Escaping the maze
Wildness and tameness in studying animal behaviour

Lynda Birke

The program of scientific experimentation that leads you to conclude that animals are imbeciles is profoundly anthropocentric. It values being able to find your way out of a sterile maze, ignoring the fact that if the researcher who designed the maze were to be parachuted into the jungles of Borneo, he or she would be dead of starvation in a week.

Elizabeth Costello, in J.M. Coetzee, The Lives of Animals

The growth of human-animal studies (HAS) represents a concern to ‘bring animals in’ – to mainstream sociology, or to the humanities, for example. But there are, of course, areas of scholarship in which nonhuman animals have always been present. Disciplinary divisions, separating social from natural sciences, have maintained a distinction that allots animals and their behaviour largely to biology, while human sociality is usually studied elsewhere. To be sure, there is overlap – in psychology, for instance – and the distinction is increasingly challenged by posthumanism, which seeks to overturn humanist beliefs in us versus the rest of nature (Wolfe 2003; Haraway 2008; Taylor 2012). Even so, the heritage of that separation persists: humans and nonhumans still usually appear as research subjects in different academic journals.

Nevertheless, the study of animal behaviour contributes crucially to scholarship in HAS. One source, for instance, is the work of cognitive ethologists (e.g. Bekoff 2002), whose focus on sentience and cognitive abilities has helped scientists re-evaluate species they study and to realize that whether we perceive animal abilities depends on what questions we ask – as Coetzee clearly recognized.

In this chapter, I focus on biological science; I do so in part because I am a biologist with a background in animal behaviour, but also because biology contributes fundamentally to our ideas of what counts as an ‘animal’ or a ‘species’. I draw, then, on my experiences of working with behavioural ideas, and use this to consider the interconnecting categories of wild/feral/tame that are a theme of this book. Biology studies living organisms, such as animals; some investigations focus on those living in natural habitats – in wild nature. Others may be concerned with animals kept in constrained conditions. This includes studies of domestic or zoo animals, as well as animals kept for laboratory use. Some of the latter may be used in investigations of that species’ biological processes, which can contribute to knowledge about
such processes in general or to veterinary science in particular. Many more, however, are used as models of primarily human diseases, serving as proxies for us in medical research.

Put another way, science relates to nonhuman animals partly by defining them and partly by using them as objects of research. Here, I consider three case studies. The first centres on ideas of ‘wild’ in thinking about species, particularly in ethology; the second moves to laboratory animals, considering them as examples of domestication (taming) in the service of science; the third focuses on feral animals who were studied precisely because of their ambiguous status between wild and tame. I will use these as examples to explore ideas of wildness/tameness/ferality in relation to science, focusing particularly on understanding of animal behaviour.

Biological science defines animals in the sense of arbitrating boundaries between species, and researches their characteristics: their physiology, ecology, behaviour. The concept of species – what is a species? how is it distinguished from another one? – may seem straightforward in colloquial use, but is notoriously troublesome and has bothered biologists seeking definitions for a long time (de Queiroz 2005). The simplest definition refers to populations that can interbreed. Since domesticated animals could in principle interbreed with wild animals of the same species, the idea of ‘species’ cuts across wild/tame distinctions. Still, what I want to stress is that biology first and foremost focuses on species, particularly how they are thought to have evolved, with implications for how animal behaviour is studied.

Domesticating behaviour

Like man [sic], the rat could be said to exist on the boundary between the natural and the unnatural. [. . .] [T]he rat is neither domesticated nor entirely wild; rather it is an unwelcome but perennial cohabitant of the built environment.


Biologists are trained to think primarily in evolutionary terms: how did that species, or that characteristic, evolve? How are organisms adapted to their way of life? Inevitably, then, what species do ‘in the wild’ is the default setting for biology; domesticated animals are largely seen relative to their wild counterparts (discussions of dog behaviour typically refer to behaviour of wolves, for example). Trained in biology, I often do this too; even when thinking about laboratory-bred rodents, I would ask myself what they would do ‘in the wild’. Perhaps their free-living ancestors did behave similarly to these highly domesticated strains, but that is an assumption. And, as the quotation above indicates, what ‘wild’ means depends on species: are rats, living close to our habitations, truly as ‘wild’ as, say, a snow leopard in the Himalayas? Wild, as other chapters explore, is a slippery concept in biology, as elsewhere.

