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Pavlov and the Equivalence of Associability in Classical Conditioning

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The discovery of selective associability of cues in classical (Pavlovian) conditioning has often been treated as an embarrassment to Pavlov, because he has been represented as a proponent of the "equivalence of associability of cues." According to that doctrine, except for the influence of differences in stimulus intensity, all environmental stimuli are equally susceptible to becoming conditioned stimuli (CSs) if they are arranged in a suitable time-relation to any effective unconditioned stimulus (US). The current paper asks whether Pavlov explicitly made such a claim and, if not, whether he *could* have endorsed equivalence of associability. Scientific controversy, the role that "the classics" play in scientific specialties, and the emblematic standing of the founding figures of a discipline or specialty constitute a framework for discussion of Pavlov's stand on the equivalence of associability.

Keywords: Pavlov, history of psychology, scientific controversy

Pavlov's influence on American psychology has been acknowledged by many writers, though it has been characterized in different ways (e.g., Boring, 1950, p. 637; Bower and Hilgard, 1981, pp. 71–72; Coleman, 1988; Joravsky, 1989; Malone, 1990, pp. 57–59; Skinner, 1966; Windholz, 1997). "Influence" is not a simple property, however, and historically important figures such as Pavlov play a variety of roles in the ongoing work of the enterprises that they initiated. One of these roles will be of concern in this paper: because some of the founder's claims, findings, and methods have become authoritative, with or without independent confirmation by subsequent researchers, they are used as a basis for predicting the results that a given research project *ought* to yield, if the "traditional ideas" are valid. A research outcome that challenges such

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canonical expectations is likely to be regarded as more significant — or at least as meriting closer examination and discussion — than findings that are merely consistent with expectation. If the finding has broader, significant implications, then the finding is regarded as even more important.

Given the above use of Pavlovian ideas, it is not surprising that a manuscript submitted to *Science* for publication by Garcia and Koelling elicited very close (and skeptical) attention from reviewers. When it was eventually published in a less prestigious journal in 1966, it generated considerable interest among investigators in the psychology of learning. In that publication, Garcia and Koelling (1966) reported their findings in a study of cue-to-outcome associations. They presented experimental evidence that audiovisual cues (clicks and light-flashes) are more readily associated with a subsequent noxious exteroceptive US (grid-shock) than gustatory cues (novel taste) are; and that gustatory cues are more readily associated with subsequent illness/malaise (induced, in separate groups of laboratory rats, either by X-irradiation or by injection of lithium chloride) than exteroceptive cues are. Their brief report — typical of publications in *Psychonomic Science* — was limited to describing their findings, which demonstrated that "stimuli are selected as cues dependent upon the nature of the subsequent reinforcer" (p. 123). In their short discussion, they simply pointed to the plausible role of natural selection in endowing rats with a food-avoidance mechanism for selecting among available cues those (i.e., gustatory) that have natural "relevance" to an aversive outcome (i.e., illness) that followed ingestion. Pavlov was not cited in Garcia and Koelling's report, but implications concerning him could be drawn from their results, and these implications — together with others that we will identify — contributed to a "biological constraints movement" that the current paper examines.

Preparedness, Biological Constraints, and the Behavior Theory Enterprise

In the late 1960s, there developed a small and unintegrated literature on selective associability and various other anomalies in animal conditioning and learning, as investigators reported a variety of classical and instrumental learning phenomena that could be summarized as follows: organisms find certain Pavlovian cue–outcome and instrumental response–outcome contingencies easy to learn and other contingencies more difficult to learn, as Garcia and Koelling (1966) had demonstrated. It was soon found that species appear to differ in regard to which contingencies are easy and which are hard to learn (e.g., Wilcoxon, Dragoin, and Kral, 1971). Unexpectedly, the "misbehavior" described by the Brelands at the beginning of the 1960s began to look like an important finding rather than like an isolated anomaly among otherwise successful applications of reinforcement theory (Breland and Breland, 1961).

