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LORDS OF THE FLY

Drosophila Genetics and the Experimental Life

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The Nature of Experimental Life

THIS BOOK IS ABOUT THE MATERIAL CULTURE and way of life of experimental scientists. It is also about a particular and famous community of experimental biologists, the *Drosophila* geneticists, and their no less famous co-worker, the fruit fly. Few laboratory creatures have had such a spectacularly successful and productive history as *Drosophila.* It first entered laboratories about 1900, revealed its talent for experimental genetics to Thomas Hunt Morgan and his students at Columbia University in the early 1910s, and after some ups and downs in status is still going strong almost a century later. If not the first "standard" laboratory creature, *Drosophila* is certainly representative of the type. So, too, is Morgan's original fly group an archetypical experimental community, and the period of its life cycle, from about 1910 to the early 1940s, more or less demarcates the beginning and end points of this book.

A great deal has been written about the "Drosophilists," as they called themselves: in a heroic vein by the last of the founding generation and their students, about twenty years ago, and again in recent years in a more critical and revisionist vein.[1] Despite some ideological

1. Garland E. Allen, "T. H. Morgan and the emergence of a new American biology," *Quar. Rev. Biol.* 44 (1969): 168–88; Allen, "The introduction of *Drosophila* into the study of heredity and evolution, 1900–1910," *Isis* 66 (1975): 322–33; Allen, *Thomas Hunt Morgan: The Man and His Science* (Princeton: Princeton University Press, 1978); Elof A. Carlson, "The *Drosophila* group: The transition from the Mendelian unit to the individual gene," *J. Hist. Biol.* 7 (1974): 31–48; Carlson, *Genes, Radiation, and Society: The Life and Work of H. J. Muller* (Ithaca: Cornell University Press, 1981); Daniel J. Kevles, "Genetics in the United States and Great Britain, 1890–1930: A review with speculations," *Isis* 71 (1980): 441–55; Jan Sapp, "The struggle for authority in the field of heredity, 1900–1932: New perspectives on the rise of genetics," *J. Hist. Biol.* 16 (1983): 311–42; Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987); Jonathan Harwood, "National styles in science: Genetics in Germany and the United States between the world wars," *Isis* 78 (1987): 390–414; Harwood, *Styles of*

differences, these various historical schools share a common concern with the conceptual and disciplinary dynamics of genetics—how competing genetic theories and visions of the field were devised, received, and contested. The richness of this literature makes *Drosophila* genetics an inviting case on which to try out a different historical approach, one focused on practices and material culture. This is not to say that the history of ideas is unimportant, only that the history of the material and human side of experimental life can be equally productive.

The material culture of experimental scientists has been surprisingly neglected by historians. Although a substantial body of literature on instruments and laboratories is now accumulating and the subject seems certain to be popular in the 1990s, the study of the material and working cultures of experimental workplaces is still very much in its infancy. Themes and approaches are diverse, and it is not yet evident which ones will be the most fruitful.[2]

A distinctive feature of this case study is its focus on an experimental organism: *Drosophila,* that crucial bit of material culture upon which generations of geneticists have come to depend for their careers and livelihoods. Why do some organisms, like *Drosophila,* become cosmopolitan, "standard" species of laboratory creatures—cornucopias of productive methods, concepts, and problems—while others do not?

Scientific Thought: The German Genetics Community, 1900–1933 (Chicago: University of Chicago Press, 1993); and Richard M. Burian, Jean Gayon, and Doris Zallen, "The singular fate of genetics in the history of French biology, 1900–1940," *J. Hist. Biol.* 21 (1988): 357–402.

2. See especially Peter Galison, "Bubble chambers and the experimental workplace," in Peter Achinstein and Owen Hannaway, eds., *Observation, Experiment, and Hypothesis in Modern Physical Science* (Cambridge: MIT Press, 1985), pp. 309–73; Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physics* (Cambridge: Harvard University Press, 1988); and Hannaway, "Laboratory design and the aim of science: Andreas Libavius versus Tycho Brahe," *Isis* 77 (1986): 585–610. For an up-to-date bibliography see Adele Clarke and Joan Fujimura, "What tools? Which jobs? Why right?" in Clarke and Fujimura, eds., *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences* (Princeton: Princeton University Press, 1992), pp. 3–44. Some useful collections of case studies are David Gooding, Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment* (Cambridge: Cambridge University Press, 1989); Homer Le Grand, ed., *Experimental Inquiries: Historical, Philosophical, and Social Studies of Experimentation in Science* (Dordrecht: Kluwer Academic, 1990); and Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch, eds., *The Social Construction of Technological Systems* (Cambridge, Mass.: MIT Press, 1987).

