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Moral Economy, Material Culture, and Community in *Drosophila* Genetics

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INTRODUCTION

How does science work—and why? How do scientists go about their business of creating knowledge of the natural world, and what has made them so good at what they do? Historians and sociologists of science have been asking themselves these questions more and more insistently since the late 1970s, and today interest in scientific work and practice is as central to science studies as the logical analysis of theories was just twenty years ago.

Interest in scientists' practices takes varied forms, but a few key concerns stand out. Material culture is one—instruments and experimental procedures, the tools and methods of knowledge production. Another is the social organization of working communities and their moral economy, that is, the social rules and customs that regulate such crucial aspects of community life as access to workplaces and tools of production, authority over research agendas, and allocation of credit for achievement. That material culture and social customs are closely related is obvious and well documented. Tools and methods only become productive when they are part of a social system for socializing recruits, identifying doable and productive problems, mobilizing resources, and spreading the word of achievements. The question, with any particular community of practitioners, is how exactly instruments, practices, and moral economy operate together to make a line of work that is productive and attracts recruits and granting agencies, or (more commonly) to make one that breaks no new ground and remains small and local.

This case study deals with one of the great success stories of modern biology, the community of *Drosophila* geneticists created around 1910 by Thomas Hunt Morgan and led by him at Columbia University until 1928 and then at Caltech until his death in 1945.[1] In Morgan's group—the "fly group"—we see the several elements of modern practice in striking form. There was a novel mode of practice: the study of how genes segregate in crosses between different mutant forms; and the construction of chromosomal maps. This mode of genetic practice, which we now take for granted simply as "modern" genetics, was novel and controversial when Morgan and three of his students—Alfred Sturtevant, Calvin Bridges, and Hermann Muller—invented it between 1910 and 1912. Unlike all other contemporaneous modes of experimental heredity, genetic mapping cut heredity loose from development and evolution to focus on the mechanics of chromosomes in genetic transmission, a narrower but more doable and productive mode of practice.

This new mode of genetic practice was made possible by the invention of a novel kind of scientific instrument, the "standard" organism—in this case, the standard fruit fly, *Drosophila melanogaster.* The standard fly and genetic mapping were inseparable: the fly was created in the course of the mapping project that Sturtevant and Bridges began on May 5, 1912, and genetic mapping would have been impossible without a standardized organism tailor-made for that purpose. Since the fly group's work, biologists have created many more such organisms—maize, bacteria, bacteriophage, mice, white rats, and more recently zebra and puffer fish, the nematode *Caenorhabditis elegans, Arabidopsis thallina* (to its devotees, a plant *Drosophila*), and so on. Many others were tried but failed, like the brine shrimp *Gammarus* (which Julian Huxley hoped would become a "British drosophila"), *Planaria* (which Morgan's American opponents hoped would put *Drosophila* out of business), grasshoppers, fungus gnats (*Sciara*), and more.

It may seem counterintuitive, even perverse, to regard living creatures as bits of technology and lump them with physical instruments such as galvanometers or reactors, but in fact they are constructed artifacts distinct in crucial ways from their wild ancestors. (Ecologically, for example, they survive only in the artificial environment of a laboratory.) Standard organisms have become such a common laboratory fixture that we tend to take them for granted and forget the peculiar circumstances that caused *Drosophila,* the pioneer and prototype, to be invented as a dissertation project of a couple of college seniors.

The human inhabitants of laboratories form no less distinctive cultures, and no community was more distinctive in its early years than the fly group. It was remarkable, for example, for its custom of sharing tools (mutant stocks) and research problems. Every member of the group got involved in everyone else's projects, and every publication was more or less a group product. It was hard in some cases to know who had done what, yet the fly group was singularly free of fights over credit—a remarkable social feat. The same was true of students: though officially Morgan's, they were in fact as much Sturtevant's or Bridges's—communal products. Competing for acolytes, so common in academic life, was virtually absent. Another distinctive fly-group custom was their readiness to let other drosophilists—as the fly people called themselves—borrow their mutant stocks. The fly group was the center of an elaborate exchange system for sharing stocks and craft knowledge. Although other biologists at the time also shared this custom (most notably natural historians), none developed it so elaborately as the fly group, and though exchange systems have since been generally adopted by biologists working with other standard organisms, drosophilists even today are noted among biologists for their civility and cooperative spirit—almost a century after the founding of the first fly group! The "moral economies" of working communities can be remarkably robust.

What were these tacit rules that guided the communal life of the fly people, and how did they originate? Answering such questions is the task of science studies. I will argue here that the drosophilists' moral economy evolved spontaneously in the early mapping project, to take advantage of the remarkable abundance of mutants and research problems produced by that tiny marvel, the standard fly, when it was used for genetic mapping. The mapping project was a cornucopia, each completed piece of work producing far more enticing problems than a few people could ever work up. This abundance almost *drove* the first drosophilists to adopt customs of sharing and free exchange. Standard fly and map, moral economy, and exchange network arose together—produced each other, one could say: the material, social, and moral aspects of a remarkable machine for producing genetic knowledge.

How, then, did this machine come to be made?