In an influential article published in the *Psychological Review*, Seligman (1970) provided a synthesis of this growing collection of seemingly related findings, and Bolles (1970) followed up in a later issue of the same journal with a related interpretation of puzzling results in avoidance-conditioning research. In his article, Seligman proposed that a laboratory animal comes to an investigator's experimental setup as a member of a species whose evolutionary history has equipped it with an apparently innate, and possibly species-specific, repertoire for handling encounters with important elements of its natural habitat, elements that resemble the significant stimuli it encounters in the laboratory. That repertoire predisposes it to learn — with ease or with difficulty — the particular CS–US contingency that the investigator has arbitrarily arranged to examine classical conditioning, or the behavior that the investigator has arbitrarily selected for an instrumental-conditioning study. According to Seligman, the rate of conditioning reflects the ease or difficulty of such learning and, therefore, is an appropriate indicator of the degree of "preparedness" of that animal species for the learning task devised in a research project. He proposed that the chief determinant of the degree of preparedness is the degree of congruence between the situational arrangement/requirement and the organism's species-typical predispositions to react to the stimuli arranged by the investigator or to emit the required instrumental behavior.

If "preparedness" had been left as an open, investigatable, empirical matter to be settled, on a case-by-case basis, by exploration of the learning of classical and instrumental contingencies in a range of rather different species, preparedness would have had no more significance than questions about other important parameters of classical and instrumental conditioning. But Seligman (1970) used his modest evidence of species differences in ease of association formation (i.e., "preparedness") to draw implications in matters of greater generality and importance. From his literature review, he concluded: (a) that behavioral regularities ("laws") demonstrated in particular combinations of species and experimental setups have less generalizability than had been widely assumed in the behavior theory enterprise; (b) that the widely practiced strategy of examining the conditioned behavior of a few arbitrarily chosen species performing under arbitrarily arranged conditions does not yield *general* lawful regularities in conditioning phenomena; and (c) that theories employing those data in formulating and testing hypotheses about presumptively general mechanisms rely on a strategy that is subverted by the phenomenon of preparedness. Consequently, the "general process approach" in such theorizing is a mistake. According to Seligman (1970), preparedness undermines the general process strategy in the psychology of learning, and the Garcia effect (selective associability of stimuli) served as a "classic" example of such preparedness.

Evidence of a heightened interest in preparedness — as well as interest in species differences in preparedness — during the period after Garcia and Koelling's (1966) article appeared in a flurry of publications, literature surveys (e.g., Shettleworth, 1972), conferences (e.g., Hinde and Stevenson–Hinde, 1973), and compilations of relevant papers on the subject (e.g., Seligman and Hager, 1972). Participants and observers began to take notice of a historical trend from the late 1960s through the 1970s, for which "the Garcia effect" was a major, though not the only, impetus. Earlier steps in this so-called *biological constraints movement* were retrospectively noticed as far back as Thorndike's (1911/1965) failed attempts to develop conditioned grooming in cats by means of a positive reinforcement contingency involving food. During the behavioristic 1940s and 1950s, interest in species differences was marginal and was carried on by European ethologists (e.g., Tinbergen, Lorenz, and others) and by a handful of psychologists in the American psychology of animal behavior and instinct (e.g., Frank Beach). By the early 1980s, this presumptive biological constraints trend had grown to a point that warranted an assessment of its fruitfulness (Domjan and Galef, 1983). Among the recent outcomes of this movement are contributions to current interest in evolutionary psychology, in the behavior-systems approach to conditioning theory, and in other "nativistic" projects.

The Biological Constraints Movement in a Larger Context

The biological constraints movement — though it was concerned with findings obtained in the animal learning laboratory — was influenced by developments in other learned disciplines and in the broader American cultural and political life in the 1970s and 1980s. The influence of cultural trends and conflicts can be conveyed by the theatrical metaphor of a stage on which the biological constraints movement is represented by an actor who is surrounded — on this conceptual and ideological stage — by other actors representing various contemporaneous movements in the learned disciplines as well as in American cultural life. The placements of the actors and their respective distances could be used to symbolize the degree of their congruence with the biological constraints movement in the period of the late 1960s to the1980s.