Why do some modes of experimental production flourish and spread around the world while others languish in local obscurity? What qualities made the partnership between fly and fly people so effective? I will seek answers to these questions in the process of experimental production, in the biological and technological nature of *Drosophila,* and in the customs and practices of the drosophilists. I hope to persuade readers of this book that experimental sciences have been shaped by their material cultures: by the practical imperatives of choosing organisms, constructing tools, and making experiments work.

In one respect this account is somewhat at odds with what has been the main line of work on experimental practice: namely, in its lack of attention to issues of epistemology and "social construction." It is a historical fact that the first scholars to study experimental practices systematically were not historians but philosophers and sociologists, who sought in scientists' workaday behavior evidence to clinch their demolition of naturalistic epistemologies of scientific knowledge. (Scientists give facts an appearance of natural inevitability by veiling the process of their construction, so sociologists seek to deconstruct facts by unveiling that process and showing how messy and contingent it really is.)[3] However, this strong connection between relativist epistemology and studies of laboratory practice was itself historically contingent upon the peculiar ecology of science studies in the 1980s. There are other, equally good reasons to do empirical studies of experimental practices. For some, myself included, it is the production process itself that is the object of interest—the material culture, social conventions, and moral ordering of experimental production.

There are, in fact, ample precedents in the science studies literature for a realist, production-oriented treatment of experimental practices. For example, in their pioneering ethnography of laboratory life,

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3. Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills: Sage, 1979), esp. ch. 5. Latour and Woolgar pointedly dropped the word *social* from the subtitle in the second edition (Princeton: Princeton University Press, 1986). See also Karen Knorr-Cetina, "Tinkering toward success: Prelude to a theory of scientific practice," *Theory and Society* 8 (1979), 347–76; Knorr-Cetina, R. Krohn, and R. Whitley, eds., *The Social Process of Scientific Investigation* (Dordrecht: Reidel, 1984); Harry M. Collins, *Changing Order,* new ed. (Chicago: University of Chicago Press, 1992); Michael Lynch, *Art and Artifact in Laboratory Science* (London: Routledge & Kegan Paul, 1985); Trevor Pinch, *Confronting Nature* (Dordrecht: Reidel, 1986); Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984); and Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992).

Bruno Latour and Steve Woolgar make the fundamental point that scientists work neither out of pure curiosity nor to win rewards of honor and status, but rather to gain the continuing privilege of working under ideal conditions with ample material and social resources. These scholars see experimental work as driven by a cycle of investment, in which experimental work is turned into publications, the symbolic capital of which is reinvested in new machinery of production, which generates more experiments, and so on. It is the work itself that drives the work—a simple but fundamental point. Latour and Woolgar make a second fundamental point when they propose that experimentalists trust and value most highly results that they can put to use productively in their own experimental work. This pragmatic conception of credibility and truth locates the causes of scientists' behavior in the production process rather than in the realm of theoretical beliefs or professional and political ideologies.[4] These perceptions of experimental work seem to me absolutely right and fundamental, and I take them as axiomatic in this study.

Other foundational concepts of a history of scientific production have been developed by Steven Shapin and Simon Schaffer in their studies of Robert Boyle and his circle in seventeenth century England. Shapin, for example, points to the literary, material, and social "technologies" of experimental workplaces, and notes how they enable communities of practitioners to judge the credibility of experimental knowledge by rules that all agree on. The special literary conventions of scientific publication and the social customs of public witnessing and discourse are as essential for experimental production as the material technology of air pumps and other instruments. Shapin and Schaffer make the epistemological point that judgments of true knowledge and personal credibility are social conventions embedded in the material, social, and moral fabric of particular communities.[5] However, their concepts and methods are equally fruitful in understanding the pro-

4. Latour and Woolgar, *Laboratory Life,* esp. chaps. 2, 5. This model is elaborated, though without additional empirical evidence, in Bruno Latour, *Science in Action* (Cambridge: Harvard University Press, 1987). Similar insights into the nature of technical work are vividly displayed in Tracy Kidder's account of a year in the working lives of a group of computer engineers: Tracy Kidder, *The Soul of a New Machine* (Boston: Little, Brown, 1981).

