Chapter 10
Controlling Away the Phenomenon: Maze Research and the Nature of Learning

Evan Arnet

10.1 Introduction

Let me begin by clarifying two different senses, or perhaps scales, of control at play in this study. The first is familiar—experimental control, in which researchers use control measures of one kind or another within the confines of an experiment or short series of experiments. The second sense understands control as a historically extended process. From this perspective, control happens alongside enduring research programs. This second sense involves more than just introducing a control arm. An experimental context or system is successively scrutinized, thereby stabilizing or stripping away all the loose interfering parts of the world until all that remains is the object of interest. The state in which the experimental setup occurs is such that it can answer certain questions with authority, at least to the satisfaction of some inquirers, but this is always contestable—by new information (or the recovery of old), shifting norms of best practices, or a revised understanding of the components.

A historical perspective on control is unusual for two reasons. The first is that, with the partial exceptions of Jutta Schickore and Hans-Jörg Rheinberger, this is simply not how philosophers of science talk about control (Schickore 2019; Rheinberger 1997). That is, when we talk about control at all.[1] The second reason is that this sense of control pushes back against a classical Hackingesque account of the phenomena, and emphasizes not the creation of laboratory phenomena but rather the elimination of interfering nature (Hacking 1983, Ch. 13). What is important to

[1] Schickore reviews the surprisingly scant literature prior to this volume (Schickore 2019). See also Sullivan 2022; Guttinger 2019.

E. Arnet (✉)
Indiana University Bloomington, Bloomington, IN, USA
e-mail: earnet@indiana.edu

J. Schickore, W. R. Newman (eds.), *Elusive Phenomena, Unwieldy Things*,
Archimedes 71, https://doi.org/10.1007/978-3-031-52954-2_10

scientists working in these ways is precisely that the phenomenon is not "created," but rather that it has a fidelity to nature that can be achieved only in the structured context of the experiment. To his credit, Hacking recognizes this, for instance when he states regarding the Hall effect that, "nowhere outside the laboratory is there such a pure arrangement" (Hacking 1983, 226). The development of control during a research program as seen here involves two simultaneous stories: one articulates the control practices in the experimental context and why they are effective, and the other articulates the central object of inquiry itself. Control as a historical process is also deeply social, with different experimenters testing different background assumptions and loose ends and then combining them into a kind of virtual understanding of the experimental context.

The "phenomenon" I shall discuss here is animal learning, in particular that studied with maze research. No one doubted that animals could learn. The challenge was to isolate the process of animal learning in its pure form, to understand "learning" as such. The central tension in learning experiments is that what counts as the phenomenon, its "pure" form, and what counts as the interference or impurity, are contestable categories. And, so the methodological argument goes, in trying to control every way an animal might "cheat" at maze learning, scientists all but eliminated the phenomenon of animal learning entirely. Later investigators raised this criticism explicitly because of, not in spite of, the rigorous laboratory approaches of early-twentieth-century comparative psychologists.

10.2 Phenomena and Control as a Historical Process

Jutta Schickore recently brought forth "control" as an object of historical and philosophical interest after a period of comparative neglect (Schickore 2019). Drawing from the life sciences, she argues for a distinction between "probes" and "checks, Over the course of developing an experimental research program, scientists create a confounder repertoire of factors that may interfere with the relationship of interest, namely, that between the independent and dependent variables.[2] Schickore notes that identifying the confounders and actually controlling for them are separate tasks. Checks constitute the more iconic function of control as a comparison or contrast against which the variable of interest can stand out. Probes are the necessary ancillary investigations to find and manage possible sources of interference in an experiment. More generally, probing helps one to focus on the phenomenon of interest by identifying and clearing away confounders, and the check elucidates causation in single decisive instances.

[2] A confounder repertoire in my understanding is best viewed as something virtual—an abstraction or reconstruction from a tradition of experimental work—as opposed to an actual complete list of confounders that exists in written form or in the mind of any single experimenter. One of Schickore's normative recommendations is for scientists to make the confounder repertoire more explicit in submitted research.

Jacqueline Sullivan extends Schickore's account into animal learning research. Investigating the development of a touchscreen operant for rats beginning in the 1990s, Sullivan attends specifically to the "dynamics" of control (Sullivan 2021, 2022). As they design the new apparatus, researchers study how varying different components affects the behavioral apparatus as a first stage of "probing" control, in Schickore's terminology. They also build in established stock controls, such as limiting extraneous auditory stimuli, for confounds well known in the field. Once the new apparatus is launched it becomes a community project, with multiple researchers critiquing, investigating, and controlling for possible sources of experimental error. The probing function of control in Sullivan's analysis is an ongoing process—it is intended not for the specification of a single experimental setup, but rather for the improvement and specification of a scientific instrument, namely, rodent touchscreen operant chambers.[3] In the shadow of its development exists a parallel lineage of control probes identifying and addressing possible confounds. The system itself is an object of investigation as scientists experimentally explore whether rodents have preferences for parts of the touchscreen, certain training regimes, or rewards.[4]

Control as understood by Schickore and Sullivan has both technical and investigatory components. As a technical practice, control involves the concrete implementation of tools, practices, and procedures to mitigate the effect of variables other than the one of interest. Alongside emerges a reflection on control practices and on methodology generally that helps to guide experiments (Schickore 2017). The technical side of control can be understood as the physical mastery of a space or system. Part of the rodent touchscreen operant chamber's appeal in the first place is that it enables a standardized setup in which a similarly standardized battery of tests can be administered relatively free from the physical involvement of the experimenter (Bussey et al. 2008; Horner et al. 2013; Dumont et al. 2021). As an investigatory practice, scientists probe the system under investigation to identify what confounds they must worry about. The notion of a "system" must be understood broadly, for, as Sullivan points out, the process is both social and collaborative, with different members of the community exploring different aspects of the rodent touch screen operant chamber and combining their findings into an overall assessment (Sullivan 2022).

This account motivates an understanding of control as a historically extended process. While a "check" or a comparison may exist in the context of a single experiment, the development of the confounder repertoire and the physical mastery of the space or system take time. Moreover, control is often an iterative process in which scientists identify and then correct for new sources of error, thus forming lineages

---

[3] For related research not on control but on the broader notion of an experimental system, see Rheinberger 1997. Like Sullivan on the operant chamber, Rheinberger emphasizes that experimental systems are historical, constituting not simply a snapshot setup but a lineage of experiments and relatively faithfully reproduced material assemblages.

[4] Similar points have been made regarding instruments being objects of reflective investigation, especially in microscopy (Schickore 2001, 2007; Rasmussen 1993, 1999; Baird 2004; Dörries 1994).

of investigation with progressive control practices. Consider, for example, the controversial Donohue–Levitt hypothesis, which says that a reduction in crime follows the legalization of abortion, on the assumption that reproductive autonomy allows parents more control over the (possibly criminogenic) situations in which their children grow up (Donohue III and Levitt 2001). The hypothesis has seen critiques pointing to possible confounds, such as changes in cocaine usage, and in response has marshaled additional data and implemented more careful statistical controls. In turn, others have responded with rejoinders (Donohue and Levitt 2004, 2020; Joyce 2004; Shoesmith 2017; Moody and Marvell 2010). To be clear, not everything done in pursuit of the Donohue–Levitt hypothesis, like looking for evidence of the effect in different countries, is best thought of as control practices. Nonetheless, a historical lineage of investigation emerges even in non-experimental cases like this, and the probing of confounds becomes an important part of these lineages.

One way to envision control as a historical process is as an expanding circle, consisting of one relatively narrowly understood central object of investigation, like a hypothesis, an instrument, or a topic such as "touchscreen learning." From it branch lines of inquiry that serve to identify and control for confounds and sources of irregularity. These branches are auxiliary investigations or modifications to the experimental setup that serve to eliminate alternative explanations or partial explanations of an observation. The research work is simultaneously creative labor, in devising possible confounds and control strategies to address them, and labor in a more straightforward sense, in doing the work to tie all these loose ends. The advantage provided by a community of investigators, both in terms of diversity of ideas and the labor of investigation, is clear (Sullivan 2022; Longino 1990, 2022).