GENETIC MAPPING AND THE CONSTRUCTION OF *DROSOPHILA*

There is a standard story of the invention of *Drosophila* genetics, which has been passed down from generation to generation of beginning biology students, like a myth of tribal heroes. The story is that Morgan brought wild *D. melanogaster* into the lab because it was better suited to Mendelian genetics than the more usual lab animals (mice, peas, snapdragons), with its short life cycle (as short as ten days), large numbers of offspring (up to one thousand), and tolerance of life in the lab, with its regular doses of X rays and ether and constant handling by humans. It is a tidy, rational story, pedagogically useful for socializing students; but it is not a true account of the mess of real research. The fly's advantages were real enough, but they were not why Morgan initially took up the fly.

In fact, the advantages of *Drosophila* for genetic work came to light unexpectedly as Morgan was using it in an experiment on evolution. Morgan was testing the idea that a wild creature, subjected to intense selection, would enter what Hugo De Vries called a "mutating period," that is, it would begin to vary beyond the usual range of variation. Why Morgan liked that odd (to our minds) idea is a long story, but in the course of the experiment Morgan observed a number of mutant forms, the most striking of which (though not the first) was the famous white-eyed mutant. It was not the kind of variation that Morgan had hoped to find (he doubted such extreme mutants had any role in evolution), but when the white-eyed mutation proved to be sex-linked (only males show the trait) and to segregate in Mendelian ratios, Morgan was forced to admit that genetic factors, or "genes," were physically located on specific chromosomes. And when a dozen or so more such mutants turned up in the following months, he realized that he had stumbled upon an animal that was far better for Mendelian genetics than any mouse or pea, with their limited number of Mendelizing traits. Soon Morgan had given up work on "mutating periods" and other projects and devoted his lab almost entirely to Mendelian genetics and *Drosophila*. Other experimental organisms were dropped. The fly took over.

The fly's ecological takeover of Morgan's lab was a major change in the insect's fortunes. *Drosophila* had been around laboratories for about a decade by then, but it had never been used for anything very important. Morgan had used it for student projects on chemical mutation and Lamarckian inheritance (all with negative results), and William Castle at Harvard had used it as a control for his experiments with rodents, to fend off possible criticism that inbreeding lowered reproductive vitality (again, negative). Other biologists found *Drosophila* handy for class demonstrations of tropisms and metamorphosis. In fact, there is evidence that *Drosophila* was first brought into laboratories primarily because it was ideally suited to student projects: it was abundant in the fall in gardens and orchards and active through the winter, when live material was needed, and cheap and easily replaced when inexperienced students killed off cultures. Plants and rodents, in contrast, were seasonal or expensive to maintain, subject to blights and epidemics, and not forgiving of student mistakes. *Drosophila* was useful for student work, but for that very reason its status as an instrument of research was decidedly low. That is, until it was serendipitously taken up in genetic experiments.

This odd story, so different from the founder myth, points to a crucial question about the relation of nature and experimental practice. If a freshet of mutant flies was what caused this dramatic change in the uses of *Drosophila*, then what caused these mutants suddenly to appear when and where they did, in Morgan's lab between January and May 1910? Why not earlier, or later? Or never? Why not somewhere else, say, in Castle's experiments at Harvard? I believe that mutants appeared as a consequence of scaling up the size of experiments. Drosophilas, like all creatures, will produce visible mutations at a definite but low rate of, say, a few per hundred thousand; therefore if experiments involve large numbers of flies, eventually enough will turn up to be noticed, especially if experimenters are on the lookout for them.

So the question is, what particular experiments circa 1910 were big enough for this statistical threshold to be crossed? Not too many, it turns out: certainly not Morgan's experiments on chemical mutation, which involved at most hundreds of flies; and probably not his students' experiments on generations of flies bred and raised in the dark (in the hope that they might, like cave animals, become eyeless). However, Morgan's search for a "mutating period," which went on without success for some two years—he complained bitterly to a colleague about that in January 1910—certainly involved a sufficient number, and sure enough, mutants began to appear in them. And Morgan's early experiments on neo-Mendelian segregation were even bigger, some requiring him to breed and scrutinize over a hundred thousand flies. It was in the course of these huge experiments that the freshet of mutant flies became a flood. Castle's experiments were probably also big enough to turn up mutants. Since he was interested only in the ability of inbred flies to reproduce, however, he counted only pupae (to save time) and did not inspect large numbers of adults, thus missing the chance to discover mutants. Thus it happened that fly genetics was invented at Columbia University, by Morgan and his students, and not in the other laboratories where *Drosophila* was also being used.

But it was not enough for mutants to exist for them to be seen: human experimenters had to be able to perceive them. Once Morgan had found a few especially striking mutants like the white-eyed fly, he was then predisposed to see more. What previously he dismissed as extreme variations, but within the normal range, he came to see as well-marked mutants. "Epidemics" of similar mutants became a familiar experience in the fly group, a combined result of scaling up and changes in experimenters' perceptions and categories. In short, the first mutants appeared when and where they did because of the unusual qualities of Morgan's experiments. In no other modes of experiment then practiced were all the conditions right.

This argument, note, is neither strictly biological nor strictly cultural but a combination of the two. *Drosophila,* because of its short life cycle and large families, produced lots of mutants. But this property presented itself to biologists, becoming visible and meaningful, only when the fly was involved in certain specific kinds of experiment. *Drosophila*'s fecundity was a complex characteristic of an organism in a particular experimental culture. This is a crucial point. We are dealing here with neither biological nor cultural determinism: experimental life is an integral and inseparable mixture of nature and culture, and nature and culture must also blend seamlessly in histories and sociologies of experimental life.

