Bruce Sinclair, "Harvard, MIT, and the Ideal Technical Education," and I. Bernard Cohen, "Howard H. Aiken, Harvard University, and IBM: Cooperation and Conflict," in *Science at Harvard University: Historical Perspectives*, ed. Clark A. Elliott, and Margaret W. Rossiter (Bethlehem, PA: Lehigh University Press Associated University Press, 1992).

206. Karl Sax, "The Bussey Institution," *Arnoldia: The Magazine of the Arnold Arboretum* 7, no. 3 (1947): 13-16; Clark A. Elliott, and Margaret W. Rossiter, eds. *Science at Harvard University: Historical Perspectives* (Bethlehem, PA: Lehigh University Press, 1992); Samuel Eliot Morison, *Three Centuries of Harvard, 1636-1936* (Cambridge, MA: Harvard University Press, 1936).

207. Oakes Ames, "George Lincoln Goodale (1839-1923)," *Proceedings of the American Academy of Arts and Sciences* 59, no. 17 (1925): 640-44.

208. For example, he published a series on plant physiology in the first American journal aimed at horticulturalists, landscape gardeners, and foresters: George Lincoln Goodale, "Principles of Physiological Botany as Applied to Horticulture and Forestry," *Garden and Forest* 2 (1889): 8-9, 20-21, 32-33, 44-45, 56, 68-69, 80-81, 92, 104-105, 116-117,128-129, 140-141, 153, 164-165, 177-178, 188, 201-202, 213-214, 225, 249-250.

209. George Lincoln Goodale, "Useful Plants of the Future: Some of the Possibilities of Economic Botany," *American Journal of Science* 42 (1891).

knowledge about plant physiology and evolution in order to diversify human diets and make agriculture more productive. Goodale saw pure science as the wellspring of new practical applications; "The history of science has shown over and over again," he wrote "that the results of pure, scientific research are, sooner or later, likely to be turned to the highest practical account."[210] Nevertheless, if researchers did not actively reach out to the applied fields, innovation might languish in the academy.

Apparently, Atkins' meeting with Goodale was encouraging. He decided to donate $2,000 towards a traveling fellowship in economic botany and $500 for the preparation of a bibliography of publications on sugar cane.[211] If successful, he intimated the possibility of more money in the future. The fellowship supported Goodale's recently graduated PhD student, Edwin Mead Wilcox, to "visit certain stations in eastern tropics, where experiments in the improvement of economic (tropical) plants, especially the sugar-cane, are now in progress."[212] This included a six-month stay at the laboratory at Buitenzorg, Java, which, as we saw in the previous chapter, had captivated so many other American botanists. There was an understanding that Wilcox would return from his fellowship to prosecute his own cane researches at Soledad, transferring what he had learned about cane hybridization in the Dutch colony into the Cuban context.

But when Wilcox got married shortly before embarking on the journey, Goodale was devastated. Goodale was immensely concerned with establishing lasting relations with Atkins,

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210. George Lincoln Goodale, "Principles of Physiological Botany as Applied to Horticulture and Forestry," 250.

211. Harvard University, *Reports of the President and the Treasurer of Harvard College, 1898-1899* (Cambridge, MA: Harvard University, 1900), 242-43.

212. Ibid.; K. W. Dammerman, "A History of the Visitors' Laboratory (Treub Laboratorium) of the Botanic Gardens, Buitenzorg, 1884-1934," 67.

and Atkins had him warned against choosing a man with "marital entanglements" for the job. Goodale became fearful of losing the businessman's favor; Wilcox's trip might now look like little more than a "honeymoon at [Atkins'] expense" rather than a serious scientific endeavor.[213] Wilcox was extremely enthusiastic about the project and was by all accounts a skilled worker in the Botanical Laboratory at Harvard and a promising agricultural botanist. Nevertheless, upon return from the East Indies, he suddenly turned down the position at Soledad.[214]

In mid-December 1899, Goodale decided to make a personal visit to Atkins at Soledad, bringing along Oakes Ames instead of Wilcox. Ames had graduated from Harvard the previous year and Goodale had just appointed him as his new Assistant Director at the Botanic Garden. He was then only twenty-six years old. Goodale was not at all sure that they could produce real results for Atkins, possessing as they did such meager knowledge of the climate and the particularities of sugar cultivation in Cuba. Ames was becoming well-known for his skills as a hybridizer, but these had mainly been applied to orchids. Goodale and Ames hardly fit the image of the confident scientific experts that Atkins must have expected. On their arrival in Cuba, they roomed at the elegant Passaje Hotel on the Prado, the central boulevard of old Havana. At dinner, Goodale shocked Ames by ordering champagne, "so that we might drink to the success

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213. Oakes Ames described Goodale as being so upset that he told him the news "almost with tears in his eyes." Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA. (Citations marked "HUA" appear courtesy of the Harvard University Archives.) See also the longer draft of this manuscript in Folder "Correspondence of Oakes Ames," Box 2, Atkins Institution Papers, AAA.

214. A somewhat cryptic letter suggests that Goodale either rescinded the offer or strongly urged Wilcox to decline: Goodale? (unsigned) to Wilcox, 1899? (undated). Box 1, Folder "1899-1900 Provision for Sub-Tropical Agricultural Experiment Station, Cuba," Atkins Institution Papers, AAA. In any case, Wilcox went on to a successful career, working at experiment stations in Alabama and Nebraska, and eventually became director of the Santo Domingo Experiment Station in the Dominican Republic. His career trajectory is one of many examples of how experience in one part of the tropical world was easily transferable to a career elsewhere in the American sphere of influence in the tropics.

of our quixotic adventure." "Surely," Ames later recalled, "rarely have two less capable men run their necks into the noose of fate. We needed, something more than champagne."[215] Still, Goodale hoped to patch things up with Atkins and planned to urge him to support an even bigger plan: the establishment of a sugar cane experiment station at Soledad.

Cuba as yet had no agricultural experiment station of its own. In the US, the USDA experiment stations, initiated with the 1887 Hatch act, were credited with helping to make American agriculture efficient and productive through the application of science.[216] According to B. T. Galloway, director of the USDA Office of Plant Industry, "no one thing has given a greater stimulus to applied botany."[217] The USDA soon began plans to set up experiment stations in the new territories acquired in 1898, with both the annexation of Hawaii and end of the Spanish-American War.[218] The European empires likewise began to look to the USDA experiment stations as a model for revitalizing colonial agricultural research.[219] Because the Teller

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215. Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA.

216. On the history of the USDA experiment stations and the scientific ideal in American agriculture, see Charles E. Rosenberg, "Science, Technology, and Economic Growth: The Case of the Agricultural Experiment Station Scientist, 1875-1914."; Charles E. Rosenberg, "Rationalization and Reality in the Shaping of American Agricultural Research, 1875-1914," *Social Studies of Science* 7, no. 4 (1977): 401-22; Margaret W. Rossiter, *The Emergence of Agricultural Science: Justus Liebig and the Americans, 1840-1880* (New Haven, CT: Yale University Press, 1975); David B. Danbom, "The Agricultural Experiment Station and Professionalization: Scientists' Goals for Agriculture," *Agricultural History* 60, no. 2 (1986), 250; Lou Ferleger, "Uplifting American Agriculture: Experiment Station Scientists and the Office of Experiment Stations in the Early Years After the Hatch Act," *Agricultural History* 64 (1990): 5-23; Deborah Fitzgerald, *The Business of Breeding: Hybrid Corn in Illinois, 1890-1940* (Ithaca, NY: Cornell University Press, 1990); Deborah Fitzgerald, *Every Farm a Factory: The Industrial Ideal in American Agriculture* (New Haven, CT: Yale University Press, 2003).

217. Beverly Thomas Galloway, "Applied Botany, Retrospective and Prospective," *Science* 16, no. 393 (1902), 50.

218. Congress finally approved funding for USDA stations in Hawaii and Puerto Rico in 1900, with stations coming later in Guam (1908) and the Virgin Islands (1917). See Richard A. Overfield, "Science Follows the Flag."; Richard A. Overfield, "The Agricultural Experiment Station and Americanization."

219. Though the European empires had of course long supported botanical gardens where acclimatization studies were done, the US model was frequently cited in the twentieth century by those who sought more extensive public funding of science in the colonies, and the revitalization of aging and neglected botanical gardens. Richard Harry

Amendment promised no annexation of Cuba, however, the USDA would not be the one to take the lead there. Goodale and Ames managed to persuade Atkins that Soledad had the potential become a center of agricultural innovation in Cuba. The idea appealed to Atkins, though success was not guaranteed. Nevertheless, the result of their meeting was the establishment of the first experiment station in Cuba—a fact that Atkins was proud to point out. Although the institutional ties with Harvard remained informal, Atkins dubbed it the "Harvard Botanic Station for Tropical Research and Sugar-cane Investigation." For Atkins, it was a wise investment. Whether the cane improvements panned out or not, by associating his name with Harvard, Atkins gained the prestige of philanthropy. The collaboration cemented his business' modern, scientific image, and boosted his standing on the Boston social scene.[220] As one promotional account of E. Atkins & Company boasted, "This work, which in other countries, was supported financially by governments and associations, was undertaken single handed by Mr. Atkins in Cuba."[221]

Global Connections, Local Transformations: Sugar Cane Research at the Harvard Station

For several years, however, the "Harvard Station" was neither Harvard's, nor much of a station. The project began simply, as an eleven acre experimental plot on Colonia Limones—a former estate of the Vilá family which Atkins incorporated into Soledad in the 1880s. A little less than twenty miles east of the city of Cienfuegos, the site was a field of rolling hills, "where a single

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Drayton, *Nature's Government*, 247.

220. On the social motivations of support to the applied sciences at Harvard by "self-made" businessmen, see Margaret W. Rossiter, "Philanthropy, Structure, and Personality: Or, the Interplay of Outside Money and Inside Influence," in *Science at Harvard University: Historical Perspectives*, ed. Clark A. Elliott, and Margaret W. Rossiter (Bethlehem, PA: Lehigh University Press Associated University Press, 1992), 15.

221. Benjamin Allen, *A Story of the Growth of e. Atkins & Co*, 24.

Royal Palm and a Ceiba tree held sway."[222] It was an arid spot, though water could be supplied by the small brook that snaked through a central ravine. A shade house was soon built to protect tender plants from the baking sun. By 1902, the first small greenhouse was added to the existing experimental plot to protect seedlings from desiccating winds and the cooler winter weather that occasionally descends on western Cuba.[223] According to the station's first horticulturalist, the location was "picturesque," but "rather barren."[224] The soil, though varied, was primarily a shallow, hard baked, red clay, underlain by a porous subsoil of *cascajo*—a weathered limestone gravel. (Appropriately, *cascajo* means "piece of junk" or "old wreck" in colloquial Spanish.)[225] Atkins had hedged his bets. The land was worth little to him for large-scale cane production, but as an experiment station it had the potential to produce big results.

The station's goal was to cultivate a variety of cane superior in disease resistance and yield to the variety 'Cristalina', which dominated the Cuba's countryside.[226] Through the nineteenth century, planters had believed that the practice of propagating cane asexually from cuttings caused the cane to degenerate over time, leading to the plant's sterility and an increased susceptibility to disease.[227] In fact, climatic factors can stunt the fertility of cane flowers, but

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222. Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA.

223. Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926* (Cambridge, MA: Harvard University Press, 1927), 3.

224. Ibid.

225. Hugh H. Bennett, and Robert Verrill Allison, *The Soils of Cuba* (Washington, DC: Tropical Plant Research Foundation, 1928), 138-139, 361.

226. The variety name is sometimes spelled 'Crystalina'. In addition to the station's own records, I have drawn much of the information in this paragraph from the work of Stuart McCook. For more on cane research at the Harvard station and agricultural extension in Cuba and Puerto Rico more generally, see Stuart McCook, *States of Nature*, 47-76.

227. In fact, loss of productivity over time was more likely due to soil exhaustion; sugar monocropping is notorious

under the right conditions some individual plants can flower and be made to set viable seed. Workers at the Dutch East Java Research Station (*Proefstation Oost Java*) and the Dodds Botanic Station in British Barbados were finally able to prove in 1886 that sugar cane could reproduce sexually and be hybridized to produce even more fertile and productive plants.[228] The new hybrid cane varieties they produced in subsequent years, however, either did not thrive when imported into Cuba or failed to out-produce the high sugar content of 'Cristalina.' Though there was no guarantee, Atkins, Goodale, and Ames hoped that the Java and Barbados results could be repeated at the Harvard station, allowing them to breed a hybrid cane that could surpass 'Cristalina' under the conditions at Soledad. If they were successful, they hoped they might even be able to breed a variety that could re-seed itself and become naturalized in the Cuban landscape.

The transformation of cane at Soledad depended on its place in a global botanical network. Through personal and professional contacts with the heads of botanical gardens and agricultural departments around the world, Ames and Goodale worked to gather the raw material needed for the hybridization work. From Java, Mexico, Australia, Hawaii, Barbados, and Jamaica came cane varieties to be crossed at Soledad.[229] Perhaps surprisingly given the economic potential of improved Cuban cane, the Harvard station's efforts to acquire cane varieties were

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for its depletion of the land. See especially: Reinaldo Funes Monzote, *From Rainforest to Cane Field*; Richard P. Tucker, *Insatiable Appetite*, 7-42.

228. On these programs, see: J. H. Galloway, "Botany in the Service of Empire: The Barbados Cane-Breeding Program and the Revival of the Caribbean Sugar Industry, 1880s-1930s," *Annals of the Association of American Geographers* 86, no. 4 (1996): 682-706; J. H. Galloway, *The Sugar Cane Industry: An Historical Geography From Its Origins to 1914* (Cambridge: Cambridge University Press, 1989); Wim Van der Schoor, "Pure Science and Colonial Agriculture: The Case of the Private Java Sugar Experiment Stations (1885-1940)," in *Les Sciences Hors D'Occident au XX Siècle*, ed. Yvon Chatelin, and Christophe Bonneuil (Paris: IRD, 1995).

229. See Goodale's correspondence in "Goodale," Box 2, Atkins Institution Papers, AAA.

characterized more by inter-imperial cooperation than by competition. International friendships and a broader botanical "gift economy" facilitated exchange.[230] Though the history of economic botany is full of tales of intrigue—from smuggling cinchona and rubber seeds in the nineteenth century to modern-day bioprospecting—sugar cane cuttings circulated remarkably freely in the early twentieth-century tropical world.[231] Moreover, while the USDA Bureau of Plant Industry would become increasingly involved in facilitating plant exchange over the coming decades, exchange generally did not have to pass through any metropolitan clearinghouse in the US. Thus, the station at Soledad was situated in a very different sort of network from that of botany in the British empire, where exchange among a constellation of colonial gardens was orchestrated from Kew Gardens.[232] The US had multiple botanical centers—the National Museum of Natural History in Washington DC, the New York Botanical Garden, Harvard, the Missouri Botanical Garden in St. Louis. As often as not, however, the exchange of plants and seeds occurred across colonial institutions. In 1905, for example, the Head Gardener of the Harvard Botanical Garden, Robert Cameron, traveled to the botanical gardens, experiment stations, plantations (sugar, cocoa, nutmeg, and banana), and logwood groves of Trinidad, Grenada, and Jamaica, bringing cane varieties and other economic plants from the British colonies to the Harvard station in

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230. On the role of gift exchange (versus commodity transactions) and reciprocity in biomedical science, see Warwick Anderson, "The Possession of Kuru: Medical Science and Biocolonial Exchange," *Comparative Studies in Society and History* 42, no. 4 (2000): 713-44.

231. Lucile Brockway, *Science and Colonial Expansion*; John Merson, "Bio-Prospecting or Bio-Piracy: Intellectual Property Rights and Biodiversity in a Colonial and Postcolonial Context," *Osiris* 15 (2000): 282-96; Londa Schiebinger, and Claudia Swan, *Colonial Botany*; Abena Dove Osseo-Asare, "Bioprospecting and Resistance: Transforming Poisoned Arrows Into Strophantin Pills in Colonial Gold Coast, 1885-1922," *Social History of Medicine* 21, no. 2 (2008): 269-90; Gabriela Soto Laveaga, *Jungle Laboratories: Mexican Peasants, National Projects, and the Making of the Pill* (Durham, NC: Duke University Press, 2009).

232. Lucile Brockway, *Science and Colonial Expansion*; Richard Harry Drayton, *Nature's Government*.

Cuba.[233] Regional interconnections could be as significant as those between colony and metropole.[234]

Ames and Goodale oversaw work at the Harvard station largely from afar, visiting periodically when their duties in Cambridge allowed. For the day-to-day work the hybridization project, Ames engaged his personal horticulturist, Robert Grey. Grey would remain in Cuba for more than thirty years. For the first several years of the station, his efforts were managed more directly by Atkins. He, after all, paid most of Grey's salary and the station's total expenses out of pocket. Grey was not a man of science, but rather a skilled gardener, following in the trade of his own father. He had in the past also worked as a natural history collector-for-hire, which had included some experience collecting in Latin America. He had a fondness for the region, and in fact petitioned Ames for the Cuba job. Ames remarked on how highly Atkins thought of Grey. Atkins enjoyed being "able to go among his gardens with a man who had a name for everything and who seemed to exude the capacity possessed by experts"—even if the Harvard men did not count him among the ranks of professional scientists.[235] Nevertheless, Grey still encountered resistance from some of Atkins' employees who distrusted "Yankees" and university men. Ames in fact suspected one of Atkins' men of sabotaging Grey's first seedlings, an incident that resulted in the hiring of an assistant for Grey, Hugo Bohnhoff.[236] Still, while Goodale and Ames

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233. "Reports." Box Q-Z, Records of the Atkins Garden and Research Laboratory, 1898-1946 UAV 231.5, HUA.

234. Galloway, focusing on the Barbados case, has also commented on the large degree of cooperation in sugar research among Caribbean colonies. J. H. Galloway, "Botany in the Service of Empire: The Barbados Cane-Breeding Program and the Revival of the Caribbean Sugar Industry, 1880s-1930s," 685.

235. Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA.

236. Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA.

(who took over the Harvard side of the cooperation completely in 1909 after Goodale's retirement) gave suggestions and facilitated the importation of plants, the station was relatively autonomous. Grey reported informally to both Atkins and the Harvard men, but generally proceeded as he saw fit.[237]

The work of cane hybridization was slow and seasonal. Most of the of the activity occurred during the winter, when the cane flowers bloomed. To coax the cane into greater fertility, Grey first had to find plants that were in flower from among the specimens imported to the station from abroad and amid those growing on Atkins' plantations. He then worked to find plants that showed a greater tendency to open their anthers (the part of the stamen that holds the pollen), which in sugar cane were usually held tightly shut. Grey had to accomplish this laborious work of identifying promising individual variations, and then extracting their pollen, under under a microscope. Next, he fertilized the cane by hand, usually crossing the locally well-adapted 'Cristalina' with some foreign variety with a desired trait. He enclosed each fertilized panicle (the branched cluster of flowers that emerges from the top of the cane) with gauze to isolate it from the unwanted pollen of other varieties. After three to six weeks, he took the ripe seed heads that appeared and sowed them into sterilized containers. These had to be protected from the harsh sun and wind, and raised up off the ground to protect the seedlings from troublesome leaf-cutter ants, locally called *bibijaguas*. The first viable seedling took over two thousand crosses to produce. Even then, they still had to undergo field trials and, if they thrived,

237. Goodale had begun the practice, despite the lack of formal affiliation, of including an account of the progress of the "Harvard Botanical Station in Cuba" in his letter to Harvard's president on the Botanic Garden in the university's annual reports. Ames continued this practice when he became director of the Botanic Garden.

undergo chemical analysis to determine if their sugar content was high enough to merit further study.[238]

In some years, unusually cool weather (1904-1905) or disruptive hurricanes (1911) meant little progress could be made at all. Knowing the cane experiments would be a "long uphill pull," Ames wrote, "I shall make an effort to render the Garden a thing of beauty in the meanwhile."[239] He donated many plants from his own greenhouses, as well as selections from the Harvard Botanical Garden, and began to design a true botanical garden alongside the experimental plots. Ames directed Grey, when not occupied by sugar work, to focus on developing the beauty and breadth of the station's garden. In his spare time during visits, Ames also collected specimens of rare plants in the nearby Trinidad Mountains. His letters to his wife during these trips reveal his thoughts about the relation of his rather esoteric interests in the systematics and evolution of orchids to the serious work of economic botany. His own research "seems so impractical! ...I rummage the woods, carving trails with a machete, go into ecstasies over a plant or two."[240] He compared himself unfavorably to "the pioneers of botanical science" who "had in view the alleviating of human ills, and worked seriously to discover new drugs."[241] At Soledad, however,

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238. "Reports." Box Q-Z, Records of the Atkins Garden and Research Laboratory, 1898-1946 UAV 231.5, HUA; Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926*, 4-9.

239. Oakes Ames, and Pauline Ames Plimpton, *Oakes Ames, Jottings of a Harvard Botanist, 1874-1950* (Cambridge, MA: Botanical Museum of Harvard University, distributed by Harvard University Press, 1979), 145-46.

240. Ames to Blanch Ames, ADD

241. Ames would later become a central figure in economic botany, teaching Harvard's course on the subject for many years, after Goodale's retirement. Indeed, he was one of the first botanists to recognize the economic potential of tropical plants' biochemical properties, which are taken for granted as the primary target of modern-day bioprospecting efforts. He also inspired his student Richard Schultes, a foundational figure in modern ethnobotany. Ames to Blanch Ames, ADD

he perhaps had a chance at making a lasting contribution. At times he indulged in furtive hopes that it might become one of the world's great tropical botanical gardens, and home to research that would improve tropical agricultural productivity. He wrote home, "I am sure I could make my everlasting reputation if I could remain here for a few years and give myself up to the work I can see ought to be and should be done."[242] Ames was devoted to the station, and able to make annual visits, but work at the Harvard Botanic Garden, where he became director in 1909, the Botanic Museum, and, after 1926, his Chairmanship of the Division of Biology took up much of his time.

When sugar work fell into a lull, Grey relied on his instincts as a plant breeder and experimented with improvements to a wide variety of other species. He introduced temperate zone vegetable crops, like potatoes, tomatoes, and cabbage, selecting them for hardiness in the Cuban climate. He also rambled in the remnant forests and abandoned fields nearby, searching for botanical specimens to send back to Ames, as well as for signs as to what kinds of plants might acclimatize well. For example, the discovery of isolated pockets of naturalized cotton—escapees from pre-war days—fed hope that the climate might be suitable for this economic plant.[243] Over time, the station continued to acquire a wide variety of economic and ornamental plants through international exchange, from mangos and rubber to roses and *Victoria regia*.[244]

242. Ames to Blanch Ames, ADD

243. It turned out that in monocrop plantations, cotton could not survive predation by boll-weevil in Cuba. Grey was unaware of the recent agricultural and environmental history of the area, which included the failure of the cotton industry in the nineteenth century. "Reports." Box Q-Z, Records of the Atkins Garden and Research Laboratory, 1898-1946 UAV 231.5, HUA; Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926*, 11.

244. On the symbolic value of ornamental plants like *Victoria regia*, see Nancy Stepan, *Picturing Tropical Nature*, 33.

Mrs. Katherine Atkins, Edwin's wife, was particularly interested and involved in the acquisition of new additions to the gardens. Atkins authorized the opening of a new section of land for the cultivation of fruit trees in January of 1902, beginning a long process of expansion over the next few decades. By the mid-1930s the station would encompass more than 300 acres. In 1903 the station's garden contained 400 species belonging to 243 genera of plants.[245] By 1933 it would have 1,970 species from 921genera.[246]

Grey's hard work began to pay off. By 1904 he had produced the first seedling cane in Cuba, and by 1908 crossing had produced cane varieties that were reliably fertile and easily produced seeds. Tens of thousands were eventually given field trials, and a few hundred were retained for seed and further hybridization. While the sugar content of 'Cristalina' was difficult to beat, many of the so-called "Harvard canes" showed better drought and disease tolerance or suitability for poorer soils. The variety 'Harvard No. 12,029' showed particular promise. As Stuart McCook has noted, however, the Harvard station was at its heart a North American institution.[247] It made relatively little effort to actually distribute cane to Cuban growers, though some results were printed in Spanish in the Cuban government's *Boletin Oficial*. Because Grey was not a professional scientist, and was busy with other tasks, the early work of the station was not published in scientific journals and received relatively little publicity in either Spanish or English. The station did not engage in traditional extension work, though Grey did proudly show

245. Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926*, 10.

246. Harvard University, *Report of the President of Harvard College and the Reports of Departments for 1932-33*, vol. 31(3), Official Register of Harvard University (Cambridge, MA: Harvard University, 1934), 260.

247. Stuart McCook, *States of Nature*, 60.

30 Harvard seedlings at the Second Cuban National Exposition in Havana in 1912. Harvard cane did eventually wind up back in the networks of botanical exchange which had helped create it, entering into breeding work, for example, at the Louisiana Sugar Experiment Station.[248] Only by the mid-1920s would cooperation with the Tropical Plant Research Foundation's new station at Baraguá, Cuba, help with the distribution of Harvard cane on the island and in Central America.[249] Harvard cane led no "varietal revolution" in Cuba, however. In Puerto Rico and parts of the British Caribbean in the early twentieth century pressure from disease encouraged the wholesale replacement of failing varieties with new breeds. In Cuba, however, no such crisis precipitated a rapid shift from 'Cristalina'. Indeed, the existing technological and labor system made any movement to a new variety extremely difficult.[250] Still, Harvard varieties found their way into some marginal Cuban croplands, where they could outcompete 'Cristalina' on exhausted soil.[251]

Thus, over its first two decades, the Harvard station met its founder's basic goals, and began to expand. It did produce useful hybrid cane, although this cane never outcompeted 'Cristalina'. Perhaps more importantly, though, the station grew to have other purposes and meanings, as well. For Ames and Goodale it fulfilled a need to do "practical" work. Cooperation

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248. Beverly Thomas Galloway, "Seeds and Plants Imported During the Period From April 1 to June 30, 1909," *USDA Bureau of Plant Industry Bulletin* 168 (1909); Hamilton P. Agee, "Progress in the Propagation of Seedlings of Sugar Cane in Louisiana," *American Breeders Magazine* 1, no. 4 (1910): 269-73.

249. Thomas Barbour, "Cane Breeding at the Harvard Cuban Station," *Harvard Alumni Bulletin* 30, no. 31 (1928): 925-26. On the Tropical Plant Research Foundation, see Stuart McCook, "”'the World Was My Garden'”," 505-06.

250. Stuart McCook, *States of Nature*, 98-102; J. H. Galloway, "Botany in the Service of Empire: The Barbados Cane-Breeding Program and the Revival of the Caribbean Sugar Industry, 1880s-1930s," 699; J. H. Galloway, *The Sugar Cane Industry: An Historical Geography From Its Origins to 1914*, 141-42.

251. Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926*.

with Atkins was for Goodale part of a larger effort to transfer the skills and knowledge of university botany into the realm of applied horticulture. Ames, taking Goodale's view of the higher purpose of economic botany to heart, saw the station at Soledad as an outlet for his genuine desire to create something with enduring practical value. For Atkins, his philanthropy marked him as a "scientific" and cultured businessman. Even if an alternative to 'Cristalina' did not emerge, Atkins gained from the prestige of association with Harvard. The station also served an important aesthetic purpose. The Atkins family played host to an impressive array of international visitors—particularly US and Cuban politicians and business leaders—and a tour of the botanical and experimental gardens was the centerpiece of a stay at Soledad.[252] For both the businessman and the university men, there was finally a "higher purpose" of benefiting Cuba and tropical agriculture. "By selection, crossing, and hybridizing, and by judicious adaptation of plants to special soils," Goodale wrote, "the Station undertakes, in a modest way, to lend a hand towards the restoration of prosperity to some of the districts which have felt the effects of neglect of agriculture through civil war."[253] Thus, in spite of its contribution to a system of dependency on sugar export and US capital, the station's work was cast as for the good of the Cuban people, not merely the profit of one sugar company. These hopes were at once sincere and self-promoting—raising their own prestige, and implicitly endorsing the supposed beneficence of US hegemony in Cuba.

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252. Dan Hazen also suggests that the Atkins, and later the Claflin, family took advantage of the tax benefits of making donations of money and additional land to the Atkins Institution. Dan Hazen, "Cienfuegos Botanical Garden: Harvard's Legacy, Cuba's Challenge," *Harvard University Gazette* (1999).

253. George Lincoln Goodale, "Horticultural Experiments and Botanical Investigations at the Harvard Station in Cuba," *American Journal of Science* 13, no. 76 (1902), 326.

Endowing Tropical Research

Over time, the station's botanical garden grew in importance—so much so that the "Cuban Garden" began to be used interchangeably with the Harvard "Station in Cuba" in correspondence. Meanwhile, the station's experimental plantations came to include nearly every major tropical economic plant, and Grey crossed varieties of corn, citrus, mango, annona and many others. However, while the station quickly moved beyond a focus on the production improved sugar cane, it was only in the 1920s that it became a fully-fledged tropical research station with a biological laboratory, housing for visiting researchers, formal administration at Harvard, and an endowment. Whereas the station at Soledad had begun by concentrating on the agricultural problems of a single sugar company, it grew to become a site of basic tropical biological research. This shift in mission largely came down to the growing involvement of a single person: Thomas Barbour, a Harvard herpetologist best known for his years as Director of the Museum of Comparative Zoology (MCZ). Barbour would not become the official director, or "Custodian," of the Harvard station until 1927, but he played a key role in turning Soledad into a destination for researchers from Harvard and other American universities. Ultimately, what would otherwise have remained a relatively isolated agricultural station became, as we will see in the next chapter, one element in a much larger effort by Barbour to lay institutional foundations for tropical biology in the Americas.

In order to understand Barbour's role in the Harvard Station's development, it is important to first understand what brought him to Cuba. The son of a wealthy Irish-American linen manufacturer, Barbour's early interest in nature emerged during travels with his father among Europe's great natural history museums and in the woods at his family's vacation home in the Adirondacks. His particular interest in the tropics went back to 1898, when at the age of fourteen he suffered an attack of typhoid fever. His parents sent him to convalesce at his grandmother's winter home in Florida. With her, he traveled to South Florida and to Nassau, in the Bahamas. The landscape and wildlife captivated him. Often of ill health, Barbour continued to travel south throughout adulthood to escape harsh New England winters. Reversing the stereotype of the unhealthy tropics, he would often joke about "hellish climate" at Cambridge and his desire to spend as much time in the sunny tropics as his Harvard duties would allow.[254]

Following his college years, his travels in the tropics continued. His specialty was in the taxonomy and the biogeography of reptiles, though he had a remarkably broad interests in the natural history of many groups of animals and plants. Inspired by reading Alfred Russel Wallace's classic *The Maylay Archipelago* as a teenager, Barbour was fascinated by the question of how geological and evolutionary change combined to create the patterns in the distribution of life that we see today. Upon completing his A.B. degree at Harvard in 1906, Barbour married Rosamond Pierce, and the two embarked on a trip throughout tropical Asia to retrace Wallace's steps.[255] He and his wife, aided by numerous local collectors, gathered and meticulously recorded the distribution of hundreds specimens of reptiles and amphibians, as well as some mammals, birds, fish, and insects. The collection went to the MCZ and was the basis for his 1910 doctoral

254. Barbour to Alexander Wetmore, 13 January 1939. RU7006, Box 4, Folder 3, Smithsonian Institution Archives.

255. They even encountered the Malay man Ali "Wallace" who had been employed as Wallace's collecting assistant. Thomas Barbour, *Naturalist at Large*, An Atlantic Monthly Press Book (Boston, MA: Boston, Little, Brown and Company, 1943), 43. On Ali Wallace and Alfred Russel Wallace's relationship, see Jane R. Camerini, "Wallace in the Field," *Osiris* 11: Science in the Field (1996): 44-65. For an account of the expedition, see Thomas Barbour, and Rosamond Barbour, *Letters Written While on a Collecting Trip in the East Indies* (Paterson, NJ: Chas. A. Shriner, 1913).

thesis.[256] In it, Barbour complicated Wallace's assertion of a single precise boundary "line" between Asian and Australian-type faunae, but supported the overall biogeographical division.[257] He hoped to apply the same methods in the Caribbean. The primary question driving this work was whether the flora and fauna of the West Indies had once had once extended across a single lost landmass, "Antillea," that had once connected the islands (as Wallace had speculated) or whether the present-day species descended from animals that had dispersed from the adjacent North and South American mainlands.[258] The answer to this question impinged on broader problems of the relative importance of isolation versus large contiguous populations in the production of new species. Thus, while Barbour acted in many ways as an old school naturalist, he firmly defended his collecting work. It was not a mere cataloguing of species, but rather a contribution to major theoretical debates in evolutionary biology.

To solve the puzzle of "Antillea," Barbour carried out fieldwork throughout southern Mexico, Central America, the Caribbean, and parts of South America during the 1900s and 1910s. His skill with the Spanish language, which he had learned in childhood, made him the perfect candidate for a variety of international liaisons. He served in 1908 as a delegate to the first Pan American Congress in Santiago, Chile, and in 1910 he represented the Association of American Universities at the reopening of the University of Mexico. In that same year he was appointed Associate Curator of Reptiles and Amphibians at the MCZ, where he had worked as a

256. It was published two years later: Thomas Barbour, "A Contribution to the Zoögeography of the East Indian Islands," *Memoirs of the Museum of Comparative Zoölogy at Harvard College* 44, no. 1 (1912): 1-203.

257. An engaging account of Wallace's line can be found in David Quammen, *The Song of the Dodo* (New York, NY: Scribner, 1996).

258. For a more detailed discussion of Barbour's biogeographical work, especially his theoretical disagreement with William Diller Matthew, see Mary P. Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum* (Chicago, IL: University of Chicago Press, 1991), 245-66.

volunteer since 1902. He would work his way up over the years, finally becoming Director of the museum in 1927.

Barbour first visited Soledad in 1908. It was his second collecting trip to Cuba, a particularly important island from a biogeographical standpoint because of its large size and sharp faunal differences from neighboring Jamaica.[259] There, Goodale introduced him to Atkins, who arranged for his lodging at one of the nearby *colonias*. Barbour used the spot as a base for collecting in the surrounding area. From that point on, Soledad became a regular destination during his annual travels in the Caribbean. Barbour became closer friends with the Atkins family as the years passed. On future visits to Cuba, he stayed with the family at their residence, the *vivienda*, at Soledad. As time passed, Atkins also met with Barbour at his offices at the MCZ in Cambridge, to discuss the affairs of the station, Cuban politics, and the role of the university in the problems of the tropics.[260] With his imposing stature (6 feet 5 inches tall and reaching nearly 300 pounds), gregarious personality, and talent as a conversationalist, he made a strong impression on Atkins.

It was only during World War I, when Barbour served as a special agent of the State Department providing intelligence on Cuba, that his involvement in the Soledad station began to solidify. Barbour lived in Cuba for most of 1917 and 1918, and into 1919, reporting on the

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259. For example, see Thomas Barbour, "Notes on the Herpetology of Jamaica," *Bulletin of the Museum of Comparative Zoology at Harvard College* 52 (1910): 271-301; Thomas Barbour, "A Contribution to the Zoögeography of the West Indies, With Especial Reference to Amphibians and Reptiles," *Memoirs of the Museum of Comparative Zoölogy at Harvard College* 44, no. 2 (1914): 209-359; Thomas Barbour, and C. T. Ramsden, "The Herpetology of Cuba," *Memoirs of the Museum of Comparative Zoölogy at Harvard College* 47 (1919): 69-213.

260. See Barbour's correspondence with Atkins as well as his statement about his friendship with Atkins in "Reports." Box Q-Z, Records of the Atkins Garden and Research Laboratory, 1898-1946 UAV 231.5, HUA.

political and sanitary conditions of the country, and other general information of US interest.[261] His proficiency in Spanish and wide array of personal contacts in the country suited him to the work. Barbour had access to the Cuban and American planters circle through Atkins, and many of his friends in the naturalist community had some connection to the Cuban government. Scholarly work provided excellent cover for spies in Latin America during World War I.[262] Who would question a man whose main concern appeared to be the collection of rare lizards? While field scientists' role in espionage became a point of contention within the disciplines of archeology and anthropology, Barbour managed to avoid controversy. He certainly had no compunctions about the work, seeing it as a patriotic duty. His service remained a secret to the public, though it possibly had the effect of oiling the wheels in future relations with the State Department, when Barbour would seek passports, duty-free shipping of seeds and specimens to Soledad, and, as we will see in the next chapter, other favors for the new station at Barro Colorado Island, Panama. During this time, he described finally getting "the 'feel' of the island"—moving beyond his fascination with Cuba's wildlife and scenery to a deeper appreciation of its people, food, and culture. "In some respects," he wrote in his 1945 memoir of his travels in Cuba, he felt he had become "almost as Cuban as North American."[263]

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261. Thomas Barbour, 29 March 1917. 837.00/1279, State Department Central File, RG59, NARA-CP; Thomas Barbour, 20 January 1918. 123B23/134, State Department Central File, RG59, NARA-CP; Thomas Barbour, 18 January 1919. 837.124/11, State Department Central File, RG59, NARA-CP; Thomas Barbour to Major Sherman Miles, 2 December 1920. Military Intelligence Division File 51-312, RG165, NARA-CP. For a survey of US intelligence work in Cuba, see Joan M. Jensen, *Army Surveillance in America, 1775-1980* (New Haven, CT: Yale University Press, 1991).

262. On the most famous case of scientific espionage, and the larger context of spying in Latin America during World War I, see Charles H. Harris, and Louis R. Sadler, *The Archaeologist Was a Spy: Sylvanus G. Morley and the Office of Naval Intelligence* (Albuquerque, NM: University of New Mexico Press, 2003).

263. Thomas Barbour, *A Naturalist in Cuba* (Boston, MA: Little, Brown and Co., 1945), 5.

The State Department work allowed him to often spend weekends at Soledad and he grew to see the "the possibilities of the place."[264] In the US, Barbour frequently visited the Marine Biological Station at Woods Hole and he had spent some time at the Bermuda Biological Station for Research. He had also visited the laboratory and garden at Buitenzorg in 1907 during his "honeymoon" expedition to the East. Like the promoters of an American tropical botanical laboratory who were discussed in the previous chapter, he dreamed of a similar institution in the American tropics—one that would be more accessible to US researchers. He saw the Harvard station's potential to become a more broadly-based tropical biological station, like Buitenzorg or Cinchona, rather than simply an agricultural experiment station. Grey, of course, had his own microscope, but the lack of a well-equipped general laboratory limited the station's usefulness to visiting researchers. The station's expanding botanical gardens and arboretum could be useful to taxonomists and horticulturalists wishing to compare living specimens of closely related tropical plants. Thanks to Ame's efforts, the station was rapidly becoming one of the best collections of tropical plants in the Western Hemisphere. The area was also an attractive one for naturalists interested in Cuba's native flora and fauna. On horseback or in Barbour's battered Ford Model T, one could access nearly unexplored field areas for collecting and observation amid the otherwise monotonous fields of cane—the forests and caves of the Sierra Trinidad, coastal mangroves, serpentine savannas, and, a bit further west, Zapata Swamp, where rare and endemic species could be found.[265] Without designated lodging, however, the station's capacity for visitors was

264. Thomas Barbour, *Naturalist at Large*, 81.

265. The biological richness of these areas are still recognized today. Zapata Swamp (Ciénaga de Zapata) is today part of the Zapata Biosphere Reserve, the largest protected area in the Caribbean. The Trinidad Mountains are also part of Topes de Collantes nature reserve.

slight. With little advertising or scientific publication, few naturalists or ecologists were even aware of the place's potential.

Above all, the lack of a formal scientific administration hampered the station's progress. Ames had long argued this point, but with apparently little response from Atkins. By 1919, however, Atkins finally began to become more receptive to the idea of putting the station on firmer financial and administrative ground. What ultimately convinced him is unclear. Perhaps Atkins was more persuaded by Barbour, an expert grantsman, than by the earlier efforts of the more shy and restrained Ames—who found "begging for gifts" to be a very "distressing duty," even for the Botanic Garden at Harvard.[266] Indeed, Ames, becoming ever more burdened by administrative duties back at Harvard, had less power to influence Atkins than Barbour, who was in such close proximity during the late 1910s.[267] Another factor in Atkins decision may have been the sugar boom that accompanied World War I. With the cessation of competition from European beet sugar, more money was freed up to invest in the Harvard Station. Moreover, the station had proved its worth over its first two decades. In fact, in 1919, Harvard researchers helped to minimize the threat of cane mosaic disease by arguing that damage from the virus could largely be mitigated by cultural methods.[268] Finally, the aging businessman may simply

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266. Oakes Ames, and Pauline Ames Plimpton, *Oakes Ames, Jottings of a Harvard Botanist, 1874-1950*, 72.

267. Ames and Barbour were close friends, carrying on a cordial correspondence and spending time together not only in Boston, but while vacationing in Florida. Their friendship appears to have mitigated some of the tension over the fact that Barbour essentially usurped Ames' role in the administration of the Harvard Station. The fact that in his published memoirs, *A Naturalist in Cuba*, Barbour implied a large role for himself in cementing the official connection of the station with Harvard did cause some strain, however. Though Ames' unpublished autobiographical writings maximize his role in getting the Atkins endowment, the many circumstances discussed above, particularly discussions with Atkins during Barbour's residence in Cuba in 1917-1919, appear to factor in.

268. Oakes Ames, "Autobiographical MS: The Atkins Garden and Research Laboratory," (undated). Box 2, Papers of Oakes Ames, Autobiographical Writings HUG 4139.5, HUA; Harvard University, *Reports of the President and the Treasurer of Harvard College, 1919-1920* (Cambridge, MA: Harvard University, 1921), 240-41.Later publications related to this research include: Edward M. East, and William Henry Weston, *A Report on the Sugar*

have begun to think of posterity. In any case, in December of 1919, Atkins made his first donation of $71,395.00 to Harvard to establish the Atkins Fund for Tropical Research in Economic Botany.[269]

While the new endowment was explicitly for "investigations carried on for the benefit of tropical agriculture and horticulture with special reference to the sugar cane and forage crops," it laid the foundation for the station to increase its capacity for a wider variety of both economic and basic biological work.[270] The garden was soon expanded, reaching about thirty acres. Barbour helped to enlarge it's living collections by drawing on his large network of connections in business and government—particularly his close friend David Fairchild of the USDA and Wilson Popenoe at the United Fruit Company. Fairchild in particular emphasized the broad range of work that could be accomplished at Soledad. Fairchild visited Soledad several times with Barbour, donating seeds and fruit trees for breeding experiments there, and the two often vacationed together in Florida and accompanied each other on Caribbean travels.[271] As a well-known plant explorer, he was an important proponent for the value of a research trip to

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Cane Mosaic Situation in February, 1924, at Soledad, Cuba, vol. 1, Contributions From the Harvard Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1925); Edward M. East, "Immunity to Sugar Cane Mosaic Acquired by the Host," *Proceedings of the National Academy of Sciences* 17, no. 6 (1931): 331-34.

269. "Atkins Fund," Box 2, Atkins Institution Papers, AAA; Harvard University, *Reports of the President and the Treasurer of Harvard College, 1919-1920*, 203-04.

270. Edwin F. Atkins to the President and Fellows of Harvard College, 2 January 1920. Box A-P, Folder "Mrs. Edwin F. Atkins," Records of the Atkins Garden and Research Laboratory, 1898-1946 UAV 231.5, HUA.

271. An account of Barbour's herpetological collections during the four voyages of Allison Armour's ship *Utuwana* in which Fairchild and Barbour participated can be found in: Robert W. Hendrson, and Robert Powell, "Thomas Barbour and the Utuwana Voyages (1929-1934) in the West Indies," *Bonner Zoologische Beiträge* 52, no. 3 (2004): 297-309.

Soledad.[272] Addressing professional geneticists, he promised that the application of their skill to the material assembled there would

> lead to the production of amazing, productive, delicious, and valuable new varieties of tropical plants, which, as the exploitation of the Torrid Zone progress, will become of the greatest commercial importance. As with our northern agriculture, so with that of the tropics—it must be based upon varieties and varieties are the result of selection and plant breeding.[273]

Fairchild cast research at Soledad not as an outpost on the periphery of American science, but as a key institution on the scientific frontier of tropical research. Fairchild urged American geneticists to "realize the need of enlarging their horizon of acquaintance with living organisms" by visiting the tropics at Soledad. He cast plant breeding as genetics (a "fundamental" science), rather than as horticulture, when he proclaimed that the work at Soledad has "opened the door to the geneticists into a vast and fascinating field for discovery."

Perhaps most importantly, in 1924, Barbour helped to build a biological laboratory at the station—"Harvard House." Although no formal classes were yet held, a few students a year began to be given funding to carry out research at the station by the late 1920s and 1930s, and better records of visitors began to be kept.[274] With this extension, variety of research possible at Soledad was greatly expanded. The horticulturalist Liberty Hyde Bailey, whose influence on American agrarian thought is difficult to overestimate, visited in 1929 for part of his definitive work on palm systematics. Due to their large size, palms often defy traditional herbarium

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272. On Fairchild's plant introduction work, see Philip J. Pauly, "The Beauty and Menace of the Japanese Cherry Trees: Conflicting Visions of American Ecological Independence," *Isis* 87, no. 1 (1996): 51-73.