‘Tame’ is no easier to pin down. It generally implies behavioural propensity in relation to humans; that is, animals accept human handling. In this chapter, I mostly use the (equally tricky) term ‘domesticated’ – the word more commonly in use in biology – referring to the species’ history in relation to humans, a long process of change and adaptation. To become ‘domesticated’, species undergo many changes in physiology, anatomy, and behaviour. Some animals, of course, may be domesticated, but not really tame (the bull used for bullfighting, for example). While some species probably played active parts by living close to humans, the process of domestication also entails humans guiding selection of particular traits (Budiansky 1992; Clutton-Brock 1987). In particular, domestication involved selecting for general docility.
Biologists also identify a ‘domestication syndrome’, describing changes not artificially selected but occurring alongside selection for docility, such as colour, shape, teeth, and brain function. Indeed, some scientists believe that species such as dogs self-domesticated first, in the sense that less aggressive animals were more likely to survive close to humans (Coppinger and Coppinger 2001). Later, human communities presumably began artificial selection, seeking further reduction in aggression. Lest this seems relatively straightforward, biologists also identify self-domestication in free-living bonobos, on the grounds that they have evolved many characteristics of the domestication syndrome (Hare et al. 2012). That is, behavioural and physiological ‘domestication’ can occur in the wild, without human intervention.

Feral animals fall in between. To biologists, feral animals imply two things. They may represent ecological disruption if they are ‘non-native’; in that case, biologists might be interested in how to manage their populations in ways less disruptive to native fauna, or with a view to culling them. Alternatively, they might study feral animals with reference to wild counterparts (horses or dogs, for example), on the assumption that, even if derived from escaped domesticates, they still exhibit ‘wild’ behaviour patterns. What interests biologists here is how much feral and wild populations match or diverge, and how ferals have adapted their behaviour to different circumstances. Sometimes, these species provide surprises: they are at times notably more flexible than studies of wild populations would suggest. Feral cats, for example, seem to show greater diversity of social structures than do their wild ancestors (Leyhausen 1988), while feral dogs may live in wolf-like packs, or in fox-like small, territorial groups (Macdonald and Carr 1995). Feral, in short, may not be completely equivalent to wild, any more than domesticated necessarily means tamed.

Turning to my three cases, I consider, first, how much ethological theory and practice hinges on prioritizing ‘the wild’. Ethologists do, of course, study behaviour of domesticated animals (though this usually falls within ‘applied ethology’, a specialized subdiscipline), but they typically do so through the lens of species-in-the-wild. My second case enters labs, to consider a special case of domestication, namely the transformation of previously wild animals into tools of the trade: lab animals, whose behaviour is manipulated to fit experimental needs. Here, their behaviour can be seen as tamed, though only up to a point. My third case takes one particular example, in which domesticated rats went feral; they were studied partly to understand ratty social behaviour in free-living groups, but also with reference to human behaviour. While I exploit the wild/feral/tame distinctions here, the point of examining these cases is precisely to illustrate the slipperiness of such categories.

**Studying behaviour: prioritizing the wild**

Broadly, scientific study of animal behaviour emerged in two areas: ethology and comparative psychology. Ethology, from the Greek for character, is a subdiscipline of biology, focusing on behaviour ‘in the natural environment’; psychology has historically prioritized examining how animals learn, often testing them in constrained environments. That is obviously a simplification which many scientists now would challenge. But these disciplines do have slightly different histories and outlooks. An introduction to ethology sums them up thus: ‘A traditional view of the distinction between ethology and psychology was that the psychologist put his animal in a small enclosure and peered in to see what it was doing, while the ethologist put himself in the box and looked out at what the animals round about were up to’ (Slater 1985: 7).

Because of ethology’s emphasis on behaviour ‘as it occurs in nature’, much of it involves field studies (sometimes, indeed, using a hide). That is, researchers go out into nature, spending hours in detailed observation of the species/behaviour of interest. Although
observational, it is today usually systematized or experimental; researchers use field studies to test specific hypotheses — for example, theories of sexual selection might predict that females behave in particular ways, which can then be investigated in the field.

Small populations can also be studied in captivity, enabling scientists to control conditions in which they make observations; many studies of bird species, such as the European Great Tit, used captive flocks in outdoor aviaries to facilitate observation. And sometimes, ethologists study animals in laboratories in order to test specific hypotheses about, say, what stimuli trigger particular behaviours. What characterizes ethological thinking, however, is the emphasis on wild animals and species’ evolutionary history. Even if researchers study behaviour in laboratory-bred rats (as I have done), then they focus on behaviour assumed to characterize the species ‘in nature’.

Niko Tinbergen — one of the ‘founding fathers’ of ethology — argued that it dealt with the ‘four whys’ of behaviour. These are: causal (why did the animal do this at this moment? hormonal influence would be one example); developmental (why did it happen at that stage of life or development?); function (what function does the behaviour serve in terms of adaptedness to living conditions?); and evolution (why/how did the behaviour evolve?). The last two, however, became priorities for several decades, to the detriment of questions of cause and development. After the mid-1970s in particular, ethology came to focus overwhelmingly on questions of evolution and adaption, with the rise of behavioural ecology and sociobiology, shifts that helped to cement the place of quantification in behavioural studies (Crist 1999; Alcock 2003; Ord et al. 2005).