Drosophila's potential for turning up mutants was most strikingly realized in mapping genetics. Mapping experiments had an autocatalytic character quite unlike that of any other experimental mode. Each mapping experiment produced more mutants, which had to be mapped, requiring more large experiments, which in turn produced more mutants to map, and so on. In this particular mode of experiment *Drosophila* became a kind of biological breeder reactor, producing more material in the course of an experiment than was consumed. This experimental chain reaction made *Drosophila* a cornucopia of research material and problems that was unlike anything that had ever turned up before in a biological laboratory. Comparison with a "breeder reactor" may seem a tasteless joke, but it is apt in capturing the experience of early drosophilists who felt that they were drowning in the flood of new material to be worked up, unable to cope. Morgan was the first to have this experience. "I am beginning to realize that I should have prepared for a large campaign," he wrote a colleague in early 1911, "but who could have foreseen such a deluge. With vicarious help I have passed one acute stage only I fear to pass on to another." But a year later he was again "head over ears in my flies," unable to keep up with the flood of material to be worked up. A decade later Charles Metz had a similar experience: "The way I am turning out mutants . . . beats any experience I ever had before. If things keep on piling up I will be swamped in another month and will have to call for help."[2] *Drosophila* never lost this capacity to nearly drown experimenters in fruitful work.

There were several reasons why mapping genetics had this special autocatalytic property. First, it was quantitative: distances were calculated statistically from the frequency of crossing over between two positions on a chromosome, and the statistics became more precise the more flies were counted. The legitimacy of gene mapping, which was then new and controversial, depended in part on its quantitative precision (biologists, perhaps because they work with inconstant living organisms, tend to be impressed by things quantitative), and that need was a powerful motive for drosophilists to do very large experiments. Hence more mutants. A second reason was the special character of mapping as a way of classifying and ordering mutants. Maps, unlike other systems of classifying data, are indefinitely elastic. They never fill up, and the fuller the better. So when new mutants turned up, drosophilists did not just set them aside as excess baggage but were obliged to put them on the map, and that required more experiments. Hence more mutants, more crosses, more mutants . . . the breeder reactor.

Other kinds of genetic practice did not have this autocatalytic property; for example, the kind of neo-Mendelian genetics that Morgan was practicing before he switched to mapping in 1912. In this earlier mode, mutants were organized not into chromosome groups but into organ groups (eye mutants, bristle, body color, and so on). This system of data management was not quantitative: its end product was not a map but a set of something like chemical formulas for each mutant form, composed of present or absent factors. New mutants did not necessarily lead to larger experiments. They did, however, require that the formulas for every mutant be revised over and over again, and it was this alarming instability of interpretation that finally caused Morgan and his students (the latter, first) to abandon organ groups for chromosome maps, which just got fuller and better. There is no better illustration of the power of material culture and practices radically to reshape experimenters' aims and concepts. We like to think that ideas and theories precede the nitty-gritty of lab practice, but in fact practice more usually precedes theory. In this case, the ideas of genes, maps, and crossing-over arose when drosophilists were forced to change their handling of data by the fecundity of their little fly.

The practice of genetic mapping also drove the transformation of *Drosophila* from a wild, inconstant creature into a reliable article of laboratory technology, the "standard fly." Few modes of practice would have impelled experimenters to undertake this daunting, decades-long task, but for mapping it was essential. Again, the argument turns on a mix of biology and human culture, and again the element of precision is crucial. Consistency and reproducibility of results are essential requirements of many kinds of experiment, but, as noted above, in none are they more essential than in genetic mapping. Mapping was quite controversial for a decade or so after it was invented. "Genes" resembled all too closely the discredited material factors of nineteenth-century theorists. And many biologists thought that the drosophilists had made a bad bargain when they traded relevance to embryology and evolution for abundant data on the structure and mechanics of chromosomes. Consistent, precise results were thus an indispensable way of giving the new practice legitimacy in the eyes of doubters. But getting consistent results with *Drosophila* was at first easier said than done.

Wild *Drosophilas* are highly variable creatures whose chromosomes are so loaded with modifier genes, hidden lethals, and other genetic junk that the first experiments seldom turned up the results predicted by Mendelian theory. Experiments with flies of the same species but from different geographical locales gave different results. As did experiments performed under slightly different conditions of food, temperature, moisture, and so on. Naturally! In nature variability ensured survival in an unpredictable environment. In the laboratory, however, variability was a threat to *Drosophila*'s survival. Every departure from theory was a reason to doubt its validity. Every discrepancy between results achieved in different labs was reason to suspect that maybe mapping genetics worked only at Columbia—a death warrant in a culture of laboratory practice that values knowledge only to the extent that is was the same everywhere. (The generic placelessness of laboratories is in part what makes experiment more valued than "mere" observation in nature.)

So the first task that Sturtevant, Bridges, and Muller faced was to clean up *Drosophila*, get rid of its variability, and fix it so that every experiment gave the same and the "right" results. They went about this in various ways. At first they hoped to disarm criticism by simply applying correction factors or disclosing the order in which chromosomal segments were mapped, but that makeshift did not work. Standardizing experimental conditions was more effective, eliminating much of the variability by using one kind of food and culture bottle and standardizing the procedures of genetic crosses. But the ultimate solution to *Drosophila*'s hard-core variability was to retro-engineer the fly itself, excising the genetic junk, and assembling from various stocks chromosomes that gave results that fit Mendelian theory. The chromosomes of standard mapping stocks were bricolages of genetic material from many flies, assembled to ensure clean experiments. The aims of Mendelian genetics were thus built into the instrument itself. What better evidence of the constructed, artifactual nature of *Drosophila*!

