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**The Curious Case of the Decapitated Frog**

Professor Alexander Klein

“Ever heard of a pithed frog? … It’s a thing these here vivisectionists do. They takes a frog and they cuts out his brains and they shove in a bit of pith in the place of ’em.”[[1]](#footnote-1)

As World War II drew to a close, an unassuming chicken became an American celebrity. He had survived his own decapitation, and for 18 months the headless bird could be found preening around sideshows across the western United States. The chicken’s nickname—richly earned, it seems—was “Miracle” Mike, and by the time Life Magazine ran a national feature on him he had become an authentic cultural icon.[[2]](#footnote-2)

Some suspected a hoax, apparently. But since the time of Aristotle, students of physiology have understood that some vertebrates can survive for months without a brain.[[3]](#footnote-3) Miracle Mike pulled through his botched decapitation merely by chance; but physiologists have long employed a purposeful procedure, called “pithing,” to prepare brainless animals for experimentation. Wells’ Amiable Tramp gives the gist of the operation, though in a laboratory the procedure is typically performed with a blunt needle.

The historian Franklin Fearing has said that experimentation on pithed animals “occupied the attention of almost all physiologists who lived during the second half of the 19th century.”[[4]](#footnote-4) Experimentalists pithed fish, birds, and even dogs. But the most frequently pithed animal seems to have been the common frog.[[5]](#footnote-5)

Why were so many 19th-century physiologists preoccupied with the pithed frog? The story begins with Eduard Pflüger’s 1853 experiments showing that decapitated animals exhibit behavior it is tempting to call purposive. The results were controversial because purposive behavior has long been regarded as a mark[[6]](#footnote-6) of consciousness. Those who continued to think it was such a mark had to count a chicken literally running around with its head cut off as conscious. Those who said purposive behavior was not a mark of consciousness could avoid saying peculiar things about decapitated animals. But this view opened the way for epiphenomenalism: just as headless chickens seem to act with purpose even though their behavior is not really guided by phenomenal consciousness, so human behaviors may seem purposive without really being guided by phenomenal consciousness.

The dispute spurred extensive experimentation, and in the heart of this paper I will evaluate whether the experiments actually have probative value for the philosophical theses at issue in the debate. I will contend that at least as our 19th century figures framed it, the choice of whether to accept that phenomenal consciousness influences behavior is not one that can be supported directly by experimental results, despite what they thought. That having been said, the story offers helpful suggestions about the role experiment might legitimately play in philosophy, even today. Let me explain.

As William James observed as late as 1890, the dispute long remained stalemated.[[7]](#footnote-7) I will contend that this is because it rested on two mutually exclusive sets of intuitions that were both consistent with the growing body of experimental results. What finally resolved the dispute (about a decade later) was that the competing sets of intuitions each issued different research programs in science, and as one of these programs proved more fruitful, the corresponding set of intuitions was thereby vindicated.

So my case study suggests an alternative model for what experimental philosophy might look like, today. Some philosophers have lately been using experiment to try to bolster their intuitions directly, and I will explore some of that work in the final section. But the moral of my story is that we should instead be exploring how our philosophical intuitions might bolster (or block) fruitful experimental research in science.

Before turning to my case study, I will begin with some historical background.

## 1. Brief Background: Mechanism and Animism

Less than a century after Descartes’ death, a young inventor named Jacques de Vaucanson (1709 – 1782) placed three remarkable automata on display in Paris. The most famous of these was a mechanical duck that flapped its wings, bobbed its head, and even ate, drank, swallowed, and defecated. What captivated audiences when the automata appeared (first at the Hôtel de Longueville in 1738, and later across Europe) was not just the duck’s range of behaviors, but its lifelike comportment in particular.[[8]](#footnote-8)

In order to build his simulacrum, Vaucanson drew from an extensive, anatomical study of living ducks. For instance, his automaton’s wings each contained more than four hundred pieces, mimicking actual anatine anatomy. The effect was an automaton that flapped and bobbed about with uncanny articulation.

Vaucanson’s duck was not merely an amusing toy, though it was certainly that. It was also a vivid illustration of the prospects for strictly mechanistic accounts of animal physiology. Here is D’Alembert on the duck in 1751:

All the movements of the duck, which swallows quickly and increases the speed of the movement of its gullet in order to pass the food down to the stomach, are copied from nature. The nutriment is digested as happens with real animals, by means of dissolution and not by trituration. The matter digested in the stomach is conveyed through tubes similar to the bowels of the real animal, to the anus where a sphincter permits the excretion.[[9]](#footnote-9) … Anatomists will find nothing to criticize about the construction of the wings. All the bones have been copied. All those protuberances called apophyses are faithfully followed as are the different joints, hollows, and curves. The three bones that form the wings are quite distinct. The first is the humerus and rotates in all directions with a bone that serves as the shoulder blade. The second is the cubitus of the wing and moves with the humerus by means of a joint that anatomists call the hinge-joint. The third is the radius that turns in the hollow of the humerus and is attached at its terminations to the small bones of the wing tip, just as is the case with the real animal.

Sculpting a life-like model of a duck is one thing, but building a convincing mechanical simulacrum that mimics a duck’s anatomy in motion is quite another. Vaucanson essentially reverse-engineered duck physiology, demonstrating just how authentic the bodily action of a strictly mechanical replica could be.

Vaucanson’s duck became a favorite reference point in discussions of an old Cartesian hypothesis.[[10]](#footnote-10) Descartes had famously contended that living animals might be machines—natural automata—in the sense of being non-conscious organisms all of whose behaviors are produced strictly mechanistically. Vaucanson’s duck seemed to advance physiologists one step closer to a constructive proof of Descartes’ conjecture, since the automatic duck’s physiology was in no way mediated by any soul or consciousness.[[11]](#footnote-11)

Those in the 17th and 18th century who largely adopted the Cartesian approach to animal physiology are often called “mechanists,” and their approach is typically contrasted with so-called “animists.” What separated the two groups was the issue of whether and to what extent the mechanical principles of Newton and Boyle could account for the functioning of living organisms, much as they could be used to account for the motions of automata of the sort Vaucanson had built.

Mechanists like Friedrich Hoffmann (1660 – 1742), Herman Boerhaave (1668 – 1738), and Julien Offray de La Mettrie (1709 – 1751) sought as far as possible to expand the purview of Cartesian, strictly mechanistic animal physiology to include even human physiology. Animists like Jean Baptiste van Helmont (1577 – 1644) and Georg Ernst Stahl (1659 – 1734) gave the soul a causal role in the production of bodily motion, including (what came to be called) “reflex action.”[[12]](#footnote-12) (A reflex action is an involuntary behavior like flinching or blinking in response to an object moving towards your eye.)[[13]](#footnote-13)

Through much of the 18th century, animistic tendencies still dominated much physiological thinking, even for those more inclined towards mechanism. For instance, Giovanni Borelli (1608 – 1679) had developed a purely mechanistic account of how the heart pumps blood. But even the mechanist Borelli adopted the animist idea that the soul was involved in this motion. As Hubert Steinke has pointed out, Borelli contended that “the unpleasant accumulation of blood in the heart of the preformed embryo would be perceived by the ‘sentient faculty’ (facultas sensitiva) of the soul through the nerves, which would then prompt the ventricle to contract.”[[14]](#footnote-14) Only after the process was thus initiated would the circulation continue mechanistically, as a kind of physical, acquired habit. But the ultimate cause of this motion was the soul.

By the turn of the 19th century, physiologists like Robert Whytt (1714 – 1766) and Marshall Hall (1790 – 1857) had been pulling away from animism,[[15]](#footnote-15) particularly in their accounts of non-voluntary motion. They developed early, mechanistic accounts of reflex action, according to which physical stimuli cause nerve signals to pass through the spinal cord and back out to produce muscular contraction, directly, with no intervention from the soul. Such anti-animistic theories established the idea of a so-called reflex arc, which came to play a central role in later physiology and psychology. As William Clifford would later put it, reflex arc theorists hold that “it is not you who wink your eye, but your body that does it.”[[16]](#footnote-16)

## 2. Pflüger (1853)

The central controversy I want to discuss begins with a challenge to Whytt and Hall’s fully mechanistic account of reflex action. The challenge dates to an 1853 book by the German physiologist Eduard Pflüger.[[17]](#footnote-17) Whytt and Hall had both discussed reflex action in pithed animals at some length, and had satisfied themselves that such behavior could be accounted for purely mechanistically.[[18]](#footnote-18) But Pflüger did not agree, and to get a grip on his worries, it helps to consider some of his experiments.

Many 19th-century texts on physiology offered instructions for performing Pflüger’s most famous experiment, and they went something like this (consult figure 1 for an illustration):

Step 1: Pith a frog and suspend it from a hook.[[19]](#footnote-19) Dip some filter paper in acetic acid, and touch the frog’s thigh.

Step 2: Observe which foot the frog uses to wipe away the acid.

Step 3: Dip the frog in water to wash off the acid. Wait five minutes.