5. Steven Shapin, "Pump and circumstance: Robert Boyle's literary technology," *Soc. Stud. Sci.* 14 (1984): 481–520; Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); and Shapin, "The invisible technician," *Amer. Scientist* 77 (1989): 554–63.

duction process itself: that is, the social and cultural conventions that regulate access to tools and communication networks and distribute tasks and moral authority among the different participants in experimental work. My aim in this book is to identify the material, moral, and social technologies that constituted the drosophilists' work culture. It is to show how a distinctive workplace culture arose out of the process of constructing a "standard" fly, and how this culture shaped the ways in which the fly was used.

Production-oriented concepts have also been used to good effect by sociologists of occupations and work, most notably Adele Clarke, Joan Fujimura, and Susan Star. Their theoretical interest in the constitution of working "social worlds" led them, via a different route from the sociologists of knowledge, to a similar quest for the essential elements of scientific production. Fujimura, for example, has pointed to the importance of doability in experimental practice: all else being equal, experimenters will usually choose problems that are likely to produce significant, usable results. Clarke and others have pointed to the complex organization of modern research and to the importance of managerial and organizational workers in integrating benchwork, fundraising, programmatics, public relations, and so on.[6] These general propositions about the practicalities of doing science have been well grounded in theories of group behavior and have proven their worth in crafting historical case studies. I have appropriated methods and insights freely from these various schools of science studies—though perhaps not always in ways that will please them.

How, then, do we look at the material culture of standard organisms like *Drosophila* and the work cultures of communities like Morgan's fly group? I look at them in three distinct but complementary ways: technologically, biologically, and morally. First, experimental organisms can be understood as technological artifacts that are constructed and embedded in complex material and social systems of pro-

6. Adele Clarke and Elihu Gerson, "Symbolic interactionism in science studies," in Howard S. Becker and Michael McCall, eds., *Symbolic Interactionism and Cultural Studies* (Chicago: University of Chicago Press 1990, 170–214; Joan Fujimura, "Constructing double problems in cancer research: Articulating alignment," *Soc. Stud. Sci.* 17 (1987): 257–93; Fujimura, "The molecular biology bandwagon in cancer research: Where social worlds meet," *Social Problems* 35 (1988): 261–83; Susan L. Star and James Griesemer, "Institutional ecology, 'translations,' and boundary objects: Amateurs and professionals in Berkeley's Museum of Vertebrate Zoology," *Soc. Stud. Sci.* 19 (1989): 387–420; and Star, *Regions of the Mind: Brain Research and the Quest for Scientific Certainty* (Palo Alto: Stanford University Press, 1989).

duction, much as machines are embedded in systems of material mass production. Second, these living instruments also have a biology and a natural history. In the wild they inhabit distinctive ecological niches and have varied relationships with other creatures, including humankind. In the laboratory they enter into yet another kind of symbiotic relationship with humans, as participants in experiments. Experimental workplaces possess distinctive natural histories—like any habitat, wild or domesticated. Finally, experimental groups possess distinctive "moral economies" that regulate authority relations and access to the means of production and rewards for achievement. These moral rules operate within and between groups or practitioners and may perhaps be seen as the operating rules—the cultural software, so to speak—of Latour and Woolgar's reinvestment cycle.

Some readers may find it jarring to think of experimental creatures as technological and biological at the same time. But there is no real incongruity. The workplaces and material cultures of experimental biologists are simultaneously technological, biological, and moral, as are households, farms, forests, or any places where people live and work. These aspects of the work of experimental biologists are not independent but interactive, and they must be brought together to explain how the work of experimental science is done.