The construction of the standard fly was accomplished mainly by Calvin Bridges and took about a decade, though Bridges never ceased to tinker with it, concocting new gadgets and synthetic culture media, introducing new and more efficient mapping stocks, and recalculating standard maps from time to time. Thus was the biological diversity and fecundity of *Drosophila* transformed by the cultural need of biologists for precise, repeatable (and therefore trustable) results into that remarkable biological artifact, the standard fly. Was it a wild creature? Yes. Was it laboratory technology? Again, yes. Nature and human culture were brought together in the drosophilists' "breeder reactor." Other modes of experimental heredity would probably not have led to the creation of a standard organism: standardization was driven by the peculiar practical and epistemological requirements of mapping. Instrument and practice coevolved in Sturtevant and Bridges's mapping project.

THE MORAL ECONOMY OF THE FLY GROUP

Tools and material alone do not make a productive research community. The production of knowledge also requires an effective *social* technology, to use Steven Shapin's useful phrase. Talented recruits must be drawn in, trained in the use of standard instruments and routines, and taught what counts for the group as significant and creditworthy problems. The disputes that inevitably arise among groups of bright and ambitious people must be managed in a way that prevents the group from falling apart into rival factions and dissention. A workplace culture has to be maintained that encourages original, thorough work. Ways must be invented of spreading the word of new practices and persuading nonpractitioners of its worth. A research community, like a standard organism, is a complex bit of technology, a social instrument artfully constructed to turn raw intellect, zeal, and ambition to producing knowledge that everyone will want to learn about or use.

No aspect of communal work is more important than its "moral economy." This phrase, borrowed from the historian Edward P. Thompson, refers to the moral (as distinct from economic and organizational) principles underlying productive activities. Thompson used the idea to explain the apparently disorderly but in fact principled actions of food rioters in eighteenth-century England. The principles, in his case, were not those of political economy (supply, demand, and price) but older moral rules that defined the mutual obligations of owners and consumers in times of shortage. Embedded in the history and daily life of agricultural communities and seldom articulated, these moral principles are nonetheless powerful guides to action, especially in times of stress, and can be discovered by careful attention to group behavior.[3] Thompson meant "moral economy" to apply only to his particular case; however, I believe that it applies generally to any kind of production, of foodstuffs, manufactures, or cultural products like science. Indeed, it seems especially applicable to scientists, who tend to trade in symbolic more than economic values.

In the case of science, three elements of communal life seem especially central to its moral economy: access to the tools of the trade; equity in the assigning of credit for achievements; and authority in setting research agendas and deciding what is intellectually worth doing. *Access, equity, authority*—much of the success or failure of research groups depends on their ability to manage these crucial and contentious elements of communal work.

The moral economy of the fly group was distinctive and unusually self-conscious. First, access: everyone in the fly group enjoyed completely free and unhindered access to the instruments of production, the communal stocks of mutant flies, research paraphernalia, and know-how. Work in the fly group was intensely communal and egalitarian—visitors often commented on that, especially those from more stratified European laboratories. Everyone in the fly group was always involved in everyone else's projects, offering continual advice and criticism, swapping mutants and tips, engaging in kaleidoscopic collaborative projects. A very high percentage of the papers that came out of the fly group were multiauthored (a custom now common, but not in those days). Calvin Bridges, who kept the communal fly stocks, was famously generous in supplying material and technical know-how to anyone who asked, as was Alfred Sturtevant with his unequaled knowledge of the *Drosophila* literature, of which he was the unofficial keeper. Individual ownership of particular stocks was strongly discouraged, as was also any effort by individuals to corner problems for their exclusive use. It was understood, of course, that individuals would not try to beat out others to a juicy problem, but all tools and all problems could be taken up by anyone with skills and a good idea of what to do with them. There was never any move to divide the field of *Drosophila* genetics into specialized subfields. The custom of equal and open access is apparent even in the physical space of the fly room at Columbia: one common space, the only door being the one to Morgan's tiny office, which was always open. (At Caltech, there were individual rooms, but there, too, no closed doors.)

The principle of equity was no less crucial to the drosophilists' moral economy: credit was given not to a person who had a good idea first but to the one who first made an idea work experimentally. Ideas that were suggested by a colleague or emerged from the flow of communal shoptalk were a communal resource and could be appropriated freely; they did not even need to be acknowledged formally in publications. Given the communal way in which the fly group worked it would have been very difficult and contentious to assign credit in any other way—who could ever say for sure who had an idea first in the fly room's unceasing buzz of shoptalk? A skillfully performed experiment, in contrast, was unambiguous. Giving credit strictly for good work skillfully and thoroughly done was the drosophilists' golden rule.

This principle of equity did not settle every problem in the assigning of credit. To those who had lots of ideas and a habit of steady, productive work, like Bridges or Sturtevant, the rule seemed eminently fair. To others with different work habits it did not. Hermann Muller, for example, was extremely quick to see the ramifications of ideas but worked very deliberately—he had a taste for grand experiments that took years to prepare and complete. He came to feel that many of the group's best ideas had been his and concluded that Morgan had concocted the rule of credit for work accomplished in order to deprive him of his due.