Step 4: Amputate the acid-wiping foot. Apply the acidulated paper to the thigh again, and observe the frog’s reaction. You should find that the frog will choose a different means to achieve the same end—it will choose a different foot to try to wipe away the irritant, typically, or it will rub the thigh against a foreign surface, if available.[[20]](#footnote-20)

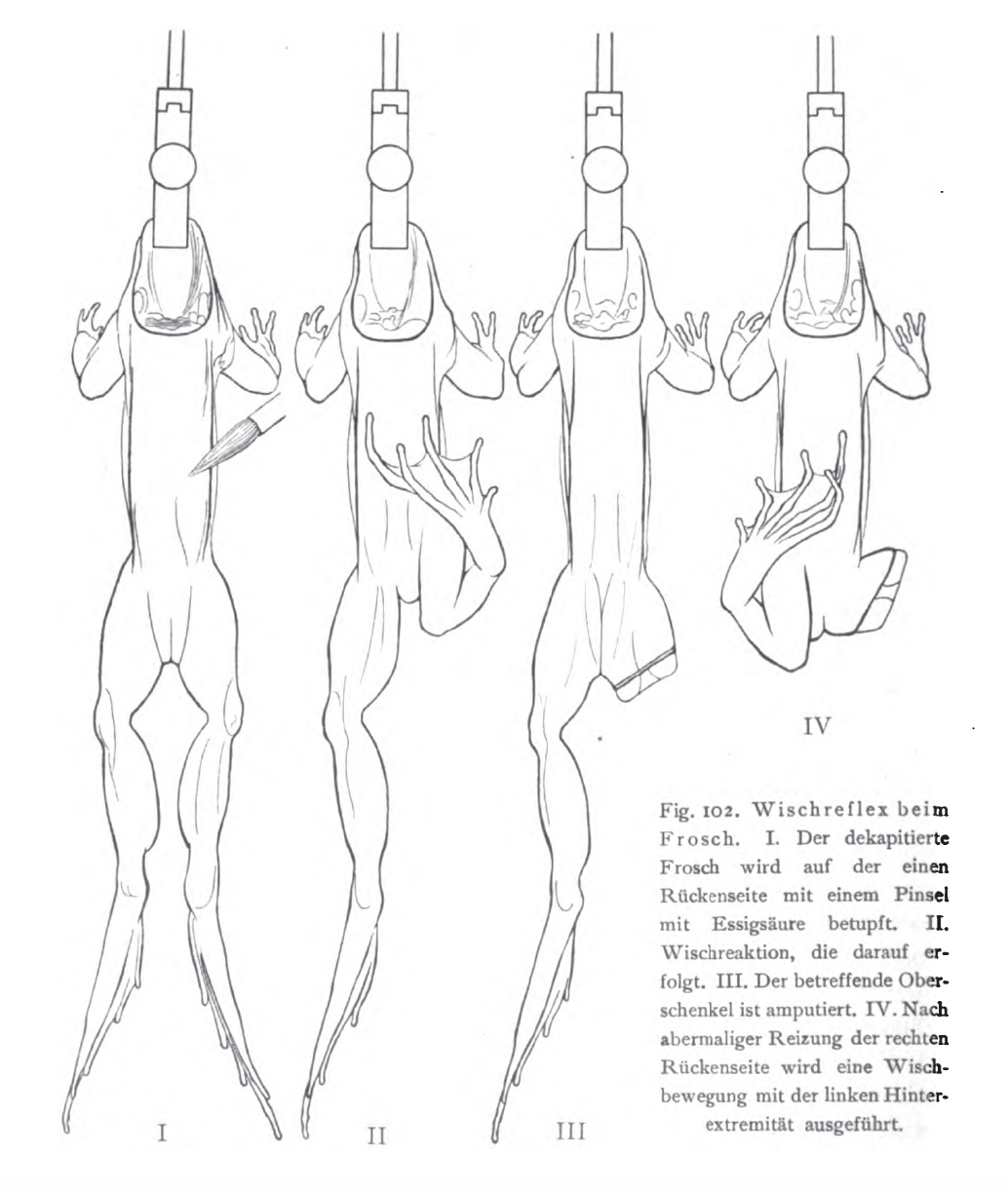


Figure 1: Pflüger's experiment, as depicted in Max Verworn, Physiologisches Praktikum Für Mediziner, 2nd ed. (Jena: G. Fischer, 1912) 198.

One might assume that the initial wiping behavior (in step two) is merely a reflex action—that is to say, a physiological process that is mechanistically determined from stimulus through to the response, without any intervention from a willful, conscious mind. Indeed, for Hall, that the frog can wipe acid in the manner of step two constituted evidence that reflex action is purely mechanical, and not controlled by the will.[[21]](#footnote-21) But what should we say about the frog’s behavior in step four, when its favored foot is impeded or amputated? Rather than reflexively waving around its stump, the frog chooses a novel means to achieve the same end, and it is tempting to count such choosing-behavior as purposive. Now, suppose one accepts purposive behavior as a mark of consciousness (or sensation, or volition, or all of these). Then one arrives at a surprising result indeed—that the brainless frog, properly prepared, remains a conscious agent.

Of course, there is a lot riding on just what is meant by “consciousness,” “sensation,” and “volition.” Pflüger himself often wrote about the pithed frog’s supposed “consciousness” (Bewusstsein), but was rather loose and poetic in spelling out what that term was to mean.[[22]](#footnote-22) Still, his basic thesis was clear enough: that in addition to the brain, the spinal cord is also an organ that independently produces consciousness. One controversial implication is that consciousness itself may be divisible (and so literally extended)[[23]](#footnote-23)—it may exist in various parts of the nervous system, even in a part of the spinal cord that has been divided from the brain.[[24]](#footnote-24)

Pflüger’s provocative discussion sparked a battery of experimental investigation from others in the coming decades. As it turns out, the pithed frog is capable of remarkably complex behavior, even more so than what we’ve just seen. For example, such a frog will swim if dropped in water, for instance.[[25]](#footnote-25) If completely submerged, it will swim to the surface. And not only that; if one impedes the emerging, decapitated frog by putting an inverted jar in its path, the frog will not easily be trapped. It will actually re-descend until it can swim out of the jar, and then will swim up to the surface (see figure 2).[[26]](#footnote-26) This is an astonishing sequence of behaviors for an animal that lacks a brain.

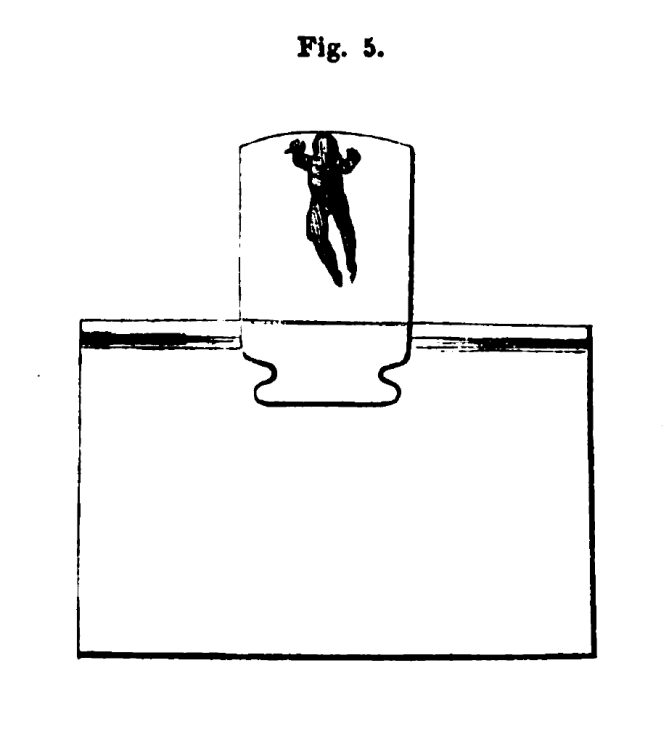


Figure 2: from Friedrich Leopold Goltz, Beiträge Zur Lehre Von Den Functionen Der Nervencentren Des Frosches (Berlin: A. Hirschwald, 1869) 70.

To give another memorable example, a frog whose cerebral hemispheres have been destroyed might be expected to have no sense of vision. And yet it will hop towards light from a distant window if its hind legs are irritated, even hopping around a barrier placed between the frog and the light. What is more, if the procedure is repeated with the barrier moved to block the original path, the frog will again simply maneuver around the barrier, finding another path with little apparent trouble.[[27]](#footnote-27)

Now, opposition to Pflüger’s original analysis had come swiftly, first of all from the philosopher and psychologist Rudolph Lotze. Lotze claimed that the amputated frog’s behavior could arise as a result of the nervous system having been in some way trained or educated before the animal was pithed.[[28]](#footnote-28) Lotze’s thought was that these behaviors seem purposive only because they are complex. If we allow that the nervous system can acquire complex, reflexive actions through bodily learning, then we can maintain that these behaviors are mechanically determined, and not guided or accompanied by any phenomenal consciousness.

The initial difficulty with this response is that pithed frogs find ways to solve physical challenges they cannot be supposed to have faced before being pithed. For instance, suppose one places a pithed frog on its back, holds one leg straight up, perpendicular to the body, and irritates the leg with acid. The pithed frog will then raise the other leg to the same, odd position so as to be able to wipe away the irritant.[[29]](#footnote-29) Few if any frogs can have experienced such an unusual leg position before being pithed and probed in this manner in the laboratory.

In any case, I now want to turn to the way this debate would play out in the coming decades, particularly in Great Britain. One can usefully divide that ensuing controversy into two categories. First, there was considerable discussion about the facts themselves, a discussion that did resolve itself through further experimentation. But it is instructive to notice that there was another, more philosophical debate over how to interpret the facts, a debate that by its very nature resisted a purely experimental resolution. I will take the debate about the facts first.

## 3. Getting Clear on Some Facts

It should be unsurprising to hear that just what behaviors the pithed frog is capable of depends on precisely what parts of the frog’s brain are disabled during pithing. In the decades following Pflüger’s original work, one does see a consensus on these details, but it forms slowly. For instance, one of Pflüger’s key allies George Henry Lewes (more on whom, below) began writing about these issues in 1859. But in his early contributions to this literature, Lewes simply reported the behaviors he had observed in what he simply characterized as “decapitated” frogs.[[30]](#footnote-30) He did not bother to report precisely which brain structures he had destroyed during the pithing process.

But later contributions to this literature took much more care in specifying which structures had been disabled in various experiments. Before turning to some of this later work, let us recall some basic anatomical details of frogs (see figure 3). The medulla oblongata is at the bottom of the brain stem. Above the brain stem is the cerebellum; above that is the optic lobe; and the cerebral hemispheres are, so to speak, highest of all.