What do control practices aim at ultimately? In the cases of experimental control where a comparison is made, or control as "check" in Schickore's terminology, the intent is often to crisply illustrate a single causal variable. From a strictly logical perspective, if it were truly certain that two experimental setups differed in only a single variable, then regardless of how messy and cluttered the experimental setups might be, they would provide clear evidence of causation. Dealing with the confounds, however, helps manage the tangle of causes surrounding an observation or an object of interest. For example, to support the Donohue–Levitt hypothesis, confounds for the observed correlation between the legalization of abortion and a reduction in crime must be cleared away. In Sullivan's example, the other factors that make a difference in the touchscreen learning experimental setup are identified, measured, and eliminated or corrected for to provide a clearer sense of the touchscreen operant chamber's utility. As another example, in the case we are about to discuss, the aim was to isolate animal learning, or at least a crucial aspect of animal learning.

The overall picture emerging here is one of elaborate simplification or isolation of the object of inquiry. In a now-classic work, Ian Hacking set out an account of "phenomena" as "something public, regular, possibly law-like, but perhaps exceptional" (Hacking 1983, 222; 1988, 1991). Notably, phenomena are not waiting out there in nature to be discovered or observed, but are rare and generally occur only in the contrived setup of the laboratory. As Hacking puts it, "The truths of science have long ceased to correspond to the world, whatever that might mean; they answer to the phenomena created in the laboratory" (Hacking 1991, 239). Hacking in turn draws from the French philosopher and historian of science Gaston Bachelard, who underscored the constructive and technological orientation of science particularly via his concept of *phenomenotechnique*, albeit in a way quite different from late-twentieth-century constructivist accounts (Bachelard 2006; Rheinberger 2005; Castelão-Lawless 1995). Inspired by contemporary work in physics, Bachelard noted that scientists are trying to realize their theoretical reality. Their guiding theory postulates certain entities and mathematical regularities of behavior, and the challenge is how, through technical mastery, to create a situation in which that theory of reality could be instantiated, observed, tested, and manipulated (Bachelard 2006). For neither Bachelard nor Hacking are phenomena "made up" in a pejorative sense; rather, they are the carefully constituted objects of scientific inquiry. Nancy Cartwright, in a similarly classic work, speaks of nomological machines. These are "fixed (enough) arrangements of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behavior that we describe in our scientific laws" (Cartwright 1997, 66; 1999). These philosophers, by emphasizing that science is in an important way the study of complicated systems and phenomena built by scientists themselves, allowed this constructive process to become part of a larger understanding of science. They thus pushed back against a naïve understanding of brute scientific observation.

Their accounts all occurred in the context of contemporary debates and my intent here is not to relitigate those issues.[5] Moreover, these philosophers are simply correct in observing that experiments are constructed systems, and emphasizing this point has been enormously fruitful over the past several decades. Focusing on control, however, reminds us why scientists build such elaborate setups, for at root it is the world that is heterogenous and complicated and that must therefore be disciplined by contrivance to be rendered clear and predictable. Controls aim not to constitute phenomena, but to expose them, to get them alone. The hope, if not necessarily the reality, is to pull an unbroken thread from the warp and woof of the world into the confines of the laboratory.[6] This tendency appears in the early to mid-twentieth-century maze research, which sought through careful instrument and experiment design to isolate animal learning as such.

---

[5] For discussion and contextualization of the philosophy of the experiment, see Simons and Vagelli 2021.

[6] This is meant as a description of the actors, but rhetoric aside, even from an analytic perspective this need not be a realist assertion. An inquirer could place certain boundaries around a "thread" of the world (individuation) such that something like this same thread stably exists among the buzzing causal confusion of the world as well as among other experimental investigations. Yet it could still be the case both that this individuation is fundamentally arbitrary and that the thread as such is not characterized in any accurate sense beyond some of its behavior. For an elaboration of some of these ideas, see Arabatzis 2006.

However, the conceptual understanding of the object of inquiry and the control practices needed to study it are always interrelated. Along with shifting accounts of animal learning came new perspectives on early-twentieth-century animal learning experimentation, including the concern that they had controlled away the phenomenon entirely. This point illustrates a complicating factor in a historical process understanding of control, as the clarity provided by the expanding circle of research—extending out from a central hypothesis or phenomenon—can be ruined by conceptual revision of just what that central object of inquiry is. Control practices can then be recast as interference.[7]

10.3 Isolating Animal Learning in the Maze

Maze research has been a dominant approach in investigating the behavioral and cognitive features of organisms, especially in the early twentieth century. It occurred in lineages of research with maze designs being developed, copied, adapted, and modified in response to new critiques or to support new research programs.[8] Seminal figures of early American psychology, including John Watson, Clark Hull, and Edward Tolman, were all maze researchers. Nonetheless, their fascination was not with the maze as such, but rather with the maze as an instrument to unlock the secrets of animal learning. As Hoffmann discusses in this volume, organisms represent a particular challenge for control practices, for the animal enters the experimental setup with its own disposition and agency. The maze was a central way to structure the animal's behavior.

Edward Tolman concluded his 1937 American Psychology Association presidential address this way: "Let me close, now, with a final confession of faith. I believe that everything important in psychology (except perhaps such matters as the building up of a super-ego, that is, everything save such matters as involve society and words) can be investigated in essence through the continued experimental and theoretical analysis of the determiners of rat behavior at a choice-point in a maze. Herein I believe I agree with Hull and also with Professor Thorndike" (Tolman 1938, 34). Notably, Tolman was no radical, and was a chief representative of the more cognitive wing of early-twentieth-century behaviorism (Tolman 1932). My interest here is the program of control centered on maze research, which took its object of interest to be animal learning and which sought to corner the pure phenomenon in a maze alongside the experimental animal.

We begin across the Atlantic in late nineteenth-century Britain. Conwy Lloyd Morgan was a psychologist and philosopher whose 1894 text, *Introduction to*

---

[7] For an illustrative example along these lines dealing not with control *per se* but rather with discovery, see Arabatzis and Gavroglu 2016.

[8] For an exploration of how artifacts can be analyzed from an evolutionary perspective, despite the fact that they do not reproduce the way organisms do, see Wimsatt and Griesemer 2007; Baird 2004. For a sampling of the incredible diversity of maze research, see Bimonte-Nelson 2015.

Comparative Psychology, arguably inaugurated Anglo-American comparative psychology as an experimental discipline (Boakes 1984; Wilson 2002; Arnet 2019a; Dewsbury 1984). Following Herbert Spencer, George Romanes, and others, Morgan had a general theory of learning, not necessarily in the sense that animal behavior was not impacted by the environment, but in the sense that there were a small number of core underlying learning faculties, corresponding to instinct, intelligence, and reason. These he took to be hierarchically arranged in accordance with a progressive theory of mental evolution (Clatterbuck 2016; Arnet 2019a). From this perspective, it is sensible to hypothesize that all non-human animals can learn in fundamentally the same way. Morgan is best known for Morgan's canon, an interpretive rule of comparative psychology encouraging investigators to default to psychological processes lower in the "psychological scale" for the inference of mind from behavior.[9] His canon was but the first of several conservative moves regarding the animal mind within early comparative psychology, and other researchers, such as physiological psychologist Jacques Loeb, were even more deflationary. Morgan was also known for his advocacy of trial-and-error learning approaches, which provided a powerful explanatory approach for animal behavior that had previously been explained via abstract reasoning (Morgan 1896). Morgan's drive for rigor, conservativism, and experimentalism helped set the stage for early American comparative psychology (Galef Jr. 1988).

