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Outsiders as Innovators in the Life Sciences

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Both intellectually and institutionally, the life sciences occupy a fascinating middle ground between the physical and exact sciences, on the one hand, and the social sciences and humanities on the other. If biology were an animal, it would be a duck-billed platypus—something that appears chimeric, yet is fully rooted in its own historical lineage of accumulating adaptations, tinkering, and change.

Like that strange aquatic mammal, "half bird, half beast,"[1] its features point to its origins and ecology. Biology as a science has come into being as a patchwork, assuming its present visage as a consequence of myriad interactions between different traditions of knowledge, method, and philosophy while maintaining an overarching quest for understanding of the natural world. Indeed, historically, many researchers have come from outside biology to ask fundamentally *biological* questions. These outsiders have played a crucial and defining role in the growth of modern biology; they have brought new skills and ideas to the "inside" and have thus added something new to biology. As a consequence, biology can feel sometimes as if it is a strange hybrid—with a bill, a flat tail, fur, and webbed feet. After all, biologists include among their number men and women who sit before computers crunching numbers, as well as cavers who crawl through subterranean spaces in search of lizards; and biology counts among its tools patch-clamps and test tubes and microchip arrays and bird-snares. Its worldviews range from reductionism to dualism, idealism to emergence. It can often seem confusing: is biology really just one thing? Like that "highly interesting novelty," as the beguiling Australian bird-and-reptile-like mammal was once called, it is indeed one thing. And like the platypus, biology has been formed by adapting forces coming from outside, from the environment of other disciplines and practices. The platypus may seem like a paradox, because it appears to be chimeric. Biology likewise appears chimeric, but has attained an internal integrity and innovative potential from those external forces.

The molecular revolution of the late twentieth century, for example, was to a large degree stimulated by the influx of physicists into biology, applying as they did both a different style and approach to the problem of heredity. Ecology and population biology, too, have been determinatively shaped by the arrival of mathematicians to these fields, using tools from their own discipline to resolve biological problems with unfamiliar instruments. Linguists have applied their training and tools to investigate problems in cognition, social scientists to attack the puzzles of animal behavior, philosophers to probe conceptual foundations, writers to sharpen their pens on evolution, computer scientists and engineers to try to crack the mystery of life. As such, these "outsiders" have supplied important sources of innovation in biology and, each in his or her way, contributed to its patchwork design. What is of interest to us here is the manner in which scientists recruited from different disciplines have helped, and continue to help, produce novel approaches, concepts, theories, experiments, practices, insights, and—ultimately—novel scientific understanding.

This book seeks to provide historical descriptions and analyses for the ways in which researchers from the "outside" have been sources of significant innovation. The collection of cases assembled here critically examines these sources of innovation by considering how different researchers were able to integrate ideas, techniques, and methods across divergent scientific communities. As will become apparent, these innovations were NOT idiosyncratic accidents, but the result of the careful work of making intellectual connections, translating idioms, creating languages, and fostering new forms of collaboration that bridged training and experience in the biological sciences with a rich array of fields, disciplines, and perspectives. In the end, outsider interventions have given biology its peculiar form.

THE PROBLEM OF INNOVATION

As early as 1667, Thomas Sprat, historian of the Royal Society of London, noted a connection between being an outsider to a trade and inventiveness. A glance from an angle, Sprat argued, might well reveal a new aspect of nature. More recently, sociologists Joseph Ben-David and Robert Merton have shown the importance of disciplinary immigrants for the development of a particular science.[2] Merton, though, problematized a strict dichotomy or divide between those considered insiders and outsiders. As a result, later thinking about disciplinary boundaries reflected a more dynamic perspective regarding disciplinary identity. Lynn Nyhart's discussion of the birth of the discipline of physiology from the older anatomy in nineteenth-century German universities, on the other hand, considered the role played by different institutions in erecting boundaries between old and incipient fields.[3]

In a broader, more theoretical manner, Peter Galison has applied the anthropological notion of the "trading zone" to scientific practice, analyzing a number of examples from physics in which scientists of different subfields have met—creating common pidgin idioms, and then creoles—in order to jointly attack conundrums.[4]

But if the "outsider" and the "outsider as innovator" have been recognized as important categories in the history of science more generally, the treatment of the "outsider" in the history of biology has been focused more narrowly on specific instances. A number of histories of the molecular revolution, for example, highlight the role of physicists, such as Max Delbrück and Francis Crick, who became biologists and played a foundational role in the creation of molecular biology.[5] Evelyn Fox Keller, in her book *Making Sense of Life*, features some of the cyberneticists and artificial lifers who used metaphors from the world of computing to help probe deep problems in development and embryology.[6] A comparative treatment of the range of "outsiders" that have shaped the course of biology more broadly is sorely missing. Bringing together a diverse set of examples allows us to explore the various conditions that fostered both their movement into biology and their innovative contributions to biological understanding.

WHAT MAKES AN "OUTSIDER"?

In *Outsider Scientists* we conceive of outsiders in terms of academic disciplines. We are interested in scholars trained or practicing in a nonbiological discipline who moved into some branch of biology. These disciplinary newcomers or outsiders bring with them perspectives, skill, and training that are often not shared by insiders—those trained within biology. The fundamental question we are considering asks how moving from a field outside of biology to a field within biology has served as a significant source of scientific innovation.[7] We have asked our authors to consider what features of their subjects' original scientific training and research experience in a nonbiological context allowed them to make innovative contributions to the field of biology that they eventually joined. But a word of caution: we do not wish to hang too much on the category of *discipline*, because we do not think that the question of training and innovation depends strictly on moving from one discipline to another, nor do we believe that disciplines, as such, are hard and fixed categories. Rather we are interested in considering movement between communities of scientists with divergent practices, paradigms, or habitus and the role that this intellectual movement plays in innovation within biology.

Movement between communities occurs not just between disciplines but also within them. Increasingly, recognized subdisciplines have developed almost insurmountable barriers, as specializations divide the landscape and render movement more difficult within. This is true for biology as much if not more than for physics, chemistry, and computing. For that reason, we also consider a number of examples in which researchers from one subdiscipline within biology crossed into a second subdiscipline to make contributions there. An exemplary case would be Ilya Metchnikoff moving from developmental biology to immunology, or Francois Jacob, moving from work on bacteria to mice. Such cases are similar to those of nonbiologists crossing into biology because here too, researchers bring with them completely new skills, perspectives and training. These particular outsiders we term “insider-outsiders.”