273. David Fairchild, "The Soledad Garden and Arboretum: The Harvard Biological Institute in Cuba (Atkins Foundation) on Soledad Estate," *The Journal of Heredity* 15 (1924), 461.

274. Information following comes from "Guest Book (Soledad," Box A-P, Records of the Atkins Garden and Research Laboratory, 1898-1946, HUA.

practices and Bailey needed access to Atkins' extensive live palm collection, as well as herbaria, lab, and field excursions in order to make determinations.[275] The entomologist Philip Darlington used Soledad as a base for several trips—some more than a month long—in the 1920s and 1930s, as he completed important collecting work for his theories on the origin of the West Indian fauna.[276] Darlington was one of the most influential biogeographer of the US, and Edward O. Wilson, another Soledad visitor as we will see in Chapter 5, would cite Darlington as his first major influence in his thinking about island biogeography. Other students came funded primarily for economic projects, but worked on more "academic" biological problems on the side. As a graduate student, for example, Carl Parsons received a fellowship for economic entomology, but also studied and published on insect mimicry.[277] Marston Bates, a longtime friend of David Fairchild's son and daughter (and his future son-in-law), spent July and August of 1932 at Soledad collecting butterflies to complete his doctoral thesis.[278] He would become a major proponent of tropical research (see Chapter 5) and a widely-read environmental writer. The student José Perez Carabia, one of a handful of Cuban collaborators with the Atkins Institution, wrote about the endemic flora of Cuba.[279] He would later become the co-founder of the *Jardín*

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275. Bailey's tropical work is barely discussed in the large literature on his influence on US agriculture. Liberty Hyde Bailey, "Palms and Their Characteristics," *Gentes Herbarum* 111, no. 1 (1933): 1-29; L. H. Bailey, *The Palm Herbarium, With Remarks on Certain Taxonomic Practices*, Gentes Herbarum, V. 7, Fasc. 2 (Ithaca, NY: 1946); Andrew Denny Rodgers, *Liberty Hyde Bailey: A Story of American Plant Sciences* (Princeton,: Princeton Univ. Press, 1949).

276. P. J. Darlington, Jr., "The Origin of the Fauna of the Greater Antilles, With Discussion of Dispersal of Animals Over Water and Through the Air," *The Quarterly Review of Biology* 13, no. 3 (1938): 274-300; Philip Jackson Darlington, *Zoogeography: The Geographical Distribution of Animals.* (New York, NY: Wiley, 1957).

277. Carl T. Parsons, "Observations in Cuba on Insect Mimicry and Warning Coloration," *Psyche* 47, no. 1 (1940): 1-7.

278. Marston Bates, "The Butterflies of Cuba," *Bulletin of the Museum of Comparative Zoology* 78, no. 2 (1935).

279. J. P. Carabia, "Endemism in the Cuban Flora," *American Journal of Botany* 29, no. 10 (1942): 4s; J. P. Carabia, "A Brief Review of the Cuban Flora," in *Plants and Plant Science in Latin America*, ed. Frans Verdoorn

Botánico Nacional de Cuba. Botanists Walter Hodge and Richard Howard visited Soledad as fellows funded through the Atkins endowment and would each, as we will see in Chapter 5, lead his own class of students there in the 1950s. Hodge would be instrumental in the organization of new institutions for education in tropical biology in the 1960s. Richard Howard, director of the Arnold Arboretum from 1954-1977, recalled his visits to Soledad as "formative."[280]

In Barbour's opinion, the "greatest contribution which the Garden has made to science is the opportunity it offers to young scientists who, for the first time, are able to see the tropical flora and fauna in their natural state, to which they will have occasion to refer during their research and teaching."[281] Barbour saw to it that the work of the station and researchers drawing on the Atkins Fund were published.[282] The availability of the station to visitors was made public and Barbour called on friends to advertise its potential to particular audiences.[283] By 1939 the station's grounds would reach 221.63 acres and demand for lodging was great enough to justify dormitory building, "Casa Catalina," which could accommodate twelve visitors at once.[284] With

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(Waltham, MA: Chronica Botanica Company, 1945); J. P. Carabia, "The Vegetation of Sierra de Nipe, Cuba," *Ecological Monographs* 15, no. 4 (1945): 322-41.

280. Richard A. Howard, "Some Reminiscences of the Soledad Botanical Garden, Cuba, 1940-1950," *Harvard Papers in Botany* 5, no. 1 (2000): 167-70.

281. Thomas Barbour, *A Naturalist in Cuba*, 95.

282. Edward M. East, and William Henry Weston, *A Report on the Sugar Cane Mosaic Situation in February, 1924, at Soledad, Cuba*; George W. Salt, and John Golding. Myers, *Report on Sugar-Cane Borers at Soledad, Cuba*, vol. 3, Harvard Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1926); Robert Melrose Grey, *Report of the Harvard Botanical Gardens, Soledad Estate, Cienfuegos. Cuba (Atkins Foundation) 1900-1926*; Nathan Banks, and J. G. Myers, *Studies on Cuban Insects*, Atkins Institution of the Arnold Arboretum (Cambridge, MA: Harvard University Press, 1928).

283. For example: David Fairchild, "The Soledad Garden and Arboretum: The Harvard Biological Institute in Cuba (Atkins Foundation) on Soledad Estate."; Wilson Popenoe, "The Harvard Botanic Garden at Soledad," *Harvard Graduates Magazine* March (1929): 4; Modesto Martinez, "El Jardin Botanico de la Universidad de Harvard (Fundacion Atkin) en Soledad, Cienfuegos, Cuba," *Revista de Agricultura, Comercio y Trabajo* April, no. 10 (1931): 9-13.

284. Casa Catalina was named after Katherine Atkins, who split the cost of construction with Barbour. Harvard

these steps, as Fairchild commented, the station had begun to make "foundations broad and secure in support of research in the vast field of the tropics."[285]

Conclusion

During the station's first two decades, the mission of the Harvard station was shaped by both the slow, periodic nature of cane research in Cuba and the varied but corresponding interests of its founders. Because hybridization work could only be done on the schedule that the cane plants and Cuba's weather allowed, space emerged for a wider variety of activities. These activities were shaped and directed by the concerns and talents of both Atkins and the Harvard men. Goodale and Ames knew how to administer a botanic garden, and knew the importance of building a collection of living tropical plants. Grey, an enthusiastic plantsman, followed his instincts in gathering and breeding plants as his time and interest allowed. For Atkins, the station fulfilled enough of its applied sugar research goals, and satisfied enough of his desire for prestige, to be granted a large degree of freedom in the work that went on there. When Barbour came onto the scene, Atkins was open to his broader vision of what the station could become. By the mid-1920s the Harvard Station was fully established as a tropical research station equipped for a range of biological work and attracted scientists from not only Harvard, but other American universities and, to a lesser extent, from across the region.

This transition toward more attractive facilities and a broader biological focus should not be confused for a shift toward a more "pure" form of science, however. For one thing, economic

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University, *Report of the President of Harvard College and the Reports of Departments for 1938-39*, vol. 31(3), Official Register of Harvard University (Cambridge, MA: Harvard University, 1940), 405-06.

285. David Fairchild, "The Soledad Garden and Arboretum: The Harvard Biological Institute in Cuba (Atkins Foundation) on Soledad Estate," 461.

botany remained at the core, and the station's mission was directly linked to US interests in the exploitation of the region. Instead, Barbour articulated a new, more explicit emphasis on the close relationship between basic research in tropical biology and potential economic outcomes. With growing American consumption of tropical resources, Barbour told Harvard alumni and potential funders, a lack of basic research on tropical organisms and environments would hold back both economic and scientific progress. The tropics brought together fundamental scientific questions about the origins of the tropics' great diversity and abundance of life with vital problems of commercial, national, and, indeed, human interest. "Who that has camped in cathedral forests on the Equator has not realized that there is the mother liquor and that nature as we see it about Cambridge, for example, is but a weak dilution?" Barbour wrote, "South, the forces of nature work at their maximum intensity, for our benefit and for our injury as well." Understanding the forces of tropical nature, for their own sake and for humanity's benefit meant taking science into the tropics, Barbour explained, "We can study these forces only where they are active. Personally this belief has become perhaps almost an obsession." With the university's strengths in many different biological departments and a new appreciation for its developing contacts in the tropics, he argued,

> The opportunity at Harvard is unrivalled [sic], and who does not believe that, when we make known what has been done with small resources, then greater assistance surely will be vouchsafed? We have made only a beginning.... Harvard has a modern laboratory in Cuba just finished and soon to be equipped, and near to it one of the first dozen of the tropical botanical gardens of the world. By endowing this, Mr. Edwin F. Atkins has shown his faith in the future development of tropical biology.[286]

286. Italics are in original. Thomas Barbour, "More About Harvard Biology," *Harvard Alumni Bulletin* 27, no. 8 (1924), 212.

Barbour's own faith in the future of tropical biology was unshakeable. As we will see in the next chapter, Barbour acted on this obsession not only at the Harvard Station in Cuba, but through work with the Institute for Research in Tropical America and a new station at Barro Colorado Island. He was a keen administrator and worked tirelessly to create solid institutional foundations for tropical biology in the Americas over the next two decades. Soledad was only the first node in a larger network that Barbour was helping to create. It was not a place for "pure botany" by any means, but, Barbour pointed out, it was "the only great tropical garden which is privately controlled by a university, and not managed by a government."[287] At the same time, ties to E. Atkins and Company and, increasingly, other US corporations gave it the stability that Cinchona, for example, did not have. As it grew, tropical biology would continue to build on these foundational economic ties.

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287. Thomas Barbour, *A Naturalist in Cuba*, 102.

Chapter 3

Organizing Biology in Tropical America: Imperial Ambitions and the Assembly of a Scientific Network

"We are building de novo*"*

The laboratory at Soledad was not the only one Barbour had a hand in building in 1924. Just before arriving at Soledad that winter, Barbour was busy helping to begin construction of a laboratory on Barro Colorado Island (BCI), Panama, the site of the new station for an organization called the Institute for Research in Tropical America (IRTA). Among the first to use the site, even as construction was underway, were David Fairchild—who accompanied Barbour to Cuba for the inauguration of the Soledad laboratory, his son Graham Bell "Sandy" Fairchild, and several of Barbour's fellow Harvard zoologists, including William Morton Wheeler.[288] Fairchild set up an improvised microscope table under "the half-finished laboratory," while only a hundred yards away Wheeler scrambled to capture a queen of the army ant *Eciton hamatum*—a specimen never before collected.[289] At dawn, they were thrilled by the booming calls of howler monkeys in the treetops.[290] They were grown men "roughing it" like boys on a camping trip, but they were also laying the foundations for an institution that would attract

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288. Joel B. Hagen, Oral History Interview with Graham Bell Fairchild, 7 June 1989. RU 009559, SIA; Joel B. Hagen, Oral History Interview with George C. Wheeler, 7 June 1989, RU 009560, SIA.

289. David Fairchild, *The World Was My Garden* (New York, NY: Charles Scribner´s Sons, 1938), 470; William Morton Wheeler, "Courtship of the Calobatas," *Journal of Heredity* 15, no. 12 (1924); William Morton Wheeler, "The Finding of the Queen of the Army Ant Eciton Hamatum," *Biological Bulletin* 49, no. 3 (1925); William Morton Wheeler, "A New Guest-Ant Other New Formicidae From Barro Colorado Island, Panama," *Biological Bulletin* 49, no. 3 (1925): 150-81.

290. Joel B. Hagen, Oral History Interview with Graham Bell Fairchild, 7 June 1989.

hundreds of American biologists over the span of the century—researchers who otherwise might never have ventured to do fieldwork in a tropical forest.[291]

James Zetek, an entomologist working in the Panama Canal Zone for the USDA, was also on the island overseeing the proceedings. Zetek was instrumental in securing the island as the site for the IRTA station. Along with Barbour, and through the auspices of IRTA, Zetek was responsible for establishing BCI and managing it for decades to come. Zetek's talent at courting local support in the Canal Zone, in conjunction with Barbour's high-level social connections and personal financial backing, were at the heart of BCI's long-term viability as a station for American biologists. Without Barbour and his contacts to, in his own words, "weave...through the intricate maze of Washington red tape and the equal skill with which James Zetek treads the same path in the Canal Zone," BCI could not have become a place for science.[292] In their respective spheres, Barbour and Zetek assembled and sustained the network of patronage and science that made BCI possible.

As Chapters 4 and 5 will show in more detail, BCI would become the twentieth century's most influential tropical research station. A six-square mile island, covered in lush tropical forest, BCI had its origins as a hill isolated by the rising waters of the Panama Canal. It is only about thirty miles from Panama City and was easily reached by American biologists traveling by steamship from either coast of the United States (see Appendix 4). Because of its accessibility and the unique opportunities for working comfortably within the "jungle," the station would attract more scientific visitors than any other tropical station in the Western hemisphere—an

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291. On Shannon: Chapman my tropical air castl p. 25

292. Thomas Barbour, *Naturalist at Large* (Boston, MA: Little, Brown and Company, 1943), 199.

average of 25 per year from its establishment in 1923 through the 1930s, and many times that in subsequent decades (see Appendix 5). No single site did more to establish a community of American biologists with experience in the tropics than Barro Colorado in the early twentieth century.

The growing fame of BCI is significant especially because of its emphasis on basic tropical biology rather than having a more narrow focus on agriculture or medicine. Although BCI and Soldad were administered by Barbour from the mid-1920s through 1940s, BCI was a very different kind of station than Soledad—one dominated by dense forest rather than expansive cane fields. From Soledad, as we saw previously, many researchers traveled throughout the Cuban countryside to collect and observe wildlife in forests and marshes further afield. But at BCI, Albert Spear Hitchcock, another of BCI's founders explained, "scientists working at the laboratory will literally have the wild at their door."[293] Barbour had had to broaden Soledad from an agricultural station and garden into a biological station. In Panama, in contrast, he and the co-founders of BCI stumbled upon an opportunity to build from the ground up what Barbour affectionately called his "naturalists' paradise."[294]

Barbour's Caribbean travels, from February to May each year, were always a relief from the frosty weather of the Northeast. In 1924, the annual respite from academic politics and long committee meetings must have been even more welcome. Barbour was in the midst of preparation for two major campaigns in Cambridge: one to consolidate and centralize Harvard's

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293. A.S. Hitchcock, "Biological Station of the Institution for Research in Tropical America," *Science Supplement* February 29 (1924).

294. Barbour and his friends used this phrase to refer to Barro Colorado Island in its early days. For example, Fairchild to Barbour, 13 August 1923. Box 14 "Fairchild, David," Folder "1923-1928," Records of the Museum of Comparative Zoology, 1860-1985 UAV 298.20, HUA.

various biological institutions, and another related effort to form an Institute for Tropical Biology and Medicine unifying the university's disparate lines of tropical research.[295] The biological departments back at Harvard might have been divided into parochial fiefdoms within botany or zoology, museum or laboratory, and "pure" or applied biology. "With respect to our tropical contacts, however," Barbour explained, "we are building *de novo,*" free from the constraints of institutional memory and separate endowments.[296] As part of his overall efforts to build support for tropical biology, he had also been serving as Harvard's representative to IRTA—a consortium of US universities, museums, botanical gardens, and scientific societies, affiliated with the National Research Council (NRC)—since its incorporation in 1921. At last, the new station at BCI represented a practical outcome of IRTA's many long, "drab, bickering meeting[s] of scientists in Washington." Camping on BCI, Barbour finally could see the "best job I had ever done in my life" taking tangible shape beneath the tropical forest's soaring canopy.[297]

BCI, however, needed institutional and financial foundations as well as physical ones, and Barbour's work in Washington and Cambridge, although frustrating, was indispensable. Barbour, who served on IRTA's executive committee, would later disparage the short-lived institute as little more than a "paper organization."[298] It is true that, within a few years, Barbour

295. Barbour laid out his ideas on these issues in, for example, Thomas Barbour, "Biology at Harvard," *Harvard Graduates Magazine* 33, no. 129 (1924): 94-97; Thomas Barbour, "More About Harvard Biology."; Thomas Barbour, "Tropical Research Institute at Harvard," *Harvard Alumni Bulletin* January (1925): 419-21. Only a curator, Barbour was not yet director of the Museum of Comparative zoology (MCZ), but was seen by most as the obvious successor to the seventy-three-year-old director Samuel Henshaw, who many at the MCZ felt was well past due for retirement. As such, Barbour was already being consulted by president Lawrence Lowell about future developments in biology at the university; Mary P. Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum*, 262.

296. Thomas Barbour, "Tropical Research Institute at Harvard."

297. Thomas Barbour, *Naturalist at Large*, 193.

298. For example, Ibid. and Barbour to Hartshorn, 24 November 1937. Box 17, Folder 2: "General Correspondence

and Zetek would be managing BCI more directly as individuals than through the official channels of IRTA and NRC bureaucracy. Nevertheless, this chapter reveals the key role that this organization played in BCI's establishment and in the assembly of the patronage network that supported the station into the future. Before returning to Zetek's role in events in the field in Panama, a detour into the history of IRTA is first necessary for understanding not only BCI's institutional foundations, but also the station's context within broader US-Latin American relations and the funding structures of US science in the 1920s. Although none of IRTA's more grandiose plans ever came to fruition, its history situates BCI as just one outgrowth of a broader US movement to organize tropical science.

Thomas Barbour and the "paper organization"

In some ways, Barbour was right. IRTA did exist more on paper—in reports, meeting minutes, and correspondence—than as any brick-and-mortar institution. IRTA's records are overflowing with American scientists' competing and contradictory visions for the future of tropical research and its relationship with US commercial and political expansion into Latin America. BCI may have been the only lasting outcome of IRTA's many "literary" schemes, but by bringing together some actors and pushing others apart, these documents produced concrete results, giving shape to the networks of people and funds that allowed BCI to materialize as a station for tropical biology. To understand BCI's emergence as a leading site for biological research in the tropics, we must first understand how it became embedded in the broader matrix of science and patronage that permitted its existence.

HA," RU 135, SIA.

The idea of an institute dedicated to research in the tropics was first suggested at the 1919 annual meeting of the US National Research Council's Division of Biology and Agriculture, and set in motion the following year.[299] Formed in 1916 as the First World War created a pressing need for closer cooperation among government, industry, and research organizations, the NRC's initial priority was the investigation of defense technologies. After the war, focus shifted toward basic science and the coordination of the nation's scientific institutions.[300] In particular, the NRC took on the facilitation of scientific projects that were in some way concerned with America's foreign relations. The NRC quickly moved to support tropical science as US economic interests increasingly turned to Latin America and the Caribbean following the First World War.[301] With British capital tied up in the war and subsequent recovery, opportunities opened for already expanding American interests. By 1929, American investment in the region would double pre-war levels.[302] American science could both facilitate such financial interests and profit from

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299. The clearest description of these events in the overview of previous work at the beginning of "National Research Council Division of Biology and Agriculture Conference on Scientific Research in the Philippines and other Tropical Countries," 5 November, 1920. Box 25, Folder 1, RU 135, SIA. See also "National Research Council Division of Biology and Agriculture Conference on Research in Tropical America," 12 June 1920. Conference on Research in Tropical America, NAS. (A duplicate can also be found in Box 25, Folder 1, RU 135, SIA.) Although there is a typo stating "1922" on the final two pages of these minutes, other letters in the NAS folder "Conference on Research in Tropical America" confirm that 1920 is the correct date.

300. A. Hunter Dupree, *Science in the Federal Government, a History of Policies and Activities to 1940.* (Cambridge, MA: Belknap Press of Harvard University Press, 1957), 309-311, 326-327.

301. See for example Roberto Cassá, "The Economic Develoment of the Caribbean From 1880 to 1930," in *The Caribbean in the Twentieth Century*, ed. Bridget Brereton, General History of the Caribbean (Paris: Unesco Publishing, 2004), 40-41; Jenny Pearce, *Under the Eagle: US Intervention in Central America and the Caribbean*, Updated ed. (London: Latin American Bureau (Research and Action) Ltd, 1982); David Healy, *Drive to Hegemony: The United States in the Caribbean, 1898-1917* (Madison, WI: University of Wisconsin Press, 1988). Louis Perez in particular explores the relationship between US interest in the Caribbean and waves of US scholarship on the region—similar cycles may apply to scientific scholarship, Louis A. Pérez, Jr., "Intervention, Hegemony, and Dependency: The United States in the Circum-Caribbean, 1898-1980," *The Pacific Historical Review* 51, no. 2 (1982), 169-75.

302. Jenny Pearce, *Under the Eagle: US Intervention in Central America and the Caribbean*, 17-18.

them. IRTA emerged amid a variety of other NRC projects on tropical subjects. Among these were the Committee on Phytopathology in Tropics (1919-1925), the Conference on Scientific Research in Philippines (1920), and the Tropical Plant Research Foundation (1924-1931), all of which brought together representatives of basic US scientific institutions with business concerns.[303]

On their own, American scientific institutions had already begun to do "excellent work in research in the tropics," noted Clarence E. McClung, Chairman of the NRC's Division of Biology and Agriculture. But at the meeting that formally incorporated IRTA, he argued that research had proceeded in a piecemeal and uncoordinated fashion. The NRC's role was to bring these various interests together, although not necessarily to lead them in any particular direction. Indeed, McClung pointed out, "When the organization is completed the Council's connection practically ceases, except in so far as it is wished to have it associated." The purpose of IRTA was to "set up a center which will increase the interest and contact with the tropics for the benefit of the whole country."[304] IRTA would coordinate scientists' previously unsystematic efforts, allowing them to more effectively serve larger national interests.

IRTA members agreed on a broad—perhaps overly broad—outline of basic needs in tropical research. Although Barbour would express frustration at the impracticality of the institute's grander proposals, the broad approach to tropical biology taken by IRTA was in

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303. See the Division of Biology and Agriculture, 1919-1939, collection, NAS.

304. "National Research Council Division of Biology and Agriculture Committee on Organization and Incorporation for the Institute for Research in Tropical America," 15 January 1921. Box 25, Folder 1, RU 135, SIA. The NRC would play a similar role in facilitating tropical biology later in the century; see Chapter 5 and Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975* (Baltimore, MD: Johns Hopkins University Press, 2000), 197-205.

harmony with his own views. IRTA's members determined in the institute's founding documents that major lines of work should include tropical agriculture, medicine, and the investigation of new plant resources, as well as "what is often termed pure science, that is, investigations which may not be directed at any particular industrial problem... In short, the most rapid progress will be made if facilities are offered to all biologists and for all kinds of biological work."[305] Above all, they decided that two major types of projects were most needed: the organization of exploration and the establishment of research stations. For the former, the advantage of an institution like IRTA would be that expeditions could be "coordinated systematically to cover the entire region." This would allow exploration to proceed in a more efficient and unified way, rather than in the haphazard fashion it had so far proceeded. As to the latter project, IRTA members discussed the necessary attributes of a potential tropical field station or network of stations. Members spent much of their time weighing the benefits and drawbacks of different locations, but ultimately they agreed:

> a station or laboratory should be so situated that the workers may be surrounded by healthful conditions, and normal social and bodily comforts. It...should be easily accessible to the larger centers of learning and commerce...the station should be in or near the great tropical forests or jungles. It need not be emphasized that localities subject to political instability must be avoided.[306]

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305. The individual authorship of IRTA documents is unclear at times, and apparently represents the consensus of the group. Unless I cite a specific author, this can be assumed to be the case. For example, "Institute for Research in Tropical America: Outline of a Plan for the Work of the Institute," 13 April 1922. Box 25, Folder 1, RU 135, SIA.

306. "Institute for Research in Tropical America: Outline of a Plan for the Work of the Institute," 13 April 1922. Box 25, Folder 1, RU 135, SIA. More examples of such recommendations include "Correspondence in re Desirable Location for a Tropical Laboratory." Series 1, Box 1, Folder "Duncan Starr Johnson Correspondence, Institute for Research in Tropical America, 1920-21," Records of the Department of Biology Record Group 04.070, JHUA.

With their concerns about health, accessibility, stability, and environment, these IRTA discussions closely echoed the botanists who communicated with D. T. MacDougal's Tropical Laboratory Commission more than two decades earlier (see Chapter 1).

In contrast to MacDougal's efforts to create an American tropical laboratory around 1900, however, by the 1920s, most major US scientific institutions already had begun their own extensive tropical projects. The number of American scientific expeditions throughout the tropical world increased dramatically in the first decades of the twentieth century.[307] The country's major botanical institutions in New York, Boston, and Washington DC battled furiously for ever-larger tropical collections.[308] Moreover, beyond Harvard's station at Soledad—which Barbour had had a growing stake in since WWI, as we saw in the previous chapter—the university's medical and botanical faculties were increasingly involved in making tropical expeditions and collections.[309] Barbour, as discussed above, was becoming interested in developing tighter connections among the various tropical projects at Harvard, and for the most part was committed to IRTA only in so far as it might facilitate these. Other IRTA members had similarly divided loyalties. Duncan Johnson, of Johns Hopkins University, was a particular

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307. A sense of this expansion can be gained by scanning, for example, the volumes on "Expeditions Organized Or Participated in By the Smithsonian Institution" and "Explorations and Field-Work of the Smithsonian Institution" in the *Smithsonian Miscellaneous Collections*. See also Brian M. Boom, "Botanical Expeditions of the New York Botanical Garden," *Brittonia* 48, no. 3 (1996): 297-307; Stuart McCook, "'The World Was My Garden': Tropical Botany and Cosmopolitanism in American Science" (Paper presented at the conference Transitions and Transformations in the US Imperial State: The Search for New Synthesis. University of Wisconsin, Madison, November 11, 2006).

308. On inter-metropolitan competition among US botanical gardens, see Peter Philip Mickulas, *Britton's Botanical Empire: The New York Botanical Garden and American Botany, 1888-1929*.

309. The most complete available list of departments and faculty involved in tropical research that I have found is Richard Howard, "Harvard's role in neotropical botany: An Historical and Partial Review," 1993, unpublished lecture, General History IX, AAA.

advocate for the Cinchona Botanical Station. Cinchona was by now under the Smithsonian's charge (see Chapter 1), but supported in part by his own university's contributions. William Beebe, also an IRTA member for several years, was already becoming well-known for his popular writings and many tropical expeditions for the New York Zoological Society. Moreover, at the time of IRTA's formation, he had recently re-established his tropical research station in British Guiana—the first zoological station of its kind—after a period of suspension during the war.[310] All of these existing commitments meant that there was much greater potential for conflict over IRTA's overall mission than the Tropical Laboratory Commission had faced at the turn of the century.

Members expressed an understandable resistance to the idea of ceding any authority to IRTA over their own institutions' ongoing or future projects. For example, plans stipulated that collections produced by any IRTA expeditions would go to the member institutions who led the expedition, not any new IRTA-run herbarium or museum. Moreover, while members discussed the possibility of uniting existing tropical research stations into a more stable and efficient network, nothing came of this idea. On one hand, an already well-established station like

310. Under the auspices of the New York Zoological Society, Beebe ran two stations, successively, in the Bartica region of British Guiana. The first, named "Kalacoon," was used in the spring of 1916 and abandoned in 1917 after an encroaching rubber plantation destroyed the surrounding forest. Its replacement was "Kartabo," which he used intermittently from 1919 until 1927, when Beebe shifted focus toward marine studies in Bermuda. Beebe recounts the activities of the stations in the annual reports of the New York Zoological Society, as well as William Beebe, et al., *Tropical Wild Life in British Guiana; Zoological Contributions From the Tropical Research Station of the New York Zoological Society* (New York, NY: The New York Zoological Society, 1917); William Beebe, *Jungle Peace* (New York, NY: H. Holt and Company, 1918); William Beebe, *Edge of the Jungle* (New York, NY: H. Holt and Company, 1921). Beebe's stations and his place in the history of tropical biology are also discussed in Carol Grant Gould, *The Remarkable Life of William Beebe: Explorer and Naturalist* (Washington, DC: Island Press/Shearwater Books, 2004), 188-217; Kelly Enright, *The Maximum of Wilderness: The Jungle in the American Imagination* (Charlottesville, VA: University of Virginia Press, 2012), 73-110. Through the course of his career, Beebe's interests shifted toward deep sea exploration and away from work in tropical forests, but his influence, particularly in popularizing tropical biology, was outsized.

Soledad had little to gain from official association with IRTA. Soledad was increasingly Barbour's pet project at this time and he was uninterested in weakening his own authority over it. He made sure to keep Soledad well clear of IRTA's plans, in spite of others' suggestions of the suitability of Soledad as part of a network of IRTA stations.[311] On the other hand, such an association could have helped struggling stations like Cinchona or the Carnegie Institution's marine station at Dry Tortugas, which was beset by particular bad fortune, gain larger financial support and attendance.[312] Duncan Johnson, who frequently brought students to Jamaica, was particularly keen to use IRTA to place Cinchona on a more stable footing.[313] Nevertheless, the frequency with which discussions of such proposals were postponed suggests that IRTA members were reluctant to use up resources trying to save sinking ships, let alone ones associated with a competing research institution.

While inter-institutional rivalries blocked the formation of new partnerships, at a deeper level, IRTA's members also expressed ambivalent and often contradictory views about how to position science within the context of the political and economic expansion of the United States into tropical countries. Some members emphasized the role of science in developing tropical products and looked forward to cooperation with interested corporations. Indeed, IRTA's

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311. See for example the selection by John R. Johnston's in "Correspondence in re Desirable Location for a Tropical Laboratory" and I. W. Bailey to Duncan Johnson, 9 February 1921. Series 1, Box 1, Folder "Duncan Starr Johnson Correspondence, Institute for Research in Tropical America, 1920-21," Records of the Department of Biology Record Group 04.070, JHUA.

312. James D. Ebert, "Carnegie Instituiton of Washington and Marine Biology: Naples, Woods Hole, and Tortugas." The possibility of IRTA taking over the Tortugas station is discussed, for example in Hitchcock to the members of the Executive Committee, IRTA, 15 January 1923. Box 1, Folder 2, RU 134, SIA.

313. See in particular Johnson to McClung, 10 April 1920. Series 1, Box 1, Folder "Duncan Starr Johnson Correspondence, Institute for Research in Tropical America, 1920-21," Records of the Department of Biology Record Group 04.070, JHUA.

founding documents explicitly justified the need for biological research in the tropics based on its utility for resource exploitation, characterizing “tropical America” as “one of the few large areas of the world now awaiting development.”[314] And for example, one IRTA proposal predicted “a long future in which the conquest of the tropics will become vital to the welfare of the world and in which biological problems will be fundamental.”[315]

Indeed, some of the plans that IRTA considered openly relied on nationalism. For example, Franklin Sumner Earle, an expert on sugar cane diseases who consulted for companies in Cuba and Puerto Rico, proposed establishing a degree-granting institution named after president Theodore Roosevelt.[316] “No one,” he argued, “appreciated more keenly than Roosevelt the importance to the future welfare of our race of this vast region with its limitless undeveloped and scarcely recognized resources.” His plan explicitly linked tropical research with national expansion, arguing that “the next hundred years will witness great tropical developments for the Western frontier no longer exists. It has drifted South.”[317] The frontier of American empire, IRTA members like Earle believed, had shifted from the American West to the tropics, and it was the task of American science to advance that frontier of economic development by improving the plant resources of the tropics. IRTA’s main planning document itself emphasized

314. “Institute for Research in Tropical America: Outline of a Plan for the Work of the Institute,” 13 April 1922. Box 25, Folder 1, RU 135, SIA.

315. “Plan for Comprehensive Investigations of the Animal and Plant Life of South and Central America,” 1920? Box 25, Folder 2, RU 135, SIA.

316. Incidentally, Earle’s daughter Ruth Earle married David Sturrock, the superintendent of the Harvard’s Atkins Institution at Soledad between 1936 and 1946 (see Chapter 2). Connections of friendship and family among Americans working in Cuba, and through the tropics, were tight. Carlos E. Chardon, “Franklin Sumner Earle,” *Mycologia* 21, no. 6 (1929): 301-03; Richard A. Howard, “David Sturrock (1893-1978)-- Mentor and Friend,” *Journal of the Arnold Arboretum* 60, no. 1 (1979).

317. “The Roosevelt Memorial Institution Devoted to the Study of Tropical America,” by F. S. Earle, 1919. Box 25, Folder 2, RU 135, SIA.

the importance of science for exploiting tropical "raw products" and developing "the tropics as a home for the white race" through research in tropical medicine.[318] In turn, tropical studies would have a "broadening effect on American biological science [that] would be of high value." "Such information as we have," one proposal noted, "we take second hand from the British or the Germans. Encouragement of work in this field, therefore, would develop our own experts in special branches of biology in which we are now deficient and dependent." Johnson argued that research stations were needed to give American students tropical experience; "this is the only way in which the ranks of tropical researchers can be increased at a rate commensurate with the increasing importance of tropical problems."[319] By developing facilities, IRTA could promote "supremacy...for American biologists" in tropical biology.[320] Barbour agreed, "it seems absolutely necessary that American biologists should have the facilities for work in the tropics which are offered to British workers at Peradynea, to Dutch at Buitenzorg, and to French at Saigon."[321] By promoting tropical research, IRTA stood to strengthen American biologists' place on the international stage, as well as advance national interests.

While IRTA members generally agreed about biology's role in "developing" the tropics, some statements showed a more cautious attitude toward the idea of direct relationships with corporations or government. After much discussion, full membership was limited to

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318. "Institute for Research in Tropical America: Outline of a Plan for the Work of the Institute," 13 April 1922. Box 25, Folder 1, RU 135, SIA.

319. Johnson to Franklin Sumner Earle, 23 December 1921. Series 1, Box 1, Folder "Duncan Starr Johnson Correspondence, Institute for Research in Tropical America, 1920-21," Records of the Department of Biology Record Group 04.070, JHUA.

320. "Plan for Comprehensive Investigations of the Animal and Plant Life of South and Central America," 1920? Box 25, Folder 2, RU 135, SIA.

321. Barbour to Frank R. Lillie (NRC), 26 September 1922. RU 135, Box 25, Folder 2, SIA.

representatives of scientific institutions. Industrial interests could be represented only by "associate members."[322] As Chairman Hitchcock explained, "The committee that drew up the constitution and by-laws was of the opinion that we should provide against control passing into the hands of industrial concerns."[323] Members expressed faith in the pure science ideal, arguing "If the fundamental exploration and research are done, the practical matters can be trusted to take care of themselves... Development of subjects of mainly economic interest should not go beyond their scientific aspects."[324] Moreover, some worried specifically about the negative appearance of close relations with commercial interests:

> If the spirit of the Monroe Doctrine, which presupposes a community of interests among nations of the New World, is to survive, it must be based upon something more than political exigency...commercialism always has an element of exploitation about it and since Latin American nations are especially suspicious of stronger powers, something more than commerce is needed to maintain confidence and good feeling. The introduction of American ideals and methods in science into these countries is likely to be well received....In spite of their backwardness in some respects, the Latin Americans have a very high appreciation for the value of science....Scientific workers with their high ideals and disinterested motives might exert a powerful influence for good.[325]

Such statements show that some IRTA members were aware of and concerned about Latin American perceptions of US neocolonialism. Yet these members held onto a rather idealistic, internationalist view of the Monroe Doctrine and they maintained a faith in a disinterested, pure

322. "IRTA Constitution," 1921. Box 25, Folder 1, RU 135, SIA. Franklin Sumner Earle, of the Central Aguire sugar company, Puerto Rico, and John R. Johnston of the United Fruit Company were the first such associate members.

323. Hitchcock later reflected, "I am not sure that this was a wise thing to do. We probably must look to interested industrial institutions for the bulk of our funds." Hitchcock to Fairchild, 27 March 1924. Folder "Barro Colorado Laboratory," David Fairchild Collection, FTG.

324. "Plan for Comprehensive Investigations of the Animal and Plant Life of South and Central America," 1920? Box 25, Folder 2, RU 135, SIA.

325. Ibid.

biology that could help to develop not merely tropical resources, but international good will. They believed that through IRTA American science could be a force, not for economic exploitation, but of hemispheric cooperation and development.

While most members at least paid lip service to these ideals, in practice the response to the question of whether Latin American individuals and organizations themselves would be permitted to join IRTA revealed lukewarm commitment. They discussed the possibility of opening the group to Latin Americans at length during their first meeting, but postponed decision.[326] A few members felt quite strongly about the issue. For example, Albert Spear Hitchcock, the committee's first Chairman agreed with associate member Franklin Sumner Earle, who "advises very strongly against limiting the control of the Institute in such a way as to exclude Latin-American scientists from full membership. He is emphatic [that this would] prevent any real cooperation of Latin-American institutions and that any implication of inequality would be deeply resented."[327] Hitchcock was a man of liberal social views; he was for example a supporter of women's rights and he often collaborated closely with Latin American scientists, treating them as equals.[328] He tried his best to push IRTA toward active cooperation with Latin American institutions, but on this issue as with others he was, as historian Pamela Henson has argued, "overshadowed by men of affluence and influence with different opinions"

326. "National Research Council Division of Biology and Agriculture Committee on Organization and Incorporation for the Institute for Research in Tropical America," 15 January 1921. Box 25, Folder 1, RU 135, SIA.

327. Hitchcock to "the members of the Institute for Research in Tropical America," 16 April 1921. Box 1, Folder 1, RU 134, SIA.

328. For example, see Pamela M. Henson, "Invading Arcadia."; Pamela M. Henson, "'What Holds the Earth Together': Agnes Chase and American Agrostology," *Journal of the History of Biology* 36, no. 3 (2003): 437-60.

on the committee.[329] Proposals encouraging the interchange of specimens with Latin American countries, the international exchange of naturalists, and the distribution of publications in both English and Spanish languished.

With a lack of consensus about who IRTA's allies should be and a hesitance to share resources among the member institutions themselves, the work of IRTA ground on with frustrating slowness. Barbour, with his fierce temper and a lack of patience for long futile meetings, showed particular irritation at its lack of action. "This N. R. Council committee is a stench in my nostrils," he wrote, producing a lot of "gas" and no practical outcomes.[330] He displayed particular venom for Hitchcock, who, in his opinion, spoke tediously during meetings and lacked decisiveness and the skill to win financial backing for any of IRTA's expansive schemes.[331] Barbour urged Hitchcock and the committee to focus on just one practical project to begin with: a research station in the Panama Canal Zone.[332]

The Canal Zone (CZ)—a ten mile wide strip of US territory carved out of the heart of the Republic of Panama by the 1903 Hay-Bunau Varilla Treaty—had been high on IRTA's list of sites from the start. Because of its accessibility and national interest, Panama had already

329. Pamela M. Henson, "Invading Arcadia," 582.

330. Handwritten note from Barbour (to Ruthven?) at the top of a copy of Barbour to Hitchcock, February 28 1922. Box 1, Folder 2, RU 134, SIA.

331. The animosity seems to have stemmed equally from the differences in their social and political views and their divergent social spheres. Hitchcock was not a member of the old-boy network and not well-versed in the niceties of fundraising. One minor incident illustrates the friction amid the group: Fairchild, whose letters are normally quite restrained and polite, privately railed against Hitchcock for his blunder in sending the IRTA circular "in a rubber stamped brown paper envelope" when Fairchild had gone through "the trouble and personal expense to have stationary printed for the Institution because I know how important it is to have things look neat...Hitchcock does not know how to handle these kind of things." Fairchild to Harper (marked as "Confidential" and "Not Sent"), 12 April 1924. Folder "Barro Colorado Laboratory," David Fairchild Collection, FTG.

332. Barbour to Hitchcock, 24 February 1922. Box 1, Folder 2, RU 134, SIA; "IRTA minutes of meeting of Exectutive Committee" 5 June 1922. RU 134, Box 1, Folder 2, SIA.

attracted the broad interest of American scientists. Most notably, an array of American universities and biological institutions had previously come together to initiate the 1910-1912 Smithsonian Biological Survey of the Panama Canal Zone, prior to the completion of the Canal in 1914.[333] IRTA's chairman, Hitchcock, was a specialist in the taxonomy of grasses working for both the USDA Bureau of Plant Industry and the Smithsonian National Herbarium, and had been one of the survey's botanists. Since 1916, Barbour himself had frequently visited the CZ during his travels throughout the Caribbean for his biogeographical research and to make collections for the Museum of Comparative Zoology (MCZ).

Perhaps more immediately, however, a project in the CZ would further Barbour's efforts to bring Harvard biology and medicine closer together through tropical work. Barbour had a connection to the newly established Gorgas Memorial Institute in Panama through Richard Strong, the Harvard Medical School's authority on tropical medicine, with whom he was working to try to create an Institute for Tropical Biology and Medicine at the university.[334] Both

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333. Box 42 and 43, RU 45, SIA

334. Although the Institute for Tropical Biology and Medicine did not take form at Harvard in the manner he had hoped, Barbour took the liberty, "in the spirit of hopefulness," of having several reports of Harvard tropical work printed with the proposed institute's name on their title pages—including work at Soledad; Thomas Barbour, "Tropical Research Institute at Harvard," 419. Gregg Mitman and Paul Erickson have examined Strong's work and the relationship between tropical medicine, biology, and business at Harvard, Gregg Mitman, and Paul Erickson, "Latex and Blood: Science, Markets, and American Empire." The Institute's six reports were Edward M. East, and William Henry Weston, *A Report on the Sugar Cane Mosaic Situation in February, 1924, at Soledad, Cuba*; Afrânio do Amaral, *A General Consideration of Snake Poisoning and Observations on Neotropical Pit-Vipers*, vol. 2, Contributions From the Harvard Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1925); George W. Salt, and John Golding. Myers, *Report on Sugar-Cane Borers at Soledad, Cuba*; Richard P. Strong, et al., *Medical Report of the Hamilton Rice Seventh Expedition to the Amazon, in Conjunction With the Department of Tropical Medicine of Harvard University, 1924-1925.*, vol. 4, Contributions From the Harvard Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1926); Richard P. Strong, *The African Republic of Liberia and the Belgian Congo*, vol. 5, Contributions From the Department of Tropical Medicine and the Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1930); Richard P. Strong, et al., *Onchocerciasis, With Special Reference to the Central American Form of the Disease*, vol. 6, Contributions From the Department of Tropical Medicine and the Institute for Tropical Biology and Medicine (Cambridge, MA: Harvard University Press, 1934).

men believed in “interdigitation of biology and medicine, especially in the tropics,” and a biological station in Panama, located next to the laboratory of the Gorgas Memorial Institute would fit well with their own ongoing plans.[335] Barbour’s involvement in the effort to unify Harvard’s many disparate botanical and zoological departments, each with separate endowments and institutional histories, must have already begun to feel like a Sisyphean task.[336] Nevertheless he hoped that tropical biology could serve as a model for the integration of biology’s subdisciplines and “cognate fields,” arguing that “biological teaching here at home has got to be rehoused and reorganized, and...a great future opportunity lies in knitting more firmly our many biological contacts with the tropics, already existing or coming into being, with our strong staff in tropical medicine.”[337] But Barbour knew Harvard was “altogether too poor” to initiate a new tropical biological station itself—or at least that, as at Soledad, the effort might be more successful if the station were established first, and Harvard’s relationship with it clarified later.[338] Thus, although Barbour found serious fault with IRTA, the organization could be useful to him.

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335. Thomas Barbour, “Tropical Research Institute at Harvard,” 419.

336. In addition to the Museum of Comparative Zoology, Zoological Laboratory, Bussey Institution for Applied Biology, and Medical School, botany alone was represented by the Gray Herbarium, Arnold Arboretum, Farlow Library and Herbarium, Botanical Museum, and the Harvard Forest, in addition to the station at Soledad. Efforts to unify Harvard’s botanical institutions in the 1940s would lead to “The Arnold Arboretum Controversy” and decades of lawsuits. See Morton Keller, and Phyllis Keller, *Making Harvard Modern: The Rise of America’s University* (Oxford: Oxford University Press, 2007), 101-03; G. H. Parker, “The New Harvard Biological Laboratories,” *Science New Series* 76, no. 1964 (1932): 158-62; Alfred Sherwood Romer, “Zoology at Harvard,” *Bios* 19, no. 1 (1948): 6-20. For an outline of the history of biological institutions and attempts to unify biology at Harvard, see Clark A. Elliott, “Chronological Overview of Harvard Science (1636-1945),” in *Science at Harvard University: Historical Perspectives*, ed. Clark A. Elliott, and Margaret W. Rossiter (Bethlehem, PA: Lehigh University Press Associated University Press, 1992).

337. Harvard University, *Reports of the President and the Treasurer of Harvard College, 1927-1928* (Cambridge: Harvard University, 1929), 20; Thomas Barbour, “More About Harvard Biology,” 213.