As we approach the dawn of the twentieth century, three factors converge in animal psychology. The first is a generalized understanding of learning (Seligman 1970). The second is a new laboratory experimentalism (Capshew 1992). The third is a deflationary tendency toward the animal mind (Dewsbury 2000; Arnet 2019a). To be clear, though, comparative psychology was not monolithic. This convergence is exemplified in the figure of Edward Thorndike, whose 1898 dissertation was a landmark in experimental comparative psychology (Galef Jr. 1988, 1998; Jonçich 1968; Thorndike 1898). Thorndike used "puzzle boxes," which were cages that required the animal to engage in some behavior, like pulling a wire loop, in order to get out. He designed them such that they would not trigger on wild or instinctive behavior but, ostensibly, on some purer form of learning. Thorndike clarifies that his design goal was to "get the association process…free from the helping hand of instinct" (Thorndike 1898, 9). For this reason, "Especial care was taken not to have the widest openings between bars at all near the lever, or wire-loop, or what not, which governed the bolt on the door. For the animal instinctively attacks the large openings first" (Thorndike 1898, 9). Thorndike then graphed the learning over time. If the animal were reasoning, Thorndike assumed, then at some point it would have an "ah-ha" moment that would appear on the graphs as a precipitous drop in escape time. Seeing no such drop, Thorndike concluded that there is nothing going on in the animal beyond associative learning. Reflecting on his methodology, Thorndike wrote, "The general argument of the monograph is used in all sort of scientific work

[9] The literature on Morgan's canon is extensive. See Costall 1993; Allen-Hermanson 2005; Thomas 2001; Sober 1998; Fitzpatrick 2008; Radick 2000; Arnet 2019a.

and is simple enough. It says: 'If dogs and cats have such and such mental functions, they will do so and so in certain situation and will not do so and so; while, on the other hand the absence of the function in question will lead to the presence of certain things and the absence of certain other things'" (Thorndike 1899, 414–15). Thorndike's view is a generalized account of learning and its epistemic implications on full display—asking how a general capacity will be made manifest in a specific situation, without assuming that the situation will in any way change how learning works.

Thorndike posited just three general types of learning: trial-and-error, imitation, and learning by ideas (Thorndike 1901, 2). In research on monkeys he argued that while humans alone learn by ideas, learning by ideas is itself an elaboration and refinement of associative learning (Thorndike 1901). Ultimately, Thorndike was able to explain almost all learning in terms of the development of associations between stimuli and responses, with animals differing both in how quickly they were able to develop those associations and in how many associations they developed. Thorndike seems to have had a formative role in at least some maze research, with his puzzle boxes sharing striking similarity to the minimalist mazes Yerkes later used in his own work (Yerkes 1901; 1902; Yerkes and Huggins 1903).

The more traditional origin for maze research is at Clark University in Massachusetts (Miles 1930; Traetta 2020). There two graduate students, Linus Kline and Willard Small, were engaged in rat learning experiments based on the model of the English Hampton Court Hedge Maze—an idea partially inspired by their adviser, Edmund Sanford, who had a strong evolutionary orientation (Goodwin 1987). In Kline's "Suggestions toward a laboratory course in comparative psychology," where he outlined his structure for a laboratory class, Kline wrote, "A careful study of the instincts, dominant traits and habits of an animal as expressed in its free life—in brief its natural history—should precede as far as possible any experimental study. Procedure in the latter case, *i.e.* by the experimental method, must of necessity be largely controlled by the knowledge gained through the former, *i.e.* by the natural method" (Kline 1899, 399, emphasis in original). Far from Thorndike's attempt to control for instinctive behavior, a maze was chosen precisely because of its similarity to the warrens used by rats. Crucially, then, the maze as envisioned by Small and Kline tested rat behavior specifically, with a fidelity to their natural environments. They also tested different species of rats and investigated the differences between them, in an approach that by and large did not continue out of Small's early experiments.[10]

While others adopted their experimental setup, they did not necessarily take up their theoretical commitments—most notably, the up-and-coming behaviorists John Watson and Harvey Carr. One central difference is that Watson wanted to study rats on their first encounter with the maze, at the beginnings of an association process, whereas Small allowed the rats to freely explore the maze prior to investigation.

[10] But see Florence Richard, who tested the differences between white (standard laboratory rat) and black rats (likely the Fancy rat) (Richardson 1909).

The implicit critique, one stated years earlier by Morgan, is that if one is interested in the process of learning, then simply looking at an already formed behavior is inadequate (Morgan 1894). In the famous "kerplunk" experiments of Watson and Carr, rats learned to run a Hampton Court Maze and then were confronted with a shortened version. The unfortunate subjects ran into the wall of the maze with an audible "kerplunk" (Carr and Watson 1908; Watson 1907). The hypothesis was that the rat was associating a series of kinesthetic and motor movements with each other. The associated movements unroll automatically as the rat races through the maze, leading to collision when the environment changes. Sensory cues were, if involved at all, decidedly secondary. In parallel, Watson and Carr proposed a general theory of learning, where animals like rats learn primarily by the random physical exploration of space, after which they chain their movements together. This view contradicts Small's cognitively-oriented suggestions about memory and mental processes. It also explained individual variation in rat behavior, namely as expected variation in a random process. Watson and Carr's conclusion was further supported by blinding some of the rats, which began a long and somewhat unsettling trajectory of sensory deprivation experiments. Small himself had been impressed by the efficient learning of a blind rat, although his rat was naturally blind. As usual, though, history is complicated. Far from being a direct follower of Thorndike, the early Watson alludes to the same naturalistic rationale for the selection of the maze (Watson 1907, 3).[11]

Here I want to step back and attend to the two different control regimes that are beginning to emerge but that have not yet been fully articulated. The temptation is to retreat to familiar discourse in comparative psychology that wrestles with how natural or artificial experiments are. Critiques along these lines were made of Thorndike's work by influential psychologists such as Wesley Mills and Conwy Lloyd Morgan (Mills 1899; Morgan 1898). This temptation would be, I believe, a mistake. Small, Watson and Carr, and even Thorndike, are fixated on something that (they believe) is out there in world. What they disagree on is how that "thing" is conceptualized, and correspondingly what the control practices required to study it are. For Small, although he prioritizes motor senses in the rat and particularly deemphasizes vision, sensation is partly constitutive of rat learning (Small 1899). To know how a rat learns is to at least in part understand how it deploys its senses. Small was thus still very much concerned with experimental control and wrote in 1901 that "the aim in these experiments, as indicated above, was to make observations upon the free expression of the animal mental processes under as definitely controlled conditions as possible; and, at the same time, to minimize the inhibitive influence of restraint, confinement, and unfamiliar or unnatural circumstances" (Small 1901, 206).

Carr and Watson, motivated by successful maze learning in blind and anosmic rats, wanted to strip away the senses (sometimes literally) to get at the distilled form of learning underneath. Despite the popularity of their modified Hampton Court

[11] More generally, Watson's preference for a Pavlovian as opposed to a Thorndikean account of learning is discussed by Gewirtz (2001).

Maze, the Clark tradition of maze work manifested in Small's research on the rat never quite made it out of Clark. It was Watson and Carr who become influential, and their research program regarding the senses a rat needs to complete the maze took off. Importantly, their claim was not simply that a rat *can* complete a maze by merely chaining together proprioceptive cues—that is, cues related to the position of its body—but that this is in fact how rats *do* learn mazes, even with their other senses intact.[12] The proposal that the vision and olfaction of rats are essentially peripheral to their learning was treated as radical, even by generally sympathetic contemporaries (Vincent 1915d), but the work of Watson and Carr had an important impact on the core experimental logic and research questions of early maze investigations.

There are two overarching features to much of this early work. The first is that rat learning is conceptualized in terms of what specific problems the rat can complete in the context of the maze (e.g., whether it can complete a maze with certain design features or remember whether food is on the left or right in a series of trials; see Hunter and Nagge 1931; Carr 1917). A maze of one design or another is almost always the context for testing animal capacities. Whether or not the maze still resembled rodent warrens, which resemblance was a key property of the Hampton Court design, is no longer a matter of concern.[13] The second overarching feature is that the cues available to the rat are carefully restricted.