Our definition of the outsider, then, is restricted. Excluded from it are outsiders on account of religion, ethnicity, gender, and character—though for all of these, to be sure, fascinating examples abound. The sole and guiding principle for *Outsider Scientists* is that the individual in question should have moved from one intellectual community, with its distinctive practices and established conceptions, into an area of biology new to that individual. Because these migrating scholars often bring with them tools, techniques, theories, and practices, we could have chosen to follow these instrumentalities into new areas, but we chose instead to follow individuals into new communities and institutions. The biographical focus of each of the following chapters is not intended to portray scientists as lone knowers, but as members of new disciplinary communities—members who significantly alter the practices of those communities.

Making judgments as to who is an outsider and who isn’t, however, necessarily remains a complicated affair. To begin with, one needs to assume that there is an “inside” outsiders must enter, and this was not always true in biology. Lamarck may have coined the term in 1802, but biology as a coherent field and well-defined community, with institutions and academic programs, particular subdisciplines, research agenda, and journals, took time to establish, and of course remains in flux. When does one mark the inception of a field: When its name is coined? When the first society of practitioners is founded? When the subject is included as a field of study in the universities? However one approaches this problem, it is clear that the trajectory and growth of biology was unique in different historical contexts, such as in the German-, French-, and English-speaking worlds.[8]

Wary of the slipperiness—and to a degree the arbitrariness—of defining a hard and fast historical date for the birth of biology as a discipline, we have chosen to include in this volume a first section that will treat a number of early examples of interesting nineteenth-century practitioners whose engagement with problems of a living nature illustrates the very difficulty involved in speaking about "outsiders" with any confidence before the late nineteenth century. Gregor Mendel was a clergyman who had little or no formal training in anything called "biology." The worlds that he uniquely united—experimental physics gleaned at the University of Vienna, the middle-European business of practical plant and animal breeding, and the local scientific society at Brünn—gave birth to a research program that would play a crucial role in the establishment of genetics and the establishment of biology as an identifiable field years later. Mendel helps us understand, both intellectually and in terms of earlier local traditions, what the creation of an "inside" for modern biology entails. Similarly, the role of Pasteur the businessman and chemist, moving into what was rapidly becoming an institutional *biologie* in France, helps put a finger on the process of the birth of the disciplinary divides that defined a distinct *biology*, as does Felix d'Herelle's uniquely self-taught (and fascinatingly international) trajectory in microbiology. Finally, to round off the early examples, the contributions of Samuel Butler, the Victorian novelist, serve to trigger a discussion of the ways in which literary engagement with the idea of evolution challenged a number of crucial divides: the science-philosophy divide via the teleology and causality debate, and the public-private divide via the debate concerning the proper forum for negotiating scientific disputes. These four individuals play an important role in allowing us more carefully to consider the criteria for "inside" and "outside" in biology as they developed historically.

Thus, the first part of the book, "Outsiders before the Inside," treats the dichotomies of teleology–efficient causality, amateur-professional, local-international, industry-academia, and public-private, each of which played a role in the birth of modern biology. Other examples could have served us here, but we have chosen these early individuals in order to create a meaningful set of contrasting cases to later figures who were involved in the creation of innovations following explicit acts of boundary crossing into areas of modern biology.

Recognizing "outsiders" in biology becomes more straightforward as we consider the development of the biological sciences in the twentieth century, and it is this century that is the main focus of *Outsider Scientists*. Here the challenge of understanding outsiders and their innovations in biology pertains less to the ambiguity of describing an "inside" or an "outside," and more to a problem of selecting a range of both diverse and representative outsiders. There have been many practitioners in biology who can be thought of as "outsiders," and we have had to think long and hard about whom to include. We would have liked to include more than eventually made it in—there are, in other words, outsiders who "got away." One thinks in particular of Max Delbrück, Herbert Simon, Isaac Asimov, Gerald Edelman, Seymour Benzer and, reaching further back, Goethe as an early "outsider before the inside," who as an artist and morphologist attempted to reconcile his divergent pursuits. It is our hope that this particular collection will spur others to examine such figures in the mode we suggest. The figures that have made it into our book have been chosen to illustrate the myriad ways in which "outsider science" comes about and functions. For each case we have chosen expert contributors, each with a broad and deep knowledge of the relevant history and context.

Before we continue any further, we'd like to address a quick word to the skeptic. The category of the "outsider" in science, the objection might run, is too diffuse to be of any value. After all, there are many ways to be an "outsider," and the dynamic of insider-outsider interactions will necessarily take many forms. Our reply to the skeptic is meant to disarm: we agree. Our goal is not to define exhaustively what it has meant to be an "outsider" to biology. Given the shifting nature of biology as a discipline, that would be a Herculean task. But we do not shy away from this diversity. Very much to the contrary, we are consciously setting out to present it in as full a fashion as possible rather than unthinkingly "lumping" all the disparate histories into a conceptual straitjacket. Clearly, the contingencies matter, as do the myriad facets of the outsider incursions—that is the point of the historical narratives that follow. Our goal is to offer a range of historical cases that allow us to comparatively understand the elements of discipline crossing that contribute to processes of scientific innovation.

Our analysis does assume that "discipline" is a legitimate historical category. While most scholars would agree that biology is and has been a discipline, they can differ on what exactly constitutes a discipline.[9] Specialization and institutionalization through markers such as professional societies, journals, and designated funding streams have typically been recognized as elements of discipline formation. More recently, epistemic criteria of problem definition and practice have been added. Minding and maintaining the boundaries of scientific disciplines has also been the object of scholarly research, especially as biology itself has emerged as a dynamic enterprise. While contemporary biology, especially since the rise of the molecular revolution, is widely recognized as a mosaic or hybrid of many diverse subfields, in the early twentieth century there was a distinct movement to seek a unified biology. Historians, such as Betty Smocovitis, have written eloquently about the desire to articulate a common core to the discipline of biology and about the challenges to this unification. While consensus on a unifying theory, even within the so-called evolutionary synthesis, proved elusive, scientific societies were formed, journals established, and the social and cultural definition of biology was perpetuated, even if it was always in motion. The fluidity of the disciplining of biology does make it a moving target for historians. However, we do not need an entity etched in stone. We need an entity that is sufficiently different from neighboring areas of inquiry that we can say that chemistry as a discipline, for instance, differs from biology as a discipline in terms of imparting to its members distinctive concepts, theories, methods, practices, and approaches. The various sub-branches of biology will differ among themselves, but the general pattern of the whole will yet distinguish it from other major areas of inquiry, such as chemistry.

