

# From the Machine to the Ghost Within

## *Pavlov's Transition From Digestive Physiology to Conditional Reflexes*

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*I. P. Pavlov's transition from research on digestive physiology to investigations of conditional reflexes involved related departures from two firmly established traditions in his laboratory. One was conceptual: The standardized line of investigation that Pavlov had applied fruitfully to the gastric and pancreatic glands proved inapplicable to the salivary glands, leading him to reevaluate his approach to "psychic secretion." The dynamics and nature of this reevaluation owed much to a second departure, this one from a standard laboratory practice: Confronted with a conceptual problem that he recognized as psychological and, therefore, beyond his expertise, Pavlov recruited outside experts to help him resolve it, thus importing perspectives from contemporary psychology and psychiatry. The important role of insights from these two disciplines in the birth of research on conditional reflexes has been obscured by Pavlov's tale about this episode—a tale repeated uncritically by subsequent commentators. The intellectual terms of Pavlov's transition are evident in the phrase he chose to replace "psychic secretion"—"uslovnyi refleks." This term is commonly translated into English as "conditioned reflex," but its original meaning for Pavlov is better translated as "conditional reflex."*

Ivan Pavlov is best known today for his research on conditional reflexes, which captured the attention of physiologists, psychologists, and the general public, for whom he (and his salivating dogs) became a symbol of the power of experimental biology to explain, and perhaps even control, human behavior.

Relatively few people are aware that Pavlov won the Nobel Prize in 1904 for contributions to digestive physiology, and fewer still recognize in today's cultural icon the scientist who, in the 1890s, insisted that an idiosyncratic psyche played a central role in glandular responses to food. Pavlov's vigorous experimental argumentation for this point, however, was widely recognized by contemporaries as an important scientific contribution. In a 1901 report supporting Pavlov's candidacy for a Nobel Prize, Finnish physiologist Robert Tigerstedt emphasized the nominee's

assertion of the influence of the psychic moment upon the secretions of certain digestive glands. We have here an extremely obvious example of how organs that are definitely not under the influence of our will can also, in their activity, be rather closely dependent on our mental state—and we have thereby

acquired a new intimation of the close dependence in which mind and body stand in relation to one another. (Tigerstedt, 1901)

The transformation of Pavlov's research interests and approach to the psyche involved related departures from two firmly established traditions in his laboratory. One was conceptual: The standardized line of investigation that Pavlov had applied fruitfully to the gastric and pancreatic glands proved inapplicable to the salivary glands, leading him to reevaluate his approach to "psychic secretion." The dynamics and nature of this reevaluation owed much to a second departure, this one from a standard laboratory practice: Confronted with a conceptual problem that he recognized as psychological and, therefore, beyond his expertise, Pavlov recruited outside experts to help him resolve it, thus importing perspectives from contemporary psychology and psychiatry. The important role of insights from these two disciplines in the birth of research on conditional reflexes has been obscured, I think, by Pavlov's tale about this episode—a tale repeated uncritically by subsequent commentators.<sup>1</sup>

### **Pavlov's Laboratory and Research in the 1890s**

The terms of Pavlov's transition were set by the scientific and managerial style that he developed in the 1890s and by his laboratory's approach to "psychic secretion" in those years. Pavlov's scientific style—his notion of "good physiology" and his related practices—owed much to the French physiologist Claude Bernard. Like

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<sup>1</sup> For an exception, and a substantially different interpretation than the one I offer here, see Joravsky (1989, pp. 134–148).

Bernard, Pavlov viewed the organism as a purposeful, complex, specifically biological machine governed by deterministic relations. Physiology's task was to uncover these unvarying relations, to control experimentally, or otherwise to account for the "numberless factors" (Bernard, 1865/1957, p. 122) that concealed them behind a veil of apparent spontaneity (Bernard, 1865/1957; Pavlov, 1897/1951c, 1923/1951a). For Pavlov, the physiologist demonstrated conceptual control over these determined relations by expressing experimental results quantitatively and explaining the similarities and differences between them in terms of underlying regularities. He always insisted on results that were "pravil'nye," a Russian word that means both "regular" and "correct"—capturing his conviction that, in physiological experiments, these were one and the same. In this spirit, the centerpiece of Pavlov's *Lectures on the Work of the Principal Digestive Glands* (1897/1951c) is a series of "characteristic secretory curves" through which Pavlov described quantitatively the responses of the gastric, pancreatic, and salivary glands to various foodstuffs.