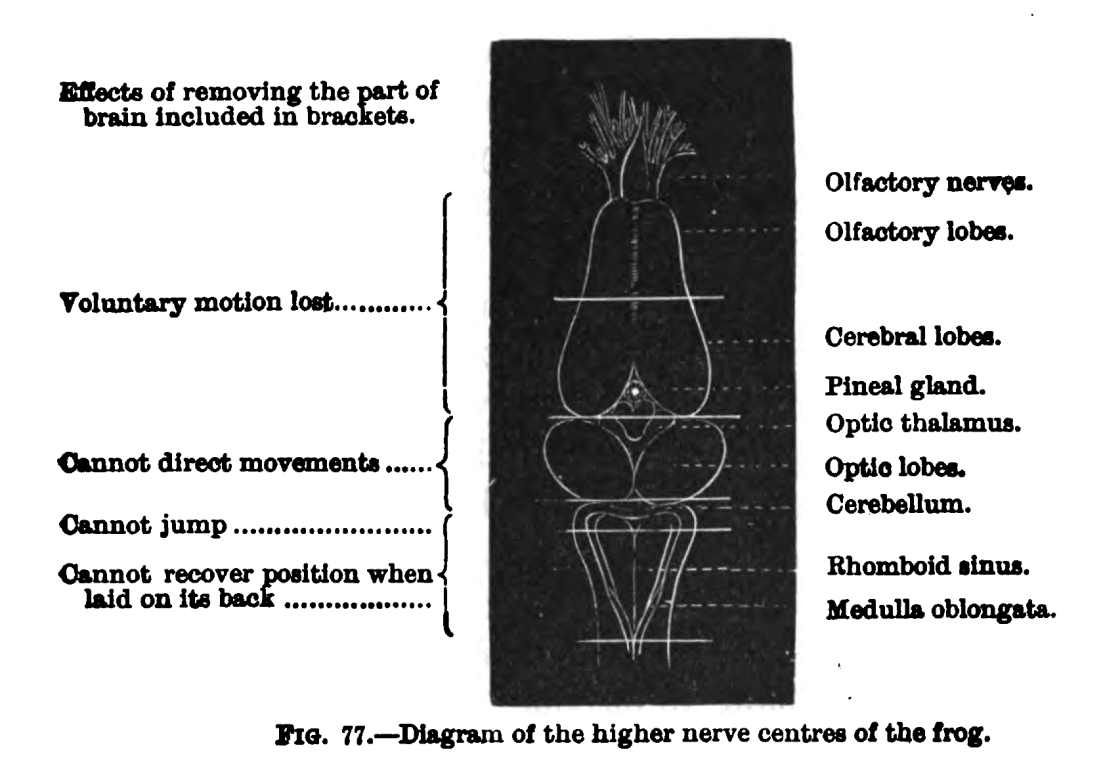


Figure 3: Frog brain structures, along with summary of pithing results circa 1898.[[31]](#footnote-31)

Now here is T. H. Huxley summarizing some of the more careful pithing results in 1870:

Let the two hemispheres of the cerebrum be cut away. The condition of the frog becomes very singular. It preserves almost all the faculties of an uninjured frog. It can see, swallow, jump, and swim; but it exerts none of these powers spontaneously. It will not even feed, but has to be fed with meat put into its throat. It is like an animal in a trance, or asleep. Nevertheless it can adjust all its movements so as to balance its body under the most difficult circumstances. [fn. omitted] In short, it adapts means to ends with wonderful accuracy and precision. But if more of the brain is removed, and the structures known as the optic lobes are cut away, this power is lost; and if the cerebellum is removed, the frog cannot even combine its actions so as to jump.[[32]](#footnote-32)

A frog that is pithed above the medulla would have that structure still in tact, attached to the spinal cord; but it would lack use of the cerebellum, the optic lobe, and the cerebral hemispheres (again, consult figure 3). Here, Huxley tells us that such a frog loses the ability to jump, even though the frog with the brain stem and cerebellum in tact is able to perform this action, at least in response to irritation. One can see why researchers wanted to get a grip on coordinated behaviors like jumping or swimming. The most clearly purposive behaviors, after all, involve a coordination of disparate movements (think of the frog escaping the inverted jar).[[33]](#footnote-33)

Although Huxley doesn’t emphasize this point, some of Pflüger’s most careful defenders would show that frogs clearly are capable of complex behavior even with everything above the medulla oblongata destroyed (but with the medulla in tact). Perhaps such a frog cannot jump, as Huxley reported. But it can regain its normal posture if placed on its back.[[34]](#footnote-34) True, everybody at the time agreed that severing below the medulla renders the frog seriously incapacitated—then it can no longer flip itself over, and it ceases even to breathe on its own. But surprisingly, such a frog can still perform the task I mentioned earlier, where an acid irritant is wiped from a leg that is artificially extended perpendicular to the body.[[35]](#footnote-35) Surely that is a complex behavior.

What is more, as early as 1871, William Rutherford reported that even without its medulla oblongata, the frog will still find another way to wipe a spot of acid from its body even when its preferred wiping-foot is amputated. In other words, Pflüger’s classic experiment still works on frogs whose spinal cord is severed below the medulla.[[36]](#footnote-36)

What I want to emphasize is that as soon as replicable experiments established what abilities the frog loses when specific brain structures are damaged, consensus seems to have formed easily. One does not have to believe that there are experimenta crucis to notice that there were few background assumptions at play that might have been dubious enough to be worth debating. Thus I would like to call these questions straightforwardly empirical. Answers to these questions come relatively directly from experiment.

## 4. Getting Clear on Interpretation? Mechanist’s Dilemma

But what about the big question that motivated this debate to begin with—namely, whether the spine itself is an organ of consciousness (or sensation or volition)? I want to argue that this is not a straightforwardly empirical question. Here we are dealing with intuitions, and intuitions that cannot be either vindicated or undermined by experiment, at least not directly.

To see this, it helps to turn to a prior question that arises in the work of Pflüger’s most able ally, the English literary critic, philosopher, and physiologist G. H. Lewes. Lewes had repeated Pflüger’s procedures and had created some new experiments of his own. But he also offered some sophisticated arguments for using this experimental basis to establish sensation and volition in the spinal cord. (Lewes tends to focus on “sensation” and “volition” rather than on “consciousness,” the latter of which he regards as a confused term.)[[37]](#footnote-37)

Pflüger’s initial work on frogs appeared in 1853. Five years late, Lewes published accounts both of his own experiments and of Pflüger’s original work. Here is Lewes laying out the assumptions behind his interpretation of these experiments:

Before detailing the evidence for the sensorial functions of the chord, it will be necessary to fix on some broad and palpable signs, such as unequivocally indicate the presence of volition. We have such signs in spontaneity of actions and choice of actions. It will scarcely be disputed that an animal manifests volition—and its act is voluntary—when the act occurs spontaneously. By “spontaneously,” I mean prompted by some inward impulse, and not excited by an outward stimulus. Spontaneity and choice are two palpable characteristics of sensation and volition, and it is these we must seek in our experiments.[[38]](#footnote-38)

Lewes eventually backed away from spontaneity as a mark of either sensation or volition.[[39]](#footnote-39) His considered view was that choice alone (choice of means to an end, or what I will sometimes call “purposiveness”) is the crucial mark of both sensation and volition.[[40]](#footnote-40)

With that standard in place, here is the crux of Lewes’s argument in support of the spinal cord as an organ of sensation and volition. If one finds choosing behaviors in pithed frogs, we should treat such frogs as having sensation and volition. Pflüger’s original experiment with the amputated frog gave us a clear example of choosing behavior in a pithed animal; so we must attribute sensation and volition to these pithed animals, on Lewes’s way of thinking, even in cases where the cord is severed from the medulla.

Lewes had a stronger argument still. He contended that in general, whatever outward mark one selects as establishing the existence of sensation and volition, the experimental data will force us either to attribute sensation and volition to both decapitated and in-tact animals alike, or to exclude sensation and volition from both. As Lewes puts it, “Experiment leads decisively to this alternative, namely, either [in-tact] animals are unconscious machines, or decapitated animals manifest sensibility and will.”[[41]](#footnote-41)

I shall call this “the mechanist’s dilemma.” Lewes’s idea is that if one accepts any observable, behavioral mark of sensation and volition, then Lewes is confident that he can demonstrate such behavior in the pithed frog. This would force us to attribute sensation and volition to both intact and pithed frogs alike. If one denies that there is an observable, behavioral mark of sensation and volition, then one is forced to deny that we have reason to think sensation and volition occurs even in healthy, intact vertebrates (including not just frogs, but even our fellow human beings).

Lewes was constantly pressing this dilemma, and sought to illustrate it through a battery of experiments. An early example involves an otherwise intact frog whose spinal cord has been cut about halfway down the back, between the fifth and sixth cervical vertebrae. Once the frog has recovered from the surgery, a casual observer would simply say that paralysis has set in below the cut. The hind legs seem totally incapacitated. If one irritates the frog’s anterior (i.e., its upper body), it will crawl away using its front legs only, dragging the seemingly lifeless hind legs along. But if the tail or hind legs are irritated with acid, the entire posterior section begins to move, attempting to initiate a crawling action. The attempt does not succeed, since in this case the front legs lie motionless, and the back legs are not strong enough to overcome the resistance.

Now what’s interesting is not the result itself—physiologists had long observed what they considered reflex action in paralyzed limbs (e.g., human paraplegics will withdraw their feet if their soles are tickled). What is interesting is the lesson Lewes drew from the case:

If the cessation of motion of the hind legs, when the animal crawled, is a proof that voluntary power was destroyed in those legs, the cessation of motion of the fore legs, when the hind legs moved, is equally a proof that voluntary power was destroyed in the fore legs. The real truth seems to be that each segment has its own volitional centre.[[42]](#footnote-42)

Here was Lewes pressing the mechanist’s dilemma, again. Suppose we propose complex action (rather than purposiveness) as a mark of volition. We cannot then say that since the paraplegic frog exhibits no complex action in its hind legs when the anterior part of the frog is irritated, those hind legs have lost all volition. For that same reasoning would then have to be applied to the front legs, which lie motionless when the hind legs are attempting to crawl. The experimentalist must either attribute conscious control to both the front and back legs (that is Pflüger and Lewes’s position), or she can attribute conscious control to neither.