Discipline crossing draws our critical attention to forms of epistemic difference that may be rooted in the style of thought an outsider brings with her, a particular set of intellectual tools, an experimental apparatus or design, or that may involve more broadly (and deeply) a general vision or specific motivation.[10] Discipline crossing may relate to the way that the reception of outsiders is determined by sociological as opposed to intellectual reasons, and how this varies depending on the particular "outside" one is coming from. The salient objective is that the cast of "outsiders" illustrate, as a group, a wide spectrum of the different facets of the phenomena.

THE OUTSIDERS WHO MADE IT IN

To help the reader, and in order to provide an organizing framework, we have divided the book into six parts. They are 1) Outsiders before the Inside, 2) Outsiders from the Physical Sciences, 3) Outsiders from Mathematics, 4) Outsiders from the Human Sciences, 5) Insider-Outsiders, and 6) Outsiders from Informatics.

As we have mentioned, the category "Outsiders before the Inside" includes accounts of Mendel, Pasteur, d'Herelle, and Butler. The histories of these figures will introduce a perspective on innovation through the integration of diverse interests, approaches, and practices before there was a clearly demarcated discipline identified as biology. Importantly, they provide a contrast to the stories of the later periods in which disciplinary markers are more easily discerned, since those markers were more actively enforced after the turn of the twentieth century. Indeed, many of the dichotomies these early examples highlight—such as teleology vs. efficient causality, amateur vs. professional, industry vs. academia—provided the definitional distinctions that later biologists used to create and enforce disciplinary boundaries. All four cases, authored by Sander Gliboff, Jonathan Simon, William Summers, and Michael Ruse, respectively, speak to the power of movement across the intellectual terrain as a means to foster new insights.

In part 2, essays by Sahotra Sarkar, Gregory Morgan, and Hallam Stevens introduce us to a sample of the many physicists who crossed into biology in the twentieth century. Erwin Schrödinger and Linus Pauling may be familiar subjects, but Sarkar and Morgan provide careful new consideration of how these two Nobel laureates translated their knowledge of physics into biological idiom, and in so doing helped create the foundations of molecular biology. Stevens describes the work of Walter Goad, a less well-known figure, who used his understanding of computational physics acquired in atomic bomb work to reshape the genetic databases and algorithms that now form the basis of bioinformatics.

In our third part, Michael Dietrich and Robert Skipper Jr., Maya Shmailov, and Jay Odenbough each consider scientists at the interface of mathematics and biology. R. A. Fisher, Nicolas Rashevsky, and Robert MacArthur all brought mathematical and statistical insights to bear on biological phenomena in ways that transformed biological practice from its earlier naturalist tradition. The statistical tools developed by Fisher alone have become completely commonplace in all branches of biology as a result.

Part 4 considers outsiders from the human sciences, with essays by W. Tecumseh Fitch on the linguist Noam Chomsky, T. J. Horder on the philosopher David Hull, and Erika Lorraine Milam on the writer Elaine Morgan. These cases do not represent equally influential incursions into biology: Chomsky's attempt to pry open the brain by exploring the rules of grammar helped bring about a revolution in the cognitive sciences, while Hull's and Morgan's grappling with particular theories of systematics and evolution, respectively, produced more of a glancing blow toward their discipline of evolutionary biology. Still, taken together, the three examples illustrate salient features of biology's intersection with the humanities.

In part 5 we meet two "insider-outsiders": Ilya Metchnikoff and François Jacob. In their essays on these internal migrants, Fred Tauber and Michel Morange show how movement across subfield boundaries can be both difficult and transformative. Drawing from his background in embryology and development, Metchnikoff challenged the prevailing theories of immunity of his day, while Jacob took principles he had learned working on bacteria and phage in molecular biology and applied them to the mouse in the study of disease.

The final section of the book deals with the influence of informaticians on the life sciences. Chapters by Ehud Lamm on John von Neumann and Norbert Weiner, Oren Harman on George Price, and Luis Campos on Drew Endy reveal how biological systems have been reimagined in sometimes radical ways by outsiders redesigning their new disciplinary homes using the theoretical frameworks and idioms of computer science and informatics.

WHAT HAVE WE LEARNED FROM OUR OUTSIDERS?

The essays that follow shed light on three elements of the relationship that is our focus: the outsider, the process of coming in to biology, and the process of innovation. Concentrating on these three elements allows us to explore the roles of features of personalities, institutions, and prior training that have shaped the wide range of scientific novelties described in the chapters. We start with the outsider.

On the Outsider

The outsiders described here are not your typical scientists. When it comes to "outsiderness" as an aspect of character, many of these individuals reveal traits that rendered their crossing of boundaries almost natural. They are *bona fide* transgressors. They see little point in respecting conventional boundaries, either because they view them as inherently ridiculous or because they don't see them at all. A quintessential example is George Price. He was nothing if not an intellectual scavenger. Trained in nuclear chemistry, he switched from the Manhattan Project to work at Bell Labs on transistors and informatics, then to cancer research at a Minnesota hospital, then to magazine writing on current affairs, then to computer problems at IBM, then finally to mathematical evolution, all the while sending unsolicited letters to Nobel laureates that claimed breakthroughs in fields as disparate as neurophysiology and economics. Price saw problems, not disciplines, and, fueled by a cocksure attitude and dismissiveness toward convention—for better and for worse—acted accordingly. Linus Pauling shared with Price a similar disposition. Fiercely independent of mind, Pauling used the occasion of his 1954 chemistry Nobel lecture to admonish young scientists never to take anything on authority and always to think for themselves, respecting no boundaries; eight years later, he was in Stockholm again receiving a second Nobel Prize, this time for Peace. Erwin Schrödinger, too, possessed an aspect of character that made him a natural outsider: the confidence of a man who thought—together with Einstein, it must be admitted, but against the better judgment of the rest of the physics community—that the apparent paradoxes of quantum mechanics would eventually disappear. It was this confidence, no doubt, that helped to stoke his pretension to explain heredity by means of quantum mechanics when he attempted an answer to the question "What is life?" in a series of lectures delivered in 1943.