The drive toward quantification and experiment is inevitable in any emerging branch of science, but, in ethology, it was influenced also by the firmly scientific footing of comparative psychology. Ethology, emphasizing instinct and ‘natural’ behaviour, had arisen primarily in Europe, while psychology, focusing more on experimental investigation of learning, developed more strongly in North America. For some decades in the twentieth century, these two remained apart (Wilson 2002), with considerable implications for how animals — and behaviour scholarship — were seen. In Images of Animals, Eileen Crist (1999) describes two different writing styles in the history of scientific reporting of animal behaviour. Descriptive styles have generally typified natural history, in contrast to the more detached styles of scientific reports. Beginning with Darwin, she explores how his descriptive narrative seemed to acknowledge animal subjectivity and to display a belief in the animal mind. However, by the middle of the twentieth century, much writing about behaviour expressed scepticism about animal minds and put emphasis on experimental method and quantification. Though many early ethologists were great observers of animals, retaining the descriptive detail of earlier naturalists, they also sought to put their work onto solid scientific foundations, doing experiments and explaining behaviour in terms of mechanisms of stimuli and responses.

In similar vein, Dewsbury (1997) compared writing for popular audiences by Tinbergen with that of Frank Beach (an American psychologist who focused on effects of hormones, particularly on sexual behaviour). The contrast illustrates further the divergent styles noted by Crist and the different emphases on description and quantification. Tinbergen made much of his joy in doing research and was happy to attribute mental states to animals: he wrote of wasps ‘happily digging’ or ‘flying leisurely’, and to him one bird can ‘obviously understand’ another (quoted in Dewsbury 1997: 376).

Beach, by contrast, used more sombre terms to describe animals: he referred, for instance, to ‘beasts’ or to ‘lowly rats’ or to alligators showing ‘stubborn resistance’; he was reluctant to impute mental states (Dewsbury 1997: 376–7); and he emphasized the centrality of experiment. The illustrations of these articles followed suit. While Tinbergen’s were full of idyllic
rural panoramas showing animals in situ, Beach’s articles portrayed animals without context; in some, the animal appeared with all background trimmed away. Dewsbury comments, ‘It is as if the animal can be shown in the abstract, independent of the environment in which it [sic] exists’ (378).5

The scientific approach to animal behaviour brought many things. On one hand, careful, systematic analysis of different factors influencing behaviour yields understanding of how, for example, species respond to specific stimuli or have become adapted to their environments. This in turn generates predictability, which is important in understanding species survival, or how animals might be managed. On the other hand, it loses individual variations, which easily disappear in scientific generalizations. As Crist notes:

In a discourse that is fundamentally statistical in character, individual variability, as such, is minimally significant; it does not appear within the descriptive apparatus. [. . .] Indeed, statistical operations precisely sink individual variation and cancel out idiosyncratic expressions, for the purposes of discerning and mapping out general patterns and trends. (1999: 147)

Tinbergen, like Darwin, brought individuals into his popular writings, but it was the scientific style, stressing objectivity and statistical verification, which predominated. Animal behaviour came to focus on generic animals, which then stood in for species.

Reliance on quantification and the prevalence of generic animals as prototypes of the species remain predominant themes of behavioural science, although, over time, many differences between scientists have diminished (Bateson 2003). But, in emphasizing objectivity, the individual animal’s subjectivity and lifeworld tend to become obscured. So, while wilderness is emphasized in the sense of the adaptations of species to their ways of life, there is less understanding of what it is like to experience that life.

Yet even though biologists study behaviour of species ‘in nature’, they face the problem that the very presence of human observers (or remote tracking equipment) can alter the behaviour they want to record. However much scientists seek distancing, animals may not necessarily be fooled by their tactics, such as the use of a hide (Martin and Bateson 1986). Mere human presence can influence behaviour in a wide range of species, from lab rats (Dewsbury 1995) to agricultural or zoo animals (Hemsworth 2003; Hosey 2005) to wild damselfly larvae (Baker and McGuffin 2007) to free-living or captive primates (Jack et al. 2008; Iredale et al. 2010). Animals, in turn, may perceive humans in many ways: as predator or as prey; as socially insignificant; as conspecific; or as symbiotic, as would be the case if the researcher were also a caretaker (Estep and Hett 1995).6

Even if scientists use automated equipment in the lab (such as devices to measure animals’ activity over 24 hours), effects of handling or human proximity still occur. Indeed, many animals do not easily habituate to human presence and continue to alter their behaviour when people are about or in a hide (Almeida et al. 2006). Even techniques for remote recording like radiotracking devices can disturb behaviour, with possible implications for long-term welfare (Peniche et al. 2011).

