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Between the Clinic and the Laboratory: Ethology and Pharmacology in the Work of Michael Robin Alexander Chance, *c*.1946–1964

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Whilst ethology has garnered the attention of historians of science, particularly those interested in the biological and behavioural sciences, historians of medicine have yet to explore ethology's medical significance. Ethology, "the biological study of behaviour", is historically associated with the work of Nikolaas Tinbergen and Konrad Lorenz, who, alongside Karl von Frisch, were awarded the Nobel Prize for Physiology or Medicine in 1973 for their research into the "organization and elicitation of individual and social behaviour patterns".[1] This particular award is notable as it was the first to be given in part recognition of the establishment of a discipline rather than for a specific discovery or advance.[2] This award was also provocative as it was the first to recognize non-reductionist behavioural research.[3] Prior to 1973, otherwise renowned psychologists including Wilhelm Wundt, Sigmund Freud, and Carl Jung had failed to gain recognition, whereas some behavioural physiologists were awarded the Nobel Prize, for example Ivan Petrovich Pavlov in 1904. Historians of medicine should be interested in ethology as the 1973 prize recognized that ethological research had "led to important results for, e.g. psychiatry and psychosomatic medicine, especially as regards possible means of adapting environment to the biological equipment of man with the aim of preventing

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[1] P Marler and D R Griffin, 'The 1973 Nobel Prize for Physiology or Medicine', *Science*, 1973, **182**: 464–6. The most comprehensive historical account of ethology to date remains R W Burkhardt, *Patterns of behavior: Konrad Lorenz, Niko Tinbergen, and the founding of ethology*, University of Chicago Press, 2005. Burkhardt adopts a largely biographical frame focused on Tinbergen and Lorenz.

[2] D A Dewsbury, 'The 1973 Nobel Prize for physiology or medicine: recognition for behavioral science?', *Am. Psychol*., 2003, **58**: 747–52.

[3] L T Benjamin Jr., 'Behavioral science and the Nobel Prize: a history', *Am. Psychol*., 2003, **58**: 731–41. The award was also controversial due to Lorenz's association with Nazi ideology, see Theodora J Kalikow, 'Konrad Lorenz's ethological theory: explanation and ideology, 1938–1943', *J. Hist. Biol*., 1983, **16**: 39–73; cf. Robert J Richards, *Darwin and the emergence of evolutionary theories of mind and behavior*, University of Chicago Press, 1987, p. 536.

maladaptation and disease".[4] This paper explores the relevance of ethology to the development of the clinical and biomedical sciences in post-Second World War Britain. In doing so it engages and extends Richard Burkhardt's metaphorical use of ecology to describe ethology as characterized by interactivity, a responsiveness to contingency, and a willingness to evolve and adapt to a diverse number of "ecological" niches. The paper also continues Burkhardt's biographical approach to ethology's history by focusing on a single figure, the comparatively little known pharmacologist and ethologist Michael Robin Alexander Chance (1915–2000). It contends that medicine formed a hitherto unrecognized example of "ethology's ecologies".[5]

Michael Chance could be described as a marginal figure to the history of medicine; his work did not have the broad influence that would have made him of obvious historical interest. Moreover, Chance existed on the borders of so many sub-disciplines that it is difficult to identify who should be interested in his work. Employed as a pharmacologist, yet identifying himself as an ethologist, Chance's research interweaved the fields of pharmacology, physiology, endocrinology, ethology, animal husbandry, psychiatry, anthropology, and sociology, amongst others. Spatially, and practically, Chance's work found application in locations as diverse as the material practices of laboratory science and the education in clinical observation of medical students. Adequately historicizing the diversity of Chance's *oeuvre* would be a major undertaking beyond the bounds of a single article. Consequently analysis is here restricted to how he integrated ethology within laboratory science in order to reveal the "social" nature of laboratory animals. In the 1940s Chance turned to ethology as a means to study how social behaviour altered laboratory animal responses and undermined their experimental reliability.[6] His work was no less instrumental in orientation than, say, the genetic standardization of mice undertaken by C C Little.[7] Chance saw the animal as a tool, but, none the less, emphasized the "nature" of the laboratory animal as a living being with social relations, relations that included that between animal and human.

The paper begins by outlining how Chance first encountered the "social" laboratory animal during his pharmaceutical work at Glaxo Laboratories. In order to contextualize his work within the culture and politics of the period**,** the origins of his interest in ethology are then traced, but not to animal behaviour. Chance's ethological interests are shown to have developed during his time at the Pioneer Health Centre, Peckham; thus he first learnt of ethology in the study of human health. Relocating the origins of Chance's ethology from the animal to the human geographically repositions his work

[4] Karolinska Institutet, 'Press release: The Nobel Prize in Physiology or Medicine', 1973, http://nobelprize.org/nobel_prizes/medicine/laureates/1973/press.html (accessed 31 March 2009).

[5] The term is Burkhardt's, see Burkhardt, op. cit., note 1 above, esp. pp. 447–84.

[6] There is a growing literature on the standardization of laboratory organisms, see, for example, Adele E Clarke and Joan H Fujimura, *The right tools for the job: at work in twentieth-century life sciences*, Princeton University Press, 1992; Bonnie Tocher Clause, 'The Wistar rat as a right choice: establishing mammalian standards and the ideal of a standardized mammal', *J. Hist. Biol*., 1993, **26**: 329–49; Robert E Kohler, *Lords of the fly: Drosophila genetics and the experimental life*, University of Chicago Press, 1994; Angela N H Creager, *The life of a virus: tobacco mosaic virus as an experimental model, 1930–1965*, University of Chicago Press, 2002; Karen A Rader, *Making mice: standardizing animals for American biomedical research, 1900–1955*, Princeton University Press, 2004.

[7] On Little and genetic standardization, see Rader, ibid.

away from the laboratory (an unusual place for ethology to be practised) to the clinic. The importance of ethology to Chance's experimental science is then explored, with particular focus upon how ethology imbued the laboratory animal with subjective "natural" characteristics, feelings, and needs. Consequently, Chance reconfigured the relationship between experimenter and experimental animal as one based on mutual obligation and co-operation. This is shown to have opened up a new territory within which the explicit recognition of an ethical relationship between researcher and laboratory animal became a necessary part of experimental practice. Accordingly, this paper argues not only that ethology operated within the laboratory despite its widespread association with the field, but also that it served as a vector by which other factors conventionally seen as being outside the laboratory became integrated within its material practices. Such factors included clinical observation and the consideration of animal welfare, both of which have been seen as extrinsic to, as opposed to integral to, the practices of laboratory science.[8]

Biological Standardization and the "Social" Laboratory Animal

During the Second World War the British War Office sought to utilize drugs such as amphetamine and benzedrine sulphate in order to keep personnel operational for longer periods of time.[9] The effective assessment of such drugs was hampered by difficulties in developing reliable methods of their measurement (or "standardization").[10] Inconstancies in the standardization of biological substances of this type were common; a 1942 survey article found that independent reports from different laboratories of the lethal dose of benzedrine sulphate in white mice varied by a factor of ten.[11] Even the leading figures of the discipline of biological standardization admitted that its methodology was all too often "a subject for amusement or despair, rather than for satisfaction or self-respect".[12] Biological standardization had emerged in the inter-war period in reaction to the flood of new substances, including antitoxins, vitamins and hormones, which

[8] For example, the history of ethical aspects of animal experimentation focus entirely upon the "antivivisectionist controversy", see Richard D French, *Making mice in Victorian Society*, Princeton University Press, 1975; Nicolaas A Rupke (ed.), *Vivisection in historical perspective*, London, Routledge, 1990; A-H Maehle, 'The ethical discourse on animal experimentation 1650–1900', in Andrew Wear, Johanna Geyer-Kordesch and Roger French (eds), *Doctors and ethics: the earlier historical setting of professional ethics*, Amsterdam, Rodopi, 1993, pp. 203–51. The result of this is to reify the presumption that the laboratory is devoid of emotion or moral thought, a presumption that has been shown to be false in a number of ways, see the literature in note 6 above, and note 13 below, for examples. Nevertheless, social histories of antivivisection and scientific histories of the laboratory rarely interact. Consequently there has been no historical analysis of the role of animal welfare in the material practice of animal dependent laboratory science. This article forms an initial foray into such a study, funded by the Wellcome Trust, grant number 084988, and titled 'Managing morals: animal experiment and animal welfare in Britain *c*.1947–1986'.

[9] R C Browne, 'Amphetamine in the Air Force', *Br. J. Addiction*, 1947, **44**: 64–70.

[10] M R A Chance, 'Population size and variation in small populations', *Proc. R. Soc. Med.*, 1964, **57**: 174.

[11] B Günther, 'Toxicity of benzedrine sulfate in the white mouse and in the frog', *J. Pharmacol. Exp. Ther.*, 1942, **76**: 375–7.

[12] J H Burn, 'The errors of biological assay', *Physiol. Rev.*, 1930, **10**: 146–69, p. 146. For a history of biological standardization as a discipline, see A F Bristow, T Barrowcliffe and D R Bangham, 'Standardization of biological medicines: the first hundred years, 1900–2000', *Notes Rec. R. Soc.*, 2006, **60**: 271–89.

were discovered during the early twentieth century but could not yet be chemically measured. It promised to be an important new field that would develop reliable methods to standardize biological substances, but its task was an impossible one. So-called "biologicals" could be measured only by their effect upon living organisms, yet individual organisms were found to be too variable to serve as reliable diagnostic tools. The conventional route toward overcoming natural variation was that of statistical science which demanded ever larger groups of organisms (usually mice, guinea-pigs, rats and rabbits) be used in an attempt to increase accuracy toward the reliability that a chemist's thermometer measured heat.