The great majority of the experiments referenced in this work were conducted by Pavlov's coworkers. These were usually physicians who, with only the most superficial knowledge of physiology, were seeking a quick doctoral degree to advance their career.<sup>2</sup> These physicians usually had a maximum of two years to choose a research topic, research it, write a doctoral thesis, and defend it. Pavlov incorporated them into a factory-like system that, essentially, used them as his own hands and eyes: He assigned them a specific topic, provided them with a suitably equipped dog technology, supervised (together with the laboratory's assistants and attendants) their research, interpreted their results, and closely edited their written products. In the years 1891–1904, about 100 of these coworkers passed through the laboratory; about 75% successfully completed their thesis and received their doctoral degree (Todes, 1997; Windholz, 1990).

Throughout the 1890s, Pavlov deployed these coworkers along standardized lines of investigation for each digestive gland. He first sought to establish nervous control over each gland; then to devise a dog technology for the precise quantitative measurement of its secretory products during normal digestion; then to establish the specific exciters for each gland; and, finally, to describe quantitatively the gland's secretory patterns. Laboratory research in these years concentrated on the gastric and pancreatic glands, because Pavlov considered these most important for digestion. The salivary glands received comparatively little attention until the latter part of the decade.

Beginning in 1894—and most famously in his *Lectures on the Work of the Principal Digestive Glands* (1897/1951c)—Pavlov described the digestive system as a "chemical factory." For him, the digestive glands responded purposefully, precisely, and regularly to different foods, producing secretions of the necessary quantity and proteolytic power for optimal digestion of an ingested foodstuff (Pavlov, 1897/1902, p. 2; 1897/1951c, p. 20).<sup>3</sup>

This digestive machine, however, was inhabited by a "ghost"—by the psyche and its capricious, highly individualized manifestations in the secretory responses of laboratory dogs. For Pavlov, digestive secretion normally occurred in two phases: The first, psychic phase was excited by appetite (through the vagus nerves) during the act of eating, and the second, nervous–chemical phase was excited by the products created when this initial "psychic secretion" acted on the food in the stomach. These products, in turn, excited the nervous mechanisms of the glands. Throughout the 1890s, Pavlov and his coworkers struggled experimentally and interpretively to separate these psychic and nervous–chemical phases and to define their different qualities.

Three points about these efforts are important here. First, the dogs used in "chronic experiments" lived much longer than those consumed in "acute experiments," enabling experimenters to identify in each a distinct personality (*lichnost'*) or character (*kharakter*). In 1896, for example, one coworker noted the following:

Dogs exhibit a great variety of characters, which it is well to observe in their relation to food and manner of eating. There are passionate dogs, especially young ones, who are easily excited by the sight of food and are easily subject to teasing; others, to the contrary, have great self-possession and respond with great restraint to teasing with food. Finally, with certain dogs it is as if they understand the deceit being perpetrated upon them and turn their back on the proffered food, apparently from a sense of insult. These dogs only react to food when it falls into their mouth. . . . Certain dogs are distinguished by a very suspicious or fearful character and only little-by-little adapt to the laboratory setting and the procedures performed upon them—it stands to reason that their depressed state does not facilitate the success of experiments. The age of dogs is also important in determining their character: the older the dog the more restrained and peaceful it is, and vice versa. (Lobasov, 1896, pp. 30–31)

Second, this assessment of a laboratory dog's psychological particularities played an important role in the interpretation of experimental results. As one coworker put it, "Professor Pavlov has many times told those working in his laboratory that knowledge of the individual qualities of the experimental dog has important significance for a correct understanding of many phenomena elicited by the experiment. During the conduct of our experiments we always kept this in view" (Kazanskii, 1901, p. 22). Judgments about these individual qualities shaped decisions about "good" and "bad" experiments and laboratory animals and, so, played an important role

<sup>2</sup> Russian medical students took only a single short course in physiology, and the nature and quality of this course varied widely among medical schools. In 1895, Pavlov became professor of physiology at St. Petersburg's Military–Medical Academy, and the course he delivered to medical students there was devoted almost entirely to digestive physiology and based largely on his own work. Even in this course, however, instruction was based entirely on lectures and did not involve laboratory exercises.

<sup>3</sup> All translations in this article are my own. Where available, I also give page references to the standard English-language translation.

in the shaping of much-varied experimental results into "characteristic secretory curves" (Todes, 1997).

Third, throughout the 1890s, Pavlov treated the psyche as a "black box." That is, although the psyche (and, therefore, the dogs' individual personalities) was a constant presence in digestive processes, and was constantly invoked in explanations of experimental results, Pavlov did not systematically address its nature or the mechanisms behind it. For his purposes, it was sufficient to separate "psychic" from "nervous-chemical" mechanisms and to use the features of each to interpret the secretory reactions of his dogs to various foods.

### **Pavlov, Vul'fson, and the Psyche's Choices**

By late 1896, laboratory research on the salivary glands had reached the point where, according to Pavlov's standardized path of investigation, the next step was to determine the specific excitants of salivation. He entrusted this task to S. G. Vul'fson, who fit the typical profile of a laboratory coworker. A physiologically untrained physician, Vul'fson required only a quick thesis to complete his requirements for a doctorate of medicine. Pavlov assigned him "to establish precisely the exciters of salivation and to confirm their specificity and purposiveness" (Vul'fson, 1898, p. 15). From March 1897 through February 1898, Vul'fson experimented on four dogs with various salivary fistulas, analyzing the quantity and quality of glandular reactions to a variety of edible and inedible substances.