To be sure, some species studied in the field do indeed seem to become habituated, so that human observers appear to make no difference (Crofoot et al. 2010). Field biologists often use habituation, or familiarizing the animals with the presence of observers, in order to minimize disturbance. The idea is that by repeated, gradual exposure animals will become so used to observers that they simply ignore them, and scientists can maintain the fiction of objective distance.
This is not easy, however, and is time-consuming. For example, meerkat colonies have long been studied, particularly in investigations of cooperative breeding, and of social/cultural learning (Thornton and Clutton-Brock 2011). Initial familiarization took over a year: after that, any pups born into habituated groups were themselves habituated, suggesting that getting used to people is socially learned. That is, the high level of social integration characterizing meerkat colonies is precisely the means whereby scientists can get the animals to stay put rather than flee at the sight of humans.

Indeed, so familiar have people become to meerkats that they do indeed seem to ignore humans completely, apparently carrying on their business regardless. At other times, they do not ignore humans but rather consider them as part of their world: researchers have famously had to contend with animals sitting on their heads or equipment, producing an iconic image made familiar through televised series about meerkat behaviour.

Such acts clearly muddy the waters between wild and tame; yet, as Candea points out in his study of scientists studying meerkats, researchers resist calling these animals domesticated, insisting on referring to them as habituated — a narrative which presumes a distancing, a non-engagement of researchers with animals. Habituation supposedly allows researchers to blend into the surroundings, to maintain the story that these are truly wild animals. Candea notes, too, how interactions with meerkats during routine weighings are not recorded: they cannot form part of the dataset, as they do not fit the naturalist frame of the study (2010: 250).

‘Wildness’, then, is carefully conserved in ethological narratives, even when talking about animals who know perfectly well that humans lurk around. ‘Wild’ animals form the subject matter of biological research, and research is often conducted and written about in ways that reinforce this narrative. Now I do not want to suggest that biologists should abandon studying behaviour just because of human influence. On the contrary, they need to be aware of how they might affect animals, and how animals might affect their knowledge. What I want to emphasize here is that ‘wild’, as characteristic behaviour of a species being observed by people, is not necessarily straightforward. The baseline of ‘animals in the wild’ not only pervades biological thinking, but also remains a largely unchallenged category. Moreover, our understanding of nature is produced by the way we do the studies of and write about animals. Human observers cannot necessarily assume that the animals are simply doing their own thing, unaware of the act of being observed, and unable to influence the outcome.

**Domesticating in the service of science**

Under precisely controlled laboratory conditions, an animal does as he [sic] damn well pleases.

The ‘First Harvard Law of Animal Behavior’  

My second theme removes us from wild nature and toward lab animals. While animals representing the wild also enter the laboratory, they have become domesticated in the process, both in the way they respond to handling and in their representation. Lab animals’ behaviour may be controlled, it may be studied for its own sake or perhaps as a model for human behaviour or mental states. Or, it may be something which otherwise could interrupt experiments, as the quotation above indicates – an unwanted intrusion in which the animal asserts something of his or her wild nature.

While some animals are kept in labs for purposes of studying something about that species, many are not, but are used instead as surrogates for illnesses of another species – almost always ourselves. Most of them are purpose-bred, and the majority are rodents. Lab animals are not,
of course, ‘in nature’; on the contrary, nature is largely excluded from laboratory spaces. In that sense, lab animals encapsulate a specific form of domestication. These are animals whose behaviour, through generations of artificial selection, renders them relatively domesticated, even ‘tame’. This, then, is far removed from studying the wild: by implication, tamed animals make more suitable surrogates for us.

Indeed, lab animals are usually produced, through breeding or genetic manipulation, to meet specific experimental needs. ‘Nature’ is transformed in labs. Both apparatuses and animals in experimental labs are preconstructed, purpose-bred, removed from their context; raw materials are carefully selected and ‘nature’ is largely excluded (Knorr-Cetina 1983: 119). Institutions such as labs and zoos endeavour to keep wild nature out. This means not only keeping out any diseases that wild counterparts might bring in, but also preventing larger animals from entering. Labs may be full of mice, but woe betide the mouse who foolishly tries to enter from outside: her fate will be poison or a trap. If animals escape, they must be re-caught and replaced in cages. If the animal was one kept apart from potential infection, behind barriers, then escape will mean sudden exposure to pathogens: whether by this means, or by human hand, the outcome for the animal almost certainly means death.