In fact, the rationale behind the moral principles of access and equity was that it was the work that mattered most, more than personal credit. Work skillfully done and written up advanced the whole field of *Drosophila* genetics and benefited everyone in it. Hence the golden rule: whoever could do a job best deserved the credit.

The same idea guided the drosophilists' handling of authority in the workplace. Research agendas and the choice of problems were not imposed by Morgan, or by Sturtevant and Bridges, but emerged out of the group's communal work on the shop floor. Consider, for example, the group's handling of students and visitors. Officially, all students were Morgan's, since he alone had professorial status.[4] However, Morgan disliked the unending task of assigning topics to graduate students and visitors, and Sturtevant and Bridges—"the boys," as they were known (they began the mapping project as college seniors!)—gradually took over the care and nurture of students. In fact, every student and visitor was a student of the whole group. All the experienced workers took responsibility for helping students select dissertation topics, and when visitors arrived, Sturtevant would ascertain their skills and suggest an appropriate apprentice work, and he and Bridges then shepherded them through more demanding projects to full independence. None of the senior drosophilists regarded students or visitors as disciples who would follow their lead and enhance their status, as was the custom in many academic departments. The fly group had many devoted alumni, but it was not a "school."

This conception of dispersed authority also guided the relations between Morgan and his "boys," though not unambiguously. Almost from the very start of the mapping project in 1912, Morgan gave Sturtevant and Bridges (and Muller, for the years he was in the group) virtually complete control over what was done on the shop floor. The boys controlled their work. Morgan, though he kept sole control of how the Carnegie Institution's grant was spent and of academic business (e.g., curricula), never attempted to set agendas or direct the group's work. Indeed, as the work became more technical and arcane, Morgan relied on the boys to help him design his own experiments. And as Morgan's interest turned to other organisms in the 1920s, it was the boys who ran the drosophila show. In the workplace Morgan, known with respectful affection as the "Boss," behaved much like one of the boys. Research agendas emerged from the group's communal work as individuals chose to pursue one or another of the various leads that turned up.

These values were not preached but practiced. Jack Schultz, who got his Ph.D. in 1929 and remained in the fly group until 1942, thought the group's cooperative spirit derived from Morgan but could recall no explicit discussion of its virtues. It was taught by personal example and came, he thought, from the paramount importance that Morgan and the rest gave to getting on with the work.[5] Doing good work was the only thing that mattered in the end, and the principles of the group's moral economy were practical encouragements to put the work first.

None of these ideals of access, equity, and authority was unique to the fly group, but few research groups displayed all of them so fully and self-consciously. What features of the fly group, then, account for its distinctive customs? The most important cause, I believe, was the fly itself, that cornucopia of research material and problems. The fly group's social organization and moral economy coevolved with the mapping project and was designed to exploit the advantages of a superabundance of material. There was nothing to be gained in limiting access to tools or in exclusive ownership of problems. And it would have been self-defeating to pursue preset agendas or divide up the field when the choicest problems turned up unexpectedly as by-products of the mapping project. Abundance did not merely reduce the temptations of self-interest. Rather, it ensured that self-interest was best served by giving free access to tools, freely exchanging ideas (even if someone else might occasionally reap the benefit), and leaving the choice of problems unconstrained. Just as Bridges's chromosome maps represented the blueprint of the standard fly, so the drosophilists' rules of moral economy constituted the design of an intricate social instrument that enabled them to make the most of the opportunities thrown their way by the mapping project and the fly.

A more parsimonious organism and experimental system would doubtless have given rise to a quite different moral economy. So, too, might the fly in a different cultural context of practice. Material culture does not by itself determine behavior. The logic of advantage and value is logical only in particular scientific cultures and contexts of practice. I have quietly assumed, for example, that the drosophilists would of course exploit the advantages of their breeder reactor. But why of course? They did, but what impelled them to do so? One part of the answer is that they were at first a small minority, pursuing an unfamiliar and suspect mode of genetic practice in a world filled with more established and trusted modes—modes made powerful by other groups exploiting the advantages of their organisms and systems. In this larger context it was logical, even necessary for survival, to take pains to exploit any local advantages. That was how experimental science in general worked (and still does work).

Another part of the answer is that Morgan, as an experimental biologist in the United States around 1900, was a member of a larger community of practice that highly valued mutual aid and cooperation (even when they did not practice it). There are historical reasons why this culture of mutual aid was especially strong at that particular time and place, which have to do with the way in which American academics assimilated European ideals of research into a system of higher education that valued mass teaching and was short on resources for doing research and training researchers.[6] Overexpansion, and the American custom that every institution had a right to participate in high culture, created a generation of academic scientists who aspired to do research but lacked the training and opportunity. In this context, mutual aid was an ideologically acceptable and affordable strategy.

Among biologists the culture of mutual aid was especially strong at centers of community life like the Marine Biological Laboratory (MBL) at Woods Hole, a major purpose of which was to enable teachers in small colleges and high schools to engage with colleagues from research universities in vanguard research.[7] Summer work at the MBL also powerfully reinforced the virtues of equal access and mutual aid. Morgan was a leading figure at the MBL, migrating there every summer during the Columbia years with the whole fly group, complete with flies and paraphernalia. He consciously exemplified the MBL's virtues of access and mutual aid.