338. Wheeler to Zetek (discussing a recent meeting with Barbour), 28 May 1923, Box 25, Folder 2, RU 135, SIA.

In spite of Barbour and Hitchcock's personal differences, they easily agreed on the advantages of Panama. Hitchcock's extensive experience ten years earlier with the Smithsonian Biological Survey of the Panama Canal Zone had made him an advocate for IRTA involvement there from the start. The isthmus of Panama was an area of extreme biogeographical significance, mixing the North American and South American floras and faunas, and dividing the marine communities of the Caribbean and Pacific.[339] With a location in Panama City, next to the Gorgas Institute, moreover, there would be the possibility of adding a marine station, which would attract broader biological interest. As well as being "healthful" and in an area where "the dangers from political instability are slight," Hitchcock noted in racialized language the social advantages for American scientists of US control of the CZ, which had "a greater concentration of intelligent white people than can be found at any other point in the American tropics." But Hitchcock also believed that a station in the city would "be likely to receive the sympathetic cooperation of Latin-American scientists," cooperation which he believed was of the utmost importance for a successful tropical station.[340]

With these key members, Barbour and Hitchcock, in agreement, the Committee assented "unanimously" to the new plan for a Panama station.[341] Barbour checked on the progress of the Gorgas Memorial Institute during his 1922 travels in the region and Hitchcock began

339. Further insight into how scientists framed the biological problems of the Panamanian isthmus and CZ can be gleaned from the arguments that lead up to the initiation of the Smithsonian Biological Survey of the Panama Canal; Box, 42 Folder 14, RU 45, SIA.

340. Hitchcock, "Report on Panama," 1923. Box 4, Folder 3, RU 134, SIA.

341. "The advantages of a research station in Panama," 1922. Box 25, Folder 1, RU 135, SIA.

correspondence into conditions in the area.[342] His primary contact in Panama was an entomologist named James Zetek.

A Man, A Plan: Zetek in Panama

If it were not for Zetek, IRTA might indeed have established its station in Panama City rather than on a forest-covered island half-way across the isthmus in the middle of the Panama Canal. A fortuitous partnership emerged between Barbour and Zetek, one which would enable the establishment of BCI as IRTA's station, and which, ultimately, would allow BCI to overshadow its founding institution. Thomas Barbour and James Zetek were in some ways an unlikely pair, with very different class backgrounds and personalities. Barbour, a Harvard museum curator, was born into wealth. He moved comfortably within elite circles in Massachusetts, New York, and Florida vacation retreats. Zetek, a US government entomologist, had had to struggle to build and maintain support for his scientific and conservation work in Panama. Together, their work managing networks of science and patronage within the US and the CZ allowed BCI to emerge as a key site for American biologists to study tropical forest biology.

When Zetek first arrived in Panama in 1911 to work for the Isthmian Canal Commission, he was only 25 years old. The young entomologist had just graduated with an A.B. degree from the University of Illinois and was far from his native Chicago. Although from a Czech immigrant family of modest-means, he soon found a place in the tight-knit and exclusive society of the Canal Zone (CZ).[343] Like many American professional men in the CZ, Zetek married a

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342. "IRTA Circular of Information No. 3," 26 June 1922. Box 25, Folder 1, RU 135, SIA.

343. On Canal Zone Society, see Julie Greene, *The Canal Builders: Making America's Empire at the Panama Canal* (New York, NY: Penguin Press, 2009); Alexander Missal, *Seaway to the Future: American Social Visions and the Construction of the Panama Canal* (Madison, WI: University of Wisconsin Press, 2008). On American entomologists' role in the Canal Zone, see Paul Sutter, "Nature's Agents or Agents of Empire? Entomological

Panamanian woman, Maria Luisa Gutierrez. The marriage entered him into a prominent Panama City family. A Catholic with a command of the Spanish language, Zetek moved easily between the American CZ and the Spanish-heritage high society of Panama City—according to the *Panama Times* newspaper, he was "one of the very few America residents of the Canal Zone who speak and write Spanish fluently."[344] He was socially active. Zetek was a founding member of the Panama Canal Natural History Society, and belonged to both the multilingual Panama Rotary Club and the elite Union Club. Beyond working for the American CZ authorities, he was also employed by the Republic of Panama to organize the 1915-1916 National Exposition in Panama City, and from 1916 to 1918 he served as professor of biology and hygiene at the National Institute of Panama.[345] He felt very much at home in Panama, to the point that he actively turned down posts that would have taken him out of Panama and tropical work.

In 1920, the USDA Bureau of Entomology hired Zetek to study the biology of the Panamanian fruit flies that plagued important fruit crops, the CZ's second most important economy after the Canal itself. At this post, Zetek was not a narrow specialist, but responsible for "a considerable amount of miscellaneous work covering the entire animal and plant

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Workers and Environmental Change During the Construction of the Panama Canal."

344. Blank, R. G. "An Island Forest in the Panama Canal." *The Panama Times (South Seas & Caribbean Mail)*, 1925. , 5. In using marriage and language skills to move between social worlds, Zetek walked a well-trod path in the history of empire, commerce, and science; Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo, eds. *The Brokered World: Go-Betweens and Global Intelligence, 1770-1820* (Sagamore Beach, MA: Science History Publications, 2009).

345. "Mr. James Zetek," in *El 'Libro Azul' de Panamá: Relato e Historia Sobre la Vida de Las Personas Más Prominentes*, ed. William T. Scoullar (Panama City, Panama: El Bureau de Publicidad de la América Latina, 1917); "Zetek: Canal Zone Biologist," *Illinois Alumni News* April (1948): 10-11; Thomas E. Snyder, et al., "James Zetek, 1886-1959," *Journal of Economic Entomology* 52, no. 6 (1959); H. B. B., and Stewart H. Jadis, "James Zetek, 1886-1959," *Nautilus* 73, no. 4 (1960): 159-60; Stanley Heckadon-Moreno, *Naturalistas de Istmo de Panamá: Un Siglo de Historia Natural Sobre el Puente Biológico de Las Américas* (Balboa, Panamá: Instituto Smithsonian de Investigaciones Tropicales: Fundación Santillana para Iberoamérica, 1998).

kingdom."[346] The work took him from his combined office and residence in Ancon—the administrative center of the CZ, abutting Panama City—throughout the CZ and Panama, and he became acquainted with members of all levels of the CZ and Panamanian administrations through his work. He diagnosed a wide variety of problems, from identifying the presence of Fusarium wilt on banana plantations to ridding a "gold roll" golf course of troublesome earthworms.[347] But Zetek had greater ambitions for science, and for himself, in Panama—nature preservation.[348]

Zetek's interest in nature preservation might appear antithetical to his role as an economic entomologist, but it is less surprising given his educational background. At the University of Illinois Zetek had studied under some of America's most influential ecologists. In addition to Henry A. Gleason and Charles C. Adams, he had worked with Stephen A. Forbes, whose "The Lake as a Microcosm" would help spawn ecosystem ecology and whose approach to combining field study with experimental methods would profoundly influence Zetek's collaboration with Barbour and IRTA in the creation of BCI.[349] Zetek was also a member of the

346. Zetek to Quintance, 20 September 1922. Box 1, Folder 10, RU 134, SIA. Other letters in this folder contains examples of the wide variety of work Zetek was called on for the CZ. See also Zetek, "Report of the Canal Zone Field Station, July 1, 1922-June 30, 1923." Box 11, Folder 2, RU 135, SIA.

347. "Gold roll" was a classification for white, American, "skilled" Panama Canal laborers. For more on the segregated CZ labor system, in relation to work on BCI, see Chapter 4 and Julie Greene, *The Canal Builders*; Alexander Missal, *Seaway to the Future*.

348. Zetek also worked on other, less successful, plans around the same time, including lobbying for the institution of a plant quarantine and an agricultural experiment station for the CZ.

349. Zetek, "The History of Barro Colorado," 1949. Box 21, Folder 4, RU 135, SIA; Stephen A. Forbes, "The Lake as a Microcosm," *Illinois Natural History Survey Bulletin* 15 (1925): 537-50. On Forbes and his ideas about fieldwork see Robert E. Kohler, *Landscapes and Labscapes*, 39,54,82,136; Daniel W. Schneider, "Local Knowledge, Environmental Politics, and the Founding of Ecology in the United States: Stephen Forbes and "the Lake as a Microcosm" (1887)," *Isis* 91, no. 4 (2000): 681-705; Sharon E. Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago, IL: University of Chicago Press, 1985), 12-18.

Ecological Society of America (ESA) and was aware of the ESA's recently formed Committee for the Preservation of Natural Conditions.[350] This committee saw "advocacy for preservation as a form of applied ecology" and was chaired by the prominent animal ecologist Victor Shelford.[351] In contrast to other American efforts, the goal of Shelford's committee was "preservation for science," not conservation for future resource use or for aesthetic or recreational purposes.[352] Ecology, Shelford argued, as "a branch of biological science which obtains its inspiration in the natural order of original habitats, must depend upon the preservation of natural areas for the solution of many problems."[353] The committee sought to maintain sites as ecological baselines and for the long-term observation of natural processes, without interference by humans. Zetek's thinking was in line with the ESA Committee, and he soon became Shelford's contact in Panama.

Zetek was particularly critical of the recent CZ policy of encouraging small-holder banana plantations along the Canal, which he argued was destroying the native flora and fauna.

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350. Zetek eventually wrote a large section on BCI within the entry for Panama in the Committee's catalog of wilderness areas whose preservation should be advocated: Edward Alphonso Goldman, and James Zetek, "Panama," in *Naturalist's Guide to the Americas*, ed. Victor E. Shelford (Baltimore, MD: Williams and Wilkins, 1926).

351. Abby J. Kinchy, "On the Borders of Post-War Ecology: Struggles Over the Ecological Society of America's Preservation Committee, 1917-1946," *Science as Culture* 15, no. 1 (2006), 32. Although scientific activism was initially noncontroversial, tensions within the ESA over how far to stray from "pure" science into actively implementing preservation would ultimately lead to a split between ESA, as a "purely scientific" organization, and the Nature Conservancy, as an activist group. See Sara Fairbank. Tjossem, "Preservation of Nature and Academic Respectability: Tensions in the Ecological Society of America, 1915-1979" (Dissertation, Cornell University, 1994). It is also interesting to note that, although the historiographic focus has been on ESA's work within the US, Shelford's committee took its scope to be the entirety of the Americas.

352. Gina Rumore, "Preservation for Science: The Ecological Society of America and the Campaign for Glacier Bay National Monument," *Journal of the History of Biology* (2011): 1-38. See also Gina Rumore, "A Natural Laboratory, a National Monument: Carving Out a Place for Science in Glacier Bay, Alaska, 1879-1959" (Dissertation, University of Minnesota, 2009).

353. Victor E. Shelford, *Naturualist's Guide to the Americas* (Baltimore, MD: The Williams and Wilkins Company, 1926), 3.

During Canal construction, between 1903 and 1914, tens of thousands of West Indian laborers, as well as a smaller number of South Asians and southern Europeans, immigrated to Panama. With the Canal's completion in 1914, American authorities were faced with a large out-of-work, landless, and predominantly Black population that, contrary to unrealistic expectations, was not immediately ready to return to their countries of origin. To mitigate the possibility of unrest, as well as maintain a reservoir of labor for potential future projects, the CZ government created a land lease program in 1921. Destitute former Canal workers were allowed one to fifty hectares for cultivation.[354] These smallholders quickly became enmeshed in the powerful United Fruit Company's banana network. They cleared their leased land to grow subsistence crops for themselves and bananas to sell from the shores of Gatun Lake, from which point the fruit would pass through many hands on its way to breakfast tables up and down the US east coast.[355]

"The jungle is being cleared on all sides," Zetek reported with alarm to his superior at the Bureau of Entomology.[356] He was appalled by the rapid destruction of forest resulting from the land lease program and wanted "to see an official condemnation of the general practice followed here of clearing the land and <u>burning</u> everything."[357] Zetek never commented on the much larger destruction caused by the construction of the Canal itself. It was apparently unquestionable to him that the American Canal project represented a larger good for both the US and Panama. He

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354. Zetek to Quintance, 25 March 1923. Box 1, Folder 11, RU134, SIA.

355. For a history of the land lease program and associated banana boom, see Ashley David Carse, "Transportation Environments: The Politics, Ecology, and Infrastructure of the Panama Canal" (Dissertation, The University of North Carolina at Chapel Hill, NC, 2011), 79-117; Ashley Carse, "Nature as Infrastructure: Making and Managing the Panama Canal Watershed," *Social Studies of Science* (2012). On the broader cultural and environmental changes in both the US and Latin America that emerged out of the banana trade, see John Soluri, *Banana Cultures*.

356. Zetek to Quaintance, 25 March 1923. Box 1, Folder 11, RU 134, SIA.

357. Zetek to Governor Morrow, 19 April 1923. Box 25, Folder 2, RU 135, SIA. Underline is in original.

reserved his ire for the West Indian lessees and CZ bureaucrats who had allowed the leases in the first place. Explaining that "already some 1800 leases have been let out, and the result is a large amount of clearing which destroys the original tropical growth," Zetek sought the help of Canal officials and ESA scientists back in the US.[358] "The land in the Canal Zone is being leased out for agricultural purposes," he warned "and as a result practically all of it is cleared or will be shortly, thus leaving nothing of the original flora and very little of the original fauna."[359] Zetek focused his efforts not on directly petitioning against the land lease program or criticizing the Canal project itself, but on the idea of preserving at least a small, typical example of natural Panama for scientific research; "The Canal Zone," he considered, "is the best sample of a typically humid, tropical area close to the United States which is under our immediate control, and which is very accessible at all times."[360] "We must not allow this only example of our natural environment to become spoiled by man's destructive hand," he believed.[361] A "sample" of wild, original Panama should be preserved for ecological research—and perhaps also so that leaders would one day recognize their policy errors. After discussions with CZ land agents, he believed he had found just the place.[362]

358. Zetek to Hitchcock, 4 March 1923. Box 25, Folder 2, RU 135, SIA.

359. Zetek to Shelford, 22 April 1923. Box 25, Folder 2, RU 135, SIA.

360. Zetek to Quaintance, 25 March 1923. Box 1, Folder 11, RU 134, SIA.

361. Zetek to Erwin, 20 April 1923. Box 25, Folder 2, RU 135, SIA.

362. These agents were N. A. Becker and Colonel William C. Erwin. To pay tribute to Colonel Erwin's assistance, and perhaps to help Americanize BCI, Zetek worked hard to have BCI renamed Erwin Island. He wrote, "I certainly hope the non-euphonius name of Barro Colorado will be displaced by that of ERWIN." Clearly, aural tastes have changed! Zetek to Erwin, 20 April 1923. Box 25, Folder 2, RU 135, SIA.

That "sample" of original Panama was Barro Colorado Island and, although Zetek often claimed it was "virgin territory," it had a deep human and natural history.[363] Long before the construction of the Canal, Barro Colorado was a hill that oversaw vast changes in the landscape below. Capped by basalt and relatively flat-topped, the steep sedimentary flanks of Barro Colorado once radiated out into the forested Chagres valley. The Chagres River began to carve this deep, narrow valley, with the coalescence of the Panamanian isthmus 3 million years ago, cutting through the rising crust of the isthmus and rushing on its way from the continental divide to empty itself into the Caribbean Sea. Humans entered the valley around 9200 BCE. By 2900 BCE they had begun to practice shifting agriculture, cutting and burning plots in the forest. When the Conquistadors came, they used the Chagres to carry Incan gold from the Pacific coast. They called one of the larger hills along the river "Barro Colorado," or "red mud," paying homage to the thin layer of clay-rich soil that supported its towering forests. Indigenous people pursued very little agriculture high up on Barro Colorado, but with the Spanish Conquest and the onslaught of disease the valley lost much of its human population. For more than two hundred years, the forest went undisturbed, apart from selective cutting for mahogany. The first large-scale clearing took place in the 1880s. About half of Barro Colorado's forest fell during the French effort to build a sea-level canal—a few rusted remnants of French equipment can still be found there. Apart from disease, the powerful and highly variable flow of the Chagres River was one of the major obstacles to the failed French attempt. The river had been a path to promised riches—from the Spanish colonists and Henry Morgan's pirates, to thousands of American forty-niners journeying off to California. It was now a liability. Damming the Chagres allowed the

363. Zetek to Hitchcock, 4 March 1923. Box 25, Folder 2, RU 135, SIA.

Americans to control the erratic river. It also created a reservoir, Gatun Lake, that allowed the locks on both sides of the Canal to operate even during dry spells. Gatun Lake, at 166 square miles, soon became the worlds largest artificial lake. Between 1907 and 1913 its waters slowly rose, inundating over a hundred thousand acres of forest, and transforming Barro Colorado from a hilltop into the lake's largest island.[364]

A "quick tie-up"

But Zetek made little progress with the idea of a biological preserve until he met the Harvard scientists William Morton Wheeler, Richard Strong, and Charles V. Piper of the USDA in February of 1923. Albert Spear Hitchcock, IRTA's chairman, had asked Wheeler and Piper to look into conditions in Panama for the establishment of the institute's research station during their trip. Wheeler was a world leader in ant taxonomy and behavior.[365] Piper was an expert in grasses, and knew Wheeler from his time on fellowship at Harvard. As mentioned in the previous section, Strong had come to the CZ to help establish the laboratory of the newly formed Gorgas Memorial Institute of Tropical and Preventive Medicine, and he must have already discussed IRTA's plans with Barbour by this time.[366] Zetek shared his USDA laboratory with

364. This physical description and environmental history draws on Egbert Giles. Leigh, et al., *The Ecology of a Tropical Forest: Seasonal Rhythms and Long-Term Changes* (Washington, DC: Smithsonian Institution Press, 1982); Richard G. Cooke, et al., "Native Americans and the Panamanian Landscape," in *Case Studies in Environmental Archeology*, ed. Elizabeth J. Reitz, Lee A. Newsom, and Sylvia J. Scudder (New York, NY: Plenum Press, 1996); Egbert Giles. Leigh, *Tropical Forest Ecology: A View From Barro Colorado Island* (New York, NY: Oxford University Press, 1999); R. S. Harmon, *The Río Chagres, Panama: A Multidisciplinary Profile of a Tropical Watershed* (Dordrecht, Netherlands: Springer, 2005); Aims McGuinness, *Path of Empire: Panama and the California Gold Rush* (Ithaca, NY: Cornell University Press, 2008).

365. Mary Alice Evans, and Howard Ensign Evans, *William Morton Wheeler, Biologist* (Cambridge, MA: Harvard University Press, 1970).

366. The institute was named for General William C. Gorgas, who was celebrated in the US for his mosquito control efforts in Florida, Cuba, and Panama, which gratly reduced the incidence of yellow fever and malaria. Willard Hull Wright, *40 Years of Tropical Medicine Research: A History of the Gorgas Memorial Institute of Tropical and Preventive Medicine, Inc. And the Gorgas Memorial Laboratory* (Baltimore, MD: Reeves Press,

Wheeler and Piper during their two-month visit and took them on many field trips throughout the CZ and Panama. Strong frequently joined them on these excursions and Zetek shared his hopes for nature preservation and science in the CZ.[367]

They responded with enthusiasm. Zetek's ambitions fell right in line with IRTA's plans, already in process in Washington DC. Zetek became IRTA's primary contact; his position as the Panamanian representative to the ESA and a USDA field agent made him an obvious choice. Following Hitchcock's inquiries, events on the ground in Panama moved quickly. The two plans—a station in Panama, a nature reserve on BCI—could be combined. After meeting and learning of the availability of BCI for a reserve, Wheeler, Strong, and Piper urged Zetek to contact Governor J.J. Morrow to ensure the island's preservation. The Governor, eager to show his interest in a progressive cause, quickly agreed to Zetek's request. On April 17, 1923, BCI became a natural park.[368] To Governor Morrow and the Canal Zone administration, BCI was worth more for the political cachet of visibly supporting science and conservation than for any strategic or economic purposes. In spite of language extolling the place as "virgin" rainforest, however, like much of the land along the Canal, four former Canal workers had recently leased a

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367. Zetek, "Report of the Canal Zone Field Station, July 1, 1922-June 30, 1923." Box 11, Folder 2, RU 135, SIA; Zetek, "The History of Barro Colorado," 1949. Box 21, Folder 4, RU 135, SIA. The latter document, a brief memoir written by Zetek many years later, is reliable in as far as it describes Zetek's own biography and general events surrounding the establishment of BCI, but it is evident from contemporary correspondence that Zetek omitted key information related to the role of A. S. Hitchcock and IRTA, as we will see in the next section, were significant. Thus, in Zetek's account, he makes himself the originator of the idea of BCI as a nature preserve and as a research station. Other letters show, however, that Hitchcock specifically asked W. M. Wheeler and C. V. Piper to discuss the possibility of an IRTA station in the CZ with Zetek during their February 1923 visit. Thus, while it is clear that Zetek was long interested in nature preservation in the CZ, he learned of the idea of a tropical research station from Hitchcock, Wheeler, and Piper. See in particular Hitchcock to Zetek, 19 February 1923. RU 135, Box 25, Folder 2.

368. Morrow, "The Panama Canal, Canal Zone, Executive Office," 17 April 1923. Box 25, Folder 2, RU 135, SIA.

small portion of the island.[369] They grew subsistence crops, such as yuca and corn, and bananas to sell to middlemen, who in turn delivered them to United Fruit. Barbour personally paid these men to give up their leases and leave when the island became a scientific station—unsurprisingly, their period of habitation was rarely discussed in early literature. Governor Morrow assured that "No further leases for land on the Island will be issued... Hunting will not be allowed except for strictly scientific purposes." The caveat remained, however, that if the Canal Administration did ever find itself in need of the island, its reserve status could be revoked.[370] It was an effortless act for the administration, since it did not actually make itself liable to support the BCI reserve financially or administratively. Zetek and the others would have to secure funding through the NRC, or elsewhere, to develop and maintain BCI for science.

Meanwhile, Wheeler leapt into action, setting his extensive scientific network in motion. Upon his return from Panama in June, Wheeler discussed the potential of BCI as the site of a research station with his influential friends at the Woods Hole Marine Biological Laboratory. Once he was back in Boston, he got in touch with Barbour. He also wrote to David Fairchild, the globe-trotting USDA plant explorer who enjoyed the backing of several wealthy patrons—Wheeler had met him while studying in Germany and at the Naples Zoological Station many years before.[371] The three were particularly close friends, and since Barbour was on IRTA's executive committee, there was an opportunity for them to sidestep Hitchcock and the slow-moving bureaucracy of the NRC. Wheeler and Fairchild shared Barbour's antipathy toward the

369. Statement reliquishing leases, signed by Alman Challa, Manuel Ortega, Temas Polo, and Sixto Rivera, February 18 1924. Box 25, Folder 3, RU 135, SIA.

370. Morrow to Zetek, 16 April 1923. Box 25, Folder 2, RU 135, SIA.

371. Joel B. Hagen, Oral History Interview with Graham Bell Fairchild, 7 June 1989. RU 009559, SIA.

NRC and Hitchcock, who was not part of their affluent social circle. They preferred to work through their own network of wealthy businessmen and philanthropists. Fairchild, through long experience with both government work and private patronage, especially loathed the possibility that BCI might become entangled in "any kind of red tape" in Washington.[372] "It will be quite a joke to get the whole thing started before the N.R.C. gets into action!" Wheeler exclaimed to Zetek.[373]

Barbour contacted his friends at the United Fruit Company, securing five free passes for credentialed scientists traveling to BCI on United Fruit Company steamers.[374] Barbour, who was "particularly flush in 1923" from stock market speculation, gave over a thousand dollars of his own funds.[375] Fairchild, for his part, tapped his millionaire friends Allison Armour and Barbour Lathrop, convincing them to provide the crucial funds to construct a laboratory building on BCI. Neither Armour nor Lathrop, a socialite who had inherited his fortune, had a direct economic interest in biological research in the tropics. But they supported the overall project of American expansion into Latin America and the general idea of science's role in this process. Moreover, they financed BCI as a personal favor to Fairchild. The few thousand dollars that they gave was small to them, but absolutely necessary for building a laboratory on BCI and making it a tropical outpost attractive to American biologists.

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372. For example, Fairchild to Harper (marked as "Written but not sent"), 12 April 1924. Folder "Barro Colorado Laboratory," David Fairchild Collection, FTG.

373. Wheeler to Zetek, 20 June 1923. Box 25, Folder 2, RU 135, SIA.

374. The number and discount on United Fruit Company passes varied over the years. On the United Fruit Company and BCI, also see Paul C. Standley, "The Debt of Natural History to the United Fruit Company," *United Fruit Company* 6, no. 12 (1931): 567-69.

375. Over the years, Barbour's own contributions would be crucial in helping BCI to make ends meet. Thomas Barbour, *Naturalist at Large*, 195.

Consolidating their control, Barbour and Wheeler made sure that Fairchild replaced Hitchcock when his term as Chairman of the IRTA executive committee ended in 1924, and Barbour soon took over the role.[376] By this time, however, IRTA had collapsed into a formality. It no longer held regular meetings; indeed "to all intents and purposes the Laboratory was the Institute [IRTA]." The official connection to the NRC was important, though. While Barbour and his friends felt they could better control BCI through private patronage networks, they needed nominal government affiliation for BCI to succeed in the highly regimented CZ. As Barbour explained, "Federal recognition...was necessary if we were to operate efficiently in the Canal Zone;" the "arrangement" with the NRC "made it possible for us to give visiting scientists commissary privileges, hospital facilities, railroad passes, free entry through customs, and the right of residence in the Canal Zone," as well as the purchase and delivery of "ice and all other supplies...by the Commissary Department of the Panama Canal." Without this "quick tie-up, the whole development of the Laboratory would have been long delayed."[377]

Conclusion

In the end, the results of IRTA's efforts took a very different form than its members initially imagined. The brief history of IRTA reveals how a network of science and patronage coalesced around BCI. Historian of science Joel Hagen has examined the financial difficulties that BCI sustained in the early years of its establishment.[378] While the networks that sustained the station

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376. Fairchild briefly held the chairmanship, but Barbour served as acting chair in Fairchild's absence for much of this period. Barbour and Fairchild also lobbied to amend the IRTA constitution to allow John R. Johnston of the United Fruit Company to serve on IRTAs executive committee even though he did not represent an academic institution.

377. Ibid., 196.

378. Joel B. Hagen, "Problems in the Institutionalization of Tropical Biology: The Case of the Barro Colorado

through its first few decades were indeed fragile, they were sufficient to allow BCI to become the longest lasting tropical biological station in the Western hemisphere. By examining how IRTA's plans, hatched in Washington DC, coalesced with Zetek's dealings in Panama, we get a better sense of what tropical biology looked like in the 1920s—in the minds of interested American scientists, in their relationships with each other, and on the ground in the establishment of BCI. Indeed, by examining IRTA's abortive plans, we can trace BCI's network more clearly by seeing what could have but did not become a part of it. Its ultimate shape had serious consequences for what science on BCI would look like in practice.

Most obviously, BCI did not emerge as a joint venture with Latin American institutions. Although a few of its members strongly argued for international collaboration, IRTA's lack of action set a precedent; few Latin American or even Panamanian scientists worked at BCI until the end of the twentieth century.[379] Yet, BCI also was not the grand national cooperative effort among US scientific institutions that IRTA originally intended. As in the case of the Cinchona Botanical Station (See Chapter 1), many scientific institutions were interested in accessing the site, but few were willing to contribute regularly to its upkeep. As Hagen has explained, the "table system," whereby scientific institutions were supposed to pay for a share of laboratory workspace, worked poorly at BCI.[380] Although, as we will see in the next chapter, a broad slice of the American biological community—one that would move well beyond the East Coast

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Island Biological Laboratory," *History and Philosophy of the Life Sciences* 12 (1990): 225-47.

379. Only since 1974 has it, as part of the Smithsonian Tropical research Institute, had BCI had formal relations with the Panamanian government. See Chapter 5 and Catherine A. Christen, "At Home in the Field: Smithsonian Tropical Science Field Stations in the U. S. Panama Canal Zone and the Republic of Panama," *The Americas* 58, no. 4 (2002): 537-75.

380. Joel B. Hagen, "Problems in the Institutionalization of Tropical Biology: The Case of the Barro Colorado Island Biological Laboratory."

elite—came to call BCI a home away from home, its finances and administration remained in the hands of Barbour and a cadre centered around Harvard and Washington DC.

While BCI never became part of any larger national effort of American tropical exploration, it did, as some IRTA members had hoped, become part of a network of research stations in a more informal way. Because of Barbour's custodianship of Harvard's station at Soledad (see Chapter 2), BCI visitors sometimes stopped first at Soledad. In fact, BCI visitors often circulated among BCI, Soledad, and the United Fruit Company's experiment stations (particularly Tela, Honduras) as well as other small private experiment stations, plantations, and gardens throughout the region. Barbour's friendships at the United Fruit Company and with Fairchild, with his own vast connections with the botanical community, often facilitated such itinerant Caribbean station "hopping."

Perhaps most importantly, rather than being directed by the state or even national scientific institutions, BCI emerged through ad hoc partnerships among individual scientists and philanthropists. This gave BCI a rare degree of independence and flexibility. Government and corporate assistance was crucial, but not overpowering. While many IRTA members clearly hoped for funding for "pure" tropical research with no strings attached, Barbour was prepared to deal with realities. Yet, as at Soledad, Barbour made sure that corporate funding would sustain but not control science at BCI. Such funding would be weak, but that weakness would also grant a kind of freedom to pursue basic research.

Chapter 4

"Jungle Island": Taming Tropical Biology

The Laboratory in the Jungle

In *Walden*, Henry David Thoreau wrote about transforming dreams into reality: "If you have built castles in the air, your work need not be lost; that is where they should be. Now put the foundations under them."[381] Referencing Thoreau, the distinguished ornithologist Frank Chapman of the American Museum of Natural History called Barro Colorado Island (BCI) his "tropical air castle," publishing under that title one of the earliest popular accounts of scientific research on the island.[382] Chapman had long dreamed of finding a home in a tropical forest, a place where he could observe nature in the tropics with the same intimacy and familiarity that Thoreau had experienced life in the woods of Massachusetts. "Camps I have had in many strange and beautiful places," Chapman wrote, "and I have occupied native dwellings in remote corners of forest and mountain. But none has been a real home and few have been in truly primeval surroundings." He longed for "a residence in a primitive tropical region," but one "where living conditions imposed no handicap of hardship or discomfort on my powers of observation or enjoyment." Chapman had despaired of ever fulfilling this fantasy, until he "found castle and foundation complete on Barro Colorado, an island in the Panama Canal Zone."[383]

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381. Henry David Thoreau, *Walden: A Fully Annotated Edition* (New Haven, CT: Yale University Press, 2004), 315.

382. Frank Michler Chapman, *My Tropical Air Castle: Nature Studies in Panama* (New York: Appleton & Co., 1929).The book was an edited compilation of Chapman's series of essays for *Natural History*. He followed it up with a sequel: Frank Michler Chapman, *Life in an Air Castle: Nature Studies in the Tropics* (New York: D. Appleton-Century Co., 1938).

383. Frank Michler Chapman, *My Tropical Air Castle*, viii-ix.

Chapman became one of the island's earliest "regulars," following the migrant birds of the Americas south to BCI each winter for more than a decade.[384] The tropical island's location within US colonial territory and proximity to the Panama Canal—the hub of world commerce—made it unusually accessible to American biologists. Add to this accessibility the publicity of Chapman's own writings and publications like Warder C. and Marjorie H. Allee's "Jungle Island," and BCI quickly became transformed into a major destination for American scientists who wanted to experience an accessible but "untouched" remnant of tropical forest.

In Chapman's view, the island was "an essentially complete faunal unit—a little world in itself." Its "inestimable value to the ecologist" came from its insulation, and hence its controlled and bounded environment, which allowed it to act as "a natural laboratory."[385] Naturalists and ecologists had a longstanding interest in observing pristine nature, unmodified by humans—even if such land was difficult, if not impossible, to find in practice.[386] This pursuit of wilderness, however illusory, extended into the tropics. And, to Americans, tropical wilderness meant one

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384. On Chapman's expansion into Latin American ornithology and relations with Colombian scientists, see Camilo Quintero, "Trading in Birds: Imperial Power, National Pride, and the Place of Nature in US-Colombia Relations," *Isis* 102, no. 3 (2011): 421-45. With his combination of extended visits and periodic return to the centers of American science, Chapman and many of BCI's researchers practiced what Robert Kohler has termed "residential science," an intensive mode of field practice, "which involves knowing a research locale as intimately its human or animal residents know it, but also as generally as do cosmopolitan scientists." Robert E. Kohler, "Paul Errington, Aldo Leopold, and Wildlife Ecology: Residential Science," *Historical Studies in the Natural Sciences* 41, no. 2 (2011), 216.

385. Frank Michler Chapman, *My Tropical Air Castle*, 3-4.

386. For example, see discussion of the Ecological Society of America's Committee on the Preservation of Natural Conditions in the previous chapter. On scientist's pursuit of pristine nature more generally, see Daniel B. Botkin, *Discordant Harmonies: A New Ecology for the Twenty-First Century* (New York, NY: Oxford University Press, 1990), 194-97; Robert E. Kohler, *Landscapes and Labscapes*; Donald Worster, *Nature's Economy* (San Francisco, CA: Sierra Club Books, 1977), 389. In environmental history, the debate over the place of non-human nature in historical narratives was ignited by William Cronon, "The Trouble With Wilderness: Or, Getting Back to the Wrong Nature," *Environmental History* 1, no. 1 (1996): 7-28; William Cronon, ed. *Uncommon Ground: Rethinking the Human Place in Nature* (New York, NY: W.W. Norton & Co., 1996).

thing: "the jungle."[387] For most researchers, organizing an expedition into deep tropical wilderness was prohibitively expensive and difficult. As we have seen, Soledad and other corporate-financed stations in the region eased this difficulty by serving as stopping-off points for research further afield. However, stations with a focus on economic botany were, unsurprisingly, located within agricultural landscapes. In contrast, Thomas Barbour and James Zetek designed BCI specifically as an access point to wilderness in the tropics. BCI earned a reputation as "jungle island," a fragment of tropical nature to be preserved forever for science.[388]

The irony that BCI, a nature reserve and biological station, was situated in the middle of the most ambitious project to engineer nature that the world had yet seen was not lost on its visitors. For them, BCI came to represent the natural Panama that had been sacrificed as the isthmus took its place as the linchpin of global trade. Chapman and many frequenters of BCI contrasted the apparent primitiveness of the jungle on BCI with the busy signs of commerce and civilization visible beyond the island's shore on the waters of the Canal. In doing so, they again echoed Thoreau, whose meditation on the whistle of a locomotive that pierced the quiet of Walden's woods evoked the jarring and ambiguous image of the "machine in the garden."[389] David Edward Starry, a travel writer who stayed on BCI for an extended period wrote,

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387. Kelly Enright has explored "the jungle" as an extension of the American wilderness ideal within the writing of several naturalist-popularizers, including David Fairchild and William Beebe, who we encountered in the previous chapter: Kelly Enright, *The Maximum of Wilderness*; Kelly Enright, "Gallery: On the Jungle," *Environmental History* 13, no. 3 (2008): 556-61.

388. This moniker "jungle island" was coined in a book for junior high school students by the ecologist W. C. Allee and his wife, children's author M. H. Allee. Theirs was one of the first of many popularizations of science on BCI: Warder Clyde Allee, and Marjorie Hill Allee, *Jungle Island* (Chicago and New York: Rand McNally and Company, 1925).

389. Leo Marx's analysis of this literary trope is a classic in environmental history; Leo Marx, *The Machine in the Garden: Technology and the Pastoral Ideal in America.* (New York, NY: Oxford University Press, 1964), 15, 23.

> My cabin was on the borderline between civilization and the jungle...My back and side windows looked out on an area that held its own against the advances of man—a veritable laboratory of primeval life. From my front window I could see ships from all parts of the world passing through Gatun Lake...pushing their blackened funnels above the green of the forests.[390]

Chapman himself commented on the "strange setting" of BCI, surrounded by "water through which pass ships of the Seven Seas."[391] He juxtaposed island life with the evidence of civilized society beyond, recalling "several Coatis, a band of Collared Peccaries, a family of Howling monkeys, and the Duke and Duchess of York passed my door...the last-named were on H. M. S. *Renown,*" which he could spy in the distance through his binoculars.[392] Or, as the visiting nature writer John C. Van Dyke put it, "Civilization stops out there in the channel where the ocean steamers go by and nature begins here at the boat landing. Back to nature! Well, here it is."[393]

For all the talk of BCI as a primitive wilderness, then, the island is perhaps better thought of as pastoral, a middle landscape shaped by a confluence of nature and human labor.[394] Not only was the island itself artificial, a byproduct of the construction of the Canal, but, just as Thoreau had depended on supplies from Concord, the maintenance of scientific life on BCI relied on

390. David Edward Starry, "A Jungle Island in Panama: The Strange Pageant of Primeval Life," *Travel* 63, no. 2 (1934), 15.

391. Frank Michler Chapman, *My Tropical Air Castle*, 27.

392. Ibid., 28.

393. John C. Van Dyke, "Barro Colorado; the Wild-Life Preserve," in *In the West Indies: Sketches and Studies in Tropic Seas and Islands* (New York: Charles Scribner's Sons, 1932), 210. Similar examples can be found in Blank, R. G., "An Island Forest in the Panama Canal."; Alfred O. Gross, "A Jungle Laboratory - Companions of the Wild at Barro Colorado Island," *Nature Magazine* 15, no. 1 (1930): 11-15.; Zetek to Barbour, 6 November 1924. Box 22, Folder 6, RU 135, SIA.

394. The idea of the middle landscape comes from Leo Marx, *The Machine in the Garden*. See also Howard P. Segal, "Leo Marx's "Middle Landscape": A Critique, a Revision, and an Appreciation," *Reviews in American History* 5, no. 1 (1977): 137-50.

replenishment from the wider commercial world that those passing steamers represented. It depended not on absolute isolation, but on the careful moderation of flows into and out of the island—flows of visiting scientists, laborers, tourists, funds, equipment, and organisms. Perhaps even more fundamentally, as a "laboratory," it was a landscape of scientific labor, a working landscape for biologists. Chapman could live and work in this tropical home away from home not because he was in utter exile from civilization, but because BCI was a middle landscape between wilderness and civilization. To use another metaphor, BCI's ecosystem became a hybrid of nature, society, and technology—an "envirotechnical system" or even a "social-ecological system"—assembled for biological research.[395]

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395. "Envirotechnical system" comes from recent work at the intersection of environmental history and the history of technology ("envirotech"). This concept incorporates the environment into the older concept of a "sociotechnical system," while also drawing on actor-network theory. As historian Sara Pritchard explains, envirotechnical analysis examines "historical interactions between ecological and technological systems, both materially and discursively," framing "technology, society, and nature as mutually constitutive." Pritchard, Sara B., *Confluence: The Nature of Technology and the Remaking of the Rhône* (Cambridge, MA: Harvard University Press, 2011), 13. See also Robert Gardner, "Constructing a Technological Forest: Nature, Culture, and Tree-Planting in the Nebraska Sand Hills," *Environmental History* 17, no. 2 (2012): 219-43; Martin Reuss, and Stephen H. Cutcliffe, *The Illusory Boundary: Environment and Technology in History* (Charlottesville: University of Virginia Press, 2010); Jeffrey K. Stine, and Joel A. Tarr, "At the Intersection of Histories: Technology and the Environment," *Technology and Culture* 39, no. 4 (1998): 601-40. A "social-ecological systems" approach (SES), in contrast, is used in sustainability studies to highlight the complex interaction between human societies and ecologies, as a way to overcome the historical separation of the natural sciences and social sciences in the study of environmental problems. See for example Richard B. Norgaard, and Paul Baer, "Collectively Seeing Complex Systems: The Nature of the Problem," *BioScience* 55, no. 11 (2005): 953-60; Fikret Berkes, et al., *Navigating Social-Ecological Systems: Building Resilience for Complexity and Change* (Cambridge ; New York, NY: Cambridge University Press, 2003); C. S. Holling, "Two Cultures of Ecology," *Conservation Ecology* 2, no. 2 (1998): 4; Carl Folke, "Conservation, Driving Forces, and Institutions," *Ecological Applications* 6, no. 2 (1996): 370-72. The two terms have quite different disciplinary contexts and connotations, not least of which is that envirotech generally operates within the realm of historical analysis and critique, while SES usually sees present-day problem-solving as an explicit goal and more actively seeks to inform policy-making. The influence of systems thinking on both, however, suggests the possibility of further interdisciplinary conversation. In spite of their distinct intellectual histories, both emerged in efforts to fuse adjacent disciplines to better confront the complexity of relationships between nature and culture—indeed with the hope of breaking down this very binary. In this chapter, I draw on both areas of scholarship—although more directly from envirotech. In doing so, I hope to call attention specifically to the ways that scientific labor and knowledge production are also embedded in and the products of such systems, a factor that has not always been brought to the fore in such analyses. At the same time, this systems approach allows me to interrogate how place shapes science in a way that makes "place" the outcome of multiple environmental, social, and technological interactions, rather than a stable category.

As this chapter will reveal, the project of making BCI a place for science transformed the natural and social space of the island. BCI's administrators intensely managed life on the island, seeking a delicate balance between allowing natural processes to proceed and providing ever closer access for scientific observation. This system required constant adjustment. Trails had to be kept clear, poachers deterred, animal populations controlled, and facilities kept in working order. Over time, trails spiderwebbed across the island and ladders rose up into the canopy. Day after day, instruments measured light and rainfall in the forest, while the lenses of cameras and binoculars captured the movements of the island's more elusive creatures. Moreover, visitors needed a place not only to work, but also to sleep and eat. A small community of laborers inhabited BCI, tending to the physical needs of visitors, maintaining the island's buildings and equipment, and taming its sometimes unruly flora and fauna.

BCI was not simply a piece of wild Panama set aside for study. It was actively constructed to facilitate biologists' access to tropical nature, to transform it into what visitors came to call "a scientist's Eden" and a "jungle laboratory."[396] The frequent use of such incongruous descriptors hints at BCI's synthesis of primitive nature and modern scientific culture. Like a laboratory—or indeed a botanical garden or museum—the system for research constructed at BCI mediated and ordered the researcher's engagement with nature. But unlike laboratory science, the object of observation was the complex, living ecology of a particular place. At BCI, biologists did not wish to disrupt natural life on the island, but neither were they

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396. For example Thomas E. Snyder, "Barro Colorado Island - a Scientist's Eden in a Wild Life Preserve," *Pan American Magazine* 42, no. 5 (1930): 331-34; Alfred O. Gross, "A Jungle Laboratory - Companions of the Wild at Barro Colorado Island."

able to observe without any form of mediation. Rather, in order to observe, biologists became a part of a hybrid ecosystem—they made "jungle island" their home.

Managing Island Life: Visitors, Laborers, Technologies, Organisms

Chapman wrote, "the damming of the Chagres made Barro Colorado an island but it did not make it an island laboratory."[397] For BCI to function as a laboratory, and specifically as a biological station for American visitors, required management. If, as we saw in the previous chapter, it was Barbour who marshaled BCI's external ties with American scientific institutions, government, and corporate sponsors, Zetek largely had a free hand in the daily operation of BCI. Barbour shared with Fairchild a philosophy that "such a laboratory succeeds or fails according to the management which is given it locally," not according to "anything which a committee in Washington can do."[398] As the man on the ground, they appointed Zetek "custodian" of BCI.[399]

For more than thirty years, until his retirement in 1956, Zetek presided over life on BCI. He made at least weekly visits from his office in Ancon, shepherding researchers and tourists alike from BCI's dock up the two hundred stairs to the top of laboratory hill. He recruited local workers and closely monitored their movements, fearful of transporting disease onto the island. He begged and borrowed, always on the lookout for surplus Army and CZ equipment that could enrich BCI's scientific infrastructure. His intimate knowledge of the "who's who" of the CZ and Panama City helped him to win favors for BCI and its visitors.[400] Although two thousand miles

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397. Frank Michler Chapman, *My Tropical Air Castle*, 1.

398. Fairchild to Harper, 12 April 1924. Folder "Barro Colorado Laboratory," David Fairchild Collection, FTG.

399. He continued to work for and receive his salary through the USDA, which Barbour considered as essentially the USDA's semiformal contribution to BCI.

400. Sandy Fairchild was among many who recollected Zetek's skill in navigating the Canal Zone hierarchy. Joel B.

away, he also maintained close contact with Barbour in Cambridge (or during Barbour's seasonal travels, his secretary, Helene Robinson). A long litany of updates, complaints, gossip, and, frequently, pleas for more money made their way to Barbour in the morning mail—"Give us this day our daily Zetek" he would call out to Mrs. Robinson upon entering his MCZ office each day.[401] Patience sometimes wore thin on both sides, but as long as Barbour was able to keep funds and visitors flowing into the station, Zetek kept the "machinery" of life on BCI well-oiled. In spite of articles extolling BCI's natural virtues as a field site, we will see that what made it an ideal place for scientific research was in reality the smooth integration of BCI's labor force, technologies, and ecosystem, in tandem with visiting scientists' needs and expectations.

Visitors: "A New Species of Mammal"

On the question of how to make BCI a "home" for scientists, Chapman wrote, "Our problem, in effect, has been to introduce a new species of mammal into the life of the island under conditions favorable to its existence and in no sense objectionable to those already living there."[402] Visitors were an inherent threat to the natural state of the island—because that "natural state" was conceived of as one without humans. Yet, without visitors, there would be no research station at all. To work around this contradiction, BCI's founders classified multiple "varieties" of human visitors, welcoming some, while excluding others. "Legitimate" researchers could be trusted not to disrupt BCI's natural balance. By controlling the flow of people onto the island, Barbour and Zetek sought to maintain BCI as place for science.