During that period, this hypothetical stage was cluttered, busy, and volatile. The actor closest to the representative of the biological constraints movement would represent the philosophy of science, to which behavioral psychologists had paid much attention since the 1930s (e.g., Stevens, 1939). During the 1960s, the philosophy of science was itself undergoing a great internal change, best summed up as a repudiation of the logical positivist theory of science, which, in the period of the 1930s through the early 1960s, had dominated thinking about the methods and the certainty of scientific knowledge claims in most of the sciences, including psychology. An informative, contemporary overview of the career of logical positivism can be found in a post-mortem survey by two philosophers of science (Achinstein and Barker, 1969). For scientific psychology as a whole, a relevant period-piece describing viable options appeared in *American Psychologist* (Gholson and Barker, 1985).

The focal point of that philosophical revolution was Thomas Kuhn's *Structure of Scientific Revolutions* (1962). There were a few predecessors, such as Norwood Hanson (Hanson, 1958) and Michael Polanyi (Polanyi, 1958), and there were successors who developed far more radical complaints against the logical positivist creed (e.g., Feyerabend, 1975), but Kuhn's work received the greatest amount of attention. Kuhn's (1962) "psychology of science" proposed that ordinary scientific work is carried out not by a prototypical scientist working alone and exhibiting heroic commitment to objectivity, rationality, and disinterestedness — as stipulated in the positivist program — but rather by a network of scientists, guided by a shared, quasi-religious commitment to a worldview ("paradigm") that their education had inculcated and which strongly affected their contact with scientific evidence.

That emphasis on social factors in Kuhn's model was amplified further in the sociology of science, represented by an actor who is more distant, elsewhere on the stage: the actor's script might describe "the strong programme" (e.g., Bloor, 1976), which further developed the institutional "big science" (Price, 1961) picture of typical natural-science team-research as an alternative to the heroic single investigator of natural science in the 1700s. The strong programme pressed the epistemological claim that scientific judgments are shaped more by the "social interests" of a research group than by "the facts." Another voice on the stage, constructivism, extended this critique into epistemological territory, and postmodernism elaborated it even further under an epistemologically skeptical theme. A related kind of skepticism would be apparent in the deliverances of another actor on this stage, presenting a critique of scientific institutions as promoters of the special interests of a "scientific establishment."

The presumptive validity of such complaints about American institutions would be supported by the speech of an actor situated at the back of the stage, in the messy "political" area. In the period that followed the decade of the 1960s, conflicts arose in a variety of loosely related issues in American culture and came to be identified as "the culture wars": the Vietnam War; the abortion issue; affirmative action; feminism and discrepancies in employment; consumers' rights; environmentalism; politically active Christianity; the adequacy of American schooling, particularly at the secondary level; pornography and American morality; immigration issues; and so on (e.g., Hunter, 1991; Shor, 1986; Zimmerman, 2002). Acrimonious debate regarding American institutions contributed to the tumult of criticism on the hypothetical stage I have described.

The humanities — especially literary criticism and history — were more deeply convulsed by these developments than were the social sciences. In psychology, the social science specialties were more strongly influenced than were the experimental psychology specialties. The natural sciences were the most resistant to these conflicts. These voices from American culture provided a supportive rationale for the moral outrage of the biological constraints movement, which positioned itself against a putatively traditional mainstream said to be stubbornly committed to doctrines — including equivalence of associability — that various then-current research findings (esp. Garcia and Koelling, 1966) contradicted. Garcia's work was particularly significant in this regard, because the rejection of the Garcia–Koelling manuscript submitted to *Science* — and the rejection of papers he subsequently submitted to *Science* and to American Psychological Association journals — was treated by some as the result of abuse of the quality-control function of the journal review process (Lubek and Apfelbaum, 1987; Mahoney, 1976; Revusky, 1977; see also Garcia's [1981] own, divergent account; see also Garcia, 2003).

That subject — that is, the influence of cultural dissent upon scientific enterprises — although it is interesting in its own right and served as a cultural context of the biological constraints movement, is only a background to the discipline-specific purpose of the current paper: to examine the attribution to Pavlov of an endorsement of the equivalence of associability in conditioning. In the current paper an attempt is made to understand the role that such attribution plays in scientific controversies, and the conditions that affect the accuracy of such attributions. The reader should keep in mind that among those influential conditions are the voices of heterodoxy, distrust, and accusation on the conceptual and ideological stage that is sketched above.