As had the coworkers who worked previously on the gastric and pancreatic glands, Vul'fson identified a "strict purposefulness" in the work of the salivary glands. The salivary response to edible substances varied in quantity according to the food's dryness and was uniformly rich in mucin. The response to inedible substances, on the other hand, was uniformly low in mucin and varied little from one inedible substance to another. This, Vul'fson observed, made good "sense": Mucin served to lubricate a foodstuff for its passage down the digestive canal. Since the dog did not swallow inedible substances, but rather ejected them from its mouth, a watery saliva low in mucin was secreted to rinse out any remnants of the ejected substance remaining in the mouth (Vul'fson, 1898, pp. 53-55).

Vul'fson also discovered, however, a fundamental difference between salivary secretion and that of other glands: In the salivary glands,

psychic secretion is a complete reflection of the direct, purely physiological secretion, differing only in amount. . . . The very same results regarding quantity and quality of saliva that are acquired when substances come into direct contact with the roof of the mouth occur also when [these substances] are used for teasing alone. (Vul'fson, 1898, p. 56)

In other words, and in contrast to gastric and pancreatic secretion, the "psychic secretion" from the salivary glands was essentially identical to secretion during the

second, nervous-chemical phase of digestion. This "psychic secretion" manifested the same ability to differentiate among substances that, in the gastric and pancreatic glands, occurred only in the nervous-chemical phase of digestion, when, according to laboratory doctrine, the specific excitability of nerve endings generated various "characteristic secretory curves" for different foods.

For Vul'fson, the specificity (or "adaptation") that was evident in "psychic secretion" from the salivary glands reflected not only "emotion, but also an element of thought—a representation [predstavleniia] about the nature of the external substances falling into the roof of the mouth" (Vul'fson, 1897, p. 113). Since this "adaptation" of salivary reactions to specific substances was "almost entirely of a psychic nature" (Vul'fson, 1898, p. 53), the usual Pavlovian program for glandular physiology seemed to lead directly into the psychology of the salivary glands.

Vul'fson, then, ascribed to the psyche a property that the laboratory had previously reserved for the nervous system: the ability to distinguish between different substances and to generate an appropriate secretory response to each. "The task of the psyche," he wrote, is "to sort out" substances, "to divide" them into two groups—accepted and rejected substances—in order to respond to each appropriately. The psyche exhibited great "scrupulousness," an unerring "judgment of particular circumstances," and the ability to "generalize" (Vul'fson, 1898, pp. 43, 53, 56).

This discovery supported Pavlov's long-standing insistence on the importance of appetite and the psyche, and he enthusiastically endorsed Vul'fson's conclusions. Lauding Vul'fson's report (which he had closely edited) for demonstrating the "subtle and sharp adaptation of the salivary glands," Pavlov emphasized that in salivation "the participation of the psyche emerges clearly, so psychology almost entirely overshadows physiology." This "dominance of psychology" was clear from "the fact that appropriate types of saliva are secreted both when a tested substance is put into the mouth and when it is only used to tease the dog." He added, "If in other cases we speak in jest, metaphorically, about 'the mind' of the glands, then in this case we should understand the term 'the mind of the glands' literally" (Pavlov, 1898, pp. 458-459).

The qualities of this "mind," however, presented an obstacle to Pavlov's standardized investigatory path. For years, he and his coworkers had recognized the importance of the psyche but had simply black-boxed it. In analyses of the gastric glands, for example, a dog's initial secretory reaction to foodstuffs was simply attributed to the influence of appetite or the psyche. This "psychic secretion" gave way, in the second phase of the digestive process, to the specific nervous mechanisms that produced the "characteristic secretory curves" elicited by particular foods. Vul'fson's research, however, demonstrated the inapplicability of this scheme to the salivary glands. Here, "psychic secretion" was essentially identi-

cal to the nervous—chemical secretion. The purposeful, precise, and specific reactions of the salivary glands to different foods resulted, then, not from the specific excitability of the nervous system (as was the case with the gastric and pancreatic glands) but rather from the psyche's ability to, as Vul'fson put it, "sort out," "arrange," and "judge."

Recognizing that here "psychology almost entirely overshadows physiology," and conceding his own lack of expertise in this area, Pavlov turned, uncharacteristically, to an outside expert.