Hagen, Oral History Interview with Graham Bell Fairchild, 7 June 1989. RU 009559, SIA.

401. Ibid.

402. Chapman, Frank Michler. "In an Eden Where Man Befriends Beast." *The New York Times*, 1932, 6, 16.

For BCI to function, Barbour and Zetek had to attract a migrant population of American scientific visitors. They knew that BCI's biggest advantage was also its most significant disadvantage to attracting the biologists they wanted: it was a station in the tropics, far away from the centers of American science. They not only had to convince scientists with little tropical experience that BCI was safe and healthy, but also to help visitors overcome the cost of travel in order to make BCI attractive. Through Barbour and Zetek's efforts "credentialed scientists" won special travel concessions. With steamers from all parts of the world passing daily through the locks of the Canal, Panama had rapidly become the most accessible point in the Western tropics.[403] Still, for the majority American biologists, the cost of travel was significant. In the mid-1920s, one-way first class fares from New York to Colon, Panama's Caribbean port, ranged from \$100 to \$145.[404] The United Fruit Company agreed to provide a limited number of annual passes on its steamers (one of the largest and most frequently running lines). The US army also authorized extra space on army transports, while the Panama Railroad Company allowed BCI researchers the \$50 rate normally reserved for CZ employees on its steamers, as well as a pass on the Panama Railroad. Once in Panama, visitors boarded the train to the Frijoles railway station, about halfway across the isthmus. At the Frijoles docks, visitors could hire a Panamanian boatman (*lanchero*) to ferry them to BCI in about an hour.[405] As Barbour's colleague William "Cap" Weston explained, "This distant frontier outpost of biological activity" was "obviously... a far cry from most of Harvard's biological organization, yet [it is] little more than a comfortable

403. See for example, "Steamer Routes, Isthmus to other parts of the world" in Hitchcock, "Report on Panama," 1923. Box 4, Folder 3, RU 134, SIA.

404. Hitchcock, "Report on Panama," 1923. Box 4, Folder 3, RU 134, SIA.

405. Warder Clyde Allee, "The Barro Colorado Laboratory," *Science* 59, no. 1537 (1924), 521.

seven days from New York."[406] Reduced rates significantly lowered the barriers for scientific visitors during the station's early years and during the Great Depression.[407]

Barbour and Zetek worked hard to attract visitors, but they had to be the "right kind"—legitimate researchers who could be trusted to preserve the natural appearance and scientific atmosphere of BCI. Barbour vetted prospective new additions to "the family group at B. C. I."[408] To get permission for an extended stay, one had to be a "credentialed scientist"—that is, to have a formal relationship with a scientific institution and a recognized status within the scientific community. Recommendation by a respected scientific authority or personal friend of Barbour's was highly valuable. In the 1920s and 1930s, the number of scientists on the island at one time hovered around a half a dozen, and were mainly PhDs with previous research experience.[409]

Other kinds of visitors were not so welcome. Panamanian hunters, who became illegal poachers with the Governor's reservation of BCI, were seen as endangering the island's natural animal populations and therefore scientists' observations of BCI's fauna. Although completely excluding poachers from BCI was never possible, these were clearly unwanted visitors in Barbour and Zetek's minds. The same was true of tourists: like poachers, Barbour and Zetek agreed, tourists might upset the natural balance and appearance of the island by littering, cutting

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406. Weston was a mycologist who also frequented Soledad. William H. Weston Jr., "The Biological Station on Barro Colorado Island," *Harvard Alumni Bulletin* December 19 (1929).

407. In an oral history interview, Robert K. Enders, a biologist who stayed on BCI for long periods of time, recalled that, with the discounts, the cost of travel to BCI was reasonable. Although the cost of a visit was not cheap, he didn't believe cost alone would prevents scientists from coming. Neal Griffith Smith, Oral history interview with Robert K. Enders, 13 April 1976, RU 009562, SIA.

408. Barbour to Zetek, 28 December 1928. Box 25, Folder 4, RU 135, SIA.

409. The average per year until World War II was about 25; see Appendix 5. Initially, some of BCI's founders would have even preferred to have excluded graduate students altogether as "infants" who someone "will have to take care of," but the need for visitors fees outweighed this sentiment. A. G. Ruthven to Barbour, 5 November 1923. Box 1, Folder 3, RU 134, SIA.

foliage, and disturbing both BCI's wildlife and its working scientists. Perhaps more importantly, tourists jeopardized BCI's status as place for science. The absence of tourists signaled BCI as a space of scientific labor, not leisure. BCI was intended to be preserved for research, not to act as a game reserve or a nature park for tourists.

Yet, who counted as a legitimate scientific visitor and who a tourist? Groups from the growing number of American cruise ships circulating through the Caribbean were a clear "no," but what if *Nature Magazine* sponsored the cruise?[410] If the station was for "real research men," what of women scientists or the wives of male scientists?[411] The acceptance of one boy scout as an assistant had Barbour worrying they would "have whole tribes in on us [if] we don't look out."[412] Both Zetek and Barbour kept up a vigorous correspondence negotiating who would have the privilege of visiting the island. Who could be part of the flow of visitors into BCI and who would be barred was determined by weighing costs and benefits to the island's environmental and financial stability, as well as its scientific reputation.

From the start, Zetek made exceptions for local dignitaries whose goodwill was needed to keep the place operating smoothly. Zetek regularly played eager host to CZ, US Army, the United Fruit Company, and Panamanian high officials and their families on day trips to BCI. While Latin American scientists were absent from the scientific community of the island, there were occasional visits from representatives of Latin American agricultural departments or groups

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410. For example, Zetek to Barbour 21 August 1930 and reply by telegram, Zetek to Barbour 23 November 1930. Box 25, Folder 5, RU 135, SIA.

411. Fairchild to Barbour, 10 September 1924. Box 1, RU 134, SIA.

412. Barbour to Zetek, 18 July 1931. Box 26, Folder 1, RU 135, SIA.

of students from the University of Panama.[413] Wealthy friends of Barbour or Fairchild also added BCI to their tropical sojourns, including Gifford Pinchot and, on more than one occasion, Mrs. Theodore Roosevelt. By 1930, the number of daytime visitors, above and beyond researchers residing on the island for an extended period, had already reached a total of one hundred. Still, this was a highly exclusive group of tourists. In such cases, any potential damage to the island's flora and fauna that might be caused by allowing non-scientists to tour BCI was counterbalanced by the promise of funds and the political good will that these visitors ensured.

Yet, as selective as BCI was in its early years, financial constraints created an incentive to permit at least a somewhat wider array of visitors than either Barbour or Zetek would have considered had the station been supported by an endowment. The small fees that visitors paid helped to keep the organization running and encouraged a larger degree of openness. Divisions between scientists and others were made not through complete exclusion, but through the timing of visits. Members of the public could visit for the day, usually on Fridays, while scientific researchers were generally longer-term residents. But there were exceptions: in spite of both men's reservations, they agreed to allow the nature writer Dallas Lore Sharp to stay for for the month of February, 1929, to gain inspiration for a series of magazine articles.[414] Following the great stock market crash later that year, and with no sign of an endowment in sight, criteria were sometimes loosened. This was particularly true during seasonal "dry spells" when few visiting

413. Zetek to Barbour, 8 January 1937. Box 26, Folder 4, RU 135, SIA; Seventh annual report of the Barro Colorado Island Station in the Panama Canal Zone, 1 March 1931. Box 11, Folder 2, RU 135, SIA.

414. Fifth annual report of the Barro Colorado Island Station in the Panama Canal Zone, 30 March 1929. Box 11, Folder 2, RU 135, SIA. Sharp published a series of articles based on his sojourn at BCI: Dallas Lore Sharp, "Machete Trails," *Atlantic Monthly* 145, no. 6 (1930): 749-58; Dallas Lore Sharp, "Poncho, Son of the Jungle," *Atlantic Monthly* 145, no. 1 (1930): 103-07; Dallas Lore Sharp, "My Jungle Study," *Nature Magazine* 17, no. 1 (1931): 10-14; Dallas Lore Sharp, "My Jungle Study (Part 2)," *Nature Magazine* 17, no. 2 (1931): 93-96.

scientists were on the island. Barbour hoped for a day when BCI would be on a firmer financial basis so that "We can then pick and choose who goes to the Island and not take Dallas Lore Sharpes' just because they happen to be able to pay their board bills."[415] Bird watchers and popular authors were not allowed as a rule, despite the fact that some of BCI's earliest scientists and founders—the Allees, Chapman, and even Fairchild and Barbour in later years—all published their own accounts of their time on the island for a general audience.[416] As BCI's fame grew, in large part due to the books and articles published by previous visitors, Barbour and Zetek found themselves policing BCI's boundaries and turning away an array of American eccentrics who they believed wished merely to commune with tropical nature at BCI rather than perform serious scientific labor.[417] Their own efforts to publicize—and thus win more funding for BCI—excited popular interest, but that interest threatened to dilute the scientific nature of the institution.

While visits by rich and well-connected tourists of both sexes might pose little threat to BCI's scientific status, the question of women's legitimacy as long-term visitors was fraught. As historian Pamela Henson has shown, BCI's founder's resisted fully incorporating women—whether they were scientists themselves or scientists' family members—into the life of the island.[418] With no dormitory facilities for women, they were effectively banned from overnight

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415. Barbour to Zetek, 28 December 1929. Box 25, Folder 5, RU 135, SIA.

416. Warder Clyde Allee, and Marjorie Hill Allee, *Jungle Island*; Frank Michler Chapman, *My Tropical Air Castle*; Frank Michler Chapman, *Life in an Air Castle*; David Fairchild, *The World Was My Garden*; Thomas Barbour, *Naturalist at Large*.

417. One example was the child prodigy and author Barbara Follet. Mrs. Wilson Follet to Barbour, 24 August 1928, and Barbour to Mrs. Wilson Follet, 27 August 1928. Box 16, Folder 4, RU 135, SIA.

418. Pamela M. Henson, "Invading Arcadia," 582-89.

stays. This prohibition acted as a glass ceiling to full participation by female scientists through much of BCI's history. Having to commute to and from the mainland each day meant that "early morning bird observations, nocturnal insect and rodent collecting, and round-the-clock flowering plant studies remained out of reach... As day trippers, they also missed the common meals, evening discussions and camaraderie in the main building on the island."[419] It was a barrier they resisted, whether by attempting to raise their own funds for a women's dorm, or in more informal ways. Upon being informed by Zetek that BCI had no facilities for ladies, for example, Winifred Duncan, a zoologist who had previously worked at William Beebe's station, "replied that she was no lady and would sleep right on the trail if need be." Zetek's control over access to the island was never complete, however. Denied the multiple day visit she wanted, Duncan nevertheless hired a boat and explored BCI for a few hours in Zetek's absence. "I know that had we met you would have been disabused of the idea you seemed to have that I was a tourist or a curiosity seeker," she wrote, as she left a handsome tip for the Panamanian caretaker, Donato Carillo, who had helped her around the island.[420]

The limitation of women's access to BCI occurred within the larger context of biologists' defense of professional boundaries. Barbour and BCI's other founders were frequent attendees of the Woods Hole Marine Biological Laboratory (MBL), where women and teachers had infamously been pushed out during efforts to professionalize American biology—despite their

419. Ibid., 587.

420. Zetek to Barbour, 6 October 1931, including excerpt of letter from Winifred Duncan to Zetek. Box 26, Folder 1, RU 135, SIA. Barbour drew on his scientific network to vet potential BCI visitors. He forwarded this letter to Chapman with the note "What do you know about this bird?" Although Chapman was an ornithologist, the question was not about the *Aves*, but rather Chapman's opinion of Duncan's legitimacy as a researcher.

key role in establishing the institution.[421] But BCI's founders felt especially vulnerable to the possibility of "drifting onto the shoals of popularity and the crowded state" because of vicious rumors swirling around another tropical station: William Beebe's station in British Guiana. Beebe openly encouraged female involvement, and was himself viewed as somewhat of a scientific outsider because of his engagement in popularization and lack of disciplinary specialization.[422] Barbour personally did accept that some women could be "bona fide" scientists, yet he and the others believed BCI should follow MBL's example over Beebe's at all costs.[423] Wheeler, Fairchild reported to Barbour, "regaled us all with stories of the women element at the other station [Beebe's] where he spent a season and we agreed... that it was a mistake to consider their admission into this laboratory which we have built."[424] The inclusion of women would leave BCI vulnerable to accusations of being merely popular or recreational—or even morally suspect.

Beyond professional concerns and risks to BCI's reputation, however, was the fact that the question of women's involvement exposed the contradictions inherent in maintaining BCI as a space for scientific work while simultaneously having researchers actually live on the island. To BCI's founders, women's involvement would mean the mixing of private and scientific life.

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421. Philip J. Pauly, "Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882-1925," 121-50.

422. Barbour engaged in a long public feud with Beebe over his "unprofessional" nomenclatural practices—a sign of Beebe's insufficient specialization in Barbour's view. For example, Thomas Barbour, "Comments on a Recent Check-List," *The American Naturalist* 54, no. 632 (1920): 284-88; William Beebe, and Thomas Barbour, "Beebe Versus Barbour," *Atlantic* 172, no. 1 (1943): 38. Fairchild to Barbour, 10 September 1924. Box 1, RU 134, SIA. On Beebe and scientific specialization more broadly, see Carol Grant Gould, *The Remarkable Life of William Beebe: Explorer and Naturalist*, 198-199, 408-409.

423. For example Bailey . Barbour to Zetek, 15 May 1931. Box 26, Folder 1, RU 135, SIA.

424. Fairchild to Barbour, 10 September 1924. Box 1, RU 134, SIA.

Fairchild—whose position in the funding stream was crucial—was particularly adamant that women were a danger to the maintenance of BCI as a serious scientific space. "Let us keep a place where real research men can find quiet, keen intellectual stimulation, freedom from any outside distractions," he wrote, "Worries over the food supply, or the petty personalities of the servants, or any those things which call for decisions to be made regarding matters which rob the men of the time they want for their work, are all mistakes."[425] Zetek shared his fear that women would involve themselves in the management of the BCI "household," interfering with the work of the cook and fomenting strife between the staff and scientists. Chapman, a key supporter of the station, feared that with women would come children, and the noise and activity of children would be an impossible nuisance to scientific observation, not to mention the natural aesthetic of the island which he so valued. The goal was to make BCI a place for scientific research, and the presence of women threatened to make painfully visible the fact that the domestic and the scientific could not be wholly separated in the life of a field station, where scientists lived as well as worked. Women imperiled not only to the professional status of the station, but also the possibility of living a scientific life on the island. "I do not believe the same curiously stimulating atmosphere can be maintained in a body of men and women that can be in a body of men alone," Fairchild wrote.[426] Many male researchers vociferously disagreed. After all, not only were their wives often also their invaluable research assistants, the restrictions on women's attendance was a serious obstacle to young family men who wished to pursue an extended visit. Clarence Ray Carpenter, the up-and-coming primatologist, voiced his frustration after a

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425. Fairchild to Barbour, 10 September 1924. Box 1, RU 134, SIA.

426. Fairchild to Barbour, 10 September 1924. Box 1, RU 134, SIA.

conversation with Zetek with humor, "Frankly, I had been led to believe that the mention of a wife was a source of considerable irritation, regardless of the fact that they do sometimes seem rather necessary and I notice that most men have them."[427] By attempting to preserve BCI as a masculine scientific space, BCI's founders ironically impeded long-term residence by the male researchers they sought to attract.

Laborers: "The Keepers of the Castle"[428]

While visitors came and went, another human population had a more permanent residence on the island: the workforce. While scientists might stay for days, weeks, or months, and even Zetek normally made only weekly visits, a small group of laborers kept BCI functioning throughout the year. Workers took care of the everyday needs of scientists living on the island. They prepared researcher's meals, cleaned their rooms, dealt with loads of their dirty laundry, and ferried visitors back and forth to the mainland. The staff were also an integral part of scientific labor on the island, helping to preserve specimens, care for living study animals, monitor scientific apparatuses, and sometimes identify organisms. Ultimately, when the scientists went home, BCI's laborers remained to maintain the facilities. There was always an electric generator, a water cistern, or a boat launch to build or repair. The trails and clearing around the laboratory needed regular upkeep to remain free of constantly encroaching vegetation, while BCI's shores had to be guarded from the trespass of poachers. In large part, BCI's staff kept life going on the

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427. Carpenter to Barbour, 5 November 1932. Box 15, Folder 3, RU 135, SIA.

428. Chapman dedicated his book in part to BCI's caretaker and cook,"Donato and Enemicia the keepers of the castle," Frank Michler Chapman, *My Tropical Air Castle*, v.

island, not only on a daily basis, but by maintaining continuity in the institution's operations over the years.

Indeed, if sheer quantity of correspondence is any judge, labor was the most essential factor to BCI's functioning as a research station. In his letters to Barbour, Zetek constantly grumbled about his difficulties with workers, crowed over the achievements of his current favorites, and chronicled the intimate details of the staff's lives. Managing labor seems to have caused Zetek more trouble than any other aspect of his job as BCI's custodian. Often, he complained of difficulty hiring "good" workers. Many aspects of work on BCI had analogues on the mainland, but the idiosyncrasy of the scientific station meant that Zetek wanted laborers who were accustomed to conditions on the island and comfortable working with scientists; he hated turnover in the workforce because it meant retraining. While BCI benefited in many ways by piggy-backing on colonial infrastructure in the CZ, the institution was at a disadvantage when it came into competition from the larger corporate or territorial interests. Labor could be expensive. Workers could become "spoiled" by wages offered by "mining and oil people" during boom times.[429] And during the ramp up to US involvement in World War II, for example, high demands for labor to reinforce the Canal's defenses and provide services to military forces would drive up labor costs for Zetek.[430] Moreover, Zetek had to find laborers willing to spend long periods of time on the island, away from urban centers and with limited access to their families.

Zetek's labor management strategies drew on his experience working in the CZ, reflecting both his personal involvement in mosquito control and the territory's broader racially

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429. Zetek to Chapman, 11 October 1928. Box 25, Folder 4, RU 135, SIA.

430. Zetek to Paul Brocket (Board of Directors, Canal Zone Biological Area), 24 September 1941, and Zetek to Barbour, 26 January 1941. Box 3, Folder 2, RU 134, SIA.

segregated labor hierarchy. In the CZ, the labor force was divided into a privileged “gold roll” and a second class of “silver roll” workers, who were afforded differing pay-scales and decidedly unequal living arrangements. In the early years of Canal construction, the two payroll designations were ostensibly used to distinguish skilled from unskilled labor, and later to track American versus foreign employees. By the time of BCI’s establishment, however, the gold and silver roll system had evolved into a much more rigid method of racially segregating the CZ workforce. The CZ administration slated white American men to work as technicians or white-collar employees while Black West Indian and other immigrants were largely relegated to more menial and dangerous tasks, and distinctions of nationality and ethnicity were used to even more finely classify CZ labor.[431]

Zetek mirrored this structure on the much smaller scale of BCI, considering supposed racial “characteristics” when making hiring decisions. Like the CZ more generally, Zetek drew from a large pool of West Indian former Canal workers and their descendants for general construction and maintenance work. These were men for whom work at BCI was one among a long series of odd jobs and seasonal work.[432] At the same time, he believed that Panamanian “natives” were better adapted to work in the forest. “Silvestre is at home in our woods,” Zetek remarked of a recent hire, arguing that he did a much better job “baling hay”—that is, the work of pressing plants to make herbarium specimens—than a young American volunteer had.[433] When it came to positions with responsibility over other workers, however, he preferred whites if

431. Julie Greene, *The Canal Builders*, 123-58.

432. For example, Pamela M. Henson, Patricia Escobar Paramo, Elizabeth Stockwell, Mitch Aide and Carolien Haverkate, Oral history interview with Oscar Dean Kidd 1987, 1990, RU 009574, SIA.

433. Zetek to Barbour, 15 August 1931. Box 26, Folder 1, RU135, SIA.

possible. Zetek's first foreman was an Australian, Frank Drayton, but after several disagreements, he hired Donato Carillo, identified as "a native of Chiriqui."[434] Donato served as Zetek's right hand man and caretaker of the island through the 1920s and early 1930s.[435] After his death and difficulty filling the position, however, Zetek proclaimed his wish to replace "half grown Panamans" with Italians.[436] He found Francisco "Chi Chi" Vitola, a Colombian of Italian descent, to take the position. He felt "fortunate in having an Italian white...He is GOOD."[437] Even in the close quarters of the island, Zetek kept mealtimes, drinking water, and latrines segregated, as they were in the CZ.[438] The complex landscape of race, class, and language could be difficult for new visitors to navigate. Zetek was much amused when the ornithologist Josselyn Van Tyne confused race and class-based designations, complaining that a recent German immigrant couple that Zetek briefly hired as cook and handyman were using the "white" toilet.[439]

Laborers, however, had their own ways of classifying visitors. Chapman's difficulties with the Spanish language made him the butt of jokes, while Barbour won real admiration for his fluency—they insisted he must be Cuban or Colombian. At the same time, mammalogist Robert

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434. Chapman makes this claim, although his specific ancestry is not discussed in correspondence, Frank Michler Chapman, *My Tropical Air Castle*, 212.

435. Workers were almost uniformly referred to by their first names or nicknames, while visiting scientists and tourists were given full names or titles and last names, unless they were particularly close friends with Barbour or Zetek.

436. Zetek to Barbour, 17 March 1935. Box 26, Folder 3, RU 135, SIA.

437. Zetek to Barbour, 8 January 1934. Box 26, Folder 3, RU 135, SIA.

438. Fausto Bocanegra, a BCI employee for more than thirty years beginning in 1952, recalls the segregation of dining, drinking water, and lavatory facilities. They were not desegregated until Zetek's replacement, Carl Koford, arrived in 1956. Giselle Mora, Oral history interviews with Fausto Bocanegra, August 1988, RU 009561, SIA.

439. Zetek joked that this was anti-German sentiment gone too far. Zetek to Barbour, 5 May 1927. Box 25, Folder 4, RU 135, SIA.

K. Ender's willingness to eat papayas and rice for breakfast, instead of visitors' usual bacon, eggs, and oatmeal, allowed him to get along well with the staff in spite of his lack of Spanish.[440]

Zetek's experience as an entomologist in the CZ also informed his labor practices. Not only was he anxious to exclude insect-borne disease from BCI, but at a more fundamental level, he managed his workers as biological beings. Beyond populating the island with a carefully selected workforce, Zetek closely monitored the movements and health of BCI's workers. He required them to have regular blood tests to prevent the malaria parasite from being passed to mosquito vectors on BCI. He also forbade them from traveling freely to the mainland and to towns where disease was prevalent, even though the isolation from friends and family proved a strain on labor relations. He even went as far as to recruit assistants from a nearby orphanage, believing that a lack of family duties would keep orphans from frequent travel to the mainland.[441]

The actions of Nemesia Rodriguez, BCI's cook in the 1920s, however, show that Zetek's control could be challenged. Nemesia frequently "ran away" from BCI. Knowing how difficult it would be to replace a good cook who knew the island's routine by heart, Nemesia would threaten not to come back to the island and thus put herself in a better position to negotiate wages and conditions with Zetek. Essentially, she employed a form of *petit marronage*—a strategy of unfree people with a particularly long history in the Americas.[442] Nemesia was even able to get

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440. Neal Griffith Smith, Oral history interview with Robert K. Enders, 13 April 1976. RU 009562, SIA.

441. For example, Zetek to Barbour, 12 May 1927. Box 25, Folder 4; Zetek to Barbour, 18 September 1932. Box 26, Folder 2, RU 135, SIA.

442. *Petit marronage* is usually applied to the actions of slaves, who might run away temporarily in order to secure better treatment (as opposed to *grand marronage*, or more permanent escape from the plantation). The term describes Nemesia's behavior exactly, despite her status as a wage laborer. See Alvin O. Thompson, *Flight to Freedom: African Runaways and Maroons in the Americas* (Kingston, Jamaica: University of the West Indies Press, 2006), 53-90.

Zetek to allow her aging mother to live on Slothia, a tiny island adjacent to BCI, where she raised chickens and eggs for the island's provisions.

Zetek's biological approach to managing BCI's labor went beyond disease control to management of family life and reproduction. Frustrated by their "misbehavior" and his inability to control their movements, Zetek tried to get Donato and Nemesia to marry, and to attend Catholic Mass with him.[443] He felt that if they settled down, they would both be more inclined to stay on the island for lengthy periods of time, and ultimately to remain in his employ long-term. At the same time, he feared that they would have children and upset BCI's scientific atmosphere. Thus, when Nemesia returned to BCI after medical treatment—which Zetek had arranged—for gonorrhea and found that she was unable to bear children, Zetek was shockingly cold about the "added attraction to it from our standpoint, namely inability to populate the island."[444] Whether Zetek knew what the outcome of this treatment would be, and whether Nemesia was ever informed of the possibility, is unclear, but the incident is revealing of both Zetek's willingness to intervene in the personal lives of BCI's staff and how he approached the labor force as the island's "population." While BCI's visitors and promoters framed the island as an uninhabited tropical wilderness, in practice, Zetek's minute management of the society of laborers paints a different picture.

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443. In the correspondence, workers were always referred to by their first names or nicknames, while visiting scientists and tourists were given full names or titles and last names, unless they were particularly close friends with Barbour or Zetek.

444. Zetek to Barbour, 30 June 1931. Box 26, Folder 1, RU 135, SIA.

Technologies: "An Out-Of-Doors Laboratory"[445]

The labor of the island's staff was part of the larger attraction of BCI as a research site that was simultaneously "wild" and "developed"—the ecology of the island was meant to be "undisturbed," and yet scientists had access to facilities for living and working with an ease not possible on a more strenuous expedition. Dorms replaced tents and cisterns provided a safe, reliable supply of water for drinking—and even for showers. The laboratory building housed equipment for microscopic and chemical analysis, a dissection table, and animal cages. There was a darkroom for photographic development and a drying closet for specimens. Delivery of ice meant the ability to slow the decay of animal specimens, as well as relief from heat and fresher food to nourish scientists' own bodies. Josselyn Van Tyne, who completed eleven months of dissertation research on toucan behavior at BCI, lauded the "quite unique" conditions; "I know of no other place in the American tropics where the biologist can live and work in comfort and safety in a virgin rain-forest jungle."[446] Similarly, the ecologist Warder Allee enthused at the ability to "live and work with comfort in the jungle itself."[447] BCI made working in the forest "easy and safe," and a "simple matter" even for those who had never been to the tropics before.[448] Stuart W. Frost, a Pennsylvania State College entomology professor who had never previously been to the tropics, chose to use four months of his first sabbatical to visit BCI in

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445. Frank E. Lutz, "An Out-of-doors Laboratory," 1935, unpublished lecture. Box 26, Folder 5, RU 135, SIA.

446. Josselyn Van Tyne, "The Barro Colorado Laboratory as a Station for Ornithological Research," *Wilson Bulletin* 42 (1930), 228; Harold Mafield, "In Memoriam: Josselyn Van Tyne," *The Auk* 74 (1957): 322-32.

447. Warder Clyde Allee, "The Barro Colorado Laboratory," 521.

448. G. R. Bisby, "The Barro Colorado Island Laboratory," *Science* 62, no. 1596 (1925); Charles W. Townsend, "An Island in the Tropics," *Harvard Graduates Magazine* 34, no. 133 (1924): 36-45.

1929. He reported back to his colleagues that on BCI, "the scientist finds shelter, a comfortable bed, good food, tables and all the necessary equipment for general investigation. Knowing that all the essentials and comforts of life are abundantly provided for, he can direct his entire time and attention to the sole purpose for which he came."[449]

If Frost had planned his own expedition into the tropical jungle, he would have had to navigate the process of hiring local labor for assistance, providing for himself and his entourage, and carrying along all necessary instruments and reference material. Researchers paying a visit to BCI were relieved of this burden of equipment and responsibility. They did not need to carry the heavy weight supplies required by an expedition, nor be experts in organizing fieldwork. A scientist working in the field could not match the global resources available at the fingertips of one working at a metropolitan center of calculation—be it a museum, herbarium, or laboratory—but neither was fieldwork ever a matter of unmediated, unencumbered observation.[450] Expeditions required transporting tools to for making measurements, visual records, and closer visual examinations.[451] Expeditions also required reference material—print technologies—in

449. Stuart Ward Frost, "Animal Life on Barro Colorado Island. A Lecture," *Journal of the Washington Academy of Science* 21, no. 8 (1931), 174; Stuart Ward Frost, "Collecting Leaf-Miners on Barro Colorado Island, Panama," *Scientific Monthly* 30, no. 5 (1930): 443-49.

450. The debate between the "closet" naturalist and the fieldworker was one of longstanding; Dorinda Outram, "New Spaces in Natural History," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996).

451. On instrumentation and expeditionary science, see Daniela Bleichmar, "Painting as Exploration: Visualizing Nature in Eighteenth-Century Colonial Science," in *Colonial Latin American Review*, Colonial Latin American Review (Routledge, 2006); Marie-Noëlle. Bourguet, et al., *Instruments, Travel, and Science: Itineraries of Precision From the Seventeenth to the Twentieth Century* (New York, NY: Routledge, 2002); Michael Dettelbach, "Humboldtian Science."; Anne Larsen, "Equipment for the Field," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996); Richard Sorrenson, "The Ship as a Scientific Instrument in the Eighteenth Century," *Osiris* 11, no. Science in the Field (1996): 221-36; David Elliston Allen, *The Naturalist in Britain: A Social History* (Princeton, NJ: Princeton University Press, 1976).

order to place field observations within the context of what others had seen before.[452] All this was available in place. BCI served as a kind of augmented expedition, allowing biologists to "travel light," both in terms of equipment and prior knowledge of the flora and fauna.

Such facilities were especially important in the tropics. As Van Tyne explained, the tropics were "extremely rich in species," but individuals of particular species were generally "not correspondingly abundant." This made merely identifying species an especial "difficulty in the way of the newcomer."[453] An ecologist could quickly master the more limited species lists of most temperate locales, but BCI's flora and fauna was bewildering without a more intensive guide. This problem was gradually overcome. Rather than focus on collecting specimens and "working up" expeditionary material back home, over time, more and more of this work could be done without leaving the island. BCI housed a growing specimen collection of the island's plants and animals, a library of reference books and articles based on previous research at BCI and the surrounding region, and a card catalogue of all species known from the island. Knowledge, including a foundation of taxonomic knowledge required for other kinds of research, could be accumulated on the spot. In effect, BCI became its own small-scale center of calculation, shrinking the distance—both physical and epistemic—between field observation and the collation of scientific knowledge. A reasonably complete taxonomic inventory of BCI's much more numerous plant species would take decades more, but the instrumentation—including

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452. See Daniela Bleichmar's work on the role of botanical reference material in Spanish exploration. Daniela Bleichmar, "Exploration in Print: Books and Botanical Travel From Spain to the Americas in the Late Eighteenth Century," *The Huntington Library Quarterly* 70, no. 1 (2007): 129-51.

453. Josselyn Van Tyne, "The Barro Colorado Laboratory as a Station for Ornithological Research."

information technologies—available on BCI allowed the study of animal behavior and ecology from the station's earliest days.[454]

An increasing array of technologies allowed greater and greater ability to observe the lives of BCI's organisms outside the walls of the laboratory building. Binoculars, for example, provided visual access to animals in the treetops. Frank Chapman, in his widely read study and popular accounts of BCI's nesting Oropéndolas, extolled the use of binoculars for observing the life histories of birds.[455] In the nineteenth century, American ornithologists had insisted that scientific ornithology required a gun, not opera glasses.[456] Chapman, however, was a major advocate for making visual observations of living birds and not relying on dead collections alone.[457] He wanted professional ornithologists to expand from their focus on taxonomy into broader studies of life history and ethology. Chapman put this philosophy to work on BCI.

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454. Paul C. Standley completed the first effort at a census of BCI plants, with the additional contributions of Leslie Kennoyer. It was not until the 1970s, with Thomas Croat's flora of the island that scientists who were not taxonomic specialists could begin to feel comfortable in their identifications. Egbert Leigh, personal communication; Paul C. Standley, "The Flora of Barro Colorado Island, Panama," *Smithsonian Miscellaneous Collection* 78, no. 8 (1927): 1-32; Leslie A. Kenoyer, and Paul C. Standley, "Supplement to the Flora of Barro Colorado Island, Panama," *Field Museum of Natural History Botany Series* 4, no. 6 (1929): 143-58; Paul C. Standley, *The Flora of Barro Colorado Island, Panama*, vol. 5, Contributions of the Arnold Arboretum of Harvard University (Jamaica Plain, Massachusetts: The Arnold Arboretum of Harvard University, 1933); Thomas B. Croat, *Flora of Barro Colorado Island* (Stanford: Stanford University Press, 1978).

455. Frank Michler Chapman, "The Nesting Habits of Wagler's Oropendola (*Zarhynchus Wagleri*) on Barro Colorado Island," *Bulletin of the American Museum of Natural History* 58, no. 3 (1928): 123-66; Frank Michler Chapman, *My Tropical Air Castle*, 24, 78-146.

456. On the emergence of bird watching and its relationship with professional ornithology, see Mark V. Barrow, *A Passion for Birds: American Ornithology After Audubon* (Princeton, NJ: Princeton University Press, 1998), 156-60; Thomas R. Dunlap, *In the Field, Among the Feathered: A History of Birders & Their Guides* (New York, NY: Oxford University Press, 2011), 13-36.

457. This is not to say that he de-emphasized collecting or saw it as in conflict with either the observation of living birds or their conservation. Chapman made enormous collections and his contributions to bird taxonomy are still recognized. François Vuilleumier, "Dean of American Ornithologists: The Multiple Legacies of Frank M. Chapman of the American Museum of Natural History," *The Auk* 122, no. 2 (2005): 389-402; Mark V. Barrow, *A Passion for Birds: American Ornithology After Audubon*.

Upgrading from an 8-power to a state-of-the-art 24-power tripod-mounted pair of binoculars, Chapman wrote, “The birds, wholly unaware of my presence, seemed to be within reach of my hand.”[458] He felt that new technologies allowed him to immerse himself in the ecology of BCI. Chapman could observe the Oropéndolas weaving their hanging-basket nests from one hundred feet away, witnessing the intimate details of their life histories without disrupting their natural behaviors. The result, he hoped was “a contribution to bird-biography and to a better understanding of the relation of a species to its habitat.”[459] Such observational work was, again, especially important in the tropics, where, in Van Tyne’s words, “less is known of their habits than was known of North American birds in the days of Audubon’s explorations ninety years ago.”[460]

The tight restriction on the collection of vertebrates—a stipulation of BCI’s status as a nature reserve—encouraged innovative technologies for assessing the range of species living on the island. While Enders, for example, made some use of traps and guns in his important initial survey of the island’s mammals, he limited his collections, focusing on accurately surveying the island’s diversity, not on creating a collection for its own sake. “Mammals were observed as much as possible,” he wrote, “only enough specimens being taken to insure identification. Where this was not necessary because of size or visibility, none were collected.”[461] But to truly create a

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458. Frank Michler Chapman, “The Nesting Habits of Wagler’s Oropendola (*Zarhynchus Wagleri*) on Barro Colorado Island.”

459. Frank Michler Chapman, *My Tropical Air Castle*, 82.

460. Josselyn Van Tyne, “The Barro Colorado Laboratory as a Station for Ornithological Research.”

461. Robert Kendall Enders, “Notes on Some Mammals From Barro Colorado Island, Canal Zone,” *Journal of Mammology* 11, no. 3 (1930): 280-92. In at least one case Enders shot an ocelot in spite of “visibility,” out of fear of personal injury. Zetek to Chapman, 15 July 1929. Box 25, Folder 5, RU 135, SIA.

"census of the living, not a record of the dead," Chapman pioneered the scientific use of camera traps—trip-wire triggered flash photography—on BCI.[462] By rigging up a real "wired wilderness," BCI's nocturnal and reclusive animals helped to "capture" themselves in photographs decades before more intensive tracking through radio-collars or GPS would emerge.[463] Similarly, the ecologist Orlando Park was able to monitor the activity of the BCI community of nocturnal and diurnal species by recording the sounds of forest over twenty-four hour periods.[464] Technologies like camera traps and audio recorders extended the power of human observation into the night, making visible (or audible) species and behaviors previously undetectable. Paradoxically, these machines in the jungle seemed to allow more "natural" interventions; through remote technology scientists could observe organisms more closely and with less disturbance than if they had been physically present.

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462. Frank Michler Chapman, "Who Treads Our Trails?," *National Geographic Magazine* 52, no. 3 (1927), 332; Frank Michler Chapman, *My Tropical Air Castle*, 196. Chapman adapted the apparatus developed by sportsman-photographers George Shiras III and William Nesbit. His work on BCI was the first to use so-called "flashlight photography" for an explicitly scientific purpose. See Thomas E. Kucera, and Reginld H. Barrett, "A History of Camera Trapping," in *Camera Traps in Animal Ecology*, ed. Allan F. O'Connell, James D. Nichols, and Ullas K. Karanth (New York, NY: Springer, 2010).

463. Etienne Benson documents the controversy that erupted in the 1950s and 1960s over the ethics of applying such technologies to monitor supposedly wild animals. On BCI, similar techniques were apparently uncontroversial. Although Chapman admitted concern that the flash of his cameras might surprise BCI's shy mammals, he and BCI's other visitors seemed satisfied that camera traps posed no threat to the "balance of nature." Perhaps the island's status as a scientific, rather than public, park cushioned the legitimacy of such techniques. Etienne Benson, *Wired Wilderness: Technologies of Tracking and the Making of Modern Wildlife* (Baltimore, MD: Johns Hopkins University Press, 2010).

464. Park's work was innovative not only in the field techniques employed, but also in their integration with the recording of daily cycles of activity in captive animals in BCI's laboratory. Park was the first to characterize a complete tropical forest community in terms of nocturnal/diurnal niche partitioning. Orlando Park, "Studies in Nocturnal Ecology, VII. Preliminary Observations on Panama Rain Forest Animals," *Ecology* 19, no. 2 (1938); Orlando Park, "Nocturnalism - the Development of a Problem," *Ecological Monographs* 10 (1940): 485-536; Orlando Park, et al., "Studies in Nocturnal Ecology, IX. Further Analysis of Activity of Panama Rain Forest Animals," *Ecology* 21, no. 2 (1940); Orlando Park, "Concerning Community Symmetry," *Ecology* 22, no. 2 (1941): 164-67.

BCI's technologies could be relatively portable and self-contained, like binoculars and microscopes, or they could be integrated into the landscape of the island, like camera traps or rain gauges. But BCI's technological infrastructure went beyond its buildings or particular pieces of equipment and into the landscape itself. The laboratory spilled out from the main building's doors and into the forest outside, and the island itself became a scientific instrument for observing tropical life.[465] Consider not only Chapman's binoculars, but also the laboratory clearing into which he peered. Maintained by BCI's staff, the clearing provided an unobstructed, cross-sectional view into the canopy. It actively attracted coatis, ants, and birds, and was also a convenient place to measure certain environmental factors.[466] Much early work went on "at the very door of the laboratory," in the clearing, and it could even draw out those interested more in indoor experimentation. For example, Frank E. Lutz, curator of entomology at the American Museum of Natural History and at the time supervisor of the Station for the Study of Insects at Tuxedo, New York, came to BCI with "a trunk full of apparatus...with the expectation of working at an indoor laboratory table." Before he could finish unpacking, another

> visitor interested in all out-of-doors asked me to go a few hundred yards up the hill to see a nest of Leaf-cutting Ants. I went, and, although the indoor laboratory facilities were an still are good, dropping my indoor problem, I spent the daylight hours of my stay sitting on the group near that nest and in my subsequent visits, so far as I was concerned, the 'laboratory' was out-of-doors.[467]

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465. For the analytical advantages of expanding the idea of a scientific instrument, see Richard Sorrenson, "The Ship as a Scientific Instrument in the Eighteenth Century."

466. For example, Paul D. Voth, "Conduction of Rainfall by Plant Stems in a Tropical Rain Forest," *Botanical Gazette* 101, no. 2 (1939); W.C. Rucker, "A Tropical Research Laboratory," *The Nations Health* 6, no. 7 (1924): 489-90; William Morton Wheeler, "Courtship of the Calobatas."; William Morton Wheeler, "The Finding of the Queen of the Army Ant Eciton Hamatum."; William Morton Wheeler, "A New Guest-Ant Other New Formicidae From Barro Colorado Island, Panama."

467. Frank E. Lutz, "An Out-of-doors Laboratory," 1935, unpublished lecture. Box 26, Folder 5, RU 135, SIA;

He explained, "It is not often that one has so good a 'set up' for certain observations."[468]

Even more than the clearing, BCI's network of trails is perhaps the paramount illustration of how BCI's landscape became a laboratory. Trails were key to making the island and its wildlife visible. As Chapman described it, "A tropical forest without trails is like a house with locked doors—we may force an entrance but we shall not be welcome"[469] Trails allowed silent movement and close observation of animals without causing disturbance. Some insects were attracted to freshly-cut trails, and new species were often found along the trails.[470] According to many observers, including Frost, "There is no doubt that one can see more tropical life on Barro Colorado Island than by penetrating the average jungle, for the trails are used by animals as well as man."[471]

BCI's trails spread over the island in spiderweb-fashion.[472] The trail system reflects the history of the island, both in its shape and in the names of trails. Each memorializes a scientist (e.g. Barbour, Wheeler, Shannon, Snyder), benefactor (Lathrop, Armour, Harvard), or employee (Nemesia, Donato, Drayton, Fausto).[473] The network of trails developed organically, according to particular needs and the availability of labor. Some terminated at small trail-end houses, donated

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468. Frank E. Lutz, "Observations on Leaf-Cutting Ants," *American Museum Novitates* 388 (1929), 1.

469. Frank Michler Chapman, *My Tropical Air Castle*, 168.

470. Leslie A. Kenoyer, "Fern Ecology of Barro Colorado Island, Panama Canal Zone," *American Fern Journal* 18, no. 1 (1928), 7.

471. Stuart Ward Frost, "Animal Life on Barro Colorado Island. A Lecture," 175.

472. BCI trail map, 1957. Box 3, Folder 11, RU 134, SIA.

473. In BCI's case the "invisible technicians" are not so invisible; they are written into the landscape. Yet, as Nils Elliot has noted, the Nemesia and Donato trails are given these workers' first names, in contrast to the more formal full or last names given to the trails of prominent scientists and donors. Nils Lindahl Elliot, "A Memory of Nature: Ecotourism on Panama's Barro Colorado Island," *Journal of Latin American Cultural Studies* 19, no. 3 (2010), 251.

for termite-resistance tests, where researchers could stay overnight. Although cleared in an ad hoc manner, BCI's trails increasingly acted as a grid, systematizing observation across the space of the island. By 1930 there were 33 kilometers of trails.[474] Each trail was labelled with a tin sign stamped with its name and numbered at hundred meter intervals, beginning with "1" closest to the laboratory and increasing outward. This system not only prevented visitors from losing their way in the forest, but allowed organisms that were observed or collected to be given a "coordinate" (e.g. "Zetek Trail 600"). As one newspaper reporter put it, "Eventually Barro Colorado will be a completely card-indexed island, with every plant, bird, animal, and insect on it in the laboratory files"[475]

The system of trails increasingly tamed the island, making it physically accessible and legible to systematic observation.[476] As the trails disciplined BCI's space to the needs of scientists, likewise an expansion in instrumentation increasingly disciplined the forest's other environmental parameters. Scientists placed rain gauges and light meters at points throughout the forest. They buried stakes in test plots to monitor the activity of termites. All of these instruments helped to standardize and extend observations over time and across space. Measurements of environmental factors made comparisons with temperate zone locales or other parts of the tropics possible, and allowed the study of relationships between the environment and specific animal behaviors and physiological responses. Among the instruments used by BCI researchers in the

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474. Sixth annual report of the Barro Colorado Island Station in the Panama Canal Zone, 18 March 1930. Box 11, Folder 2, RU 135, SIA.

475. Calhoun, C. H. "Jungle Folk Sanctuary is Provided in Panama." *The New York Times*, 1929. , 13.

476. The concept of "legibility" comes from James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, CT: Yale University Press, 1998).

1920s and 1930s were a Friez recording anemometer, Friez and Tycos recording thermometers, a Jules Richard recording evaporimeter, Livingston cup atmometers, Weston and Macbeth illuminometers and many unspecified barometers, hygrometers, and ph colorimeters—the list reads like a laboratory supply catalogue, enough to make Humboldt blush.[477] Most importantly, this technology was seamlessly integrated into the natural and social fabric of the island. With the "controlled and limited population" of laborers on BCI, Weston explained, instrumentation could be left out without risk of disturbance;

> there is no native population save the selected, limited, and sympathetic staff... With this personnel, experiments, delicate arrangements of apparatus, and work in process of completion, may be left in and around the laboratory with the certainty that they will remain scrupulously undisturbed...cameras may be set out, instruments may be placed, plants may be tagged or measured, and left utterly unguarded and unconcealed...they will remain untouched until one's return days or even weeks later.[478]

Visitors' accounts of BCI often describe an experience of pure nature, but if BCI acted as a laboratory it was not a naturally occurring one. Rather, it was one mediated by technologies, and one constantly in need of maintenance by the labor force, Zetek, and visiting researchers themselves.

