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A Brave New Animal for a Brave New World:

The British Laboratory Animals Bureau and the Constitution of International Standards of Laboratory Animal Production and Use, circa 1947–1968

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Abstract

In 1947 the Medical Research Council of Britain established the Laboratory Animals Bureau in order to develop national standards of animal production that would enable commercial producers better to provide for the needs of laboratory animal users. Under the directorship of William Lane-Petter, the bureau expanded well beyond this remit, pioneering a new discipline of “laboratory animal science” and becoming internationally known as a producer of pathogenically and genetically standardized laboratory animals. The work of this organization, later renamed the Laboratory Animals Centre, and of Lane-Petter did much to systematize worldwide standards for laboratory animal production and provision—for example, by prompting the formation of the International Committee on Laboratory Animals. This essay reconstructs how the bureau became an internationally recognized center of expertise and argues that standardization discourses within science are inherently internationalizing. It traces the dynamic co-constitution of standard laboratory animals alongside that of the identities of the users, producers, and regulators of laboratory animals. This process is shown to have brought into being a transnational community with shared conceptual understandings and material practices grounded in the materiality of the laboratory animal, conceived as an instrumental technology.

It is by no means generally realised ... that much money can be saved by working with only first-class stock of, for example, mice or guinea-pigs. Under existing conditions there are occasions when up to fifty per cent of animals sent to an institution never survive to the experimental stage. Moreover, even if they live to be used for research or assay, the death of their companions rightly casts suspicion on the survivors and the careful worker proceeds to use, as a precaution, twice as many animals as he would do if he had confidence in their health.

—Conference on the Supply of Experimental Animals (1945)

WHEN THE LABORATORY ANIMALS BUREAU was established in 1947 by the British Medical Research Council (MRC), its agenda was to facilitate access to information regarding laboratory animal supply, its practices were to be bureaucratic, and it was staffed by a director and a secretary. Celebrating its twenty-first anniversary in 1968, and renamed the Laboratory Animals Centre in recognition of its role in governing the national supply of laboratory animals, it had grown to be one of the largest MRC units, employing, in addition to its director, eight full-time research scientists and a complement of administrative and technical support staff in addition to housing several visiting and temporary researchers from across the world. It was internationally recognized as a center of expertise in a new subdiscipline of “laboratory animal science and medicine” and for producing highly standardized laboratory animals. This essay traces the transformation of the bureau from its ostensibly bureaucratic role of managing information about laboratory animals to its later position as a research center for the study and production of laboratory animals. Against a background of socialist-influenced state planning and the expanding internationalization of

science, it is shown how the discourse of standardization was utilized by diverse parties to enroll the state (in the form of the MRC), industry (the producers and suppliers of animals), and biomedical scientists (the users of laboratory animals) within a shared community established by reconceiving laboratory animals as a national and, later, transnational resource. This was a dynamic process centered on the construction of shared conceptual understandings of and material practices concerning the laboratory animal that consolidated its identity as an instrumental technology. At the same time, it facilitated the co-constitution of the individual identities of, and the relations between, the producers and the users of laboratory animals, and the bureau itself, within a new community.¹

The bureau was established in response to a claim by the Conference on the Supply of Experimental Animals that researchers who thought their work reliable lived in a “fool’s paradise.” The conference was an independent body, coordinated by the Association of Scientific Workers, representing twenty leading British scientific societies. According to its 1945 report, British biomedical science was hampered, uncertain, wasteful, and inconsistent owing to a lack of national standards comparable to those that the MRC had established at its flagship unit, the National Institute for Medical Research (NIMR). The bureau was established in 1947 to act as a “clearing house for information on the supply and demand for experimental animals” and to work toward regulating the “present haphazard system, or lack of system,” for sourcing laboratory animals by making this expertise available to all. Its first director, Ronald Everett Glover, had previously managed the NIMR animal production facilities but left the bureau within a year to take up the position of professor of veterinary pathology at the University of Liverpool. His replacement, William Lane-Petter, was medically qualified and had enjoyed a career in private practice before serving in the Royal Army Medical Corps between 1938 and 1945 (see Figure 1). On discharge Lane-Petter became a Home Office inspector under the Cruelty to Animals Act (1876). His appointments despite lacking personal experience in the use or production of laboratory animals beyond the enforcement of legislations is indicative of the intended function of the bureau. In addition to administrative competence, Lane-Petter demonstrated an ability, likely derived from his clinical experience, to communicate with, and be taken seriously by, both users of animals (educated persons in academic, industrial, and medical settings) and producers of animals (largely undereducated, working-class, “every-day sorts” who bred animals in a variety of settings from farms to urban garden sheds).² Nevertheless, he disregarded the bureau’s narrow remit and worked to establish its reputation as a center for research in a new field of “laboratory animal science.” Touring the world to pursue his agenda, Lane-Petter transformed the bureau into an international center that absorbed, innovated on, and distributed knowledge about laboratory animal production and use. The international work of Lane-Petter and the bureau led to the establishment of the International Committee on Laboratory Animals (ICLA) in 1956. Supported by the United Nations Scientific and Cultural Organization (UNESCO), the ICLA was to accomplish on a global scale what the bureau had achieved for Britain, thereby constituting a new transnational community grounded in the material production, provision, and use of the laboratory animal.

This essay argues that standardization discourses were not only regulative but also productive. Standardization formed the conditions of possibility that legitimized the processes that dynamically co-constituted the identities of, and triangulated relations between, institutions such as the bureau and laboratory animal producers and users nationally and worldwide. The importance of standardization to the biomedical sciences, particularly in terms of the origins, utility, and proliferation of research organisms, has been the focus of several studies.³ This literature prioritizes material agency to cast experimental organisms as constitutive of the practices, social structures, and knowledge claims of laboratory-based science. However, to a greater or lesser extent it retains the presumption that standard laboratory organisms emerge within fields and disciplines constructed by

individual scientists for context-laden purposes. Accounts of the standardization of *Drosophila* are narrated against the context of early genetic research, for example, and the inbred mouse emerged against the background of cancer research.⁴ Historical attention, then, has focused on the situated configuration of laboratory organisms as tools “right” for specific jobs. Adele Clarke and Joan Fujimura, with others, have illustrated that “‘tools,’ ‘jobs,’ and the ‘rightness’ of the tools for the jobs are each situationally constructed ... they are co-constructed, mutually articulated through interactions among all the elements in the situation.”⁵ Such studies rightly assume the mutual dependency of discipline, purpose, and material resource in processes of construction. However, they say little as to how scientists obtained what might be termed “generalized” laboratory organisms, utilized in large quantities for routine procedures such as the biological standardization of therapeutics, pregnancy and other medical diagnosis, or toxicity testing. From the late nineteenth century, biomedical experiment came to depend on live animals to model disease and, increasingly, as a means to measure the potency of a host of new biological substances such as vitamins, hormones, and antitoxins.⁶ Comparatively little work has addressed the tensions that emerged as a result of the biomedical sciences’ increasing reliance on live research material in the twentieth century: Where did the animals come from? How did they travel to the laboratory and how, once there, were they routinely configured for the laboratory? An exception is the work of Adele Clarke, who has shown that a shift during the opening decades of the twentieth century led to radically different needs for research materials, requiring scientists to negotiate access to live research materials themselves and so to create informal supply networks via zoos, museums, hospitals, and the like.

For much of the twentieth century, laboratory organisms were quantitatively more likely to be used in routine procedures than in specialist research, yet the latter remains the focus of most historical studies. This is despite the fact that it was routine procedures that drove ever more industrialized approaches to animal use, pushing the “informal networks” of animal supply to the point that they could no longer sustain the needs of the biomedical sciences (considered broadly to include the pharmaceutical industry).⁷ Admittedly, routine work is not the most exciting of scientific practices. Nevertheless, the generalized laboratory animal remains quantitatively and productively important. This essay examines the production of standardized laboratory animals outside of specific disciplinary knowledges in order to trace the origins of a generalized subdiscipline of “laboratory animal science.” At the same time, it offers an alternative to the predominantly American focus of the existing literature. Standardization discourses resonated with scientific needs because the *raison d’être* of experimental science was the production of universal knowledge detached from localized contexts. Standardization operated as a vector of internationalization, acting not only to regulate but being productive in a sense beyond that of enabling communication between dispersed communities. The language of standardization facilitated the construction of new identities while simultaneously mediating relations between institutions such as the bureau and laboratory animal producers and users worldwide. Without comparative studies of different national contexts, it is difficult to see how this worked internationally. Reconstructing the history of the Laboratory Animals Bureau will shed light on the constitution of the generalized laboratory animal, its users, and its producers and in so doing advance an understanding of how standardization discourses worked to constitute identities creatively as well as to regulate toward norms on a global scale, processes on which the infrastructure of twenty-first-century biocapital was built.⁸

CONSTITUTING A NATIONAL SUPPLY OF LABORATORY ANIMALS

Historians have shown how, from the nineteenth century, industry increasingly turned to science in the search for innovation and productive efficiency.⁹ Total war in the twentieth century catalyzed this trend and introduced the state as a mediator.¹⁰ This was not a linear

process of industry utilizing science; rather, organizations such as the British Science Guild and the Association of Scientific Workers enrolled industrial and state interests to serve the ends of science.¹¹ It was against this background that the Laboratory Animals Bureau was established by the MRC in response to a campaign by an extraordinary coalition of scientific organizations constituted and coordinated by the Association of Scientific Workers and acting under the name of the Conference on the Supply of Experimental Animals. The agenda was to enlist the state to provide a national supply of standardized laboratory animals; their target was the research councils. Animal-dependent biomedical research in Britain, the conference claimed in a 1945 report, was suffering a crisis in reliability due to the low quality of animals then available. The authors—all biomedical scientists, led by the biochemist Alfred Louis Bacharach and the anatomist John Beattie—called for the research councils to establish a “Central Bureau” that would represent the state and act as guarantor of a national stock of standardized laboratory animals. The bureau’s work would be twofold: it would act to constitute a national supply of laboratory animals as well as establish standards to govern that supply.¹² In the 1950s a laboratory animal “industry” as we would recognize it today was yet to emerge in Britain. Animals were procured for scientific use from a commercial market that catered predominantly to the pet and fancy trade. Indeed, in Britain at least, the *laboratory* animal as a creature *of the laboratory* was yet to come into being; arguably, it was constituted with the establishment of the Laboratory Animals Bureau in 1947. Originally, the bureau was to be called the Experimental Animals Bureau. Bacharach objected, appealing to logical consistency to point out that “the animals are not strictly speaking experimental” and so “the phrase laboratory animals would be preferable.”¹³ The animals that concerned the bureau would be for, and of, the laboratory. In Britain, then, standardization was not imposed on science by the needs of industry. On the contrary, standardization was used by the biomedical community to constitute an industry to support its needs.

The MRC, in part because of prior experience, had no desire to become involved with the supply of laboratory animals. Between 1925 and 1927, in partnership with Edinburgh University, it had funded the “Edinburgh Stock Farm,” which was intended to supply animals to laboratories in Scotland and the North of England. The idea of Francis Albert Eley Crew, a leading expert on animal breeding, the farm attempted to emulate the animal facilities available at the MRC’s National Institute for Medical Research on a commercial basis similar to that of the American Wistar Institute.¹⁴ However, the project consistently suffered large financial losses, forcing the council to withdraw its support. Largely as the result of the long service of Arthur Landsborough Thomson, Second Secretary of the MRC from 1919 to 1957, an informal policy of avoiding commercial animal production lingered within the council. By deploying the discourse of standardization, the lack of which was said to imperil the reliability of British science, the conference coerced the MRC to act, albeit reluctantly. The council first explored the establishment of a laboratory animal “trade organization” capable of self-regulation, thereby releasing itself from any involvement. However, since what trade existed showed no interest in organizing itself, the MRC was obliged to accept a new role in mediating the relationship between animal users and breeders. The lack of an animal breeding industry—and, consequently, of any sense of corporate identity or commercial interest to rival that of the numerous biomedical societies—meant that there was little protest of the MRC’s involvement from breeders, with the single exception of “dealers” (discussed below). Resistance to state involvement came mainly from within the council itself, which sought to minimize its responsibilities. Landsborough Thomson repeatedly emphasized that the bureau was to act only as “a centre for the dissemination of information and will not handle any animals.”¹⁵ Nevertheless, this insistence on a limited, bureaucratic role failed to curtail the activities of the bureau, thanks in large part to Lane-Petter, who succeeded Glover as director before any substantial action was undertaken.