Analysis of the Issue

Clarifying Equivalence of Associability

The preceding account has shown that the publication by Garcia and Koelling (1966) had a substantial impact on the psychology of learning and suggests that it contributed to a growing awareness of social factors in science. The role of their "paradigm experiment" in Seligman's (1970) article on preparedness has already been noted. In that article, Seligman identified a number of findings from classical and instrumental conditioning which contradicted the orthodox doctrine that he called the "equivalence of associability"of CSs and USs. Concerning this doctrine, he offered the following characterization in the second paragraph of p. 407: "The basic premise can be stated specifically: In classical conditioning . . . any CS and US can be associated with approximately equal facility . . . . I call this the assumption of equivalence of associability" (Seligman, 1970, p. 407). The shorter label, "equipotentiality," was employed in the book edited by Seligman and Hager (1972), and it came to be the preferred label in the literature (e.g., Domjan, 1997), but I will retain "equivalence of associability" in order to separate the subject of this paper from other topics that have been covered by the label, "equipotentiality" (e.g., localization of function in the cerebral hemispheres).

Equivalence of associability expresses two distinct claims regarding classical conditioning:

(1) that all environmental stimuli *can* become CSs; that is, they can all "get conditioned";

(2) that all environmental stimuli are *equally* effective as they become CSs, or require an equal amount of training to become effective CSs: that is, they get conditioned with equal "ease." In order to distinguish these claims, I will refer to the first as "weak equivalence of associability" and the second as "strong equivalence of associability," with the phrases shortened for the sake of fluency.

To document his contention that important theorists, including Pavlov, espoused equivalence of associability (as an undifferentiated claim), Seligman relied upon a succession of different quotes that expressed such equivalence (undifferentiated). The first quotation took Pavlov's (1927/1960) assertion that "signalling [sic] stimuli can get linked up with any of the inborn reflexes" (p. 17). Although Seligman's second quotation was from Pavlov's (1928) *Lectures*, a nearly identical statement occurs in Pavlov's (1927/1960) more familiar treatise, his *Conditioned Reflexes*: "Any agent in nature which acts on any adequate receptor apparatus of an organism can be made into a conditioned stimulus for that organism" (p. 38; see also p. 73). In his list of three quotes, Seligman (1970) followed up with W.K. Estes's (1959) statement: "All stimulus elements are equally likely to be sampled and . . . on any acquisition trial all stimulus elements sampled by the organism become connected to the response reinforced on that trial" (p. 399). Having pointed to the evidence marshaled by Seligman, I will go on to consider whether Pavlov did endorse or could have endorsed the notion of strong equivalence of cues in conditioning.

Did Pavlov Explicitly Endorse Equivalence of Associability?

It is apparent from the surrounding text in *Conditioned Reflexes* (1927/1960, pp. 38–43) that Pavlov's statement concerning "any agent in nature" served as a justification of his use of artificial stimuli such as lights and tuning forks. Four considerations seem to have been operative in his statement, and his exposition appears to have been intended to handle them.

(1) Because of their artificial character, stimuli such as the ticking of a metronome might be more difficult for a nonhuman organism to associate with food or other significant events than for a human to do the same. Pavlov's "any agent in nature" statement answers that possibility by serving as an empirical generalization summarizing his demonstrations of effective conditioning with a variety of artificial CSs (pp. 38–39). I have referred to such a generalization as "weak equivalence." Pavlov's findings warranted further studies employing artificial stimuli, which offered the advantage of control over the major conditioning parameters: Pavlov was ever mindful of the desirability of controlling environmental conditions, as is evident throughout his *Conditioned Reflexes*.

(2) Although laboratory CRs to artificial stimuli can be established, they might nonetheless be just a research oddity having nothing in common with natural food-related behavior. To counter that possibility, it would be necessary to show that natural food signals — qualities that are regularly found to evoke interest, approach, salivation, etc., such as the sight and smell of the dog's standard food — actually are learned signals, that is, natural CSs. In the laboratory of a Prof. Vartanov, the required demonstration was carried out by a Dr. Zitovich, and Pavlov (1927/1960, pp. 22–23) cited their finding that the sight and smell of food do not elicit salivation unless they have accompanied consumption of the food (i.e., have developed this capability through conditioning). The demonstration provided support for believing that the phenomenon of conditioning that is demonstrated under the unnatural conditions of the laboratory also occurs in the natural life of organisms.