## Psychology to the Rescue: Snarskii Opens the Black Box

A. T. Snarskii was an atypical coworker. Holding both university and medical degrees, he entered Pavlov's laboratory to study a subject about which he possessed greater expertise than the chief. Snarskii had worked both in neurologist-psychiatrist V. M. Bekhterev's clinic for mental and nervous illnesses and, shortly thereafter, in the Alexander III Charity Home for the Mentally Ill (which was directed by Pavlov's longtime friend, A. V. Timofeev). Pavlov usually assigned an incoming coworker to an ongoing line of investigation without respect to that coworker's background. In 1900, however, and despite the fact that five other new coworkers arrived at the laboratory that year, he recruited Snarskii to study the "mind of the glands."

The doctoral thesis (Snarskii, 1901) that resulted was also atypical. Most important, unlike the vast majority of coworkers' theses, Snarskii's thesis made extensive use of scientific authorities from outside the laboratory: He cited physiologists who had addressed the biology of purposeful behavior (including Jacques Loeb and I. M. Sechenov), the Russian zoopsychologist V. A. Vagner, and a range of Russian and Western European psychologists (including G. I. Chelpanov, William James, and Wilhelm Wundt).

Snarskii mobilized these authorities to criticize Vul'fson's (and, implicitly, Pavlov's) conclusion that the psyche actively "chooses," "sorts out," "arranges," and "judges." By the standards of contemporary psychology, Snarskii insisted, Pavlov's "mind of the glands" did not deserve the word "mind." Like the contemporary psychologists whom he cited, Snarskii distinguished among a wide variety of mental qualities that involved a broad range of different capacities. He concluded that "psychic secretion" reflected not high-level processes such as will, choice, and judgment, but rather the relatively low-level process of "visual associations." Citing Wundt and other "authoritative teachers of psychology," Snarskii argued that "psychic secretion" resulted from "the simplest process that united new impressions with preceding ones: elementary memory." This process of recognition by means of "newly-established associations"—which Wundt termed "recognition anew"—was devoid of the higher rung psychological qualities that Vul'fson and Pavlov had attributed to it:

To conclude from this entirely elementary act that the dog makes a "choice" about what kind of saliva to secrete in the given case is to make an unfounded logical leap. Direct recognition does not rise even to [the level of] a free representation, to say nothing of the long chain of psychic acts—such as the formation of concepts, judgments and conclusions—that must precede a conscious choice and decision. (Snarskii, 1901, p. 9)

Snarskii offered a different explanation, which he developed through a polemic against Vul'fson's conclusions:

When the dog recognizes a previous irritant . . . it repeats a habitual reflex; but repeats it automatically, without any participation of conscious, active will. Schematically, this would be expressed as follows: a common reflexive arc is established between the direct irritant and the act of salivation. We can imagine that the centripetal end of this arc is . . . split, and therefore the very same salivary reflex can be received by the representations associated directly with the irritation. . . . This act is accomplished entirely stereotypically, automatically, through a well-trod path. The consciousness of the dog plays no "important" role; it "chooses" nothing and in itself does not "determine" the activity of the salivary glands. (Snarskii, 1901, pp. 9–10)

For Snarskii, then, the "psychic secretion" of the salivary glands was an "association" or "habitual reflex" that was accomplished in the subcortical region of the brain, entirely outside of the brain's conscious centers in the cortex (Snarskii, 1901, p. 50).

In Snarskii's person, contemporary psychology had challenged the lay, black-boxed notion of the psyche that had governed laboratory discourse throughout the 1890s. One could argue that Snarskii's approach to "psychic secretion" was both truer to contemporary trends in psychology and "more physiological" than that previously propounded by Vul'fson and Pavlov. Snarskii's new perspective did not, however, reveal a means by which "psychic secretion" could be addressed in a manner consistent with Pavlov's notion of "good physiology." The chief again looked outside his laboratory for "a person with whom one could go further" (Pavlov, 1926).

## A Key Analogy From Psychiatry: Tolochinov on the Eye and Knee Reflexes

Pavlov settled on a second atypical coworker, I. F. Tolochinov. Like Snarskii, Tolochinov was a veteran of Bekhterev's laboratory who worked at the Alexander III Charity Home for the Mentally Ill. Unlike Snarskii, Tolochinov had already received his doctoral degree for a thesis completed under Bekhterev on changes in the nerve fibers of the brain during paralytic imbecility (Tolochinov, 1900). From November 1901 until about April 1902, Tolochinov left his job at the Charity Home several afternoons a week to conduct experiments in Pavlov's laboratory.

In this article, I can only touch on some key aspects of Tolochinov's research. First, his initial trials were oriented toward acquiring consistent results with "psychic secretion" under varying conditions—in other words, toward making the "ghost" behave as regularly as the machine it inhabited. Tolochinov's first success, from

which Pavlov later dated the beginnings of research on conditional reflexes, occurred in February 1902, when he discovered the phenomenon that would later be termed "extinction." ("Extinction" was the term soon applied to the disappearance of a conditional response after the conditional stimulus had been repeated several times without repetition of the unconditional stimulus.)