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477. These measure wind speed, temperature, evaporation rate, evaporation rate (again), light intensity, atmospheric pressure, humidity, and ph, respectively. These examples come from Warder Clyde Allee, "Measurement of Environmental Factors in the Tropical Rainforest of Panama," *Ecology* 7, no. 3 (1926): 273-302; Warder Clyde Allee, and Magnhild Torvik, "Factors Affecting Animal Distribution in a Small Stream of the Panama Rain-Forest in the Dry-Season," *Journal of Ecology* 15, no. 1 (1927): 66-71; Orlando Park, "Studies in Nocturnal Ecology, VII. Preliminary Observations on Panama Rain Forest Animals."

478. William H. Weston Jr., "The Biological Station on Barro Colorado Island."

Organisms: "Who Treads our Trails?"[479]

The technology of BCI, its facilities, trails, and instrumentation, made wildlife visible only in particular, constrained ways. While the island was increasingly systematized, further complexity always lay beyond researchers' grasp. Even expert taxonomic botanists, confronted with such a diversity of unfamiliar tropical trees, often found leaves fallen to the forest floor that they were unable to match to the trunk of their rightful owner. Increasingly aware of the vertical stratification of the rainforest, researchers wanted to go where the trails could not take them: up. To ecologists, this stratification opened questions of adaptation to environment; as Theodore H. Eaton, a Yale ecologist studying adaptive coloration wrote, "There are two great realms of animal life in the tropical forest, the tree realm and the earth realm. Each has imposed on its inhabitants a multitude of special laws and drawn out of them, so to speak, innumerable potentialities they might never have realized if there had been other choices."[480] Warder Allee classified no less than eight distinct forest strata, but reaching the upper levels was no easy task.[481] The high forest canopy remained largely inaccessible. Fallen trees might provide access to specimens of epiphytes (plants that grow on other plants), and, in the early years of BCI's existence, before a shoreline flora had developed around the edge of the newly-formed island, some mid-forest fruits and flowers overhanging the water were reachable by *cayuca* (dug-out

479. Frank Michler Chapman, "Who Treads Our Trails?"; Frank Michler Chapman, *My Tropical Air Castle*, 196.

480. Theodore H. Eaton, "On Barro Colorado," *Yale Review* 29, no. 2 (1939), 338.

481. Warder Clyde Allee, "Distribution of Animals in a Tropical Rain-Forest With Relation to Environmental Factors," *Ecology* 7, no. 4 (1926), 446.

canoe).[482] Paul C. Standley even boasted of being the first botanist to identify “monkey-collected” specimens—fruits that black howler monkeys hurled down at the primatologist Clarence Ray Carpenter. “Such intelligent cooperation between man and beast in scientific investigation is without parallel,” Standley reported with tongue planted firmly in cheek.[483]

But beyond collecting specimens, researchers wanted to observe the living canopy, just as they were able to observe life on the forest floor. In a limited way, “natural ladders” of branches and inclined trunks sometimes offered a way up to study ants and butterflies.[484] In the 1920s, Allee made the first concerted effort to climb into the trees using a spike ladder made from the bolts of abandoned machinery from the days of the French canal effort, although on his first several attempts he found the wood so resistant that the spike shot back out at him. He was limited to the softer trunk of a sandbox tree, and even then he relied on the labor of an expert tree-climber and former rubber collector named Santiago to drive in the spikes up to ninety feet.[485] Years later, Allee’s former student Orlando Park tried another method, rigging together an “aerial chair” suspended by ropes—it was not for the faint of heart.[486] The heights of the “tree realm” remained elusive until the 1990s.[487]

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482. Walter N. Bangham, “An Accessible Tropical Vegetation,” *Science* 72, no. 1870 (1930), 457.

483. Paul C. Standley, “Monkeys Aid Scientist in Collecting Plants,” *Field Museum News* 3, no. 8 (1932), 2. Also mentioned in Zetek to Standley, 25 May 1932. Box 2, Folder 4, RU 134, SIA.

484. Stuart Ward Frost, “Collecting Leaf-Miners on Barro Colorado Island, Panama,” 448; Theodore H. Eaton, “On Barro Colorado,” 338.

485. Warder Clyde Allee, and Marjorie Hill Allee, *Jungle Island*, 38-42.

486. “Dr. Orlando Park in aerial chair,” photo, Data File Manager, File Referer No. #35655, STRI, http://biogeodb.stri.si.edu/bioinformatics/dfm/metas/view/35655. In 1928, Zetek also had a twenty-eight foot high redwood platform tower built out at the center of the island, but this gave very limited access to the surrounding canopy. Fourth annual report of the Barro Colorado Island Station in the Panama Canal Zone, 1 March 1928. Box 11, Folder 2, RU 135, SIA.

487. Geoffrey G. Parker, et al., “Access to the Upper Forest Canopy With a Large Tower Crane,” *BioScience* 42,

BCI's organisms challenged human plans, sometimes passively, as in the tantalizing canopy, and other times more actively getting in the way of scientific research. BCI's trails provided access to the forest, but that same forest could quickly reclaim the trails if workers did not regularly maintain them. In the tropical heat and humidity, mold could grow over camera cases and lenses overnight.[488] Termites attacked the wood in the station's buildings, and could even penetrate concrete made of calcium-rich sand.[489] Nevertheless, the nuisance of fungi and insects simultaneously brought resources to BCI, in the form of funding by Eastman Kodak on the problems of photography in the tropics and the donation of most of the original buildings by lumber and chemical companies that Zetek convinced of the opportunity to field-test their materials.[490] And while Zetek zealously guarded the island from the importation of disease organisms, certain resident pests were impossible to avoid. "Regardless of their interest in entomology," a newspaper reported dryly, visitors to BCI "may involuntarily make a collection of ticks"[491]

The purpose of BCI as a research station was to get scientists in touch with tropical organisms in their natural state, but the problem of keeping BCI natural was a thorny one. The questions of what constituted the "nature" that was to be protected on BCI were settled through

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no. 9 (1992): 664-70; Margaret D. Lowman, and Philip K. Wittman, "Forest Canopies: Methods, Hypotheses, and Future Directions," *Annual Review of Ecology and Systematics* 27 (1996), 65;

488. William H. Weston Jr., "The Fungi of Barro Colorado," *Scientific Monthly* 36 (1933), 399; A. C. Noe, "Photography in the Tropics," *Turtox News* 16, no. 6 (1938).

489. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

490. See for example, Folder 3 "Buildings and Grounds: Termite Control," Box 1; Folder 1 "Financial Affairs: Contributions, Eastman Kodak, 1947-1963," Box 3, RU 135, SIA; and "Studies on Deterioration and Corrosion" in James B. Zetek, "Report on the Canal Zone Biological Area," *Annual Report of the Smithsonian Institution* (1948), 141-42.

491. Calhoun, C. H., "Jungle Folk Sanctuary is Provided in Panama."

the management of plant and animal bodies, and the results were sometimes paradoxical. Even as facilities and trails were built on the island for the convenience of scientists, for example, great efforts were made by Zetek and his labor force to maintain the place as "untouched." Armed only with machetes, workers patrolled BCI's shores for poachers hunting peccaries, deer, and agouti, or for trespassers gathering ipecac roots to brew and distill *raicilla* (a local form of moonshine), even though such human activities long predated presence of scientists.[492] At the same time, Zetek found himself unexpectedly campaigning on the side of the invasive water hyacinth, *Eichnornia crassipes*, which was choking the Canal. Even though Zetek was himself one of the earliest voices raised against the danger of non-native plants to the ecology of the CZ, Zetek had to petition CZ Governor Julian Schley to protect BCI from being sprayed along with the rest of the shoreline with the broadly toxic sodium arsenite.[493]

The challenge of balancing scientists' requirements that the island remain "natural" with their other needs, both for living on the island and for closer access to wildlife to observe and experiment on, was significant. Initially, Zetek allowed laborers to keep a garden of bananas, citrus, and some vegetables on BCI near the shore below the laboratory building, on land once cultivated by one of the former CZ lessees living on the island (see Chapter 3). Many of the fruit trees were cultivars donated by Fairchild, and they were a welcome supplement to the diet (mainly canned food) of BCI's visitors and laborers. This early compromise on the side of

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492. Box 22, Folder 6, RU 135, SIA; Giselle Mora, Oral history interviews with Fausto Bocanegra, August 1988, RU 009561, SIA; Pamela M. Henson, Oral history interview with Pablo Rodriguez-Martinez, June 1990, RU009581, SIA;

493. Zetek, for example, had advocated for a plant quarantine in both the CZ and Panama, and warned against indiscriminate plant introduction projects—a campaign that had put Zetek at odds with one of BCI's major supporters, Fairchild. Zetek to Schley, 2 July 1933; Zetek to Barbour, 2 July 1933; Zetek to Barbour, 5 July 1933. Box 26, Folder 3, RU 135, SIA.

residents' needs against the potential for a more natural appearance was apparently unproblematic to Zetek and Barbour. At the same time, Zetek bristled over suggestions from representatives of Goodyear that part of the island be cleared for Hevea trees, creating an experimental rubber plantation. He wanted to "keep everything out that was not originally here," and Barbour, despite his eagerness to cooperate with companies in other ways, agreed.[494] By the 1960s, however, the BCI garden was allowed to transition to forest. The lines between natural and human seem to have sharpened over time. Today, even the lab clearing has been allowed to fill in with vegetation, in spite of the objections of laborers and "old timer" researchers who found the clearing beautiful and argued that it helped BCI to support a larger number of species by increasing habitat diversity.[495]

In the case of the *Hevea*, experimentation seems to have gone forward on another island. Likewise, in the name of protecting BCI's flora and fauna, the station's administrators often, in effect, exported the environmental impact elsewhere. For example, while most captive experimentation on BCI's vertebrate animals was temporary, ending in release, some physiological work involved vivisection and resulted in the animal's death. In the 1920s and 1930s several researchers, including S. W. Britton, George B. Wislocki, and Curt P. Richter, were interested in sloth physiology, including problems of its neurophysiology, thermoregulation, reproductive anatomy, and the role of the endocrine system in the organism's

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494. Zetek to Barbour, 15 May 1925. Box 25, Folder 3, RU 135, SIA; Zetek to Barbour, 22 May 1935, and telegraphed reply 27 May 1935. Box 22, Folder 6, RU 135, SIA.

495. Giselle Mora, Oral history interviews with Fausto Bocanegra, August 1988, RU 009561, SIA; Neal Griffith Smith, Oral history interview with Robert K. Enders, 13 April 1976, RU 009562, SIA. The exact dates and circumstances of the termination of the BCI garden are not clear from the station's records, although changes in the vegetation are noticeable in photographs, particularly those in the Digital File Manager, STRI.

famously slow metabolic rate.[496] Zetek was personally quite disgusted by the work (and even more concerned about how female visitors might react to the sight of the mutilated bodies of sloths accumulating below Britton's balcony).[497] Nevertheless, he paid locals to supply Britton and others with the sloths they wanted from other Canal islands and the mainland, protecting BCI's resident animals at a cost to those in the surrounding area.

In spite of Zetek's strenuous efforts to maintain BCI's natural balance, the station's activities had unintended consequences for the island's ecology. The abundance of fruit in BCI's garden and the presence of garbage near the laboratory building affected the behavior and population numbers of some of BCI's more charismatic mammals. Coati mundi, known in Spanish as *gato solo* and taxonomically as *Nasua narica*, became a particular problem. Not only were these raccoon relatives intelligent and opportunistic, their antics encouraged scientists, tourists, laborers, and Zetek alike to feed them—a probable cause of their eruption in abundance by the 1930s. In spite of the policy against killing vertebrates, when Karl Schmidt, traveling with the Crane Pacific Expedition of the Field Museum of Natural History, requested permission to collect a "small series" of birds and mammals, Barbour only half joked that "if Mr. Schmidt's party would make up a series of about one thousand Nasua I think we would give him a crown of

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496. S.W. Britton, "On Deslothing the Sloth," *Science* 90, no. 16-17 (1939). For the range of work, see for example, Curt P. Richter, and Leo H. Bartemeier, "Decerebrate Rigidity of the Sloth," *Brain* 49, no. 2 (1926): 207-25; Orthello R. Langworthy, "A Physiological Study of the Cerebral Motor Cortex and the Control of Posture in the Sloth," *Journal of Comparative Neurology* 62, no. 2 (1935); George B. Wislocki, and Robert K. Enders, "Body Temperatures of Sloths, Anteaters and Armadillos," *Journal of Mammalogy* 16, no. 4 (1935); S.W. Britton, and R.F. Kline, "Augmentation of Activity in the Sloth by Adrenal Extract, Emotion and Other Conditions," *American Journal of Physiology* 127, no. 1 (1939).

497. Zetek to Barbour, 22 January 1938. Box 15, Folder 2, RU 135, SIA; Zetek to Barbour, 20 February 1938. Box 26, Folder 4, RU 135, SIA.

gold."[498] A few years later, Zetek reported on having "deported" some coatis to the mainland and made better efforts to contain food waste in response the coati depredation.[499] By the 1950s, administrators resorted to culling the coati population near the laboratory building. They did so discreetly, however, recognizing that coatis had come to "have a prominent part in the show" for tourists; a moderate population was advantageous for entertaining visitors who might otherwise be disappointed at not having encountered the abundant wildlife of their tropical expectations.[500]

Tourists were not the only ones seeking a personal relationship with wildlife. Getting close to nature meant not only reaching for the canopy or studying organisms on the dissecting table, but getting to know living animals as individuals.[501] BCI's organisms were direct actors in the life of the island, objects of (and sometimes impediments to) observation, however, they also played a role in scientists' emotional experience of the island. In the words of Alfred O. Gross, an ornithologist of Bowdoin College, BCI allowed naturalists to dwell with "jungle companions," studying "their innermost secrets of life."[502] Chapman spoke of his "monkey neighbors" and discussed many of the animals whose behavior he studied as if they were close friends.[503] He fancied that, as he observed them, they watched back, "I sometimes feel that I am

498. Barbour to Zetek, 5 November 1928. Box 25, Folder 4, RU 135, SIA.

499. Zetek to Barbour, 20 January 1930. Box 25, Folder 5, RU 135, SIA.

500. Alexander Wetmore to John Enos Graf, 1956. Box 22, Folder 6, RU 135, SIA. In recent years, coatis re much more difficult to see on BCI and the surrounding area. Their populations fluctuate greatly, and in recent years tuberculosis and canine distemper have taken their toll.

501. On individuals in animal histories see Georgina M. Montgomery, "'Infinite Loneliness': The Life and Times of Miss Congo," *Endeavor* 33, no. 3 (2009): 101-05; Linda Kalof, and Georgina M. Montgomery, *Making Animal Meaning* (East Lansing, MI: Michigan State University Press, 2011).

502. Alfred O. Gross, "A Jungle Laboratory - Companions of the Wild at Barro Colorado Island," 15.

503. Calhoun, C. H., "Jungle Folk Sanctuary is Provided in Panama."; Frank Michler Chapman, *Life in an Air Castle*, 161. On scientists' practice of naming individual animals, see Gregg Mitman, "Pachyderm Personalities: The Media of Science, Politics, and Conservation," in *Thinking With Animals: New Perspectives on Anthropomorphism*,

an object of no small interest to Bats, Coatis, Monkeys, Trogons, and other beasts and birds that live in the adjoining forest. I encourage these investigations and intimacies. The more the native inhabitants know about me the better friends we shall be, for my intentions are above suspicion."[504] Scientists often named animals that they encountered frequently in the forest or in the cages near the laboratory building, and some were cared for like pets by staff and scientists alike. Chapman shared his experiences of the "jungle folk," chronicling for his readers the lives of Claudia, a forlorn baby howler monkey, and Peter and José, the coatis who skillfully solved the puzzles he set before them.[505] In Chapman's absence, José "was adopted by the Laboratory family. Possibly it would be more accurate to say that the family was adopted by José...seven months later I found him occupying much the same position as a household cat."[506] Chapman himself was too saddened by Claudia's death, after a year in captivity, even to examine her preserved body.[507]

Plants and animals were challenging, both physically and philosophically, posing the problem of defining where nature began on BCI. Organisms forced BCI's scientists and administrators to come to terms with the blurry boundary between the wild and the human, between the jungle and the laboratory, even if their choices were not always consistent or

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ed. Lorraine Daston, and Gregg Mitman (New York, NY: Columbia University Press, 2005).

504. Frank Michler Chapman, *My Tropical Air Castle*, 21-22.

505. Frank Michler Chapman, "The Conquest of Claudia," *Natural History* 29, no. 4 (1929): 369-79; Frank Michler Chapman, *My Tropical Air Castle*; Frank Michler Chapman, "Presenting the Coati," *Scribner's Magazine* March (1929): 292-300; Frank Michler Chapman, *Life in an Air Castle*.

506. Ibid., 54.

507. Many years later, BCI scientists continued to name and take a personal interest in individual animals. A touching example from a later period is this notice of the passing of the well-loved head of the island's only troupe of spider monkeys: Robert Silberglied, "The Death of Chumbo," August 1978. Box 13, Folder 10, RU 7316, SIA.

unproblematic. Keeping BCI natural could never be a simple matter of leaving the island to its own devices, and maintaining the island as a home for scientists meant some compromises had to be made. The "natural" state of BCI was the product of conscious planning and labor to strike a balance between wildness and access for researchers.

Conclusion

"Time, developing Thoreau's concept," Chapman wrote, "has placed a foundation beneath my tropical air castle." BCI was, finally, a tropical wilderness that he could call home. Chapman explained, "for the naturalist Barro Colorado's greatest attractions are, *first*, the prevailing conditions are primitive; *second*, they are under control."[508] Chapman's first point was hailed unproblematically by all of BCI's visitors. Within reach of the average American biologist was a "practically virgin tropical rain forest," a "virgin, luxuriant tropical jungle."[509] To those inclined to a more spiritual turn of phrase, it was an "island ark," a "scientist's Eden," or a "naturalists' paradise."[510] Even Fairchild, who did so much to promote economic botany and experimental plantations at stations like Soledad, said that he had "not seen any place in my travels which compares with Barro Colorado Island in point of excitement of the field naturalist kind." In the

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508. Ibid., preface and foreword. Italics are in original.

509. Edward Alphonso Goldman, and James Zetek, "Panama," 620; Thomas E. Snyder, "Barro Colorado Island - a Scientist's Eden in a Wild Life Preserve," 332.

510. Alfred O. Gross, "A Jungle Laboratory - Companions of the Wild at Barro Colorado Island," 14; Thomas E. Snyder, "Barro Colorado Island - a Scientist's Eden in a Wild Life Preserve." Barbour and his friends used "naturalists' paradise" to refer to BCI in its early days. For example, Fairchild to Barbour, 13 August 1923. Box 14 "Fairchild, David," Folder "1923-1928," Records of the Museum of Comparative Zoology, 1860-1985 UAV 298.20, HUA. See also Wilcox, Uthai Vincent. "A Paradise for Zoologists." *New York Times (1923-Current file)*, 1928, 98-98; Roger Brownell, "An Island Eden in Panama," *Travel* 54(4), no. 4 (1930): 39-40; Chapman, Frank Michler, "In an Eden Where Man Befriends Beast."; Elbert C. Cole, "Jungle Paradise," *The Middlebury College News Letter* 12, no. 4 (1938).

Dutch and British colonies, he noted, they "have built palatial laboratories, but these are far removed from the new, fresh, wild jungle." "Everywhere," he warned, "it is the destructive activity of man that is clearing off the jungle and replacing the gorgeous forest with weedy growth or rubber plantations in rows. Hold the virgin character of Barro Colorado at all costs."[511] Amid the massive changes wrought by the engineering of the Panama Canal, and surrounded by the constant flux of the world's commerce, one visitor voiced hope that BCI would always maintain nature's "*status quo*."[512]

But the question of what constituted the natural *status quo* to be preserved was problematic. As we have seen, even beyond the fact that BCI was essentially a man-made island, and one partially covered by second growth forest, BCI required a scientific infrastructure to make it accessible for research. Thus, Chapman's second point—that BCI was "under control"—was in tension with his first. Control meant that the island was "set up" for scientists, organized to make living and working there convenient for them. Not only did scientists express enthusiasm about being able to visit BCI without the risk to health or comfort associated with tropical expeditions, but they recognized BCI's scientific advantages, as a permanent station, to accessing deeper knowledge of tropical environments. As Weston explained:

> An expedition can reach far-off regions and stay long enough to gather material, notes, and data, later worked over in detail to yield results of scientific value within certain fields. Certain types of work, however, can be done only in a permanent station, and such work on the wealth of important material in which the tropics abound can be done only in adequate tropical stations. Painstaking, long-continued, and persistent work that is demanded in ornithology in following the nesting habits and the rearing of the young...life histories of certain parasitic fungi, the development of certain little-known

511. Calhoun, C. H., "Jungle Folk Sanctuary is Provided in Panama," 13.

512. John C. Van Dyke, "Barro Colorado; the Wild-Life Preserve," 211.

> orchids and ferns, the intricate relation of some types of plants to the several factors of the environment, require a station such as this.[513]

Expeditions, although they might enter the heart of the jungle, could skim only the surface of the problems of living tropical organisms, and their relationships with the environment and with each other. Intensive study required a jungle "home," and the practice of a form of "residential science."[514] Residence, however, left a footprint on BCI's landscape. Living and working there meant controlling the island through human labor and technology, a fact in tension with the image of BCI as total wilderness.

BCI was not a wilderness, but a working scientific landscape. It became a hybrid ecosystem, one that included not only plants, animals, and the island itself, but also diverse groups of humans and their technologies. Zetek and Barbour—and as we will see in the next chapter BCI's subsequent administrators—sought to maintain a balance among these elements that would attract researchers and enable their work, and yet allow the island's plants and animals the largest concomitant degree of freedom to live their lives and carry out natural processes. This was not the complete freedom connoted by wilderness, but neither was it a simplified model system of the laboratory. Managing this balance was always a difficult task, involving negotiation among the many actors involved. The system was never perfect. Nevertheless, it allowed researchers a very rare intensity of access to a living tropical forest. Mediated through the island's unique natural, social, and technological infrastructure, scientists could observe tropical nature on BCI more deeply than possible on a traveling expedition, and

513. William H. Weston Jr., "The Biological Station on Barro Colorado Island."

514. Robert E. Kohler, "Paul Errington, Aldo Leopold, and Wildlife Ecology: Residential Science."

with a greater complexity of factors than permitted in a metropolitan laboratory. While all scientific spaces are in some ways "systems" for research, mediating scientists' observation of nature through technologies and social relations, and constrained by the materiality of natural things themselves, the arrangement that evolved at BCI structured biologists' engagement with tropical nature in new ways.

At the most basic level, the bounded space of the island not only facilitated its management, but also made it, in Fairchild's words, able to "be studied as a unit of jungle."[515] Or as one journalist expressed it, there was "no other place like it, where within so small and manageable compass the history and process of life can be studied as a going concern microcosmically, so to speak and cannot change or get away in any direction."[516] As we will see in the next chapter, BCI's nature as an island and a "unit" of study resonated with emerging frameworks in ecology, including the concepts of ecological communities and ecosystems, and also allowed it to be put into service as a "natural experiment" for new theoretical perspectives in island biogeography.

At the same time, the relative stability of the system assembled on BCI allowed much longer-term observation of the ecology of this "unit" of tropical forest than was previously possible. Certainly, the station was a fragile system and one needing constant maintenance. Nevertheless, BCI was ultimately resilient enough to outlast other American tropical stations—particularly those aimed at basic forest ecology like Cinchona or Beebe's station in British Guiana, both of which folded in the same decade that BCI emerged. BCI's resilience—in space

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515. David Fairchild, *The World Was My Garden*, 468.

516. John Palmer Gavit, "Barro Colorado - Outpost of Research," *Survey Graphic* 24, no. 4 (1935), 199.

on the island, as well as forward through time—meant that researchers could spend weeks or months studying living tropical organisms, and could return, as many did, over and over again through the years. As we will see in more detail in the next chapter, this extension of research through time opened up many areas that would be invisible to shorter-term research—the study of animal behavior, life histories, mutual adaptations among BCI's plants and animals communities, tropical seasonality, and changes in the composition of species on the island over the decades.

Finally, as we will see in more detail in the following chapter, BCI's stability and the concentration of research in one place allowed diverse knowledge to accumulate over time—a phenomenon that BCI researcher Egbert Leigh has called a "unity of place," where each research project provides "both the empirical background and intellectual foundation for further work, and essential context for other, very different projects."[517] In this way, the complexity of the tropical forest could itself become the subject of inquiry. Although BCI was laboratory-like in its disciplining of space and limitations on entry, unlike a laboratory, scientific control was approximated not by banishing ever more conflating variables, but by monitoring more and more variables within the space of the island.

Thus, the tension between Chapman's two points did not negate the scientific purpose of the station. It was a productive tension, allowing biologists—nearly—to tame the complexity of BCI while leaving it intact. And as American biologists became embedded in BCI, transforming the island into a place for science, it in turn changed them. Scientists became invested in BCI, returning year after year and encouraging colleagues and students to visit. Becoming bound to

517. Egbert Giles Leigh Jr., "Barro Colorado."

this place, at least a proportion of the American biological community began to see the tropics not primarily as an exotic locale, but as a familiar home. As we will see in the next chapter, these researchers began to identify themselves as “tropical biologists.” As tropical nature was constructed at BCI, research at BCI helped to construct the discipline of tropical biology. Tropical biologists and the “jungle island” made each other.

Chapter 5

"A Full Scale Program for Tropical Ecology": The Emergence of an Interdiscipline

"We have our biological work pyramided in complexity"

In September of 1944, Orlando Park addressed the Ecological Society of America (ESA) as that organization's past president. Speaking just three months after D-Day, Park took the time to imagine the future role of American ecologists in a post-war world.[518] For the past two years, annual meetings had been cancelled due to interruption by the war. As his colleagues gathered after this long absence, Park articulated ten recommendations to improve what he perceived as the lackluster professional status of ecology. While his first nine points concerned the overall promotion of ecological teaching, research, and increased cooperation with other subdisciplines of biology, Park devoted an unusual degree of attention to his tenth recommendation. "An effort should be made to develop a full scale program in tropical ecology," he argued, "This is one of our last chances for international distinction." Tropical ecology, he argued, "would fit into what appears to be a general program for postwar hemispheric cultural solidarity and economic cooperation."[519] He believed that with peacetime, the "development of the American tropics" was all but inevitable. He saw economic development as positive for the people of the region, but

518. Orlando Park, "Observations Concerning the Future of Ecology," *Ecology* 26, no. 1 (1945): 1-9. The war's effect on the meeting are described in F. R. Moulton, "Annual Meeting at Cleveland, Ohio, September 11-16, 1944," *Science New Series* 99, no. 2569 (1944): 227-29; "Proceedings: Ecological Society of America, 1944: Meeting, September 12, 1944," *Ecology* 26, no. 2 (1945): 216-34.

519. In his optimism about international unity and cooperation, as in his ecological ideas, Park drew from the social thinking of the "Chicago School" in which he was trained; Gregg Mitman, "From the Population to Society: The Cooperative Metaphors of W. C. Allee and a. E. Emerson," *Journal of the History of Biology* 21, no. 2 (1988): 173-94; Gregg Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950* (Chicago, IL: University of Chicago Press, 1992).

worried that the potential for environmental destruction was enormous. Ecology's role should be, on one hand, to further the process of development by allying with tropical medicine and agriculture, but on the other hand, to help to "prevent the repetition of past mistakes in conservation or lack of conservation made elsewhere."[520] Park's speech represented the tropics not only as an intellectual frontier for American ecology, but also as a proving ground for the profession, one that would allow ecology to showcase its relevance to the project of hemispheric development that would begin as the war finally came to a close.

Park's recommendation was also, he admitted, "frankly the consequence of a strong personal bias."[521] He had spent several summers on Barro Colorado Island (BCI) beginning in 1936 investigating the question of why some animals are nocturnal. His work there provided the crucial tropical component of his over a decade year long project to understand the ecology and evolutionary origins of nightly rhythms of activity within animal communities.[522] He had been introduced to BCI as a student at the University of Chicago of Warder Allee, whose *Jungle Island* had done so much to popularize the station well beyond the circle of his own students and colleagues. Park had also passed on his love of the tropics by bringing his own Northwestern University students to BCI.[523] Indeed, Park went so far as to specifically endorse BCI in his speech as a key site for American tropical research.

520. Orlando Park, "Observations Concerning the Future of Ecology," 6.

521. Ibid.

522. Orlando Park, "Quantitative Determination of Rhythmicity in Organisms," *Ohio Journal of Science* 41, no. 1 (1941); Orlando Park, "Concerning Community Symmetry."; Orlando Park, "Nocturnalism - the Development of a Problem."; Orlando Park, et al., "Studies in Nocturnal Ecology, IX. Further Analysis of Activity of Panama Rain Forest Animals."; Orlando Park, "Studies in Nocturnal Ecology, VII. Preliminary Observations on Panama Rain Forest Animals."

523. These included Albert Barden and Eliot Williams who worked on the nocturnalism project.

Park's field experience, he told his audience, had showed him that "before the tropics can be adequately controlled...there is a great deal of basic work to be done." Above all, he argued, biologists had only scratched the surface in understanding what, today, we might refer to as the "biodiversity" of the tropics. Park, instead, framed the problem in terms of a uniquely close relationship among taxonomy, species ecology, and community ecology in the tropics. "The diversified environment supports a tremendous number of organisms which are represented by an unusually large number of species," he explained. The sheer number of species meant that, in the tropics, taxonomy remained "a basic problem, and in many groups, for example, insects, this problem is in its infancy." An adequate understanding of species ecology—the life history, behavior, and physiological needs of individual species—depended fundamentally on taxonomic knowledge of tropical species' existence in the first place. In turn, he argued, "since the tropical species populations do not exist in a vacuum, nicely isolated from each other, but rather form natural survival units which we call communities, we have our biological work pyramided in complexity by the complex interrelationships which are developed in every community."[524] Any characterization of community ecology in the tropics, Park argued, was inextricable from continuing biological work in taxonomy and species ecology. Biologists working in the tropics would have to attack problems at every level, from the base to the point, of this pyramid of complexity.

An understanding of the dense and intricate interrelationships of tropical communities impinged directly on applied concerns, from the prevention of tropical diseases to the improvement of tropical agriculture. Park argued, however, that the intellectual richness of the

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524. Orlando Park, "Observations Concerning the Future of Ecology," 6-8.

problems they presented meant that even "from a purely scientific viewpoint, the American tropics offer a brilliant future" and should be consciously cultivated by the profession. To this end, he advocated the establishment of a new journal "dealing with tropical biology in its broadest aspects" and pointed out that his general argument "regarding conservation, fellowships, and research grants-in-aid becomes especially pertinent" in regard to tropical research. "We should be prepared," he argued, "to assume our full responsibilitity [sic] in American biology in the study of the ecology of the tropics."[525]

Park's speech highlights the three related themes that this chapter explores: the emergence of diversity as a central intellectual question among biologists working in the tropics, American biology's relationship with the changing political context of the postwar Caribbean area, and the question of the professional status of tropical biology. Thanks to stations like BCI and Soledad, as well as opportunities for work with US corporations and government agencies in US tropical holdings and during World War II, it is likely that many more members of Park's audience had experience working in the region than at any previous ESA meeting. But American ecologists were still a long way from a coherent "program" in tropical research. If Park felt that ecology as a whole still lacked a clearly defined intellectual core and professional identity in the mid-1940s, this was inarguably so for tropical ecology. American biological institutions, including Soledad and BCI, had achieved an increasingly strong foothold in the Caribbean area. Nevertheless, these dispersed efforts were only a shadow of the much broader proposals to organize American biology in the tropics—proposals that previous chapters have examined, beginning with Daniel T. MacDougal at the turn of the century and followed in their own ways

525. Ibid., 8.

by Thomas Barbour and the Institute for Research in Tropical America in subsequent decades. Certainly, none of Park's audience members would have self-identified as a "tropical ecologist" or "tropical biologist."

Yet, within two decades, American biologists could become members of two professional organizations for tropical biology. They could also publish in several new specialty journals specifically devoted to tropical research. When Park spoke in 1945, tropical biologists had few sources of funding outside of their university departments or applied corporate or government work. By 1960, the National Science Foundation was funding research projects and national conferences on topics in tropical biology.

This chapter traces the emergence of tropical biology as a coherent field of research, intellectually and professionally, in the 1950s and 1960s. Tropical biology became an "interdiscipline," bearing many of the hallmarks of a discipline or subdiscipline, but bringing together members with primary professional identities from across the full range of biological disciplines. As an interdiscipline, tropical biology never erected sharp boundaries around itself, but instead came together as a community around a set of core interests. At the time of Park's ESA speech, tropical publications did not constitute a coherent body of work. With a few important exceptions, as we will see, they tended to add evidence to problems emerging primarily out of other biological disciplines, such as ethology or physiology, and they did not consistently cite each other or a core body of tropical work. By the 1950s and 1960s, however, tropical biology was generating its own research questions. In effect, biologists would attempt to turn Park's pyramid of complexity on its head, asking how the interrelationships of biological communities could explain species diversity in the tropics. From Theodosius Dobzhansky's meditation on “Evolution in the Tropics” to Paul W. Richards’ landmark *The Tropical Rain Forest: An Ecological Study*, and from Marston Bates’ popular, *Where Winter Never Comes*, to Robert H. MacArthur’s slew of highly mathematized studies, tropical species diversity would become recognized as a distinct biological problem to be addressed by ecological and evolutionary approaches.[526]

Although the intellectual core of tropical biology emerged out of research that took place throughout the global tropics, this chapter begins by returning to BCI. Through the first half of the twentieth century, BCI attracted the greatest concentration of American biologists working on basic tropical research. Moreover, as a permanent research station, and one situated both within a tropical forest and on an island, BCI was particularly well suited to the articulation and study of species diversity. Within a bounded unit of a highly species-rich environment, biologists could document the numbers and composition of BCI’s species over time. Moreover, they could observe behavioral and ecological interactions within and among those species over extended periods, providing fodder for evolutionary and ecological understandings of the composition of species on the island. Thus, I begin by showing how the problem of species diversity emerged in the context of research at BCI before expanding out once again to take in the wider view of tropical biologists’ efforts to organize themselves in the second half of the chapter.

As the first section of this chapter will show, pre-war research on BCI closely resembled Park’s “pyramid” model. While individual studies were largely disconnected, a strong tradition

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526. Theodosius Dobzhansky, “Evolution in the Tropics,” *American Scientist* 38 (1950): 209-21; P. W. Richards, *The Tropical Rain Forest*; Marston Bates, *Where Winter Never Comes: A Study of Man and Nature in the Tropics* (New York: Scribner, 1952); Robert H. MacArthur, et al., “On the Relation Between Habitat Selection and Species Diversity,” *The American Naturalist* 100, no. 913 (1966): 319-32; Robert H. MacArthur, “Environmental Factors Affecting Bird Species Diversity,” *The American Naturalist* 98, no. 903 (1964): 387-97; Robert H. MacArthur, “Patterns of Species Diversity,” *Biological Reviews* 40, no. 4 (1965): 510-33.

of community ecology and animal behavior studies, in addition to significant headway in surveying the island's species, was in place at BCI by the 1940s. In part due to BCI's location in the strategically important Canal Zone, however, the institution suffered neglect during World War II. Thomas Barbour's death in 1946 dealt a second blow. The next section of this chapter examines how the station was reorganized and rejuvenated after the war under its new administration by the Smithsonian Institution, and with the help of BCI's "alumni" scientists. Then, in the final two sections, we will see how tropical biology coalesced intellectually and institutionally at BCI and beyond. A new generation of tropical biologists built on the foundations constructed by the previous one, while drawing from broader developments within biology, to ask how ecological and evolutionary processes might actually produce the diverse species composition witnessed on BCI, and, ultimately, to consider the degree to which their observations held true throughout the tropics.

Community Ecology and a Community of Ecologists in the Tropics

Five years before his address to the ESA, as war was only beginning to brew far off in Europe, Orlando Park found himself taking part in another discussion concerning the place of the tropics in biology and ecology on a fall evening in 1939 at the home of Park's mentor, W. C. Allee. This gathering of the Chicago "Ecology Group" had been designated "Barro Colorado Night."[527] Almost fifty graduate students and faculty from the University of Chicago, Northwestern University, the Field Museum, and other nearby institutions assembled to hear Victor Dropkin speak about the social biology of termites and his experiments on their symbiotic gut protozoa at

527. On the Ecology Group, see Gregg Mitman, *The State of Nature*, 134.

BCI. Dropkin was a graduate student of Alfred Emerson, who was also in attendance.[528] Emerson was himself a veteran of tropical termite work at BCI, as well as in the Belgian Congo and British Guiana—where he had been introduced to the subject as a Cornell graduate student by William Beebe in 1920.[529] Emerson had encouraged Dropkin to work at BCI after spending six months there in 1935. After Dropkin's talk came a presentation of slides and Kodachrome movies captured by Ralph Buchsbaum, a former student and current member of the University of Chicago zoology department, and his wife and collaborator Mildred Buchsbaum. During the previous summer, the two had studied the distribution of invertebrates in relation to small, temporary pools of water on the island.[530]

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528. Victor H. Dropkin, "Host Specificity Relation of Termite Protozoa," *Ecology* 22, no. 2 (1941): 200-02; Victor H. Dropkin, "The Use of Mixed Colonies of Termites in the Study of Host-Symbiont Relations," *The Journal of Parasitology* 32, no. 3 (1946): 247-51. Emerson also cited Dropkin's unpublished work as evidence of how termites use chemical markers to distinguish colony members from outsiders; Alfred E. Emerson, "Social Coordination and the Superorganism," *American Midland Naturalist* 21, no. 1 (1939), 191.

529. New York Zoological Society, *Annual Report of the New York Zoological Society*, vol. 25 (New York, NY: The Society, 1920); Alfred E. Emerson, "The Termites of Kartabo, Bartica District, British Guiana," *Zoologica* 6, no. 4 (1925): 291-459. For several years, Emerson had also attempted to reestablish a station at Kartabo, after Beebe had moved on from the site to pursue the study coral reefs. He organized some of the earliest field classes in tropical biology for American students there, in 1924 and 1925, while he was an instructor at the University of Pittsburgh. For example, see Alfred E. Emerson, "The Jungle Laboratory of Tropical Biology Conducted by the University of Pittsburgh," *Science* 61, no. 1576 (1925): 281. Emerson had actually approached Barbour about the possibility of Kartabo and BCI "cooperating for the good of the science;" see Emerson to Barbour, 20 February 1929. Box 2, Folder 1, RU 134, SIA. But with BCI still lacking its own endowment (and probably influenced by his low opinion of Beebe's scientific work), Barbour never followed up on Emerson's proposition. By the 1930's Emerson seems to have given up on Kartabo and was sending students like Dropkin to BCI instead. On Emerson's tropical entomological work, see Edward O. Wilson, and Charles D. Michener, "Alfred Edwards Emerson 1896-1976," *National Academy of Sciences Biographical Memoirs* (1982): 157-77.

530. Ralph Buchsbaum, best known for the breakout popular success of his 1938 book *Animals Without Backbones*, was also responsible for exposing a broad set of audiences to BCI. He continued to present slides and color film of BCI at various venues, including, for example, the annual meeting of the Illinois State Academy of Science at Northwestern University in 1941, where close to one thousand Junior and Senior Academy members attended his talk entitled "A Summer in a Tropical Rain Forest of Barro Colorado Island, Panama;" R. F. Paton, "The Illinois State Academy of Science," *Science New Series* 93, no. 2426 (1941): 621-22. In the 1960s, as a producer of films for Encyclopaedia Britannica, Buchsbaum also included BCI among his subjects; for example Ralph Buchsbaum, "The Tropical Rain Forest" (Film, Distributed by Encyclopaedia Britannica Educational Corporation, 1961); John Walker, et al., "Army Ants: A Study in Social Behavior" (Film, Encyclopaedia Britannica Educational Corporation, 1966). Buchsbaum's efforts to collect footage are mentioned in Folder 4 "Photographs: Correspondence on Films, Photographs and slides" in Box 10, RU 135, SIA; *Annual Report of the Board of Regents of the Smithsonian*

A "free-for-all discussion" followed, and attendees enthused about the progress to-date of research at BCI and the future potential of work there. A show of hands revealed that at least as many of the Ecology Group members present had been to BCI as had attended MBL at Woods Hole.[531] "Now when you can compete with the Marine Biological Station, of many years' reputation, you really are 'getting places,'" Dropkin wrote to Zetek.[532] Whether experience at BCI would have been as prevalent at any similar local meeting is debatable, but the Chicago "Barro Colorado night" does speak to how tropical experience had begun to spread through the American biological community, from advisors to students, through repeated station visits. Certainly BCI's popularity was by the 1930s ascendant among devotees of the "Chicago school" of ecology, a group of scholars who emphasized social behavior and cooperation within and among populations of organisms.[533] Not only was this group led by BCI visitors Allee, Emerson, and Park, all of whom praised the island as a site for tropical research, but BCI also facilitated the observation of exactly the kind of complex, interconnected communities that resonated with the group's interests. By the close of the evening, Dropkin reported, "we were all ready to stand up and cheer for BCI."[534]

Institution, 1962, vol. 4518 (Washington, DC: US Government Printing Office, 1963), 182; Smithsonian Year, 1966 (Washington, DC: Smithsonian Institution, 1966), 169. See also Ralph Buchsbaum, *Animals Without Backbones* (Chicago: The University of Chicago Press, 1938).

531. Dropkin to Zetek, 7 December 1939 and Ralph Buchsbaum to Zetek, 18 November 1939. Box 2, Folder 7, RU 134, SIA. Dropkin stated that more attendees had been to BCI than MBL, while Ralph Buchsbaum claimed there were "exactly the same number" who had been to each station. According to Buchsbaum, these included Allee, the Buchsbaums, Dropkin, Emerson, Orlando Park, his students Albert Barden and Eliot Williams Jr., and Paul Voth. No mention was made of Karl P. Schmidt, who was also a regular at the Ecology Group and who had visited BCI while traveling with the Crane Pacific Expedition in 1928.

532. Dropkin to Zetek, 7 December 1939. Box 2, Folder 7, RU 134, SIA.

533. Gregg Mitman, *The State of Nature*.

534. Ralph Buchsbaum to Zetek, 18 November 1939. Box 2, Folder 7, RU 134, SIA.

Only Thomas Park, Orlando's younger brother, and an influential ecologist in his own right, expressed skepticism about research at BCI. Thomas Park was well-known for his advocacy of experimental ecology and statistical approaches.[535] In Ralph Buchsbaum's view, Thomas Park showed "unconcealed contempt for any kind of descriptive work in zoology." Buchsbaum explained that Thomas Park "created quite a stir by suggesting that the research at BCI was of questionable value because it was not 'quantitative.' From Orlando Park and Allee down to Mildred and myself, we all jumped on him." Emerson, who had himself performed experimentation on termite colonies, was particularly quick to protest against Thomas Park's charge.[536] Emerson "called attention to many papers of a careful, quantitative nature and then ended up by citing someone's estimate that BCI was turning out more publishable research per dollar than any other biological station."[537] Others took Thomas Park's remarks as an opportunity to reassert the need for work in natural history, especially considering that so many tropical organisms had hardly been observed in the wild. Mildred Buchsbaum, to her husband's evident delight, quipped that BCI was "a place where zoologists might take some time off from laboratory research to learn a few things about animals."[538]

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535. David B. Mertz, "Thomas Park, 1908-1992," *Bulletin of the Ecological Society of America* 73, no. 4 (1992): 248-51.

536. In fact, to both Barbour and Zetek's horror, Emerson had hoped to import a termite colony from BCI to his Chicago laboratory. Barbour thought he should "rather expect a jail sentence...than a permit" to import living—and presumably hungry—termites into the US. Zetek to Barbour, 13 May 1929, and Barbour to Zetek, 27 May 1929. Box 25, Folder 5, RU 135, SIA.

537. Ralph Buchsbaum to Zetek, 18 November 1939. Box 2, Folder 7, RU 134, SIA. The source of Emerson's statistic is not clear, although it sounds like something that Zetek would say. Given the station's shoestring budget and rapidly increasing publication list, it is not unreasonable.

538. Ralph Buchsbaum to Zetek, 18 November 1939. Box 2, Folder 7, RU 134, SIA.

At first glance, it might be easy to side with Thomas Park. For certain, the work completed at BCI during the station's first three decades, while voluminous, was eclectic and uncoordinated (see Appendix 6). Although they worked hard to manage both the daily operations of the island and the flows of researchers and funds from outside, neither Zetek nor Barbour had sought to direct the type of research done on the island. As discussed in Chapters 2 and 3, Barbour loathed efforts to manage scientific research and believed in absolute freedom for researchers. Apart from placing limits on collecting specimens or doing experimental research that might be destructive to the island's fauna and flora, they allowed scientists to proceed at their discretion. Without a scientific staff for guidance, BCI's visiting researchers were largely on their own. Indeed, historian of science Joel Hagen has characterized work on BCI as lacking "a common intellectual perspective" or "coherent research program" until the 1960s.[539] Even BCI's present-day researchers tend to categorize early BCI work as primarily descriptive natural history, reserving their accounts of the station's role in theoretical debates in biology to the past few decades. In fact, the varied responses to Thomas Park's criticism of BCI research in the 1930s—at once a defense of the quantitative nature of some work done at the station and a protest against mainstream biology's emphasis on laboratory experimentalism to the neglect of organisms living in nature—seem to hint at a certain level of contradiction even among the enthusiastic members of the Chicago ecology group.