The moral economy of the fly group, then, may best be understood as an amplified form of values widely held by American experimental biologists at the time. The amplifier was *Drosophila*, the breeder reactor. Its abundance cooled any selfish temptation to retreat from the ideals of mutual aid and helped drosophilists make their values of access to tools, credit for work, and equal authority in the workplace into living values.

We cannot understand how research communities define what is virtuous and pursue "advantage" without some knowledge of the culture of science in general and of the larger societies in which scientists operate. No community is unconstrained in the way it conceives of basic social qualities like virtues and advantage. Scientists operate socially and ethically with what is given and familiar in their society, as do the members of any subculture. It would be an odd group of scientists whose working customs were not variants of those they had been brought up to live by, and it is unlikely that such a group would be long-lived. Close inspection of group behavior takes us a good way toward understanding why they do what they do, but it risks a vulgar functionalism, which explains behavior in terms of its benefits to group members but leaves unexamined how conceptions of "benefit" and its proper pursuit derive from the larger social context.

THE MORAL ECONOMY OF EXCHANGE

The fly group's values were not restricted to them but were extended to drosophilists throughout the world by a remarkable custom of free exchange of mutant stocks. This was the custom from the very earliest years, even before the mapping project began in 1912. If a qualified researcher asked for stocks, the fly group provided them free of charge and with no strings attached; and not just stocks that had been worked out and published. Even new, actively producing mutants were expected to be shared if someone dreamed up a productive use for them. Though the custom of exchange was imitated by biologists who worked with other organisms, few engaged in exchange as systematically and devotedly as did the fly people. Drosophilists were unabashedly proud of their tribal custom. For them it was far more than a technical aid to work; it was a mark of professional identity, a badge of citizenship in a special community.

It would be hard to overstate the importance of this custom of exchange. Exchange enabled the fly group to exploit the abundance of *Drosophila* to a degree not possible had they tried to maintain a local monopoly. Most important, the custom of exchange enabled mapping genetics to become, not one among several competing modes of genetic practice, but the premier mode. Exchange is what made mapping genetics the standard genetics and the fly the *standard* fly. It made a restricted, local form of practice into a universal, cosmopolitan one; it spread the fly group's customs and moral economy to drosophilists everywhere. Free exchange of knowledge and the results of work was an essential feature of experimental science for centuries, but exchanging the tools and means of knowledge production was a far more powerful instrument of dispersal. Formal publications spread the *word* of mapping and the standard fly, but exchange of stocks and know-how spread the *work*.

The exchange system, like the fly group, was an intricate piece of social machinery that required careful design and tending. The intricacy lay less in its mechanics—swapping tubes of flies was not much more complicated than using the public mails—than in its moral rules. Just think of the risk that the fly people took when they sent their most productive mutant stocks to potential rivals! How easy to take unfair advantage! Yet cases of abuse were almost unheard of. Drosophilists were and still are a remarkably civil and trusting bunch, owing to the moral rules that guided their system of exchange.

Though seldom articulated, these rules can readily be discerned in the numerous letters that attended tubes of flies from one drosophilist to another. One was *reciprocity*: the privilege of receiving stocks entailed the obligation to reciprocate if asked. To be sure, the fly group and other large centers gave far more than they received—Bridges spent a great deal of his time sending stocks and directions for their use—but the principle of reciprocity displayed the presumption of equality among producers. Cash was generally not an acceptable alternative to swaps, though stocks provided for classroom use, where there was no basis for reciprocity, gradually moved from the moral economy to the cash nexus. This clear demarcation between swapping and cash exchange illustrates the moral character of reciprocity as nothing else does. We have to do not with market values (political economy) but a moral economy.

A second principle of this moral economy was *disclosure*. Recipients of stocks were expected to inform donors fully of the experiments they intended to do with them, especially if they were working on similar problems. Disclosure was vital for securing trust among potential rivals and dispelling suspicions that were entirely natural among people who were constantly exposing each other to the temptation to betray a trust. Disclosure discouraged poaching by making it impossible (or very awkward) for recipients to give ignorance as an excuse for misbehavior, and giving donors the opportunity to nip unwitting competition in the bud. Failures to disclose were taken as reason to suspect borrowers' intentions and to cut them out of the exchange system (though it almost never came to that).

An important by-product of the custom of disclosure was that exchanging stocks could also serve as an informal system of communicating plans and results in advance of publication. Every stock carried with it information about fast-breaking results and future plans. This informal communication system among producers was faster and more useful for actual practice than official publication (being personal and less guarded), and it was certainly a major reason for the remarkable dispersive and productive power of fly genetics.

A third principle of exchange was *limited ownership.* While problems might be owned temporarily by individuals, tools were regarded as the property of the whole community of producers. Within limits: it was customary with special stocks, like Bridges's multiply-marked mapping stocks, or versatile triploids or translocations, to get permission to use them from their inventors. This courtesy showed respect for the skill and hard work that had gone into constructing these special tools. It was taken for granted that permission would not be refused. But not to ask was taken as reason to suspect that the borrower might be a poacher. When the Swedish geneticist, Gert Bonnier, failed to get Bridges's permission before using one of his special stocks, drosophilists wondered if he could be trusted with further gifts. In this case, as in most, it was simply a beginner's ignorance of etiquette. But when Curt Stern asked the fly group to leave to him a problem on which they had both started work, Sturtevant responded sharply that no one had copyright on any problem, and besides, Stern could rest assured that they were doing a state-of-the-art job. There was no arguing with the drosophilists' golden rule, and Stern did not pursue the matter.[8] The sanctions against abuse of the exchange system were insubstantial but remarkably effective. I know of no instance of serious abuse nor any in which the implicit threat of exclusion was carried out.