Recognition of animal variation, that the animal was not simply a machine, occurred at different times and in different contexts throughout twentieth-century biomedical science. For example, O E Dror has illustrated how inter-war physiologists recognized (and subsequently mechanized) emotion as a source of unpredictable variance in the laboratory.[13] Importantly, the pharmaceutical use of animals differed from their use in physiology. In the latter the animal was both the subject and the object of knowledge. The physiological functions of the animal body formed the area of study, whilst the body itself was the means to that end. In pharmacology, and in particular the field of biological standardization, the subject of knowledge was biochemical substances and the animal served as no more than a purportedly objective diagnostic technology. There was, therefore, embedded in the pharmaceutical use of animals, an elision between the instrumental purpose to which animals were used and how they were conceived. The one mutually reinforced the presupposition of the other, both being further sustained by a lack of interest or expertise in the subject of animal behavioural physiology. This goes some way to explaining why Dror has found that, despite the careful mechanization of emotion in the inter-war period, the animals that physiologists worked with "were not perceived or handled as tools or instruments".[14] In contrast the pharmaceutical sciences used the animal as a measuring device that had to be, and so was all too often presumed to be, a mechanistically reliable tool.

Michael Chance graduated from University College London in 1937 with a degree in zoology, and, having spent six months working on the biological assay of pituitary hormones under Alan Parkes at the National Institute for Medical Research, joined Glaxo Laboratories as a pharmacologist in 1938.[15] Perhaps from the start, Chance saw conflicts between his zoological knowledge of animals as living organisms and the presumptions regarding their use in pharmacology. At Glaxo he worked under the direction of the biochemist Alfred Louis Bacharach, who was among the first in Britain to advocate inbred animals as standardized laboratory tools, or "the litmus paper of the vitamin chemist".[16] Yet despite being used as reliable reagents akin to litmus paper, animals did not behave

[13] O E Dror, 'The affect of experiment. The turn to emotions in Anglo-American physiology, 1900–1940', *Isis*, 1999, **90**: 205–37. For variation as a productive force, see Daniel P Todes, 'Pavlov's physiology factory', *Isis*, 1997, **88**: 205–46.

[14] Dror, ibid., p. 237, n. 98.

[15] Chance began work with laboratory animals on 11 October 1937, being granted a Home Office licence for the "administration of hormonal substances to normal animals and to those from which various glands have been removed". See National Archives, Kew (hereafter NA) HO 45/23629.

[16] A L Bacharach, 'The albino rat in biochemical investigation', *Pharmaceutical J. and Pharmacist*, 1926, **62**: 629–30.

as reliable reagents. A minority who were willing to approach the animal as being more than a diagnostic tool had argued that animal physiology varied both between individuals and over the seasons.[17] Moreover, not all laboratories had access to inbred animals, which had led in the 1940s to growing demands for the establishment of a national supply of genetically and pathogenically "standardized" laboratory animals in post-war Britain. Bacharach was a leading agitator in this movement, and was responsible for the Association of Scientific Workers' wartime campaign to persuade the Medical Research Council (MRC) to accept responsibility for the regulation (if not provision) of laboratory animal production.[18]

When asked by the War Office in 1942 to measure the potency of stimulants such as amphetamine, Chance therefore had a wealth of expertise, not to mention standardized animals, to draw upon at Glaxo.[19] However, he demonstrated little interest in inbreeding as a means to standardize laboratory animals (and thus the results thereby obtained by their use) as advocated by Bacharach. Instead, he focused on a singular observation reported in 1940 by J A Gunn and M R Gurd (of the Nuffield Institute for Medical Research, Oxford) to explain the difficulties in developing a reliable means to measure amphetamine. In their investigation of adrenaline Gunn and Gurd had noted that "symptoms of excitement are much more pronounced if several injected animals are kept together ... If one animal is kept alone in a jar, no very striking symptoms of excitement may be exhibited".[20] The implication, though they did not state so explicitly, was that social interaction had physiological consequences. Thus rather than follow Bacharach (and the vast majority of researchers across the biomedical sciences concerned with standardizing animals) in turning to genetics to explain irreconcilable experimental results, Chance prioritized social interrelation as mediating the behavioural and physiological responses of animals. This choice shaped the entirety of his subsequent career, and anticipated important developments in post-war laboratory animal use across the biomedical sciences.

Careful observation of the effects of aggregation demonstrated that the presence of other mice was found to double the toxicity of amphetamine. This observation opened up a new discourse that prioritized the social to explain the difficulties hitherto experienced in biological standardization. The comparative uniqueness of Chance's approach is revealed when his first paper of 1946 is considered against others in the *Journal of Pharmacology*. Chance's paper is unusual in containing detailed accounts of the behavioural repertoire of amphetamine-dosed mice. He began by describing how a solitary "benzedrinized" mouse entered into an alternating sequence of spontaneous rapid movement and squeaking followed by periods of stillness where the mouse maintained

[17] For example, J W Trevan, 'The error of determination of toxicity', *Proc. R. Soc.*, 1927, **101B**: 483–514.

[18] See R G W Kirk, '"Wanted—standard guinea pigs": standardization and the experimental animal market in Britain *ca.*1919–1947', *Stud. Hist. Philos. Biol. Biomed. Sci.*, 2008, **39**: 280–91. In contrast, the American demand for standardized organisms has been related to genetic and cancer research. See, for example, Kohler, and Rader, both op. cit., note 6 above.

[19] At Glaxo, Chance became the first person to be licensed to conduct "severe and novel" experiments on animals in a commercial laboratory, see NA HO45/23629, 'Advisory Committee on the administration of the Cruelty to Animals Act'.

[20] J A Gunn and M R Gurd, 'The action of some amines related to adrenaline. Cyclohexylalkylamines', *J. Physiol.*, 1940, **97**: 453–70, p. 457.

unusual rigidity and alertness but at all times remained close to the wall of its box with eyes looking upwards. Chance interpreted such behaviour as being generally "apprehensive".[21] This abnormal behaviour was found to be exaggerated in groups, moving through three distinct forms of interaction. In the first, each mouse exhibited spontaneous squeaking and quick movement about the box, avoiding all encounters with others. After fifteen to thirty minutes the second phase began which saw mice begin to react to random squeaks by jumping in the air. Random encounters in this way at first caused "fright", where one mouse evaded the other by jumping or running away. Eventually such encounters led to pairing, where mice reared on their hind legs with fore-paws raised and heads swaying in unison, a position Chance labelled a "defensive encounter". This behaviour became synchronized, with the group alternating between rapid movement and "escape reactions", followed by periods where mice paired and swayed silently until a random noise initiated a new period of jumping and evasion. The final stage of behaviour was marked by aggressive encounters where mice attacked one another, with some falling into fatal convulsions. This detailed observational description of laboratory animal behaviour was unusual in a pharmacological paper, where conventionally the only behavioural observation noted was death. Chance speculated that the number of encounters between mice somehow catalysed the drug's effect upon the central nervous system. His findings were of pharmaceutical importance because if the toxicity of a drug altered in accordance with the degree of aggregation, the latter would have to become a fundamental consideration in experimental design. Genetic standardization would need to be extended to include the standardization of social interactions if a reliable laboratory animal were to be constructed for pharmaceutical experimentation. In subsequent decades the interaction of pharmaceutical drugs and social behaviour became a growing concern as psychotropic drugs came to prominence, giving birth to new subdisciplines such as psychopharmacology and psychopharmacogenetics. Chance's work influenced the formation of these fields and the behavioural analysis he developed came to be recognized as an important, albeit problematic, tool of psychopharmacology.[22]

Chance's identification of the social behaviour of laboratory animals was of instrumental importance not only to pharmacology. It was equally of relevance to the emerging study of animal behaviour known as ethology. Consequently, Chance published the same paper with minimal alterations in two journals for distinct audiences, the *Journal of Pharmacology* and *Behaviour*. Instigated in 1946 by the Dutch ethologist Nikolaas Tinbergen, who was soon to join Oxford from Leiden, *Behaviour* boasted as editors well known (or shortly to be so) names including the Cambridge ethologist W H Thorpe, the Swiss zoologist Heini Hediger, and the American psychobiologist Frank Beach (the latter enrolled to provide representation from across the Atlantic for what some saw as the new

[21] M R A Chance, 'Aggregation as a factor influencing the toxicity of sympathomimetic amines in mice', *J. Pharmacol. Exptl. Therap.*, 1946, **87**: 214–19, p. 217.

[22] For Chance's contribution to psychopharmacology, see C R B Joyce (ed.), *Psychopharmacology: dimensions and perspectives*, London, Tavistock Publications, 1968, esp. pp. 283–318; L L Iversen, Susan D Iversen and Solomon H Snyder (eds), *Handbook of psychopharmacology*, 16 vols, New York, Plenum Press, 1977, vol. 7, esp. pp. 3–35. For psychopharmacogentics, see P L Broadhurst, *Drugs and the inheritance of behavior*, New York, Plenum Press, 1978. For the history of psychopharmacology, see David Healy, *The creation of psychopharmacology*, Cambridge, MA, Harvard University Press, 2002.