Organisms as Technology

What exactly do we mean when we speak of experimental creatures as instruments and technological artifacts? They are not literally machines, of course, but neither is the resemblance merely a matter of analogy or metaphor. Experimental creatures are a special kind of technology in that they are altered environmentally or physically to do things that humans value but that they might not have done in nature. Some are dramatically designed and constructed: the "standard" organisms—*Drosophila,* white mice and rats, maize, *E. coli* or *Neurospora*—which have been reconstructed genetically through generations of selection and inbreeding into creatures whose genetic makeup and behavior are quite different from their natural ancestors'. These are the constructed creatures that most resemble spectrophotometers, bubble chambers, ultracentrifuges, and other physical instruments. The extent of construction varies a good deal, however. With "found" objects like sea urchin eggs, frogs, or primates (including humans), the artifice resides less in physical reconstruction than in the accretion around these creatures of bodies of knowledge about how they behave and how

they can be made to do useful tricks in experimental laboratories. The majority of laboratory creatures are, I suspect, technologies in this more limited sense, though we have as yet little basis for generalizing. The transformation of "natural" creatures begins when they enter into their first experiment, and the more productive they become, the more they come to resemble instruments, embodying layers of accumulated craft knowledge and skills, tinkered into new forms to serve the peculiar purposes of experimental life.

What models exist for studying experimental creatures in this way? There are as yet very few studies of experimental creatures as constructed artifacts, apart from this one and Bonnie Clause's history of the standard Wistar rat. Bruno Latour has been urging scholars for some years to regard creatures (and inanimate things) as the equals of human actors, but he does not actually deal with the biology of viruses in his study of Pasteur.[7] Historians of biology may, however, draw on the richer literature on chemical and physical instruments for useful models for similar treatments of the material culture of experimental biology. Case studies from the material culture of physical science and technology demonstrate, for example, how the agendas and social relations of working groups become embodied in the machinery of experimental production.[8] Experimental plants and animals, too, have pro-

7. Bonnie Clause, "The Wistar rat as a right choice: Establishing mammalian standards and the ideal of a standardized mammal," *J. Hist. Biol.* 26 (1993): 329–49; Bruno Latour, "Give me a laboratory and I will raise the world," in Karin Knorr-Cetina and Michael Mulkay, eds., *Science Observed* (London: Sage, 1983), pp. 141–70; and Latour, *The Pasteurization of France* (Cambridge: Harvard University Press, 1988). See more generally Adele E. Clarke, "Research materials and reproductive science in the United States, 1910–1940," in Gerald Geison, ed., *Physiology in the American Context 1850–1940* (Bethesda, Md.: American Physiological Society, 1987), pp. 323–50; and Clarke and Fujimura, eds., *Right Tools for the Job.* For a cultural-studies approach to experimental creatures see Donna Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989); and Michael Lynch, "Sacrifice and the transformation of the animal body into a scientific object: Laboratory culture and ritual practice in the neurosciences," *Soc. Stud. Sci.* 18 (1988): 265–89.

8. John Lankford, "Amateurs versus professionals: The controversy over telescope size in late Victorian science," *Isis* 72 (1981): 11–28; Joel D. Howell, "Early perceptions of the electrocardiogram: From arrythmia to infarction," *Bull. Hist. Med.* 58 (1984): 83–98; Galison, "Bubble chambers"; Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); James R. Wright, Jr., "The development of the frozen section technique, the evolution of surgical biopsy, and the origins of surgical pathology," *Bull. Hist. Med.* 59

grams and agendas built into them. Machines have "politics," and so, too, do standard experimental creatures.[9]

Instruments produce nothing by themselves, of course, but only as parts of complex material and social systems of production that enable experimenters to mobilize material resources, socialize recruits, and persuade other workers to accept their results and adopt their methods of production. Historians of technology have been especially alert to the complexity of material production, and historians of biology may find useful models in that work for their own studies of experimental production.[10] A bottle of flies is not of much use for experimental production in itself, but only as part of an assemblage of material instruments, standard recipes and procedures, and working relationships. We need, therefore, to take as our units of study those systems of material, literary, and social technologies in which working groups make substantial investments and which have the property of expanding and diversifying into many lines of work over a period of time. These are the major systems of material culture that constitute the world of experimental science.