The initial work of the bureau was indeed bureaucratic. Responding to the fact that little was known about those who produced and those who used laboratory animals, Lane-Petter instigated a nationwide survey of laboratory animal provision and use. Access to state records held by the Home Office enabled him to locate users who were registered under the Cruelty to Animals Act of 1876. Similarly, and historically uniquely, owing to lingering wartime rationing the Ministry of Agriculture maintained records of those who claimed feed rations to breed laboratory animals, allowing Lane-Petter to locate producers. Questionnaires were then dispatched, introducing the bureau and requesting information on the respective activities and needs of producers and users of laboratory animals. Responses to this “census” were used to construct a quantified portrait of the laboratory animal population in Britain. Drawing on recent articulations of the work of Michel Foucault, this process of survey, or census, should be interpreted not only as an exercise in quantification but also as acting to constitute that which it quantified. Moreover, it acted in such a way as to constitute the laboratory animal, its users, and its producers within a landscape that invited intervention.¹⁶

Hitherto, obtaining laboratory animals had been a matter of localized ad hoc arrangements between individual laboratories and local suppliers. Suppliers were often not themselves breeders but, rather, so-called dealers, who bought animals in bulk from small-scale breeders and sold to a closely guarded network of laboratory contacts. Dealers’ practices relied on obstructing communication between laboratories and breeders, and so they resisted the formation of a national commercial market and the type of community identities that the bureau constituted via its national survey. For this reason, among others, Lane-Petter identified dealers as parasitical and responsible for the fact that “traffic in animals had become in many cases indistinguishable from a racket.” Dealers were thought to have undermined the council’s attempt to form a trade association by convincing breeders that it was detrimental to their interests. They certainly resisted the bureau’s census and encouraged breeders to ignore it, a tactic that delayed rather than prevented its completion. Lane-Petter employed the census information to eradicate dealers by offering a service that only the bureau could provide: placing breeders directly in touch with laboratories at no charge. Consequently, the operational model of dealers became redundant, and the bureau obtained the goodwill of breeders who, having been enabled to trade directly with laboratories, enjoyed an immediate gain in animal profitability. Similarly, Lane-Petter won the trust of users, who began to seek the bureau’s advice on where to obtain animals. In a relatively simple process, then, dealers were eradicated and the bureau was established as the governing power at the center of a newly constituted national laboratory animal market.¹⁷

Guaranteed custom, improved profitability, and a stabilized market proved valuable enticements to encourage breeders voluntarily to “submit to some sort of discipline.” Consequently, Lane-Petter introduced an informal scheme whereby the bureau circulated among laboratories a register of breeders who, in addition to having established minimal standards of practice, agreed to abide by two further principles: that animals would be bred in closed populations and would travel directly from breeder to laboratory user. These provisions countered dealing in animals and also combated infection among the laboratory animal population.¹⁸ Assurance of healthier animals encouraged users to pay slightly more to bureau-approved breeders, while breeders enthusiastically cooperated when increased profitability was promised. The system proved an immediate success. Within a year some breeders called for a more rigorous scheme incorporating “the inspection of their premises and the issue of some form of licence,” an idea that appealed to Lane-Petter. A formalized scheme allowed bureau inspection to be dynamically integrated with the development of disciplinary standards, the latter being constituted in part from the best practices observed in the former. In 1949 Lane-Petter announced a new “accreditation scheme” intended to ensure

that the national supply of laboratory animals was healthy, experimentally reliable, and, as far as was reasonably possible, standardized.¹⁹ Privately, it was intended to facilitate the development of standards of laboratory animal production that it publicly enacted. Thus regulation and the constitution of standards evolved dynamically. Initially, the demands on breeders were minimal, in part to encourage their participation but also because there was no clear sense of what form a standardized laboratory animal should take, let alone how one should be produced. Minimum levels of cleanliness and recommendations for regular feeding seemed commonsensical, but there was little Lane-Petter could say with certainty as to good practices of breeding. Nevertheless, the scheme operated to triangulate the needs of users and breeders while fashioning, over time, a shared understanding of what a laboratory animal should be. Moreover, it also contributed toward constituting the interrelated meanings and identities of what it was to be a user or producer of laboratory animals. At the center of this process was the bureau, acting to govern and facilitate laboratory animal production and use while at the same time building its own identity.

The accreditation scheme was voluntary, yet the competitive advantage of government endorsement encouraged participation. Participating breeders were required to produce animals predominantly, if not entirely, for laboratory use, with stock being self-contained and the introduction of additional animals permissible only with the knowledge of the bureau (buying stock for resale was prohibited). Accreditation demanded compliance with “standards of housing, caging, feeding and general management of hygiene ... compatible with the production of first class animals,” but the bureau did not define what these standards were. Rather, Lane-Petter, or a local representative drawn from the Public Health Laboratory Service or the Veterinary Investigation Service (of the Ministry of Agriculture), made the judgment after inspecting the breeder’s facilities and subjecting a random selection of animals to pathological examination. If the breeding quarters appeared to be sturdy, hygienic, and secure and the animals well fed and free from infection, the breeder was generally accredited. Early accreditation visits were as much about accumulating knowledge as about enforcing standards. It is telling, for example, that a breeder’s attitude was considered important: if he or she appeared cooperative, then accreditation more often than not was awarded. Lane-Petter frequently claimed that the “better breeders” had “welcomed the discipline” of the scheme, without noting that deference to discipline had been an important factor in their becoming accredited to begin with.²⁰ The accreditation scheme allowed the bureau to track laboratory animal movement nationally, as animals generally traveled only from breeder to user and went from breeder to breeder only with the bureau’s knowledge. This provided a relatively sophisticated means of continuous surveillance that allowed the bureau to control the movement of animals, and the spread of potential infections, with minimal effort. Importantly, the origins of any reported disease outbreak could be quickly pinpointed and the problem contained. At the same time, annual inspection of breeders’ facilities allowed the bureau to note best practices; thus the regulation and constitution of standards co-evolved.

Lane-Petter’s primary concern was preventing the spread of infection. Breeders were encouraged to be vigilant and to suspend trade voluntarily should infection be suspected. Prior to the advent of the bureau and its new practices latent infection revealed itself only when an animal was placed under stress, such as when traveling from breeder to laboratory or during a scientific procedure. Consequently, it was predominantly a problem faced by users. Encouraging breeders to report even minute signs of ill health in their stock was part of a deliberate strategy to relocate the problem of infection from the site of use (the laboratory) to the site of production (the breeder). Here, Lane-Petter could be described as engaged in manufacturing what W. E. Bijker has called a “technological frame,” intended to allow productive interactions between users and producers, around the newly constituted laboratory animal—just one example of how the bureau facilitated the constitution of a

shared social community from previously incongruent but not incommensurable practices.²¹ The disparate practices of users and breeders were reconciled—cognitively, socially, and materially—within a new community centered on the laboratory animal. Breeders had little understanding of the needs of the biomedical sciences, while animal users prioritized their work over questions of animal health (not always being willing to recognize that the two were intertwined). Governing the relations of users and producers so that their agendas, behaviors, and practices coalesced allowed these differences to be overcome. This can be seen in Lane-Petter's construction of a nationwide pathology service in 1949: accredited breeders were allocated to a local laboratory willing to autopsy animals to detect signs of infection. The Public Health Laboratory Service and the Veterinary Investigation Service were persuaded to provide this service gratis because, as Lane-Petter argued, it ultimately accrued to their own benefit. Helping breeders to identify infections would improve the quality of animals available for scientific use. When an infection was detected in a breeder's stock, trade would be suspended and a representative of the bureau would take appropriate action—partial or total slaughter, followed by thorough disinfection of the premises—before accreditation was reinstated and the breeder returned to business.²² Thus, regimes of surveillance, prevention, and control encouraged vigilance, compliance, and voluntary reporting among breeders. Other aspects of the scheme reinforced the internalization of discipline. For example, Lane-Petter established insurance protection against losses from “deaths from authorized slaughter; deaths from accident; and deaths from disease” that was deliberately intended to encourage self-policing via the provision that the insurance would be in effect only when “such deaths have been notified and submitted for laboratory examination.” Insurance also worked to standardize the risks infection posed to laboratory animal producers, further regulating risk while relocating it from the user to the breeder.²³ While appearing to be a relatively liberal scheme, operating by self-policing and internalized discipline, accreditation was nonetheless effective in allowing the bureau to govern the national production of laboratory animals at a minute level. At the same time, the scheme worked to bring a national population of such animals into existence.

A second technology of governance developed by Lane-Petter to address the notorious instability of the commercial market for laboratory animals worked by similar modes of nonrepressive discipline. Laboratory animal production was unstable for many reasons, a primary one being outbreaks of infection that caused sudden shortages in affected species. Seasonal breeding patterns also contributed to the problem, as did a general lack of communication between breeders and users. For example, when users embarked on new animal-intensive procedures, simply assuming that the local market could supply animals without advance notice, shortages inevitably followed. Whatever the cause, shortages raised the cost of a given species, which encouraged breeders to increase production to maximize profitability—inevitably leading to gluts and collapsing prices that provoked new shortages as breeders cut back on production.²⁴ To break these cycles and stabilize the market, the bureau instituted a bimonthly publication listing all known laboratory animal breeders in Britain, their available stock, and their accreditation status, further reifying the national market for laboratory animals by inscribing it in a material and accessible form. The publication was titled *Parade State*, in reference to the military practice of regularly reporting the strength of the nation's armed forces to the Secretary of State; analogously, *Parade State* presented a bimonthly account of the nation's laboratory animals.²⁵ Lane-Petter deliberately evoked an association between laboratory animals and the military to emphasize the importance of the former in safeguarding the national health. Laboratory animals should be a fundamental state concern, he argued, as “the health, wealth, and happiness of man ... look not least to medical science for their realization; and ... this being so, [medical science] must be fed with the very stuff of medical research, good laboratory animals.” Furthermore, in the new era of the British welfare state the supply and well-being of laboratory animals had important ramifications for the national economy: “The cost of laboratory animals, the

cost of medical research, and the cost of maintaining the health of the population are in steeply ascending order of magnitude. Wise policy begins at the beginning, which is also the cheapest end of the scale.”²⁶ Just as the militaristic parade state required continuous surveillance of the state’s military strength at the microlevel of the rank and file, so too did the accreditation scheme and *Parade State* address the strength and reliability of the laboratory animal population. Just as the parade state had constituted the military and made it visible in a way that simultaneously necessitated and made possible intervention and regulation, so too had the bureau’s regimes of governance. Moreover, both were justified in terms of their role in national defense: the former by protecting against militaristic aggressors and the latter by safe-guarding health and economic production against disease and degradation. Equally, both operated through the “power of writing” inscribed in the material practices of forms, record keeping, bureaucratic administration, and the like. Regimes of continuous surveillance operated to universalize and individualize separated individuals and encourage the internalization of regimes of governance legitimized by standardization discourse. Both the accreditation scheme and *Parade State* might thus be viewed as examples of disciplinary power focused on life, a form of Foucauldian biopower applied to nonhuman animals.²⁷

Both the accreditation scheme and *Parade State* deployed the discourse of standardization to constitute, regulate, and reify the national market for and the population of laboratory animals. These mechanisms allowed Lane-Petter to triangulate the needs of users and breeders while fashioning a shared understanding of what laboratory animals should be. Simultaneously, the work of the bureau contributed to constituting the interrelated meanings and identities of what it was to be a user or producer of laboratory animals, a process that developed in tandem with the construction of the bureau’s own identity. Equally important, these new mechanisms established the bureau as an authority at the center of British laboratory animal production and supply. From this position, Lane-Petter could work toward the standardization of the production of laboratory animals themselves—an effort that saw the bureau move beyond its initial bureaucratic remit and toward becoming a research institute.