Lewes might have hoped that mechanists would find the wholesale denial of conscious control in cases like this to be intolerable (remember, in this experiment the animal has its brain, brain stem, and upper spine completely in tact). But by the early 1870s, some mechanists were quite prepared to choose the pointy horn of the dilemma. They came to reject the existence of conscious control (as we normally think of it) all together, even for intact humans. Thus was born the conscious automaton theory.

## 5. Getting Clear on Interpretation? Spinal Consciousness Dilemma

T. H. Huxley offered the most influential and provocative version of the conscious automaton theory in an 1874 address in Belfast. For Huxley, “consciousness” (his word) accompanies the body without acting on it, just as “the steam-whistle which accompanies the work of a locomotive engine is without influence upon its machinery.” This is an early form of epiphenomenalism. True, Lewes had focused on the role of volition (rather than consciousness) in various actions; but Huxley immediately makes clear that he think “volition,” too, is an epiphenomenon, calling it “an emotion indicative of physical changes, not a cause of such changes.”[[43]](#footnote-43)

Huxley canvassed some of Pflüger and Lewes’s experimental results, but drew a starkly different lesson. The results did establish purposive behavior in the pithed frog, Huxley readily conceded. But the real import of these experiments was to show us that purposive actions are not sufficient to establish the existence of either consciousness or volition.

There are two issues between Huxley on one side, and Pflüger and Lewes on the other, that need to be teased apart. When it comes to the issue of whether any observable properties of behaviors (such as choosing) are marks of consciousness, Huxley has effectively chosen what I’ve called the sharp horn of Lewes’s mechanist dilemma. He’s happy to say that there are no marks of consciously-controlled behavior, and then to accept that no behavior is consciously-controlled.

But the bodily seat of consciousness is a separate issue. Remember that as an epiphenomenalist, Huxley did not deny that consciousness exists—he just denied that consciousness makes a causal difference to behavior. So in theory, he could coherently have taken the sharp horn of the mechanist’s dilemma, but still agreed with Pflüger and Lewes that the spine is an organ of consciousness. But that is not what happened—Huxley also departed from Pflüger and Lewes in insisting that only the brain produces consciousness. Let us call this the “spinal consciousness dilemma.”

We must ask whether either dilemma could be resolved experimentally. Did anybody devise an experiment that would give us reason to choose one horn over the other, in either case? Could anybody devise such an experiment?

Let us take the spinal consciousness dilemma first. Huxley at least tried to ground his claim that the brain is the sole organ of consciousness in empirical evidence. He pointed out that in humans, total loss of consciousness results from serious injury to “the anterior division of the brain” (I assume he meant the frontal lobe of the cerebrum, though he may have meant the entire cerebrum).[[44]](#footnote-44) On that basis, he characterized the “brain” as the “seat” of human consciousness.[[45]](#footnote-45) He then insisted that for broadly evolutionary reasons, we should assume that what is true of humans is true of other vertebrates.[[46]](#footnote-46) For Huxley, the upshot (contra Pflüger and Lewes) is that we should assume that “a frog’s spinal cord is not likely to be conscious” since “a man’s is not.”[[47]](#footnote-47)

The ball was back in Lewes’s court, at least when it came to the question of a so-called “spinal soul.” Three years later, he responded by rejecting the notion that an organ can be found to be the “seat” of some vital process just in case the process ceases whenever the organ stops working. Such a view would require us to say something absurd, Lewes pointed out: “that the medulla oblongata was the ‘organ’ of Respiration, because Respiration ceases when this centre is destroyed.”[[48]](#footnote-48) Again, the medulla is in the brain stem. Successful respiration in vertebrates requires a medulla, but surely it requires lungs, blood, and countless other bodily structures as well. What is more, Huxley’s claim that a human becomes “unconscious” when her cerebrum is damaged does not even attempt to address Pflüger and Lewes’ suggestion that the spine might produce some separate consciousness of its own, a consciousness that (for all we can tell) might persist even when the brain’s consciousness lapses.

Huxley, in fact, saw the impasse, and actually acknowledged that Pflüger and Lewes’s position on spinal consciousness could not be defeated by any rational argument. But their position should obviously, Huxley contended, be regarded as absurd nevertheless:

It must indeed be admitted, that, if any one think fit to maintain that the spinal cord below the injury is conscious, but that it is cut off from any means of making its consciousness known to the other consciousness in the brain, there is no means of driving him from his position by logic. But assuredly there is no way of proving it, and in the matter of consciousness, if in anything, we may hold by the rule, ‘De non apparentibus et de non existentibus eadem est ratio’ [‘what does not appear and what does not exist have the same evidence’].[[49]](#footnote-49)

So even while seeming to offer experimental evidence that the brain alone is the organ of consciousness, in the next breath Huxley conceded that experiment could not settle this debate. Huxley simply pronounced it absurd to say that the spine has its own consciousness, claiming that such a consciousness could never be observed.

Notice, though, that Huxley’s pronouncement piggybacks on the position he took in the mechanist’s dilemma. His claim that spinal consciousness cannot not be observed amounts to the claim that such a consciousness cannot be observed first-personally. But that is the crux of the mechanist’s dilemma. Remember that he could not accept Pflüger and Lewes’s reasons for thinking the spinal frog conscious precisely because he (Huxley) was unwilling to accept a third-personally observable, behavioral mark of consciousness, such as purposive action. Thus the response Huxley in fact developed to the spinal consciousness dilemma depended on his response to the mechanist’s dilemma. And if that is right, then the basic disagreement between the two parties is actually over whether there is a behavioral mark of consciousness.

Here is William James offering a pessimistic assessment of the prospect of settling the disagreement via further experimentation:

If we start from the frog’s spinal cord …, saying, as that acts so intelligently, though unconscious, so the higher centres, though conscious, may have the intelligence they show quite as mechanically based; we are immediately met by the exact counter-argument from continuity, an argument actually urged by such writers as Pflüger and Lewes …. [Y]ou can either level up or level down by their means; and it is clear that such arguments as these can eat each other up to all eternity.[[50]](#footnote-50)

If one has the intuition that purposive behavior (say) is a reliable mark of phenomenal consciousness, then these experiments exert evidentiary pressure to ascribe consciousness not only to the intact frog but to the pithed frog as well. Instead, if one has the intuition that there is no reliable, third-person mark of consciousness, then one can easily avoid claiming that the pithed frog is conscious. But in that case one has just as little reason to think the apparently purposive behavior of the intact frog is any more controlled by consciousness than is its behavior after decapitation. James thinks the dispute cannot be settled by further experiment, and his reading seems reasonable.

Interestingly, the notion that the experimental study of consciousness cannot begin until one brutely stipulates a behavioral mark has resurfaced in the more recent literature. Here is David Chalmers:

Consciousness just is not the sort of thing that can be measured directly. What, then, do we do without a consciousness meter? … How does all this experimental research proceed? I think the answer is this: we get there with principles of interpretation, by which we interpret physical systems to judge the presence of consciousness. We might call these pre-experimental bridging principles. They are the criteria that we bring to bear in looking at systems to say (1) whether or not they are conscious now, and (2) which information they are conscious of, and which they are not. We cannot reach in directly and grab those experiences, so we rely on external criteria instead. That is a perfectly reasonable thing to do. But something interesting is going on. These principles of interpretation are not themselves experimentally determined or experimentally tested. In a sense they are pre-experimental assumptions.[[51]](#footnote-51)

One lesson of our 19th-century debate about pithed frogs is right in line with Chalmers’ suggestion here. Before we can say whether pithed frogs are conscious—before we can experimentally study consciousness at all, it seems—we must stipulate a third-personal accessible mark of consciousness. And no experiment can force us to use one mark or another, it seems. We are rationally free to choose whatever mark we find intuitively acceptable. We are free, that is, within some limits I now want to explore.

## 6. A De Facto Resolution

Critics might worry that I am overreaching when I claim that no experiment could ever show us whether there is a mark of conscious control of behavior, or if so what the “right” mark is that we should look for. After all, it is notoriously difficult to speculate about what would happen if we were to inquire indefinitely. But in the case of our historical dispute over pithed frogs, we have a record of how the dispute was in fact resolved. And despite a half century of experimentation, it was not an experiment that finally resolved the dispute, as I will now argue.

Recall that Pflüger published his original work in 1853, and we have traced the way the debate developed through Lewes’s response to Huxley in 1877. By that time, the stakes had grown considerably. If one goes back to the days of Marshall Hall, the question was how to account narrowly for reflex action. But Hall had conceived of reflexes as only one of four basic types of muscular action (the other three he called “voluntary,” “respiratory,” and “involuntary”).[[52]](#footnote-52) Pflüger had initially attacked Hall’s mechanistic account of reflex action, specifically.

But by the time of the Lewes contribution from 1877 that I’ve been discussing, the question was no longer whether this one subset of muscular action could be accounted for purely mechanistically. Now, the question had become whether the mechanistic approach to reflex action might be expanded to cover all muscular action. Thus, here is Lewes’s description of the situation in 1877:

The aim of Physiology is to ascertain the particular combinations of the elementary parts involved in each particular function—in a word, the mechanism of organic phenomena; and the modern Reflex Theory is an attempt to explain this mechanism on purely mechanical principles, without the co-operation of other principles, especially those of Sensation and Volition.[[53]](#footnote-53)

Notice that Lewes now construed what he called the “Reflex Theory” as an approach to physiology full stop—as an approach to explaining “the elementary parts involved in each particular function” without appealing to “Sensation and Volition.” And Lewes’s suggestion that the reflex theory had expansionist ambitions was not at all unique. James had characterized Huxley’s epiphenomenalism as “an inevitable consequence of the extension of the notion of reflex action to the higher nerve centres.”[[54]](#footnote-54)

The science of physiology was in a war with itself over what a genuine explanation should look like. A kerfuffle over beheaded frogs had turned into a controversy over the very shape a science of physiology should take.