(1985): 295–326; David Gooding, "'In nature's school': Faraday as an experimentalist," in Gooding and Frank A. J. L. James, eds., *Faraday Rediscovered* (London: Macmillan, 1985), pp. 105–35; Boelie Elzen, "Two ultracentrifuges: A comparative study of the social construction of artifacts," *Soc. Stud. Sci.* 16 (1986): 621–62; Bruce Hunt, "Experimenting on the ether: Oliver Lodge and the great whirling machine," *Hist. Stud. Phys. Sci.* 16 (1986): 111–34; Timothy Lenoir, "Models and instruments in the development of electrophysiology, 1845–1912," *Hist. Stud. Phys. Sci.* 17 (1987): 1–54; Isobel Falconer, "J. J. Thomson's work on positive rays, 1906–1914," *Hist. Stud. Phys. Sci.* 18 (1988): 267–310; and Robert G. Frank, Jr., "The telltale heart: Physiological instruments, graphic methods, and clinical hopes, 1854–1914," in William Coleman and Frederic L. Holmes, eds., *The Investigative Enterprise: Experimental Physiology in Nineteenth-Century Medicine* (Berkeley: University of California Press, 1988), pp. 211–90.

9. Langdon Winner, "Do artifacts have politics?" *Daedalus* 109 (1980): 121–36; and Susan E. Lederer, "Political animals: The shaping of biomedical research literature in twentieth-century America," *Isis* 83 (1992): 61–79.

10. Thomas P. Hughes, "The evolution of large technological systems," in Bijker et al., *Social Construction of Technological Systems,* pp. 51–82; and Hughes, *Networks of Power: Electrification in Western Society, 1880–1930* (Baltimore: Johns Hopkins University Press, 1983), chap. 1. Latour and Woolgar use the term *culture* in much the way that Hughes and others use *system:* see *Laboratory Life,* pp. 54–55.

Organisms and Natural History

The biology and natural history of experimental creatures is no less important for understanding their modes of life than their history as technology. Experimental plants and animals get into laboratories from nature or, more commonly, from a semidomesticated "second nature" that they already share with humans.[11] Many creatures were already more or less domesticated upon their arrival in laboratories, having served as agricultural producers, civic decoration, or household pets (e.g., peas, primroses, fowl, guinea pigs, dogs). Others, such as mice, rats, weeds, and fruit flies, lived as half-wild commensals in a close but irregular relationship with humankind, as hangers-on in homes, gardens, and city streets—just beyond the thresholds of experimental labs. Still other creatures moved to and fro between domesticated and commensal lives (e.g., pigeons, cats, fish).[12]

It is useful to regard the construction of standard laboratory creatures as a special kind of domestication. The resemblance is obvious between standard laboratory creatures and the highly engineered, standard biological machines of large-scale agricultural technology, such as staple grains and fowl. But an understanding of the biology of domestication also illuminates how creatures get into labs unintentionally, before their potential as technology is recognized. A leading study is David Rindos's application of evolutionary theory to the early, preagricultural stages of domestication. He argues that plants and animals coevolved with hunting-and-gathering peoples, developing the characteristics of morphology and dispersal that were later and deliberately exploited in the domestication process. The establishment of relations between creatures and humankind was not at first intentional, Rindos argues, but the result of automatic biological and evolutionary processes.[13] In the same way, experimental biologists and their creatures

11. For contemporary uses of "second nature" see Donald Worster, "Transformations of the earth: Toward an agroecological perspective in history," *J. Amer. Hist.* 76 (1990): 1087–1106, on p. 1089; and William Cronon, *Nature's Metropolis: Chicago and the Great West* (New York: Norton, 1991), p. xvii. The phrase was apparently coined by Marx.

12. Harriet Ritvo, *The Animal Estate: The English and Other Creatures in the Victorian Age* (Cambridge: Harvard University Press, 1987); and James Serpell, *In the Company of Animals* (Oxford: Basil Blackwell, 1986).

13. David Rindos, *The Origins of Agriculture: An Evolutionary Approach* (New York: Academic Press, 1984). A readable popular treatment of Rindos's ideas

may enter into relationships that cause the behavior of experimenters and the biological nature of plants and animals to change in unintended and unexpected ways—as we will see. In a few cases, as with *Drosophila,* mutual dependence may evolve into the more intense relationship characteristic of standard organisms and experimental mass production.