"European" science of animal behaviour).[23] Chance's paper on laboratory animal social behaviour formed the fourth of fourteen, appearing alongside articles by Tinbergen, Hediger, and Johan Bierens de Haan, all familiar names in the history of animal behavioural research. That Chance published his observations of laboratory animal behaviour in the inaugural volume of *Behaviour* is significant. On one level, it is indicative of his growing interest in the ethological analysis of animal behaviour. More crucially, the fact that Chance's observations of benzedrinized mice appeared in what was a new international journal intended to serve as the platform for the science of ethology demonstrates laboratory sciences' hitherto unrecognized significance to this emerging field.[24] Through the 1950s Chance continued to develop his interest in the sociology of laboratory animals, adapting the principles of ethology to address the ecology and behavioural needs of laboratory animals. At the same time he began ethological studies of the social structure of primates, activities that became a source of some conflict with his university as they appeared unrelated to his role as lecturer in pharmacology. From the late 1950s Chance began a long campaign to be officially regarded as an ethologist, which led in 1966 to his reappointment as reader in ethology on condition he continued to teach pharmacology in the Medical School.[25] The fact that Chance came to ethology as a pharmacologist after having chosen to focus on laboratory animal social behaviour raises the question of why he pursued this route. His interest in the ethological analysis of animal behaviour might appear explicable with reference to his undergraduate zoological background, as might his subsequent interest in primate social structure and its evolutionary significance to human behaviour.[26] Yet, prior to his interest in the ethological approach to animal behaviour there was every reason, given the influence of Bacharach at Glaxo, for him to have adopted the conventional genetic route to the standardization of laboratory animals as opposed to speculating on the social. What, then, was it that led Chance to focus on social behaviour as an explanation for the variability of laboratory animals?

Human Ethology and the Pioneer Health Centre

The social basis of relations within society was much debated in Second World War Britain in anticipation of the post-war egalitarian reconstruction of the country. Chance, being a committed socialist and active Labour Party member, participated in this debate as a member of the Socialist Clarity Group responsible for publishing *Labour Discussion Notes*, a bi-monthly publication aimed at encouraging socialist thinking among the working class. In the post-war period Chance's ethological analysis of social structure was orientated towards explaining the rise of Nazism as a social pathology, and demonstrated a concern with the cause and prevention of fascism and the shaping of a healthy

[23] Tinbergen began publicizing *Behaviour* in 1946, the first issue was not published until 1948.

[24] M R A Chance, 'A peculiar form of social behaviour induced in mice by amphetamine', *Behaviour*, 1948, **1**: 64–70.

[25] See *Faculty handbooks* 1965/66 and 1966, Archives of the University of Birmingham, University of Birmingham, UK.

[26] See, for example, M R A Chance and C J Jolly, *Social groups of monkeys, apes and men*, London, Cape, 1970.

society.[27] His analysis of social structure was built on the supposition that fascism was a social pathology, a mass-neurosis, with its origins in humanity's evolutionary past.[28] Arguably Chance's political interests in this sense shaped his science.[29] However, Bacharach too was a committed and active socialist but he advocated genetic standardization as the means to produce reliable laboratory animals. Whilst political concerns sustained Chance's interest in the social behaviour of animals, therefore, they can not be said to have alone determined the origins of this predisposition. In fact it was at the Pioneer Health Centre, Peckham, that Chance first encountered the idea that social relations were central to physiological well being. He worked as director of the laboratory at the Pioneer Health Centre between leaving Glaxo in May and joining Birmingham in September 1946.[30] It was at Peckham that he learnt of ethology as a methodological approach to the study of the complexity of socio-physiological relations, an experience that fundamentally altered his thought and shaped the entirety of his future career.

The Pioneer Health Centre was established by George Scott Williamson, a pathologist at the Royal Free Hospital, and Innes Hope Pearse, House Physician at the London Hospital, in 1926.[31] Believing that modern urban living was not conducive to the promotion of health, and that the medical profession was "content with palliation as its highest goal", Scott Williamson and Pearse sought to pioneer a new approach to health based on the concept that the "power of the environment may yet be potent to save the individual" from the threat of urban living.[32] Their approach differed from mainstream medicine in that it sought to investigate "health" as opposed to "disease", with health conceived as a relational process emerging from the synthesis of organism and environment, and not simply a desirable yet fixed "state". In their evocatively titled *Biologists in search of material*, Scott Williamson and Pearse argued that medicine, as much as biological disciplines such as physiology and pathology, was incapable of revealing anything about the healthy organism. These disciplines were dependent upon an experimental methodology which, "conditioning the environment to secure a specific effect", inevitably led to the "emasculation of the organism". In any case, biology should not be the study of "the organism on the defensive . . . running away from *living* so as to ensure *survival*" (as medicine studied disease) but rather the study of the "organism

[27] In the 1960s Chance was a member of the Centre for Research in Collective Psychopathology at the University of Sussex, an interdisciplinary group that sought to explain Fascism as a mass-psychosis.

[28] See, for example, M R A Chance, 'What makes monkeys sociable', *New Scientist*, 5 March 1959, pp. 520–3, where Chance suggests that Germany's decision to "line up behind an intimidating leader" had a "strong instinctive component" (p. 523).

[29] For the Left and science, see Gary Werskey, *The visible college: a collective biography of British scientists and socialists of the 1930s*, London, Free Association Books, 1988.

[30] Letter from M R A Chance to the Editor of *Radio Times*, 11 Nov. 1995. M R A Chance Papers, in the care of Dave Stevens, London (hereafter MRAC).

[31] See J Lewis and B Brookes, 'The Peckham health Centre, "PEP", and the concept of general practice during the 1930s and 1940s', *Med. Hist.*, 1983, **27**: 151–61; J Lewis and B Brookes, 'A reassessment of the work of the Peckham Health Centre, 1926–1951', *Milbank Memorial Fund Quarterly–Health and Society*, 1983, **61**: 307–50.

[32] I H Pearse and G Scott Williamson, *The case for action: a survey of everyday life under modern industrial conditions, with special reference to the question of health*, London, Faber and Faber, 1931, p. 5.

actively embracing the environment, i.e. in health".[33] In 1946 Scott Williamson distinguished the Peckham approach arguing:

> The medical approach is essentially one derived from the study of pathology. The Peckham approach—the determination of what is right with the family and its personnel and home—is derived from the other branch of biology not previously differentiated, and for which hitherto there has been neither name nor technical procedure. Let us call this branch of biology—ethology ...
>
> The systematic study of *pathos* of the people, or pathology, has proved its worth; but the best that can be made of a bad job with it is to stop the bad from getting worse.
>
> In the Peckham Health Centre we have begun the study of the *ethos* of the people, and have made a first tentative technological approach to their ecology ... any centre dealing with the pathology of a patient ... can have not the slightest resemblance in practice to a *health* centre dealing with the ethology and ecology of the family.[34]

In referencing "family" and "ecology" as key to the Peckham approach, Scott Williamson indicated that health was to be understood to derive from physical and social environmental relations conceptualized holistically not reductively.[35] Of interest is the philosophical and entomological reasoning that led Scott Williamson to claim ethology as the methodology of the "Peckham approach". The term developed from an entomological contrast with the root of pathology, "pathos", suggesting the contrast of ethos, or character, and thus "ethology". Scott Williamson thereby arrived at the point J S Mill had first visited a century earlier in positing ethology as a "science of character".[36] The genealogy of Scott Williamson's ethology is quite distinct from that of the animal behavioural ethology associated with Tinbergen and Lorenz. Nevertheless, the material practices and philosophical presuppositions of both closely resonate with one another.

The Pioneer Health Centre was imagined as a "laboratory" wherein the relationship between the individual and the environment could be experimentally explored whilst circumventing the perceived problem of the act of observation altering the observed.[37] Membership of the Centre was open only to families and not individuals, as the family was considered by Scott Williamson to be the basic unit of life.[38] The internal social relations of the family as well as its interaction with the physical and social environment of the community were the subject of study; "the laboratory of the biologist" was to be "in the field of the leisure of the family".[39] The Health Centre, therefore, served as a community centre in which families would cultivate their health whilst pursuing leisure activities. Swimming pools, recreation rooms, a gymnasium, library, a children's

[33] I H Pearse and G Scott Williamson, *Biologists in search of material*, London, Faber and Faber, 1938, pp. 18–19.

[34] G Scott Williamson, 'Peckham, the first health centre', *Lancet*, 1946, **i**: 393–5, pp. 393–4.

[35] The Pioneer Health Centre should be viewed as a component part of a wider holistic movement that existed in the inter-war period which consciously identified itself in contrast to more reductive, biomedical sciences, see C Lawrence and G Weisz, *Greater than the parts: holism in biomedicine 1920–1950*, Oxford University Press, 1998.

[36] J S Mill, *A system of logic*, London, Longmans, 1879 [1843], bk 6, ch. 5, Section 4, p. 457.

[37] I H Pearse and L H Crocker, *The Peckham experiment: a study in the living structure of society*, London, George Allen and Unwin, 1943, p. 20.

[38] Ibid., p. 43.