Even for those who hypothesized that rats were using more than proprioceptive cues, the research program was often one of disaggregating the role of different senses in rat learning. Central to this project was preventing the animal from using environmental cues outside the maze—and thus began an elaborate tradition of experimental control involving myriad modifications to animals, the maze, and surrounding environments. In her research, Stella Vincent removed whiskers to evaluate their impact on maze performance. She also painted the correct path white and erroneous paths black to test the involvement of vision, among other controls (Vincent 1915d; 1912).[14] Notably, Vincent explicitly saw the introduction of additional visual information into the maze design as a control for the hypothesis that rats are using kinesthetic information alone. Warner and Warden sought to standardize the maze design itself (Warden 1929; Warner and Warden 1927). Florence Richardson, who studied with Watson, performed similar sensory deprivation experiments, but also manipulated the complexity of the task by using "problem boxes" apart from the maze (Richardson 1909). Walter S. Hunter developed a temporal maze in which spatial clues were eliminated (Hunter 1920).

If we take a step back, we see first the fixation on animal learning, and more precisely Watson and Carr's kinesthetic hypothesis as the initial object of interest. We then see the expanding circle of experimentation, which manipulates

[12] This assumption was challenged from the outset; see, for example, Washburn 1908.

[13] Not everyone was so enamored. B. F. Skinner partially eschewed mazes in his later research, and influential critiques came from maze researchers such as Walter S. Hunter (Hunter 1926; Skinner 1938).

[14] Also see Vincent 1915a, 1915b, 1915c.

surrounding variables such as the maze design and available sensory cues. Alongside these more dramatic practices, a collection of stock control measures, such as starting experiments as the same time, keeping the experimental area free of wild rats and their odor trails, placing rewards in the same place, and maintaining stable food reward amounts and varieties between rats, develops as well.[15] This is the collection of a thousand and one little things that maintain the integrity of an experimental system.[16]

All of these control practices in combination serve to purify the *phenomenon* of animal learning.[17] That is, they free it from intervening variables. This is especially true for Watson and Carr, who were trying to establish that the association of random movements into fluid behavior was a vast part of what animal learning is. Control practices are relational, relying on a specific understanding of the target of investigation. In the tradition established by Watson and Carr, kinesthetic association as the nucleus of animal learning remains constant even if rats do occasionally incorporate other senses—and therefore their experimental program of stripping away the senses is a compelling one. If learning is not additive in this way, and instead learning is holistically different when more senses are involved, then the relationship between sensory deprivation (the control practice) and animal learning (the target of control) is also different. Just what is being exhibited by sensory-deprived rats (e.g., *one way* an animal can learn a maze, versus how an animal *always* learns a maze) is a site of conflict and fertile ground for introducing additional probes and checks.

10.4 Reconceptualizing Animal Learning

Nonetheless, for all the experimental rigor and complexity on display, this early-twentieth-century emphasis on proprioception and the concatenation of random movements in animal learning faded away. There is a larger story here, but I shall skip to the latter half of the century and focus on two researchers, David Olton and William Timberlake.[18] Both were trained at the University of Michigan, a leading location for neo-Hullian learning theory (Arnet 2019b; Shapiro n.d.).[19] Both focused on rat behavior. And both explicitly wrestled with the legacy of early maze research.

---

[15] Sullivan refers to this collection of standard control practices as "canonical" (Sullivan 2022, 1207).

[16] These matters also relate to whether an experimental system can effectively be maintained and reproduced between different uses, different researchers, and different labs. In this sense it is a detailed description that facilitates sameness of setup, but in the context of a single trial or experiment, sameness comes from inventorying and clearing away other possible intervening variables.

[17] For a similar point, see Steinle on the epistemic goal of exploratory experimentation (1997).

[18] For more general historical discussion of twentieth-century comparative psychology, see Burkhardt 2005; Braat et al. 2020; Dewsbury 1984; Watrin 2017; Watrin and Darwich 2012.

[19] Although Olton also focused on neurophysiology.

Their analysis recasts early experimental work as stripping away not simply intervening variables, but also crucial aspects of animal learning itself.

Olton is best known for his research on spatial memory and "place learning," here understood as the use of discriminative stimuli associated with a particular location (e.g., the way you could know that you were in a certain room by a familiar picture on the wall; see Shapiro n.d.). In a 1976 paper, Olton and Robert Samuelson introduced the radial arm maze (Olton and Samuelson 1976). The eight identical spokes of the maze radiating from a central point were intended to force the rat to use spatial cues from the surrounding environment to orient itself in the maze.[20] In their initial study, a food reward was placed on the end of each spoke, a rat placed in the center, and then the movement through the maze monitored. Rats were tested in how quickly they could get to each food reward without revisiting a spoke (which would not have new food). Orienting themselves with respect to the older maze research tradition, Olton and Samuelson wrote, "In spite of the ubiquitous nature of place learning, most experiments have treated place learning as a factor to be controlled and have chosen to assess rats' cognitive abilities by making place learning impossible" (Olton and Samuelson 1976, 97). The most dramatic example is the research of Walter S. Hunter mentioned above, who had rats memorize series of left and right alternations all done in a single box, thereby trying to eliminate spatial clues entirely. Olton and Samuelson instead made place learning the object of investigation in their study. But Olton was interested in more than just a change in focus. He contended that even Watson and Carr's original aspiration to understand animal learning was undermined by controlling away spatial cues as interference, when those cues were actually essential to understand how rats learn and navigate. In his historical work, he called out the maze explicitly:

> Structural characteristics of the apparatus [maze] suppress some kinds of behaviors and enhance others. Thus the maze itself influenced the types of behaviors and the types of theories that were developed from these observations. On the other hand, mazes reflected the theoretical biases of their users. Experimenters had a predilection to address certain types of issues, and the mazes were constructed with these issues in mind. (Olton 1979, 583)

The early maze tradition, especially as a reaction to Watson and Carr, had primarily been one of narrowing and isolating the capacities of the animal in order to see what functions remain when the animal is stripped of its senses and environmental cues. Researchers assumed that this practice did not distort the phenomenon; that is, they believed that the effect of additional sensory cues is at most additive to the underlying skeleton of learning. (In the extreme case of Watson and Carr's early research, the additional senses hardly did anything at all.) In addition, in terms of understanding animal capacities, the Watson and Carr-led maze research tradition sought to understand what an animal can still accomplish given certain restrictions, but it did not seek to know what the animal can do with full recourse to their faculties. As Olton put it later, "On the one hand, rats consistently demonstrate a preference for

[20] For example, if the maze were placed in a room in which there was a sink, a cabinet, and a poster visible on the walls from the maze, then these would be spatial cues.

solving discrimination tasks on the basis of spatial cues; on the other hand, experiments just as consistently prevent rats from exploiting this preference" (Olton 1978, 341). Olton and Samuelson argued that it is because of a history of certain kinds of control practices that scientists allegedly had a stunted vision of rat learning and cognition. In their 1976 paper, Olton and Samuelson conclude "the introduction of a spatial location paradigm may change [increase] our estimate of rat's cognitive capacities"(114). They also tied their work to ecological considerations, such as foraging behavior, based on the kinds of foraging strategies and food finding capacities rats exhibited more generally (Olton 1978). What is invariant between expected natural behavior and the laboratory is no longer something as abstract as the general structure of learning, but a specific foraging strategy (win-shift) that can be triggered in the lab.

Nonetheless, Olton's core interest remained spatial memory. The reconceptualization of animal learning as such is made far more explicit by another researcher, William Timberlake.

Timberlake began as a learning theorist but quickly took to more ethology-inflected work and integrated it into his behavior systems approach (Arnet 2019b). He was part of a larger movement looking to bring evolution and ecology to American laboratory psychology, and to studies of learning in particular. This movement also included researchers such as Sara Shettleworth, Robert Bolles, Martin Seligman, and Michael Domjan. I shall focus on two of Timberlake's criticisms for maze research.