Second, according to Tolochinov's later account, his interpretation of this phenomenon drew on his experience in psychiatry with the knee and eyelid reflexes. Tolochinov recalled that he had initially used the lexicon prevailing in the laboratory after Snarskii's work: "'representation,' 'association,' and so forth" (Tolochinov, 1912, p. 1278). Explaining his subsequent decision to abandon such psychological terms, Tolochinov wrote:

It had been noticed long ago that in several patients knee reflexes sometimes result, not only from the blow of a hammer, but even when this instrument is merely waved with the intention to strike the lig. patel. propr. . . . It is also remarkable that this phenomenon is to a certain degree involuntary; therefore it is most easily understood as a reflexive act from the brain cortex by means of waves of light, just as the reflexive response of the knee to a blow is the result of mechanical waves. This is the same type of phenomenon as the nictating reflex of the eyelid, which occurs, not only when the eyelid is touched, but also when any object, or the investigator's fingers, make a more or less rapid approach to the eye.

On these foundations I proposed that the phenomena of salivation during irritation of the dogs at a distance by foodstuffs be considered a *reflex* at a distance, which was accepted by *prof. I. P. Pavlov*, who termed it a *conditional reflex*, as distinct from the *unconditional reflex* received when the mucous membrane of the roof of the mouth is irritated directly by edible and inedible substances.

My conviction of the truth of this new view . . . was further strengthened by the circumstance that the salivary reflex elicited at a distance obeyed the same basic physiological law as the nictating reflex of the eyelid or the knee reflex elicited at a distance. That is, it obeyed the law of extinction or decline of the reflex, and, mainly, when certain conditions were observed, it was distinguished by an involuntary, fatal character. (pp. 1281-1282)

As had Snarskii, then, Tolochinov brought to Pavlov's laboratory professional experiences and expertise critical to the reevaluation of "psychic secretion" and foreign to Pavlov himself. For Tolochinov, the extinction of "psychic secretion" was reminiscent of his recent experiences in Bekhterev's clinic with the knee and eyelid reflexes. Bekhterev had devoted special attention to these reflexes and regularly demonstrated to physicians like Tolochinov the usefulness of the knee reflex as a diagnostic tool for nervous and mental diseases (see Bekhterev, 1896, 1901). Just as Snarskii had drawn on authorities in psychology to strip the "mind of the glands" of will and judgment, to portray "psychic secretion" as a simple "association" or "habitual reflex"; so did Tolochinov draw on clinical psychiatry to establish that "psychic secretion" behaved similarly to other "reflexes from a distance" that were "distinguished by an

involuntary, fatal character." This rendered it, at least in principle, accessible to physiological investigation.

## Pavlov's Difficult Transition

These developments did not in themselves determine Pavlov's decision to shift investigations from digestion to the psyche. The laboratory, after all, was constantly uncovering new phenomena and investigatory possibilities, many of which were never pursued. Pavlov was both an experimental physiologist and the manager of a large laboratory enterprise; so, his evaluation of this new line of research necessarily involved both scientific and managerial decisions. Given Pavlov's notion of "good physiology," the key scientific question was as follows: Could investigations of the psyche, like his research on digestion, generate precise, repeatable patterns that could be expressed quantitatively and interpreted according to their purposefulness? Given the institutional imperatives of his laboratory, the key managerial question was as follows: Could this line of research consistently generate fresh dissertation topics that could be satisfactorily completed by physiologically untrained physicians within two years? Only when Pavlov decided that both questions could be answered affirmatively did he shift the focus of laboratory research.

His decision was also influenced by a series of other considerations. First, the discovery of secretin by Bayliss and Starling (1902) undermined Pavlov's nervist portrayal of digestive processes, reopening and complicating questions that he had considered closed. As one coworker (Tsitovich, n.d.) recalled, this introduced a certain "dissonance" in the laboratory. As a theorist, Pavlov could accommodate himself, however reluctantly, to the existence of humoral mechanisms; but, as an experimentalist, he found this more difficult. Furthermore, Pavlov simply found nervous mechanisms more aesthetically pleasing and, throughout the 1890s, had consistently avoided topics that forced him to confront humoral ones. Second, developments in Russian psychology and psychiatry had normalized the previously controversial view that psychological phenomena might be explicable physiologically. By 1900-1901, Pavlov found himself taking a considerably "less physiological" position toward "psychic secretion" than did a number of his medical students, who "often asked: but can't this be explained as a reflex, just one from another sensory organ?" (Orbeli, 1967, p. 172). Studies of brain localization in Bekhterev's laboratory also portrayed psychic secretion as "nothing other than a reflex transmitted to the gastric glands through the central nervous system" (Gerver, 1900, p. 142; see also Bekhterev, 1902; Gorshkov, 1900). Finally, Pavlov had always been interested in the mysteries of the human mind and human behavior and had imbibed in the 1860s a positivist faith that a scientific understanding of these subjects was the surest path to improving human society.