Nevertheless, a closer look at the first twenty years of BCI research reveals underlying themes, a general interest both in the composition of the island's animal and plant species and in

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539. Joel B. Hagen, "Problems in the Institutionalization of Tropical Biology: The Case of the Barro Colorado Island Biological Laboratory," 238.

how these species interacted with the environment and each other. While ecologists elsewhere may have sought to distinguish ecological from natural historical work, such as taxonomy or life history studies, these practices were actively synthesized at BCI.[540] A broad emphasis on community ecology pervaded work on BCI, yet, due to the island's large numbers of relatively unknown species, community ecology necessarily occurred in close parallel with efforts in taxonomy and the biology of individual species (or "species ecology" in Orlando Park's words). In short, early research at BCI provided the model for the portrait of tropical biology that Orlando Park would paint at the 1944 ESA meeting.

Community ecology and the question of diversity

Those who came to investigate BCI's plant and animal communities encountered a rich set of questions. In 1924 shortly after the station's opening, Allee had made his two month stay on BCI, which became the basis for several scientific articles and the popular *Jungle Island.* His interest had been in making studies of the effect of the physical factors of the environment, such as light, moisture, and water pH, on the distribution of animal life, both along the length of one of the island's streams and vertically from the forest floor and up his ninety foot "spike ladder" into the canopy (as discussed in the previous chapter).[541] Although he focused on animals rather than plants, Allee's methods and interest in how organisms adapted to the action of the physical environment were similar to those of Forrest Shreve, who we encountered in Chapter 1—in fact

540. For a discussion of ecologists' efforts to separate themselves from natural history, see for example Robert E. Kohler, *Landscapes and Labscapes*, 74-78, 92-93.

541. Warder Clyde Allee, and Magnhild Torvik, "Factors Affecting Animal Distribution in a Small Stream of the Panama Rain-Forest in the Dry-Season."; Warder Clyde Allee, "Distribution of Animals in a Tropical Rain-Forest With Relation to Environmental Factors."; Warder Clyde Allee, "Measurement of Environmental Factors in the Tropical Rainforest of Panama."

Allee cited Shreve to compare conditions in Panamanian and Jamaican forest.[542] His preliminary conclusions were that "the forest floor...maintains the greater variety and the greater number of animals," a pattern he attributed to the difficulty of adapting to arboreal life as compared to the more constant and moist conditions in the litter of the forest floor.[543] In Jamaica, Shreve had found no evidence of a single dominant flora of the tropical rain forest as predicted by Clements, seeing instead "diversity of life forms" undergoing constant change.[544] Allee took a somewhat different tack, eschewing the question of dominant species and suggesting instead that canopy trees, of whatever species, could be considered the dominants because they "received the full impact of the environment" and modified conditions for other organisms.[545] In a tropical forest, Alee argued, the animal community depended on the trees. Given the brief nature of his time on BCI, Allee admitted that his findings were "tentative pending intensive study."[546] Although not personally able to return to BCI, Allee continued to publish on the data collected on his initial

542. Forrest Shreve, *A Montane Rain-Forest*; Warder Clyde Allee, "Measurement of Environmental Factors in the Tropical Rainforest of Panama," 294-296, 299. Bennet notes that Allee's BCI data were still being regularly cited into the 1960s, in spite of their basis in one dry season's worth of measurement, a situation which he considered "mute testimony to the fact that studies of this kind have not advanced a great deal in the intervening 40 years." Charles F. Bennett, "A Review of Ecological Research in Middle America," *Latin American Research Review* 2, no. 3 (1967), 12.

543. His difficulty maneuvering in the canopy may account for his difference from present-day biologists on this account. Warder Clyde Allee, "Distribution of Animals in a Tropical Rain-Forest With Relation to Environmental Factors," 461, 467.

544. Forrest Shreve, *A Montane Rain-Forest*, 109-10.

545. Warder Clyde Allee, "Distribution of Animals in a Tropical Rain-Forest With Relation to Environmental Factors," 466.

546. Warder Clyde Allee, "Animal Ecology of the Tropical Rain Forest," *Ohio State University Bulletin* 36, no. 3 (1931), 465.

visit, and kept up with research on the island through his colleagues, students' visits, and correspondence.[547]

Even Allee's brief studies of the island began to raise questions about the diversity of species and inter-species interactions on the island, and by extension of the tropics more broadly. Although most of his publications based on BCI research focused on correlating physical environmental factors with the distribution and number of species, by 1931 he published a paper on the animal ecology of tropical forest that, while still discussing physical factors, also highlighted the complex interrelationships of tropical forest communities. Allee began first by dispelling simplistic myths about the tropics—the supposed constancy of the climate, inherent unhealthiness, and prevalence of "jungle" vegetation, for example. Then, he ventured to suggest twenty-five broad generalizations, "without noting certain exceptions and reservations," based on his experience at BCI and supplemented by some of the currently available literature.[548] Some of these generalizations dealt with climatic phenomena of the tropics—the greater significance of wet and dry seasons as compared to cold and warm ones, for example. The majority of his generalizations, however, dealt with patterns of species diversity and their affect on animal adaptations. For example, Allee commented on broad latitudinal patterns, such as the fact that "reptiles have their greatest number of species and of individuals in the tropics. Birds are more abundant in the tropics than elsewhere," and similar patterns in primates and nectar-feeding animals. He also expressed perhaps the quintessential truism about tropical forests, that "Tropical forests tend to be occupied by many species with relatively few individuals per species." He

547. See Folder 1, Box 15, RU 135, SIA.

548. Ibid., 100.

offered a concomitant observation about tropical forest trees; he noted they "usually do not exist in stands of single or few species, but individuals of different species are scattered more or less singly through the forest." This pattern imprinted itself in the food habits and distribution of fruit-eating animals, meaning that they either remained discreetly scattered throughout the forest, or else had to migrate in search of fruiting trees. Thus, many of his generalizations related to the conclusions of his earlier BCI work, emphasizing how animals adapted to an environment dominated by tropical trees. He noted the general prevalence of arborealism, even among groups of animals whose species were ground-dwelling in the temperate zone. He also commented on the prevalence in tropical forests of a variety of adaptations to living in trees, such as hooked claws, prehensile tails, and parachutes. He commented on the importance of vocalization in birds and monkeys given the poor visibility of the forest and noted the dependence of many tropical flowering plants on birds as pollinators.[549]

Allee's generalizations read as a list of future research projects. Although not phrased as questions, they begged further research into the causes of these tropical phenomena—particularly the diversity of tropical trees—given their clear differences from the relations seen in the temperate zone. The high numbers of tropical species did suggest to Allee a difference in the driving forces behind evolution in the tropics, "With the abundance of tropical species one finds that the struggle for existence between animals is extraordinarily intense in contrast with the relatively slight struggle with inanimate nature." While adaptations in the temperate zone might

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549. Strikingly absent is any reference to parasitism, another phenomenon that his contemporaries had identified as far more prevalent in the tropics. Perhaps this example was more familiar to botanists. For example, David Fairchild singled out "the phenomenon of parasitism" for future research at BCI, noting that "Everywhere one sees a wide diversity of plants with diseases and insects inhabiting and apparently consuming them, but the luxuriance of the plant continues and the worst parasites seem to fall a prey to their own enemies." David Fairchild, "Barro Colorado Island Laboratory," *The Journal of Heredity* 15, no. 3 (1924), 99.

be primarily directed against a harsh and changeable climate, in the tropics adaptations gave species a competitive edge in a crowded ecological community. Yet, many of Allee's more specific generalizations dealt, not with competition, but with mutualistic relationships. Allee did not comment on this apparent contradiction.[550] His time at BCI suggested many fertile problems in the relations of tropical forest communities, but relying only on two months of study and a mere handful of contemporary studies of other tropical communities, rigorous explanation of these patterns awaited future work.[551]

Taxonomy

Although Allee's primary interest was in assessing BCI's ecological communities, he recognized "various stages of complexity in studies on the relations between animals and their environment. One of the simplest is the determination of what animals actually live in a given region." Rather than emphasizing a contrast between natural historical methods and ecology, Allee suggested continuity from basic taxonomic to ecological work, particularly when approaching a relatively unknown and complex tropical community as at BCI. Taxonomy was "highly important" because without this work "further analysis is difficult or impossible."[552] In fact, to identify the

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550. Only one other of Allee's generalization calls directly on natural selection as an explanation of patterns of distribution, his assertion that, "the depths of the forest furnish a place of refuge from more advanced or more aggressive and better adapted animals." He included "primitive races of men" within this claim, and did not apparently see any conflict with his earlier statement about the strength of the struggle for existence among species. Warder Clyde Allee, "Animal Ecology of the Tropical Rain Forest," 100.

551. Allee's citations of recent observations of animal life are few: Richard Hesse, *Tiergeographie Auf Ökologischer Grundlage.* (Jena: G. Fischer, 1924); Stephen S. Visher, "Tropical Climates From an Ecological Viewpoint," *Ecology* 4, no. 1 (1923): 1-10; William Beebe, et al., *Tropical Wild Life in British Guiana; Zoological Contributions From the Tropical Research Station of the New York Zoological Society*; Henry Walter Bates, *The Naturalist on the River Amazons*, 2d ed. ed. (London: J. Murray, 1864).

552. Warder Clyde Allee, "Distribution of Animals in a Tropical Rain-Forest With Relation to Environmental Factors," 464-65.

species that composed the animal communities that he studied, Allee relied on cooperation from taxonomic specialists, especially those with previous BCI experience—twelve of the twenty-four scientists he thanked for species identifications were BCI researchers.[553] And as Allee's own research showed, even animal ecologists had difficulty ignoring plants and plant taxonomy when working in a tropical forest. As early as 1927, the first flora of Barro Colorado Island was published by Paul C. Standley, a botanist initially of the US National Museum and later the Chicago Field Museum of Natural History. By today's standards, BCI's flora was barely known in those early years of the station. At the time, however, Standley argued of BCI that "no local flora of tropical America is better known, and its variety is equal to that of most localities of similar altitude."[554] Standley was the foremost authority on the plants of Central America in the US during the early twentieth century, and probably no other botanist was better placed to make such an assessment. Famous for his furious pace in the field and an uncanny ability to identify the most obscure plants at a mere glance, Standley's first-pass flora was based on little more than a week on the island (in addition to specimens collected by others), but it still amounted to 611 species.[555] He was careful to acknowledge, however, that "No one familiar with tropical

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553. The BCI researchers he thanked for identifying collections were A. N. Caudell, Ralph Vary Chamberlain, R. A. Cushman, Harrison Dyar, H. E. Ewing, C. T. Greene, Carl Hubbs, Karl Schmidt, Frank Smith, Thomas Elliot Snyder, William M. Wheeler, and James Zetek. Ibid., 446. Although Hagen has emphasized the island's relative lack of a coherent scientific community in the 1920s and 30s, Allee's footnotes are one example of how BCI researchers found need to collaborate with each other even after they left the island. Joel B. Hagen, "Problems in the Institutionalization of Tropical Biology: The Case of the Barro Colorado Island Biological Laboratory."

554. Paul C. Standley, "The Flora of Barro Colorado Island, Panama," 3-4. This claim was often repeated, for example, BCI was the "best known area in tropical America" in Robert K. Enders, "Changes Observed in the Mammal Fauna of Barro Colorado, 1929-1937," *Ecology* 20, no. 1 (1939): 104-06. Another, later example states that BCI was botanically "as thoroughly known as any area of comparable size in tropical America." John E. Ebinger, "Botanical Opportunities on Barro Colorado Island, Canal Zone Biological Area," *Plant Science Bulletin* 8, no. 1 (1962): 5-7. The truth of this statement is difficult to confirm, although botanists at the time would have agreed that the flora of tropical America was very poorly understood compared to the temperate zone and given the high concentration of scientific work within the small area of the island, it is not at all unreasonable.

555. Several recollections of Standley's remarkable speed and memory can be found in Louis Otho Williams,

conditions would venture to say that it is nearly complete, for by the very nature of its vegetation, such an area, with its many local or infrequent species, it is almost impossible to exhaust."[556] Standley continued to be surprised by new species among specimens sent to him for identification in subsequent years, and followed up with supplements to the BCI flora and a new flora of the Canal Zone.[557] BCI was widely accepted as number one among the "better collected localities" in Panama.[558]

Taxonomic progress was much faster for the more limited number of vertebrate animals than for plants or invertebrates. By 1935, Swarthmore mammalogist Robert Enders had catalogued fifty-three, about half, of BCI's mammals. He wrote that he had never "encountered as great a diversity of habitat, a greater concentration of many species of mammals, as occurs on" BCI.[559] "An observer, familiar with forest life in temperate regions, is astounded at both the many species and the large numbers of individuals supported by [BCI's] forest."[560]

Homage to Standley: Papers in Honor of Paul C. Standley (Chicago, IL: 1963); Louis Otho Williams, "Paul Carpenter Standley, 1884 - 1963," *Taxon* 12, no. 7 (1963): 245-47. He would collect more than 130,000 plants during his lifetime, primarily from the New World tropics. See also Stanley Heckadon-Moreno, *Naturalistas de Istmo de Panamá*.

556. Paul C. Standley, "The Flora of Barro Colorado Island, Panama," 3, 5.

557. Ibid.; Paul C. Standley, *Flora of the Panama Canal Zone*, vol. 27, Contributions From the US National Herbarium (1928); Leslie A. Kenoyer, and Paul C. Standley, "Supplement to the Flora of Barro Colorado Island, Panama."; Paul C. Standley, *The Flora of Barro Colorado Island, Panama*; Paul C. Standley, "Contributions Toward a Flora of Panama IV," *Annals of the Missouri Botanical Garden* 27 (1940).

558. Robert W. Schery, "A Few Facts Concerning the Flora of Panama," in *Plants and Plant Science in Latin America*, ed. F. Verdoon (Waltham, MA: Chronica Botanica Press, 1945), xx.

559. Robert K. Enders, "Mammalian Life Histories of Barro Colorado Island, Panama," *Bulletin of the Museum of Comparative Zoology Harvard* 78, no. 4 (1935), 389.

560. Ibid. Even so, Enders far underestimated some groups, such as bats, which were extremely difficult to count until new technologies in nets and motion capture cameras were developed in the late twentieth century. Enders identified sixteen species in 1935, estimating that there were probably twice as many. Today seventy-two species are known. For the example of bats and early difficulties estimating species numbers, see Jr. Leigh, Egbert Giles, ""The Intellectual History of STRI" Smithsonian Tropical Research Institute Webcast," *Smithsonian Tropical Research*

The taxonomic work of Enders and others was not only basic to behavioral and ecological studies, as Allee suggested, but raised related questions about the change in the composition of species on BCI over time. When it first became a biological station, the great numbers of species found on BCI at first led Zetek, Chapman, and Allee to assume that species had been concentrated as the waters of Lake Gatun had risen to form the island—transforming BCI into a kind of "island ark."[561] But by the 1930s, this view declined.[562] The ability to return to the island over the decades and the circumscribed area of study allowed Enders, for example, to observe changes in both the population numbers and overall composition of BCI's mammals. Between 1929 and 1935, Enders believed there was little appreciable change in BCI's fauna. If the island's animals had been concentrated by the flooding in 1914, such a concentration would not have persisted over two decades, he reasoned. If more species could be found on BCI than on the Panamanian mainland, Enders argued that this was more likely to be due to hunting and the destruction of Panama's forests than to any artificial concentration on the island.[563] By 1937, however, he was convinced that "profound changes" were in fact occurring "in the relative

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Institute Webcast, mms://STRI-APP13.si.edu/VideoStri/Seminar10-2-2012.wmv (accessed October 20, 2012).

561. Warder Clyde Allee, and Marjorie Hill Allee, *Jungle Island*, 17; Frank Michler Chapman, "An Island Ark: An Unusual Wildlife Refuge Near Panama," *World's Work* 53, no. November (1926): 61-74.See also "Noah's Ark in Jungle Isle." *Los Angeles Times*, August 15 1924, 7.

562. Frank Michler Chapman, *Life in an Air Castle*, 209-10.This language persisted into the 1950s, even after this view of the faunal composition fell out of favor: Marguerite O. Nix, "On Noa's Ark Island," *The Panama Bulldozer, Panama Engineer Division, USA* 5, no. 12 (1944): 12-13; Calhoun, C. H. "Panama's Island Ark." *The New York Times*, 1948. ; J. P. McEvoy, "Barro Colorado, Tropical Noah's Ark," *The Reader's Digest, May* (1955). For an overview of the debate, see William E. Glanz, "Neotropical Mammal Densities: How Unusual is the Community on Barro Colorado Island, Panama?," in *Four Neotropical Rainforests*, ed. Alwyn H. Gentry (New Haven, CT: Yale University Press, 1990).

563. Robert K. Enders, "Mammalian Life Histories of Barro Colorado Island, Panama," 389.

abundance of various species."[564] Large mammals, such as tapir and white-lipped peccary, had noticeably decreased in numbers. Signs of puma and ocelot had disappeared altogether. Enders blamed the changes on the incursion of poachers on the island, rather than natural population cycles or any response to the original isolation of the island—phenomena which would attract researchers in the second half of the twentieth century.

Enders admitted the incompleteness of his work, however, acknowledging that his conclusions were—as Thomas Park would later object—"not upon a solid quantitative basis." Population numbers, and even the presence or absence of certain elusive creatures, were difficult to assess with certainty even on highly monitored BCI. Instead he offered his provisional assessment based on his extensive field experience, his "familiarity with the area and its inhabitants: familiarity gained over a period of eight years and reinforced by extensive notes and frequent estimates."[565] Like many BCI researchers, Enders valued quantification as an ideal, but recognized difficulties achieving this ideal, continuing to value the authority of long-term experience in a place and careful natural historical observation in the field. Enders' was some of the first work to raise changes in species and population numbers on BCI as a scientific problem—changes which would become important in the second half of the twentieth century to the study of both the biogeography of species diversity on islands and in tropical forests. The availability of BCI for long term assessments made the otherwise elementary work of surveying its fauna into a question about shifts in the island's species composition over time.

564. Robert K. Enders, "Changes Observed in the Mammal Fauna of Barro Colorado, 1929-1937," 105.

565. Ibid., 104.

The behavior and ecology of species

More than taxonomic collecting, however, what drew researchers to BCI was the ability to observe living organisms in their natural habitats. In particular, animal behavior, physiology, and life histories—what Orlando Park termed "species ecology"—formed a major focus among BCI visitors. Again, at first glance, this would seem to place BCI firmly within the frame of descriptive natural history. Chapman's popular and heavily anthropomorphic writings about the activity of BCI's birds and mammals, discussed in the previous chapter, certainly give this impression. Yet, visitors also found at BCI an ideal place to perform disciplined, long-term observations that could bear equal weight with the more dominant laboratory studies of animal behavior. Many BCI researchers saw field and laboratory work as complementary. Overall, the pendulum had swung toward the lab in scientific culture, but fieldwork necessarily served as a corrective to biases based on the study of animals separated from their natural communities and environments. The primatologist Clarence Ray Carpenter and the entomologist and comparative animal psychologist Theodore C. Schneirla are perhaps the best examples of researchers who took advantage of BCI's unique circumstances to set precedents for rigorous methods in field ethology.

Carpenter's influence on field methods in primatology is difficult to overstate, and has been explored recently by historians of science.[566] As Georgina Montgomery has argued,

566. Georgina M. Montgomery, "Place, Practice and Primatology: Clarence Ray Carpenter, Primate Communication and the Development of Field Methodology, 1931-1945," *Journal of the History of Biology* 38, no. 3 (2005): 495-533; Georgina M. Montgomery, "Primates in the Real World: Place, Practice and the History of Primate Field Studies, 1924--1970" (Dissertation, University of Minnesota, 2005); Amanda Rees, "A Place That Answers Questions: Primatological Field Sites and the Making of Authentic Observations," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 37, no. 2 (2006): 311-33; Amanda Rees, *The Infanticide Controversy: Primatology and the Art of Field Science* (Chicago, IL:

primatologists realized as early as the 1920s that monkeys and apes failed to reproduce the full sweep of natural behaviors in the laboratory. Yet few reliable field studies of primates existed. Primates were difficult to track in the field, and because primate species are concentrated in the tropics, far from US and European primatologists' homes, field studies required expensive and difficult travel, largely through undeveloped areas. The pioneering ethologist Robert Yerkes sought to rectify this situation by sending his students throughout the world to complete more rigorous, long-term studies, but those he sent to study chimpanzees and gorillas encountered difficulties locating and making continuous observations of wild populations.[567] Spurred by a letter from Frank Chapman explaining how easily howler monkeys could be observed on the BCI, Yerkes sent another student, Carpenter, to the island.[568] Between 1931 and 1933, Carpenter spent eight months in close observation of howler monkeys and other primates on BCI.[569] Carpenter was able to succeed where Yerkes' other students failed largely due to the circumstances he found on BCI, where a wild population of howler monkeys was readily accessible. Not only could Carpenter reside on BCI in relative comfort, returning year after year assured that this population would remain on the island, but trails also allowed him to approach monkeys without causing disturbance and laboratory facilities made possible collaboration, for example, with the Harvard anatomist George B. Wislocki to determine periods of estrus in the

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The University of Chicago Press, 2009), 25-46.

567. Georgina M. Montgomery, "Primates in the Real World: Place, Practice and the History of Primate Field Studies, 1924--1970," 49-50.

568. Ibid., 83.

569. Clarence Ray Carpenter, "A Field Study of the Behavior and Social Relations of Howling Monkeys," *Comparitive Psychology Monographs* 10, no. 2 (1934); Clarence Ray Carpenter, "Behavior of Red Spider Monkeys in Panama," *Journal of Mammalogy* 16, no. 3 (1935): 171-80; Clarence Ray Carpenter, "Behavior and Social Relations of Free-Ranging Primates," *The Scientific Monthly* 48, no. 4 (1939): 319-25.

animals.[570] During these field seasons, as Montgomery has argued, Carpenter developed practices to standardize and quantify his observations, making field data reproducible. His innovative practices included the experimental playback of primate vocalizations, more accurate methods of making censuses of populations, and dyadic analysis, a method which allowed an observer to record complex group interactions.[571] In spite of "fashions" favoring "ever increasing laboratory control" and the "serious neglect in applying the scientific method to field studies of animals," Carpenter was adamant that "the standards of scientific research which apply to laboratory investigations *can* be applied to field studies. Absolute objectivity, accuracy of recording and report, and adequate samplings of observations can be made to characterize alike field investigations and those of the laboratory."[572] Over his career, Carpenter worked throughout the world, further refining his methods in Thailand and Puerto Rico. His early experience at BCI, however, strongly shaped his opinions about the characteristics of an ideal field site and gave him the opportunity to push the boundaries of what methods of observation could actually be possible in the field. The methods pioneered by Carpenter on BCI would set the standard for primatology in the second half of the twentieth century.[573]

570. Carpenter to Barbour, 9 February 1933 and Carpenter to Barbour, 15 December 1935. Box 15, Folder 3, SIA; Clarence Ray Carpenter, "Behavior of Red Spider Monkeys in Panama," 175.

571. Georgina M. Montgomery, "Primates in the Real World: Place, Practice and the History of Primate Field Studies, 1924--1970," 70, 83.

572. Clarence Ray Carpenter, *Naturalistic Behavior of Nonhuman Primates* (University Park: Pennsylvania State University Press, 1964), 342-43.

573. Montgomery discusses the broad influence of Carpenter's methods; Georgina M. Montgomery, "Primates in the Real World: Place, Practice and the History of Primate Field Studies, 1924--1970," 66-74.

Theodore Schneirla has received less direct attention from historians of science.[574] Nevertheless, his experiences on BCI, influence, and philosophy of field practice in ethology are similar to Carpenter's, despite the very different nature of his study animals. Schneirla, who taught psychology at New York University and later served as curator of the Department of Animal Behavior at the American Museum of Natural History, worked on the behavioral ecology of BCI's army ants from his first visit in 1932 through the 1950s.[575] As in primatology, the field study of tropical insect populations lagged far behind laboratory study. Indeed, as the biologist Charles Bennet has argued, insect studies in the tropics were almost exclusively confined to economic and medically relevant species and problems until the 1930s.[576] Schneirla himself lamented the restriction of laboratory behavior studies essentially to "rat learning" and saw the diversity of the tropics as a rich source of species for comparative work.[577] As mentioned in Chapter 3, William Morton Wheeler was the first to study *Eciton* species on BCI, but Schneirla's

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574. But see Richard W. Burkhardt, *Patterns of Behavior: Konrad Lorenz, Niko Tinbergen, and the Founding of Ethology* (Chicago, IL: University of Chicago Press, 2005), 362, 363-364; Tania Munz, "The Bee Battles: Karl Von Frisch, Adrian Wenner and the Honey Bee Dance Language Controversy," *Journal of the History of Biology* 38, no. 3 (2005): 535-70; Gregg Mitman, "From the Population to Society: The Cooperative Metaphors of W. C. Allee and a. E. Emerson."; Gregg Mitman, "When Nature is the Zoo: Vision and Power in the Art and Science of Natural History," *Osiris* 11: Science in the Field (1996), 129-31.

575. T. C. Schneirla, "Studies on Army Ants in Panama," *Journal of Comparative Psychology* 15, no. 2 (1933): 267-299; T. C. Schneirla, "Raiding and Other Outstanding Phenomena in the Behavior of Army Ants," *Proceedings of the National Academy of Sciences* 20, no. 5 (1934): 316-21; T. C. Schneirla, "A Theory of Army-Ant Behavior Based Upon the Analysis of Activities in a Representative Species," *Journal Comparative Psychology* 25, no. 1 (1938); T. C. Schneirla, "Further Studies on the Arny-Ant Behavior Pattern," *Journal of Comparitive Psychology* 29, no. 3 (1940); T. C. Schneirla, "Social Organization in Insects, as Related to Individual Function," *Psychological Review* 48, no. 6 (1941); T. C. Schneirla, and G. Peil, "The Army Ant," *Scientific American* 178, no. 6 (1948): 16-25; T. C. Schneirla, et al., "The Bivouac or Temporary Nest as an Adaptive Factor in Certain Terrestrial Species of Army Ants," *Ecological Monographs* 24, no. 3 (1954): 269-96. See also L.R. Aronson, E. Tobach, J.S. Rosenblatt, and D.S. Lehrman, eds. *Selected Writings of T. C. Schneirla* (San Francisco, CA: WH Freeman & Co., 1972).

576. Charles F. Bennett, "A Review of Ecological Research in Middle America," 16.

577. Richard W. Burkhardt, *Patterns of Behavior*, 362.

work on these and other army ants is considered foundational not only for focusing on a broader range of rarely studied organisms, but also for his research practices and the theoretical perspectives that emerged out of them. Schneirla saw field and laboratory conditions as "overlapping and mutually complementary areas of investigation."[578] Schneirla regularly experimentally manipulated ant colonies in the field, removing the larval brood, for example, to determine the effect on raiding behavior in the colony. (Such interventions posed fewer problems when working with insects, rather than the protected vertebrate species on BCI.) He synthesized this research with laboratory studies of the response of individual workers to squirming larvae and inactive pupae.[579] "In terms of the logic of science," Schneirla argued, "there is really no experimental method as distinct from observation...observation and experimentation are as closely related in field study as they are in other areas of scientific investigation."[580] For Schneirla, observation meant not merely watching, but a disciplined practice of perception and record keeping. Schneirla trained himself to use shorthand and was able to write "'by tactual control' while maintaining 'visual touch' with events."[581] Schneirla's fieldwork on BCI was integral to the development of his theories about how ontogenetic changes during the development of the brood affected foraging behavior in army ant colonies. By observing ant colonies in place over time, Schneirla became convinced that neither genes nor environmental stimulus acted as a single overriding factor in determining behavior—a conviction that, by

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578. L.R. Aronson, E. Tobach, J.S. Rosenblatt, and D.S. Lehrman, *Selected Writings of T. C. Schneirla*, 5.

579. Lester R. Aronson, Ethel Tobach, Daniel S. Lehrman, and Jay S. Rosenblatt, eds. *Development and Evolution of Behavior: Essays in Memory of T. C. Schneirla* (WH Freeman & Co., 1970), 11.

580. L.R. Aronson, E. Tobach, J.S. Rosenblatt, and D.S. Lehrman, *Selected Writings of T. C. Schneirla*, 4.

581. Lester R. Aronson, Ethel Tobach, Daniel S. Lehrman, and Jay S. Rosenblatt, *Development and Evolution of Behavior: Essays in Memory of T. C. Schneirla*, 11.

denying the reality of instinct, put him out of step with mainstream ethology.[582] By the 1970s his students and supporters would champion, under the term "Schneirla's theory," his emphasis on multifactorial explanations of the ontogenetic development of group behavior.[583]

Schneirla and Carpenter were certainly among a minority of ethologists actively involved in long-term field studies of animal behavior before World War II, but, along with other BCI researchers including Enders, Emerson, Caryl Parker Haskins, and Nicholas Collias, they helped to organize the 1947 Committee for the Study of Animal Societies Under Natural Conditions, and ushered in a new wave of field ethology, not only in the tropics, but more broadly as well.[584] Experiences at BCI deeply informed the Committee's ideas about the "field study of animal societies" and, more specifically, how to organize the Jackson Hole Wildlife Park as a natural laboratory.[585]

By the 1960s and 1970s both Schneirla and Carpenter would be cited as models of field methods in the study of animal behavior. Whereas pre-war ethology had focused on individual organisms isolated under laboratory conditions, on BCI the complex interactions of groups and their environment came to the fore. BCI offered access and control, fostering a view of fieldwork that was not placed in opposition to experiment, and which was seen as complementary to

582. Gregg Mitman, and Richard W. Burkhardt, Jr., "Struggling for Identity: The Study of Animal Behavior in America, 1930-1945," in *The Expansion of American Biology*, ed. Keith Benson, Jane Maienschein, and Ronald Rainger (New Brunswick, NJ: Rutgers University Press, 1991).

583. At the time, Schneirla's research was in many ways out of sink with the mainstream in ethology for its dismissal of instinct, but would be looked back at as prescient by a new generation of ethologists in the 1970s and 980s. See Ethel Tobach, "Theodore C. Schneirla: 1902-1968," *The American Journal of Psychology* 94, no. 2 (1981): 355-57; Lester R. Aronson, Ethel Tobach, Daniel S. Lehrman, and Jay S. Rosenblatt, *Development and Evolution of Behavior: Essays in Memory of T. C. Schneirla*.

584. Gregg Mitman, "When Nature is the Zoo: Vision and Power in the Art and Science of Natural History."

585. Schneirla to Zetek, March 26 1947. Box 20, Folder1, RU 135, SIA; Ibid., 129.

laboratory-based research. But at BCI, researchers like Carpenter and Schneirla could not restrict their attention solely to the behavior of individual howler monkeys or army ants. The behavior of these organisms and the highly complex tropical forest environment necessitated not only a consideration of the group, but also their interactions with the physical environment, food sources, and competing and mutualistic species. Moreover, the possibility of long-term field research brought developmental, seasonal, and population cycles into view to a degree not achieved elsewhere in the tropics for several more decades.[586] In all, BCI encouraged a much broader synthetic and ecological view of animal behavior.

Crisis and Transition: The Meeting of the "Alumni"

BCI could not be said to have a rigidly defined central research program, but American biologists quickly recognized the station as having carved out an important niche in the study of ecology in the tropics. This was the consensus of a group of thirty BCI scientists—nicknamed the "alumni"—who met in a conference room at the Smithsonian Institutionon October 31, 1953.[587] Among those present was Schneirla, who passed around a list of scientific papers that had been based on research on the island. It was a wide-ranging body of work, but one that took advantage of the unique research situation available at BCI to study the behavior and composition of a living tropical forest community. Since its founding, the station had averaged thirty-five researchers a year, visiting usually for one to six month periods.[588] In all, nearly eight

586. On the paucity of long term tropical research sites in this period, see Charles F. Bennett, "A Review of Ecological Research in Middle America," 14.

587. Secretary of the Smithsonian Leonard Carmichael, for example, referred to the group as "our BCI 'Alumni.'" Carmichael to Leland Shanor, 29 January 1954. Box 7, Folder 2, RU 135, SIA.

588. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

hundred papers based on fieldwork at BCI had been published in the station's first thirty years, more than twenty-five papers per year since its inception. (See Appendices 5 and 6.)

With the Cinchona Botanical Station in Jamaica and Beebe's zoological station in British Guiana closed, BCI was the only operating research station located in a tropical forest that remained under the control of American biologists. Harvard's Soledad station enjoyed an expanding budget and, as we will see, was now receiving formal classes in tropical botany. Nevertheless, it lacked the access to a tropical rain forest—that iconic tropical biome—offered by BCI. Personally, Schneirla had "found the Island not only invaluable but indispensable as a research zone where intensive forest investigations can be carried out in close correlation with laboratory studies." Eugene Eisenmann, a Panama-born ornithologist who was also on staff at the American Museum of Natural History, echoed Schneirla's sentiments about the island. BCI, he declared, was "the only place under the American flag" with "an essentially virgin tropical rain forest."[589] "Hundreds of scientific papers have been published based on studies made on the island, yet, as is well known, the surface has barely been scraped in any field of biological research in the tropics," Eisenmann explained. Having recently produced a much-needed checklist of BCI's birds, Eisenmann understood BCI's diversity of species from experience.[590]

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589. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA. Eisenmann was in the minority at this meeting not only for being born in Panama (he had a Panamanian mother and American father, and moved to the US after spending the first ten years of his life in Panama), but also for being an "amateur." Eisenmann did not have a doctorate in science, but came to ornithology after a career in law. John Bull, and Dean Amadon, "In Memoriam: Eugene Eisenmann," *The Auk* 100, no. 1 (1983): 188-91; Stanley Heckadon-Moreno, *Naturalistas de Istmo de Panamá*, 253-60.

590. Eugene Eisemann, "Annotated List of Birds of Barro Colorado Island. Panama Canal Zone," *Smithsonian Miscellaneous Collection* 117, no. 5 (1952). This "prosaic-sounding title" obscures its role as the "principal ornithological source for research by zoogeographers, population ecologists, environmental physiologists, and other ornithologists" who have used BCI in their research. Thomas R. Howell, "Eugene Eisenmann and the Study of Neotropical Birds," *Ornithological Monographs* 36 (1985), 1.

He argued forcefully that BCI represented a unique opportunity for both the study and conservation of the natural "richness of its flora and fauna," which was so representative of tropical forests.

> The varied life of this forest type is suggested by the fact that from this small island there have been recorded almost 60 species of mammals, more than 300 species of birds, over 1,200 known species of plants with many more to be described. To preserve the Island as a sample of a fast disappearing world is important in itself.... Yet it is also unique as a biologic field laboratory...It has served, and should continue to serve, as a place for acquainting biologists and students with nature in its most opulent aspects.[591]

"Clearly, this place is an invaluable national asset," Schneirla told the assembled group.[592]

But these scientists gathered not simply to praise BCI's achievements. Rather, they sought to save it for future generations of biologists.[593] After the station's first two decades of growth, BCI had entered a prolonged crisis. As we will see, the station struggled to bounce back following the disruption to travel in the Canal Zone caused by World War II. Perhaps even more devastating to the institution than the war was the death in 1946 of Thomas Barbour, who had not only helped to found BCI, but whose network of personal connections and substantial financial investments in the station had kept it afloat. On the island itself, the carefully balanced system for scientific research that had been built up over the previous thirty years showed signs of stagnation and decay.

Amid the more senior researchers who were present at the 1953 meeting was Edward S. Hodgson, an assistant professor from Columbia University, representing a younger generation of

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591. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

592. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

593. who had spent time on BCI, or were part of the Smithsonian administration that now found itself in charge of the island,

BCI visitors. By the time that he arrived on the island the previous summer, he explained to the group, he had found trails overgrown with vegetation and buildings showing damage from termites and the humid climate. Hodgson must have felt anxious about tales of increasingly bold incursions by poachers.[594] He was also surely aware of growing tensions between the permanent staff and visiting scientists, including grumblings that laborers treated scientists as "nuisances" who interfered with the "little Panamanian community on the Island," and reciprocal complaints of scientists' imperious behavior toward staff members.[595] All was not harmonious on BCI. To top it off, one day after a tourist group had passed through, Hodgson discovered that one of them had stuffed cigarette butts and candy wrappers into the entrances of some of the ant nests that he had been studying. He was certainly disturbed by the fact that this had ruined part of his own ongoing research, but he also contemplated with distress the larger problem of the future prospects of this place, which had been "invaluable...in the progress of my ecological research."[596] Unless reforms were made quickly, he worried, could BCI remain a place for science?

BCI would surely have gone the way of the Cinchona or British Guiana stations had Barbour and Zetek not shown such intense personal dedication to the place from the 1920s through the 1940s. The close reliance of the institution on a few individuals, however, simultaneously had put BCI in a precarious position. Although only in his late fifties, Barbour

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594. For example, see "Folder 6: General Correspondence Fauna and Flora including poaching" in Box 22, RU 135, SIA and Panama Star Herald articles about poaching in Box 23, Folder 4, RU 135, SIA.

595. Walter Clark to J. E. Graf Assistant Secretary of the Smithsonian, 30 June, 1954. Box 7, Folder 3, RU 135, SIA; Graf to Cleaveland C. Soper, 22 December 1953. Box 9, Folder 6, RU 135, SIA.

596. Edward S. Hodgson to Secretary of the Smithsonian Institution Leonard Carmichael, 28 October 1953. Box 7, Folder 1, RU 135, SIA.

was increasingly in poor health in the 1940s. Increasingly conscious of his mortality, Barbour worked tirelessly in the last years of his life to finally put BCI on a firm financial and institutional basis. Barbour could feel reasonably assured of Soledad's ongoing security as tropical station because of the Atkins endowment for tropical agriculture, but finding permanent funding for basic tropical research was more difficult. He thought of BCI as his "troublesome offspring," and knew he would not always be around to ensure that BCI's financial ends were met.[597] Beginning in 1940, he put together the best solution he could find to sustain the institution after his death. Barbour succeeded in having BCI reorganized into a newly formed "Canal Zone Biological Area" (CZBA) under the administration of the federal government, drawing on help from his Washington connections—including his brother, Senator William Warren Barbour and Alexander Wetmore, the Assistant Secretary of the Smithsonian, who had previous experience at the station and worked extensively throughout Latin America. In February of 1944, Barbour suffered a blood clot on the lung, complicated by pneumonia, which restricted his travels. By 1946, Barbour suffered a cerebral hemorrhage and died on January 8.[598] With the crucial help Wetmore, now the Smithsonian's Secretary, the CZBA was transferred to the more permanent care of the Smithsonian Institution on July 16, 1946.[599]

With the disruption caused by World War II, however, government custody failed in the short term to bring the steady funding that BCI desperately needed. Ultimately, the transfer to the

597. He wrote "I sometimes feel as if it were an offspring of mt [sic] own and at times a rather troublesome one," Barbour to William Crocker (National Research Council), 5 May 1928. Box 16, Folder 1, RU 135, SIA.

598. Arthur Loveridge, "Thomas Barbour, Herpetologist: 1884-1946," *Herpetologica* 3, no. 2 (1946), 39.

599. "Canal Zone Biological Are Board of Directors Annual Meeting," 21 June 1946 and Wetmore to Zetek, 16 July 1946. Box 7, Folder 2, RU 135, SIA.

Smithsonian and Wetmore's care would lead to the institutional stability that it had never had under the symbolic aegis of the IRTA, but despite the Smithsonian takeover, the structure of the organization on the ground changed little in the years immediately following Barbour's death. During World War II, the flow of researchers to BCI had slowed to a trickle. Most young scientists found themselves swept up in the war effort and travel was restricted in the highly strategic Canal Zone. A shrewd opportunist, Zetek managed to keep the island in use by recasting BCI as a site for field-testing equipment under tropical conditions. He was able to interest the US military and Eastman Kodak Corporation in studying the deterioration and corrosion of equipment and film, and the associated financial contributions helped sustain BCI through the lean years of the war and immediately after.

At the wars end, the number of scientific visitors rapidly returned and soon surpassed pre-war levels (see Appendix 5), but the financial effects of the war persisted and Zetek's personal ability to maintain the island greatly weakened over time. Most universities failed to renew the yearly subscriptions that they had suspended during the war years. After all, as historian Joel Hagen has explained, faculty could visit BCI whether or not the university invested in a subscription, and university departments found it difficult to justify the expenditure.[600] Perhaps as a result, Zetek began to accept more paying tourists than he likely would have had the station enjoyed a steady source of cash. By the time of the 1953 "alumni" meeting, Zetek was 67 years old and frequently ill. His visits to the island became more irregular. Through Zetek's increasing infirmity, his Panamanian secretary, Adela Gomez, worked heroically to maintain the institution's status quo in the face of a severe lack of funds and administrative support. Both the

600. Hagen

station's leadership at the Smithsonian and visiting researchers knew this situation could not go on forever.

The 1953 BCI meeting, then, was a response to the lingering uncertainty of BCI's future and position within the Smithsonian Institution. Leonard Carmichael, the incoming Secretary of the Smithsonian, unlike his predecessor, Wetmore, had never been to BCI. He called the meeting in order to hear first-hand accounts of BCI from some of its most well-known regular visitors, allowing him to gain an understanding of the station's significance to biological research, as well as its day-to-day operations. In spite of this decade of decline, the group that gathered proved that BCI's reputation among scientists remained strong. Over the years, BCI had accumulated a substantial body of supporters. The "alumni" were eager to share not only their remembrances of the island, but also their enthusiasm for the place and ideas for how it could be rejuvenated to better serve science into the future.

Allee, although unable to attend, wrote to the group that he was "much concerned that the Island be conserved as nearly as possible in its natural condition as a location at which careful biological studies can be carried on."[601] Emerson was present at the meeting and emphasized the importance of investing in BCI. He noted a current trend toward the "multiplication of small marine stations, but," pointed out that, apart from BCI, "we do not have a good terrestrial biological station" in the tropics. Emerson recalled his past experience at the now-defunct British Guiana station, as well as his favorable impression of a recent visit to new biological stations in Africa, which were receiving "generous support" from the Belgian Congo. Echoing American biologists like MacDougal, Fairchild, and Barbour who, decades earlier had sought to emulate

601. Allee to Leonard Carmichael, 28 October 1953. Box 23, Folder 3, RU 135, SIA.

European colonial biological institutions, "We obviously need a strong station in the American tropics," Emerson affirmed.[602]

A strong station was one not only with a solid financial basis and access to natural conditions, but one with a strong intellectual program. Such a program need not be narrow, however. Emerson approached the question of BCI's future development "from the viewpoint of the broad biological picture." He saw greater potential to "expand the program to provide for all the needs in fundamental ecology." Emerson agreed that the station had proved its attractiveness to taxonomists and ecologists, but also hoped "the program could be carried beyond this into the fields of physiology, embryology, and behavior studies," and particularly into comparative studies in these fields "of animal life now found in abundance." Schneirla agreed, "We must direct our thinking towards a broad picture, a research program in which all important types of problems are pursued consistently." Carmichael seemed convinced by the idea of a broad program of basic biology. After asking "about applied research on the island," the Secretary noted that there was "a certain danger is involved [in applied research]. Too much research today is project research. What about problems which do not have direct immediate application."[603] Although Zetek had courted Army and Corporate funds during the lean times of the war, the Alumni and the Smithsonian administration agreed that BCI should be rededicated to basic research.