PATHOGENIC STANDARDIZATION

While the MRC allowed considerable freedom to directors in determining the work and development of their units, the Laboratory Animals Bureau was additionally governed by an Advisory Committee on Laboratory Animals to ensure that it met the needs stipulated by the Conference on the Supply of Experimental Animals. To this end, the committee comprised expert users of laboratory animals known to the MRC along with representatives of the conference. These categories were not mutually exclusive—Kenneth Mather, for example, had worked closely with the conference but was appointed to the committee on account of his expertise and not as a representative of the conference *per se*. The goal was to temper the more radical demands of Bacharach, a socialist and a conference representative, to form a balanced, apolitical committee capable of directing the bureau toward necessary and achievable aims. Both Edward Mellanby, then Secretary of the MRC, and his deputy Landsborough Thomson, were sensitive to the left-wing agenda of the Association of Scientific Workers and had no desire to allow politics to infiltrate the activities of the council. Regulating the market in animals was troubling enough; nationalizing the market as Bacharach wished was beyond the pale. Politics, as well as prior experience, formed the context of the council’s demand that the bureau be confined to a bureaucratic remit. The establishment of a national supply of state-produced standardized laboratory animals was too socialist in design and too reminiscent of left-wing state planning for the MRC to countenance.²⁸ Despite the new Labour government’s establishment of the welfare state and an unprecedented program of industrial nationalization, advocates of the state planning of

science had met fierce resistance from the Freedom of Science movement. Such ideological differences, however, were quickly resolved, and after the conclusion of World War II science, industry, and the British state found consensus in a not-quite-socialist yet not-quite-democratic settlement.²⁹ The 1947 establishment of the Advisory Council on Scientific Policy, for example, which brought together representatives from the research councils, scientific experts, and industrial leaders to advise government about scientific policy in peacetime, embodied this new consensus. While reminiscent of and informed by American developments associated with Vannevar Bush's *Science—The Endless Frontier*, it was nonetheless a distinctly British compromise between socialist-style state planning and the democratic ideal of a liberal free market.³⁰ This political context was reflected in the Advisory Committee on Laboratory Animals and in the work of the bureau. The accreditation scheme, for example, embodied this consensus in that it ostensibly maintained a free market in laboratory animals and yet, as we have seen, worked toward establishing state governance via voluntary disciplinary mechanisms that coerced rather than required compliance.

The success of the accreditation scheme, however, masked a serious problem. The bureau was established at the center of a powerful yet simple governmental system, but it relied on the state for its authority through the MRC. The bureau could not yet claim to possess demonstrable expert knowledge, although such knowledge was often implied in its dealings with both the producers and the users of laboratory animals. Since 1947 Lane-Petter had worked to create a database to consolidate existing knowledge of animal breeding, which was scattered across disparate disciplinary journals. This unique information resource was intended to provide the bureau with an authority based on expert information. However, the distinct contexts and disciplinary assumptions that informed the diverse existing literature made for conflicting claims that were difficult to reconcile into unambiguous recommendations. Moreover, visits to accredited breeders had revealed a wealth of anecdotal practices that further complicated matters. Increasingly, Lane-Petter came to argue that, in order for standardized methods of laboratory animal breeding and management to be established, a disciplinary identity based on authoritative expertise would have to be forged. Consequently, the bureau would have to develop a research role. Writing to Landsborough Thomson in 1950, Lane-Petter explained that he would “not like this Bureau to become little more than a post office, for that would put a fatal limit on its usefulness.”³¹ The constraints imposed by existing circumstances were demonstrable, for example, in the fact that the accreditation scheme existed for the purpose of regulating but lacked definitive regulations to enforce. Lane-Petter identified three areas requiring urgent research: the detection and eradication of latent infection, cost-effective methods to mass-produce standardized animals, and the identification of the best genotype to guarantee a standard phenotype. The detection and eradication of latent infection was deemed most urgent, as Lane-Petter suspected it underlay all aspects of animal use. For example, he speculated that latent infection might increase phenotypic variation during development, thereby explaining recent uncertainties as to whether genetic standardization produced phenotypically uniform laboratory animals (a subject I will return to later in this essay). The MRC recognized the “obvious practical importance” of the proposals but rejected them owing to financial constraints. Lane-Petter, however, saw no logic in the MRC's conserving funds in order to use them to support research that would be compromised by the unreliability of the laboratory animals it depended on. He interpreted the decision as indicating that the council did not want the bureau to have a research role. While the MRC was indeed experiencing financial problems at this time, Lane-Petter was correct in his suspicions. Landsborough Thomson had privately agreed with Alan Parkes, then chair of the Advisory Committee, that it was not in the council's interest to allow the bureau to pursue infection research.³² In the terse exchange of views that followed, Lane-Petter managed to turn the MRC's resort to pleas about financial stringency to his own advantage, arguing that since the council agreed in principle as to the

importance of the research objectives he set out it therefore could not object to his seeking alternative sources of support. Outmaneuvered, Landsborough Thomson agreed that Lane-Petter could do as he wished, on the condition that the MRC undertook no responsibility itself.

A number of pharmaceutical companies had previously contacted the bureau seeking advice on disease control. Lane-Petter now approached them, and others, with an outline of proposed research and its potential benefits, seeking to build a coalition of support for bureau-led research. As major users of laboratory animals for research and standardization purposes, and always cost conscious, pharmaceutical companies proved especially receptive to Lane-Petter's proposals. Several agreed to subsidize the research and continued to sustain it over a number of years, as the development of preventative measures against infection promised substantial future savings.³³

Commercial interests determined that research should begin with mice, as they were both the most widely used species and one in which infection was a recognized problem.³⁴ In 1951 Lane-Petter began a vaccine trial with the hope of eradicating endemic ectromelia infection in mice. Vaccination, as opposed to eradication by slaughter, has been seen by historians as a continental European practice.³⁵ It is likely that Lane-Petter was drawn to the practice because of his medical background. Volunteer breeders working with vaccine lymph provided by D. McClean of the Lister Institute vaccinated mice over a period of two years, with mixed results. While noting that "the ease with which it can be applied makes it a practical proposition for large scale breeding, even by commercial breeders with no scientific training," Lane-Petter recognized that he lacked the expertise to assess the success of the vaccination program and that the bureau lacked the facilities to do proper studies.³⁶ The bureau required a full-time microbiologist to assess the efficacy of vaccination; to this end, A. A. Tuffery was appointed to the staff on 23 March 1954. Having been established as a bureaucratic unit, the bureau lacked the laboratory equipment and space necessary for Tuffery's work. Lane-Petter had worked hard to acquire the limited laboratory space—within the MRC's buildings at Hampstead—necessary for his own investigations, and he was aware that the MRC would not support any official request for larger facilities. Knowing that the Microbiological Research Facility (Porton Down) suffered from recurrent outbreaks of Tyzzer's disease, however, Lane-Petter negotiated the use of facilities there from the Ministry of Supply gratis. This quid pro quo offer of "expertise" in return for facilities was characteristic of Lane-Petter's managerial style (compare, for example, the accreditation scheme's pathology service). Such arrangements kept financial costs low, allowing Lane-Petter to make maximum use of the flexibility he enjoyed in determining how his budget should be spent on the bureau's work.

Tuffery began, in a move now characteristic of the bureau, with a nationwide census of mouse infection, opening up new territories for intervention and governance.³⁷ Like the animal census before it, this constituted as much as quantified endemic infection within the national mouse stock while further developing the identities of and relationships between users and breeders. For example, by triangulating the responses of different laboratories it was possible to demonstrate how users varied in their knowledge of, and interest in, infection. Those respondents who reported infection generally did so only when animals had shown explicit symptoms of disease—most commonly unexplained death. The exceptions were those whose research was in some way related to the pathogen described. Instances where laboratories obtained animals from the same breeders but reported different experiences of infection were interpreted as failures in users' awareness of endemic infection, as when Tuffery asked respondents to look for a specific pathogen it was generally found.³⁸ In this way, users' practices were problematized: their assumptions were shown to help constitute infections as "latent." By definition, latent infections were assumed to be

hidden, almost impossible to detect—“latent” connoting presence without manifest exhibition or development. This language, Tuffery contended, encouraged the expectation that infection would not readily be found. This assumption was common among users, many of whom claimed that routine screening for infection was impractical, requiring so much time as to preclude any real research being undertaken. This was, as Lane-Petter argued, short-term reasoning producing a false economy, since by corrupting experimental results infection proved costly in the long run.³⁹ Linking the economy of experimental science to its reliability again served to emphasize the importance of laboratory animal provision as the first consideration of biomedical research.

Having identified the negative expectations caused by labeling infection as “latent,” Tuffery suggested adopting “inapparent” as a more positive term that would encourage the expectation of infection. It was hoped that such a conceptual adjustment, alongside efficient new practices of detection, would render infection easily visible. However, developing simple, efficient, and effective practices of detecting and eradicating infection proved difficult. It was simply not known, Tuffery lamented, “how and why diseases spread in mouse colonies, how they get in, and how they persist, or die out, how husbandry can affect disease incidence (control), and which factors in mouse husbandry are the important ones, from the disease and control point of view (i.e. hygiene).”⁴⁰ Thus Tuffery had to develop epidemiological practices to trace the ecology of endemic infections in order to construct measures of control and eradication. As a result of his identifying vectors of spread—for example, via air, food, and, most important, human handlers—more robust standards of hygiene became incorporated into laboratory animal production (via the accreditation scheme) and use (through recommendations to laboratory users). However, methods of combating infection other than slaughter and disinfection ultimately proved impractical. Vaccination and chemotherapy proved costly in terms of time and money. It also seemed impossible to guarantee that an animal population was infection free, even after total slaughter, environmental disinfection, and restocking.⁴¹ The only way to do so was to screen every individual by autopsy for each known pathogen—which was obviously impractical. Even with random sampling, by 1957 more than thirty different endemic pathogens had been identified for mice alone. Extending the results of screening from a sample to a whole colony could be a statement only about the likelihood of infection, the accuracy of which was directly proportional to the cost and effort invested.