The first was a recovery of the initial ecological focus of the maze, testing questions such as whether a rat would be motivated to run a maze even with no reward (Timberlake 1983b). In a reinforcement approach to animal learning, it was assumed that the incentive to learn an artificial system such as a maze was controlled for by not providing the animal food so that it hungered, and then allowing food alone to serve as an interventionist variable. Timberlake challenged this assumption by providing evidence that rats have intrinsic motivation to engage in edge-following behavior (i.e., to run mazes); this challenges the very idea that animal motivation was being controlled as assumed (Timberlake 2002).[21] Great experimentalists such as B. F. Skinner had, in Timberlake's estimation, partly put themselves on the map by getting the animal to cooperate in the artificial circumstances of the laboratory. This achievement occurs against the backdrop of the tongue-in-cheek Harvard Law of Animal Behavior: "Under carefully controlled experimental conditions, the animal will behave as it damn well pleases." Experimentally cooperative animal behavior, however, was not the product of luck; according to Timberlake, it was generated by the experimentalist's carefully fitting the setting and apparatuses to the physical and behavioral features of the organism.[22] There are reasons that pigeons were expected to peck keylights, that auditory stimuli were placed in ranges that did not

[21] Although early-twentieth-century researchers had explored this concept as part of broader studies of rat motivation, albeit without Timberlake's ecological tie-in. Timberlake himself references this literature (Timberlake 1983b, 170–71). See also Tolman 1930.

[22] For a peek behind the curtain, see Skinner 1956; Hoffman, this volume.

startle animals, and that passages were approximately rat-sized. From lever shape to maze design, instruments are tuned to organisms. While demonstrative of experimental skill, Timberlake contended that the epistemological effect of tuning was to smooth the ecological traits into the experimental backdrop where they can no longer be seen. He also argued that tuning facilitated the extremely general and abstract accounts of learning that characterized early-twentieth-century learning theory. By design, experiments and instruments were intended to suppress behavioral instincts and idiosyncrasies in order to produce something standardizable and easy to study. They sought a fit between organism and experiment and, once achieved, Timberlake thought that it was too easy to forget the species-specific design principles that made it possible. His own research interpreted laboratory practices as selectively eliciting aspects of the animals' evolved patterns of motivation and behavior (Timberlake 2002).

Second, along with other ecologically influenced psychologists, Timberlake wanted to reconceptualize animal learning and critiqued general process accounts in which learning is understood as abstract and domain-general. Timberlake in particular adopted a behavior systems approach, a development of the framework originally suggested by the famed ethologist Niko Tinbergen (Timberlake 1993, 1983a; Bowers 2017, 2018). On this view, organismal behavior was understood as a structured and hierarchical system of motivations and associated behaviors that had formed in the environment and evolutionary history of the organism. From the ecological perspective, learning is no longer domain-general, as traditionally characterized in behaviorist and learning-theoretic approaches. Instead, the sensitivity and richness of animal learning is, by evolution and development, rooted in the actual environment of the animal. These characteristics then make it into the lab. When undergoing reinforcement learning, animals readily "misbehave" by exhibiting species-typical behavior even when these behaviors are not reinforced (Breland and Breland 1961), and more ethology-inflected psychologists argue that it is easiest for animals to learn along the contours of their existing patterns of behavior. Timberlake writes, "Researchers…are studying niche-related behavior in specific species, whether they planned to or not" (Timberlake 2002, 372). Consequently, any attempt to fully abstract away from this in order to achieve a pure form of animal learning would contradict what constitutes learning, at least from the perspective of Timberlake and other like-minded psychologists.

Neither Olton nor Timberlake are, to be sure, opposed to control practices generally. Olton and Samuelson used Old Spice deodorant as an olfactory control in their early research, on the assumption that if rats were following odor trials—an alternative hypothesis to spatial learning—their performance would be worse if the apparatus were doused in Old Spice. Instead, Olton, Samuelson, and others are recasting the relationship between control practices and the phenomenon of interest. Olton contends that researchers cannot see the importance of spatial cues in learning because they have for years simply controlled it away as part of their experimental setup. He reintroduces spatial cues to experimental paradigms in order to illuminate a (hopefully) more complete animal learning. Similarly for Timberlake: mazes, as instruments composed of walls and edges, had been hiding the natural tendency of rats to follow trails and edges as opposed to using other patterns of search and locomotion. Stock control practices of food deprivation, to ensure that rats are hungry for food rewards, therefore masked the rats' intrinsic motivation (Timberlake 1983b, 2002; Hoffman et al. 1999; Timberlake et al. 1999).

Along with reconceptualizing learning to emphasize the evolved behaviors and tendencies the animal brought into the experimental situation, Timberlake sought to reopen long-closed features of experimental design. For example, building off the work of Pavlov, in mid-twentieth-century animal psychology, the predictive stimulus was generally seen to be a neutral stimulus with the animal's reaction to the stimulus being shaped purely by the experimental context. (A predictive stimulus simply indicates the coming of another stimulus, e.g., a light turning on to signal the arrival of food.) In experimental work, Timberlake found that when a live rat was used as the predictive stimulus, and then food was presented, it elicited social feeding behaviors from the subject rat (Timberlake and Grant 1975). His point was not that traditional stimuli such as key-lights are artificial, but rather that they are not neutral and may intersect with the dispositions of the research animal. Put differently, they may be confounds.

Unlike some ethology-inflected researchers, Timberlake was enthusiastic about the structure and control provided by laboratory investigations. He was, after all, trained in the American laboratory tradition of comparative psychology. Where he and researchers such as Watson and Carr would disagree is over the phenomena they see at the heart of the experimental system. On Timberlake's account, there is no abstract general process structure of learning to be found; the learning theorist is instead trying to investigate a sophisticated hierarchically organized structure of motivation and behavior, the behavior system, which the animal brings into the lab. Animal learning is not simply association or reinforcement but modification of the animal behavior system. Control practices for Timberlake must be understood in relation to the specific animal under study, and universality, where it exists at all, is a function of shared evolutionary history and biological needs, rather than the nature of learning.

Olton's research on spatial learning becomes enormously influential in the field; Timberlake's specific behavior systems approach somewhat less so. But perhaps the biggest beneficiary of Olton's and Timberlake's fight with the past has been the rat. Olton and Timberlake were part of a larger shift in late twentieth-century psychology that emphasized the sophistication and nuance of animals. This shift appreciated animals' cognitive and behavioral capacities and saw their dispositions in light of their evolutionary history.[23] Early researchers were not interested in all that an animal could do. The late nineteenth century was, after all, overflowing with accounts of the incredible observed behavior of animals that had nonetheless been successfully explained by simple approaches such as trial-and-error learning (Romanes 1882; Morgan 1894). And so scientists such as Watson and Carr sought the underlying architecture of learning, controlling away ecological variability and

[23] The essential text for the revival of interest in the animal mind is Donald Griffin's *The Question of Animal Awareness* (Griffin 1976).

the functional capacities of the experimental animal—sometimes through paint and protocols, sometimes through surgery. Experimental design and associated control practices emerge as a few factors among many that lead to conservativism about the animal mind in early American comparative psychology.

10.5 Conclusion

Scholars such as Ian Hacking and Hans-Jörg Rheinberger have emphasized the built nature of the experiment. Phenomena are not simply stumbled across, but have to be carefully created in the confines of the laboratory. Hacking pairs this with his famous characterization of the "self-vindication" of experimental work, in which a form of coherence is achieved through different aspects of the experimental setup being fit and calibrated to each other (Hacking 1992). This is a powerful analysis. We can see such fit implicitly at play in maze research, especially in the critiques of Olton and Timberlake on the theoretical limitations imposed by maze design.