The complexity of the scientific and managerial issues at stake made Pavlov's decision-making process slow and contradictory. As one longtime coworker later recalled, "Of course, I[van] P[etrovich] expected attacks

on his new child and suffered through a great series of doubts and vacillations" (Savich, 1924, p. 18). In July 1902, after Tolochinov delivered a short paper on his research to the Northern Congress of Physiologists in Helsingfors (i.e., Helsinki), Pavlov reportedly spoke animatedly to scientists in the corridor about the potential of this new line of investigation and about his plans to pursue it single-mindedly. Yet, the response was apparently "restrained" (Samoilov, 1925, p. 214), and Pavlov himself, in his lectures to medical students some months later, analyzed "psychic secretion" precisely as he had before Tolochinov's (and even Snarskii's) work. Explaining the dog's secretory reactions to teasing experiments, he noted that it "can think, desire, and express its feelings. It follows instructions, guesses, shows what is pleasant and unpleasant to it" (Pavlov, 1902-1903, p. 18). One searches in vain for a crucial experiment that convinced Pavlov that "psychic secretion" was a reflex or launched his new line of investigation.

We can, however, track Pavlov's transition through the scientific products of his laboratory, the changing pattern of coworker assignments, and his annual reports to his patron, Prince Ol'denburgskii. In February 1903, Pavlov's comments about one coworker's paper on the nerves of the salivary glands indicated a significant, public shift in his interpretation of "psychic secretion" (Pavlov, 1952b), and two months later he delivered his first public address on the new line of investigation to the XIV International Congress of Physiologists in Madrid, Spain (Pavlov, 1923/1951a, pp. 23-39). The pattern of coworker assignments highlights this same period: In 1902, only Tolochinov was assigned to research on "psychic secretion"; when he departed the following year, only one of five new coworkers was assigned to continue this work. It is significant, however, that in October 1903 Pavlov pulled a favorite coworker, Boris Babkin, off a developed investigation of the pancreas and assigned him instead to the new subject. In 1904, one of two new coworkers was assigned to the new line of investigation; in 1905, two of three; in 1906, three of four; and, in 1907, all new coworkers. Pavlov's annual reports to Ol'denburgskii fit the same basic chronology. He first mentioned the new line of investigation in his report of December 1903. He listed it last among the laboratory's research topics from 1903 to 1906 and as the only topic in his report of 1907 (Pavlov, 1903-1907).

## Are Conditional Reflexes Conditioned?

The conceptual dynamics of Pavlov's transition can be appreciated by considering the term that he chose to replace "psychic secretion"—"*uslovnyi refleks*," which has become known to English speakers as "*conditioned reflex*." The Russian phrase, however, can be translated as either "conditioned reflex" or "conditional reflex." The latter is much closer to Pavlov's original meaning. It is significant that in the French abstract of the report in which Tolochinov first used this term—an abstract edited by Pavlov—the term "*uslovnyi refleks*" is trans-

lated, not as "le reflex conditionné," but precisely as "le reflexe conditionnel" (Tolotschinoff, 1902).

What, exactly, did Pavlov mean by "conditional reflex" in the years that are covered here? Why did he use this term to replace Snarskii's "association, or habitual reflex" and Tolochinov's "reflex at a distance?" According to L. A. Orbeli, who worked in the laboratory from 1901 to 1917, Pavlov used the term "conditional reflexes" "in part because their very inclusion as reflexes then had for him a conditional character" (Orbeli, 1967, p. 172). This fits Pavlov's common usage of the word *uslovnyi*, which he employed as a synonym for "tentative" or "hypothetical" (see, e.g., Pavlov, 1901/1952a, p. 164). For Pavlov, I think, the term "conditional reflex" reflected not only whatever ontological reservations he may have had but, more important, the test that this potential new line of investigation had to pass to qualify as good physiology.

The promise and the peril of research on "psychic secretion" both resided in the apparent "conditionality" of the relationship between stimulus and response. On the one hand, this conditionality perhaps represented the animal's complex but determined adaptation to the subtlest change in its conditions—to changing signals about available food or an approaching predator (Pavlov, 1903/1951a, pp. 29-30; also in 1923/1928, p. 52). On the other hand, this conditionality might represent either the indeterminacy of psychological phenomena or a determinacy that is inaccessible to physiological methods. In either case, conditionality would deprive experiments on this subject of the determinedness that constituted the *sine qua non* of "good physiology." As Pavlov put the central question in 1903 (answering it, perhaps, with a bit more conviction that he actually felt),