There are many reasons why lab escapees might not be left to wander about. Both the animals themselves, and the people in the lab, are potentially at risk, whether of transmitting modified genes or disease, or of more direct injury. These are animals out of place. Stock animals may be returned to their cages, but if they had already entered experimental protocols, then this may not be possible. Just as animals who up and die by themselves should not be counted as part of an experiment (see Birke and Smith 1995), so neither do animals who have upped and made their way toward the wild life – after all, the scientists will not know what has happened to them in the meantime. Tameness and wildness are contested territories in laboratory spaces.

Historically, to become lab animals entailed a gradual transformation. A century ago, while some animals were bred for purposes of experiments, others were simply taken from their usual habitat – even, in one case, rats from local rubbish tips (Foster 1980). Increasingly, however, controlled breeding ensured specific traits and produced ‘standardized models’ of many species, especially rodents (Rader 2004). Animals long seen as vermin, carriers of disease, were thus turned into symbolic saviours. Domestication here is two-pronged: first, animals themselves are manipulated genetically to produce desired characteristics; then, they are symbolically ‘de-wilded’, as they move from sewer to salvation.

At the same time, they remain animals, who may or may not cooperate with experimenters. In doing experiments, scientists must transform animals, both metaphorically and literally, from behaving beings to becoming data. Lynch (1985), in a study of neuroscientists using rats, noted contrasting everyday ideas of ‘naturalistic animals’ and how the scientists spoke about the ‘analytic animal’ – that is, after it became data, after its brain has been sectioned, sliced, and turned into material on a slide. When they said, ‘that was a good animal’, they meant good results from a well-prepared specimen. This is quite different from our common sense observation of other species, he argues. Referring to ‘good animals’ was not likely to mean the scientists’ recognition of individuals’ particular qualities in life. On the contrary, what was left of them was seen as fitting certain standards – whether of presenting brain tissue or because the slide fitted accepted hypotheses about brain anatomy. However good the slide preparation, if the material did not fit the hypothesis, then I suspect the scientist would have been less keen on praising the animal from whom it came.

In lab studies, the drive toward quantification is clear. Observing laboratory practices, sociologist of science Bruno Latour wryly noted that these have to do with ‘the transformation
of rats and chemicals into paper’ (Latour 1990: 39). Scientists, he argues, are obsessed with graphs and diagrams, but seldom see the animal who supplied the data: ‘Bleeding and screaming rats are quickly dispatched. What is extracted from them is a tiny set of figures’, he suggests (39).

The distancing described here is also evident in the way experimental reports are usually written; consistent use of the passive voice and reduction to numbers ensure that human involvement appears minimal. Even if animals had previously had names, they seem so often to appear as numbers in written reports. Ironically, this happens even if the animal in question in lab studies is a chimpanzee, who typically might be named by caretakers, a detail omitted in subsequent papers (Wieder 1980). The ‘domestication’ of nature in labs thus not only entails selective breeding, but also taming nature’s recalcitrance through carefully managed experimental practices and writing techniques. It involves, too, denying the animality of animals, something brought forcefully home to me by a lab technician who informed me that the scientists she worked with preferred opaque cages so they could not see the animals watching them.

Yet animal recalcitrance returns: animals may indeed watch us or be seen as ‘doing what they damn well please’ however much we try to control variables. Many factors, from different housing conditions, strains of animal, labs, or human handlers, can all affect – and change – experimental outcomes (Birke et al. 2007: 45; Balcombe 2006). Indeed, such effects can alter outcomes in, say, physiology or toxicology, and with implications for the interpretation of results in relation to human medicine (Sherwin 2004). This does not necessarily invalidate findings – which may still be extrapolable to other situations – but it does mean that interpreting data drawn from controlled lab studies requires caution.

Certainly, making inferences about animal behaviour on the basis of lab studies can be tricky. As Coetzee recognized in the quotation at the start of this chapter, there are profound problems in concluding that particular species are imbecilic if animals fail to work out our anthropocentric questions. Testing intelligence in laboratory conditions has a long history, though it tells you only that the animals did or did not work out that particular task in that particular setting. But such impoverished settings do not approximate how animals live their lives if they are not in captivity (Rogers 1997; Segerdahl 2012) and probably tell you more about the people setting the task than they do about the animals doing them.

Controlled testing conditions can thus constrain how scientists interpret animals’ behaviour or abilities. Another example comes from rats in lab-based studies of mammalian sexual responses. While ‘rat sex’ can be investigated in the wild, it is also part of studies into, say, effects of hormones on developmental sequences of behaviour. For many years, scientists studying rat sexual activities focused on lordosis – the posture permitting coitus – while failing to see the active role females play in solicitating sexual activity. This oversight, suggested McClintock and Adler (1978), resulted from using tiny testing cages, which severely limited females’ reactions. In such restricted spaces, these animals certainly cannot do what they please.