But not long after, a clear victor emerged. “That the majority of physiological opinion by the close of the century was in favor of the position of Pflüger’s opponents seems certain. Mechanistic physiology and psychology was firmly seated in the saddle,” wrote Fearing in his classic history.[[55]](#footnote-55)

Indeed, the concept of a mechanistic reflex arc came to dominate not just physiology, but psychology too. The behaviorist B. F. Skinner, for example, wrote his 1930 doctoral dissertation on how to expand the account of reflex action to cover all behavior, even the behavior of healthy organisms.

Skinner once wrote that Bertrand Russell had “pointed out that the concept of the reflex in physiology had the same status as the concept of force in physics,” and the comment is apt.[[56]](#footnote-56) The reflex arc, mechanistically understood, had become something like an a priori presupposition of empirical research in both physiology and psychology.[[57]](#footnote-57) It was a priori in the sense that it could not be established by any direct experiment, as I have tried to show. What drove physiologists to Huxley’s side in this old debate seems to have been broad, programmatic concerns. Research in physiology and psychology that sought mechanistic, reflex-arc explanations just flourished. Through the innovations of people like Skinner and, before him, Pavlov, behaviorism established itself as the dominant research paradigm.

We get a hint of the programmatic nature of this drift towards Huxley from a 1911 textbook by the Harvard physiologist W. B. Cannon. Cannon had just given directions on how students can perform Pflüger’s old experiment on the pithed frog. But unlike the 19th century discussions we have talked about, Cannon’s treatment was not followed by any tortured rumination on whether the spine is an organ of consciousness, or on whether purposiveness is a mark of consciousness. Instead, we get this:

Purposive movements are not necessarily intended movements. It is probable that reaction directed with apparent purposefulness is in reality an automatic repetition of movements developed for certain effects in the previous experience of the intact animal.[[58]](#footnote-58)

That’s it. Cannon gave no real argument for why students should not regard purposive movement as a mark of genuine volition (beyond a quick gesture at Lotze’s long-discredited retort to Pflüger). Without citing any studies, experimental or otherwise, Cannon simply reported as established fact that purposiveness does not entail intentional action.

It was not established fact, as I have tried to show, but rather an established convention. By discarding the burden of a behavioral mark of conscious control, physiologists freed themselves up to model every bodily motion as a fully mechanistic transaction between stimulus and response, without any super-physical input from consciousness, sensation, or volition.

James nicely captured the fin-de-siècle optimism among mechanists:

The conception of reflex action is surely one of the best conquests of physiological theory; why not be radical with it? Why not say that just as the spinal cord is a machine with few reflexes, so the hemispheres are a machine with many, and that that is all the difference?

In their general drift, both physiology and (with the rise of behaviorism) psychology chose to “be radical with it” indeed.[[59]](#footnote-59)

## 7. On Experiment and Philosophy, Then

I began by characterizing the debate about decapitated frogs as involving incompatible philosophical intuitions. But where, one might wonder, is the philosophy in all of this?

To see what I have in mind, it is helpful to distinguish two broader issues that one might think are intertwined in the frog debate. On the one hand, there is the question of whether non-human vertebrates have what we now call “phenomenal consciousness.”[[60]](#footnote-60) Here the issue is whether there is “something it is like”[[61]](#footnote-61) for, say, a frog to swim out from under an enclosure, or whether animals are unconscious automata, as Descartes thought. On the other hand, there are questions about the nature of so-called cognitive control, or “the ability to coordinate thoughts and actions in relation with internal goals.”[[62]](#footnote-62) Everyone involved in our 19th-century debate accepts that vertebrates have phenomenally conscious mental states; and they all accept that vertebrates are typically capable of cognitive control. The question is whether phenomenal qualities themselves play a causal role in the control of behavior.

Contemporary psychologists may well have abandoned this hybrid question, but it is still alive today in philosophical debates about epiphenomenalism. As a leading proponent of this latter doctrine puts it, epiphenomenalism is the view that “[p]henomenal consciousness is inefficacious,”[[63]](#footnote-63) and not just for the specific purposes of cognitive control—inefficacious for producing any bodily changes of any kind. This is precisely the philosophical issue I take to have been up for debate in our 19th-century controversy.

But Huxley’s version of epiphenomenalism[[64]](#footnote-64) did not rest on any particular experimental result. It rested on a brute intuition, as we are now in a position to see. Again, if one has the intuition that choosing behavior (for example) is a reliable mark of conscious control, then these experiments exert evidentiary pressure to ascribe conscious control to intact and pithed frogs alike, as well as to healthy human beings. But then one has effectively adopted interactionism. Instead, if one has the intuition that there is no reliable, third-person mark of conscious control, then one has no evidence that the pithed frog’s behavior is consciously controlled. But in that case one has just as little evidence for thinking the behavior of intact frogs or even intact humans are controlled by consciousness. And then one has effectively adopted epiphenomenalism.

Epiphenomenalism and interactionism can “eat each other up to all eternity,” at least as Huxley and Lewes framed the issue, because even as physiologists amassed voluminous experimental results about pithed vertebrates, those results remained perfectly consistent with either set of intuitions. But it now seems that those intuitions are closer to methodological stipulations than to any independently contentful statement about how the world is. So is there a fact of the matter about whether epiphenomenalism or interactionism is actually right?

If my analysis is correct, then these positions come down to incompatible methodological stipulations that only masquerade as empirical claims. And such stipulations are not themselves truth-apt. They are conventions more typically judged on the basis of pragmatic considerations concerning the wider research programs they support. Indeed, it looks as though Huxleyan methodological mechanism in fact issued in a theoretical paradigm that was (for a good while) more empirically adequate with respect to the wider body of experimental results in physiology and psychology, on the whole, and perhaps was more fruitful, useful, and so on.[[65]](#footnote-65) The attention-grabbing philosophical claim—epiphenomenalism—got carried along as a kind of free-rider, and then only to the very limited extent that behaviorists wished to talk about philosophical questions about the mind at all.

The mechanistic paradigm flourished at least until behaviorism became moribund in the latter part of the 20th-century. And then, as we have just seen quite explicitly in Chalmers, one finds a different set of methodological stipulations (he calls them pre-experimental assumptions) supporting a new, interactionist science of consciousness.[[66]](#footnote-66) Thus epiphenomenalism and interactionism are both byproducts of a package of procedural assumptions needed to get different research projects off the ground. It is misleading to see them as making contentful claims that could be directly tested on their own.

## 8. On Experiment and Philosophy, Now

Finally, the role of experiment in philosophy has been much discussed lately because of the rise of so-called experimental philosophy, or x-phi.[[67]](#footnote-67) So it is fitting to close by contrasting their uses of experiment with those we find at play in our 19th century debate.

X-phi is a 21st-century movement united not just by practitioners’ use of empirical data to untangle philosophical knots, but by their use of experiment to study intuitions in particular.[[68]](#footnote-68) The rationale for this recent experimental work has a lot to do with the foundational place of intuitions in more traditional analytic philosophy. When we construct a philosophical argument, we must be willing to grant that some premises do not themselves need to be supported by further argument. That is, it seems we must accept that some premises are simply “intuitive” or (as I am using the word) obvious. Much of the x-phi literature today amounts to an empirical investigation into whether the intuitions of professional philosophers are actually shared by non-professionals. Sometimes researchers survey cross-cultural populations, and sometimes they simply survey a lay population in one region (often the researchers’ own corner of the United States).[[69]](#footnote-69)

To take one example relevant to this paper, consider a recent, x-phi response to the familiar “hard problem” of consciousness. As Chalmers originally conceived it, the hard problem takes off from the claim that every conscious state has its own phenomenal feel—again, that there is “something it is like” to see red, or hear a clarinet play a b♭, or feel hunger pangs, or whatever. Even if we could explain how the brain achieves each cognitive function—how it gathers color or auditory or bodily information, for example—there would still remain a “hard” question: “Why should physical processing give rise to a rich inner life at all?” as Chalmers puts it.[[70]](#footnote-70) After all, it seems conceivable (and therefore logically possible, for those who think conceivability entails possibility) that there could exist zombies, creatures physically indistinguishable from us who perform all the same functions we do, yet who lack any inner experience at all.[[71]](#footnote-71) Chalmers claims that the fact that we are not zombies therefore demands explanation.

Critics have asked why we should think conscious states all have a phenomenal feel. Chalmers acknowledges that the issue comes down to a brute intuition.[[72]](#footnote-72) He says that phenomenal feels are “the most central and manifest aspect of our mental lives.”[[73]](#footnote-73) In other words, it is just supposed to be obvious that our mental states all feel like something.

But some recent work in x-phi challenges the alleged obviousness of this claim. Sytsma and Machery administered a series of surveys to determine whether “ordinary people (viz. people without training in philosophy or in consciousness studies)”[[74]](#footnote-74) agree with most philosophers that conscious experiences all have a phenomenal feel in the relevant sense. And they found some striking evidence of divergence—ordinary people are apparently less inclined than philosophers to say that there must be some phenomenal feel to every mental state.[[75]](#footnote-75)

Whether or not one finds their line persuasive, notice the stark difference between the ways contemporary philosophers and our 19th-century figures use experiment. Contemporary philosophers are testing what we might call the intuitiveness of our intuitions. They want to establish (or more typically, refute) claims about the obviousness of some philosophical proposition by examining what people outside the bubble of professional western philosophy say about that proposition. In contrast, we have seen that the 19th-century figures in our story were not testing how widespread their own intuitions were.[[76]](#footnote-76) They were trying to produce direct, experimental evidence for claims they previously did not think required such evidence.

Unfortunately, the intuitions at issue in our 19th-century debate ultimately resisted this kind of verification, as I’ve said. But their attempted verification should teach us a lesson about how philosophy might make fruitful contact with experimental results.