The center of gravity in our subject lies, then, in this: is it possible to include all this apparent chaos of relations within certain bounds, to make these phenomena constant, to discover their rules and mechanism? It seems to me that the several examples which I shall now present give me the right to respond to these questions with a categorical "yes" and to find at the basis of all psychic experiments always the very same special reflex as a fundamental and most common mechanism. True, our experiment in physiological form always gives one and the very same result, excluding, of course, any extraordinary conditions—this is an unconditional reflex; the basic characteristic of the psychic experiment, on the other hand, is its inconstancy, its apparent capriciousness. Nevertheless, the result of the psychic experiment also recurs, otherwise we could not even speak about it. Consequently, the entire matter is only in the great number of conditions influencing the result of the psychic experiment as compared with the physiological experiment. This will be, then, a conditional reflex. (Pavlov, 1923/1951a, p. 30; also in Pavlov, 1923/1928, pp. 52-53)

For Pavlov, the "conditional reflex" was a suitable subject for physiological research only if it was, in the final analysis, a fully determined "conditioned reflex." As an experimentalist and laboratory manager, he defined the question operationally: "To what extent can regular, quantitative, determined results be acquired in the labora-

tory?" This is what made Tolochinov's discovery of extinction so important to Pavlov: It represented the first case in which conditional reflexes behaved in a quantifiably repeatable, orderly fashion. After his report in Helsinki, Tolochinov conducted a number of experiments—from August 1902 to April 1903—that gradually reinforced Pavlov's intuition that research on conditional reflexes could reveal "firm lawfulness, . . . constantly recurring facts" (Pavlov, 1923/1951a, p. 33). For example, conditional reflexes diminished and disappeared if conditional stimuli were repeated without repetition of the unconditional reflex on which they were based, they were renewed by a strong unconditional irritation, and they were stronger when an object irritated several sensory organs rather than just one. Babkin's (1904) research further buttressed Pavlov's growing confidence that the conditional reflex would, with sufficient research, prove to be fully determined.

Once Tolochinov's and Babkin's experiments had established a few basic, repeatable patterns, Pavlov could address the "conditional reflex" in precisely the same manner as he had addressed digestive physiology. Feeding the same dog the same quantity of the same food in two different experiments had never, after all, yielded exactly the same secretory results. The differences were explained by reference to the dog's personality, mood, and so forth, and varying results were thereby contained within "characteristic secretory curves." Similarly, differing results in two apparently identical experiments with conditional reflexes could be contained within a few basic patterns by invoking numerous uncontrolled variables (what Bernard had termed the "numberless factors" in any complex organic machine), allowing Pavlov to interpret the "conditional reflex" as a "conditioned reflex."<sup>4</sup>

Confidence in the ability of experiments on the conditional reflex to generate relatively regular, quantifiable results was also critical to Pavlov's concerns as manager of a large laboratory. Regardless of its scientific promise, the new line of investigation was only feasible if it could be pursued by the physiologically untrained physicians who performed the vast majority of experiments in his laboratory. Pavlov could not sit on the bench beside each of these coworkers, who usually numbered about 15 at a time. For him to adequately supervise their work and interpret their results—to exercise "quality control"—Pavlov required that experiments be of relatively simple design and, most important, that their results be expressible quantitatively. This was not only necessary to Pavlov's notion of good physiology, it also provided a simple language in which his coworkers could gather results and communicate them to the chief for final interpretation. Pavlov trusted numbers both as a reflection of physiological reality and as a managerial tool (Porter, 1995). By 1903–1904, he was confident that the investigation of conditional reflexes could generate reasonably regular, repeatable numbers in the same way as had investigations of digestive physiology over the previous 15 years and,

so, that it met the scientific and managerial criteria for a new focus of laboratory research.

## Pavlov's Tale

Pavlov related several times the story of his transition to research on conditional reflexes—always with the story line first presented in a speech of 1906 at a London ceremony honoring T. H. Huxley. Space limitations permit me to analyze only one aspect of this tale, which I present in the version that Pavlov offered in the preface to *Lectures on Conditioned Reflexes: Twenty-Five Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals* (1923/1928):

I began to investigate the question of this [psychic] secretion with my collaborators, Drs. Vul'fson and Snarskii. While Vul'fson collected new and important material regarding the details of the psychic excitation of the salivary glands, Snarskii undertook an analysis of the internal mechanism of this excitation from the subjective point of view; that is, considering the imagined internal world of the dogs (upon whom our experiments were conducted) by analogy with our own thoughts, feelings, and desires. There then occurred an event unprecedented in the laboratory. We differed sharply from each other in our interpretations of this world and could not by any further experiments come to agreement on any general conclusion, despite the laboratory's consistent practice by which new experiments undertaken by mutual agreement usually resolved any disagreements and arguments.