"Cocksure," "arrogant," "confident"—these are appellations we find applied again and again to our outsiders, and not by insiders alone. Nicolas Rashevky stormed into the life sciences from mathematics seeking to shake its very core ("You name it, he had a theory on it," one commentator quipped); "hot-tempered" Ilya Metchnikoff humiliated Nobel laureates in a field he had never studied but wished to transform; Drew Endy, hyperconfident and extolling a culture of "cool," sought to revolutionize biology by using engineering principles to synthesize life itself. The diminutive Elaine Morgan, Erika Milam tells us, "had sass," marshaling wit and humor to take her male-chauvinist targets to task. Earlier in the century, Felix d'Herelle, marshaled the autodidact's bold self-possession to revolutionize microbiology, and R. A. Fisher, like a terrier hound (which he incidentally resembled), showed incorrigible persistence against opponents in applying statistics to evolution and heredity. Of course, outsiders' personalities were nevertheless by no means static.

Often outsiders possessed a broad "vision" which they actively pursued: Rashevsky and Fisher and MacArthur sought to mathematize biology and Pauling sought to bring physical chemistry to biology, for example. Fisher believed that this kind of intervention from the outside was most difficult for insiders to accept: "A new subject for investigation," Dietrich and Skipper quote him as saying, "will find itself opposed by indifference, by inertia, and usually by ridicule. A new point of view, however, affecting thought on a wide range of topics may expect a much fiercer antagonism." Sometimes, as with Price, there is no more than a kind of problem-specific intellectual opportunism. Sometimes, as with Endy, perhaps both are present.

But incursions from the outside are not always the result of a particular aspect of personality; sometimes they simply describe the act of crossing an unseen, or alternatively a closely patrolled, divide to solve a particular problem. Our "Outsiders before the Inside" are examples of the former. Each, in his own way, moved from one métier to another without necessarily exercising the muscles of overbearing confidence, or expressing hatred of authority, or indulging in contempt for convention (think of the gentle curate, Mendel)—though Butler probably imagined himself a Renaissance man. Louis Pasteur, to the medical establishment, might have been insufferable, but microbiology, at any rate, had yet to define its boundaries. Walter Goad, by contrast, is a modern example of a man who didn't possess the fiery "outsider" character, but nevertheless recognized a void, entering the field, with the help of a long-standing institutional interest at Los Alamos in biology and medicine, to apply numerical data management tools to genetic databasing. It was his tool—the computer—rather than his temperament that led Goad into biology, allowing him to import ready-made ways of thinking, doing, and organizing with little resistance.

Regardless of personality, the outsider's training was always of the utmost importance. Perhaps we should not be surprised to find that no small fraction of our outsiders actually had a prior connection to biology before trying to enter the field. Drew Endy may have received a D in high school biology for failing to recite the Latin names of 200 insects, but Robert MacArthur, whose dad was a geneticist, actually got his PhD in ecology. Norbert Weiner, too, studied biology before becoming a mathematician, showing particular interest in physiology and teleology. Fisher, from the outset, had been hooked by eugenics and biometry, alongside mathematics, and Schrödinger, though most people don't know this, was an international authority on the physiology and biophysics of color vision. Still, it is the prior training in the nonbiological discipline that we are most interested in, since the training usually lays the foundation for the incursion to begin with. We'll address this particular issue when we turn to innovation, but first let's take a look at the process of the outsider moving in.

On Moving In

The process of crossing a divide entails a number of elements that we find recurring in one form or another in many of the outsiders considered here. These are the role of patrons and forward-looking funding bodies; the role of institutions; collaboration both with insiders and fellow outsiders; courting; and—closer to the content of innovation itself—processes of translation, simplification (especially with theoreticians), and sometimes popularization (especially with outsiders from the humanities). Each of these features represents an aspect of institutions and the social context that supported the outsider seeking to bring an original result, method, or perspective to the life sciences.

To begin with, a patron, it would seem, is a wonderful thing to have for an outsider. A number of our outsiders manifestly benefited from having enthusiastic supporters, though others neither sought nor were offered assistance. Perhaps the starkest example of patronage here is that of Major Leonard Darwin, Charles Darwin's fourth son and an avid eugenicist, who, from quite early on, decided that he was going to do everything he could to help advance R. A. Fisher's career. This meant helping him publish his famous 1918 paper on the correlation of relatives on the supposition of Mendelian inheritance. The paper played a historical role in wedding Mendelian genetics to Darwinian selection. It was, however, initially rejected by Fisher's great nemesis, Karl Pearson, for publication in his journal *Biometrika*. Thanks to Leonard Darwin's intervention, the paper was published in the *Transactions of the Royal Society of Edinburgh*. Moreover, Darwin arranged to have Fisher supported by monthly stipends, enabling him to develop his synthesizing insights, which culminated in what became his magnum opus, *The Genetical Theory of Natural Selection*, in 1930. George Price, too, enjoyed patronage from John Maynard Smith and Bill Hamilton, collaborators both; but their aid went beyond the usual bounds. It was Hamilton, after all, who by way of a ruse cajoled the editor of *Nature* into publishing Price's pathbreaking covariance paper, which no doubt otherwise would not have seen the light of day. Similarly, without the generosity and encouragement of John Maynard Smith, their historic joint paper, which applied game theory to animal conflict, would most probably have never been written.

Felix d'Herelle provides perhaps the starkest counterexample, a man who made it decidedly on his own. Having left school at seventeen, and working from the periphery, d'Herelle was neither a member nor even known to either the Koch or Pasteur school of microbiology, at least for quite some time. Relying on his own reason and confidence, and all the while moving from place to place (Canada to Guatemala to Mexico to Argentina to Columbia to Algeria to Tunisia to Cyprus to France, and more), he never enjoyed any form of patronage, except for a short-term commission tendered by the Argentine Minister of Agriculture to exterminate locusts in his country. D'Herelle was a lone maverick.