[39] G Scott Williamson, 'The biological significance of the family', *Social Service Review*, July 1932, Pioneer Health Centre, Peckham, with papers of George Scott Williamson MD (1884–1953) and Innes Hope Pearse (1889–1978) Archives and Manuscripts Collection, Wellcome Library, London, item SA/PHC/D2/3/2.

playground, self-development rooms, a farm, a cafeteria, and halls for the purpose of dances, lectures, concerts and theatre were provided to facilitate the health and leisure pursuits of members. However, all these activities also enabled a staff of biologist-observers to accumulate knowledge about the "natural" behaviour of families. Peckham, therefore, operated on two levels. For its members it served as a community centre facilitating family health, but for the staff it operated as a laboratory for the study of human ethology.

From 1935 the Centre occupied a purpose-built building designed by the British architect Evan Owen Williams to embody the holistic, organic, and relational ethos of the Peckham philosophy. The architectural design prioritized the principles of freedom of access and visibility so as to enable family members to behave "naturally", whilst allowing unbroken surveillance of their activities. The building itself was expected to evolve as emerging knowledge of the needs of the inhabitants obliged the alteration of the physical environment in order to better facilitate their needs. Consequently, the structure of the building had to be flexible, and so it was built using light materials that allowed for future alteration. The *Architectural Review* celebrated the novel construction in which "the function of each part ... was quite clearly subsidiary to the function of the whole", making the building "architecturally alive" and allowing "no crudity of execution [to] destroy that vitality, any more than elaborate consideration of detail can bring a dead building to life".[40] Far from using aggrandizing rhetoric, the *Review* correctly detected that the physicality of the building was orientated towards facilitating the "natural" behaviour of its occupants to an unprecedented degree. For example, space had been allocated according to the purpose of the activity that was intended to occur within it. In areas provided to facilitate self-determination in movement, where social activity was to be encouraged (such as the recreation rooms) there was ample space, but in purely functional areas (such as the entrance hall) space was kept to the minimum required to allow for and guide movement. Open fireplaces existed in the study and recreation rooms not because they were necessary, modern heating made them redundant, but for "psychological reasons" to provide "a focal point around which social mixing is natural".[41] This environment, designed to facilitate the "natural" behavioural needs of families, was presumed equally to facilitate their healthy development.

It was here, in this human laboratory writ large, that Chance first encountered ethology as director of the laboratory services that oversaw the recording of the physiological characteristics of families. Recording an *individual* member's physiology, including blood sugar, blood haemoglobin, and blood pressure, formed part of the annual *family* "health overhauls", which exemplified the distinctive Peckham approach focused upon the health of the whole, yet with an eye to the individual.[42] Biochemical tests formed only a part of the consultation, close observation of inter- and intra-social interaction of the family within the wider social milieu of the Centre was equally important. Disease, after all, was thought to emerge from disjunctions in social health, in the synthesis of individual and environmental interaction within a social whole. The interrelation of sociological and biological factors within a dynamic, relational, and process orientated

[40] J M Richards, 'The Pioneer Health Centre: the idea behind the idea', *Archit. Rev.*, 1935, **77**: 203–16, p. 208.

[41] Ibid., p. 209.

[42] Pearse and Scott Williamson, op. cit., note 33 above, p. 48.

understanding of health was to form the basis of Chance's future understanding of behaviour, both human and animal. This linkage of the physiological and social also provided the basis of his claim that the physiological uniformity of a laboratory animal could be controlled only by managing social relations. In this regard, it is of note that Scott Williamson claimed to have been inspired to pursue the study of health in 1912 when he began working with a friend on the infectivity of airborne tubercle bacilli. Scott Williamson observed laboratory rats to be unusually resistant to infection until it was found that separating family units caused the rats to succumb. The suggestion that social conditions directly affected physiological health, and, moreover, that the maintenance of a "family unit" was an important promoter of health, led to the Pioneer Health Centre as a means for Scott Williamson "to get the human animals into my cage so that I could observe them and experiment with them".[43] Ironically, then, Chance's passage through Peckham saw him absorb a philosophy of human health and a set of material practices that originated from observations of laboratory rats to which he was later to reapply them.

Despite the Centre having a developed epistemology grounded in the holist understanding of health as an evolving process emerging from synthetic relationships between individuals and their social and physical environments, the Peckham approach was criticized for lacking a rigorous scientific methodology. Like the later work of Tinbergen and Lorenz, ethology at Peckham stumbled on the question of how to observe behaviour in its natural environment without prejudicing the naturalness of that behaviour in the act of observing. The solution adopted at Peckham was for the observers to be integrated within the social and physical environment, becoming active participants. Such biologist-participant-observers were known as "bionomists" and their work was "human ethology".[44] Bionomics was first coined in 1888 with relation to "the branch of biology which deals with the mode of life of organisms in their natural habitat, their adaptation to surroundings".[45] Yet, in a similar way to ethology, the word "bionomics" had largely been ignored in favour of the word "ecology"; the 1948 *Encyclopaedia Britannica* described it as "the study of an organism in relation to its environment" and related it to ecology and zoology.[46] Whilst Scott Williamson's ethology was eclipsed by the emerging field of animal behavioural work, bionomics remained a niche term that Chance later attempted to adopt and redefine in his work on laboratory animals. In this, Chance himself pioneered a new approach to laboratory animals with lasting consequences, which, as yet, has remained unexamined by historians.

Bionomics, an Instrumental Ethology

In Birmingham, Chance incorporated his Peckham experience with his work on laboratory animals and supplemented it with the growing literature on animal ethology. In 1947 he explored how temperature influenced the degree of mutual excitement

[43] G Scott Williamson, 'Health centres: a lecture given to the Town and Country Planning Association', *Nursing Times*, 1946, **39**: 64–5, p. 64.

[44] I H Pearse, *The quality of life: the Peckham approach to human ethology*, Edinburgh, Scottish Academic Press, 1979, pp. 151–2.

[45] *Oxford English Dictionary*.

[46] Anon., 'Bionomics' *Encyclopaedia Britannica*, Chicago, Encyclopaedia Britannica, 1948, vol. 3, p. 621.

experienced by benzedrinized mice, determined that the intensity of light had no obvious effect, and ascertained that the presence of intermittent sounds significantly increased the toxicity of amphetamine.[47] By the early 1950s Chance held a consolidated view that animal variance itself was variable in relation to the social and physical environment, implying the need to find ideal environments where animal uniformity would be maximized. In widening his investigations to encompass the role of the physical as well as social environment, he anticipated a similar move by Tinbergen, whose work was increasingly to influence Chance as the decade progressed. Yet distinctive to Chance's construction of ethology was the degree to which it was determined by purpose, the study of laboratory animals being tied to their standardization for the purpose of reliable experimental design. Chance argued that genetic and pathogenic standardization of laboratory animals could only be partially successful in producing reliable laboratory animals, as the social and physical environment was equally if not more important in determining their physiological reactions.[48] Only if the "natural" behaviour of laboratory animals was understood, and their needs provided for, could the uniformity of laboratory animals be guaranteed. Ethology, in a specific and instrumentally orientated form, could provide this knowledge. By the mid-1950s Chance had a well developed programme of instrumental ethology which he named bionomics in an (unacknowledged) echo of the Pioneer Health Centre.[49] The application of ethology to the study of the needs of laboratory animals was to be the inaugural bionomic study.

The laboratory-centred focus and instrumental orientation make Chance's work representative of a distinct tradition of ethology that has yet to attract historical attention. Bionomics might easily be described as "applied" ethology, its purpose being to identify the natural behavioural needs of animals which, when understood, could be used to improve the ways and means by which humans used animals for their own ends. The ethological study of laboratory animals in order to standardize their properties and reconstruct them as reliable laboratory tools was intended as a "proof of concept" project opening up a new field of bionomic ethology. The project crystallized in 1955 as a proposal passed to Solly Zuckerman (then professor of anatomy at the University of Birmingham) for assessment.[50] Chance argued that a "bionomics laboratory" was necessary within the department of pharmacology to explore the "economy of the animal" and develop techniques to maximize its efficient utilization.[51] The production of standardized laboratory animals was taken as an example where failure to consider the role of the environment had led to the adoption of uneconomic and potentially unnecessary practices such as

[47] M R A Chance, 'Factors influencing the toxicity of sympathomimetic amines to solitary mice', *J. Pharmacol*., 1947, **89**: 289–96.

[48] M R A Chance, 'Environmental factors influencing gonadotrophin in the rat', *Nature*, 1956, **177**: 228–9.

[49] Letter from M R A Chance to S Zuckerman, 13 June 1955, file SZ/BU/9/5, Zuckerman Archive, University of East Anglia, UK (hereafter ZA). Chance never attributed his use of bionomic nor his thinking to his experience at Peckham.

[50] For Zuckerman, see P L Krohn, 'Solly Zuckerman Baron Zuckerman, of Burnham Thorpe, OM, KCB', *Biog. Mem. Fellows R. Soc*., 1995, **41**: 577–98; J Burt, 'Solly Zuckerman: the making of a primatalogical career', *Stud. Hist. Philos. Biolog. Biomed. Sci*., 2006, **37**: 295–310.