Dr. Snarskii held to his subjective explanation of the phenomena, but I, struck by the fantastic nature and barrenness for science of such an approach to the problem, began to seek another exit from this difficult position. After persistent deliberation, after a difficult intellectual struggle, I decided, finally, in the face of the so-called psychic excitation, to remain in the role of a pure physiologist, that is, of an objective external observer and experimenter, dealing exclusively with external phenomena and their relations. For implementation of this decision I also began with a new co-worker, Dr. I. F. Tolochinov, and there subsequently followed twenty years of work with the participation of many tens of my dear coworkers. (Pavlov, 1923/1928, pp. 38–39)

According to Pavlov, a distant influence from his youth gave him the courage to address psychological phenomena "objectively":

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<sup>4</sup> In my opinion, the nature of this interpretive process—in which a constantly increasing number of new variables and laws was invoked to contain a constantly increasing amount of varying data within a mechanistic framework—was the main reason that Pavlov, for all his public confidence, suffered privately, at least through the mid-1920s, from grave doubts about his research on conditional reflexes. For example, in 1926—after some 25 years of experimental work—he oscillated between confession and self-congratulation in comments to a small gathering of coworkers:

I must thank you for all your work, for the mass of collected facts—for having superbly subdued this beast of doubt. And now—when the book [*Lectures on the Work of the Large Hemispheres of the Brain*, 1926/1951b] is appearing—now, I hope, this beast will retreat from me. And my greatest gratitude for liberating me from torment is to you. On the other hand, you have taken part in the creation of a new chapter of science. For this I congratulate you. (Pavlov, 1926)

I think that . . . the most important impetus for my decision, although at the time an unconscious one, was the influence, from the long distant years of my youth, of the talented brochure of Ivan Mikhailovich Sechenov, the father of Russian physiology, entitled *Reflexes of the Brain* (1863/1866). You know, the influence of an idea that is powerful by virtue of its novelty and truthfulness to reality, especially in one's younger years, is so profound, so enduring, and, one must add again, often concealed. In this brochure, a brilliant attempt was made—a truly extraordinary attempt for that time (of course theoretically, in the form of a physiological scheme) to represent our subjective world in a purely physiological manner. (Pavlov, 1923/1928, p. 39; 1923/1951a, p. 14)

The reader, I hope, has noticed that Pavlov's version of his conflict with Snarskii corresponds neither to the content of Snarskii's thesis nor to Pavlov's actual position on "psychic secretion" in the years immediately before and after Snarskii's research. Pavlov and Snarskii clearly differed about something, and perhaps their disagreement indeed concerned differing estimations of contemporary psychology. As the reader has seen, however, far from "holding to a subjective explanation of the phenomena," Snarskii was the first laboratory coworker to insist that "psychic secretion" was "an association" or "habitual reflex" and that "the consciousness of the dog plays no important role." Furthermore, he developed this idea in a polemic against Vul'fson's view that the psyche actively "chooses" and "judges"—a view that Pavlov had enthusiastically endorsed and continued to propound in his lectures through at least the fall of 1902. As for the "unconscious" influence of Sechenov's *Reflexes of the Brain*—this tract, published in 1863 and unmentioned by Pavlov until his tale of 1906, was cited, for the first time in any laboratory publication, by Snarskii in his doctoral thesis.

Why—if my own account of the transition is correct—would Pavlov have told such a tale? This is especially puzzling because he was usually scrupulous, even generous, in crediting coworkers for their contributions. I suggest two reasons. First, in his tale, Pavlov cast himself as a committed struggler for the scientific worldview in the spirit of Darwin, Huxley, and other such heroes. Snarskii (as the subjective psychologist) served him well here as a villain. Second, Pavlov's tale established a reputable physiological paternity for a line of research that he regarded, in Savich's words, as a vulnerable "child." Dismissed by many as speculative and ridiculed by others as "spitting science," the study of conditional reflexes was a risky endeavor for a basically conservative man who treasured the respect of his colleagues. In Pavlov's tale, this line of investigation was born through a combination of Vul'fson's and Tolochinov's experiments, the conceptual influence of "the father of Russian physiology," and his own courage and faith in the scientific worldview. It was, in other words, respectably modern and objective; it was "good physiology," untainted by influences from psychology, a discipline that had been associated in Pavlov's formative intellectual years with barren, reactionary metaphysics. In this same spirit, as

Pavlov embraced his new line of investigation with growing enthusiasm and confidence, he redefined it in increasingly physiological terms. In his annual reports of 1903–1905 he termed it "the study of questions of experimental psychology on animals," in 1906 he renamed it "the objective investigation of the higher divisions of the central nervous system," and in 1907 finally settled upon "the investigation of the activity of the large hemispheres and sense organs" (Pavlov, 1903–1907).

Pavlov's tale, then, was a part of this changing lexicon, this redefinition and legitimization of his new endeavor. In this sense, it was a final step in his transition to research on conditional reflexes.

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