In this view laboratories and landscapes are not such different places as may appear, though it is conventional wisdom to distinguish sharply between nature and artifice. Laboratories of experimental biology are a distinctive kind of ecosystem, in which creatures live and evolve in symbiotic and commensal relations with humankind. They have natural histories, no less than fields and forests do, and the boundaries between lab and field are active places that may be traversed or occupied in a variety of imaginative ways. Experiments may be carried out in nature but with laboratory instruments and methods (as in, e.g., ecology, astrophysics, anthropology), or in laboratories with material and methods drawn from fieldwork (e.g., population genetics and animal behavior). Simulations of nature may be staged in the laboratory (e.g., cloud chambers, aquaria, insect colonies). Bits of nature may be turned into something like the controlled environment of the laboratory (e.g., quadrats, monkey islands).[14] There is no hard and fast distinction between domesticated and wild places. The boundary zones between lab and field resemble the edge habitats that we humans create so abundantly wherever we go in the natural world.

is Stephen Budiansky, *The Covenant of the Wild: Why Animals Chose Domestication* (New York: William Morrow, 1992).

14. Ronald C. Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895–1955* (Berkeley: University of California Press, 1981), chap. 3; Joel B. Hagen, "Experimentalists and naturalists in twentieth-century botany: Experimental taxonomy," *J. Hist. Biol.* 17 (1984): 249–70; Christopher Hamlin, "Robert Warington and the moral economy of the aquarium," *J. Hist. Biol* 19 (1986): 131–53; Peter Galison and Alexi Assmus, "Artificial clouds, real particles," in Gooding, Pinch, and Schaffer, eds., *Uses of Experiments,* pp. 225–74; James A. Secord, "Extraordinary experiment: Electricity and the creation of life in Victorian England," ibid., pp. 357–84; Alex Soojung-Kim Pang, "Spheres of Interest: Imperialism, Culture, and Practice in British Social Eclipse Expeditions, 1860–1914," Ph.D. diss., University of Pennsylvania, 1991, chaps. 4, 5; Pang, "The social event of the season: Eclipse expeditions and Victorian culture," *Isis* 83 (1993): 252–77; and Henrika Kuklick, *The Savage Within: The Social History of British Anthropology, 1885–1945* (Cambridge: Cambridge University Press, 1992), pp. 133–49.

Geographers and ecological historians have been epecially vigorous in blurring the distinction between nature and technology. They have shown how many of the landscapes that may seem natural to us were in fact artificially created by humankind with their technologies of fire, plow, and seed.[15] Conversely, they have shown how landscapes that may seem thoroughly tamed and domesticated are nonetheless natural ecosystems. Donald Worster, for example, treats different kinds of agricultural landscape as modes of technological production in his study of the Dust Bowl of the American Southwest. William Cronon similarly writes ecologically about domestic, urban, and technological environments in his remarkable study of Chicago and its connections, via the traffic in agricultural and forest commodities, to the landscapes of the upper Middle West.[16] Landscapes are technologies; technological workplaces have natural histories. Laboratories of experimental biology have that same dual character.

The Moral Economy of Laboratories

Experimental laboratories are places not only of material and social order, but also of moral order, and the moral economy of laboratory life is an equally essential part of experimental production. The term is, of course, borrowed from E. P. Thompson's celebrated 1971 essay on eighteenth-century English bread riots. Thompson meant by *moral economy* the customs, traditions, and moral rules that consumers (especially poor consumers) expected would regulate the market for basic foodstuffs and, in times of dearth, prevent landowners and traders from withholding the essentials of life for the sake of profit. (It was these unstated rules, Thompson argued, that structured behavior in

15. Carl Sauer, "Man's dominance by use of fire," in *Selected Essays 1963–1975* (Berkeley: Turtle Island Foundation, 1981), pp. 129–56; William Cronon, *Changes in the Land: Indians, Colonists, and the Ecology of New England* (New York: Hill & Wang, 1983); and Timothy Silver, *A New Face on the Countryside: Indians, Colonists, and Slaves in South Atlantic Forests, 1500–1800* (Cambridge: Cambridge University Press, 1990).