[51] M R A Chance, 'Bionomics laboratory in the department of pharmacology, Medical School, Birmingham', 13 May 1955, ZA SZ/BU/9/5.

inbreeding. Bionomic analysis would identify the relatively small and easily overlooked environmental changes that caused disproportionate variance in common laboratory animals with emphasis laid upon the role of temperature, humidity, lighting, sound, nutrition, social interaction, the physical living space, and "natural behaviour".[52] Until this point the "natural" behaviour of laboratory animals had barely been recognized as a category, certainly it was of little or no concern to ethologists of the Lorenz–Tinbergen school, who constructed their science in opposition to laboratory based studies.[53] Lorenz was particularly vociferous on this issue, characterizing ethology against the methodological mistakes made by behaviourists and comparative psychologists. The latter he castigated for focusing upon a single species (the rat) and presuming that natural behaviour could be understood through the experimental study of laboratory animals.[54] Lorenz was famously committed to the concept that domestication necessarily brought about the degeneration of natural behaviour, an opinion he retained despite its resonance with Nazi ideologies with which his own name was all too often connected.[55] In pursuing ethology in the laboratory for instrumental ends, and in recognizing domesticated laboratory animals as animals with natural needs, Chance's ethology is distinguished from what has hitherto been recognized by historians as ethology.

Chance saw bionomics as the route to identifying and then providing for the natural (instinctual) needs of laboratory animals in order to produce a "normal", physiologically "uniform", and thus "co-operative" laboratory animal that would in turn be both reliable and economical. However, Zuckerman failed to see any merit in bionomics and the laboratory was not to be. By the 1950s Zuckerman was a rising star of British science, well connected professionally as well as socially and politically. His scientific name had been established in the late 1920s by his lengthy study of the social structure of primates, the analysis of which he undertook on the presumption that sexual physiology originated and sustained social interaction.[56] Zuckerman's involvement with the field of endocrinology suggests that he had the capacity to recognize the importance of Chance's arguments on the need for standardized animals. Yet Zuckerman objected to what he saw as a contradictory oscillation between the laboratory animal as both object and subject of study. He complained that he was "not at all clear" about the proposal as at one moment it was "concerned with the factors which modify bioassay responses", and the next "with the study of animals' 'natural' behaviour in various circumstances".[57] Zuckerman accused Chance of being unable to decide between a pharmacological investigation of how particular physiological responses were affected by changes in the environment or an ethological study of how an animal's behaviour

[52] Ibid., pp. 2–3.

[53] See J R Durant, 'Innate character in animal and man: a perspective on the origins of ethology', in C Webster, *Biology, medicine and society*, Cambridge University Press, 1981, pp. 157–92, and R W Burkhardt , 'Ethology, natural history, the life sciences, and the problem of place', *J. Hist. Biol.*, 1999, **32**: 489–508.

[54] For example, K Lorenz, 'The comparative method in studying innate behaviour patterns', in *Physiological mechanisms in animal behaviour (symposia of the Society for Experimental Biology)*, Cambridge University Press, 1950, pp. 221–68.

[55] See Burkhardt, op. cit., note 1 above, esp. pp. 249–54. On Lorenz's association with Nazi ideology, see Kalikow, op. cit., note 3 above.

[56] S Zuckerman, *The social life of monkeys and apes*, London, Kegan Paul, 1932.

[57] Letter from S Zuckerman to M R A Chance, 7 June 1955, ZA SU/BU/9/5.

changed in nature.[58] The fact that Zuckerman carefully placed the word *natural* in the term *natural behaviour* in quotation marks signified an important distinction in how he, as opposed to Chance, understood the ontology of the laboratory animal. For Zuckerman the laboratory animal was distinct from what it may have been in the wild, distinct in that it was no longer dignified with natural behaviours akin to its wild ancestor. Moreover, reference to natural behavioural needs threatened to undermine the status of the laboratory animal as a technical object, a tool, used to achieve specific ends. In contrast, Chance believed laboratory animals remained imbued with behavioural needs, albeit distinct from those they may have had in the wild, but nevertheless natural for an animal whose natural environment was the laboratory. If these needs were not recognized and met, then animals would fail in the work they were put to in the laboratory. It is likely that Zuckerman and Chance's positions were closer than either recognized, the miscommunication arising in part from Zuckerman's erroneous presumption that Chance sought to examine animals in the wild. Chance disputed Zuckerman's reading, arguing that the laboratory animal was a subject with history, individuality and nature, all of which had to be understood and managed if the animal was to be experimentally reliable and economically utilized. Ethology, Chance argued, had yet to be applied to instrumental ends, and a "major effort in this direction" was desirable not only to guarantee the reliability of animal dependent science but to ensure that new knowledge of animal behaviour produced by ethology was employed to its full potential.[59] Zuckerman remained sceptical, blocking the university from providing any support, arguing:

> We all know that an indefinable number of factors govern the responses of laboratory animals, and for that reason we discipline ourselves by a variety of procedures which have been evolved mainly through bitter experience. I have no doubt that the situation is far from ideal, but should not think that its existence makes it "sufficiently clear" that what we want is a comprehensive study of environmental factors . . . I should not support any attempt to stimulate studies on animal behaviour on the score that doing so will improve bioassays.[60]

His defence of individual "bitter experience" against a standardized methodology placed Zuckerman outside the mainstream currents of animal-dependent science of this time. Given his status and position it is improbable that he was unaware of the national effort to standardize the production and use of laboratory animals emerging from the 1940s.[61] Moreover, the fact that Zuckerman's own science was grounded in a faith in experiment guaranteeing the universality of scientific knowledge, his objection to a standardized approach to the problem of animal variation requires explanation.

In fact, Zuckerman's position is consistent in that his conviction that experiment alone guaranteed scientific knowledge had led him to reject ethology as a viable science.[62] He viewed ethology as a return to the anthropomorphic and anecdotal approach to animal behaviour which he thoroughly rejected in his 1932 study of the social structure

[58] 'Comments on Chance's proposals', dated 19 May 1955, ZA SZ/BU/9/5.

[59] 'The present status of environmental control in bioassay', pp. 2–4, ZA SU/BU/9/5.

[60] Letter from S Zuckerman to M R A Chance, 20 June 1955, ZA SU/BU/9/5.

[61] Kirk, op. cit., note 18 above.

[62] For Zuckerman's antipathy to ethology, see the postscripts to the second edition of *The social life of monkeys and apes*, London, Routledge and Kegan Paul, 1981.

of primates.[63] Zuckerman was not alone in harbouring such suspicions, although he was particularly active in vocalizing them. One might read the whole of Tinbergen's work of the 1950s as an attempt to establish ethology as a science against those who critiqued it for relying upon subjective methodology. This is particularly evident in Tinbergen's meticulous avoidance of animal mentality so as to evade accusations of anthropomorphism.[64] Nevertheless, Tinbergen was aware that the claims of many ethological studies *were* based on subjective observations that could not be proved by replication, particularly those of Lorenz. This has led one biographer to describe Tinbergen as being the one who "turned ideas into science".[65] Despite Tinbergen's drive toward objectivity, ethology remained associated, for many, with subjective relationships and speculative claims. Consequently, and regardless of Tinbergen's assertion that ethology was the "biological study of behaviour", a precise definition for ethology proved difficult to come by.[66] The only unique methodological characteristic to those outside (and many inside) the field was an indefinable *je ne sais quoi* in the relationship between ethologist and animal.[67] For example, in 1957 William S Verplanck could describe an ethologist only as "a behaviourist who likes his animals".[68] Some, notably Lorenz, celebrated this characteristic, claiming ethology's methodological success derived from being "emotionally involved" with one's animals to the point of "falling in love".[69] Chance was no different in this respect and is remembered for having an enthusiasm for animals that fascinated his colleagues.[70] Nevertheless, establishing the authority of ethological knowledge upon personal expertise and subjective relationships made ethologists experts of a peculiar kind. The exclusivity of ethology's methodology did not make for an amicable relationship with established "scientific" approaches to animal behaviour. This is reflected in the fact that reputable journals such as *Nature* were willing to debate whether ethology was a respectable science at all.[71]

For Zuckerman and others the answer was no. Ethology was anecdotal and anthropomorphic, and masqueraded as science. Ethology threatened to return to science aspects of nineteenth-century studies of animal behaviour that Zuckerman had personally sought to eradicate. For Zuckerman scientific methodology demanded that "animals have only objective existence".[72] In particular, he would not tolerate the ethological tendency to read from animal to human behaviour.[73] He did not deny that the difference between animal and human was one of degree, but he felt that the immensity of that degree was often

[63] Zuckerman, op. cit., note 56 above.

[64] E Crist, *Images of animals: anthropomorphism and animal mind*, Philadelphia, Temple University Press, 1999.

[65] H Kruuk, *Niko's nature*, Oxford University Press, 2003, p. 321. See also p. 300 for Lorenz's propensity towards making authoritative statements based on "suggestive hints in his observations" as opposed to "proven facts".

[66] N Tinbergen, 'On aims and methods of ethology', *Zeitschrift für Tierpsychologie*, 1963, **20**: 410–33.

[67] See, for example, C G Beer, 'Was Professor Lehrman an ethologist?', *Animal Behaviour*, 1975, **23**: 957–64.

[68] W S Verplanck, 'A glossary of some terms used in the objective science of behaviour', *Psychol. Rev.*, 1957, **64**: 1–42, p. 14.

[69] See Konrad Lorenz, *Studies in animal and human behaviour*, 2 vols, London, Methuen, 1970, vol. 1, p. xvi.

[70] Lou Fourcher, personal communication, 15 March 2004.

[71] Anon., 'Is ethology respectable?', *Nature*, 1967, **216**: 10.

[72] Zuckerman, op. cit., note 56 above, p. 11.