Individual patrons may not be necessary, but some form of support or acceptance is often crucial, such as forward-looking funding bodies. Warren Weaver of the Rockefeller Foundation is a celebrated example of a supportive administrator, one who had the foresight to offer critical aid to both Pauling and Rashevsky at Cal Tech and at the University of Chicago, respectively.[11] In both cases, Weaver saw what many who were unequivocally insiders didn't notice, that outside tools—structural chemistry and mathematics, in these cases—could go a long way to help solve important "insider" problems. Drew Endy, on the other hand, at least when he began, was rather impeded by the main funding taps: one agency threw his grant request out the window citing irrelevance, and worse, complete lack of believability.

Indeed, outsiders don't always find institutional homes that are willing to take a chance on projects that seem to many unimportant or even sinister. Pauling, however good a chemist he was known to be, ended up creating his own institute, the Institute of Orthomolecular Medicine, later renamed the Linus Pauling Institute of Science and Medicine. He used the institute to pursue his vitamin C research, which does indicate that buying a home for "free thinking" doesn't always lead to the best results. Rashevsky, too, found the going rather rough at the University of Chicago, Weaver's support notwithstanding. Moving (or rather being moved) from the Department of Psychology to the Department of Physiology and back again, he found his work continually falling between the cracks. He was too mathematical for the biologists and too biological for the physicists and mathematicians. Finally, he solved the problem by creating his own *Journal of Mathematical Biophysics*, which had more success than the Institute of Orthomolecular Medicine.

Outsider incursions always occur within some institutional context. Some outsiders were fortunate to find the "right" institution, one that provided support for newcomers, encouraged collaboration, and sought interdisciplinary connections to address biological problems. Fisher, for example, was free at the Rothamsted Experimental Station to pursue both practical and theoretical work integrating statistics, biology, and eugenics that might very well have been impossible elsewhere (including Cambridge). Price, too, years later, enjoyed the backing of a kind institution: when he walked off the street into the University College London biostatistics department with his covariance equation written on a piece of paper—the ultimate outsider act if ever there was one—he was afforded an honorary research position within the hour and summarily helped to secure a grant for further research; University College London, mind you, was a world-leading center of genetics at the time. Goad's career, too, makes the point of institutional importance. Indeed, Los Alamos's wartime successes rendered it continually crucial to national security, which meant greater latitude for senior scientists in following curiosity-driven research. As Hallam Stevens shows, the "exigencies of wartime work" also promoted interdisciplinary collaboration. This meant for Goad that he could play a leading role in convincing the National Institutes of Health to fund the GenBank project. Institutional backing, then, seems to be a relative quantity when it comes to outsiders. Some, like Fisher, Price, and Goad, were lucky to be spurred on and provided the means by their institutions; others, like Rashevsky and Pauling, fought within until they found external solutions; still others, like Butler, d'Herelle, and Morgan, didn't even try.

But if institutional support has a variable influence, collaborations, almost across the board, seem crucial. Pauling (with Alfred Mirsky, Karl Landsteiner, and then Emile Zuckerkandl), Price (with John Maynard Smith and Bill Hamilton), and Weiner (with Arturo Rosenblueth) all prove how important work with bona fide insiders possessing complementary (rather than identical) skills can be. None of these outsiders would have been able to get very far without their collaborators. Their work on protein stabilization by hydrogen bonds, antibody specificity, evolutionary molecular clocks, the evolutionary stable strategy, multilevel selection, and negative feedback, respectively, would have been the worse for it. Pauling explained the cooperative dynamic nicely: "Landsteiner would ask, 'What do these experimental observations force us to believe about the world?' and I would ask, 'What is the most simple, general, and intellectually satisfying picture of the world that encompasses these observations and is not incompatible with them?' " The collaborators' methodological departures and differences in perspective, as well as their help in the more mundane technicalities—such as learning correct notations and suitable experimental designs—proved crucial to these outsiders for solving important problems. In some cases, as in Price's, affixing one's name beside that of a well-known insider also made a great difference.

Outsiders turn to fellow outsiders for collaborations as much as they do to insiders. Pauling, for instance, worked with the biochemist Robert Corey on determining structures of amino acids and on models of protein chains; Corey was as much an outsider in his way to biochemistry as Pauling was to molecular biology. Weiner joined hands with an electrical engineer collaborator, Julian Bigelow, to work together with the insider Rosenblueth. Goad worked with the physical chemist John Camm; together they examined transport processes in biological systems on IBM 704 and 7094 machines. Francois Jacob sent his own bacteria and phage men, Hubert Condamine and Charles Babinet, to study mammalian embryology and to return to his lab; Jacob himself declined to learn from the insiders directly. Schrödinger may have wanted to shake up biologists, but it was the physicists—Seymour Benzer, Crick, George Gamow, Salvador Luria—who heeded his call more than anyone else. But Endy is perhaps the ultimate example of an outsider who knew he would need to turn to like-minded outsiders to get anywhere at all: he extended his hand to Tom Knight and his fellow electrical engineers at MIT rather than engage true insiders in biology. When he found himself at a disciplinary crossroads—should he study more molecular biology from the inside or think as an outsider engineer?—Endy determined to "screw it," since the complications and details of biology seemed "of little interest."

Courting the inside is sometimes a requisite for outsiders, even if looking at Endy's path doesn't immediately divulge this. The biological world into which both Weiner and von Neumann were attempting to enter, for example, was anything but hospitable. Ehud Lamm quotes E. B. Wilson, at the 1934 Cold Spring Harbor Symposia on Quantitative Biology, offering a number of axioms to the initiated, the first of which was "science need not be mathematical," and the second, "simply because a subject is mathematical it need not therefore be scientific." The inside, clearly, was less than inviting. Incidentally, Weiner and von Neumann ended up choosing different approaches to engaging biologists—the former seeking out collaboration, the latter going it alone.

The writers Samuel Butler and Elaine Morgan, too, understood full well that they needed to court their readership—whether by gripping drama, scathing wit, gentle humor, or all of these—and directed their talents inward as much as out. Still, popularization was an issue: the way to succeed, both authors knew, entailed capturing the hearts and minds of the public. As Michael Ruse explains, when it came to evolution, before 1859 the subject was considered a pseudoscience, after 1930 it was professionalized, and in between its status was ambiguous (though it may be objected that early figures as central as George Cuvier and Karl Ernst von Baer took evolution seriously enough to go to great lengths to dispute it, and that evolution was taught in many German universities by 1860, and even before).[12] Focusing on the English-speaking world, Ruse argues that evolution was a popular science during this period, and the popular book or novel as legitimate and influential a venue as the scientific paper. Insiders like T. H. Huxley and George Romanes grasped this, which is why they themselves attempted to speak to the public alongside their more professional writings, understanding full well, if reluctantly, that this meant the door had been pushed wide open. Butler capitalized, building a successful career as a popular writer on evolution. After him, Morgan did too, though in her day she was required to mount a tighter argument, based on a careful reading of the scientific evidence. And while Noam Chomsky may have awaited his Steven Pinker, he has nevertheless used the television to great effect in making himself known as a public intellectual, as much for his linguistic theories as for his politics.