Nevertheless, Tuffery’s research into latent infection during the 1950s was productive. His epidemiological studies helped establish standards of material accommodation (including the overall setup, bedding materials, ventilation, protection against vermin, caging, means of transportation, etc.) and practices of husbandry (hygiene, diet, general management) that were intended to minimize the spread of infection within animal stock and that were applicable to accredited breeders and laboratory users alike. One effect was the configuring of laboratory animal users—their practices, their expectations, and, most important, their deficiencies—in Steve Woolgar’s sense of “configuring” as “defining the identity of putative users, and setting constraints on their likely future actions.” Thus users were constituted as subjects open to intervention and reform, as were breeders. These processes were not independent but co-constitutive—users and breeders were coerced into constituting themselves and their relations through the bureau. The process of configuration, therefore, was less a process of constitution than one of dynamic co-constitution. For example, Lane-Petter and Tuffery drew on the practices of breeders and users, via observations related to accreditation, their voluntary participation in vaccination trials, and visits to laboratory animal houses, to produce a regulatory frame that would enable them to govern the production, supply, and use of laboratory animals. Standards of production relied as much on the cooperation of users as on the practices of breeders, whether this involved agreeing to purchase animals only from accredited breeders, adopting stringent approaches to hygiene,

establishing barriers between stocks sourced from different breeders, or communicating future needs to breeders in advance to facilitate adequate production. In this sense the bureau acted to triangulate user-producer relations in a process that saw the dynamic co-constitution of both within a shared frame of understanding.⁴² By the mid-1950s, the bureau was acting to govern a newly constituted community grounded in the materiality of the laboratory animal, transcending its intended bureaucratic remit of collating and disseminating information. The identities of breeder-as-producer and laboratory-as-user remained fluid, as did that of the bureau itself. By acting to triangulate these relationships, identities, material practices, and needs, Lane-Petter further transformed the bureau, so that by the close of the decade it had itself become both a user and a producer of laboratory animals.⁴³

GENETIC STANDARDIZATION

The prominence of *pathogenic* standardization in the bureau's work invites explanation, as historical analysis has hitherto prioritized practices of *genetic* standardization.⁴⁴ In part, this is a consequence of existing studies having centered on the American national context and prioritized the disciplinary practices of genetic and cancer research. It is likely that disciplinary practices played a greater role than national context in determining the prominence of modes of animal standardization. Since heredity was integral to the conceptual framework of genetic and cancer research, genetic standardization might be expected to be the norm. Importantly, as genetic standardization made breeding an experimental practice undertaken within the laboratory, those disciplines that prioritized such practices became isolated from problems associated with the commercial market. Nevertheless, until the late twentieth century cancer and genetic research used small numbers of animals compared to routine experimental science as a whole. For the majority of the twentieth century, the main use for laboratory animals was in routine procedures, such as biological standardization and medical tests, which became prevalent during the interwar period and made biomedical science increasingly dependent on the commercial market.⁴⁵ For example, when the British journal the *Lancet* reported the development of the Zondek-Ascheim pregnancy test in 1930, the authors noted that its widespread use would increase medical science's dependence on the commercial market for animals.⁴⁶ It was these shifts in animal use that instigated demands for a national supply of standardized animals in Britain, resulting in pathogenic standardization being a major focus of concern to the bureau. This is not to say that pathogenic standardization was *only* a British concern. When the American Institute of Laboratory Animal Resources established its own accreditation scheme in 1956 it was modeled on the British example, addressing the problem of endemic infection. However, comparing the British and American schemes reveals that the former placed much greater emphasis on pathogenic standardization whereas the latter prioritized genetic standardization.⁴⁷ This was because of the different disciplinary origins of concern over, and demand for, standardized laboratory animals. In Britain, the demand for national standardization came from routine disciplines dependent on the commercial market for the large numbers of experimental animals they required. In America, comparable voices appear to have been less involved in debates about animal standards. The American accreditation scheme was established primarily to meet the needs of the Cancer Chemotherapy National Service Center; consequently, genetic and cancer research motivated American demands for standardized laboratory animals, and the needs of such work called for genetic standardization.⁴⁸

Nor is this to say that geneticists were uninvolved with the campaign for and the work of the bureau. It is telling, however, that Bacharach, a biochemist, was the strongest advocate for genetic standardization on the Advisory Committee. Bacharach had been among the first in Britain to adopt the inbred animal as a standard tool (so reliable as to be "the litmus paper of the vitamin chemist"). In 1950 he continued in this vein, arguing that the bureau should act

as a “gene conservatory,” working to produce and conserve inbred animals. As a socialist, Bacharach viewed regulation as a poor second to the nationalization of the production of standardized laboratory animals.⁴⁹ There was no market mechanism, he complained, to conserve animal strains that had fallen into disuse but must, nevertheless, be maintained for scientific posterity. The argument that inbred strains could not be entrusted to a commercial market presented Lane-Petter with an opportunity to expand the work of the bureau that he was glad to take. Allying with Bacharach, Lane-Petter forced the MRC to appoint the geneticist Hans Grüneberg to the Advisory Committee to provide expert advice on the worth of genetic standardization.⁵⁰ Far from advocating the reliability of inbred animals, however, Grüneberg cautioned against the widespread assumption that genetic standardization produced phenotypically uniform laboratory animals. In part, then, the bureau was slow to promote genetic standardization because the reliability of inbred animals came into doubt in the 1950s. But there were other reasons as well.

For Lane-Petter, at least initially, genetics was important in terms of the economic aspects of the inheritance of breeding productivity.⁵¹ Developing intensive breeding practices was a more urgent and pragmatic matter than investigating inbreeding, as the former could be directly recommended to breeders. In contrast, even if genetic standardization produced reliable animals, there was significant doubt as to the capacity of breeders to practice inbreeding. While orchestrating the work of the conference, Bacharach had been appalled to discover that many breeders “cannot spell their names in block letters” and so could scarcely be trusted “to maintain the rigorous inbreeding essential for the maintenance of pure lines.” Such questions about breeder competence emerged in part from disparate understandings of the meaning of inbreeding across the biomedical and breeding communities. Biomedical science defined “inbreeding” as exclusive brother-sister mating over twenty generations—which, in the words of Bacharach, necessitated a “considerable amount of careful record making.”⁵² Breeders also practiced what they referred to as inbreeding as a means to stabilize an animal strain, but the practice was not rigorously defined and it remained but one among several techniques, in which selection by eye and the introduction of “new blood” also played a role.⁵³ Known as “breeding to type,” these practices integrated nineteenth-century techniques with modern genetic understandings of heredity. Thus, while opposed, these disparate regimes of inbreeding were not unrelated and shared practices and a body of knowledge. Lane-Petter, for example, attempted to eradicate endemic infection using selective breeding. Prior to being eclipsed by material and conceptual transformations such as the advent of antibiotics and a retreat from explanations of character grounded in genetic science, the inheritance of resistance to infection was a relatively large field of research.⁵⁴ Yet it is equally possible that Lane-Petter encountered these concerns in his observation of breeders’ practices. Whatever the case, and despite Landsborough Thomson’s insistence that the bureau “not actually handle any animals,” Lane-Petter began surreptitiously experimenting with methods of intensive breeding of mice, having acquired a pair from a local schoolboy in 1949. Diplomatically stressing in 1952 that “the Bureau was not primarily a research unit,” he nonetheless used his prerogative to expand the staff to include a student laboratory technician and a part-time animal technician to investigate “simple problems of breeding and feeding.”⁵⁵ It was not, then, that Lane-Petter was unable to constitute a shared frame for users and producers within which inbreeding as a form of standardization could serve the needs of industrial *and* scientific production. Rather, the bureau did not initially promote genetic standardization because the role of inheritance in the economics of production was considered a more pressing practical concern.

This said, Lane-Petter was merely postponing the construction of such a shared frame. He did so for the pragmatic reasons already discussed, but also to capitalize on Grüneberg’s skepticism, which presented the opportunity to forward his demand that the bureau pursue original research. Grüneberg privately warned in 1950 that the role of inbreeding in the

production of standardized laboratory animals was becoming increasingly unclear, as genetically standardized animals appeared unexpectedly variable.⁵⁶ That inbreeding produced genetic standardization was not in question, but the widespread *theoretical* assumption that genetic standardization produced phenotypic standardization now was. Doubts crystallized in 1954 with a remarkable flow of publications, beginning with a paper by Grüneberg in an April issue of *Nature* that described dramatic skeletal variation in genetically standardized mice.⁵⁷ In September J. D. Biggers and P. J. Claringbold published a literature review showing that the link between genetic standardization and phenotypic uniformity lacked experimental substantiation and, furthermore, that such experimental evidence as existed appeared to call the connection into question. Had the supposed uniformity of inbred animals been nothing but a mathematical hoax? Grüneberg thought so, and he demanded that theoretical expectations now be “confronted with reality.” Others agreed. For example, Anne McLaren and Donald Michie independently reported the results of just such an investigation, concluding that “the widely adopted policy of using inbred strains for bioassay may be a mistaken one.” A theoretical explanation for these experimental findings was provided by the principle of “genetic homeostasis,” described by the American poultry geneticist Israel Michael Lerner in his 1954 book of that title. Heterozygous genotypes, Lerner claimed, were better able to regulate development toward uniformity by resisting environmental change.⁵⁸ Inbreeding produced homozygosis, thereby destabilizing development and increasing phenotypic variation. By the close of 1954 evidence and theory had coalesced to bring about a seismic turnaround in the assumptions about the genetic norm of an organism. Subsequently, far more emphasis was placed on the environmental determination of phenotype—an emphasis underlying the bureau’s interest in husbandry and pathogenic standardization. Moreover, the new literature tended to imply that heterozygous genotypes were more vigorous and healthy. Uniformity and health were thus grounded in difference; it was now good to be hybrid.⁵⁹

By 1956 biologists found themselves “completely at sea” as to the utility of genetically standardized inbred animals *as reliable standard laboratory animals*.⁶⁰ In this context, there was no reason for the bureau to act as a “gene conservatory” and no justification for advocating genetic standardization to commercial breeders. Clarification as to the most suitable genotype for producing standardized laboratory animals was, however, urgently required. In 1954, with the support of the Advisory Committee and a number of prominent biomedical scientists, Lane-Petter secured MRC support for research into comparative assessments of the utility of inbred, cross-bred, and conventional laboratory animals. A small budgetary increase for the bureau was also allowed, much to Lane-Petter’s surprise, though it was emphasized that further expansion would not be favorably considered, particularly with reference to the production of animals. Landsborough Thomson repeatedly reminded Lane-Petter that the council “would not wish to enter into deliberate competition with commercial breeders.”⁶¹ Nonetheless, Lane-Petter set out to achieve just that.

A young biologist, Joyce L. Bloom, was appointed to systematize Lane-Petter’s existing colonies within an “Experimental Breeding Unit.” Bloom was also tasked with creating inbred strains of mice for use in the comparative research, a lengthy technical process requiring the careful selection of breeding pairs, continual surveillance for mutation, and laborious bureaucratic record keeping.⁶² While inbred strains were being established, Bloom and Lane-Petter worked to adapt the latter’s research on intensive production into a means to mass-produce and supply inbred animals. That this work began immediately, before it was certain that inbred animals were in fact desirable, is an indication of Lane-Petter’s long-term ambition that the bureau should become a center for the production and supply of specialized animals. Mass-producing inbreds for supply posed problems, as there was no guarantee that the recipients of inbred animals possessed the special expertise required to maintain the genetic standard. Moreover, maintaining strains in different locations increased

the risk that animals would differentiate from the standard, whether through human error, mutation, or genetic drift. To circumvent these potential problems, Bloom and Lane-Petter developed a system to minimize the expertise required at the user's end and the risk of strain divergence. Production was split between a "primary colony" (where strains were expertly maintained) and "production units" (which could be located anywhere to produce animals for experimental use). Genetic differentiation was thought to be unlikely for up to three generations if breeding was constrained within the same generation. Maintaining production units thus required minimal expertise. They operated by a simplified "traffic-light" system: the original breeding stock from the primary colony were maintained in white-labeled cages; their progeny were placed in green cages; green-cage progeny were placed in yellow cages; and yellow-cage progeny were placed in red cages (as the third generation, these animals were never used for breeding). All generations were genetically standardized and suitable for experimental purposes, and each generation except the third could be used for breeding with animals in same-colored cages. By conserving expertise at one location (the primary colony) and making mass-production relatively undemanding, this system allowed genetically standardized animals to be reliably supplied to even the least skilled laboratories throughout the world.⁶³

The work of developing inbred lines and testing the traffic-light system resulted in the production of large numbers of surplus animals. The latter project, in any case, required the "experimental" distribution of animals to user-based production units. Finally, there remained the question of whether genetically standardized, hybrid, or conventionally outbred animals were most reliable for experimental research. Taken together, these points allowed Lane-Petter to establish the bureau as a producer and supplier of experimental animals, albeit on an "experimental" and noncommercial basis. From the summer of 1956 inbred mice were offered for experimental purposes, on the condition that users made a "comparison of their usefulness." The bureau provided healthy animals to laboratories at no cost; in return, Lane-Petter was supplied with valuable information on the characteristics and usefulness of the different strains that the bureau lacked the personnel and expertise to acquire on their own. This characteristic quid quo pro arrangement allowed Lane-Petter to pursue the agreed research in the most efficient way possible while simultaneously, and quasi-legitimately, pushing the boundary of the bureau's remit almost to the breaking point. Distribution of the bureau's animals was likely relatively small scale, as there is no evidence of complaints from commercial breeders. Nevertheless, by 1957 Lane-Petter reported that bureau strains were "breeding satisfactorily" and that the "farming out technique for producing large numbers of inbred mice in the user laboratory is progressing satisfactorily and expanding rapidly." With inbred strains established, Bloom left the bureau to pursue a genetics course at Edinburgh University.⁶⁴ Her replacement, Annie M. Brown, was better qualified to investigate the comparison of inbred, hybrid, and conventionally bred animals. Collating the reports of users with her own findings, Brown showed that no specific genotype was universally more reliable than another. Rather, the standard came to depend on the context of the work, and the user ultimately determined which standard was "the best tool for their job."⁶⁵ Brown collated detailed records of the characteristics of different animal strains to guide prospective users, allowing the bureau to provide a range of standardized animals appropriate for various experimental practices.