Consequently, the use of artificial stimuli might allow the investigator to model in the laboratory the organism's ontogenetic conditioning experience to "natural CSs" (e.g., Pavlov, 1927/1960, p. 50). That would require that Pavlov show that learned reactions to signals in the natural environment and CRs to artificial CSs in the laboratory display similar properties. This he did rather casually (see p. 22) by pointing to similarities of response latency and magnitude in the two cases (also pp. 49–50). These examples make it apparent that Pavlov's concern was to remove the laboratory dog from its extra-laboratory conditioning history and to examine, under circumstances closely controlled by the researcher, the process of conditioning to stimuli with which the dog could not have had prior acquaintance: thus his use of artificial CSs, such as clicking metronomes, that were "hitherto in no way related to food" (p. 26).

(3) Pavlov's lengthiest treatment of the range of conditionable stimuli is introduced by a short passage in which he provides an "amplification and restriction" of his "general statement" concerning "any agent in nature" (Pavlov, 1927/1960, p. 38). What sounds like a claim of weak equivalence of cues introduces a six-page exposition in which he illustrates the impressively

great variety of stimuli that, alone or in arbitrary combination, can be made effective as CSs (pp. 38–43). The passage is virtually a celebration of the capabilities of the dog's cerebral cortex to develop "internal relations" that correspond closely to the external relations that the experimenter arranges between arbitrarily selected properties of signals and effective USs (pp. 13–15). What might appear to be an assertion about the equivalence of different stimuli functioning as CSs looks more like a testimonial to the capabilities of the canine brain.

(4) The above considerations would have indicated to Pavlov that there are few limitations upon the range of stimuli that can become CSs (Pavlov, 1927/1960, pp. 29–31) and that conditioned reflexes can even be conditioned to a great range of unnatural stimuli: such information demonstrates that the brain of the dog is up to the paradigmatic environmental challenge to this "complex animal system," namely to "establish dynamic equilibrium with the environment . . . [by] reaction to signals presented by innumerable stimuli of interchangeable signification" (p. 15). Pavlov's exposition clearly places dogs among the higher animals (p. 1), and thereby strengthens the extension of his principles to human learning of signal relations.

By providing the context of exposition for Pavlov's statements about the range of conditionable cues, I have elucidated what is the meaning of the quotes taken from Pavlov by Seligman (1970). By contrast, when brief quotations, such as those used by Seligman, are removed from their expository context, they can be treated as evidence for theoretical commitments that were not endorsed by Pavlov. (For an illuminating clarification of the misuses to which familiar quotes from the writings of J.B. Watson have been put, see Todd, 1994, pp. 97–101; for generalization of the problem of selectivity in quoting, see Todd and Morris, 1992).

The quotation from W.K. Estes (1959) — which expresses strong equivalence of associability — will receive only brief consideration, because remarks made above in regard to Pavlov also apply to the quotation from Estes; and because the current paper is concerned with the position of Pavlov, not Estes, concerning equivalence of conditionability. Although it is true that Estes made the attributed statement, (a) it is a statement made in exposition of a theory of stimulus–response associative learning; and (b) it is preceded by Estes's proviso that "as a first approximation it is assumed that . . ." (1959, p. 399). The omitted expression indicates that Estes made the assertion "all stimulus elements are equally likely . . ." as a simplifying assertion in developing a theory and not as a summary or extension of reported empirical findings. Again, the surrounding text provides a different meaning to the quoted passage than the meaning that Seligman drew when he used the passages from Pavlov and Estes to show that equivalence of associability is "not a straw man" (Seligman, 1970, p. 407).

But Could Pavlov Have Embraced Equivalence of Associability?

Supplying a larger context also provides a basis for estimating whether an individual *could* have endorsed a hypothesis or theory (such as equivalence of associability) on which that person took no published position. The larger context can be provided by writings other than those in the immediate vicinity of the chosen quotes (i.e., those expressing weak equivalence, in the case of Pavlov). Estimation is an uncertain venture, but the quantity and distribution of available evidence increase confidence in this practice.