On Innovation

Outsiders bring with them new language as well as designs. In order to express their innovative ideas, then, they must engage in a process of translation.[13] Schrödinger, for example, transported terminology such as "isomers" from organic chemistry to describe different stable states of genes, and "tunneling" from physics to speak of the process of translation between such states to help explain, among other things, mutations. "Negative entropy" was another concept he used to translate a concept from thermodynamics into one in molecular biology, a translation that may well have given birth to more confusion than clarity.

Price, too, as Harman shows, went about the business of translating concepts, in his case from Claude Shannon's channel capacity informatics to selection dynamics more broadly. It was the precision and beauty of the formalization of the theory of communication that Price sought to translate into biology. Von Neumann and Weiner were very much engaged in a kind of translation enterprise as well, using "self-reproducing automata" and "negative feedback" as central concepts otherwise unheard of within biology. And Endy fashioned repressible promoters as transistors, the biological and genetic equivalents to toggle switches and oscillators.

Translating, or rather getting insiders to understand translations, isn't always easy, as the correspondence between Joshua Lederberg and Jon von Neumann attests. Lederberg wanted to know how intracellular components correspond to the elements of the cellular automata model, but von Neumann's conceptual model had no relation to biological realities. Lionel Penrose, too, found it difficult to find answers to specific biological questions in von Neumann's model, in particular those having to do with the physical and chemical aspects of self-reproduction. For both biologists, the engagement with the mathematician proved frustrating.

Indeed, biology is a messy science, full of details and exceptions. It is for this reason that many of the outsiders coming in—in particular, theoreticians and those with mathematical and physical skills—sought to simplify matters in order, as it were, to see the forest independently of the trees. Rashevsky, to be sure, wanted to transform biology into a deductive science rather than an empirically based one, where theory based on oversimplified—often grossly oversimplified—scenarios (of cell division, growth, nerve conduction, brain function, etc.) could be used to predict trends rather than exact values, and help direct avenues of research. In looking at island biogeography, species abundance distribution, and optimal foraging strategies, MacArthur preferred "patterns" to trends. Indeed, science itself was to him essentially a matter of detecting patterns, an approach that allowed him to transform ecology from a descriptive science to a structured and predictive one. But integrating genetics, ecology, biogeography, and ethology was no small order, and by necessity it called for simplifying. Indeed, the members of the Marlboro Circle to which he belonged—Egbert Leigh, E. O. Wilson, Richard Levins, and Richard Lewontin—all agreed that only two of any three goals—generality, realism, and precision—could ever be maximized, and MacArthur preferred the first two. They called it the "simple theory" approach. "Our truth is the intersection of independent lies," was how Levins put it, with commendable honesty.

Price, too, sought to simplify by generalizing. His tautological covariance equation had exactly zero biological assumptions in it, which is precisely why it was difficult for many biologists to appreciate its import (many still don't). Endy's synthetic biology, almost by definition, was a science of standardization, hence simplification. And von Neumann, looking at self-reproduction, was interested in the internal functional organization of the system, an exercise accomplished by axiomatizing the behavior of the system's components. This again was an idealization, and hence a simplification, rather than an attempt to describe real biological phenomena.

Simplifications offered by outsiders invariably ruffle the feathers of insiders to the point of fury. Theodosius Dobzhansky was being gentle when he intoned, as Lamm quotes him: "Experience has shown that, at least in biology, generalization and integration can best be made by scientists who are also fact-gatherers, rather than by specialists in biological speculation." The future Nobel laureate neurophysiologist John Eccles was harsher, letting the community know that he thought his own field would be strictly impeded, rather than advanced, by superficial analogies to automata. And Endy's detractors so resisted the idealization behind his "Standard Parts List for Biological Circuitry" that Endy invited disgruntled biologists to send complaints to the "Office of Biological Disenchantment, MIT 68-580" (in other words, his office in the electrical engineering department). Still, despite its detractors, simplification has been a real motor for innovation.

At the base of the ability to simplify, generalize, and translate stands the particular training and intellectual territory from whence the outsider arrives. Take for example Pauling's background in crystallography, which solidified his unshakeable belief in the idea that properties of all substances depend on their structure. It was this "methodological structural reductionism," as Gregory Morgan calls it, that pushed him to seek the explanation of cellular behavior at a "deeper" level, leading to the solution of the alpha helix, among other problems. Take Weiner's work on target-tracking machines for the American Air Force during World War II: it was here that he first encountered the oscillatory movement for which he found an analogue in the intention tremors of human cerebral patients, and which, via the wedding of feedback to intentionality, became the conceptual centerpiece of the science of cybernetics. Or take Price's work at Bell Labs with Shockley and Bardeen, which familiarized him with Claude Shannon's theory of communication, a theory he then sought to apply to the process of biological selection. Or Goad's earlier work in physics on the hydrogen bomb, which, Hallam Stevens claims, is responsible for the introduction of computers into mainstream biology. Goad recognized that the kind of numerical and statistical methods he had used to solve data-intensive problems in fission and fusion could be applied, using digital electronic computers, to genetics. GenBank, the database he created, was premised on the notion that biological problems could be framed as pattern-matching and data-management problems. Ultimately, the system of sharing, communication, organizing, and distributing DNA sequence data that it produced allowed for the birth of the Human Genome Project. In all these cases, there is a direct connection, even a direct analogy, between prior work in one field and the later work in biology.