Taking a perspective of control as an intended purificatory process, in which scientists attempt to stabilize intervening variables to expose the contours and nature of a phenomenon or an intervention, can foreground other aspects of experimental work. First, it highlights that the fit or coherence is more than the smooth operation of the experimental system. It is a critical coherence based on scientists scrutinizing the system in the hope of detecting confounds that are hard to detect precisely because the experimental system operates smoothly whether they are present or not. Second, and true to the specifically historical perspective adopted by this paper, control helps to make sense of how scientists relate experiments to each other. For instance, Stella Vincent filled a gap in Watson and Carr's research by seeing the impact made by a purely visual variable. Over the short term, the cumulative effect of control practices is relatively linear and progresses by explaining how, in the contexts of experimental traditions, an expanding circle of control helps to clear away intruding causes and thus expose phenomena. Longer-term, however, there may be more drastic shifts in the understanding of the phenomenon of interest, and consequently in the relationship between control practices and phenomena. Olton's asterisk-shaped maze and experimental setups sought to recover previously controlled-away spatial cues. Timberlake showed that stimuli previously regarded as neutral can actually be confounds. Perhaps surprisingly, early conceptualizations of learning stemming from maze research were critiqued specifically because of their tightly implemented control practices and experimental rigor—they had controlled away the phenomenon.

Acknowledgments This article benefited from the immense assistance and insights of the other contributors to this volume and the Mellon-Sawyer Seminar more broadly. Particular thanks go to Kärin Nickelsen and Caterina Schürch for their incisive feedback on an earlier draft, and to Jutta Schickore, Claudia Cristalli, Bill Newman, and Jared Neumann for being fellow travelers in the investigation of scientific methodology. It is only with their help that I have (hopefully) shed light on the smallest corner of experimental control.

References

Allen-Hermanson, Sean. 2005. Morgan's Canon Revisited. *Philosophy of Science* 72 (4): 608–631.

Arabatzis, Theodore. 2006. *Representing Electrons: A Biographical Approach to Theoretical Entities*. Chicago: University of Chicago Press.

Arabatzis, Theodore, and Kostas Gavroglu. 2016. From Discrepancy to Discovery: How Argon Became an Element. In *The Philosophy of Historical Case Studies*, ed. Tilman Sauer and Raphael Scholl, 203–222. Boston Studies in the Philosophy and History of Science. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-30229-4_10.

Arnet, Evan. 2019a. Conwy Lloyd Morgan, Methodology, and the Origins of Comparative Psychology. *Journal of the History of Biology* 52 (3): 433–461. https://doi.org/10.1007/s10739-019-09577-2.

———. 2019b. William Timberlake: An Ethologist's Psychologist. *Behavioural Processes* 166 (September): 103895. https://doi.org/10.1016/j.beproc.2019.103895.

Bachelard, Gaston. 2006. Noumenon and Microphysics. *The Philosophical Forum* 37 (1): 75–84. https://doi.org/10.1111/j.1467-9191.2006.00230.x.

Baird, Davis. 2004. *Thing Knowledge: A Philosophy of Scientific Instruments*. Berkeley: University of California Press.

Bimonte-Nelson, Heather A. 2015. *The Maze Book: Theories, Practice, and Protocols for Testing Rodent Cognition*. New York, NY: Humana.

Boakes, Robert A. 1984. *From Darwin to Behaviourism: Psychology and the Minds of Animals*. Cambridge: Cambridge University Press. http://agris.fao.org/agris-search/search.do?recordID=US201300673256.

Bowers, Robert Ian. 2017. Behavior Systems. In *Encyclopedia of Animal Cognition and Behavior*, ed. Jennifer Vonk and Todd Shackelford, 1–8. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-47829-6_1232-1.

———. 2018. A Common Heritage of Behaviour Systems. *Behaviour* 155 (5): 415–442.

Braat, Michiel, Jan Engelen, Ties van Gemert, and Sander Verhaegh. 2020. The Rise and Fall of Behaviorism: The Narrative and the Numbers. Preprint. January 29, 2020. http://philsci-archive.pitt.edu/16865/.

Breland, Keller, and Marian Breland. 1961. The Misbehavior of Organisms. *American Psychologist* 16 (11): 681–684. https://doi.org/10.1037/h0040090.

Burkhardt, Richard W. 2005. *Patterns of Behavior: Konrad Lorenz, Niko Tinbergen, and the Founding of Ethology*. Chicago: University of Chicago Press.

Bussey, Timothy J., Tina L. Padain, Elizabeth A. Skillings, Boyer D. Winters, A. Jennifer Morton, and Lisa M. Saksida. 2008. The Touchscreen Cognitive Testing Method for Rodents: How to Get the Best out of Your Rat. *Learning & Memory* 15 (7): 516–523. https://doi.org/10.1101/lm.987808.

Capshew, James H. 1992. Psychologists on Site: A Reconnaissance of the Historiography of the Laboratory. *American Psychologist* 47 (2): 132–142. https://doi.org/10.1037/0003-066X.47.2.132.

Carr, Harvey. 1917. A Preliminary Study. *The Journal of Animal Behavior* 7: 365.

Carr, Harvey, and John B. Watson. 1908. Orientation in the White Rat. *Journal of Comparative Neurology and Psychology* 18 (1): 27–44.

Cartwright, Nancy. 1997. Where Do Laws of Nature Come From?*. *Dialectica* 51 (1): 65–78. https://doi.org/10.1111/j.1746-8361.1997.tb00021.x.

———. 1999. *The Dappled World: A Study of the Boundaries of Science*. Cambridge: Cambridge University Press.

Castelão-Lawless, Teresa. 1995. Phenomenotechnique in Historical Perspective: Its Origins and Implications for Philosophy of Science. *Philosophy of Science* 62 (1): 44–59. https://doi.org/10.1086/289838.

Clatterbuck, Hayley. 2016. Darwin, Hume, Morgan, and the Verae Causae of Psychology. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 60 (December): 1–14. https://doi.org/10.1016/j.shpsc.2016.09.002.

Costall, Alan. 1993. How Lloyd Morgan's Canon Backfired. *Journal of the History of the Behavioral Sciences* 29 (2): 113–122. https://doi.org/10.1002/1520-6696(199304)29:2<113::AID-JHBS2300290203>3.0.CO;2-G.

Dewsbury, Donald A. 1984. *Comparative Psychology in the Twentieth Century*. Stroudsburg: Hutchinson Ross Publishing Company.

———. 2000. Issues in Comparative Psychology at the Dawn of the 20th Century. *American Psychologist* 55 (7): 750–753. https://doi.org/10.1037/0003-066X.55.7.750.

Donohue, John J., III, and Steven D. Levitt. 2001. The Impact of Legalized Abortion on Crime*. *The Quarterly Journal of Economics* 116 (2): 379–420. https://doi.org/10.1162/00335530151144050.

Donohue, John J., and Steven D. Levitt. 2004. Further Evidence That Legalized Abortion Lowered Crime: A Reply to Joyce. *The Journal of Human Resources* 39 (1): 29–49. https://doi.org/10.2307/3559004.

Donohue, John J., and Steven Levitt. 2020. The Impact of Legalized Abortion on Crime over the Last Two Decades. *American Law and Economics Review* 22 (2): 241–302. https://doi.org/10.1093/aler/ahaa008.

Dörries, Matthias. 1994. Balances, Spectroscopes, and the Reflexive Nature of Experiment. *Studies in History and Philosophy of Science Part A* 25 (1): 1–36. https://doi.org/10.1016/0039-3681(94)90018-3.

Dumont, Julie R., Ryan Salewski, and Flavio Beraldo. 2021. Critical Mass: The Rise of a Touchscreen Technology Community for Rodent Cognitive Testing. *Genes, Brain and Behavior* 20 (1): e12650. https://doi.org/10.1111/gbb.12650.

Fitzpatrick, Simon. 2008. Doing Away with Morgan's Canon. *Mind & Language* 23 (2): 224–246. https://doi.org/10.1111/j.1468-0017.2007.00338.x.