Experimental laboratory science thus involves rigorous policing of the boundaries of ‘wild’. ‘Wild’ is excluded from lab practices through separation of animals designated as part of lab work; through specialized breeding; through converting animals into data; through controlling variables and writing styles. Meanwhile, strenuous efforts are made to ensure that potentially recalcitrant animals are domesticated and constrained. And these animals must become habituated to human handling and presence, just like their wild counterparts in field studies.

If behaviour is under investigation in labs, it is not the free flow of living that makes behaviour meaningful to the animal, but a small fraction of specific sets of responses that can be
investigated experimentally. This tight control is in many ways, of course, critical to the success of science: by doing carefully structured experiments, scientists can tease out multiple factors which can influence how biology works, thus achieving some sort of predictability. What I want to emphasize here, however, is that it also facilitates a domestication – not only of the animal per se, but also of how we come to understand nature as ‘wild’.

Science in labs may partly be about studying ‘wild nature’, but it is also about domesticating her. There is an irony here, since those who handle animals regularly (animal technicians, more often than research scientists) know perfectly well that these are more than tools of the trade, they are ‘animals’ – they need feeding and cleaning, and they bite. In Tiptree’s 1978 short story about a psychologist having qualms about what his lab did to rats, the protagonist muses about their role in research, concluding that emotionalism in rodents is two things: defecation and biting psychologists. His qualms, in short, come from seeing the rats as shifting back from domesticated data to ‘wild’, to being animals, who sometimes do as they ‘damn well please’. In this guise, they both represent prototypical wild species, or stand as exemplars of (say) mammalian physiology, and also represent slippage away from wildness.

Following ferality

I think I could turn and live with animals, they are so placid and self-contain’d;
I stand and look at them long and long.

Walt Whitman, ‘Song of Myself’

How feral animals behave is less commonly investigated, not least because of ethology’s focus on the wild; yet these populations are potentially interesting, as they can tell us much about adaptability, in that transition between ‘tame’ and ‘wild’. Even when they are studied, however, the framework remains with wild species – how they use territory, for example – rather than how they engage with humans or with our social spaces. For that, we need analysis from other disciplines, such as geography.

What behaviour science does less often is to study ferality-in-the-making. This particular process of transition asks many questions of how a species adapts to different situations. Here, I will consider one particular example studying the transition from domesticated to wild: J.B. Calhoun’s observations in the mid-twentieth century of colonies of rodents moved into open spaces. I use it here because it follows into the wild some of the animals we have met in the lab, tracing their behavioural changes, and because, like many lab studies, it was seen as providing parallels to human behaviour.

This was not an investigation of existing communities, and the animals were arguably not truly feral, in the sense that they occupied large spaces constructed by scientists to produce what Calhoun described as ‘rat or mouse utopias’ (Ramsden and Adams 2009). But it was research concerned with following what happened once animals moved from relative domestication (many had been living in labs) to greater freedom; in that sense, it was a study of ferality-in-the-making. And what it showed was animals very far from Whitman’s bucolic image.

As populations grew, these utopias quickly descended into hell, according to Calhoun’s colleagues. Animals lost social cohesion, even after population numbers restabilized: they seemed no longer to behave like rats or mice (Ramsden and Adams 2009). Calhoun had begun his career studying free-living rat populations as part of a project of rodent control, but then became interested in questions of crowding: these studies became highly influential, as his rodent colonies provided ample illustration of social pathology and were readily extrapolated to commentaries about human urban existence.
Part of the consequent backlash against the popularization of this work was the suggestion that, unlike rats, humans could tolerate greater proximity as a consequence of culture. Rodents, that is, could go wild in the sense of turning savage, but humans could transcend that kind of ferality. ‘Going feral’ thus seems to be something implying breakdown in human behaviour, a lapse into animality that challenges our humanity. Referring to anthropological work that, Calhoun thought, implied social breakdown with increasing population density, he said, ‘I have put mice to the same test and they failed to remain mice’ (quoted in Ramsden and Adams 2009: 30).

Yet other, more recent studies of lab rats allowed to become feral have come to different conclusions, reporting that they started to adapt to their new environment within days. Reporting on The Laboratory Rat: A Natural History, a film based on one such experiment, Peplow (2004) notes that while animal behaviourists might not be surprised, many biomedical researchers are shocked that animals inbred for many generations are capable of fending for themselves. The scientist who produced the film, Manuel Berdoy, commented, ‘This shows that while we can take the animal from the wild, we have not taken the wild out of the animal’ (quoted in Peplow 2004). 11

‘Wild’, then, is something held to be inherent in a species, a potential which they can quickly recover if, that is, they are not under stress. The implication here is that, despite generations of lab breeding, this ‘wild’ resurrects itself, and ‘tamed’ animals revert to states unmediated by human intervention. The rats in Berdoy’s film are, moreover, framed in such a way that they fascinate; we observe them being social, making burrows, eating, and raising families. These are not the rats of the sewer, nor are they the rats of Calhoun’s dystopia. They are, on the contrary, more reminiscent of Whitman’s ‘placid, self-contained’ animals, in nature. Ferality can be constructed in many ways.