No trade secrets, no monopolies, no poaching, no ambushes—these were the practical rules for maintaining trust and harmonious relations among the fly people.

The moral rules of the *Drosophila* exchange system were clearly adaptations of the fly group's customs of communal work to a wider community, in which trust could not be maintained in daily, face-to-face interactions. The question again is: How did these customs arise, and why did they work so well? The answer is in part individual preference: as Morgan wrote his patron, Robert S. Woodward, "The method of locking up your stuff until you have published about it, or of keeping secret your ideas and progress have never appealed to me personally." But what made it possible for individuals to act on their preference for openness was the abundance of the fly: "It may be," Morgan went on, "that we can claim no special virtue here, for Drosophila is like the air we breathe—there is enough for all."[9]

Evidence does in fact exist for Morgan's suggestion that it was the abundance of the fly that enabled drosophilists to be generous. The first glimpse we have of the exchange system in action comes from 1911–12, when Morgan was desperately trying to keep his nose above the flood of mutants. In his need he turned to teachers of biology in small colleges, inviting them to perform some part of the *Drosophila* work in return for a Ph.D. degree. Morgan provided the mutant stocks and know-how, and his distant partners did the work on their own limited schedules. The need for this makeshift diminished when Sturtevant and Bridges became permanent members of the mapping team and as resident graduate students took up some of the load. The mapping project only increased the flood of material, however, and Morgan's improvised system of putting out became a regular system of exchange among *Drosophila* workers, many of them fly-group alumni.

The custom of exchange mixed altruism with enlightened self-interest, bringing substantial benefits to the fly group. Spreading the work of mapping made it widely familiar and acceptable. Prospective students and converts could sign on in confidence that they would have continuing access to the tools of their new trade after leaving the fly group. Who would sign on without such assurance? Morgan was well aware that strength would lie in numbers. Practices limited to one place are open to attack as merely local and idiosyncratic; scientists trust knowledge that is widely practiced and cosmopolitan. And what more compelling reason to trust an unproved mode of practice than to see it work with your own eyes, as a student, or to make it work with your own hands? Finally, exchange kept the fly group au courant about what other drosophilists were doing, enabling them to spot important new leads and avoid dead ends. None of this makes the custom of exchange any less virtuous: the point is that the abundance of the fly and of genetic mapping united virtue with self-interest.

The fly people did not invent the custom of exchange; natural historians, for example, had been swapping specimens for centuries.[10] But the drosophilists adapted this custom to the world of experimental laboratories and standard organisms, refining its practices and giving it the strength of communal virtue. Similar systems of exchange have since been created for just about every standard organism—maize, bacteria, phage, and so on. But the fly people still, even today, identify most strongly with the ideals of mutual aid and civility. These virtues and practices define who they are and what makes them distinctive. In part this reflects the unusually durable authority of the Morgan group, who set the pace and moral tone of work in a world grown vastly larger and competitive. The standard fly and the practices and values that have accreted around it were both a means of production and the vehicle of a community's distinctive way of life.

CONCLUSION

Can we generalize from this case study? The fly group was a particular—even peculiar—case. First, it was an extramurally funded research institute somewhat awkwardly encysted in an academic department of zoology, with its quite different customs. (Academic biologists normally avoided competition by working on different organisms.) Second, the grant that enabled Morgan and his students to stay together for thirty years froze the psychosocial dynamic of "Boss" and "boys" at the stage of mentor and students, and this neotenous relation perturbed personal relations in the group. Finally, comparably large and talented groups did not arise to challenge the fly group's leadership for almost twenty-five years, and the absence of institutional rivalries certainly encouraged free exchange. (This changed somewhat in the 1930s.) But if the drosophilists were so special, what can we learn from this case study about experimental biology in general, or physiology, or physics?

In fact we learn a good deal, because while scientists handle the issues of practice and moral economy in varied ways, depending on local contingencies of personality, politics, and material conditions, these issues are universal to experimental life. The practical process of producing experimental knowledge requires that problems be defined, tools and methods devised, wherewithal secured, communities and intergroup relations constituted, moral order maintained. A study of variation in the practices of different groups is very likely, then, to provide insights into these fundamental elements of scientific work.

There is much evidence for this observation. That workers with various standard genetic organisms have devised similar exchange systems strongly suggests that the causal link between material culture and moral order is a general one. That the standard animals of biomedical disciplines (mammals, mainly) tend to be sold for cash and not swapped suggests how a connection with a powerful profession alters this linkage.[11] There is evidence, too, that abundance of material generally supports a moral economy of mutual aid. But where the rewards of labor are limited to a single winner, as in searches for new vitamins and hormones, infectious agents (the AIDS virus, e.g.) or genes—there secrecy, competitive races, and a bitch-the-other-guy-if-you-can morality prevails. Drosophilist George Beadle discovered this to his disgust when he ventured into the field of vitamin assay. The recent decline of open exchange among molecular biologists is clearly related to their involvement in the highly lucrative biotechnology industry. Now, new tools are patented, and university lawyers rule on proposed exchanges.