Here is a suggestion for an alternative form that x-phi might take, today. Whatever one thinks of the role of intuitions in analytic philosophy, intuitions also play an important role in science in that they are often bound up with our choice of methodological stipulations (again, see Chalmers’s “pre-experimental assumptions”). I have only been looking at consciousness studies; but many other varieties of experimental research no doubt rely on pre-experimental assumptions about how to define central theoretical terms and tests as well. Philosophers interested in both experiment and intuition might go into business, so to speak, evaluating these kinds of pre-experimental assumptions we see at play in science.

The pressing questions would not be how popular this or that pre-experimental assumption shows itself to be in a survey. If I am right that these assumptions are not directly verifiable, we should be concerned with two other questions. First, does this pre-experimental assumption or that one support or inhibit some promising avenue of inquiry? And second, if scientists were to choose some particular set of pre-experimental assumptions, what would be the wider conceptual implications? Behaviorists did not care to look very closely at the philosophical implications of their work, but perhaps that oversight eventually contributed to their eclipse.

Huxley’s “On the Hypothesis that Animals are Automata” provides a remarkable example of the sort of alternative x-phi work I am proposing. Huxley saw clearly that physiologists would be led to epiphenomenalism if they responded to the Pflüger-Lewes experiments by abandoning the notion that there is a behavioral mark of consciousness. It took almost a century of behaviorist research in psychology for a creeping discomfort with this implied epiphenomenalism to break through and produce a new generation of researchers willing to take dualism seriously again. But we should be heading into this new world of so-called “consciousness science,” I suggest, with a clear awareness of the conceptual implications of our new pre-experimental assumptions. Philosophers are well placed to help us draw out those implications, but only if they are willing to examine those assumptions in the context of the concrete scientific research they stand to support.

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Gresham College

Barnard’s Inn Hall

Holborn

London

EC1N 2HH

[www.gresham.ac.uk](http://www.gresham.ac.uk)

1. This is a quotation from the “Amiable Tramp” character in H. G. Wells, *The Wonderful Visit* (New York: Macmillan and co., 1895) 139. [↑](#footnote-ref-1)
2. Chris Stokel-Walker, “The Chicken That Lived for 18 Months without a Head,” in *BBC News Magazine* (London: 2015). [↑](#footnote-ref-2)
3. Franklin Fearing, *Reflex Action: A Study in the History of Physiological Psychology* (New York: Hafner, 1930/1964) 10 – 11. [↑](#footnote-ref-3)
4. Ibid., 161. [↑](#footnote-ref-4)
5. Ibid., 166. Today pithing is an accepted way to euthanize frogs so they can be used for student dissection and such. But this procedure involves *double* pithing, where both the brain *and the spinal* *cord* are destroyed. The cases I discuss in this paper exclusively deal with the single-pithed frog—i.e., the frog whose brain has been destroyed (in whole or in part), but whose spinal cord has been left at least largely in tact. [↑](#footnote-ref-5)
6. By “mark” I do not mean either a necessary or a sufficient condition. To borrow an example from Russell, a card’s being in the catalog may be a *mark* of the book’s being on the library shelf in the sense in which I am using the term. But the card’s being in the catalog is neither necessary nor sufficient for the book’s being on the shelf. Nobody I shall be considering thinks that choosing behavior is either necessary or sufficient for consciousness. A fortiori, they do not think choosing behavior *constitutes* consciousness. The card can be an extremely useful guide to telling us whether a book is on the shelf, even if cards are sometimes mistakenly left in the catalog after the book has been checked out. Similarly, choosing behavior might be a reliable indicator of consciousness even if there are rare circumstances where such behavior exists in the absence of consciousness. The exceptions would have to be rare and, ideally, explainable. But at any rate, this is why I talk about the issue in terms of whether choosing behavior is a “mark” (rather than a necessary or a sufficient condition) of consciousness. [↑](#footnote-ref-6)
7. William James, *The Principles of Psychology*, ed. Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis (Cambridge: Harvard University Press, 1890/1981) 138. [↑](#footnote-ref-7)
8. Minsoo Kang, *Sublime Dreams of Living Machines: The Automaton in the European Imagination* (Cambridge: Harvard University Press, 2011) 104. [↑](#footnote-ref-8)
9. Although Vaucanson had claimed that his duck somehow broke the grain down and produced waste internally, later investigation in fact revealed that the mouth tube did not actually connect to the anus, and that the “excrement” was likely loaded separately for each performance; Jessica Riskin, “The Defecating Duck, or, the Ambiguous Origins of Artificial Life,” *Critical Inquiry* 29, no. 4 (2003): 609. [↑](#footnote-ref-9)
10. The duck became an important metaphor for materialists in particular, from La Mettrie to Lange; Julien Offray de La Mettrie, *Machine Man and Other Writings*, trans. Ann Thomson (Cambridge: Cambridge University Press, 1996) 34; Friedrich Albert Lange, *The History of Materialism and Criticism of Its Present Importance*, trans. Ernest Chester Thomas, 2d ed., 3 vols. (London: Trübner & Co., 1873-1875/1880) II.75. D’Alembert distanced himself from materialism, but the *Encyclopédie* in which the quoted passage on Vaucanson appears was publicly attacked for promoting this heretical view; Thomas L. Hankins, *Jean D'alembert: Science and the Enlightenment* (Oxford: Clarendon Press, 1970) 72. D’Alembert’s co-editor of that volume was Diderot, who was more unabashedly materialistic. [↑](#footnote-ref-10)
11. The decapitated frog literature I will discuss was also very much animated by Descartes’ conjecture. Thus it is not surprising that T. H. Huxley’s “On the Hypothesis that Animals Are Automata” dutifully trots out Vaucanson’s duck; see Thomas Henry Huxley, “On the Hypothesis That Animals Are Automata, and Its History,” in *Collected Essays: Method and Results* (New York: Appleton, 1874/1894), 230. It was not uncommon for 19th century physiologists to analogize reflex action to human-made automata. For instance, see William Benjamin Carpenter, *Principles of Comparative Physiology*, 4th ed. (London: Churchill, 1854) 673; William Benjamin Carpenter, *Principles of Mental Physiology: With Their Applications to the Training and Discipline of the Mind, and the Study of Its Morbid Conditions* (New York: Appleton, 1874) 100. [↑](#footnote-ref-11)
12. The division between mechanists and animists came out particularly sharply in an 18th-century dispute between Hoffmann and Stahl; *see* Lester S. King, “Stahl and Hoffmann: A Study in Eighteenth Century Animism,” *Journal of the History of Medicine and Allied Sciences* 19, no. 2 (1964). On Vaucanson’s automata as inspirations for La Mettrie, and on La Mettrie’s relationship to Descartes, see Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 87 – 88. And for general background on Hoffmann, Boerhaave, van Helmont, and Stahl, see Elizabeth Haigh, “Animism, Vitalism, and the Medical University of Montpellier,” *Medical History* Supplement, no. 4 (1984); King, “Stahl and Hoffmann: A Study in Eighteenth Century Animism,” ; Lester S. King, “Basic Concepts of Early 18th-Century Animism,” *American Journal of Psychiatry* 124, no. 6 (1967); Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, chs. 3 – 6. [↑](#footnote-ref-12)
13. The term “reflex” was first used by the French doctor Jean Astruc in 1736: “As with light, angles of incidence and reflection are equal, so that a sensation produced by a concussion of the animal spirits against the fibrous columns [of the spinal cord] is reflected and causes motion in those nerve tubes which happen to be placed directly in the line of reflection;” quoted in translation at Edwin Garrigues Boring, *A History of Experimental Psychology*, 2d ed. (New York: Appleton-Century-Crofts, 1929/1950) 35. [↑](#footnote-ref-13)
14. Hubert Steinke, *Irritating Experiments: Haller's Concept and the European Controversy on Irritability and Sensibility, 1750-90* (Amsterdam: Rodopi, 2005) 22. [↑](#footnote-ref-14)
15. Whytt’s relationship to animism is somewhat complicated. On one hand, Huxley treats Whytt as the “father” of modern animism because of the latter’s opposition to Albrecht von Haller; Thomas Henry Huxley, “Has a Frog a Soul? And of What Nature Is That Soul, Supposing It to Exist?,” *Papers Read before the Metaphysical Society*, no. Privately Published (1870): 4 – 5. What is more, Fearing sees a direct influence from Stahl in Whytt’s notion that a “sentient principle” controls or directs involuntary motions; Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 78. But others have contended that since Whytt does not make the nervous system “subservient” to consciousness, he is *not* properly grouped with Stahlian animism; see Julius Rocca, “William Cullen (1710-1790) and Robert Whytt (1714-1766) on the Nervous System,” in *Brain, Mind and Medicine: Essays in Eighteenth-Century Neuroscience*, ed. Harry A. Whitaker, C. U. M. Smith, and Stanley Finger (New York: Springer, 2007), 88 – 89. Still, even Rocca acknowledges that Whytt generally preserves a causal role for this non-material “sentient principle” (which Fearing calls nothing but “a convenient verbal substitute for an all-pervasive soul”) in physiological processes, and in that sense is very much an animist; Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 78. Also see footnote 19, below. In any case, Whytt’s positive influence on Hall is not in doubt; and for a discussion of Hall’s theoretical approach, *see* ibid., ch. 9. [↑](#footnote-ref-15)
16. William Kingdon Clifford, “Body and Mind,” in *Lectures and Essays, by the Late William Kingdon Clifford*, ed. Leslie Stephen and Frederick Pollock (London: Macmillan and co., 1874/1886), 251. [↑](#footnote-ref-16)
17. The book is Eduard Pflüger, *Die Sensorischen Functionen Des Rückenmarks Der Wirbelthiere, Nebst Einer Neuen Lehre Über Die Leitungsgesetze Der Reflexionen* (Berlin: Hirschwald, 1853), in which Whytt and Hall were both explicit targets. [↑](#footnote-ref-17)
18. See Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 162, though “purely” is perhaps too strong for Whytt. The latter insisted on a background role for a “sentient principle” even in reflex action, as at Robert Whytt, *An Essay on the Vital and Other Involuntary Motions of Animals* (Edinburgh: Hamilton, Balfour, and Neill, 1751) 117 – 18. But despite this, Whytt is typically taken to have laid the groundwork for Hall’s more fully mechanistic account of reflex action, as at Boring, *A History of Experimental Psychology*, 35 – 39, so much so that Hall was publically accused of failing properly to acknowledge his own reliance on the work of Whytt and other pioneers; see Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 136 – 39. [↑](#footnote-ref-18)
19. Pflüger himself apparently performed his experiments on fully decapitated (rather than merely pithed) frogs. See below, fn. 31. But later physiologists more typically worked on pithed frogs. To get a sense of the pithing procedure, one can consult most any laboratory manual for physiologists through at least the turn of the 20th century. Here is a typical example:

    *Pithing a Frog.* — Wrap a frog in a cloth and hold him head upward in the left hand. Press the front of the head downward with the left index finger, thus making a bend at the occipito-vertebral junction. Push the point of a pithing needle directly down into the depression between the skull and the first vertebra, and move the point quickly from side to side, thus cutting across the medulla. Push the needle forward into the skull cavity to destroy the brain…. Walter Bradford Cannon, *A Laboratory Course in Physiology*, Second ed. (Cambridge: Harvard University Press, 1911/1913) 4

    One might well find these procedures troubling in that experimentalists seem to be inflicting pain on sentient animals with little regard for their welfare. This work was indeed highly controversial in its own day. Amidst riotous debates in newspapers, numerous laws proposing to ban vivisection were brought to legislative bodies across the western world. Victorian-era vivisectionists were sometimes taken to court in attempt to stop the animal experimentation, such as in the case of Moritz Schiff in Italy; *see* Anonymous, “Dr. Schiff, of Florence, and the Vivisection Question,” *Medical Times and Gazette* 1875. One can get a sense of the uproar from William James’s various op-eds on vivisection. James first wrote in *Nation* about founder of the ASPCA Henry Bergh, who had been trying to get the state of New York to ban all animal experimentation completely; *see* Susan E. Lederer, *Subjected to Science: Human Experimentation in America before the Second World War* (Baltimore: Johns Hopkins University Press, 1995) 32. James responded that the entire science of physiology (or at any rate, the only secure knowledge that then existed in the field) was based on vivisectional evidence, so that to ban vivisection was to ban an entire science. In an uncharacteristically extreme passage, he even contended that this research was so valuable that a “dog strapped on a board and howling at his executioners” in a physiological experiment, if he could only understand the higher scientific good he was serving, “would religiously acquiesce in his own sacrifice.” William James, “Vivisection,” in *Essays, Comments, and Reviews*, ed. Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis (Cambridge, MA: Harvard University Press, 1875/1987), 11 – 12. James again weighed in on the issue the following year in *Nation* in response to a proposed ban in Britain on vivisection; *see* William James, “More on Vivisection,” in *Essays, Comments, and Reviews*, ed. Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis (Cambridge, MA: Harvard University Press, 1876/1987). But he softened his view towards the end of his life, as evidence by a final letter published in the *New York Evening Post,* where he came out in favor of sound regulation of vivisection; *see* William James, “On Vivisection,” in *Essays, Comments, and Reviews*, ed. Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis (Cambridge, MA: Harvard University Press, 1909/1987). [↑](#footnote-ref-19)
20. Pflüger’s original discussion can be found at Pflüger, *Die Sensorischen Functionen Des Rückenmarks Der Wirbelthiere, Nebst Einer Neuen Lehre Über Die Leitungsgesetze Der Reflexionen*, 16. Just a few examples of related descriptions can be found at J. S. Haldane, “Life and Mechanism,” *Mind* 9, no. 33 (1884): 40; James, *The Principles of Psychology*, 22; George Henry Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind* (London: Trübner & Co., 1877) 429 – 30. [↑](#footnote-ref-20)
21. Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 162. [↑](#footnote-ref-21)
22. E.g., “Consciousness [*Bewusstsein*] is life and becoming. Life is motion. The causes that underlie this life are determined, and yet could not be fundamental. They are a something that at one and the same time is done and is not done. This motion, what we call consciousness, is a part of the whole, this life a part of the whole life of the world.” Pflüger, *Die Sensorischen Functionen Des Rückenmarks Der Wirbelthiere, Nebst Einer Neuen Lehre Über Die Leitungsgesetze Der Reflexionen*, x – xi, my translation. [↑](#footnote-ref-22)
23. *See* Huxley, “Has a Frog a Soul? And of What Nature Is That Soul, Supposing It to Exist?,” 5 – 6. [↑](#footnote-ref-23)
24. Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 162 – 63. [↑](#footnote-ref-24)
25. Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 190. [↑](#footnote-ref-25)
26. Friedrich Goltz, perhaps the most skillful experimentalist of all the figures I am discussing, first published this incredible result in Friedrich Leopold Goltz, *Beiträge Zur Lehre Von Den Functionen Der Nervencentren Des Frosches* (Berlin: A. Hirschwald, 1869) 70. [↑](#footnote-ref-26)
27. This result is also due to Goltz, at ibid., 65. [↑](#footnote-ref-27)
28. Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 164. [↑](#footnote-ref-28)
29. Huxley, “Has a Frog a Soul? And of What Nature Is That Soul, Supposing It to Exist?,” 3. [↑](#footnote-ref-29)
30. E.g., throughout George Henry Lewes, “The Spinal Chord a Sensational and Volitional Centre,” in *Report of the Twenty-Eight Meeting of the British Association for the Advancement of Science; Held at Leeds in September of 1858* (London: John Murray, 1859); George Henry Lewes, “Sensation in the Spinal Cord,” *Nature* 9 (1873). Pflüger also frequently discusses the “beheaded” (*enthauptet*) or “decapitated” *(geköpfte*) frog Pflüger, *Die Sensorischen Functionen Des Rückenmarks Der Wirbelthiere, Nebst Einer Neuen Lehre Über Die Leitungsgesetze Der Reflexionen*, e.g. x, 14, 55, 128, but it should be noted that he is generally more careful than Lewes in identifying which brain structures he had destroyed in which experiments e.g., at ibid., e.g. 14 – 15, 18. Later on, Lewes would become much more careful to identify brain structures, especially in Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind* . Now in the English-speaking world, Lewes was the chief defender of Pflüger’s position, and it may be that the general vagueness in the early English-language literature about which brain structures were being destroyed in the “decapitated” animals is a symptom of Lewes’s influence on how this debate played out in the UK. [↑](#footnote-ref-30)
31. The diagram is from T. Lauder Brunton, *Lectures on the Action of Medicines: Being the Course of Lectures on Pharmacology and Therapeutics Delivered at St. Bartholomew's Hospital During the Summer Session of 1896* (London: Macmillan, 1898) 227. [↑](#footnote-ref-31)
32. Huxley, “Has a Frog a Soul? And of What Nature Is That Soul, Supposing It to Exist?,” 3 – 4, emphasis added. A treatment of the effects of successively destroying these structures in birds can be found at Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 160 – 62, though Lewes *interprets* the results differently than Huxley. [↑](#footnote-ref-32)
33. Even Pflüger’s indefatigable ally G. H. Lewes, who I will discuss below in more detail, accepted the importance of behavioral *complexity*: “With diminishing sensibility we see diminishing power of co-ordination of behavior,” he wrote at Lewes, “The Spinal Chord a Sensational and Volitional Centre,” 138. He went on to argue that such co-ordination *is* evident in the decapitated frog, although in this early article he did not identify the specific brain structures he had destroyed in “decapitating” his animals. [↑](#footnote-ref-33)
34. For instance, here is G. H. Lewes:

    When we find combined movements persisting after the cerebellum has been destroyed, we may be sure that the cerebellum is not the organ by which such combinations take place; and when we find sensation and volition manifested after the cerebrum has been removed, we may be sure that the cerebrum is not the organ for these sensations and volitions. … And this we do find. … If all the cranial centres as far as the medulla oblongata are removed from young rabbits, dogs, or cats, there are unmistakable evidences of Sensibility in their cries when their tails are pinched …. It is said indeed that the cries are not signs of pain; and this is probable; but they are assuredly signs of Sensibility. … The frog thus mutilated has lost indeed all its special senses, except touch, but it still breathes, struggles when grasped, thrusts aside the pincers which irritate it, or wipes away acid dropped on its skin. If the eye be lightly touched, the eyelid closes; if the touch be repeated three or four times, the foreleg is raised to push the irritant away; if still repeated, the head is turned aside; but however prolonged the irritation, the frog neither hops, nor crawls away, as he does when the cerebellum remains. Place the brainless frog on his back, and if the medulla oblongata remains he will at once regain the normal position; but if that part is absent he will lie helpless on his back. Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 164 – 65