[73] For example, S Zuckerman, 'On aggression, Konrad Lorenz', *Nature*, 1966, **212**: 563.

disregarded by ethologists who inclined toward anthropomorphic explanations.[74] Zuckerman therefore would have been predisposed to reject Chance's bionomic laboratory on the grounds of its ethological methodology. This disposition can only have been reinforced by Chance's wider ethological work, part of which involved the reworking of Zuckerman's conclusions on primate social behaviour.[75] Prior to 1955 Chance's primate studies had on the whole reinforced Zuckerman's earlier thesis that sexual attraction was the fundamental bond in primate society.[76] However, from 1956 Chance diverged from this view, arguing that threat and aggression were the structuring factors of society. This proved a fundamental shift in Chance's thought that increasingly shaped his investigations of social structure. At the same time it led to a growing sense of acrimony with Zuckerman. In 1958, at the fifteenth meeting of the International Congress of Zoology, Zuckerman relentlessly dissected Chance, even when Chance was merely asking another speaker a question.[77] At another juncture, Zuckerman denied that his work in any way implied the need for comparative analysis such as that undertaken by Chance. Finally, he challenged Chance to explain fully why "the advent of ethology provides a theory which allows one to link the behaviour of man to that of the rest of the animal kingdom".[78] Regrettably Chance's replies are unrecorded; but these episodes suggest that Zuckerman was indeed predisposed towards dismissing Chance on the grounds of his ethological methodology. Zuckerman's unassailable influence ensured the university rejected the proposal for a bionomic laboratory. Chance was thereby forced to seek outside support in order to pursue this research. Ironically, it was the subjective relationship that Zuckerman so reviled, conjoined with the Peckham association between environment and health, which secured the necessary funding for Chance to continue the ethological study of laboratory animals.

Ethology and the Welfare of Laboratory Animals

Chance found support for the ethological study of laboratory animals from the Universities Federation for Animal Welfare (UFAW), a small self-consciously "scientific" animal welfare society. Established in 1926 by Charles Hume, UFAW provided a forum for those with scientific expertise to act towards bettering the welfare of animals without fear of involvement with antivivisectionism. UFAW's constitution had forbidden the discussion of vivisection until 1942 when Hume sought to utilize its scientific credentials to improve laboratory animal welfare. He combined being a committed High Anglican with active participation in organizations such as the British Science Guild and the Association of Scientific Workers (AScW), the latter of which began a campaign in the 1940s

[74] Zuckerman, op. cit., note 56 above, p. 17.

[75] M R A Chance and A P Mead, 'Social behaviour and primate evolution', in *Evolution (symposia of the Society for Experimental Biology VII)*, Cambridge University Press, 1953, pp. 395–439; letter from M R A Chance to S Zuckerman, 27 July 1953, ZA SZ/BU/9/5.

[76] M R A Chance, 'Social structure of a colony of Macaca mulatta', *Br. J. Animal Behav.*, 1956, **4**: 1–13.

[77] H R Hewer and N D Riley, *XVth International Congress on Zoology: proceedings*, London, XVth International Congress on Zoology, 1959, p. 860. Zuckerman interjected to ask whether the observations Chance mentioned were made by him directly or by others.

[78] Ibid., p. 862.

(co-ordinated by Chance's colleague Bacharach) to improve the quantity and quality of laboratory animals available to British science.[79] Through the AScW Hume recognized that the fear of low quality laboratory animals undermining the reliability of British science offered UFAW an opportunity to intervene on the subject of laboratory animal welfare. In 1947 UFAW appropriated the demand for standardized laboratory animals to forward its own agenda with the publication of *The UFAW handbook on the care and management of laboratory animals*. One of the first general guides to offer standardized practices for laboratory animal management, the *UFAW handbook* was immensely successful as a "standard" reference work.[80] The historical importance of this book lay in the amalgamation of practices of standardization with the promotion of animal welfare. It deployed welfare for instrumental ends to produce, as Bacharach put it in the pages of the *British Medical Journal*, "a very practical blend of economics and humanitarianism".[81] The philosophy of the *UFAW handbook* compounded economics and ethics within material practices demonstrating that:

> in the long run it pays to be kind to animals ... you can ... get with healthy contented animals more information from the same number, or the same amount of information from a smaller number, than you can from sick or miserable animals.[82]

This amalgamation of economics and humanitarianism, filtered through the Peckham approach to health and environment, allowed Chance to recast bionomics (the study of "animal economy") as a programme to find the best environment to promote the health, welfare and efficacy of laboratory animals. Bionomics as a name was dropped in favour of welfare, but there was no alteration in the aims and practices of the work. The ethological study of laboratory animals remained instrumental in focus, but became embedded within a discourse that seamlessly integrated scientific reliability and economic efficacy with that of animal welfare.

Chance may have encountered UFAW through the *UFAW handbook*, or via Bacharach, who joined the organization in the 1950s, or possibly through Alistair Worden, who was a long-time member, edited the *UFAW handbook*, and would have known Chance's work from editing the journal *Animal Behaviour*.[83] Whatever the case, in late 1955 UFAW awarded Chance a financial grant "in the hope that his work will lead to a substantial lessening of the number of animals required in certain bio-assays".[84] The announcement in the *Lancet* instigated a series of letters that echoed Zuckerman's prior criticisms. The psychologist R H J Watson (University College London) objected to the claim that Chance's work "inaugurates a long-overdue study of the psychology of laboratory

[79] C W Hume, *The status of animals in the Christian religion*, London, UFAW, 1957. For the British Science Guild, which served as a model for UFAW, see R MacLeod, 'Science for imperial efficiency and social change, reflections on the British Science Guild, 1905–1936', *Public Understanding of Science*, 1994, **3**: 155–93.

[80] A N Worden, *The UFAW handbook on the care and management of laboratory animals*, London, Baillière, Tindall and Cox, 1947.

[81] A L Bacharach, 'Laboratory animals: the UFAW handbook on the care and management of laboratory animals', *Br. Med. J.*, 1949, **ii**: 20–1.

[82] Ibid., p. 20.

[83] Bacharach served on UFAW's Scientific Advisory Committee from 1956 to 1962 but may have been associated with it earlier, meeting Hume through their involvement with the AScW. See Bound Annual Reports, 1955, **29**: 3, UFAW Archive, Wheathampstead (hereafter UFAW).

[84] Bound Annual Reports, 1955, **29**: 5, UFAW.

animals" as this claim ignored the "considerable volume of work" by comparative psychologists.[85] Like Zuckerman, Watson had misunderstood the subtlety of Chance's approach. Hume responded:

> What UFAW is inaugurating is a study, for the benefit of the animals themselves, of their psychology under the conditions which laboratory conventions have provided for them but which may, for all that is at present known, be far from optimal from the animals' point of view . . . In the course of an assay on which the weight of the ovaries was the criterion, Chance found that when female rats were solitary in their cages the results showed an enormous variance, whereas when each rat had one female cage-mate a sensational reduction in variance was obtained. Could all Munn's army of mazologists have predicted this curious psychosomatic effect? It may well turn out to be only one of a number of hitherto unnoticed factors affecting the mental comfort of laboratory animals.[86]

Hume's response indicates Watson's criticism was motivated by inter-disciplinary conflict resulting from ethology's challenge to the laboratory based methodology of behaviourists and comparative psychologists. Those more favourable to ethology, such as the Cambridge based psychologist O L Zangwill, wrote to welcome UFAW's sponsorship of Chance.[87] The dispute in the *Lancet*, like Zuckerman's criticism before it, focused on the credibility of ethological methodology and the purpose to which it was to be put. This illustrates the resistance to ethology in some quarters, as well the novelty of Chance's approach. Notably, there was no objection to the economic thrust of the project, or to the involvement of UFAW and the consequent amalgamation of experimental, economic, and welfare considerations.

UFAW hoped Chance would establish that "unlike test tubes, laboratory animals (including the humble mice) have minds and feelings as well as bodies".[88] UFAW's sponsorship of Chance formed part of a wider project to develop "humane experimental technique" which produced the approach to laboratory animal use known as the "3Rs". These were the *refinement* of experimental technique so as to minimize suffering, the *reduction* of the number of animals used in a given experiment, and the *replacement* of sentient animals wherever possible by technical innovation. The 3Rs are conventionally associated with the publication of *The principles of humane experimental technique* by W M S Russell and R L Burch in 1959.[89] This book was the product of a research project funded by UFAW and is best seen as the codification and extension of the organization's agenda to promote the welfare of laboratory animals. Its principle author,

[85] Anon., 'UFAW', *Lancet*, 1958, **ii**: 631–2, p. 632; R H J Watson, 'Psychology of laboratory animals', *Lancet*, 1958, **ii**: 747.

[86] C W Hume, 'Psychology of laboratory animals', *Lancet*, 1958, **ii**: 802. Norman L Munn was a psychologist known for his textbooks which compiled the results of comparative psychologists' studies of animal behaviour. See, for example, N L Munn, *Handbook of psychological research on the rat: an introduction to animal psychology*, Boston, Houghton Mifflin, 1950. Comparative psychology famously used rat choice in the maze as its model for the majority of its studies, see, for example, E C Tolman, 'The determiners of behavior at a choice point', *Psychol. Rev.*, 1938, **45**: 1–41, p. 34.

[87] O L Zangwill, 'Psychology of laboratory-animals', *Lancet*, 1958, **ii**: 851. In 1953, Zangwill, along with W H Thorpe, initiated a research group at Cambridge to encourage communication between the diverse approaches to animal behaviour, see W H Thorpe and O L Zangwill, *Current problems in animal behaviour*, Cambridge University Press, 1961.