16. Donald Worster, *Dust Bowl: The Southern Plains in the 1930s* (New York: Oxford University Press, 1979); Worster, "Transformations of the earth"; Worster, "Seeing beyond culture," *J. Amer. Hist.* 76 (1990): 1142–47; William Cronon, "Modes of prophecy and production: Placing nature in history," ibid., pp. 1122–31; Cronon, "A place for stories: Nature, history, and narrative," ibid., 78 (1992): 1347–76; and Cronon, *Nature's Metropolis.*

bread riots).[17] In an extended usage Thompson's concept has proved widely useful to scholars of various kinds. James Scott, most notably, has used the idea to explain the surprisingly peaceable relations between peasant farmers and landowners in South Asia. Although Scott locates moral economy in production and not marketing, as Thompson does, they both deal with nonmonetary obligations and rights of access to the necessities of life, and that makes Scott's a legitimate extension, in Thompson's view.[18]

The concept of moral economy may be similarly extended to the productive life of experimental workplaces. There, too, unstated moral rules define the mutual expectations and obligations of the various participants in the production process: principal scientists and their assistants, mentors and students, well-placed and peripheral producers—researchers who may be collaborators one day and competitors the next. Moral conventions regulate access to tools of the trade and the distribution of credit and rewards for achievement. As the moral economy of eighteenth-century English laborers was rooted in a concrete, historical system of agricultural production and marketing, so are the moral economies of experimental scientists rooted in specific configurations of material, literary, and social technology.

It may seem odd to some, even offensive, to apply to elite, middle-class practitioners a concept that was invented to apply to poor laborers and peasants. Yet if social inequalities are less pronounced and less jarring in experimental laboratories, they do exist, and issues of access and equity are no less real and emotional issues to middle-class professionals than to laborers. The extension therefore seems justified. Indeed, scientific work seems especially well suited to moral analysis, because its reward are not cash (for academic scientists, at least) but authority and access to tools and craft knowledge—the symbolic capital of Latour and Woolgar's credit cycle.

Few historians of science have as yet used the idea of moral economy in a systematic way (though the term is beginning to be fashionable). The outstanding exception is Steven Shapin's work on the moral economy of experimental workplaces in seventeenth-century England.[19] Shapin shows how social identities and class relations within these early laboratories were appropriated from the social conventions of an urban gentry, and how these borrowed conventions became permanently embedded in the culture of experimental science. As Thompson locates the moral economy of the crowd in everyday marketing practices, so Shapin finds the moral economy of experimental scientists in the social practices of gentlemen's houses, where the first experimental laboratories were in fact located. Likewise, we will see the moral economy of drosophilists in the places where they work and where their standard laboratory creatures live. We will seek the drosophilists' moral economy in their conventions of recruitment and socialization, collaborative habits of working and publishing, and their distinctive custom of freely exchanging *Drosophila* stocks—the basis of their communal identity and livelihood.

17. E. P. Thompson, "The moral economy of the English crowd in the eighteenth century," *Past and Present* 50 (1971): 76–136, rpt. in Thompson, *Customs in Common* (New York: New Press, 1991), pp. 185–258; and Thompson, "The moral economy reviewed," ibid., pp. 259–315. Thompson contrasts this moral economy with the deregulated, "de-moralized" political economy of free-market capitalism.

18. James C. Scott, *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia* (New Haven: Yale University Press, 1976); and Thompson, "Moral economy reviewed," pp. 340–49. Thompson warns against stretching moral economy to include all social values or attitudes, lest the concept lose its usefulness as a specific analytical tool: ibid., pp. 338–39.

A Social History of Scientists

This book is not a systematic history of *Drosophila* genetics. Indeed, I have done my best to prevent it from growing into one. Rather, it is a thematic study of aspects of drosophilists' working customs that I found relevant to the study of experimental science in general. In selecting topics I have followed the fly, beginning with the story of how *Drosophila* found its way into laboratories and how this wild, highly variable creature was constructed into a standard instrument that could be used for precise, quantitative genetic mapping (chapters 2 and 3). I then turn to the construction around the Morgan group's mapping project of a distinctive human community, the fly group, and of an extended network of fly people who were connected by means of a system of exchanging standard stocks and craft knowledge (chapters 4 and 5).