Endy, too, in calling for an "open source biology" based on "tools of mass construction," deployed an analogy between Boolean electrical gates that can perform simple operation such as AND, OR, NOT, NAND, NOR and genetic components that could be construed as "BioBricks." In so doing, he swept away with one blow the model organism approach, since, to his mind at least, standardized biological parts rendered such lab-based practices unnecessary. As Luis Campos argues, this was the ultimate outsider biology—outside of the confines of even the organism itself or the species—but it was based on a functional analogy and a confident assurance of its validity. Pasteur, too, saw an analogy between fermentation and infectious diseases, as Jonathan Simon elucidates. This vision drew a direct line for him between his chemical work, on the one hand, and his medical-biological work on the other. The Frenchman's eye for the utility of organisms oriented microbiology first to industrial production and then to the treatment of disease, two directions that would be enhanced by d'Herelle's international work in the twentieth century.

Analogy is not the only route for the introduction of a divergent disciplinary understanding into biology. Sometimes a general approach, or a particular skill, will suffice. Take, for instance, Metchnikoff. It was the Russian's embryological preoccupations and Darwinian framework that informed his challenge, both methodological and theoretical, to the prevailing immunological theories of his time. These proved of little interest to microbiologists and immunochemists, who had no background in development to speak of and who were focused on defining the mediators of immune response rather than looking at the etiology of the entire system. But Metchnikoff had the vision to perceive the connection between one set of problems—evolutionary and developmental—and the mystery of identity as presented by immunity. As Fred Tauber argues, it may very well be the case that only an outsider, aloof from the immediate concerns of the dominant scientific community, could have posed the question of identity in the face of dynamic change so starkly.

Elaine Morgan, too, crossed a divide by bringing with her something of value. In her case, it was her pen. An English major at Oxford and a mother of three, Morgan had spent a life writing screenplays and dramas for the BBC from a quiet base in Wales. But with an irascible wit and a fortunate turn of phrase, she was able to address that most basic of a readership's faculties directly: its common sense. "Learn to trust the evidence of your own senses over that of the written word," she wrote with just a pinch of disingenuousness, knowing full well that the written word was her best weapon. Indeed, Morgan could afford to give a full and forceful treatment to a theory that had been proposed thirty years earlier by an insider, Alister Hardy, who, not yet having "Sir" affixed to his forename, had been wary to publish anything that might damage his nascent scientific career. When it came to advocating the "aquatic ape" theory, Morgan, by her own admission, had "nothing to lose, no high academic position to think of." What she did have, however, was the skills of a dramaturge.

Coming from the outside in and of itself may allow for exercising greater imagination alongside, or even in some cases as an alternative to directly transporting particular tools, skills, or methods from home disciplines. Schrödinger's precise combinatorial model, Sahotra Sarkar argues, and his truly revolutionary and insightful notion of a genetic code, had less to do with his background as a physicist and more with an unrestrained inventiveness. George Price, too, in translating game theory from economics and international affairs to animal behavior, wasn't necessarily applying hard-won skills to a new setting, but rather exercising the kind of imaginative associative thinking which is so often stifled by internalist training and worldview. To be sure, outsiders have often been lambasted for being unqualified: such was Elaine Morgan's fate, as well as that of Rashevsky's, at least up to a certain point. But Samuel Butler, who had the distinction of being attacked by both T. H. Huxley and his grandson Julian, rather saw being an amateur as an advantage. (Huxley the grandson regarded Butler, alongside George Bernard Shaw, as "literary men," whose views are "based not on scientific fact and method, but on wish-fulfillment"). As Michael Ruse shows, Butler remained an outsider with respect to clubs and scientific societies and the like, but thought that being an amateur actually gave him a fresher perspective over the professionals (including and especially Darwin!).

A fresh perspective, then, may be the lot, or luck, of a different kind of professional even when he thinks of himself, or is termed, an "amateur." Prior training in a different discipline or intellectual community may afford special access to associative or imaginative thinking and the ability to analogize. Specific methods and tools from home disciplines can serve as keys with which to enter from the outside and unlock particular questions. And the fact that the outsider may have little to lose may pose as an advantage, too.

But so can something even more prosaic. When François Jacob decided that he was going to try to apply the principles he had learned from studying bacteria and the viruses that attack them to the mouse and the diseases that attack it (and other mammals), he was following a considered personal philosophy of science that regarded theories and models as nothing more than the recombination of elements present in previous theories and models. This to him was not only how "Nature the tinkerer" worked, but how science itself advances. And outsiders, he believed, are often in the best position to introduce new combinations. But, as Michel Morange aptly recognizes, this may very well be due simply to the fact that outsiders are much less conscious of obstacles, while being prone to "transgressions" because they are not at all familiar with the "rules of the game." In Jacob's case, there were massive obstacles to climb in moving from the simplest of model organisms to the most complex. Had these obstacles been known to him more precisely, they might have dissuaded him from trying. In the end, due to his work and others', the mouse became the choice model organism for studying mechanisms of disease in humans. In Schrödinger's case there were massive biochemical and molecular obstacles to scale as well, and much relevant biological knowledge remained unmastered. But as Francis Crick made clear, the main point of *What Is Life?*—that biology needs the stability of chemical bonds—"was one that only a physicist would feel it necessary to make." And, of course, it made a difference. Lack of knowledge, or naiveté, may be as important to innovation as highly specific forms of know-how and sophistication.

The Platypus

Outsiders don't always leave a mark, even when they try hard to do so. In this book David Hull is a stark example, a man as well placed as any on both sides of a disciplinary divide (in his case philosophy and systematics). Hull himself believed that a philosopher could "uncover, explicate and possibly solve problems in biological theory and methodology," but, as T. J. Horder shows, he ended up contributing more to "studying the science of science scientifically"—a title of one of his papers—than to any debate within systematics. Indeed, Hull was very much aware that the majority of scientists invariably find the work of philosophers, at least when it comes to their own field, superfluous. This is not to say that there is an unbreachable chasm between philosophy and the "hard core" of scientific practice, but that some disciplinary divides are harder to negotiate than others. Persuading biologists of the deep relevance of philosophy to their work continues to be a challenge.

What is clear, however, is the extent to which modern biology has been constituted as a pastiche, a conglomeration of different methods and tools and points of view and approaches. The term "genetics" was coined by William Bateson in a private letter in 1905, but as a glance at Gregor Mendel's story makes obvious, the modern theory of heredity had come into being as a blend between experimental physics, commercial plant breeding, botany, animal husbandry, local natural history, and even law. Microbiology, too, was later forged in a disciplinary furnace, to which chemistry, agriculture, medicine, and economics all contributed. And molecular biology was constituted like a tassel of disparate strands, as researchers from different fields led by particular problems found themselves obliged to master a host of tools and methods hitherto unlisted on the "how to" menu of the biologist.