[88] Bound Annual Reports, 1964, **38**: 21, UFAW.

[89] W M S Russell and R Burch, *The principles of humane experimental technique*, London, UFAW, 1959. Chance's influence on this work is made evident in the references, see particularly pp. 123–33.

William Moys Stratton Russell (the son of the marine biologist Sir Frederick Stratton Russell) was an Oxford trained zoologist strongly influenced by Tinbergen's ethological method. Russell's development of the 3Rs as principles of "human experimental technique" was also influenced by Chance's work, and as a consequence the two became lifelong friends, sharing interests in the study of human and animal behaviour. Arguably, the main legacy of Chance's work was in anticipating and thereby shaping the development of the refinement of experimental animal use.[90] Today the 3Rs form the basis of ethical approaches to animal experiment throughout the world.

The 3Rs were first publicly articulated on 8 May 1957 at a joint symposium on humane technique in the laboratory organized by UFAW and the Medical Research Council's Laboratory Animals Bureau. Chance was a participant at this symposium, presenting a paper on the contribution of the environment to laboratory animal uniformity.[91] Reflecting both the work of Russell, and the philosophy of UFAW, Chance described how the equivalence of health and happiness with "normal" and "uniform" physiology made the material practices of promoting laboratory animal welfare inseparable from the promotion of experimental reliability. However, he lamented the simplistic approach of many to laboratory animals:

> One gets the impression ... discussing these matters with biochemists, pharmacologists, and other workers in the sciences ancillary to medicine that humidity is important to control lest the animals tend to dry up, rather than that the alterations in the physiology which may be made necessary by too humid or too arid an atmosphere are themselves factors which will distort the animal's response to drugs or various experimental practices.[92]

In the 1960s physical environmental factors such as cage design, temperature, sound, and lighting, as well as the social relations of laboratory animals, became established as new territories of intervention. In this regard, cage design was a particular focus of the work undertaken by Chance, with emphasis shifting from convenience to human user to the "happiness of the cages' inhabitants".[93] In 1963 Chance obtained his laboratory, named after his benefactors. The UFAW Environmental Research Unit (Humane Experimental Technique) was inaugurated at the University of Birmingham with a symposium on the design of laboratory animal caging.[94] By advocating the instrumental necessity of providing a "happy home life" for laboratory animals, Chance promoted a subjective understanding of laboratory animals which encompassed a consideration of their physiological and psychological needs. The symposium placed emphasis upon the importance of meeting animal needs as much as, if not more than, those of the human user. This contrasted with conventional approaches to experimental design, as a result of which, after decades of working with rats, science still did "not know how to treat a rat, for example, to put him on his best behaviour for the test in hand".[95] The phrase "to put him on his best behaviour" is indicative of Chance's tendency to view the relationship between

[90] Emphasis on the design of environment to meet the needs of the animal is now termed "environmental enrichment".

[91] M R A Chance, 'The contribution of environment to uniformity', in *Collected Papers – Laboratory Animals Bureau*, 1957, **6**: 59–73.

[92] Ibid., p. 59.

[93] Bound Annual Reports, 1958, **32**: 8, UFAW.

[94] Bound Annual Reports, 1957, **31**: 4, UFAW.

[95] Chance, op. cit., note 91 above, p. 71.

experimenter and animal as one of co-operation. A further example can be seen in Chance's argument that, conventionally, experiments using rats:

> have been carried out during the day-time, which is most convenient for ourselves but, as it happens, is in the middle of the "night" for the rat. Most laboratory procedures, therefore, involve for a start, kicking the rat out of bed and then asking it to go through some fairly active procedures.[96]

From the "rats' point of view" they spent their "day" in full activity running two to three miles even in cage conditions and then spent their "night" being deprived of sleep by curious experimenters. Such conditions, Chance contended, ensured only that laboratory animals displayed uncooperative and irrational behaviour. It made them poor research objects, particularly when investigating drugs such as amphetamine which directly impacted upon sleep and activity. Taking the animals' point of view into consideration suggested:

> that a moderate degree of alertness will be found a necessary condition for bringing uniformity into the behaviour of rats . . . bioassay procedure will benefit from an amount of constraint sufficient to call the attention of the rat to the matter at hand. Misunderstandings between rats and experimenters would then be much fewer![97]

This language is strikingly anthropomorphic and echoes the ethos of the Pioneer Health Centre. The focus on the relationship between experimenter and experimental object, the emphasis upon the provision of the right environment, and the assumption that the promotion of health and welfare would bring about a co-operative and productive experimental subject mirrored the Peckham philosophy. When Chance subsequently installed reverse lighting into his laboratory (which for the rats made human day night and human night day) the principle was similar to Scott Williamson's decision to install open fires to act as "a focal point around which social mixing is natural".[98]

This movement from human to animal was not specific to Chance; after all the Pioneer Health Centre began with Scott Williamson's observations of laboratory rats. Others too recognized the importance of the relationship between experimenter and laboratory animal at this time. Peter Medawar, a strong advocate but no practitioner of ethology, described a "depth of obligation" whilst the physiologist E D Adrian identified it as essential to the reliability of experimental science.[99] William Lane-Petter, director of the MRC's Laboratory Animals Bureau, understood this relationship as analogous to the mutual obligation existent in the encounter between clinician and patient:

> Veterinarians and paediatricians, whose patients normally possess uncomplicated mentalities, are familiar with their ability to tolerate without distress lesions and manipulations that most human adults would find insupportable; but they also know this tolerance can only be evoked if there is

[96] Ibid., p. 70.
[97] Ibid., p. 71.
[98] This was achieved by providing white light at night and low level red light during the day to simulate night.
[99] P B Medawar, 'Foreword', in *Collected Papers – Laboratory Animals Bureau*, 1957, **6**: 5–7, p. 5; E D Adrian, 'Experiments in the nervous system', Stephen Paget Memorial Lecture, 22 November 1950', *Conquest*, 1951, **39**: 2–14.

a satisfactory relationship between patient and clinician. The same is true of the experimental animal.[100]

The clinical analogy served to convey the increasing importance of the notion of co-operation between experimenter and laboratory animal, and the consequent acknowledgement of shared physical, psychological and social relationships. In this, ethology was the principle vector through which scientific and moral necessity came to be integrated within the material practice of the laboratory. As a result, ethology opened a new relational territory wherein the subjectivity of laboratory animals could be recognized.

Reference to clinical practice was more than analogy; clinical observation and narrative description were implicit to these developing experimental practices and reflected the wider growth in reflexive thinking of the time. There is little to distinguish Chance's methodological approach to laboratory animals from Scott Williamson's approach to human health (itself drawn from observations of laboratory animals). Both were grounded in "ethology" and embodied a shared ethos and similar practices established about non-interventionist observation and narrative description. Indeed, the same ethological practices Chance used to comprehend the behavioural needs of laboratory animals he later deployed to understand the behavioural patterns of psychiatric patients.[101] For Chance, ethology consisted of objective practices of observation and analysis that could be applied equally to human or animal behaviour.[102] In being predicated upon evolutionary theory, ethology could not help but transgress the boundary between "human" and "animal", a characteristic particularly evident in Lorenz's popular writings. In 1967 the notion of a human–animal boundary was fundamentally challenged by Desmond Morris's bestseller *The naked ape*.[103] Morris was a student of Tinbergen in the 1950s and a contemporary of Russell in the Department of Zoology at Oxford. He had remained friends with his old tutor, who, late in his career, followed his student and became convinced that ethology's potential lay in its application to human behaviour.[104] For some, this was no more than "human bird watching", but it was considered important enough none the less for the MRC to invite Tinbergen to speak to them on the subject on more than one occasion.[105]

The ease with which ethology moved from the animal to the human is exemplified by a course that Chance began to teach to medical students at Birmingham in the late 1960s.

[100] W Lane-Petter, 'Humane vivisection', *The Practitioner*, 1963, **190**: 81–4, p. 81.

[101] M R A Chance, 'Ethological discoveries on spacing and their relevance to psychiatry', *University of Birmingham Faculty of Medicine and Dentistry Bulletin*, 1970, **27**: 21–3; R H Polsky and M R A Chance, 'An ethological perspective on social behaviour in long stay hospitalized psychiatric patients', *J. Nerv. Mental Dis.*, 1979, **167**: 658–68; R H Polsky and M R A Chance, 'An ethological analysis of long stay hospitalized psychiatric patients. Senders and receivers in social interaction', *J. Nerv. Mental Dis.*, 1979, **167**: 669–74; R H Polsky and M R A Chance, 'Social interaction and the use of space on a ward of long term psychiatric patients', *J. Nerv. Mental Dis.*, 1980, **168**: 550–5.

[102] M R A Chance, 'How behaviour analysis became possible', MRAC.

[103] Desmond Morris, *The naked ape*, London, Jonathan Cape, 1967.

[104] N Tinbergen, 'On war and peace in animals and man', *Science*, 1968, **160**: 1411–18. Russell, too, believed this to be the case and had anticipated Morris's work, see C Russell and W M S Russell, *Human behaviour*, London, André Deutsch, 1961.

[105] For Tinbergen's invitation to discuss "human bird watching" with the MRC's Clinical Psychiatry Committee on 5 February 1962, see file FD 22 66 in the MRC Archive, National Archives, Kew, UK.