Between 1953 and 1958 Lane-Petter transformed the bureau into a center for the production and supply of laboratory animals. Tellingly, in this period not one report on the bureau's activities, which had previously been produced annually, was submitted to the MRC.⁶⁶ By the time Lane-Petter accounted for the activities of these years, the transitions he had instituted were officially recognized by the MRC. No longer restricted to a bureaucratic role, the Laboratory Animals Bureau was renamed the Laboratory Animals Centre in 1958 and expanded further in subsequent years.

A BRAVE NEW ANIMAL FOR A BRAVE NEW WORLD: STANDARDIZATION AS A VECTOR OF INTERNATIONALIZATION

From the mid-1950s Lane-Petter began to articulate an agenda geared toward making laboratory animal production, husbandry, and management “more of a science and less of a craft” by establishing a subdiscipline of “laboratory animal science.” The new field was to be international in scope, with the bureau at its center—a credible ambition, as standardization discourses had already proven capable of constituting internationalized disciplinary knowledge. Biological standardization, for example, was a notable achievement of the League of Nations and had also established the international reputation of the MRC.⁶⁷ When Lane-Petter sought the support of UNESCO in 1956 for an international meeting on laboratory animal standards, he was emulating the process followed by Henry Dale in the 1920s. Dale orchestrated the constitution of biological standardization as an international discipline via the League of Nations and ensured that the NIMR became a custodian of biological standards, and thus a necessary passage point for biomedical science worldwide.⁶⁸ The bureau and UNESCO shared a heritage in that Julian Huxley, UNESCO’s first director-general, was a past president of the Association of Scientific Workers and had recruited like-minded socialist-humanist friends to UNESCO in 1946.⁶⁹ In 1933 Huxley had publicly recognized that “science is influencing world structure,” observing that “a really good programme of research cannot help being essentially international in its approach” because “standardization is just as important for applied biology—in medicine, veterinary science, agriculture, and public health—as it is for applied physics and chemistry in industry.” That scientific standardization was internationalizing, Huxley argued, offered Britain, “as a small and centralized country, with world wide connections, and a high professional and scientific standard,” an opportunity for “the export of brain work” that “could become an important item in our economy if we took steps to encourage it.” By 1956 Huxley’s term of office had long since ended; but his influence remained, ensuring that UNESCO would fund a meeting in Paris to discuss the problem of laboratory animal supply on a global level.⁷⁰

The Paris meeting resulted in UNESCO agreeing to support a new body, the International Committee on Laboratory Animals (ICLA), to pursue on a global scale what the British bureau had begun on a national level. Lane-Petter, appointed honorary secretary, shaped the work of the ICLA by borrowing from the practices of the bureau.⁷¹ Consequently, a global census of the laboratory animal population was among the first tasks the ICLA undertook. Equivalents to the British bureau had emerged in the Netherlands (Centraal Proefdieren Bedrijf TNO) in 1949, in the United States (Institute of Animal Resources) in 1952, and in France (Centre de Sélection des Animaux de Laboratoire) in 1953. The ICLA catalyzed this trend, as further organizations were established in the Federal Republic of Germany (Zentralinstitut für Versuchstierzucht) in 1957, Belgium (Comité National Provisoire sur les Animaux de Laboratoire) and India (Laboratory Animal Information Service) in 1958, and Australia (Laboratory Animal Centre) and Italy (Centro di Informazioni sulla Produzione degli Animali di Laboratorio) in 1959, among others. Full analysis of the work of these institutions cannot be undertaken here, but it is worth noting that they closely reflected the trajectory of the British bureau. For example, the French Centre de Sélection des Animaux de Laboratoire began the production of specialist strains in 1957—about the same time as the British bureau.⁷² One reason the British model was so influential is that Lane-Petter had begun visiting laboratory animal users throughout the world in the early 1950s. Consequently, the bureau became internationally known and functioned as “a sort of a Mecca that attracted people from all parts of the world” who wished to learn about “laboratory animal science.”⁷³ Importantly, this process was not unidirectional. These international interactions operated as reciprocal exchanges, enabling the bureau to absorb

practices from across the world while innovating on and further disseminating them. Thus laboratory animal science was dynamically constituted, as were international standards of practice and production. An important example was Lane-Petter's adoption of gnotobiotic (so-called known life or germfree) technology for the pathogenic standardization of animals from the late 1950s.⁷⁴

From 1953 Lane-Petter began making annual visits to America, where he toured universities, research institutes, pharmaceutical laboratories, commercial breeding establishments, and other places of scientific interest and attended the yearly conferences of the Animal Care Panel, an organization established in 1950 to promote the welfare of laboratory animals.⁷⁵ It was at the 1957 conference that he encountered gnotobiotics, the science of creating "germfree" organisms by isolating them from all microorganisms using the latest sterilization and barrier control technology. Germfree science had developed piecemeal since the late nineteenth century, becoming feasible for widespread use only in the late 1950s, when Philip C. Trexler found a way to use plastic to make gnotobiotic technology affordable.⁷⁶ When Trexler showcased his new methods at the 1958 meeting of the Animal Care Panel Lane-Petter recognized that they would allow for the production of pathogenically standardized animals free of infection. This would simplify practices of infection surveillance considerably and resolve many of the difficulties Tuffery had struggled with. He returned to Britain convinced that the bureau should pursue a new and more "sophisticated" approach to laboratory animal production, drawing on gnotobiotics. Several American laboratories had begun to develop pathogenically standardized animals in the late 1950s; in Britain only ICI had moved in this direction.⁷⁷ Nobody, however, had attempted to combine pathogenic standardization with genetic standardization to produce a new standard of laboratory animal. Achieving this, Lane-Petter decided, would form the basis of the future work and international reputation of the bureau.

No organization, Lane-Petter reported to the MRC in 1958, was at present capable of producing genetically and pathogenically standardized animals and making them freely available. This threatened to undermine scientific work—for what good was a standard that was not universally available? To ensure that the standard laboratory animal could "yield its full potential" of "standardization and reliability" and "serve all laboratories in the country," it was "essential" that the bureau be reconstituted as a larger "Laboratory Animals Institute." The proposal was timely: a break in policy continuity had occurred as a result of Landsborough Thomson's retirement, and the MRC possessed a good-sized vacant building at Carshalton owing to the closure of the Serum Research Laboratory.⁷⁸ Lane-Petter's proposals were approved by Sir Harold Himsworth, who had replaced Mellanby as MRC First Secretary in 1950, allowing the bureau to move to Carshalton and consolidate its work—geographically, practically, and conceptually—in an expanded organization renamed the Laboratory Animals Centre. The bureau's two main strands of research, Tuffery on infection and the Experimental Breeding Unit, now at Carshalton, were expanded and eventually combined to produce considerably more than the sum of their parts—a new standard of laboratory animal, pathogenically and genetically standardized.

For this purpose, Carshalton's disused stables were redeveloped into an innovative new building wherein regimes of hygiene were developed to new levels of sophistication. The architectural design allowed for the control of every environmental variable in nine breeding rooms, a large subcultivation room, and a room for experimental research (see Figure 2). Airflow was carefully governed to create a pathogenic barrier between the interior and exterior environments. Above-average air pressure ensured that accidental air movement was always outward rather than inward, and incoming air was passed through a ventilation system to filter airborne pathogens. Similarly, objects entering or leaving the building were subject to stringent hygienic precautions. Equipment, including animal food and bedding,

was variously decontaminated using autoclaves (an apparatus that superheated steam for sterilization), ethylene oxide fumigation chambers, and dunk tanks filled with chemical disinfectants. All persons entering the building were required to strip and shower before clothing themselves in predecontaminated work wear, with personal items restricted to decontaminated spectacles, locker keys, and plain rings. Interaction with other staff, particularly those who did not work in the building, was discouraged. Restrictions even extended into private life, as workers were asked to refrain from interactions with normal animals. Microbial security required such stringency, as any breach would require the whole process to be restarted, with significant costs in terms of both money and time. Perceptive readers may ask of Figure 2 how the central rose garden was tended, as a gardener—and his dirty equipment—could not be allowed to pass through the building. Access was gained by ascending the outside building by ladder, crossing the roof, and descending in the same manner—a process also followed by visitors who wished to look inside the building, as the only windows faced the internal quadrangle. The inclusion of an inaccessible rose garden is indicative of the level of importance placed on “naturalizing” this unusual working environment for its human inhabitants. Lane-Petter hoped to minimize the impact of working in what he described as a “prison environment” by providing a viewable garden as a means to offer workers a sense of the natural in what was otherwise a very unnatural artificially lit environment.⁷⁹

The intention was not to produce germfree animals but, rather, animals that were certified to be free of specific pathogens—specific pathogen free (SPF).⁸⁰ Nevertheless, the passage of animals into the new building was the most carefully controlled element in the whole process. The initial stock of animals consisted of germfree animals sourced from the Lobund Institute at the University of Notre Dame (Indiana, USA) that were carefully exposed to specified pathogens within the new Carshalton building and were intended to serve as foster parents.⁸¹ Subsequently, animals entered the building prior to birth, inside an intact surgically removed and chemically decontaminated uterus taken from a genetically standardized animal at full term. Once inside, these animals were surgically removed from the uterus and fed on milk provided by the American stock—thus they were considered free of all pathogens other than those encountered within the building. This integration of technology and nature required several innovations, such as the synchronization of gestation periods to enable scheduled transference, the development of complex surgical procedures, and the establishment of methods to decontaminate animal food without rendering it toxic. Nonetheless, a stock of genetically and pathogenically standardized animals was established in a remarkably short time. Without this “upgrading” system, which circumvented the time-consuming process of breeding inbred strains from conventionally bred pathogenically standardized animals, combining pathogenic and genetic standardization would have taken years. In 1960, then, when Elizabeth Russell of the Jackson Laboratory (USA) speculated that “in the future genetic control and pathogen-free maintenance should in some cases be combined ... if either discipline is to have its full usefulness,” this process was already well advanced in Britain.⁸² By combining the latest gnotobiotic technology with the natural pathogenic barrier of the uterus, the Laboratory Animals Centre established itself at the forefront of research into, and the mass production of, a new standard of laboratory animal.