Perusal of widely separated portions of *Conditioned Reflexes* finds Pavlov waxing enthusiastic over the capabilities of the dog's cerebral cortex, as I have already noted. In the Dover reprint, illustrative passages can be found at many places in the text. Many such passages express Pavlov's astonishment at the sensory acuity of the dog. For instance, there is Pavlov's claim that "the cerebral hemispheres were sensitive to far finer gradations of stimulus than we could furnish" (1927/1960, p. 21). Throughout his treatise, Pavlov speaks of the adaptive value of signalization learning as consisting of "the establishment of the most complicated and delicate correlations between the organism and its environment" (p. 152). The superlative form of the adjectives is Pavlov's means of expressing a high estimation of the effectiveness of signalization learning as an adaptation to the complex challenges in the interaction between the dog — a representative higher animal — and its environment. For example, there is his characterization of the dog's cerebral cortex as "a signalizing apparatus of tremendous complexity and of most exquisite sensitivity" (p. 19). An alert reader can hardly fail to notice the sentiment of wonder expressed by Pavlov's emphatic, possibly extravagant, language.

In addition, although his experimental arrangements necessarily employed a radical simplification of the stimulating conditions to which the dog was exposed (e.g., pp. 20–21), Pavlov's appreciation of the role of cerebral processes is a highly generalized one: the brain is the vehicle by which the complex interaction of the dog and its environment is made to be an "exact correspondence" (p. 7) that undergoes continual adjustment from moment to moment, as circumstances change. Finally, Pavlov's admiration includes the further notion that such brain processes exhibit coherence from occasion to occasion, and that this regularity is distinctive to each dog and depends on the dog's nervous system "type" (e.g., pp. 284–289; also pp. 45–46, 51, 182–184). The synthetic concept of "type" required that Pavlov recognize similarities or regularities in a given dog's behavior in and out of the conditioning situation. The notion of nervous type is very closely related to the concept of human "character-types": indeed, Pavlov borrowed terms from the "ancient classification of the so-called temperaments" (p. 286) for the purpose of naming these regularities. It is obvious that Pavlov's ideas about cerebral mechanisms in mammals reflect his appreciation of the pervasive role these mechanisms have in managing the complexity of a higher animal's existence.

This characterization of Pavlov's "philosophy of the organism" makes it possible to apply a criterion of consistency to the question whether Pavlov *could* have advocated the notion of strong equivalence of cues in signalization learning. Strong equivalence implies that an environmental arrangement of cue and consequence is omnipotent, in the sense that the mere overlap of a stimulus with an effective US guarantees effective conditioning. That claim is clearly at the "environmentalist" extreme on a continuum from nature to nurture. However, Pavlov was not a simple-minded environmentalist, but an evolutionist very familiar with the works of Herbert Spencer, who was his favorite writer, at least for a time (Babkin, 1949, pp. 32, 35), and such intellectual commitments are evident from the first page of *Conditioned Reflexes*.

A laboratory dog brings to the conditioning apparatus not only the nervous system mechanisms of its species, which — in Spencer's optimistic, progressive version of evolution — have been improved over the eons, but also idiosyncratic variations that reflect the nervous system type of each individual dog. Even a dog's momentary state of alertness is an important factor in the effectiveness of the stimulus arrangement for conditioning (e.g., Pavlov, 1927/1960, p. 28). Clearly, the individual dog carries such factors into episodes wherein the process of signalization learning is manifested, and is not a "passive recipient" of the undifferentiated influences of an omnipotent environment. Proceeding from such an outlook, Pavlov would surely not have expected to find that all cues are equally effective in signal learning, even though he had found that many natural and artificial environmental stimuli could indeed come to serve as CSs. Accordingly, his treatment emphasizes the capability of the dog's brain to make any environmental stimulus a CS, if the conditions are suitable. He certainly was aware of such exceptions as overshadowing, in which the more intense component of a complex conditioned stimulus can make the less intense component less effective or completely ineffective (e.g., pp. 141–143).