Most ethological thinking, I have argued, relies on concepts of the ‘animal in the wild’, representative of the species. Even when long-domesticated species are studied, their behaviour is often referenced back to putative wild ancestors. While similar discourses enter the laboratory, the use of animals as models in biomedicine entails particular forms of domestication, a taming of the animal through lab practices, and ways of asking animals questions. These ‘tools of the trade’, however, can occasionally revert to wildness/ferality, either through escape into the pipework of the lab building or when placed experimentally outside. How their ‘wildness’ is then interpreted, however, depends partly on how they adapt to their new situation, and partly on human framing of what the animals do or represent.

Thinking about animals ‘in the wild’ lies at the heart of biological practice, part of our belief in the centrality of evolution and natural selection, so that ‘wild’ is in effect an unremarked category. While I have framed this chapter with reference to these categories of wild, the categories nevertheless remain slippery, as Calhoun’s rat colonies indicate. Wild animals become domesticated through our scientific narratives, while domesticated animals in labs and homes can slip back into, or demonstrate flashes of, wildness.

Putting oneself in the animal’s place

A person who studies behaviour and never treats the animal as though it were human is liable to miss some of the richness and complexity of what it does. Many experienced ethologists have found how much they are helped if they put themselves in the animal’s place and consider how they would deal with the situation.

P. Bateson, ‘Ethology’
As an undergraduate, I had long wrestled with my conscience about studying biology. Fascinating though physiology was, I was extremely uncomfortable doing anything invasive with animals. Yet fascination for them won out, and I turned to ethology, where at least I could study whole animals, without having to dismember them. Nonetheless, you were supposed to keep a distance: emotionally and epistemologically, but also materially, by keeping as hidden from the animals as much as possible. It was, moreover, a training that prioritized testing hypotheses, producing graphs, and making generalizations about the species, as well as an abhorrence of ‘anthropomorphism’.

Paradoxically, most of the ethologists I knew liked animals and had long-standing interests in natural history; indeed, my passion for animals and wanting to understand why they do what they do is precisely what drew me to ethology in the first place. Colleagues would, moreover, often tell anecdotes about specific individuals among the species they studied. Anthropomorphism seemed all right in the coffee room, even if banned from the written paper.

To some extent, that tension – between personal experience and pressure to conform to scientific standards – persists. In ethology, detailed descriptions of individuals (and reference to mental states) can sit alongside narratives driven by hypotheses and distancing, often in the same textbooks or journals. In primatology particularly, descriptive modes of writing are common; field studies of great ape societies are clear examples. Similarly, while there is a long history of abjuring anthropomorphism, biologists working in ethology are also sometimes willing to exploit it, as Bateson explains in the quotation above.

What are the implications of these shifts in scientific thinking about behaviour for wild/feral/tame (domesticated) distinctions? Reading Charles Darwin, I am struck by how both ‘domesticated’ and ‘wild’ animals interweave through the text. In, for example, his work on the expression of emotions, tame cats feature as much as wild canids (1872). Yet as ethology gained strength, the emphasis on generic cases grew. As these had to stand to illustrate specific species, so the wild animal became more prominent; only the wild animal could truly exemplify species, while domesticated animals were often seen as departures from these idealized norms. The result was that, for many years, the study of domesticated species was relegated to the sidelines.

Seeing animals as exemplars of species was further underlined by ethology’s preference for explanations in terms of function and evolution, especially in the surge of interest in behavioural ecology. Once animals come to stand in for species, then individuality drops away. ‘Wild’ thus separates out from ‘tame’ in the very provenance of biological practice and thought; the ‘wild’ remains the gold standard, against which the behaviour of ‘domesticated’ or ‘feral’ animals can be compared.

Ethologists – although undoubtedly aware of how the presence of human observers has an impact on wild animals – seem almost to be looking out at pristine nature, in which human presence is played down. In this sense, behavioural science can be said to be ‘performing the wild’ (Whatmore 2002). The very act of making observations – and being observed in return – produces a different situation, a relationship at a distance, rather than seeing wildness through a neutral lens. It is, in that sense, its own kind of domestication; the relationship with the animal, however remote, transforms wildness at the point of observation.

Scientists, furthermore, must draw not only on their much-valued detachment but also on personal experiences. As Candea noted in his study of behavioural research into meerkat behaviour, scientists had to

draw on both distant and personal modes of knowledge while remembering to keep them separate. [. . .] [T]he data that would later feed into analyses of meerkat behavioral
biology began its life in the fine-grained knowledge of particular animals by particular people.