In January 1964 the new facility began providing SPF animals on a commercial basis via a modified form of the traffic-light system that ensured that the technical aspects of the production and maintenance of animals remained with the Laboratory Animals Centre while mass production could be undertaken by comparatively less skilled users. By 1965 the strains were well established; sales figures reported as “quite impressive” demonstrated that the program was “fulfilling a necessary function.” The combination of gnotobiotic technology and inbred strains constituted not only a new standard of laboratory animal but a completely new form of life about which little was known. Nevertheless, Lane-Petter was

convinced that SPF animals were “the healthy animals we have been looking for for years ... in a short time the questionable term ‘SPF’ etc will be forgotten, because all laboratory rats, mice and probably every other species will be of this standard of health.”⁸³ Enthusiasm overshadowed the paucity of knowledge, and the latter half of the 1960s witnessed the continued expansion of breeding and research projects at the Laboratory Animals Centre.

This occurred, however, without Lane-Petter, who resigned in 1965 to join the American commercial breeder Carworth, which sought to make use of his expertise and contacts to establish an outlet in Britain.⁸⁴ Perhaps, since the future of the Laboratory Animals Centre now lay in consolidation and expansion, he wished to leave this to others and pursue new challenges. Alternatively, Lane-Petter may have thought industry offered better support than the MRC: his resignation followed one last altercation with the council regarding a request to establish germfree facilities at Carshalton. In 1964 Lane-Petter warned that British gnotobiotic research was in a “backward state” and that there was an “urgent need for the development of the subject,” without which the center’s SPF production would remain dependent on germfree animals imported from America. Much to the annoyance of Lane-Petter, who made a point of recording his dissent from the decision and whose resignation was tendered a few months later, the MRC “felt unable to recommend the proposal.” Over a decade earlier, the council had rejected Lane-Petter’s proposal for research on infection, recognizing its merit only after six years of independent funding. Despite the growth of the bureau into a world-renowned facility for the study and production of laboratory animals, there remained a sense within the council that laboratory animal supply was something it should not have become involved with. Tellingly, Lane-Petter’s successor, the veterinarian John Bleby, successfully established a germfree unit at the Laboratory Animals Centre without the council’s knowledge by drawing funding from the general budget. When representatives of the MRC discovered the unit on a visit three years later, their praise was universal—diplomatically, Bleby chose not to draw their attention to their earlier views.⁸⁵

CONCLUSION

On its twenty-first anniversary, in May 1968, the Laboratory Animals Centre was at the height of its influence and confidently celebrated its history as the first organization in the world to instigate state regulation of the production and supply of standardized laboratory animals. Under Bleby, research and production activities prospered, with the work now reorganized into several subdepartments: information and services (the original remit), accreditation, breeding and genetics, pathology and microbiology, animal health, environmental physiology, and nutrition (see Figure 3 and Cover). The Laboratory Animals Centre was established as an internationally recognized supplier of animals, with sales of genetically and pathogenically standardized animals increasing by 76 percent between 1965 and 1968.⁸⁶ In these years animals were sent to Belgium, Cyprus, Czechoslovakia, France, Holland, Hungary, India, Ireland, Israel, Italy, Kenya, Mexico, Nigeria, Norway, Rumania, Sweden, Switzerland, the United States, and West Germany as well as nationally. Many of these animals were exclusively produced by the Laboratory Animals Centre, most famously the SPF cats developed by Bleby.⁸⁷ From 1968 a new classification system for pathogenic standardization was introduced in recognition that commercial breeders had begun to follow Carshalton’s lead. Animals were categorized according to their pathogenic status, ranging from Category 1 (little if any pathogenic standardization; these animals were suitable for teaching purposes) to Category 5 (germfree animals suitable for gnotobiotic and immunological research). Categories 3 and 4 were SPF animals considered “likely to become the standard laboratory grades.”⁸⁸ In 1971 the facility formalized its products with a new publication, *Standardised Laboratory Animals*, that provided “information on the origin and characteristics of the species and strains maintained at the Centre,” in addition to specifying levels of pathogenic standardization and suggestions for use. The standard animal

BALB/c, for example, was a Category 4 genetically standardized albino mouse, acquired in 1913 by Halsey Bagg, inbred from 1923 by E. C. McDowell, and passed in 1932, in its twenty-sixth generation, to G. D. Snell—from whom it was acquired by Lane-Petter in 1955 (see Figure 4). In 1964 BALB/c was transferred to the SPF facility, before being reestablished in 1969, in its thirty-third generation, as a Category 4 animal. Known characteristics of BALB/c included a low incidence of mammary tumors, a 25 percent chance of developing primary lung tumors, and less than 12 percent susceptibility to leukemia. It possessed a long breeding life and had relatively high blood pressure, with older animals commonly found to suffer heart lesions and arteriosclerosis. In personality, BALB/c was described as “fairly docile.”⁸⁹ *Standardised Laboratory Animals* allowed users to choose a standard animal suitable for their purposes as easily as they might choose any other piece of technology or equipment from a trade catalogue. The Laboratory Animals Centre had moved considerably beyond its intended bureaucratic role as a “clearing house of information” on laboratory animals.

Nonetheless, the bureau’s development was facilitated by the management of information and the authority that came from being a state-funded organization. Access to state-held information drawn from the Home Office and the Ministry of Agriculture had allowed Lane-Petter to constitute a national population of laboratory animals, drive dealers from the market, and establish the bureau as a regulatory body. The bureaucratic process of census, too, facilitated the co-constitution of the identities and relations of breeders and users. This allowed the bureau to develop its own identity at the boundaries of those of breeders and users while facilitating the development of a shared community around the knowledges, practices, and interests grounded in the material product of the laboratory animal. The accreditation scheme, for example, ostensibly acted to regulate, but in doing so it also worked to create identities for, and triangulate relationships between, users and producers. The development of laboratory animal science both facilitated and was an outcome of this process. Disciplinary knowledge worked as a tool to facilitate interaction between social worlds, acting to stabilize and bring coherence across disparate conceptual understandings, material practices, interests, agendas, and social positions.⁹⁰ That this occurred against a specific sociocultural context—or, rather, because of this context—is the meaning of the “brave new world” alluded to in this essay’s title. As Steven Shapin and Simon Schaffer have argued, with a view to the period discussed here, the “practices involved in the generation and justification of proper knowledge were part of the settlement and protection of a certain kind of social order.” The post-World War II development of standardized laboratory animals reflects this truism, but it does not wholly confirm the Mertonian view that only a liberal and open society is capable of sustaining science—the view that Shapin and Schaffer take to be representative of this period. Rather, the postwar settlement allowed for state intervention, regulation, and the provision of laboratory animals under the umbrella of a not-quite-socialist yet not-quite-democratic political consensus that prevailed in late-1940s Britain. Democracy increasingly came to be recognized as fundamental to socialism, and vice versa. Britain at this time was a nation in transformation, arguably united as a nation as never before. Julian Huxley, for example, wrote in 1941 of the need for a new kind of leader who was sensitive to the emerging British “group mind” and capable of facilitating its growth and orchestrating its membership toward cooperative interaction.⁹¹ This notion helps us to understand Lane-Petter’s role in constituting a shared community around the laboratory animal while simultaneously creating his own role and that of the bureau. That a *national* supply of laboratory animals should have emerged at this time was therefore as much a political effect as a response to material change, indicative of the emerging infrastructural arrangements that sustained the formation of late twentieth-century biocapital. Materiality, of course, remained important. Indeed, the argument is not that the identities and practices of the new laboratory animal community, or the animals themselves, were framed by, or in some way reflected, a local or national political “context.” Rather, it is

that they embodied this political settlement, one effect of which was that the local, the national, and the international coalesced in multiple ways about the material laboratory animal. This process was enabled by the language of standardization. A useful comparison might be made to the creation of British Rail, which drew on standardization to create a national network from what had been multiple local networks.⁹² Similarly, Lane-Petter constituted a national market for laboratory animals from existing local arrangements. The railway analogy is pertinent in many ways, beyond the not inconsequential fact that the trains facilitated the work of the bureau as laboratory animals traveled from breeder to user via rail. Standardization discourse was as important to the nationalization of the railways, conceptually and practically, as it was to the production, supply, and use of laboratory animals. Finally, the nationalized railways were transforming Britain into a smaller nation—just as the developing passenger airline industry was constituting a smaller, globalized, world. Science broadly conceived, drawing on standardization, was, in the words of Huxley, “influencing world structure.”⁹³ But in the process it too was transformed.

In being constituted by, within, and because of standardization discourses, the laboratory animal embodied this process—quite literally. The role of laboratory animals as diagnostic tools was to enable accurate experimental replication and thereby guarantee the universality of scientific knowledge as a coherent and truthful account of nature. Yet, as we have seen, coherence was manufactured within a diversity of geographic, material, social, and cultural locations that centered on, and were constituted alongside, the laboratory animals themselves. Disparate geographic locations ranged far beyond the laboratory to include the meeting rooms of the MRC Advisory Committee, the garden sheds in which small-scale fancy breeders seeking accreditation produced their animals, and the animal houses where animals were maintained prior to experiment, among many others. Material factors other than the animal itself included the bureaucratic practices of information gathering, recording, and manipulation; the design of animal cages, water bottles, and types of bedding and feed; practices of hygiene; and, ultimately, in SPF facilities, the micromanagement of life. Social backgrounds ranged equally widely, across classes and educational levels from the upper echelons of the MRC to the working-class breeder, and across disciplinary knowledges from the medical to the veterinary, encompassing breeding, nutrition, microbiology, and more. As the standardization of animals became increasingly international, cultural considerations became ever more evident (why, one might wonder, were the dog, cat, and horse deemed deserving of specific legislative care in Britain?). Laboratory animals served as boundary objects, in the sense articulated by Susan Leigh Star and James Griesemer—that is, they “are weakly structured in common use, and become strongly structured in individual site use, ... have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable.” The standardized animals supplied by the Laboratory Animals Centre, to borrow from Bruno Latour, could be seen as immutable mobiles in that they were considered to have uniform physiological responses and thus stabilized animal-dependent experimental research.⁹⁴ Yet laboratory animals remained fluid enough to be configured in specific ways appropriate to a diversity of possible uses and to be understood outside of science proper—by breeders, among others. This was possible owing to the constitution of a shared conceptual and material framework around standardized laboratory animals that nonetheless retained a plasticity of use. The importance of Lane-Petter, the bureau, and organizations such as the ICLA was to constitute the laboratory animal as such and, in so doing, to bring into being a transnational community that, while disparate, could communicate productively through shared conceptual and material practices.⁹⁵