Fundamental to BCI's role in basic research was its status as a site for long term research. Schneirla highlighted in particular the importance of "the fact that it is an island, a clearly

602. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

603. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

bounded natural research laboratory where systematic, programatic investigation of tropical life can be made and repeated at intervals in order to learn what changes occur over considerable periods of time."[604] The insular nature of BCI and the growing knowledge of its resident species and their behavior could allow biologists to track variations in animal populations and plant associations over time, opening an opportunity to monitor the species composition of a tropical forest in a way never before attempted. "We have really very little information about the continuous life in any tropical forest," Schneirla explained, "and Barro Colorado Island is a place at which this lack can be remedied for the American tropics." Marston Bates, a tropical entomologist who also happened to be David Fairchild's son-in-law and a friend of Barbour since childhood, could not be present.[605] In his correspondence with the group, however, he echoed Schneirla's emphasis on the study of the forest's "continuous life." "I have long thought that Barro Colorado needed a resident staff working on long-term projects," Bates told Secretary Carmichael. Bates had written in 1945 of the need for a tropical station with a "permanent staff" and a "long term program of research" on the "basic problems of evolution." BCI was an exception among "the laboratories in the American tropics, [which] as far as I know them, are largely engaged either on medical or agricultural work of direct practical importance." Yet even there basic problems received "short term...periodical or occasional visits" in comparison with

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604. Double underline is in original. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

605. Marston Bates remains a sorely under-appreciated figure in the history of biology and environmental thought, in spite of his fascinating early career in international fieldwork and later fame for his popular writings exploring how ecology could be applied to social and environmental problems. One environmental historian writes, "Bates does not seem to be on my colleagues' radar." Elliott West, "The Gosh! Factor," *Environmental History* 10, no. 4 (2005): 763-64.

what he believed was needed for a "long range study of the 'species problem.'"[606] Bates' work with the United Fruit Company and the Rockefeller Foundation had taken him to Albania, Egypt, Guatemala, Honduras, and Colombia, and he and not been back to BCI since 1935. However, he had kept up with the station through his close association with Barbour and his communication with Carmichael recalled Barbour's own views. "The maintenance of a laboratory for the study of tropical rain forest conditions is very important for the development of biology," he told Carmichael, "since the rain forest in many respects represents 'optimal' conditions for terrestrial life." While Bates acknowledged Charles Elton's idea that the simplified ecologies of the arctic made that region more amenable to ecological research, he had argued previously that "the abundance of organisms" and the "complexity of the environment" in the tropics "offer some overwhelming advantages" and raised evolutionary questions. He noted in particular the prevalence of polymorphism and the existence of geographical variation within "apparently uniform climatic environments."[607] Bates explained to Carmichael that "Almost all of our knowledge of this environment has been collected on an 'expedition' basis, and we need more sustained observation, and a program of continuing studies."[608]

Conversation also covered the possibility of strengthening BCI's role in graduate education. Enders was one of the strongest early advocates of bringing students to BCI for tropical experience. The Swarthmore students Enders had brought to BCI over the years were

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606. Marston Bates, "The Advantage of the Tropical Environment for Studies on the Species Problem," in *Plants and Plant Science in Latin America*, ed. Frans Verdoorn (Waltham, MA: Chronica Botanica Company, 1945), 235-36.

607. Ibid.

608. Bates to Carmichael. 2 Novemeber 1953. Box 23, Folder 3, RU 135, SIA.

informally known as the “Panamaniacs.”[609] Many had gone on to further tropical work in academia, as well as in government work; one was an author of the Army Manual of Tropical Medicine and another an Air Force physiologist who had worked in Burma’s tropical forests. Enders had even brought twelve undergraduates to BCI. All but one of whom became biology PhDs, “This was largely due to their experience in the tropics.” He explained, “Nowhere else in the world is there a place where under such safe conditions such a fine flora and fauna are available to the beginner in tropical research. If for no other purpose, support of the Island is justified on the basis of a training ground for tropical forest work.”[610] Wetmore explained that had attempted unsuccessfully to secure grants for student research during his tenure as Secretary of the Smithsonian, and the those present agreed with Schneirla that systematic use of grants could help to “support... a balanced investigative program for the Island.”[611]

Finally, the group also found wide agreement on the advantages of a director who could reside on the island the majority of the time, perhaps even a larger scientific staff, as Bates suggested. Eisenman argued for “a resident naturalist” who could “orient visiting scientists in the natural history of the Island and be of help to students and visitors.”[612] Allee was in agreement with those who could attend the meeting about the importance of “a resident naturalist actually living on the island.”[613] In spite of their tight control over life on BCI, neither Barbour nor Zetek

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609. Neal Griffith Smith, Oral history interview with Robert K. Enders, 13 April 1976, RU 009562, SIA.

610. “Minutes of the Barro Colorado Island Meetings,” 31 October 1953. Box 7, Folder 1, RU 135, SIA.

611. “Minutes of the Barro Colorado Island Meetings,” 31 October 1953. Box 7, Folder 1, RU 135, SIA.

612. Added to the manuscript in hand writing. “Minutes of the Barro Colorado Island Meetings,” 31 October 1953. Box 7, Folder 1, RU 135, SIA.

613. Allee to Carmichael. 28 October 1953. Box 23, Folder 3, RU 135, SIA.

had exerted direct influence on research directions at BCI. Barbour had shaped research directions on BCI only in very limited ways, by discouraging those he considered, for example, or encouraging young researchers who he felt showed promise.[614] Zetek's USDA entomological work had kept him too busy to provide visiting scientists with much guidance in their research or orientation to the island's species. Although he had a great breadth of interest in entomology and ecology, he also had difficulty keeping up with current trends in ecology. By the 1950s, the field seems to have left him behind. He continued to speak with pride about how well-suited BCI was to ecological research, but confessed that he found "some so-called ecology" to be "very hard to follow and comprehend. Some of the more modern requires mental twists."[615] It might be impossible to find a new man with the "proper connections" and "relationships in Panama" that Zetek had so carefully nurtured.[616] Nevertheless, a scientist with both a grasp of the natural history of the island and a finger on the pulse of current developments in biology more broadly could help to guide the intellectual directions of research at the station.

A New "Research Program"

In the subsequent years, the Smithsonian administration gradually carried out the broad outlines the "alumni's" recommendations. Most importantly, after his retirement in 1956, Zetek's custodianship was replaced with a "Resident Naturalist" position (later renamed the "Resident Director"). This position was first filled by Carl Koford, who resigned after a year largely due to

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614. For example, Frederick Rowe Davis, *The Man Who Saved Sea Turtles: Archie Carr and the Origins of Conservation Biology* (Oxford: Oxford University Press, 2007), 33, 44, 56-58, 67, 146, 241,253.

615. Zetek to Graf, 21 March 1955. Box 16, Folder 4, RU 135, SIA.

616. "Minutes of the Barro Colorado Island Meetings," 31 October 1953. Box 7, Folder 1, RU 135, SIA.

conflict with Smithsonian Assistant Secretary Remington Kellogg. Martin Moynihan soon took his place, and was largely responsible for shaping BCI into the institution it is today—an international research center with its own staff of scientists and an emphasis on graduate training. At the same time, the ecological and evolutionary causes of species diversity in the tropics emerged as a new intellectual focus.

Moynihan was an Oxford-trained ornithologist who had studied under the influential ethologist Niko Tinbergen and served as a postdoctoral fellow at the Harvard Museum of Comparative Zoology with the celebrated evolutionary biologist Ernst Mayr. As historian Catherine Christen has explained, he had a strong vision for the expansion of BCI's mission and was able to deftly navigate the bureaucracy of the Smithsonian from his appointment in 1957 through the end of his tenure in 1974.[617] Under Moynihan, the Canal Zone Biological Area expanded to include not just BCI, but other small islands nearby and segments of the Canal Zone mainland that he was able to procure from the US military. This enlargement provided "areas of different ecology" for comparison with BCI's forest, and permitted leeway for certain kinds of experimentation discouraged on BCI, such as the removal or introduction of species.[618] By 1966 he would establish "facilities on both sides of the isthmus...for marine research," transforming the Smithsonian's tropical bureau from a single station on BCI into the multi-station Smithsonian Tropical Research Institute (STRI), as it is known today.[619] Moynihan also took advantage of

617. Catherine A. Christen, "At Home in the Field: Smithsonian Tropical Science Field Stations in the U. S. Panama Canal Zone and the Republic of Panama," 544-65.

618. Martin Humphrey Moynihan, "Report on the Canal Zone Biological Area," *Annual report of the Smithsonian Institution* (1958), 185.

619. Martin Humphrey Moynihan, "Report of the Canal Zone Biological Area," *Annual report of the Smithsonian Institution* (1965), 310. See also Jan Sapp, *What is Natural?: Coral Reef Crisis* (New York, NY: Oxford University Press, 1999), 78.

newly available NSF funding, both for his own behavior research as well as to expand BCI's facilities, including a $100,000 grant in 1965 for the installation of an electric cable to provide more reliable power supply, which allowed a wave of new instrumentation on the island.[620] Moynihan also reduced the frequency of tourist visits and oversaw exponential growth in visits by researchers in the 1960s. Most importantly, Moynihan created graduate student internships and gradually assembled a scientific staff on the island, growing from the appointment of the first graduate assistant in 1958 to a staff of 9 by the time he left.

With the arrival of Moynihan, BCI had a resident director with intellectual goals for tropical biology as well as administrative skill. Significantly, Moynihan's ability to personally select BCI's new staff scientists gave him substantial ability to shape future research at the station. Yet, in key ways, Moynihan strengthened—albeit in an environment of greater financial resources—the intellectual approach of his predecessors. Echoing Barbour's disdain for "organized" research, Moynihan "distrusted team research, believing that the best ideas come from people pursuing projects they devised themselves"[621] Although Barbour and Moynihan could hardly have been more different as individuals, they shared a mentoring philosophy that involved identifying promising people and giving them the freedom to pursue their own interests. In fact, Moynihan's "research program" was extremely broad. He wanted nothing less than "that the bureau be re-organized, formally, as an institute for research on tropical biology as a whole."[622] "Research in the tropics," Moynihan told Secretary Carmichael,

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620. See for example Toby A. Appel, *Shaping Biology*, 202.

621. Egbert Giles Leigh Jr., "Barro Colorado," 88.

622. Moynihan to Carmichael, 5 July 1963. Box 23, Folder 3, RU 135, SIA.

> is obviously essential for proper understanding of many important biological processes. Some biological problems can be studied only in the tropics, and others can be studied in more detail and/or more easily in the tropics than in other regions. This is primarily due to the fact that the numbers and variety of species are much greater in the tropics than elsewhere. The tropics include the 'optimum' habitats for almost all types of organisms; and there is good evidence that most groups of organisms have originated and/or evolved most rapidly in the tropics. Both the primitive 'relicts' and the most highly specialized and recently developed species tend to be concentrated in the tropics. Thus, the materials for research on many aspects of evolution, ecology (e. g. competition), and behavior (e. g. social relations) are simply more abundant, and therefore more accessible, in the tropics than elsewhere.[623]

He explained the importance of the tropics to specific research problems, like the causes of reproductive cycles and the initiation of migration, which had been mostly studied in the temperate and arctic zones. These physiological processes had been correlated with environmental factors, such as day length, that "cannot apply to tropical species. Research in the tropics is necessary in order to correct and complete the information derived from the study of species of other zones."[624] In part due to the need to suit Smithsonian administrators and NSF grant reviewers, Moynihan did perhaps articulate more clearly than his predecessors what a broad program of "tropical biology" entailed at BCI. Yet, in his reference to the abundance of research material in the tropics and the lack of tropical work needed for biological generalizations, one can hear echoes of both the turn-of-the-century Tropical Laboratory Commission and the discussions of IRTA recounted in previous chapters. Moreover, it was in Moynihan's interest to contrast historical work at BCI with his new plans. He suggested that previous BCI work was "for long mostly confined to systematic and faunal behavioral studies,"

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623. Moynihan to Carmichael, 5 July 1963. Box 23, Folder 3, RU 135, SIA.

624. Moynihan to Carmichael, 5 July 1963. Box 23, Folder 3, RU 135, SIA.

and that he would "expand to include long-term research in behavioral, evolutionary, physiological and ecological studies."[625] As we have seen, these areas had in fact been explored by early BCI researchers, even though the evolutionary implications of work at BCI had usually remained implicit, in discussion of "adaptations."

Rather than overturning past work, postwar research at BCI was built upon the basis of the major areas of research that emerged before the war. Taxonomic work necessarily continued given the large proportion of species still unknown and the difficulties presented to all but expert taxonomists. Even by the mid-1960s, BCI biologist Egbert Leigh recalls that experienced botanists continued to struggle to identify BCI's diversity of plants. Beginners could not approach BCI's plant species with confidence until the 1970s, when Thomas Croat, with the help of the ecologist Robin Foster, revised and completed the work that Standley had begun half a century earlier, incorporating identification keys for non-specialists.[626] Life history and behavior work, including Alexander Skutch's work on tropical birds and John H. Kaufman's NSF-funded studies of BCI's coati population, were not qualitatively different than past studies, although they were even more sustained and had the benefit of long-term ecological data.[627] Nicholas Collias and Charles Southwick, and Stuart Altman, for example, directly followed up Clarence

625. Moynihan to Ripley, 1964. Box 2, Folder 2, RU 135, SIA.

626. Egbert Leigh, personal communication. Thomas B. Croat, *Flora of Barro Colorado Island*.

627. Alexander F. Skutch, "Life History of the Black-Throated Trogon," *Willson Bulletin* 71, no. 1 (1959); Alexander F. Skutch, "Roosting and Nesting of Aracari Toucans," *The Condor* 60, no. 4 (1958); John H. Kaufmann, and A. Kaufmann, "Some Comments on the Relationship Between Field and Laboratory Studies of Behavior, With Special Reference to Coatis," *Animal Behaviour* 11, no. 4 (1963): 464-69; John H. Kaufmann, "Ecology and Social Behavior of the Coati, Nasua Narica, on Barro Colorado Island, Panama," *University of California Publications in Zoology* 60, no. 3 (1962): 95-222.

Carpenter's studies of BCI's howler monkeys "using as nearly as possible the methods described by Carpenter."[628]

Nevertheless, broader shifts the theoretical perspectives of biology did manifest themselves in BCI research during Moynihan's directorship. Historian Joel Hagen has pointed to "shared commitment to the modern evolutionary synthesis," channeled through Moynihan's experience with Mayr, as a source of a new "intellectual bond that had not existed" during BCI's early years.[629] The synthesis, with its emphasis on population biology, was of course influential in a broad sense, but new questions in evolutionary and ecosystem ecology had a much more specific and direct affect on work at BCI. Beginning in the 1950s, both fields converged on the question of species diversity. Tropical environments, with their large numbers of species compared with temperate environments, appeared as a proving ground for ideas about ecological controls on speciation. The question of what tropical ecosystems could tell biologists about speciation had been flagged as early as 1950, by none other than evolutionary synthesist Theodosius Dobzhansky. In "Evolution in the Tropics," Dobzhansky wrote that "the greater diversity of living beings" found in the tropics was "the outstanding difference which strikes the observer."[630] Since Dobzhansky posited that any such differences must be the outcome of a difference in evolutionary patterns, he suggested that "Tropical environments provide more

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628. N. Collias, and C. Southwick, "A Field Study of Population Density and Social Organization in Howling Monkeys," *Proceedings of the American Philosophical Society* 96, no. 2 (1952), 143; Stuart A. Altmann, "Field Observation on a Howling Monkey Society," *Journal of Mammology* 40, no. 3 (1959).

629. Joel B. Hagen, "Problems in the Institutionalization of Tropical Biology: The Case of the Barro Colorado Island Biological Laboratory," 243.

630. Theodosius Dobzhansky, "Evolution in the Tropics," 212.

evolutionary challenges," but challenges that "stem chiefly from the intricate mutual relationships among the inhabitants," rather than the physical challenges of colder latitudes.[631]

These patterns had been recognized since the nineteenth century, but they now seemed to point toward the need for better empirical study of these "intricate mutual relationships." This shift in perspective cast ongoing work at BCI in a new light and spawned greater ecological interest in the tropics more generally. The breadth of this burgeoning interest can perhaps be characterized, on one hand, by the British ecologist Paul W. Richards' 1952 synthetic comparison of the composition and structure of rainforests in Borneo and British Guiana, widely regarded as foundational in tropical forest biology.[632] On the other hand, that same year, Marston Bates published for a broad public audience an account of tropical life that stood both as a celebration of global human diversity and the first popular explanation of how niche partitioning could explain "this tremendous diversity of kinds of plants and animals." "The forest as a whole," he wrote, "with its niches and niches within niches, becomes a sort of multiplication table of possibilities for different kinds of life."[633]

Biologists continued to try to test and quantify this new way of conceptualizing species diversity. Based in Brazil, Dobzhansky and his collaborators followed up with work on *Drosophila* and trees, helping to set off a new efforts to estimate species diversity in tropical

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631. Ibid., 221.

632. P. W. Richards, *The Tropical Rain Forest*. In it, he attacked the problem of mixed rainforests lacking a single dominant tree species, as previously raised by Shreve and Allee in our discussion. See the section discussing selections on "Floristic Composition and Species Richness" in Robin Lee Chazdon, and T. C. Whitmore, *Foundations of Tropical Forest Biology*, 513-22. See also Carl F. Jordan, *Tropical Ecology*, 243-54; John C. Kricher, *Tropical Ecology*, 12-15.

633. Marston Bates, *Where Winter Never Comes*, 207, 208.

forests.[634] Although "species diversity" was coined by the German botanist Christen C. Raunkaier in 1906, its use, and efforts to quantify it, exploded in the 1950s and 1960s.[635] In the late 1950s, Robert MacArthur's contributions to a mathematical theory to describe explain the relative abundance of species was a particular turning point—and although highly abstract, they drew from MacArthur's observations of bird species in Central America.[636] MacArthur's advisor and mentor G. Evelyn Hutchinson synthesized this theoretical approach and brought it to a wider audience of biologists in his own highly-cited "Homage to Santa Rosalia Or Why Are There So Many Kinds of Animals?"[637] In it, he suggested that a community with many species also has a complex trophic structure—more of a food web than a food chain. Such communities may exist because they are more stable than a simple communities, while simpler, less diverse communities may be explained by environmental limits. Hutchinson's paper centered around a contemplation of the diversity of life in a small pond in Sicily, not a tropical forest, but its broad implications were immediately understood.[638] The new mathematical approaches to

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634. Theodosius Dobzhansky, and C. Pavan, "Local and Seasonal Variations in Relative Frequencies of Species of Drosophila in Brazil," *Journal of Animal Ecology* 19, no. 1 (1950): 1-14; G. A. Black, et al., "Some Attempts to Estimate Species Diversity and Population Density of Trees in Amazonian Forests," *Botanical Gazette* 111, no. 4 (1950): 413-25; J. Murça Pires, et al., "An Estimate of the Number of Species of Trees in an Amazonian Forest Community," *Botanical Gazette* 114, no. 4 (1953): 467-77.

635. Robert P. McIntosh, "An Index of Diversity and the Relation of Certain Concepts to Diversity."

636. Robert MacArthur, "On the Relative Abundance of Species," *The American Naturalist* 94, no. 874 (1960): 25-36; Robert H. MacArthur, "On the Relative Abundance of Bird Species," *Proceedings of the National Academy of Sciences of the United States of America* 43, no. 3 (1957): 293-95.See also Sharon E. Kingsland, *Modeling Nature*, 183-86. MacArthur's influence on tropical fieldwork is clear—many sought to refute or refine ideas that were at first seen as oversimplifications. The influence of fieldwork on MacArthur is more difficult to get at. His respect for field biology is clear from interviews of BCI scientists, but his writing often obscured the empirical reference points from which he drew.

637. G. Evelyn Hutchinson, "Homage to Santa Rosalia or Why Are There So Many Kinds of Animals?," *The American Naturalist* 93, no. 870 (1959): 145-59.

638. Sharon E. Kingsland, *Modeling Nature*, 186-87.

understanding species diversity inspired theoreticians and fieldworkers alike to test and refine the models produced by MacArthur, Hutchinson, and others.

Back on BCI, the influence of Hutchinson was direct.[639] Many of Moynihan's staff scientists were Hutchinson's students or devotees of his systems view of ecology, quantitative and evolutionary outlook. Moynihan's own interests focused on the social behavior of tropical birds and primates, but he recognized BCI's strengths in ecological fieldwork and hired researchers whose primary interests were ecological.[640] New staff members, among them Egbert Leigh, Stanley Rand, Neal G. Smith, and future STRI director Ira Rubinoff, were interested in the potential of tropical case studies to explore the questions around trophic dynamics and the limits to diversity that Hutchinson had raised.

MacArthur himself visited BCI in 1961, 1966, 1969 as part of his continuing work modeling controls on species diversity.[641] Although better known for their experimental manipulation of small islands in the Florida Keys, MacArthur's collaboration with Edward O.

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639. On Hutchinson's broader impact, see Nancy G. Slack, *G. Evelyn Hutchinson and the Invention of Modern Ecology* (New Haven, CT: Yale University Press, 2010); Sharon E. Kingsland, *Modeling Nature*.

640. For example, Martin Humphrey Moynihan, *Hostile and Sexual Behavior Patterns of South American and Pacific Laridae*, vol. Suppl. 8, Behaviour (Behaviour Supplement 8, 1962); Martin Humphrey Moynihan, *The Organization and Probable Evolution of Some Mixed Species Flocks of Neotropical Birds*, vol. 143 no. 7, Smithsonian Miscellaneous Collection (Washington DC: Smithsonian Institution, 1962); Martin Humphrey Moynihan, "Inter-Specific Relations Between Some Andean Birds," *Ibis* 105 (1963): 327-29; Martin Humphrey Moynihan, "Display Patterns of Tropical American Nine-Primaried Songbirds. Iv. The Yellow-Rumped Tanager," *Smithsonian Miscellaneous Collection* 149, no. 5 (1966): 1-34; Martin Humphrey Moynihan, "Communication in the Titi Monkey, Callicebus," *Journal of Zoology, London* 150 (1966): 77-127.

641. P. H. Klopfer, and R. H. MacArthur, "Niche Size and Faunal Diversity," *The American Naturalist* 94, no. 877 (1960): 293-300; Peter H. Klopfer, and R. H. MacArthur, "On the Causes of Tropical Species Diversity: Niche Overlap," *The American Naturalist* 95, no. 883 (1961): 223-26; Robert H. MacArthur, and John W. MacArthur, "On Bird Species Diversity," *Ecology* 42, no. 3 (1961): 594-98; Robert H. MacArthur, et al., "On Bird Species Diversity. Ii. Prediction of Bird Census From Habitat Measurements," *The American Naturalist* 96, no. 888 (1962): 167-74; Robert H. MacArthur, "Environmental Factors Affecting Bird Species Diversity."; Robert H. MacArthur, "Patterns of Species Diversity."; Robert H. MacArthur, et al., "On the Relation Between Habitat Selection and Species Diversity."; Robert H. MacArthur, "Patterns of Communities in the Tropics," *Biological Journal of the Linnean Society* 1, no. 1-2 (1969): 19-30.

Wilson on the *Theory of Island Biogeography* in 1967, depended in part on his experience at BCI and elsewhere in Panama.[642] Importantly, the large body of data accumulated on BCI's composition of species helped to contribute to the theory's empirical basis. Seemingly mundane data, such as Eugene Eisenmann's checklist of BCI birds, took on new significance, allowing MacArthur and Wilson to quantify and compare the relative diversity of different islands.[643] Early observers, like Chapman, Enders, or Standley, had been surprised by the numbers of species on BCI and had assumed either that species had been concentrated there, or that it was representative of the diversity of comparable forests elsewhere. As data accumulated, finer scale comparisons were possible, and it became clear that BCI was actually somewhat less diverse than comparable mainland forests, and had lost species during the station's lifetime. The *Theory of Island Biogeography* was an attempt to explain patterns like this, but not only did it posit how an island's size and nearness to a mainland set limits on the number of species present, it also had serious applications to conservation in questioning the effectiveness of isolated nature reserves—be they literal island or not.[644] The question of the distribution of species diversity was no longer a theoretical conundrum, but a problem with economic and policy implications—just as proponents of basic tropical research had long argued. Not just MacArthur and Wilson's work, but the new focus on the factors that limited diversity made even the most obscure study of now was of potential relevance to conservation efforts.

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642. Robert H. MacArthur, and Edward O. Wilson, *The Theory of Island Biogeography* (Princeton, NJ: Princeton University Press, 1967).

643. Ibid., 104.

644. For an engaging account of the theory of island biogeography and the "SLOSS" (Single Large or Several Small) debate about the size and effectiveness of nature reserves, see David Quammen, *The Song of the Dodo*.

"The broad subject which attracted the largest single group of visitors with similar interests," Moynihan wrote in the 1966 annual report of BCI, "was not unexpectedly, species diversity."[645] Moynihan recognized the growing power of these debates increasingly articulated species diversity as a central focus of research at the station.[646] The growing theoretical interest in the tropics was on one hand a boon to BCI, reducing the need for Moynihan to justify the station's existence. But on the other hand, a single site could no longer be rationalized as a representative slice of tropical nature. Moynihan new this coming in, an his efforts to acquire more land in the Canal Zone reflected his knowledge of the need for the station itself to diversify. Moreover, ironically, the first "resident" director of BCI traveled extensively, including to Peru and Bolivia, Colombia and Venezuela, for comparative work. Whereas Zetek had jealously tried to keep researchers on BCI—sometimes confiscating the boat!—Moynihan strongly encouraged his staff scientists to travel the world in order to better understand BCI's forest in relation to tropical forests elsewhere in the world. He sent the young STRI staff scientist Egbert Leigh, for example, around the world—to Ivory Coast, Madagascar, India, Malaya, and New Guinea—to compare the structures of forests in the global tropics.[647] New evolutionary and ecological perspectives made Moynihan's expansive vision of STRI's mission crucial. If theoreticians were going to make generalizations about the tropics, it was important to know how representative data at BCI was. Moreover, with the expansion of air travel and global

645. Martin Humphrey Moynihan, "Smithsonian Tropical Research Institute," *Smithsonian Year (Annual report of the Smithsonian Institution)* (1966), 169.

646. He earlier had referred to essentially the same concept using the phrase "number and variety of species" in the Smithsonian Annual Reports and his correspondence, but as "species diversity" grew in frequency so did references to the question of the causes of species diversity.

647. Martin Humphrey Moynihan, "Smithsonian Tropical Research Institute," *Smithsonian Year (Annual report of the Smithsonian Institution)* (1969), 226.

development, more areas of the tropics were more accessible to biologists than they had been in the early twentieth century. If BCI, and STRI, was to remain relevant both intellectually or institutionally BCI must to be placed within the broader context of the global tropics.

Beyond BCI

Even as Moynihan pushed the Smithsonian agenda in tropical field research beyond BCI and into the global tropics, opportunities for American tropical biology had already begun to expand far beyond BCI. While BCI remained a leader among stations for basic research in the tropics, American universities also began to create new opportunities for tropical study in the post-war period. Harvard's station at Soledad, renamed the Atkins Institution in 1932, was still the longest-standing university-operated station. Through the 1930s and 1940s, Soledad had weathered the Depression, world war, several periods of labor unrest, the Cuban revolution of 1933, and damage by hurricanes in 1935 and 1948. Nevertheless, formal affiliation with Harvard and the Atkins endowment, however strained, granted the Soledad station institutional stability that BCI never enjoyed during Barbour's lifetime. Moreover, it had been relieved of competition with BCI by both it's agricultural setting and Barbour's joint control of both institutions from the 1920s through the 1940s.

Like BCI, however, Soledad also underwent major reorganization after Barbour's death. A gift of $25,000 a year for five years from the Claflin family, descendants of Edwin Atkins' son-in-law, William H. Claflin, allowed Harvard to instate a new resident directorship to replace Barbour's position as custodian of the station. It also made possible an expansion in the research projects undertaken at the garden.[648] In the wake of Barbour's passing, the first priority of the

648. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1946-1947*

new director, Arthur Kevorkian, was to return the station to its core mission as a "center of research in tropical agriculture," a mission from which it had strayed both because of Barbour's encouragement of a broader range of basic biology, but also as a result of a degree stagnation during the war and Barbour's final years of illness. During Kevorkian's directorship and those of his followers, the station strengthened its specialties in tropical forestry and the improvement of Cuban food and forage crops. In these areas, the station increasingly cooperated with Cuban institutions, such as the Experiment Station at Santiago de las Vegas, the Topes de Collantes forestry station of the Ministry of Agriculture, freely exchanging plants and expertise.[649]

In re-emphasizing applied botany, Soledad diverged from BCI after Barbour's death. Yet, in some ways there were more similarities than differences, as the new administrations of both stations instituted resident directors who had much more power to shape the broad trends of research at the station. Moreover, relevant basic research in tropical botany, including research into the composition of Cuban forests and the evolutionary origins of corn, remained important at Soledad. Visitors, including graduate students (some of whom received Atkins funds), also continued to pursue a wide range of research at Soledad. The collection of tropical plants in the Soledad garden also became an increasingly important living reference for both taxonomists and economic botanists, particularly as post-war nationalist movements arose in South and East Asia and left the future of some of the world's greatest botanical gardens uncertain. As one visitor to

(Cambridge, MA: Harvard University, 1949), 268, 274.

649. For example, Ibid., 274-77.; Harvard University, *Report of the President of Harvard College and Reports of Departments, 1947-1948* (Cambridge, MA: Harvard University, 1950), 250-56; Harvard University, *Report of the President of Harvard College and Reports of Departments, 1957-1958* (Cambridge, MA: Harvard University, 1959), 469-70. These also included in retrospect perhaps more dubious projects, such as providing landscaping plants for the US base at Guantanamo Bay and research into the use of kudzu as a forage crop.

Soledad explained, "Since the Dutch are withdrawing from Java, Buitenzorg seems destined for obscurity, and the political situation in Ceylon may at any time eclipse the Peradeniya garden, which leaves the Atkins Institution as the only other garden to my knowledge with so diverse and mature a collection of tropicals."[650] Maintaining Harvard's collection was crucial for tropical botanists both because of the large number and variety of specimens and their maturity compared to those available at more recently founded gardens.

The most important breakthrough at Soledad, however, was Biology 215, the graduate course in tropical botany that Harvard began to operate in 1950. Of course, Barbour had encouraged a handful of graduate students to pursue work at Soledad—in fact Marston Bates had completed his dissertation on "The Butterflies of Cuba" while based at the station.[651] Nevertheless, the shift toward formal education was a significant change, and one that drew the station drew it closer to Harvard itself. Biology 215 was announced as the "first course in tropical botany ever to be offered by an American university" and an "important milestone in broadening the training of candidates for advanced degrees in botany."[652] The course had goals for education in both basic and applied tropical botany. It was intended to provide a broad introduction "to the vegetation of the tropics which is profoundly different from that of temperate regions" as well as to acquaint "students with tropical economic plant products such as rubber, palm oil, manila hemp, quinine, and many others which have become essential to the American

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650. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1948-1949* (Cambridge, MA: Harvard University, 1952), 269.

651. Marston Bates, "The Butterflies of Cuba."

652. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1949-1950* (Cambridge, MA: Harvard University, 1954), 298.

economy during this critical period."[653] Soledad's new administration came to see that "in providing training for graduate students the Atkins Garden can serve one of its most useful functions...By training and placing men in key positions in other parts of the tropics, the Atkins Garden can multiply its own modest efforts manyfold."[654] Students who had worked at Soledad could be found working throughout the tropical word, for example at the Federal Experiment Station in Puerto Rico or on the staff of the agriculture program of the Rockefeller Foundation in Colombia.[655] Perhaps the course's most famous graduate was Edward O. Wilson, whose first encounter with the tropics was in the 1953 class of Biology 215. The first course included eight students and was lead, with assistance from Soledad's new director, I. D. Clement, by Walter H. Hodge, botanist who already had extensive experience in Latin America, having worked for the United States State Department of Economic Warfare at the Cinchona Mission in Peru during the war, followed by a year as visiting professor and head of the Department of Biology at Colombia's *Universidad Nacional* in Medellín. Hodge would later play a key role in the National Science Foundation program for tropical biology and the formation of the Organization for Tropical Studies.[656] In 1950, the class spent a month in Cuba, both at the station and in field trips throughout the island, followed by a two week visit to the United Fruit Company's station at Lancetilla, Honduras. In future years the course would be lead by the future director of the Arnold Arboretum, Richard A. Howard, the University of Michigan taxonomist Grady Webster,

653. Ibid., 289.

654. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1954-1955* (Cambridge, MA: Harvard University, 1956), 363-64.

655. Ibid.

656. Toby A. Appel, *Shaping Biology*, 158, 199-200, 223-225, 258.

and the pioneering ethnobotanist Richard Shultes—a student of Oakes Ames, whose efforts had established the Soledad station in the first place (see Chapter 2).

At BCI graduate student education remained more informal, but at Soledad Biology 215 was a major innovation. Other American universities followed suit and began to create tropical field courses in the 1950s and early 1960s, including the Associated Colleges of the Midwest, the University of Florida, Kansas, Southern California, and Miami[657] Institutions for tropical biology, particularly for terrestrial tropical biology, in use and accessible to US researchers remained rare, but were rapidly on the increase (see Appendix 2). Of the 268 worldwide biological stations available at the end of the war, listed by Homer Jack in his indispensable assessment of the "Biological Field Stations of the World," only eight were non-marine tropical stations.[658] Of these, three were controlled and actively being by American biologists.[659] These three were BCI and Soledad, as well as Rancho Grande, a new cloud forest station operated between 1945 and 1948 by William Beebe and the New York Zoological Society with

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657. Donald Stone, "The Organization for Tropical Studies (OTS): A Success Story in Graduate Training and Research," in *Tropical Rainforests: Diversity and Conservation*, ed. Frank Almeda, and Catherine M. Pringle (San Francisco, CA: California Academy of Sciences, 1988).

658. I have not included the three subtropical Florida stations in this count. Homer Alexander Jack, "Biological Field Stations of the World," *Chronica Botanica* 9, no. 1 (1945): 1-73.

659. The other five terrestrial tropical stations that Jack listed were the *Estação Biológica do Departamento de Botánica do Estado* at Alto da Serra, near Sao Paulo, Brazil; the Visitors' Laboratory of the Royal Botanic Garden at Peradeniya, Ceylon; the Treub Laboratory of the Government Botanical Gardens at Buitenzorg, Java, and its mountain substation at Tijbodas; and the Cinchona station in Jamaica (in spite of the fact that it was closed at that time). Jack does not include places that operated by corporations but which biologists used as bases for fieldwork, although this would not greatly increase the total number. As mentioned earlier, the United Fruit Company Tela station at Lancetilla, Honduras, was the most significant of these. In addition to its use by the Harvard tropical botany class, Barbour himself was involved in snake antivenin research there in the 1930s, and botanists like Paul C. Standley used it as a base for collecting. See for example Paul C. Standley, *Flora of the Lancetilla Valley, Honduras* (Field Museum of Natural History: Chicago, 1931); Paul C. Standley, "Lancetilla Experiment Station," *Science* 68, no. 1760 (1928): 265-66; Afrânio do Amaral, *A General Consideration of Snake Poisoning and Observations on Neotropical Pit-Vipers*.

cooperation from the Venezuelan government and the Creole Petroleum Company (a division of Standard Oil).[660] Although not listed by Jack, there was also the El Verde station in Luquillo forest in Puerto Rico, begun by the USDA Forest Service in 1940, and which would become an increasingly important site for long-term tropical forest research in the post-war period as Howard T. Odum used the site for an Atomic Energy Commission radioecology project.[661] By 1957, the University of Michigan had begun talks with the government of Mexico about the possibility of establishing another new station in Chiapas.[662] BCI's Smithsonian administrators looked at these new developments with some degree of chagrin, since they were still in the process of rebuilding their own station.[663] Certainly, Moynihan's efforts both to expand from BCI into STRI and to encourage global comparative work among his staff scientists was in part a reaction to competition from other tropical research opportunities. The post-war period was a boom-time for tropical biology. Institutionally, there was a proliferation of emerging research sites and university initiatives. Intellectually, the question of species diversity was helping to

660. Rancho Grande was Beebe's most recent attempt to replace the ill-fated Kartabo station, but he would abandon it following the 1948 Venezuelan coup d'état, with hopes that it would be continued by the Venezuelans. The circumstances surrounding its founding, and the difficulties leading to its abandonment by Beebe in 1948 are discussed in Carol Grant Gould, *The Remarkable Life of William Beebe: Explorer and Naturalist*, 357-68. See also "The Rancho Grande Station of the New York Zoological Society," *Science* 101, no. 2617 (1945): 195. Beebe's account of the 1945-1946 field season appears in William Beebe, *High Jungle.* (New York: Duell, Sloan and Pearce, 1949).

661. Howard T. Odum, and Robert F. Pigeon, *A Tropical Rain Forest; a Study of Irradiation and Ecology at el Verde, Puerto Rico.* (Oak Ridge, TN: Division of Technical Information, U.S. Atomic Energy Commission, 1970); Thomas R. Crow, "A Rainforest Chronicle: A 30-Year Record of Change in Structure and Composition at el Verde, Puerto Rico," *Biotropica* 12, no. 1 (1980): 42-55. On Howard and Eugene Odum and radioecology, see Joel B. Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*, Book ed. (New Brunswick, NJ: Rutgers University Press, 1992), 100-21.

662. "The university of michigan Committee on a Proposed Center for Tropical Studies." Box 18, Folder 4, RU 135, SIA.

663. A. R. Kellogg to T. H. Hubbell, 5 March 1958 and T. H. Hubbell to A. R. Kellogg, 15 May 1958. Box 18, Folder 4, RU 135, SIA.

bring coherence to previously disparate lines of research. Orlando Park's hope for a "full scale program for tropical ecology" seemed finally to be coming to fruition.

Conclusion

Early twentieth-century research attracted the greatest concentration of American biologists working on basic tropical research. If research at BCI began to show stronger intellectual coherence in the 1950s and 1960s, this emerged both through larger developments in biological theory as well as out of a broadening and longer-term empirical basis of knowledge about the island's environment and organisms. Indeed, a field of "tropical biology" had started to take shape united not only around a geographical region of study but also by questions increasingly seen as uniquely stemming from the nature of the tropics. Out of the crisis of World War II, BCI emerged not only in a stronger position itself, but, by the late 1950s, surrounded by a prolific growth of ideas and fledgling institutions for tropical biology.

The recommendations made by Park in 1945 were slowly put into action. Universities began to invest in active graduate training in tropical biology, starting with the 1950 Harvard course at Soledad. Soon, new journals specifically devoted to tropical biology were founded, including the *Revista de Biologia Tropical* in 1953, *Tropical Ecology* in 1960, and *Biotropica* in 1969.[664] In 1960, ten years after its establishment, the National Science Foundation began a program to support tropical biology. It funded not only research and infrastructure improvements at BCI, but a series of national conferences on topics in tropical biology, including the 1960 Miami Conference on Tropical Botany, the 1962 Conference on problems in Education and

664. http://www.ots.ac.cr/tropiweb/intpages/history.html

Research in Tropical Biology in Costa Rica, the 1962 Neotropical Botany Conference in Trinidad, and others.[665] Out of these conferences emerged new unity for American tropical biologists through the Association for Tropical Biology, and new research institutions like the Organization for Tropical Studies.

But another factor was also at play. The outpouring of organizational activity in tropical biology around 1960 was not merely a logical response to intellectual developments. It was also a response to a radically altered political landscape. While tropical biology began to coalesce intellectually at the middle of the twentieth century, its institutions had remained dependent on US hegemony in the Greater Caribbean. In the post-war period, that hegemony was seriously challenged by the 1959 Cuban Revolution and widespread protest against decades of US interventionism and neocolonial business practices in the Caribbean and Latin America. Confronted by political crises in host countries, American biologists scrambled to unify and attempted to realign themselves to face new political realities.

In his 1945 speech to the ESA, Orlando Park had spoken not only of the intellectual richness of the tropics as an area of study, but also the potential role of tropical biology in building "hemispheric cultural solidarity and economic cooperation" in the post-war era.[666] Yet little real effort at cooperation was ever attempted. Ties to US government and corporations had been crucial to the long-term survival of American field stations in the Caribbean from the beginning of the century, even when, as at Soledad and BCI, universities and other academic

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665. *Conference on Tropical Botany, Fairchild Tropical Garden, May 5-7, 1960 (Washington, DC: Division of Biology and Agriculture, National Academy of Sciences-National Research Council, 1960).* See also Toby A. Appel, *Shaping Biology*, 197-205.

666. Orlando Park, "Observations Concerning the Future of Ecology," 6.

institutions were in control. Even BCI, which had carved out a solid reputation in basic biological research through its semi-independent status, still fundamentally depended on its location within what remained essentially colonial territory: the US-controlled Panama Canal Zone. Where, as in Cuba, American hegemony was less total—where a much stronger national scientific community existed and where the Cuban government could affect labor at the Soledad station through wage and hiring requirements—station administrators were obliged to engage in at least some degree of cooperation with government efforts in agricultural diversification and forest conservation. At BCI, the station's increasing financial and institutional stability after World War II actually depended on an even closer relationship to the US government, through the Smithsonian, rather than through increasing independence or relationships with the Republic of Panama. Indeed, in his efforts to expand STRI locally, Moynihan remained careful to stay firmly within the bounds of the Canal Zone, as Catherine Christen has shown, rather than to attempt ventures in the republic even when Panamanian sites might have been scientifically more attractive.[667] While social life on BCI changed enormously after Zetek's departure, including the end of racial segregation and increasing involvement of women scientists, there was little effort during the first seven years of Moynihan's tenure as director to attract Latin American researchers or to reach out to Panamanian institutions. STRI remained essentially an American outpost in the heart of Panama.

From the beginning of the twentieth century, biologists like Thomas Barbour had worked to link basic tropical biology to the expanding networks of US power in the Greater Caribbean.

667. Catherine A. Christen, "At Home in the Field: Smithsonian Tropical Science Field Stations in the U. S. Panama Canal Zone and the Republic of Panama," 573-74.

Now this source of strength became its greatest weakness. In aftermath of the Cuban Revolution and with the hardening of the US embargo in 1963, Harvard's station at Soledad was cut off from American biologists. At the same time, demands for the return of the Canal Zone to the Republic of Panama grew louder as protests erupted between 1962 and 1964. BCI faced a new turning point perhaps more serious than the one it had confronted at the end of World War II. Even as tropical biology gained new recognition on the US national scene, the future of its critical long-term field stations suddenly faced uncertainty. With the vulnerability of American tropical biological institutions exposed, American biologists began to realize they would have to form new alliances if they hoped to survive these political transformations and solidify the emerging interdiscipline of tropical biology.

Conclusion: Revolution and Realignment

Consider three disparate moments in July of 1953. In Washington DC, the new Secretary of the Smithsonian Institution, Leonard Carmichael, sat at his desk gathering correspondence to familiarize himself with the obscure tropical outpost his institution had inherited, Barro Colorado Island (BCI) in the Panama Canal Zone. He began preparations for the October meeting of the BCI "alumni" to consider the future of the station. A thousand miles south, E. O. Wilson traveled up a steep logging road in Cuba's Trinidad Mountains, near Soledad, with the class of Harvard's Biology 215. Reaching a forested slope too rugged to be of use to either farmers or loggers, Wilson stooped amid the limestone rocks to uncover a cluster of *Macromischa wheeleri*, a species of ant that had not been seen for the past forty years. On the 26th of that same month, across the island to the east in Santiago de Cuba, Fidel Castro and a small band of fighters launched an attack on the heavily defended Moncada Barracks.[668] The assault was a bloody military failure, but as a symbolic gesture against the recent US-backed coup by the dictator Fulgencio Batista, it effected the first spark of the Cuban Revolution that would be completed in 1959.

Carmichael's paperwork in Washington and the activities of visiting researchers and students at BCI and Soledad perhaps seem wholly unrelated to Castro's audacious strike, except in so far as political turmoil can always have the potential to disrupt scientific research. Yet a deeper underlying connection exists. Whatever the varied personal political feelings of American

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668. Wilson himself briefly juxtaposes his trip to Cuba with the attack on the Moncada Barracks. Edward O. Wilson, *Naturalist* (Washington, DC: Island Press, 1994), 148-49.Wilson, *The Diversity of Life*, 408.

tropical researchers, their ability to access Caribbean environments—particularly for long-term research—depended on ties to the US power structure in the region.

No one knew this better than Thomas Barbour, whose administrative abilities, wealth, and high-level friendships were largely responsible for allowing BCI and Soledad to become the only two early twentieth century American-run biological stations to survive World War II. He took full advantage of his connections in the business world and in government to shepherd the development of these stations for tropical research. At Soledad, Barbour courted the sugar baron Edwin Atkins, and won an endowment that allowed the station to expand beyond a program of economic botany. At BCI, he drew on his connections with the United Fruit Company and the US military to secure passage and reduced rates for researchers, and built a partnership with the USDA entomologist James Zetek to manage relations in the Canal Zone. Barbour's frustration with slow progress in the Institute for Research in Tropical America, in fact, stemmed largely from his feeling that other members were unwilling to take action to forge the alliances with business and government that were needed to make a station succeed amid the realities of operating in a tropical dependency of the US. Barbour argued for basic biological research, but not in isolation from economic interests. He envisioned tropical biology as a coherent whole, in need of greater investment in both basic and applied research, and requiring more attention to connections among botany, zoology, and all biological subdisciplines. Moreover, in the early twentieth century, he knew that few opportunities existed for American biologists to combine laboratory and field work or to make long term observations and experiments in the tropics. Barbour was not along in the scientific community in many of these positions, but he was in a fortunate position to do something about them.