(2010: 250)

Only thus can they begin to ‘put themselves in the animal’s place’.

Despite my immersion in the distancing and domestication of nature in labs, I love ethology. To be sure, there are some who insist on being ‘objective scientists’ (whatever that means), but there are many ethologists just as willing to meet animal others as selves, with minds of their own. There is still, in some ethological writing, a sense of wonder about the natural world, a willingness to put oneself in the animal’s place and ponder who s/he is. Some are not afraid, either, to name animals they study or swap anecdotes about them. Marc Bekoff underlined the importance of telling stories in understanding animals, noting that

Anecdotes are central to the study of behavior, as they are to much of science. As we accumulate more and more stories about behavior, we develop a solid data base that can be used to stimulate further empirical research, and yes, additional stories. The plural of anecdote is data.

(2002: 47)

It is these stories that often come closest to how many people experience relationships with other animals, be that the dog by the fireside or the wild animals watching us watching them. They tap into an emotional connection that cuts across wildness or tameness: these are animals in all their wonderful individuality. They are both representatives of species and simultaneously just themselves.

At the moment, scientific study has not prioritized interspecies relationships. Ethologists mostly study animals ‘out there’, rather than how they bond (or not) with us. Insofar as they study relationships, that usually means relationships within a species, within social groups. But observing humans are, in many ways, part of the milieu in which those observed animals find themselves, and we need to understand better how that interspecies sociality is experienced by them as well as by us. We need, I would argue, to figure out better ways in which relationships between humans and nonhumans, as processes of communication and interconnecting, might be studied.

Emotional engagements of human and nonhuman are too often written out of science, even while they may be part and parcel of how lab or field studies are run. What happens to the interpretation of results if we acknowledge the animality of lab animals and their responses to us? If we pause to wonder who they are? What happens to our understandings of wild, of species, of specific animals, when we focus on connections between us, the interspecies intermingle (Haraway 2008)? What happens to concepts of wild if we were to think about nonhumans as active participants in the processes of knowledge production? The dispassionate gaze of the scientist, calmly observing ‘wild’ nature, begins to make less sense. Rather, we would have to locate ourselves alongside the creatures we study, to understand what connects us; if we focus on relationships, rather than on us observing them, then ‘understanding behaviour’ takes on a different hue. And, for all that ethology is firmly rooted in scientific traditions that distance us, I believe it is ethologists who are among those most willing to break the chains.

Acknowledgements

This chapter is dedicated to the memory of Quin, my beloved friend, and horse of my heart, who died aged 21 in February 2012. His ‘wildness’ in competition arenas was legendary, but
we still frequently won, as long as he got his way. He set forth his views of humans and their failures to understand animal intelligence (via an amanuensis, namely me) in Birke (2004). I am also very grateful to Jo Hockenhull for helpful suggestions and comments on an earlier draft. And last, but not least, I want to thank, profoundly, the many and varied nonhumans who have watched me watching them over the years. I have never doubted their intelligence.

Notes

1 In particular, domestication has involved modifications of the hypothalamo-pituitary-adrenal axis (HPA) involved in ‘flight or fight’ responses. That is, stress responses are damped down in tamed animals.

2 Garry Marvin notes that these ambiguous animals become closer to wild at the point of their deaths (2006: 18).

3 Wilson (2011) pointed out that there were several women active in ethology in its early years. As is so usual, their work has largely been overlooked, and the story of ‘founding fathers’ as sole progenitors persists in ethology textbooks.

4 The contrast between descriptive, naturalistic accounts and experimental ones is discussed also by Vinciane Despret in this volume.

5 Abstraction is part of many scientific representations, such as diagrams of the human body (Birke 1999). While it permits clarity, it also decontextualizes, with implications for how we perceive the part of the body/animal or whatever.

6 These authors used a categorization earlier drawn up by Swiss zoologist Hediger (1964).

7 A common laboratory joke, this line is believed to have originated with Harvard biologist George Wald (Dubos 1968).

8 Here, I refer primarily to lab use of apes. Studies in the wild, such as Jane Goodall’s work with chimpanzees, typically refer to them as individuals.

9 Anne Fausto-Sterling (2000) also noted continuing use of circular arenas, to stop females backing into corners and making themselves unavailable.

10 Though, on feral cats, see Kerby and McDonald (1988); and Boitani et al. (1995) on feral dogs.

11 For further details of the project, see www.ratlife.org.

12 See discussion in Birke (1994).

13 The apocryphal story of Schrödinger’s cat seems relevant here.

14 Though see the essays in Davis and Balfour (1995).

References