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References

1. For the epigraph see Conference on the Supply of Experimental Animals SEA/M1/2; File FD1/382, Medical Research Council Archive, National Archives (hereafter NA), Kew, United Kingdom. 1945; Sept.. p. 3
2. Ibid; p. 10-14. On Glover see White EG. Dr. R. E. Glover. University of Liverpool Recorder. 1976; 70:15. On Lane-Petter see Anon. William Lane-Petter, May 9th 1912–January 8th 1988. ILAR News. 1988; 30:18. For breeders' backgrounds see Conference on the Supply of Experimental Animals SEA/M1/2; p. 6-10.
3. See Clarke, Adele E.; Fujimura, Joan H. *The Right Tools for the Job: At Work in the Twentieth-Century Life Sciences*. Princeton Univ. Press; Princeton, N.J.: 1992. Clause, Bonnie Tocher. *The Wistar Rat as a Right Choice: Establishing Mammalian Standards and the Ideal of a Standardized Mammal*. *Journal of the History of Biology*. 1993; 26:329–349. [PubMed: 11623164] Kohler, Robert E. *Lords of the Fly: Drosophila Genetics and the Experimental Life*. Univ. Chicago Press; Chicago: 1994. Gaudillière, Jean-Paul. *The Practices of Human Genetics*, ed. Michael Fortun and Everett Mendelsohn. Kluwer; London: 1999. *Circulating Mice and Viruses: The Jackson Memorial Laboratory, the National Cancer Institute, and the Genetics of Breast Cancer, 1930–1965*; p. 89-124. Logan CA. *Before There Were Standards: The Role of Test Animals in the Production of Empirical Generality in Physiology*. *J. Hist. Biol.* 2002; 35:329–363. [PubMed: 12269345] Creager, Angela N. H. *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965*. Univ. Chicago Press; Chicago: 2002. and Rader, Karen. *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955*. Princeton Univ. Press; Princeton, N.J.: 2004.
4. Kohler. *Lords of the Fly*. and Rader. *Making Mice*.
5. Clarke; Fujimura, editors. *Right Tools for the Job*. p. 5 (cit. n. 3)
6. Clarke, Adele E.; Geison, Gerald L. *Physiology in the American Context, 1850–1940*. American Physiological Society; Bethesda, Md.: 1987. *Research Materials and Reproductive Science in the United States, 1910–1940*; p. 323-350. on p. 343 n 12. Regarding the new reliance on animals see Bynum WF. *C'est un malade': Animal Models and Concepts of Human Diseases*. *Journal of the History of Medicine and the Allied Sciences*. 1990; 45:397–413.
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8. On standards see Bowker, Geoffrey C.; Star, Susan Leigh. *Sorting Things Out: Classification and Its Consequences*. MIT Press; Cambridge, Mass.: 2000. Lapland, Martha; Star, Susan Leigh. *Standards and Their Stories: How Quantifying, Classifying, and Formalizing Practices Shape Everyday Life*. Cornell Univ. Press; Ithaca, N.Y.: 2009. and Drori, Gili S.; Meyer, John W.; Ramirez, Francisco O.; Schofer, Evan. *Science in the Modern World Polity: Institutionalization and Globalization*. Stanford Univ. Press; Stanford, Calif.: 2003. p. 280-292. On "biocapital" see Rajan, K. *Sunder Biocapital: The Constitution of Postgenomic Life*. Duke Univ. Press; Durham, N.C.: 2006. Rose, Nikolas. *The Politics of Life Itself: Biomedicine, Power, and Subjectivity in the Twenty-First Century*. Princeton Univ. Press; Princeton, N.J.: 2007. and Franklin, Sarah. *Dolly Mixtures: The Remaking of Genealogy*. Duke Univ. Press; Durham, N.C.: 2007. A useful review is Helmreich, Stefan. *Species of Biocapital*. *Science as Culture*. 2008; 12:463–478. The animals considered here are not

necessarily “models,” though they could be used in that way and increasingly were from the 1960s, as “modeling” became more prevalent. The bureau’s primary concern in its early years was the animal as a diagnostic tool, distinct from a model system of a disease. On the animal model see Keller, Evelyn Fox. *Models of and Models For: Theory and Practice in Contemporary Biology. Philosophy of Science*. 2000; 67:S72–S86. and Creager, Angela N. H.; Lunbeck, Elizabeth; Wise, M. *Norton Science without Laws: Model Systems, Cases, Exemplary Narratives*. Duke Univ. Press; Durham, N.C.: 2007.

9. See Reich, L.S. *Cambridge Univ. Press; Cambridge: 1985. The Making of American Industrial Research: Science and Business at GE and Bell, 1876–1926*. Hounshell, David A.; Smith, John Kenly, Jr.. *Science and Corporate Strategy: Du Pont R & D, 1902–1980*. Cambridge Univ. Press; Cambridge: 1988. Swann, John Patrick. *Academic Scientists and the Pharmaceutical Industry*. Johns Hopkins Univ. Press; Baltimore: 1988. Carlson, W. Bernard *Innovation as a Social Process: Elihu Thomson and the Rise of General Electric, 1870–1900*. Cambridge Univ. Press; Cambridge: 1991. Quirke, Viviane. *Collaboration in the Pharmaceutical Industry: Changing Relationships in Britain and France, 1935–1965*. Routledge; London: 2006. and Anderson, Julie; Neary, Francis; Pickstone, John V. *Surgeons, Manufacturers, and Patients: A Transatlantic History of Total Hip Replacement*. Palgrave; Basingstoke: 2007.
10. E.g., in 1948 the British government established the National Research and Development Corporation to facilitate relations between science and industry in an attempt to avoid repeating the penicillin debacle, where British innovation had not been capitalized upon. See Keith ST. *Inventions, Patents, and Commercial Development from Governmentally Funded Research in Great Britain: The Origins of the National Research and Development Corporation*. Minerva. 1981; 19:92–122. For a broad history of the relationship between science and industry in Britain see Edgerton, David. *Science, Technology, and the British Industrial “Decline,” 1870–1970*. Cambridge Univ. Press; Cambridge: 1996.
11. MacLeod, Roy. *Science for Imperial Efficiency and Social Change: Reflections on the British Science Guild, 1905–1936*. *Public Understanding of Science*. 1994; 3:155–193. and McGucken, W. *Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931–1947*. Ohio State Univ. Press; Columbus: 1984.
12. Regarding the need for a supply of standardized laboratory animals see Kirk, Robert G. W. ‘Wanted— Standard Guinea Pigs’: Standardization and the Experimental Animal Market in Britain, c. 1919–1947. *Studies in History and Philosophy of Science*. 39:280–291. on the twofold purpose of the bureau see Conference on the Supply of Experimental Animals SEA/M1/2; p. 14–15. (cit. n. 1) The conference’s work was undertaken by a subcommittee consisting of John Beattie (chairman), Alfred Louis Bacharach (hon. secretary), the biochemist Stanley Nicholas Farmer, the statistician and geneticist Kenneth Mather, the biochemist D. McClean, the bacteriologist H. J. Parish, the veterinarian A. W. Stableforth, and the physiologist James Yule Bogue (who later withdrew, to be replaced by the animal pathologist James Robert Maitland Innes).
13. A. L. Bacharach to A. S. Parkes, 8 Apr. 1947, NA FD1/383. This is not to deny that the term “experimental animal” was in use prior to 1947; rather, I mean to note a proportional shift in terminology from “experimental” to “laboratory” after this date.
14. F. A. E. Crew to W. Fletcher, 10 Dec. 1925, NA FD1/1372. Crew was the first in Britain to import and breed the American Wistar rat.
15. On the topic of establishing a laboratory animal “trade organization” see Advisory Committee on the Supply of Laboratory Animals, Minutes of the Third Meeting; 1948; Jul 20. p. 3. NA FD1/383 The quotation comes from R. E. Glover to A. Landsborough Thomson, 27 Oct. 1947, NA FD1/383 (Glover is explaining that he has understood the limitations of the bureau’s role).
16. On the instigation of the survey see “Progress Report 1949–1950, Laboratory Animals Bureau,” 1 May 1950, p. 3, NA FD1/378; regarding the findings see Lane-Petter W, Barber AB, King HJ. *Survey of Laboratory Animals in Great Britain*. *British Veterinary Journal*. 1955; 3:282–299. On the importance of quantification see Porter, Theodore M. *Trust in Numbers*. Princeton Univ. Press; Princeton, N.J.: 1995. For the cultural authority of the census as a means to constitute “governmental objects” see Hannah, Matthew G. *Governmentality and the Mastery of Territory in Nineteenth-Century America*. Cambridge Univ. Press; Cambridge: 2000. See also Foucault, Michel. *Governmentality*. In: Faubion, James D., editor. *Power: Essential Works of Foucault*,

- 1954–1984. Penguin; Harmondsworth: 2002. p. 201–222. I use “constitute” in the sense articulated by Ian Hacking. See, in particular, Hacking, Ian. *Biopower and the Avalanche of Numbers. Humanities in Society*. 1982; 5:279–295. and Hacking. *Historical Ontology*. Harvard Univ. Press; Cambridge, Mass.: 2004. *Making up People*; p. 99–114.
17. Progress Report 1949–1950, Laboratory Animals Bureau; p. 3 (quotation) Notes on the Work of the Bureau. 1950. p. 1. ca. NA FD1/378 (obstruction by dealers) and Laboratory Animal Bureau—Supplies of Animals. Dec.. 1949 p. 1. NA FD1/383 (the bureau at the center of a newly constituted market)
 18. Advisory Committee on the Supply of Laboratory Animals, Minutes of 5th Meeting; 1949; Nov.. p. 4. NA FD1/383 (quotation) and Report of the Bureau from October 1948 to September 1949. p. 1. NA FD1/383 Dealing was problematic because the practice of purchasing animals from various locations and housing them together encouraged the spread of infections; thus the eradication of dealers and the reduction of disease were related goals.
 19. Advisory Committee on the Supply of Laboratory Animals, Minutes of 4th Meeting; Apr.. 1949 p. 2. NA FD1/383 (quotation) The accreditation scheme first covered guinea pigs and later included mice and rabbits, these being the most popular species utilized for routine medical and toxicity testing and bioassay. See Lane-Petter W. *The Accreditation Scheme for Laboratory Animals*. Monthly Bulletin of the Ministry of Health. 1953; 12:165–175.
 20. “Outline of a Scheme for the Accreditation of Commercial Breeders of Guinea Pigs, Rabbits, and Mice Destined for Laboratory Use,” NA MAF189/676 (compliance with standards); and Laboratory Animals Bureau News Letter. Dec. 1950 :2. (remarks on “better breeders”). Reports detailing accreditation visits and the varied breeding practices observed can be found in NA MAF189/676.
 21. See Bijker, W. E. *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change*. MIT Press; Cambridge, Mass.: 1995. p. 122–127. Bijker is particularly useful in that his articulation of the “technological frame” moves beyond the psychosocial to stress that “interactions in relevant social groups are never governed by cognitive and social factors alone. Artefacts, as stabilized in previous construction processes, play a crucial role too” (p. 124).
 22. Regarding cooperation in the matter of the pathology service see Memorandum on Co-operation between the Public Health Laboratory Service and the Laboratory Animals Bureau in the Diagnosis of Disease amongst Breeders’ Stocks of Laboratory Animals. 1950. ca. NA FD1/378 The steps taken to address infections that were discovered worked well in practice, with total slaughter required only about 25 percent of the time. See W. Lane-Petter to G. S. Wilson, 24 July 1952, NA FD1/379.
 23. “A Livestock Insurance for Breeding Guinea Pigs Accredited by the Laboratory Animals Bureau,” July 1952, NA FD1/379. The insurance was underwritten by Arthur R. Jones, Ltd. (Incorporated Insurance Brokers), Leeds. On the internalization of discipline see Foucault, Michel. *Discipline and Punish: The Birth of the Prison*. Penguin; Harmondsworth: 1979. For risk management as a technique of governance see Dean, Mitchell. *Governmentality: Power and Rule in Modern Society*. Sage; London: 1999. Ch. 9
 24. E.g., a severe guinea pig glut occurred between 1948 and 1950, as a “shortage of the late war years, and other factors, attracted a large number of would-be-breeders immediately after the war,” causing “the biggest glut in recent years.” The inevitable collapse in prices and production led to a severe shortage in the summer of 1951: as “confidence in the future of the business had been badly shaken, the normal seasonal breeding programme was not undertaken.” Lane-Petter W. *Supplies: Guinea-Pigs*. Laboratory Animals Bureau News Letter. 1951; 2:i.
 25. The military antecedent has been identified as an early example of the manifestation of modern disciplinary power; see Foucault. *Discipline and Punish*. :187–188. (cit. n. 23).
 26. Lane-Petter W. *The Place of Laboratory Animals in the Scientific Life of a Country*. Impact of Science on Society. 1959; 9:178–196. 180, 195.
 27. Foucault, Michel. *The Will to Knowledge: The History of Sexuality*. Vol. 1. Penguin; Harmondsworth: 1998.
 28. *The Need for a National Policy for the Supply of Experimental Animals*. 1943. ca. NA FD1/6244 In its original form the committee consisted of A. S. Parkes (physiologist and chairman), A. L. Bacharach (representing the Conference on the Supply of Experimental Animals), W. P. Blount