Concepts of practice and moral economy are well on their way to being central, defining concerns of science studies, but in an oddly contentious way. Like all the human sciences, science studies is struggling through the great postmodern pandemic, factionalized by theories, methodologies, and interests that seem to have more to do with staying visible and getting a leg up than with achieving a shared understanding of how science works. In the study of scientific practice, theoretical approaches have succeeded one another at an ever faster pace—interest, social construction, actor network, social world, discourse, agency, and mangle, just to mention some of the better known. A vanguard strategy has its uses, true: displays of arcane expertise warn off critics, confer academic status, and protect turf. But there are costs, and even people who are sympathetic are getting confused and a little fed up.

The approaches just listed are hardly worthless. I know of few that have not contributed in important ways to our understanding of science, and I have myself borrowed from a number of them though allied myself with none. Readers in the know will easily recognize how compatible my account is with those especially of Steven Shapin (moral economy, trust), Bruno Latour I (credit cycle) and II (actor-networks, immutable mobiles), and the sociologists of work.[12] My complaint, rather, is that we in science studies are too quick to turn ideas into ideologies and methods into methodologies, and to let means of analysis become ends. Perhaps this odd custom is the best we can do to constitute community and authority in a field that is ambiguously multidisciplinary and has no powerful constituencies to provide shelter in political storms and to broker consensus. In a period of general and profound distrust of central institutions and authorities, scholars who seem overly concerned with consensus and community make themselves and their work easy targets in the skirmishing of cultural politics. But the times may be changing, and if the problem of reconstituting community in a pluralist society should become an urgent social issue, we may be sure that the human sciences—and science studies with them—will not be far behind.

We practitioners of science studies should settle down and concentrate on the subject and the questions that are ours alone and that define us as knowledge producers: How does science work, and why? How do scientists go about their business of producing natural knowledge, and what is it about their practices and communal customs that has won them such high standing in modern societies? How do methods coevolve with material culture, and modes of practice with moral economies? How do the customs of particular communities vary? How do they derive and depart from those of the larger society? It is in the hope of answering these important questions that we study the material culture and practices, the moral economies, and the social history of groups like the drosophilists.

NOTES

1. This paper is a brief reprise of the main ideas of my book, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994). I have kept references to a minimum, assuming that readers who want the whole story and full documentation will turn to the book.

2. T. H. Morgan to Charles B. Davenport, 14 March 1911; and Charles Metz to Davenport, 13 Apr. 1911, 13 July 1922, Davenport Papers, American Philosophical Society. Morgan to Hans Driesch, 1 January 1912, Morgan Papers, American Philosophical Society.

3. Edward P. Thompson, "The moral economy of the English crowd in the eighteenth century," and "The moral economy revisited," in Thompson, *Customs in Common* (New York: New Press, 1991), 185–258, 259–351.

4. Sturtevant and Bridges were officially employees of the Carnegie Institution of Washington from 1914 and had no academic appointments. Sturtevant was made professor when the group migrated to Caltech in 1928, but Bridges never was.

5. Jack Schultz to George Beadle, July 31, 1970, Schultz Papers, American Philosophical Society.

6. Robert E. Kohler, "The Ph.D. machine: building on the collegiate base," *Isis* 81 (1991): 638–62.

7. Philip J. Pauly, "Summer resort and scientific discipline: Woods Hole and the structure of American biology, 1882–1925," in *The American Development of Biology*, eds., Ronald Rainger, Keith R. Benson, and Jane Maienschein, (Philadelphia: University of Pennsylvania Press, 1988), 121–50.

8. Kohler, *Lords of the Fly*, 145–46, 150.

9. Morgan to Robert S. Woodward, July 25, 1917, Carnegie Institution Archives.

10. Anne L. Larsen, "Not Since Noah: The English Scientific Zoologist and the Craft of Collecting, 1800–1840" (Ph.D. dissertation, Princeton University, 1993).

11. Bonnie T. Clause, "The Wistar rat as a right choice: Establishing mammalian standards and the ideal of a standardized mammal," *Journal of the History of Biology* 26 (1993): 329–50.

12. Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985). Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994). Steven Shapin, "Trust, honesty, and the authority of science," in *Society's Choices: Social and Ethical Decision Making in Biomedicine*, eds. Ruth E. Bulger, Elizabeth M. Bobby, and Harvey V. Fineberg (Washington, D.C.: National Academy Press, 1995), 388–408. Bruno Latour and Steven Woolgar, *Laboratory Life: The Construction of Scientific Facts*, 2d ed. (Princeton: Princeton University Press, 1986), ch. 5. Bruno Latour, "Visualization and cognition: thinking with eyes and hands," *Knowledge and Society* 6 (1986): 1–40. Bruno Latour, *Science in Action* (Cambridge, Mass.: Harvard University Press, 1987). Everett C. Hughes, *Men and their Work* (New York: Free Press, 1958). Adele E. Clarke and Elihu M. Gerson, "Symbolic interactionism in social studies of science," in *Symbolic Interactionism and Cultural Studies*, eds. Howard S. Becker and Michael M. McCall (Chicago: University of Chicago Press, 1990), 179–214.