Titled 'How to observe', the course was a response to an earlier investigation on the capacity of medical students objectively to observe behaviour. Chance had assessed the abilities of medical students to compare and describe the behaviour of pairs of rats, in each case one normal and one having been dosed with a psychotherapeutic drug thought to restrict certain behaviours and exaggerate others.[106] Chance found that what the students decided to *look for* had little to do with what they *looked at* but rather emerged from preconceived ideas drawn from existent medical knowledge and traditional terms for describing human behaviour. Outside of preconceived concepts (such as "aggression", "anxiety", "intelligence") students lacked the ability objectively to describe observed behaviour, and in many cases failed to distinguish between drugged and normal rats. Nor did they attempt to observe the rats' "natural" behaviour, instead they chose to produce behaviour by experimentally interfering. The fact that students deferred to what they thought they knew rather than what was actually before them, in Chance's view, revealed serious deficiencies in the medical curriculum as then taught. In clinical practice such tendencies produced "wasted effort", as lines of investigation based on false premises led to possibly tragic consequences. Accordingly, Chance developed a course to teach students not just what to observe but how to reflect upon the way observational evidence is integrated with existing knowledge. Based on the practical observation of the behavioural changes in rats dosed with various psychotherapeutic drugs, the course was predicated upon the assumption that the observation of animal and human behaviour, whether for the purpose of laboratory science or clinical medicine, was grounded in the same practices.[107] In this way ethology provided a vector of communication between the laboratory and the clinic, locations with systems of knowledge and practices that have often been taken to be incommunicable by historians of science and medicine.[108]

Conclusion

Chance's ethology re-introduced to the laboratory the animal as a subjective, feeling, living being, and in doing so made explicit the subjective relationship between laboratory animal and experimenter. This is evident in Chance's articles published in pharmaceutical journals where the animal, its behavioural patterns and needs are brought to the fore. Ethology's introduction into the laboratory transformed the way laboratory animals were represented, and anticipated a wider reconfiguration of the ways in which animals were conceptualized, encountered, and used in laboratory science. Chance's work contributed to the opening up of a new territory of intervention for those concerned with the health, welfare and management of laboratory animals. This territory was mapped by old and

[106] M R A Chance and D A Humphries, 'Medical student's powers of observation', *Br. J. Med. Ed.*, 1967, **1**: 141–34.

[107] M R A Chance, 'A class in the observation of behaviour', MRAC.

[108] C Lawrence, 'Incommunicable knowledge: science, technology and the clinical art in Britain 1850–1914', *J. Contemp. Hist.*, 1985, **20**: 503–20; C Lawrence, 'Still incommunicable: clinical holists and medical knowledge in inter-war Britain', in C Lawrence and G Weisz, *Greater than the parts: holism in biomedicine 1920–1950*, Oxford University Press, 1998, pp. 94–111; I Löwy and J P Gaudillière 'Disciplining cancer: mice and the practice of genetic purity', in J P Gaudillière and I Löwy, *The invisible industrialist: manufacturers and the production of scientific knowledge*, London, Macmillan, 1998, pp. 209–49.

new organizations such as UFAW, the MRC's Laboratory Animal Bureau (established in 1947) and the Animal Technicians Association (established in 1949). It was codified in new journals such as *Collected Papers – Laboratory Animals Bureau*, *The Journal of the Animal Technicians Association*, and *Laboratory Animals*, as well as in monographs including the *UFAW handbook*. Perhaps most importantly, ethology served as a vector by which experimental and ethical necessity, the instrumental and the moral, could be reconciled.

Burkhardt, in his definitive history of the Lorenz–Tinbergen approach to ethology, argued that the "course of ethology's development has been more responsive to contingencies, more 'ecological' in its relations to the specific and diverse settings of its ongoing construction and thus more interesting historically".[109] In this sense Chance's work forms a historically unexplored example of ethology's ecologies, an important instance of the adaptability of ethology. Burkhardt's biographical approach to the understanding of ethology, a model that is extended here to include Michael Chance, followed Lorenz's characterization of his and Tinbergen's approaches to ethology. Lorenz, the "farmer", bred and raised the animals he studied, lived in close proximity to them, and consequently knew almost instinctively normal and abnormal behaviours and was happy to claim knowledge that rested on such subjective relationships. Tinbergen, "the hunter", tracked and observed animals in the wild, explored their adaptation to natural environments, focused on the development of a rigorous methodology, and was cautious about the claims he made for ethological science. If we were to extend the metaphor to characterize Chance, he would be the "worker", conscious that domesticated animals retained natural needs, and aware that they would necessarily be used for instrumental ends. He developed methods to harmonize nature and purpose, life and productivity, to benefit both. Pushing further to encompass Chance's politics, a better metaphor would be the "socialist worker". The political context to Chance's work was arguably Marx's writing on work and alienation. Marx, of course, did not have animals in mind when he wrote:

> First, the fact that labour is external to the worker, i.e., it does not belong to his essential being; that in his work, therefore, he does not affirm himself but denies himself, does not feel content but unhappy, does not develop freely his physical and mental energy but mortifies his body and ruins his mind. The worker therefore only feels himself outside his work, and in his work feels outside himself. He is at home when he is not working, and when he is working he is not at home. His labour is therefore not voluntary, but coerced; it's forced labour. It is therefore not the satisfaction of a need; it is merely a means to satisfy needs external to it.[110]

None the less, Marx's words resonate with Chance's work. Chance himself did not explicitly use the language of Marx. Yet he is not far in principle from the numerous Marxist discussions about work and alienation present in post-war western culture, particularly in the literature on human industrial health.[111] Given Chance's politics, his

[109] Burkhardt, op. cit, note 1 above, p. 13.

[110] K Marx, *Economic and philosophic manuscripts of 1844*, New York, International Publishers, 1964, p. 72, quoted in Bertell Ollman, *Alienation: Marx's conception of man in capitalist society*, Cambridge University Press, 1971, p. 137.

[111] The prevalence of alienation in post-Second World War thought is attributed to the translation of Marx's so-called *Economic and philosophical manuscripts of 1844*, see 'Alienation', in C D Kernig, *Marxism, Communism and western society*, New York, Herder and Herder, 1972, pp. 88–93, p. 90. This is not to say that Marx's thinking would have

growing pharmacological interest in toxicology, and his ethological analysis of the psychiatric patient, it is probable that he was familiar with the strand of Marxist-psychological literature which applied the concept of alienation to explain mental and physical ill health in the modern worker.[112] Whether in the provision of health facilities to the working class at Peckham, the understanding of fascism as a social pathology, or in the production of the left wing periodical *Labour Discussion Notes*, Chance's socialist politics shaped his own productive activity including his approach to laboratory animals. Certainly Marxist ideas of alienation resonate with Chance's emphasis upon finding the right relationships and the right environments to ensure a co-operative, healthy, productive and contented laboratory animal.

This paper has traced how ethology opened up a new territory in which the subjectivity of laboratory animals was recognized and brought to the fore in post-war biomedical science. The dynamic relationships which the animal shared with its environment and, most importantly, the human researchers within that environment, consequently became increasingly important considerations in laboratory science. Equally important was the reflexive nature of ethological thought which made the relationship between observer and observed methodologically prominent. In laboratory science this encouraged an explicit recognition of the importance of the mutual, intra-dependent relationship between animal and human, experimental object and experimenter, which remained instrumental but, none the less, increasingly emphasized co-operation and mutual obligation. Evidence for this can be found in the way Chance's work was absorbed within the wider agenda of UFAW to promote a humane approach to experimental technique. The fact that Chance's principles of environmental design were integrated within the work of the MRC's Laboratory Animal Centre equally underlines the point.[113] Commenting on the work of Lorenz, Vinciane Despret has observed that with ethology the "practice of knowing has become a practice of caring".[114] So it was in the laboratory, where the introduction of ethological modes of thought and practice placed a new emphasis upon subjective social relationships, which in turn brought to the fore notions of mutual responsibility and co-operation between human and animal. In this sense, the production of reliable science could be said to have become dependent upon the production of a responsible, and response-able, scientist.[115]

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suggested animals could suffer alienation—on the contrary Marx denied animal consciousness arguing that the "animal is one with its vital activity". Whilst it remains unclear to what extent Chance attributed consciousness to animals, in proposing laboratory animals were no longer one with their vital activity Chance may have considered them "alienated" in some sense.

[112] See, for example, Olman, op. cit., note 110 above; J Sterner, 'Determining margin of safety-criteria for defining a "harmful" substance', *J. Indus. Med.*, 1943, **12**: 514–18; F A Patty, *Industrial hygiene and toxicology*, 2nd ed., New York, Wiley, 1962, vol. 2, pp. 1303–4.

[113] See, for example, G Clough and M R Gamble, *Laboratory animal houses: a guide to the design and planning of animal facilities*, [Carshalton], MRC, 1976.

[114] Vinciane Despret, 'The body we care for: figures of Anthro-zoo-genesis', *Body and Society*, 2004, **10**: 11–134, p. 130.

[115] For elaboration on this point, see R G W Kirk, 'Reliable animals, responsible scientists: constructing standard laboratory animals in Britain *c*.1919–1976', PhD thesis, University College London, 2005. For the notion of "responsible/response-able", see Donna J Haraway, *When species meet*, Minneapolis, University of Minnesota Press, 2008, esp. pp. 70–3.