The emphasis in these first chapters is on the biological and ecological nature of *Drosophila* and on the construction around the stan-

19. Steven Shapin, "The house of experiment in seventeenth-century England," *Isis*, 79 (1988): 373–404; Shapin, "'A scholar and a gentleman': The problematic identity of the scientific practitioner in early modern England," *Hist. Sci.* 29 (1991): 279–327; Shapin, "Who was Robert Hooke?" in Michael Hunter and Simon Schaffer, eds., *Robert Hooke: New Studies* (Woodbridge, U.K.: Boydell Press, 1989), pp. 253–86; and Adi Ophir and Steven Shapin, "The place of knowledge: A methodological survey," *Sci. in Context* 4 (1991): 3–21.

dard fly of a material culture of experiment. A second focus is the creation of the drosophilists' distinctive way of life and the rules of etiquette that tempered generational tensions within the Morgan group and regulated the communal use of standard tools. It is in the exchange network, especially, that the drosophilists' distinctive customs and moral economy are most clearly expressed.

In part 2 I turn again to the standard fly, to examine how the capacities for genetic mapping that were built into it in the 1910s hampered efforts in the 1920s to use it to explore the genetics of development and evolution (chapter 6). Finally, I relate how in the 1930s new experimental systems were invented to reunite classical genetics with development and evolution, by applying to *Drosophila* methods from other disciplines, such as embryology, biochemistry, and natural history (chapters 7 and 8).

These topics do not exhaust the possibilities of a history of experimental organisms and practices, but they do apply generally to other creatures and other disciplines. All laboratory creatures must somehow get across the threshold between nature and lab, and many are constructed once they get there. All standard organisms are associated with complex systems of centers and networks for dispersing standard tools. That is what *standard* means: the things that everyone uses. And the specialized purposes built into standard organisms almost inevitably become confining as research fashions change. Thus, *Drosophila* and the drosophilists are a special case but also, I hope, an exemplary one for historians of biology.

There are real advantages in taking as a unit of historical study those who share a particular organism, rather than those who share a theory, problem, or discipline. I hope that these advantages will become evident to readers. But there are disadvantages, too, which should be pointed out. Following the fly makes it more difficult, for example, to study patterns of competition and emulation among geneticists who work on different organisms—a most interesting problem, which I had reluctantly to set aside for another day. Following the fly also led me away from analyzing the diffusion of *Drosophila* production to secondary and tertiary institutions, since the limiting factors for building programs were not access to standard tools but patronage and local institutional politics. For the same reason I set aside a planned chapter on the experiences of peripheral and underprivileged drosophilists, when it became clear that their disabilities were institutional and not specific to their material culture or moral economy. Those who wish to do intellectual or institutional history may not want to follow flies, mice, or maize, but rather theories, problems, or money.

One final caveat: readers familiar with the history of genetics will note the absence here of any systematic discussion of the major concepts and discoveries of classical *Drosophila* genetics. I go into considerable technical detail about the production process—instruments, procedures, strategies—but not about the products of research. This will disappoint some readers; however, there are excellent books on the intellectual history of genetics, to which they may turn. My aim here is to reveal the nature of experimental work and life to readers who may not be passionately interested in the history of genetics as such.

Indeed, one potential benefit of studying the material culture of scientific work is that this aspect of science may be more accessible to nonspecialists than scientific knowledge. Historians can explain in detail how experimental production works without getting entangled in technicalities. That is because the processes of experimental production are fewer and simpler than their products. Histories of experimental practices and lifestyles may enable historians of science to win a wider audience for their work outside their own speciality without letting go of their special subjects, namely, scientists and their work. The natural history, material culture, and moral economy of science offer the prospect of a general history of science that is not Balkanized by disciplinary specialization.

I am not the only one to entertain such hopes. David Miller recently observed that experimental practices are an ideal subject matter for a newly capacious mode of institutional history that can deal equally well with social organization and intellectual creativity:

> It is not stretching usage too far to describe "experiment" as a scientific institution. The creation and development of the experimental form of life is one of the most central institutional developments in modern science. Recent work, with its focus on the generation of the material, literary and social technologies involved in the experimental form of life, actually provides just that integration of the so-called "life of the mind" and institutional and organizational history which . . . [many historians of science] so much want to see.[20]

This study of *Drosophila* and the drosophilists should be read as an experiment in the kind of history for which Miller calls: a material, cultural, and social history of scientists at work.

20. David P. Miller, "Values redivivus?" *Soc. Stud. Sci.* 22 (1992): 419–27, on p. 424.