Indeed, when Noam Chomsky challenged behaviorism, wielding the sword of generative linguistics, he was functioning as an outsider storming the gates, whereas when he championed animal behavior–influenced nativist biolinguistic theories, as W. Tecumseh Fitch shows, he was more like the insider importing "outsider" ideas. The overall result of these interventions was the refashioning of something called cognitive sciences that expanded the purview of biology. Whether or not "synthetic biology" will become a mainstay of the life sciences remains to be seen, but if Endy's gradual refashioning of the engineering vision from a revolutionary agenda into "nothing new here" is an indication of anything, it is that "biology" as a discipline is an ever-changing quantity.

The point, as Heraclitus might have appreciated, is that what is construed as "in" and "out" is and always has been in constant flux. The biological traveler can never step twice into the same river; he will find that not only have the waters changed—their color, temperature, and speed—but the banks, too, have changed, laying out a new topography. Indeed, the very organisms swimming about have morphed and been hunted and restocked from neighboring rivers. In biology—especially in biology—"outsiders" have been transformative.

In a very real sense, as Richard Lewontin stresses in the epilogue to this volume, the extreme dynamism of the life sciences problematizes the concept of biology "outside the box." Indeed, it problematizes the very notion of a "box." An example comes from a 1997 paper from the *Proceedings of the National Academy of Sciences* on resistance to phosphate insecticide in a sheep blowfly. The paper—the fruit of a collaboration between botanists, zoologists, and chemists—makes the point starkly: here, the biological effect of resistance is unrobed step by step all the way down to a single atom effect, moving the analysis from biology to chemistry to physics. Does speaking of an epistemic biological "box" help to understand how science is done in this case, Lewontin asks? Indeed, is this really an epistemic box at all? The answer, it seems, is to a great degree a reflection of the history of modern biology: the gradual erasure, made possible both by methodological and theoretical advances, of the boundary between life and non-life, as well as the growing ability to look at systems as constituted by components, amenable to mathematical and physical analysis. This is not merely, or simply, a story of reduction, but rather more accurately of accumulating more tools deemed relevant to the solution of mysteries provided by the natural world. As the tools multiply, so the epistemic "box" is enlarged to accommodate them. Yes, asking how a blowfly has become resistant to phosphate insecticide would be considered by most to fall under the purview of biology, but the way we attempt to answer such a question today as opposed to forty or sixty or one hundred years ago renders "biology" an ever-changing constant.

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Looking at the "duck-bill mole" and other oddities of Australian wildlife, Governor John Hunter offered in 1793 that "a promiscuous intercourse between the different sexes of all these different animals might account for their unlikely forms."[14] Indeed, so strange was the beast that the great German comparative anatomist Johann Blumenbach christened it in 1800 *Ornithorhynchus paradoxus*. It would take time and careful scrutiny before the evolutionary lineage of the platypus was better understood, but even today, with all that we know, it remains a wondrous vision.

And so does biology, that most hodgepodge of all sciences. Still, as we hope readers will agree, the chimeric character of biology has often served it well. The cases in *Outsider Scientists* reveal how personal features such as persistence, institutional features such as the presence of willing patrons, mentors, and collaborators, and intellectual features such as the ability to create useful analogies and translations between fields fostered and promoted innovation in biology by newcomers as they constantly shaped and reshaped its form. The outsiders profiled in these pages, never content to merely "think differently," engaged themselves in the hard work of articulating connections between ideas, practices, people, and institutions that allowed their work to get a hearing among biologists and to gain a measure of influence. The success of these outsiders speaks not merely to their persuasiveness, but to their ability to understand key features of disparate fields and then build the bridges that connect and ultimately transform them.

NOTES

1. Ann Moyal, *Platypus: The Extraordinary Story of How a Curious Creature Baffled the World* (Washington: Smithsonian Institution Press, 2001), 5.

2. Joseph Ben-David, "Role and Innovations in Science," *American Journal of Sociology* 65 (1960): 557–568; Robert Merton, "The Perspectives of Insiders and Outsiders," in *The Sociology of Science* (Chicago: University of Chicago Press, 1973), 99–136.

3. Lynn Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995).

4. Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

5. Robert Olby, *Francis Crick: Hunter of Life's Secrets* (Cold Spring Harbor: Cold Spring Harbor Press, 2009); Horace Judson, *The Eighth Day of Creation* (New York: Penguin, 1979).

6. Evelyn Fox Keller, *Making Sense of Life* (Cambridge: Harvard University Press, 2002).

7. On the connections between interdisciplinarity and innovation, see Jonathon N. Cummings and Sara Kiesler, "Collaborative Research Across Disciplinary and Organizational Boundaries," *Social Studies of Science* 35 (2005): 703–722.

8. Compare, for example, Lynn K. Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago: University of Chicago Press, 2009); and Jane Maienschein, *Transforming Traditions in American Biology, 1880–1915* (Baltimore: Johns Hopkins University Press, 1991).

9. Robert Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge: Cambridge University Press, 1986); Jean Gayon, "Is Molecular Biology a Discipline?" *History and Epistemology of Molecular Biology and Beyond*, Preprint 310 (Berlin: Max Plank Institute for the History of Science, 2006), 249–252; Alexander Powell et al. "Disciplinary Baptisms: A Comparison of the Naming Stories of Genetics, Molecular Biology, Genomics, and Systems Biology," *History and Philosophy of the Life Sciences* 29 (2007): 5–32; Evelyn Fox Keller, "Physics and the Emergence of Molecular Biology: A History of Cognitive and Political Synergy," *Journal of the History of Biology* 23 (1990): 389–409; Vassiliki B. Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," *Journal of the History of Biology* 25 (1992): 1–65; Tim Lenoir, *Instituting Science* (Stanford: Stanford University Press, 1997).

10. Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Chicago: University of Chicago Press, 1993).

11. See J. Rogers Hollingsworth's analysis of scientific discovery at the Rockefeller University in *Creating a Tradition of Biomedical Research*, ed. Darwin Stapleton (New York: Rockefeller University Press, 2004).

12. See Robert Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle Over Evolutionary Thought* (Chicago: University of Chicago Press, 2009).

13. See also Galison, *Image and Logic*, on trading zones and the creation of creoles.

14. Moyal, *Platypus*, 11.