- (Ministry of Supply), A. E. Childs, W. S. Gordon (Agricultural Research Council), K. Mather (geneticist and statistician who had served on the Conference on the Supply of Experimental Animals working committee), A. A. Miles (biochemist), R. M. A. Welchman, F. R. Winton (pharmacologist), Samson Wright (physiologist), and the bureau director: NA FD1/383.
29. For state planning of science see Association of Scientific Workers. *Science and the Nation*. Penguin; London: 1947. for the “freedom of science” response see Baker, JR. *Science and the Planned State*. Allen & Unwin; London: 1945. On the accommodation reached see McGucken. *Scientists, Society, and State*. p. 265-300. (cit. n. 11) and Werskey, Gary. *The Visible College*. Free Association Books; London: 1988.
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 31. Lane-Petter to Landsborough Thomson, 7 Nov. 1950, p. 2, NA FD1/380. As laboratory animal care and management did not formally exist as a disciplinary category, relevant knowledge was widely dispersed across the scientific literature. See *Notes on the Work of the Bureau*. p. 8 (cit. n. 17)
 32. For Lane-Petter’s speculations about infection see *Notes on the Work of the Bureau*. p. 10-11. Regarding the rejection of his proposals on financial grounds see Wilson to F. H. K. Green, 11 Nov. 1950; Wilson to Green, 22 Jan. 1951; and Green to Lane-Petter, 30 Jan. 1951: NA FD1/380. For Lane-Petter’s “interpretation” see Lane-Petter to Green, 31 Jan. 1951, NA FD1/380; and Lane-Petter, “Place of Laboratory Animals in the Scientific Life of a Country” (cit. n. 26). On Landsborough Thomson’s agreement with Parkes see “Interview with Dr. A. S. Parkes, 11 Oct. 1950,” NA FD1/383; and File No. 710/a/1 F H K Green, 29 Nov. 1950, NA FD1/380
 33. Boots, Burroughs Wellcome, British Drug Houses, May & Baker, Herts Pharmaceuticals, and ICI had independently approached the bureau: NA FD1/8680. Funds for the project were donated by British Insulin Manufacturers, Distillers Company (Biochemicals), Ltd., Crookes Laboratories, Parke Davis & Co., Ltd., Organon Laboratories, Ltd., Glaxo Laboratories, Ltd., Roche Products, Ltd., Smith and Newby Research, Ltd., and May and Baker, Ltd.: NA FD1/8680.
 34. *The Use of Mice in Large Numbers for Scientific Purposes: Supplementary Memorandum*. Laboratory Animals Bureau News Letter. 1952:3. Mice were used for numerous routine purposes, including bioassay, serum titration, toxicity testing, compound screening, chemotherapeutic trials, and virulence tests.
 35. Abigail Woods has argued that slaughter was a peculiarly British approach to animal disease control; see Woods. *The British Response to Foot and Mouth Disease, 1920–2001*; paper presented at the Wellcome Trust Centre for the History of Medicine; University College, London. 2001; Oct..
 36. *Ectromelia in Mice—An Experiment in Vaccination: Supplementary Memorandum*. Laboratory Animals Bureau News Letter. 1953; 6:4.
 37. Tuffery AA. *The Laboratory Mouse in Great Britain, II: Intercurrent Infection (Infantile Diarrhoea)*. *Veterinary Record*. 1956; 68:433–440. Tuffery. *The Laboratory Mouse in Great Britain, III: Intercurrent Infection (Ectromelia)*. *ibid.* :478–481. Tuffery. *The Laboratory Mouse in Great Britain, IV: Intercurrent Infection (Tyzzers’ Disease)*. *ibid.* :511–515. and Tuffery. *The Laboratory Mouse in Great Britain, V: Intercurrent Infection (Salmonellitis)*. *ibid.* :568–571.
 38. Tuffery. *Laboratory Mouse in Great Britain, II*. p. 435
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Triangulating Clinical and Basic Research: British Localizationists, 1870–1906. *History of Science*. 1986; 24:29–48. [PubMed: 11620922] The notion of dynamic co-constitution is comparable to Hacking's more philosophically oriented "dynamic nominalism," although the former would place greater emphasis on material agency. See Hacking. *Making up People*. (cit. n. 16).

43. The co-constitution of user/producer identity across scientific, industrial, and consumer cultures has been a growing interest in technology studies. See Oudshoorn, Nelly; Pinch, Trevor, editors. *How Users Matter: The Co-Construction of Users and Technology*. MIT Press; Cambridge, Mass.: 2005. For related historical work see Gaudillière, Jean-Paul; Löwy, Ilana. *The Invisible Industrialist: Manufacturers and the Production of Scientific Knowledge*. Macmillan; London: 1998.
44. Clause. *Wistar Rat as a Right Choice*. (cit. n. 3) Kohler. *Lords of the Fly*. (cit. n. 3) Löwy, Ilana; Gaudillière, Jean-Paul. *Disciplining Cancer: Mice and the Practice of Genetic Purity*. In: Gaudillière; Löwy, editors. *Invisible Industrialist*. p. 209–249. Gaudillière. *Circulating Mice and Viruses*. (cit. n. 3) and Rader. *Making Mice*. (cit. n. 3)
45. Ilana Löwy has stressed the importance of purpose and disciplinary context in shaping laboratory animals; see Löwy. *The Experimental Body*. In: Cooter, Roger; Pickstone, John V., editors. *Medicine in the Twentieth Century*. Harwood; Amsterdam: 2000. p. 435–449. For the development of inbred animals in British genetic and cancer research see Palladino, Paolo. *On Writing the Histor(ies) of Modern Medicine*. *Rethinking History*. 1999; 3:271–288. and Marie, Jennifer; Univ. College. Ph.D. diss. London: 2004. *The Importance of Place: A History of Genetics in 1930s Britain*. Regarding dependence on the commercial market see Kirk. *Wanted—Standard Guinea Pigs*. (cit. n. 12) The advent of transgenic laboratory animals in the late twentieth century coincided with the decline in routine use of animals for biomedical standardization, medical diagnostics, and, to a lesser extent, toxicity testing, reversing the pattern described here.
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69. Joseph Needham, for example, was recalled from China to develop UNESCO's Division of Natural Sciences.
70. Huxley J. International Needs and Scientific Achievements. Listener. 1933; 10:940-942. on p. 942. Huxley was also aware that "standardization so far can only be achieved by the use of experimental animals" and that without it "we would not dare use such wonderful treatments as antitoxin for diphtheria": Huxley. Science and Health. *ibid.* :706-739. on p. 708. For Huxley's importance and legacy at UNESCO see M' Bow, A-M. Preface. In: Baker, JR., editor. Julian Huxley: Scientist and World Citizen. UNESCO; Paris: 1978. and Armytage, WHG. The First Director-General of UNESCO. In: Keynes, M.; Harrison, GA., editors. Evolutionary Studies: A Centenary Celebration of the Life of Julian Huxley. Macmillan; London: 1989. p. 186-193. Baker's authorship of a eulogy of Huxley is indicative of the consensus that quickly emerged between advocates of socialist state planning and the Freedom of Science movement, of which Baker was a leading voice in the 1940s: Baker, editor. Julian Huxley. p. 5-51.
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74. On the reciprocal learning that took place see Lane-Petter to R. H. L. Cohen, 2 Mar. 1954, NA FD1/8681. On gnotobiotics see Fitzgerald, Gerard James; Carnegie Mellon Univ.. Ph.D. diss. 2003. From Prevention to Infection: Intramural Aerobiology, Biomedical Technology, and the Origins of Biological Warfare Research in the United States, 1910-1955. For a contemporary overview see Trexler PC. Rationale for the Development of Gnotobiotics. Laboratory Animals. 1978; 12:257-262. [PubMed: 153430]
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78. Informal Conference on the Husbandry of Animals; p. 3-5. (cit. n. 67) Landsborough Thomson's retirement is noted in an obituary: Sir Landsborough Thomson: Ornithology and Medical Research. Times. Jun 11. 1977 :16. col. F.
79. Progress Report 1953-1960 of the Laboratory Animals Centre. p. 6 (cit. n. 40) (precautions and restrictions for workers) Anonymous to Robert G. W. Kirk, 20 Aug. 2007 (gardener's access); and

- E. M. B. Clements to Mr. Whittaker, memo, ca. 1961, NA FD23 407 (quotation). Because of the requirements of pathogenic security, only connecting corridors allowed natural light and a view to the garden.
80. Other terms used to describe such animals include “clean,” “disease free,” “pathogen free,” “caesarean started,” and “caesarean derived.”
 81. Lev, M.; Lane-Petter. *Animals for Research. Germ-Free Animals*; p. 139-176.(cit. n. 65)
 82. Laboratory Animal Advisory Committee Minutes of the First Meeting; Apr.. 1964 p. 1-2.NA FD 9/387 (introduction of animals into the new facility)Dinsley, M. *Laboratory Animals Centre Collected Papers, Vol. 12: Choice of Experimental Animal*. HMSO; London: 1963. The Case for the Healthy Inbred Mouse; p. 47-55.on p. 53 (“upgrading” system)andRussell ES. *New Trends in the Use of Genetically Controlled Laboratory Animals in Biomedical Research. Proc. Animal Care Panel*. 1960; 10:167–176. on p. 175.
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 84. Lane-Petter resigned and took up the directorship of Carworth Europe, based at Huntingdon; he remained there until he moved to Cambridge to direct the university’s Central Animal Service from 1972 until his retirement in 1977.
 85. Laboratory Animal Advisory Committee Minutes of the First Meeting; p. 2(cit. n. 82)Announcements. *Nature*. 1965; 205:452. (“unable to recommend the proposal”). and Anonymous to Kirk, 20 Aug. 2007.
 86. *Laboratory Animals Centre Progress Report 1965–1968*. Apr.. 1969 p. 1-2.(the various departments), 8 (sales figures), NA FD12/259The facility employed a full-time veterinary pathologist (D. Blackmore), two geneticists (M. Festing and M. Cryer), a microbiologist (R. Hare), a parasitologist (D. Owen), a veterinary surgeon (G. Townsend), a nutritionist (G. Porter), and several technical and clerical support staff.
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 93. Huxley. *International Needs and Scientific Achievements*. p. 942(cit. n. 70)For the British debate over standardization and its association with the United States seeZeitlin, J. *Americanisation and Its Limits: The Reconstruction of Britain’s Engineering Industries, 1945–1955*. In: Whiteside, N.; Salais, R., editors. *Governance, Industry, and Labour Markets in Britain and France: The Modernising State in the Mid-Twentieth Century*. Routledge; London: 1998. p. 96-116.
 94. Star, Susan Leigh; Griesemer, James. *Translations, and Boundary Objects: Amateurs and Professionals in Berkeley’s Museum of Vertebrate Zoology, 1907–1939*. *Social Studies of Science*. 1989; 19:387–420. on p. 393. andLatour, Bruno. *Science in Action*. Harvard Univ. Press; Cambridge, Mass.: 1987. p. 226-227.

95. For a useful overview of transnational history as it can apply to histories of science and technology see van der Vleuten, Erik. Toward a Transnational History of Technology. *Technology and Culture*. 2005; 21:1–20.

Figure 1.
William Lane-Petter (1981).

Figure 2.
 "New Selection Centre" floor plan. From M. Dinsley, "The Case for the Healthy Inbred Mouse," in *Laboratory Animals Centre Collected Papers, Vol. 12: Choice of Experimental Animal* (London: HMSO, 1963), pp. 47-55, on p. 51.

Key to staff photograph

Figure 3.
Laboratory Animals Centre twenty-first anniversary staff photograph (1968), with key.

Figure 4.
BALB/c strain. From R. F. Parrot and M. F. W. Festing, *Standardised Laboratory Animals* (Carshalton: Medical Research Council Laboratory Animals Centre, 1971), p. 11.