Gewirtz, Jacob L. 2001. J. B. Watson's Approach to Learning: Why Pavlov? Why Not Thorndike? *Behavioral Development Bulletin* 10: 23–25. https://doi.org/10.1037/h0100478.

Goodwin, C.J. 1987. In Hall's Shadow: Edmund Clark Sanford (1859–1924). *Journal of the History of the Behavioral Sciences* 23 (2): 153–168. https://doi.org/10.1002/1520-6696(198704)23:2<153::aid-jhbs2300230205>3.0.co;2-4.

Griffin, Donald Redfield. 1976. *The Question of Animal Awareness: Evolutionary Continuity of Mental Experience*. New York, NY: Rockefeller University Press.

Guttinger, Stephan. 2019. A New Account of Replication in the Experimental Life Sciences. *Philosophy of Science* 86 (3): 453–471. https://doi.org/10.1086/703555.

Hacking, Ian. 1983. *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge; New York: Cambridge University Press.

———. 1988. The Participant Irrealist at Large in the Laboratory. *The British Journal for the Philosophy of Science* 39 (3): 277–294.

———. 1991. Artificial Phenomena. *The British Journal for the History of Science* 24 (2): 235–241. https://doi.org/10.1017/S0007087400027096.

———. 1992. *The Self-Vindication of the Laboratory Sciences,[w:] A. Pickering (red.), Science as Practice and Culture*, 29–64. London: The University of Chicago Press, Chicago.

Hoffman, Cynthia M., William Timberlake, Joseph Leffel, and Rory Gont. 1999. How Is Radial Arm Maze Behavior in Rats Related to Locomotor Search Tactics? *Animal Learning & Behavior* 27 (4): 426–444. https://doi.org/10.3758/BF03209979.

Horner, Alexa E., Christopher J. Heath, Martha Hvoslef-Eide, Brianne A. Kent, Chi Hun Kim, Simon R.O. Nilsson, Johan Alsiö, et al. 2013. The Touchscreen Operant Platform for Testing Learning and Memory in Rats and Mice. *Nature Protocols* 8 (10): 1961–1984. https://doi.org/10.1038/nprot.2013.122.

Hunter, Walter S. 1920. The Temporal Maze and Kinaesthetic Sensory Processes in the White Rat. *Psychobiology* 2: 1–17. https://doi.org/10.1037/h0073855.

Hunter, Walter S. 1926. "A Reply to Professor Carr on 'The Reliability of the Maze Experiment'."

Hunter, Walter S., and Joseph W. Nagge. 1931. The White Rat and the Double Alternation Temporal Maze. *The Pedagogical Seminary and Journal of Genetic Psychology* 39 (3): 303–319. https://doi.org/10.1080/08856559.1931.10532407.

Jonçich, Geraldine. 1968. *The Sane Positivist: A Biography of Edward L. Thorndike*. Middletown: Wesleyan University Press.

Joyce, Ted. 2004. Did Legalized Abortion Lower Crime? *Journal of Human Resources* XXXIX (1): 1–28. https://doi.org/10.3368/jhr.XXXIX.1.1.

Galef Jr. Bennett G. 1988. "Evolution and Learning before Thorndike: A Forgotten Epoch in the History of Behavioral Research." In Evolution and Learning, edited by R. C. Bolles and M. D. Beecher, 39–58. Hillsdale: Lawrence Erlbaum Associates, Inc.

Galef, Bennett G., Jr. 1998. Edward Thorndike: Revolutionary Psychologist, Ambiguous Biologist. *American Psychologist* 53 (10): 1128–1134. https://doi.org/10.1037/0003-066X.53.10.1128.

Kline, Linus W. 1899. Suggestions toward a Laboratory Course in Comparative Psychology. *The American Journal of Psychology* 10 (3): 399–430. https://doi.org/10.2307/1412142.

Longino, Helen E. 1990. *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press.

———. 2022. What's Social about Social Epistemology? *The Journal of Philosophy* 119 (4): 169–195. https://doi.org/10.5840/jphil2022119413.

Miles, Walter R. 1930. On the History of Research with Rats and Mazes: A Collection of Notes. *The Journal of General Psychology* 3 (2): 324–337.

Mills, Wesley. 1899. The Nature of Animal Intelligence and the Methods of Investigating It. *Psychological Review* 6 (3): 262–274. https://doi.org/10.1037/h0074808.

Moody, Carlisle E., and Thomas B. Marvell. 2010. On the Choice of Control Variables in the Crime Equation. *Oxford Bulletin of Economics and Statistics* 72 (5): 696–715.

Morgan, Conwy Lloyd. 1894. *An Introduction to Comparative Psychology*, Contemporary Science Series. London: Walter Scott Publishing Co.

———. 1896. *Habit and Instinct*. London: E. Arnold.

———. 1898. Review of Thorndike's Animal Intelligence. *Nature* 58 (1498): 249–250.

Olton, David S. 1978. Characteristics of Spatial Memory. In *Cognitive Processes in Animal Behavior*, ed. Stewart H. Hulse, Harry Fowler, and Werner K. Honig, 341–373. Routledge.

———. 1979. Mazes, Maps, and Memory. *American Psychologist* 34 (7): 583–596. https://doi.org/10.1037/0003-066X.34.7.583.

Olton, David S., and Robert J. Samuelson. 1976. Remembrance of Places Passed: Spatial Memory in Rats. *Journal of Experimental Psychology: Animal Behavior Processes* 2 (2): 97–116. https://doi.org/10.1037/0097-7403.2.2.97.

Radick, Gregory. 2000. Morgan's Canon, Garner's Phonograph, and the Evolutionary Origins of Language and Reason. *The British Journal for the History of Science* 33 (1): 3–23. https://doi.org/10.1017/S0007087499003842.

Rasmussen, Nicolas. 1993. Facts, Artifacts, and Mesosomes: Practicing Epistemology with the Electron Microscope. *Studies in History and Philosophy of Science Part A* 24 (2): 227–265.

———. 1999. *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940–1960*. Stanford: Stanford University Press.

Rheinberger, Hans-Jörg. 1997. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube (Writing Science)*. Stanford: Stanford University Press.

———. 2005. Gaston Bachelard and the Notion of 'Phenomenotechnique. *Perspectives on Science* 13 (3): 313–328. https://doi.org/10.1162/106361405774288026.

Richardson, Florence. 1909. A Study of the Sensory Control in the Rat. *The Psychological Review: Monograph Supplements* 12 (1): i–124. https://doi.org/10.1037/h0093009.

Romanes, George J. 1882. *Animal Intelligence. The International Scientific Series*. London: Kegan Paul, Trench & Co.

Schickore, Jutta. 2001. Ever-Present Impediments: Exploring Instruments and Methods of Microscopy. *Perspectives on Science* 9 (2): 126–146. https://doi.org/10.1162/106361401317447255.

———. 2007. *The Microscope and the Eye: A History of Reflections, 1740–1870*. Chicago: University of Chicago Press.

———. 2017. *About Method: Experimenters, Snake Venom, and the History of Writing Scientifically*. Chicago: University of Chicago Press.

———. 2019. The Structure and Function of Experimental Control in the Life Sciences. *Philosophy of Science* 86 (2): 203–218.

Seligman, Martin E. 1970. On the Generality of the Laws of Learning. *Psychological Review* 77 (5): 406–418. https://doi.org/10.1037/h0029790.

Shapiro, Matthew L. n.d. "Olton, David (1943–1994) |." Encyclopedia.Com. Accessed 14 Sept 2022. https://www.encyclopedia.com/psychology/encyclopedias-almanacs-transcripts-and-maps/olton-david-1943-1994

Shoesmith, Gary L. 2017. Crime, Teenage Abortion, and Unwantedness. *Crime & Delinquency* 63 (11): 1458–1490. https://doi.org/10.1177/0011128715615882.