As Soledad and BCI grew, the kinds of research American biologists were able to accomplish expanded to include not only more comprehensive taxonomic surveys, but also more intensive and long-term studies of animal behavior and community ecology that had little obvious relevance to applied concerns. This was particularly true at BCI, where a unique combination of a nature reserve and biological laboratory gave researchers a different quality of access to a tropical forest than available anywhere else. As the only US-run tropical forest station in continuous operation since the 1920s, researchers at BCI could amass a set of basic, long-term observations of tropical organisms and their life histories. For over a hundred years, the prevailing discourse about the tropics among European and American travelers had recognized the great luxuriance of tropical vegetation and the astounding numbers and variety of species in many tropical environments. As we saw in Chapter 1, American botanists at the turn of the century saw the intellectual potential of tropical research to challenge biological assumptions based on a narrow temperate zone bias in existing research. Their focus, however, was on understanding plant physiology under the tropical environmental conditions that were thought to be primeval and ideal for plant growth. Only when scientists had the opportunity to engage in long-term, place-based research could scientists ask how such a diversity of species could coexist. Stable tropical research institutions allowed many biologists to pursue research on a wide variety of different organisms interacting within the same community, and to do so from an array of perspectives across the biological subdisciplines—including physiology, behavior, taxonomy, ecology, and evolution. The accumulation of research allowed biologists to ask more complex questions about the interactions among species and how intricate webs of interdependent species had evolved. Ultimately, research stations in the tropics made questions about the evolutionary and ecological causes of species diversity accessible to investigation.

Even as space opened up for research without immediate practical use, shaped more by academic and professional concerns within biology than by direct economic imperatives, however, colonial political and economic relations continued to underpin the existence of US tropical research institutions. Flows of funds and visitors depended on nurturing relations with corporations and US government agencies that permeated governance, finance, and networks of transportation in the Greater Caribbean. The organization of labor at tropical stations also reflected colonial models. At Soledad, administrators drew laborers from a plantation workforce with recent roots in slavery, while BCI in large part reproduced the hierarchy of Canal Zone society. Moreover, American station science fundamentally depended on the kind of long-term access to land that could only be guaranteed by US or corporate ownership. Harvard's Cuban station was usually referred to as "Soledad"—the name of the American-owned sugar plantation on which it was located, a plantation which had been acquired from Cuban families through defaults on predatory American loans. Although under quite different circumstances, BCI also depended on land appropriation. Barbour bought out several small-holder lessees who were living on the island when the Canal Zone Governor declared it a nature reserve in 1922. Legally speaking, BCI was firmly embedded in the political structure of the US-controlled territory, which had been cut through the heart of Panama after the US-supported Panamanian insurgency separated the new country from Colombia in 1903. Conversely, it was essentially a lack of permanent control over land that had doomed not only American use of the Cinchona station, as we saw in Chapter 1, but also William Beebe's efforts to maintain stations in British Guiana (now Guyana) and later in Venezuela.

At Soledad and BCI, Barbour succeeded in creating space for basic tropical biology, not by isolating it, but by aligning it with the broader networks of US interests in the region. By virtue of these connections, the stations Barbour helped to build persisted after his death. Yet, the onset of the Cold War and a new era of nationalism in Latin America and the Caribbean ushered in a very different world than the one in which Thomas Barbour had worked to create these institutions over previous decades. By the 1960s, US hegemony was being openly challenged.

As revolution swept over Cuba in 1959, the current director of Soledad, I. Duncan Clement, at first presented an optimistic and accommodating front. He reported that the garden and laboratory remained in "splendid condition," suffering "almost no damage during the revolution," a fact attributed "in part to the high regard which Cubans, irrespective of their political views, have for it."[669] After Barbour's death, subsequent directors of the station had tried to strengthen ties with the Cuban government, framing the station's agricultural work as useful to the Cuban people and the island's economy rather than just to one company or industry. Clement and his wife remained at Soledad even as other Americans fled the island—for months they found themselves behind rebel lines at night as Castro's forces swept out from refuges in the hills to the east to engage with Batista's troops. When the revolution was concluded, Clement attempted to make new alliances, responding to requests for planting stock and technical advice on reforestation, as well as offering consultation about the possibility of creating of an ecological

669. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1958-1959* (Cambridge, MA: Harvard University, 1960), 455.

field station in Zapata swamp.[670] There was some hope that Soledad could adapt to the new conditions.

Clement could do little in the face of the rapid deterioration of US-Cuban relations, however. With the initiation of the 1960 US embargo, the staff of the Soledad station made preparations for a separation they hoped would be temporary. They packed up equipment and sent planting records and inventory cards to Cambridge. The station's herbarium, library and lab were "put in stand-by condition." They pruned the arboretum's trees, preparing them "to withstand a period of neglect." The American staff, along with two Cuban employees who were unsympathetic with the new regime, returned to Cambridge or transferred their work to the Fairchild Tropical Garden in Miami for an interim of uncertain length.[671] Following the 1962 Cuban Missile Crisis and the hardening of the US embargo in 1963, however, it was obvious that the separation would be of much longer duration. The Soledad station was reportedly one of the last US properties to be appropriated by the Cuban government.[672] Angel Valiente, a former employee of the station who became a supporter of the Revolutionary movement after his father had been murdered by the Batista regime, now became superintendent of the garden. Although both Valiente and the Harvard administrators tried to maintain contact, letters and scientific journals mailed to the station's new Cuban administration began to be returned as

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670. Ibid.

671. The Cuban employees were Felipe Gonzalez and Esperanza Vega. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1951-1962* (Cambridge, MA: Harvard University, 1963), 565-66. Gonzalez worked at the Fairchild Tropical Botanical Garden until his retirement in 1980; John Popenoe, "News of the Garden," *Fairchild Tropical Garden Bulletin* (1980): 4. (FTG)

672. "An interview with Richard A. Howard at the Harvard University Herbaria on April 15, 1993, interviewed by Judith A. Warnement, Librarian, produced by Bruce Baird-Middleton." Box 2, VHS tape 2, Richard A. Howard Papers, IB RAH, AAA.

undeliverable.[673] Through the dedication of its remaining Cuban staff, Soledad's garden and arboretum would survive as the Jardín Botánico de Cienfuegos, managed by the Academia de Ciencias de Cuba. Its ties with Harvard were severed.

The loss of the Cuban outpost was a blow not only to Harvard biologists, who had built up a strong program in tropical biology with Soledad as a cornerstone, but also to the broader community of American biologists with interests in the tropics. And as demonstrations against US control of the Panama Canal Zone grew in intensity during the same period, many wondered if the Smithsonian's station at Barro Colorado Island would be the next to be lost. Dissatisfaction with the terms of the Canal Treaties grew in the 1950s, and Panama won some concessions through negotiations in 1954. Scattered protests occurred in 1959 and 1962 as Panamanians increasingly sought to assert their sovereignty in the Canal Zone, and in 1962 US president John F. Kennedy agreed to allow the Panamanian flag to fly alongside the American at certain locations within the Canal Zone.[674]

By 1963, BCI director Martin Moynihan could read the writing on the wall. Moynihan told Smithsonian Secretary Leonard Carmichael, "It seems very possible that the Canal Zone will eventually be returned in whole or in part, to the Republic of Panama. Even if this should not occur, the wishes of the Panamanian government certainly will become increasingly influential in determining the policy of the Canal Zone government." Moynihan pointed out the worry that the Smithsonian's position in the Canal Zone was "slightly dubious" given the most recent US-Panama treaty assuring that Canal Zone activities would be limited to the actual operation of the

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673. Richard A. Howard, "Some Reminiscences of the Soledad Botanical Garden, Cuba, 1940-1950," 170.

674. Robert W. Aguirre, *The Panama Canal* (Boston, MA: Martinus Nijhoff Publishers, 2010), 233.

Canal. Nevertheless, he felt “sure that, if we play our cards correctly, we will have no difficulty in continuing operations in this area, no matter what happens to the Canal Zone itself. But we must start making plans for the future now.” Above all, he advocated getting “on the best possible terms with the Panamanian government, and attempt[ing] to secure favorable publicity.”[675] Few Panamanians were even aware of BCI’s existence, and if it were to believed to be a branch of the increasingly unpopular Canal Zone government, the results could be disastrous. Moynihan suggested looking at the legal model of the Gorgas Memorial Laboratory, which had US government support and had long operated within Panama. And although he doubted there would be any takers, he also thought that an offer of fellowships to Panamanian students in biology might be the best route for publicity, especially because university students were “among the most radical elements in Panamanian political life.”[676] For the first time, the Smithsonian began talks to set up fellowships for Latin American scholars through the Organization of American States.[677]

Tensions came to a head in 1964, as riots erupted following the January 9 “Flag Pole incident,” a dispute over the right of Panamanians to fly the flag of their Republic within the Canal Zone. Begun as a clash between Panamanian students and Canal Zone police and residents, fighting spread into Panama City and throughout the country. At least twenty-five people died and hundreds were injured.[678] In the week following the violence, Moynihan calmly

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675. Moynihan to Carmichael, 26 February 1963. Box 23, Folder 2, RU 135, SIA.

676. Moynihan to Carmichael, 26 February 1963. Box 23, Folder 2, RU 135, SIA.

677. Martin Humphrey Moynihan, “Report on the Canal Zone Biological Area,” *Annual report of the Smithsonian Institution* (1964): 231-35.

678. Robert W. Aguirre, *The Panama Canal*, 233-37.

communicated to Smithsonian Secretary Carmichael, “It seems quite possible that Panama and the United States will start negotiating or discussing the status of the Canal Zone again some time in the near future.” As he had warned earlier, he believed that Panama might ask for the transfer of land not being used to run the Canal and asked the Secretary to “keep in touch with the State Department, to find out if Barro Colorado is mentioned.”[679] Like Clement at Soledad, Moynihan tried to downplay the risk to the station posed by changing political circumstances. In spite of assurances that BCI was well removed from the conflict, “an appreciable number of scientists... cancel[ed] their proposed visits” in 1964.[680] Suddenly the future was uncertain for even the most well-established of institutions for American research in the tropics.

In the final chapter we encountered the explosion of interest in the tropics among biologists at the beginning of the 1960s. The question of what tropical research could contribute to ecology was a live one, and an understanding of how trophic and community dynamics controlled species diversity was the prime contender for an answer.[681] The loss, and potential loss, of long-term tropical research sites just as they were becoming more and more relevant to broader theoretical debates shocked biologists into action. As previously discussed, several universities had already independently begun to initiate and expand their tropical programs in the 1950s, but the vulnerability of well-known sites for long-term tropical research pushed the American scientific community finally to come together around tropical biology. During a series of NSF-sponsored conferences on tropical biology, the need for cooperation, among US

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679. Moynihan to Leonard Carmichael, 17 January 1964. Box 2, Folder 1, RU 135, SIA.

680. Martin Humphrey Moynihan, “Report on the Canal Zone Biological Area,” 232.

681. This question was explicit: P. W. Richards, “What the Tropics Can Contribute to Ecology.”

biological institutions and with institutions in countries in the Caribbean and Latin America, became clear.

Finally, American tropical biologists began to seriously consider working at and with existing Latin American and Caribbean institutions. At the first NSF conference at the Fairchild Tropical Garden in Miami in 1960, botanists agreed that "tropical botany is now and will become increasingly important as an area demanding maximum cooperation between men of all nations concerned."[682] It emphasized the importance of tropical botany to conservation, as well as the need for new research institutions. In a widely read report, conference organizers Walter Hodge (who had led Harvard's first Biology 215 in 1950) and David Keck helped to expand the conception of where Americans might work in the tropics. Hodge and Keck visited sixty-one different research centers, universities, museums, gardens, and stations in the tropics of the Western Hemisphere, in twenty-one different countries, "where basic biological research and/or research training may be conducted and where the participation of foreign scientists is invited."[683] They listed BCI as "without exception...still the best field station in the wet lowland neotropics."[684] But—although Cuba was conspicuously absent—the report helped to open the possibility of moving forward through international cooperation rather than primarily by controlling American stations abroad.

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682. *Conference on Tropical Botany, Fairchild Tropical Garden, May 5-7, 1960, 13.*

683. W.H. Hodge, and D. D. Keck, "Biological Research Centres in Tropical America," *Bulletin of the Asscoiation for Tropical Biology* 1 (1962), 107. For additional historical background on the Hodge and Keck report see Toby A. Appel, *Shaping Biology*, 199; Donald Stone, "The Organization for Tropical Studies (OTS): A Success Story in Graduate Training and Research."

684. W.H. Hodge, and D. D. Keck, "Biological Research Centres in Tropical America," 110.

At the 1962 follow-up conference, in San Jose, Costa Rica, Clement, the exiled head of Harvard's Soledad station, spoke strongly in favor of the need for new institutions set up on a basis of hemispheric cooperation.[685] It was no coincidence that Cost Rica appeared as a viable partner to American biologists; it was a country with a longstanding exceptionalist national narrative, which emphasized a record of democracy, environmental conservation, and a reputation as the "Switzerland of Central America" for both the country's supposed egalitarianism and racial purity.[686] Led by Harvard and the University of Michigan, whose efforts to establish a station in Mexico had stalled out, talks began that would lead to the establishment of the Organization for Tropical Studies (OTS), the following year.[687] Harvard and Michigan, along with six other American universities and the University of Costa Rica, joined together to create a network of research stations and an educational program in tropical biology. E. O. Wilson, now a Harvard professor, signed onto OTS on behalf of his university. For the next

685. In fact, the 1961 class of Biology 215 was held in Costa Rica. Donald Stone, "The Organization for Tropical Studies (OTS): A Success Story in Graduate Training and Research," 148.

686. Lowell. Gudmundson, *Costa Rica Before Coffee: Society and Economy on the Eve of the Export Boom* (Baton Rouge, LA: Louisiana State University Press, 1986), 1-24; Catherine A. Christen, "Development and Conservation on Costa Rica's Osa Peninsula, 1937-1977: A Regional Case Study of Historical Land Use Policy and Practice in a Small Neotropical Country" (Dissertation, Johns Hopkins University, 1994); Catherine A. Christen, et al., "Latin American Environmentalism: Comparative Views," *Studies in Comparative International Development* 33, no. 2 (1998): 58; Kirk S. Bowman, "New Scholarship on Costa Rican Exceptionalism," *Journal of Interamerican Studies and World Affairs* 41, no. 2 (1999): 123-30; Sterling Evans, *The Green Republic: A Conservation History of Costa Rica*, 1st ed. ed. (Austin, TX: University of Texas Press, 1999); Stuart McCook, "'Giving Plants a Civil Status': Scientific Representations of Nature and Nation in Costa Rica and Venezuela, 1885-1935," *The Americas* 58, no. 4 (2002): 513-36.

687. Laura Tangley, "Studying (and Saving) the Tropics: At Age 25, the Organization for Tropical Studies is Adding More Conservation Activities to Its Successful Teaching and Research Agenda," *BioScience* 38, no. 6 (1988): 375-85; Donald Stone, "The Organization for Tropical Studies (OTS): A Success Story in Graduate Training and Research."; L. J. Burlingame, "Evolution of the Organization for Tropical Studies," *Revista De Biologia Tropical* 50, no. 2 (2002): 439-72; Julie S. Denslow, "The Organization for Tropical Studies: 27 Years of Research and Education in the Tropics," *Journal of Vegetation Science* 1, no. 1 (1990): 133-34; Jay M. Savage, "The Founding of the Organization for Tropical Studies, Inc. (OTS: Present at the Pre-Conception, Conception, and Birth: a Personal Perspective)," http://www.ots.ac.cr/index.php?option=com_content&task=view&id=703&Itemid=785 (accessed October 26, 2012).

generation of tropical biologists, OTS's network of Costa Rican stations would replace Soledad, where Wilson had himself first encountered the tropics.

At a third 1962 conference in Trinidad, the formation of the Association for Tropical Biology was set in motion, and would become official in 1963. American tropical biologists now had a professional society to solidify their community, and specialty journals were soon to follow. American biologists, however, increasingly understood the need to participate in and help to strengthen an international community of tropical biology, not just an American one. Not only was OTS a joint enterprise, but at BCI, more serious efforts to foster the involvement of Latin American researchers and to cooperate with the government of Panama grew over the years, moving beyond the perfunctory and politically expedient efforts of the past. Following Moynihan, BCI's second resident director Ira Rubinoff would formalize relations with the Republic of Panama beginning, making STRI a joint Smithsonian-Panamanian operation. STRI would survive the 1977 Torrijos–Carter Canal Treaties and the ultimate transition of the Canal into Panamanian hands in 2000.[688]

By the mid 1960s, the landscape of tropical biology had significantly changed. New connections were being formed between American and Latin American scientific communities. Old connections, as that between Harvard and Soledad were severed, although work within Cuba went on in relative isolation. Some old institutions went forward in new guises, as, for example, the Atkins endowment was redirected toward Barry Tomlinson's tropical botany research and to graduate student travel, often to the OTS stations or BCI and the Smithsonian Tropical Research

688. Catherine A. Christen, "At Home in the Field: Smithsonian Tropical Science Field Stations in the U. S. Panama Canal Zone and the Republic of Panama," 564-75.

Institution in Panama.[689] BCI was the most significant institution to survive, but it did so not as single field station, but as an international center for research in tropical biology. Indeed, Moynihan's successful move to expand the Smithsonian's tropical program beyond BCI and into STRI in 1966, discussed in the last chapter, was in part a response to the momentum built through the NSF-sponsored meetings on tropical biology, not to mention the growth of competition in the form of OTS.[690]

Not only were the institutional foundations and professional status of tropical biology altered and strengthened in the 1960s, however. Discourse surrounding the purpose of tropical biology also took on new forms. In the early twentieth century, as we have seen throughout this dissertation, American biologists' justifications for tropical research depended fundamentally on two arguments. First, that biology was biased toward research in the temperate zone and that tropical research was needed to ensure that biological knowledge was truly general. Second, that the development of the resources of the tropics depended on scientific understanding of tropical plants and animals. Some efforts, like MacDougal's American tropical laboratory, emphasized the first. Others, like the Harvard-Atkins partnership, emphasized the second. Most often, like the Institute for Research in Tropical America, they were a complicated admixture of both justifications.

In the 1960s, a new emphasis on the conservation of tropical diversity as a rationale for tropical biology began to emerge, pushed both by the broader flowering of the US environmental

689. Harvard University, *Report of the President of Harvard College and Reports of Departments, 1970-1971* (Cambridge, MA: Harvard University, 1972), 393.

690. Catherine A. Christen, "At Home in the Field: Smithsonian Tropical Science Field Stations in the U. S. Panama Canal Zone and the Republic of Panama."

movement as well as by scientists' personal encounters with tropical habitat loss. Conservation had in fact been among the interests of Americans working in the tropics since the 1920s, as BCI's reservation as a nature preserve illustrates. But, as the case of BCI also shows, American scientists interests were primarily concerned with preserving representative "remnants" of tropical nature for future scientific research. As species diversity became a central intellectual problem in the 1960s, particularly in tropical biology, the potential of species diversity to become a target of conservation soon followed. Perhaps as much as any efforts to increase the diversity of nationalities represented among their ranks, the new emphasis on the conservation of tropical diversity served rhetorically to align tropical biologists with the interests of tropical countries rather than the US corporate and state interests, which in an era of resurgent Latin American and Caribbean nationalism were now tarnished with the label of neocolonialism.

As the imperative to conserve the tropics' vast diversity of species became a consensus among tropical biologists in the 1960s, however, the new argument did not supplant the earlier rationales for tropical research. Both the need for the correction of biases in biology and the scientific development of natural resources remained significant justifications for research in tropical biology. But, significantly, diversity conservation gave these arguments new valences. Tropical biologists continue to point to the relative taxonomic ignorance of tropical species and the underrepresentation of tropical sites in ecological research, despite the fact, they emphasize, that the tropics contain the world's greatest diversity and face the most significant environmental threats. (Perhaps, the primary difference today is the significant over-representation of Panama and Costa Rica within tropical data-sets, due to their unusual histories of tropical station science.[691]) At the same time, the conservation of tropical ecosystems, and especially rainforests, is justified by the economic value of the diversity they contain, whether as the "world's medicine cabinet" for bioprospecting efforts or as fully-functioning "service providers" for clean air, carbon sequestration, and erosion control that cannot be matched by low-diversity man-made replacements.

Although the neologism "biodiversity" was still two decades away, tropical biologists were already emphasizing the uniquely rich species diversity of the tropics as the chief justification for conservation in the 1960s. The early twentieth-century history of tropical biology is thus of broad importance. Not only can it tell us about the importance of long-term institutions and place-based research to the coalescence of the field of tropical biology, but it also gives us insight into the emergence of the most direct precursor of the powerful late twentieth-century discourse of biodiversity. Members of the newly formalized community of tropical biologists—including E. O. Wilson, Peter Raven, and Daniel Janzen—became key proponents of biodiversity in the 1980s, and it is no coincidence that rainforest conservation became a key popular touchstone for this movement.[692] More subtly, significant rhetorical elements of arguments for tropical research were carried beyond the tropics into the global discourse of biodiversity. Long an argument applied to the tropics by researchers from the temperate zone, biodiversity makes urgent the need to inventory the huge numbers of still unknown species inhabiting the globe. Resource development has also found a new life within the biodiversity

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691. Laura J Martin, et al., "Mapping Where Ecologists Work: Biases in the Global Distribution of Terrestrial Ecological Observations."

692. All three were authors in the publication that brought "biodiversity" into the public discourse, in which the conservation of tropical environments was foregrounded: Edward O. Wilson, et al., *Biodiversity* (Washington, DC: National Academy Press, 1988).

discourse, offering the promise of sustainable development through the conservation and use of biodiversity.[693]

In turn, the institutions we have encountered through our story have enthusiastically embraced "biodiversity." Today, STRI's mission statement is "to increase understanding of the past, present, and future of tropical biodiversity and its relevance to human welfare."[694] Likewise, the Jardín Botánico de Cienfuegos, in its recent efforts to increase its connections with the international scientific community, has recast itself as a repository of tropical plant biodiversity.[695] Tropical biologists remain key players in this global discourse. Whether this new discourse can truly challenge the structures that contributed to its emergence in the twentieth-century Caribbean, however, or whether these structures have re-emerged in new forms remains to be seen.

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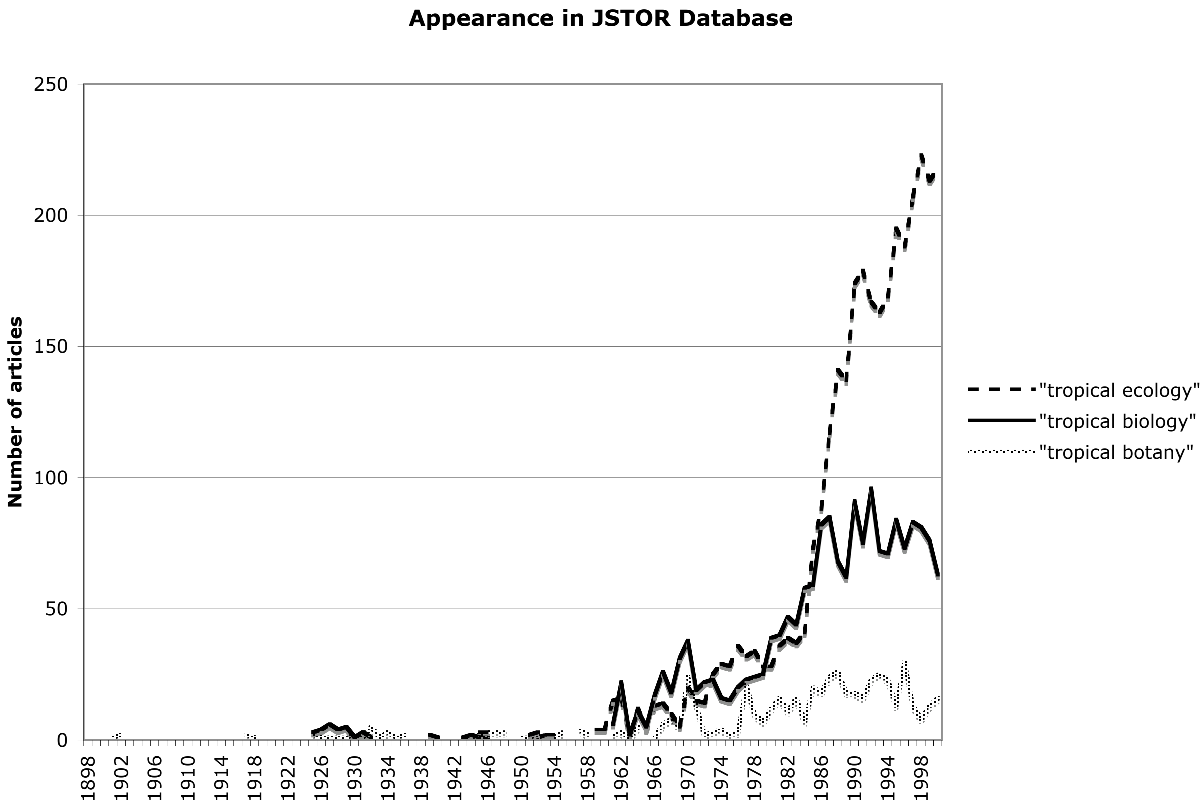
693. Markku Oksanen, and Juhani Pietarinen, *Philosophy and Biodiversity*, 16-17, 244; Sahotra Sarkar, *Biodiversity and Environmental Philosophy: An Introduction*.

694. "STRI Homepage," http://www.stri.si.edu/ (accessed October 20, 2012).

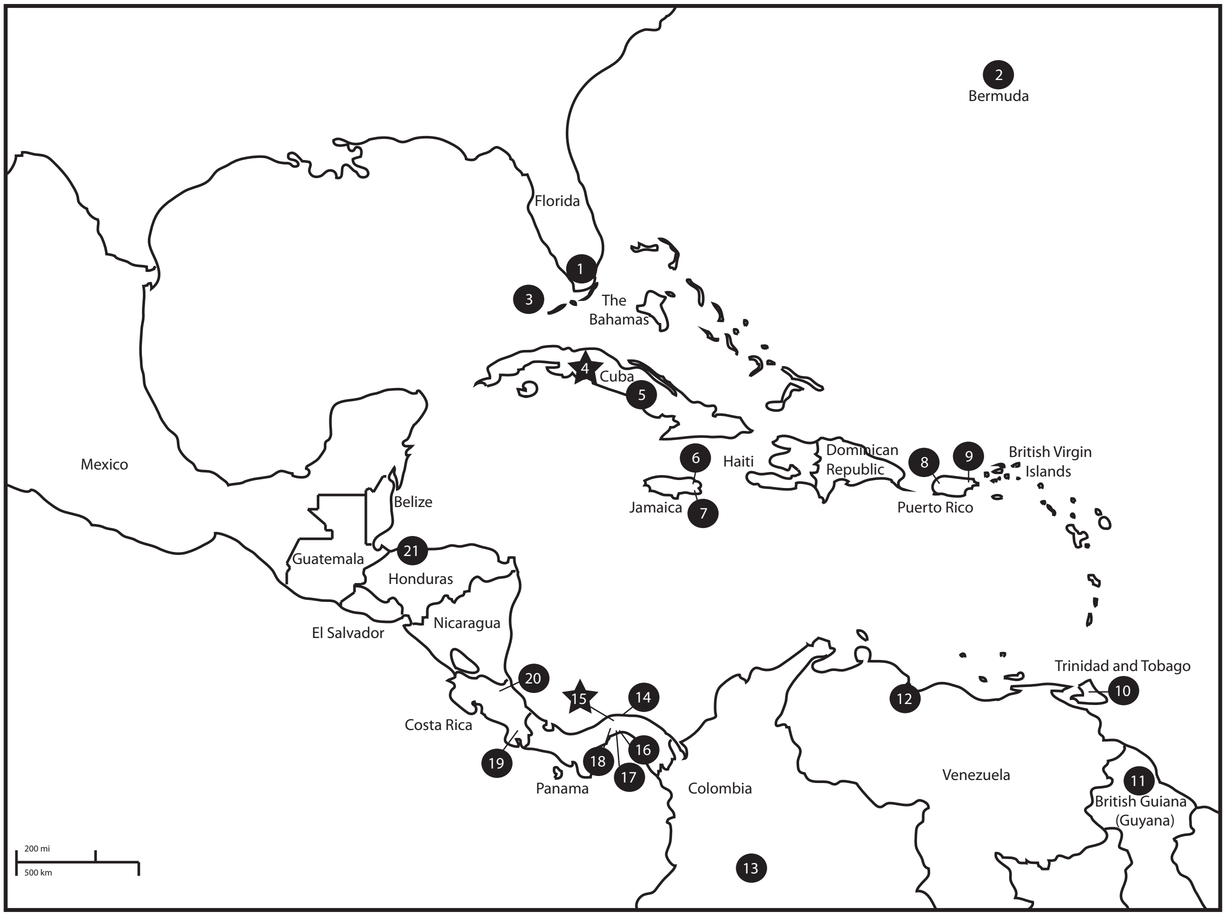
695. Dan Hazen, "Cienfuegos Botanical Garden: Harvard's Legacy, Cuba's Challenge."; Fernando C. Agüero Contreras, and Cristóbal Ríos Albuerne, "El Jardín Botánico de Cienfuegos: Sintesis Histórica," *Islas* 111 (1995): 146-53; Lázaro J. Ojeda Quintana, et al., "El Jardín Botánico de Cienfuegos, Ciento Cinco Años en la Conservación de la Diversidad Biológica Vegetal," *Centro Agrícola* 34, no. 1 (2007): 51-55. See also one travel writer's account of a recent visit, Tom Miller, *Trading With the Enemy: A Yankee Travels Through Castro's Cuba* (New York, NY: Basic Books, 2008).

Appendix 1: Tropical Publications



Frequency of articles containing the search terms “tropical ecology,” “tropical biology,” and “tropical botany” in the JSTOR database. Created using http://dfr.jstor.org, accessed February 1, 2011.

Appendix 2



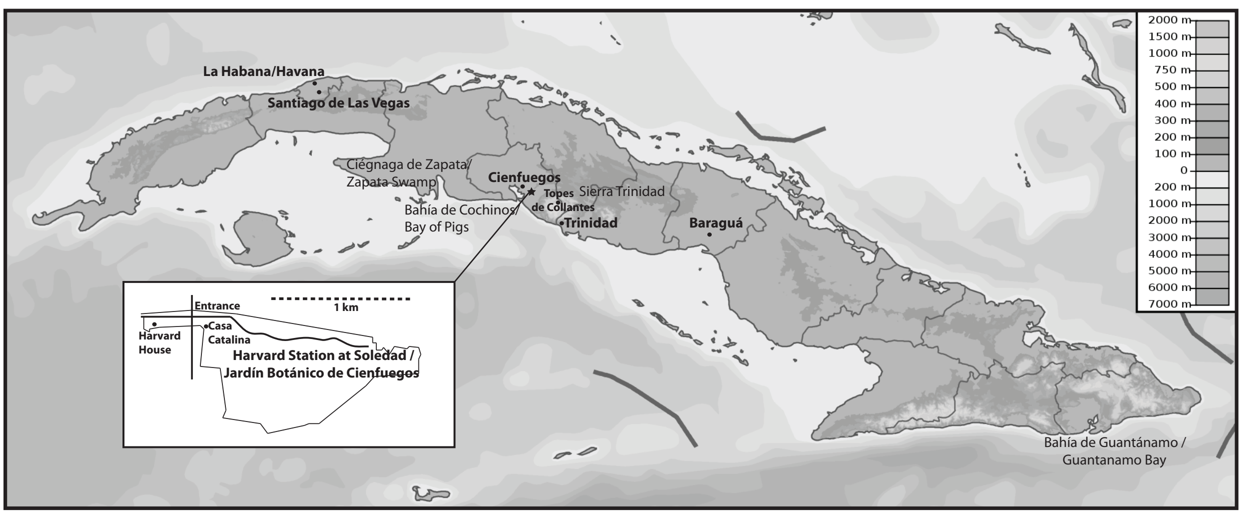
US-run Stations in the Greater Caribbean

1. Fairchild Tropical Botanic Garden (1938) *Bot.*
2. Bermuda Biological Station for Research (1903) *Mar.*
3. Dry Tortugas Marine Biology Laboratory, Loggerhead Key (1903-1939) *Mar.*
4. Harvard Botanical Station for Tropical Research and Sugarcane Investigation / Atkins Institution / Jardín Botánico de Cienfuegos (1899) *Ag. Bot. Bio.*
5. Cuban Sugar Club Experiment Station of the Tropical Plant Research Foundation (1925-1932) *Ag.*
6. Cinchona Botanical Station (1903-1921) *Bot. Bio.*
7. Johns Hopkins Marine Laboratory (1891-1897) *Mar.*
8. El Verde Field Station, University of Puerto Rico (1940) *Ag. Bio.*
9. Puerto Rico Agricultural Experiment Station of the USDA (1901) *Ag.*
10. William Beebe Tropical Research Station, “Simla” (1949) *Bio.*
11. New York Zoological Society Tropical Research Stations, “Kartabo” and “Kalacoon” (1916-1927) *Bio.*
12. Rancho Grande Station of the New York Zoological Society / Estación Biológica Fernández Yépez (1945) *Bio.*
13. Villavicencio Field Laboratory of the Rockefeller Foundation (1938-1948 withdrawal of Rockefeller Foundation) *Med. Bio.*

14. Galeta Marine Laboratory of the Smithsonian Tropical Research Institute (1964) *Mar.*
15. Barro Colorado Island / Canal Zone Biological Area/Smithsonian Tropical Research Institute (1923) *Bio.*
16. Summit Botanical Station / Canal Zone Experiment Station / Parque Municipal Summit (1923, today a botanical garden and zoo) *Bot.*
17. Gorgas Memorial Laboratory (1921-1990) Med.
18. Panama Extension Station of the Missouri Botanical Garden (1926-1939) *Bot.*
19. Las Cruces Biological Station (1962, purchased by OTS in 1973) *Bio.*
20. La Selva Biological Station (1954, purchased by OTS in 1968) *Bio.*
21. United Fruit Company Station at Tela, Lancetilla (1925, today Jardín Botánico Lancetilla) *Bot. Ag. Med.*

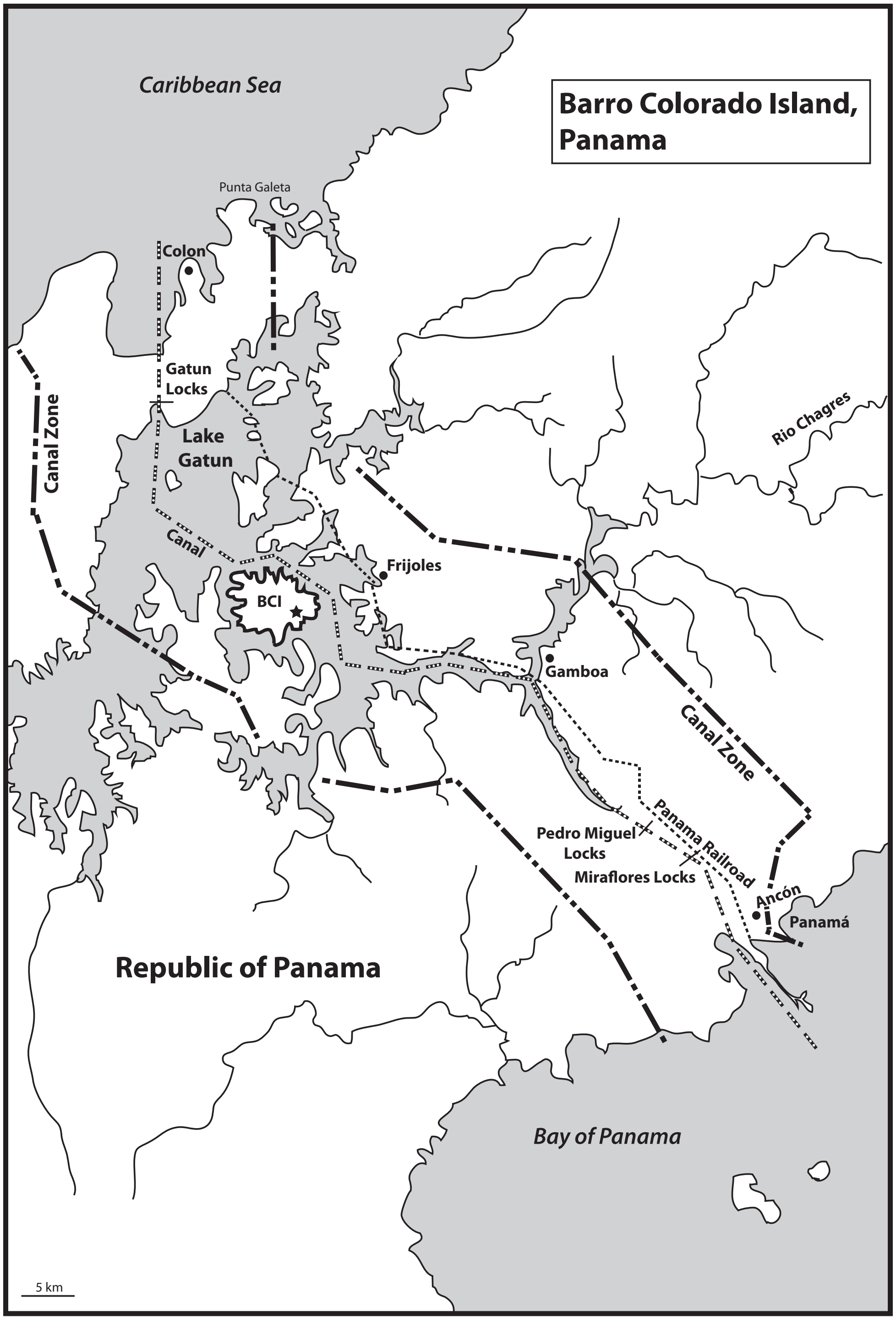
Mar.: Marine Station
Med.: Medical Laboratory
Bot.: Botanical or Introduction Garden
Ag.: Agricultural Experiment or Forestry Station
Bio.: Biological Field Station
(Date established-Date closed)

Appendix 3



The Harvard Station at Soledad, Cuba

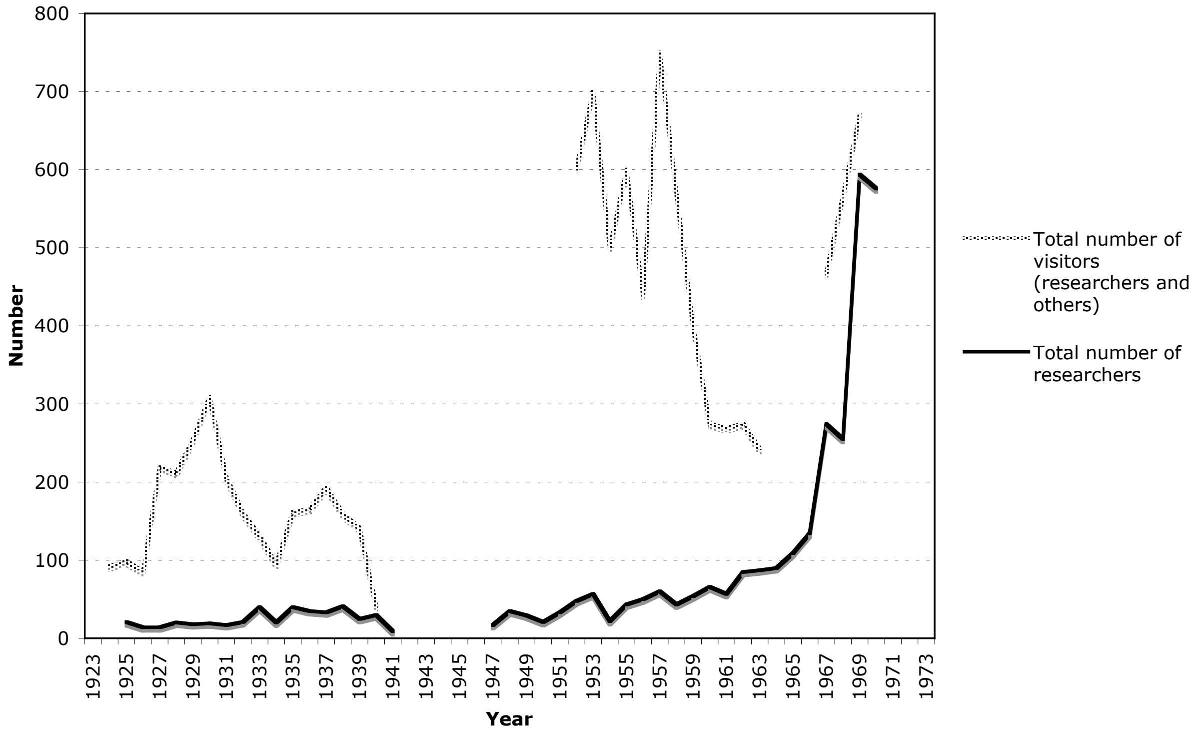
Appendix 4



Barro Colorado Island, Panama
Caribbean Sea
Punta Galeta
Colon
Gatun Locks
Lake Gatun
Canal Zone
Canal
Frijoles
BCI
Gamboa
Rio Chagres
Canal Zone
Panama Railroad
Pedro Miguel Locks
Miraflores Locks
Ancón
Panamá
Republic of Panama
Bay of Panama
5 km

Appendix 5

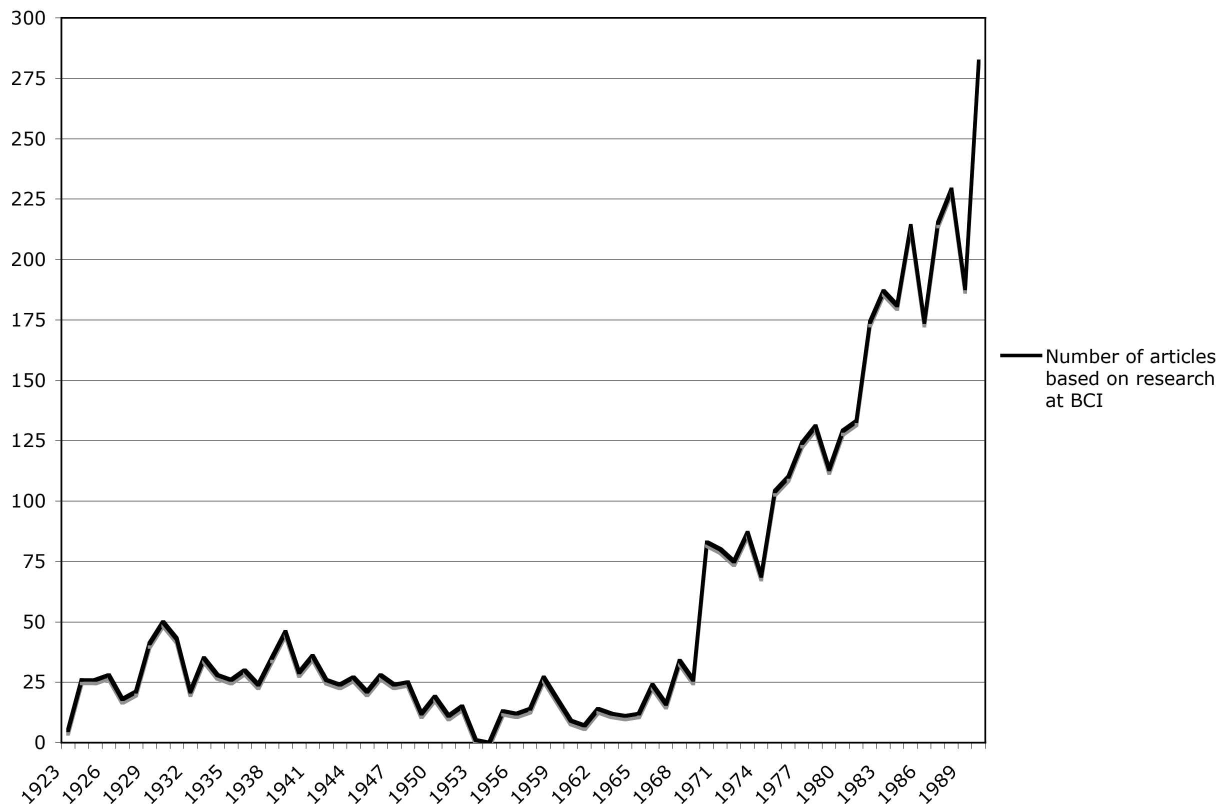
Barro Colorado Island visitors



Number of visitors to BCI based on data from the guest books and annual reports of the station. Gaps represent periods for which no data exists, or when non-researcher visitors were not listed. The BCI guest books (visitors' register) covers the period 1923-1940 and can be found in Box 4, RU 134, SIA. Annual reports were used thereafter.

Appendix 6

Barro Colorado Island Publications



This data comes from the Barro Colorado Island Bibliography maintained by the Earl S. Tupper Tropical Sciences Library at STRI and reflects papers that directly report on research carried out at BCI. The numbers are approximate because the bibliography is not complete, relying as it did on self-reporting by visiting BCI researchers. At the same time, it includes a small proportion of newspaper and magazine articles, especially those published during the stations first decade, which cannot technically be classified as scientific research papers. Nevertheless, it seems unlikely that the number for any given year is off by more than an article or two in either direction. Note that, as a rule, the numbers cited here do not reflect articles or books either of synthesis (such as textbooks) or of an abstract theoretical nature, which may drawn on BCI research as examples or raw data, whether or not such work is explicitly referenced. The Bibliography can be found online at http://www.stri.si.edu/english/research/bibliography/results.php?keyword=Barro%20Colorado. An EndNote Library file was kindly provided to the author by the STR Library.

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Atkins Institution Papers VI AT
General History IX

Fairchild Tropical Botanical Garden Archive (FTG):
David Fairchild Collection
Fairchild Tropical Garden Collection

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Daniel T. MacDougal Papers

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