    Note that Brunton’s diagram in my figure 3, above, is somewhat ambiguous about precisely which cut prevents the frog from jumping, and which prevents it from regaining normal posture after being placed on its back; so Brunton may perhaps have disagreed with Lewes on this latter point. Still, the Lewes passage offers a host of other examples of complex behaviors that persist even when a frog is pithed just above the medulla oblongata (or as he puts it, just below the cerebellum). [↑](#footnote-ref-34)
35. Inexplicably, Huxley himself acknowledged that the medulla-free frog performs this unusual behavior, and in the very same article where he had denied that frogs without a medulla cannot perform “complex” actions; Huxley, “Has a Frog a Soul? And of What Nature Is That Soul, Supposing It to Exist?,” 3. [↑](#footnote-ref-35)
36. William B. Rutherford, “Lectures on Experimental Physiology; Lecture Vi,” *The Lancet* 8 (1871): 397. I do not know whether others might have reported similar findings earlier than Rutherford. In any case, the result was presumably replicated, since widely-used physiology and psychology textbooks would come to incorporate this result; see James, *The Principles of Psychology*, 28; Cannon, *A Laboratory Course in Physiology*, 38 – 39. [↑](#footnote-ref-36)
37. Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 353 – 55. [↑](#footnote-ref-37)
38. Lewes, “The Spinal Chord a Sensational and Volitional Centre,” 136. [↑](#footnote-ref-38)
39. One can understand the theoretical exigencies of this move. As we just saw from the Huxley passage, hemisphere-less frogs lose the motivation to act on their own—they can hop and swim and the like, but only if prompted in some way by an outer stimulus. Lewes came to accept that in the case of these decerebrated frogs (birds as well), there is indeed a genuine loss of “spontaneity” in the sense of a loss of ability to at without sensory prompting; Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 160, 406. Since he was in the business of arguing that the spine produces sensation and volition on its own, he could not continue to accept spontaneity as in any way necessary for sensation or volition. He did not simply reject the spontaneity criterion because it was incompatible with his own view—he argued for his position by contending that there is no way to make the distinction between spontaneity and reflexivity sharp: “What are called the spontaneous actions are simply those which are prompted by internal, or by not recognisable stimuli; and could we see the process, we should see a neural change initiated by some stimulation, whether the change was conscious and volitional, or unconscious and automatic.” The passage continues with this example. If a dog “wags his tail at the sight of his master, or wags it when dreaming, the stimulation is said to be spontaneous; but if after his spinal cord has been divided the tail wags when his abdomen is tickled, the action is called reflex.” Ibid., 426. I take it him to be (rather elliptically) making several points. First, allphysical behaviors are the result of a nerve signal triggering a muscle contraction; but in point of fact, we do *not* reserve the label “spontaneous” only for the cases where we cannot trace that triggering nerve signal to an external stimulus. Take the example of a dog wagging his tail because he sees his master and the case of the dreaming dog wagging his tail. We call *both* behaviors “spontaneous,” Lewes thinks, even though in the first case we actually *can* connect the triggering nerve signal to an external stimulus. Second, Lewes is suggesting that we should be *less* inclined to call the sleeping dog’s tail-wagging a “spontaneous” act than the case of the dog greeting his master, even though the lack of external stimulus is much clearer in the sleeping dog case. Finally, Lewes is also suggesting that we have no physiological basis for distinguishing that first tail-wagging behavior from the case of the decapitated dog wagging his tail when his chest has been tickled; both actions are triggered by nerve signals, and both nerve signals can be correlated with a specific, external stimulus. So there is no good reason to call the one spontaneous and the other reflexive. Again, his point is that we cannot clearly distinguish reflexive from spontaneous actions. [↑](#footnote-ref-39)
40. Ibid., 427 – 30. [↑](#footnote-ref-40)
41. Lewes, “The Spinal Chord a Sensational and Volitional Centre,” 136. [↑](#footnote-ref-41)
42. Ibid., 137. [↑](#footnote-ref-42)
43. Huxley, “On the Hypothesis That Animals Are Automata, and Its History,” 240. [↑](#footnote-ref-43)
44. Ibid., 220. [↑](#footnote-ref-44)
45. Ibid., 205; Thomas Henry Huxley, *Lessons in Elementary Physiology*, 6th ed. (London: Macmillan and Co., 1872) 14. [↑](#footnote-ref-45)
46. Huxley, “On the Hypothesis That Animals Are Automata, and Its History,” 221, 36. [↑](#footnote-ref-46)
47. Ibid., 222. [↑](#footnote-ref-47)
48. Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 162. [↑](#footnote-ref-48)
49. Huxley, “On the Hypothesis That Animals Are Automata, and Its History,” 220. In legal contexts, where the Latin phrase is often used, it is standardly translated somewhat loosely as “what is not juridically presented cannot be judicially decided.” [↑](#footnote-ref-49)
50. James, *The Principles of Psychology*, 137 – 38. This is a lightly reworded version of a passage that James first published in William James, “Are We Automata?,” *Mind* 4, no. 13 (1879): 2. [↑](#footnote-ref-50)
51. David John Chalmers, *The Character of Consciousness* (Oxford: Oxford University Press, 2010) 91 – 92, my underline, original italics. [↑](#footnote-ref-51)
52. Marshall Hall, “On the Reflex Function of the Medulla Oblongata and Medulla Spinalis,” *Philosophical Transactions of the Royal Society of London* 123 (1833): 638. [↑](#footnote-ref-52)
53. Lewes, *Problems of Life and Mind, Second Series: The Physical Basis of Mind*, 354. [↑](#footnote-ref-53)
54. James, “Are We Automata?,” 2. [↑](#footnote-ref-54)
55. Fearing, *Reflex Action: A Study in the History of Physiological Psychology*, 185. [↑](#footnote-ref-55)
56. Skinner is quoted at Laurence D. Smith, *Behaviorism and Logical Positivism: A Reassessment of the Alliance* (Stanford: Stanford University Press, 1986) 264 – 65. [↑](#footnote-ref-56)
57. Elsewhere I have explored “presuppositions” in empirical psychology; *see* Alexander Klein, “*Divide Et Impera!* William James’s Pragmatist Tradition in the Philosophy of Science,” *Philosophical Topics* 36, no. 1 (2008). For a detailed account of the way in which the concept *force* functions as an a priori presupposition in Newtonian physics, *see* Michael Friedman, *Dynamics of Reason* (Stanford, CA: CSLI Publications, 2001). [↑](#footnote-ref-57)
58. Cannon, *A Laboratory Course in Physiology*, 38. Similar interpretations crop up even earlier, for instance in Austin Flint, *A Text-Book of Human Physiology*, Fourth ed. (New York: D. Appleton and company, 1888) 600. [↑](#footnote-ref-58)
59. I explore Huxley’s automatism along with James’s own account of consciousness, particularly as they both relate to the pithed frog experiments discussed in this paper, in Alexander Klein, “Consciousness as Caring: William James’s Evolutionary Hypothesis,” (Forthcoming). [↑](#footnote-ref-59)
60. The canonical account of what phenomenal consciousness amounts to is Ned Joel Block, “On a Confusion About a Function of Consciousness,” *Behavioral and Brain Sciences* 18, no. 2 (1995). [↑](#footnote-ref-60)
61. The phrase comes from Thomas Nagel, “What Is It Like to Be a Bat?,” *The Philosophical Review* 83, no. 4 (1974). [↑](#footnote-ref-61)
62. Etienne Koechlin, Chrystèle Ody, and Frédérique Kouneiher, “The Architecture of Cognitive Control in the Human Prefrontal Cortex,” *Science* 302, no. 5648 (2003). I thank Luca Barlassina for pushing me to distinguish these two issues. [↑](#footnote-ref-62)
63. William S. Robinson, *Understanding Phenomenal Consciousness* (Cambridge: Cambridge University Press, 2004) 159. [↑](#footnote-ref-63)
64. I discuss other versions of epiphenomenalism during the late 19th century (such as that of Shadworth Hodgson and the young William James) in Klein, “Consciousness as Caring: William James’s Evolutionary Hypothesis,” . [↑](#footnote-ref-64)
65. On the sort of pragmatic considerations about theory choice I have in mind, *see* Thomas S. Kuhn, “Objectivity, Value Judgment, and Theory Choice,” in *The Essential Tension: Selected Studies of Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977); Friedman, *Dynamics of Reason* ; Michael Friedman, “Kant, Kuhn, and the Rationality of Science,” *Philosophy of Science* 69 (2002). [↑](#footnote-ref-65)
66. Of course there are many other currents in late 20th-century psychology that departed from behaviorism, and consciousness science is hardly the most important or mainstream. [↑](#footnote-ref-66)
67. I use the term “x-phi” to designate the more recent movement exclusively, so that we can distinguish it from older traditions that used experiment differently in philosophy. [↑](#footnote-ref-67)
68. Justin Sytsma, “Introduction,” in *Advances in Experimental Philosophy of Mind*, ed. Justin Sytsma (New York: Bloomsbury Academic, 2014), 1. [↑](#footnote-ref-68)
69. One can consult numerous collections for an overview of this movement: for example, Joshua Knobe and Shaun Nichols, *Experimental Philosophy* (Oxford: Oxford University Press, 2008); Joshua Knobe and Shaun Nichols, *Experimental Philosophy*, vol. 2 (New York: Oxford University Press, 2014). And Justin Sytsma, *Advances in Experimental Philosophy of Mind* (New York: Bloomsbury Academic, 2014) is a kind of text-book introduction to the field. [↑](#footnote-ref-69)
70. David John Chalmers, “Facing up to the Problem of Consciousness,” *Journal of Consciousness Studies* 2, no. 3 (1995): 201. [↑](#footnote-ref-70)
71. David John Chalmers, *The Conscious Mind: In Search of a Fundamental Theory* (New York: Oxford University Press, 1996) 94 ff. [↑](#footnote-ref-71)
72. Ibid., xiii. [↑](#footnote-ref-72)
73. Chalmers, “Facing up to the Problem of Consciousness,” 206. [↑](#footnote-ref-73)
74. Justin Sytsma and Edouard Machery, “Two Conceptions of Subjective Experience,” *Philosophical Studies* 151, no. 2 (2010): 299. [↑](#footnote-ref-74)
75. For a series of responses and rejoinders to this work, *see* Brian Talbot, “The Irrelevance of Folk Intuitions to the “Hard Problem” of Consciousness,” *Consciousness and Cognition* 21, no. 2 (2012); Justin Sytsma and Edouard Machery, “On the Relevance of Folk Intuitions: A Commentary on Talbot,” *Consciousness and Cognition* 21, no. 2 (2012); Brian Talbot, “The Irrelevance of Dispositions and Difficulty to Intuitions About the “Hard Problem” of Consciousness: A Response to Sytsma, Machery, and Huebner,” *Consciousness and Cognition* 21, no. 2 (2012). [↑](#footnote-ref-75)
76. There is a more direct precedent for these current attempts to test how widely-shared various intuitions are about experience. I have in mind Francis Galton’s pioneering use of circulars, for example in Francis Galton, “Statistics of Mental Imagery,” *Mind* 5, no. 19 (1880). [↑](#footnote-ref-76)