Simons, Massimiliano, and Matteo Vagelli. 2021. Were Experiments Ever Neglected? Ian Hacking and the History of Philosophy of Experiment. *Philosophical Inquiries* 9 (1): 167–188. https://doi.org/10.4454/philinq.v9i1.339.

Skinner, B.F. 1938. *The Behavior of Organisms: An Experimental Analysis*. Oxford: Appleton-Century.

———. 1956. A Case History in Scientific Method. *American Psychologist* 11: 221–233. https://doi.org/10.1037/h0047662.

Small, Willard S. 1899. Notes on the Psychic Development of the Young White Rat. *The American Journal of Psychology* 11 (1): 80–100.

———. 1901. "Experimental Study of the Mental Processes of the Rat. II." The American Journal of Psychology 12 (2): 206–239. https://doi.org/10.2307/1412534.

Sober, Elliott. 1998. Morgan's Canon. In *The Evolution of Mind*, ed. D.D. Cummins and C. Allen, 224–242. New York, NY: Oxford University Press.

Steinle, Friedrich. 1997. Entering New Fields: Exploratory Uses of Experimentation. *Philosophy of Science* 64 (S4): S65–S74.

Sullivan, Jacqueline A. 2021. Understanding Stability in Cognitive Neuroscience through Hacking's Lens. *Philosophical Inquiries* 9 (1): 189–208. https://doi.org/10.4454/philinq.v9i1.346.

———. 2022. Novel Tool Development and the Dynamics of Control: The Rodent Touchscreen Operant Chamber as a Case Study. *Philosophy of Science* 89: 1203–1212. https://doi.org/10.1017/psa.2022.63.

Thomas, Roger K. 2001. *Llloyd Morgan's Canon: A History of Its Misrepresentation*. History and Theory in Psychology.

Thorndike, Edward L. 1898. Animal Intelligence: An Experimental Study of the Associative Processes in Animals. *The Psychological Review: Monograph Supplements* 2 (4): i–109. https://doi.org/10.1037/h0092987.

———. 1899. A Reply to ' The Nature of Animal Intelligence and the Methods of Investigating It.'. *Psychological Review* 6 (4): 412–420. https://doi.org/10.1037/h0073289.

———. 1901. The Mental Life of the Monkeys. *The Psychological Review: Monograph Supplements* 3 (5): i–57. https://doi.org/10.1037/h0092994.

Timberlake, William. 1983a. The Functional Organization of Appetitive Behavior: Behavior Systems and Learning. *Advances in Analysis of Behavior* 3: 177–221.

———. 1983b. Appetitive Structure and Straight Alley Running. In *Animal Cognition and Behavior*, ed. Roger L. Mellgren, 165–222. Advances in Psychology, vol. 13. Amsterdam: North-Holland. https://doi.org/10.1016/S0166-4115(08)61797-5.

———. 1993. Behavior Systems and Reinforcement: An Integrative Approach. *Journal of the Experimental Analysis of Behavior* 60 (1): 105–128. https://doi.org/10.1901/jeab.1993.60-105.

———. 2002. Niche-Related Learning in Laboratory Paradigms: The Case of Maze Behavior in Norway Rats. *Behavioural Brain Research* 134 (1): 355–374. https://doi.org/10.1016/S0166-4328(02)00048-7.

Timberlake, William, and Douglas L. Grant. 1975. Auto-Shaping in Rats to the Presentation of Another Rat Predicting Food. *Science* 190 (4215): 690–692.

Timberlake, William, Joseph Leffel, and Cynthia M. Hoffman. 1999. "Stimulus Control and Function of Arm and Wall Travel by Rats on a Radial Arm Floor Maze." Animal Learning & Behavior 27 (4): 445–460. https://doi.org/10.3758/BF03209980.

Tolman, E.C. 1930. Maze Performance a Function of Motivation and of Reward as Well as of Knowledge of the Maze Paths. *The Journal of General Psychology* 4 (1–4): 338–342.

———. 1932. *Purposive Behavior in Animals and Men*. Berkeley: University of California Press.

———. 1938. The Determiners of Behavior at a Choice Point. *Psychological Review* 45 (1): 1–41. https://doi.org/10.1037/h0062733.

Traetta, Luigi. 2020. At the Beginning of Learning Studies There Was the Maze. *Open Journal of Medical Psychology* 9 (04): 168.

Vincent, Stella B. 1912. *The Functions of the Vibrissae in the Behavior of the White Rat*. Vol. 1. Chicago: University of Chicago.

———. 1915a. The White Rat and the Maze Problem: The Introduction of a Tactual Control. *Journal of Animal Behavior* 5: 175–184. https://doi.org/10.1037/h0075779.

———. 1915b. The White Rat and the Maze Problem: The Introduction of an Olfactory Control. *Journal of Animal Behavior* 5: 140–157. https://doi.org/10.1037/h0070053.

———. 1915c. The White Rat and the Maze Problem: The Number and Distribution of Errors- A Comparative Study. *Journal of Animal Behavior* 5: 367–374. https://doi.org/10.1037/h0075635.

———. 1915d. The White Rat and the Maze Problem: The Introduction of a Visual Control. *Journal of Animal Behavior* 5 (1): 1–24. https://doi.org/10.1037/h0072410.

Warden, C.J. 1929. A Standard Unit Animal Maze for General Laboratory Use. *The Pedagogical Seminary and Journal of Genetic Psychology* 36 (1): 174–176.

Warner, Lucien Hynes, and Carl John Warden. 1927. "The Development of a Standardized Animal Maze." In Archives of Psychology, vol. 15, no. 92, edited by R. S. Woodworth, 1–59. New York, NY: Archives of Psychology.

Washburn, Margaret Floy. 1908. Review of *Review of Orientation in the White Rat*, by Harvey Carr and John B. Watson. *The Journal of Philosophy, Psychology and Scientific Methods* 5 (10): 275–277. https://doi.org/10.2307/2011775.

Watrin, João Paulo. 2017. The 'New History of Psychology' and the Uses and Abuses of Dichotomies. *Theory & Psychology* 27 (1): 69–86. https://doi.org/10.1177/0959354316685450.

Watrin, João Paulo, and Rosângela Darwich. 2012. On Behaviorism in the Cognitive Revolution: Myth and Reactions. *Review of General Psychology* 16 (3): 269–282. https://doi.org/10.1037/a0026766.

Watson, John B. 1907. Kinæsthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze. *The Psychological Review: Monograph Supplements* 8: i–101. https://doi.org/10.1037/h0093040.

Wilson, David A.H. 2002. Experimental Animal Behaviour Studies: The Loss of Initiative in Britain 100 Years Ago. *History of Science* 40 (3): 291–320. https://doi.org/10.1177/007327530204000302.

Wimsatt, William C., and James R. Griesemer. 2007. Reproducing Entrenchments to Scaffold Culture: The Central Role of Development in Cultural Evolution. In *Integrating Evolution and Development: From Theory to Practice*, ed. Roger Sansom and Robert N. Brandon, 227–323. Cambridge, MA: MIT Press.

Yerkes, Robert M. 1901. The Formation of Habits in the Turtle. Popular Science Monthly, March.

———. 1902. "The Formation of Habits in the Turtle" *Popular Science Monthly,* March. "Habit Formation in the Green Crab, Carcinus Granulatus." Biological Bulletin III: 241–244. https://doi.org/10.2307/1535878.

Yerkes, Robert M., and Gurry E. Huggins. 1903. Habit Formation in the Crawfish Cambarus Affinis. *The Psychological Review: Monograph Supplements* 4 (1): 565–577.

Evan Arnet is a philosopher and historian researching the long-term epistemology of science and was a Mellon-Sawyer Fellow at the Department of History and Philosophy of Science and Medicine, Indiana University (Bloomington). He is particularly interested in the role of reflection and critique in the development and refinement of scientific methodology, especially in the animal behavior sciences. He is currently a research development specialist at Indiana University.