On the other hand, it is important to note that Pavlov's *Conditioned Reflexes* does not provide any basis for thinking he would have *expected* that the effectiveness of a cue depends on the type of US that it signals (i.e., selective associability, the "Garcia effect"). After all, he employed both appetitive (food) and aversive (oral squirt of dilute acid) USs, and he treated these functionally distinct USs as interchangeable for the purpose of demonstrating signalization phenomena (e.g., pp. 26–27, 51–58, 61). The fact that these appetitive and aversive USs involved the same motor system — salivation — probably contributed to his tendency to regard them as equivalent for the purpose of exposition. Such considerations provide the basis for an admittedly speculative deduction that, if he were posed the question, Pavlov would have expressed doubt regarding the strong version of equivalence of associability, although he expressed the weak version of that claim, for reasons that I have presented.

Did Pavlov Present Laboratory Results Contrary to Strong Equivalence?

The above arguments made use of material that could be considered peripheral (and therefore, of doubtful relevance) to Pavlov's scientific beliefs; that is, I used writings that are indicative of Pavlov's metatheoretical convictions and expressive of Pavlov's quasi-religious sentiments concerning signalization in the higher animals. Nevertheless, Pavlov did report specific laboratory findings that show that he would not have endorsed strong equivalence of associability.

When Pavlov reported that different categories of stimuli were consistently nonequivalent in regard to demonstrated laboratory phenomena, he implicitly rejected strong equivalence of associability. For instance, when reporting in Lecture 15 that continuation of reinforcement at the same CS–US interval led to progressive delay of the CR and to its eventual disappearance from the interval, he noted that laboratory stimuli exhibit a characteristic order in the amount of training required to bring about this disappearance of the CR: increasing amounts of training were required for thermal, tactile, visual, and auditory CSs (Pavlov, 1927/1960, p. 235), an obvious contradiction of the principle of strong equivalence of associability. A similar ordering was found in the readiness of conditioned stimuli to induce drowsiness and sleep in the conditioned dog (p. 253). Because the topic of inhibitory conditioning, as well as Pavlov's work on sleep, was neglected in American research on classical conditioning in the period up to the late 1960s (see Rescorla, 1969), the above findings could have been overlooked or regarded as unimportant by American researchers. Pavlov's demonstration of overshadowing is quite a bit more familiar to American psychologists, and his findings therein display *the same ordering* of thermal, tactile, visual, and auditory stimuli (Pavlov, 1927/1960, p. 60). The passages identified above show that, although Pavlov held that most cues *can* be made into effective CSs, he did not claim that different classes of cues have the *same* potential for conditioning. That is, he endorsed weak equivalence but not strong equivalence of associability: he could hardly be regarded as an apostle of the full, undifferentiated version of the equivalence of associability of cues in classical conditioning.

Documenting the Impact of Misrepresenting Pavlov

Was the Misrepresentation Passed on to Textbooks?

The misrepresentation of Pavlov's position on the later issue of equivalence of associability may have influenced professional discussion of biological constraints. Some of that influence could, with much effort, be documented from journal articles, correspondence, and so on. A related, more convenient approach was provided by a sample of 89 textbooks in the psychology of learning published from 1952–1995 (Coleman, Fanelli, and Gedeon, 2000), which spanned the period of growth of the biological constraints movement. Textbook authors publishing from 1972 to 1995 would have had the opportunity to cite and use the articles by the Brelands (1961), Garcia and Koelling (1966), and Seligman (1970), and their textbooks provide a measure of the visibility of those articles and an indication of whether the misrepresentation of Pavlov was influential in at least this portion of the psychology of learning literature.

Seventy-three textbooks in the sample were published in the 24-year period from 1972 to 1995. Of these textbooks, 78.1% cited the Brelands' 1961 article, 74% cited Garcia and Koelling (1966), and 70% cited Seligman (1970). Of the textbooks that cited Garcia and Koelling, 85.2% also cited Seligman's 1970 article. Citation rates were greater for all three publications in the second half of the 24-year period (mean rate of 87.1%) than in the first half (mean rate of 64.3%), suggesting that the topic of biological constraints gained visibility in the textbooks published from 1972 to 1995.

Of greater interest is the question whether Pavlov was represented as a proponent of claims that were falsified by conditioned taste-aversion research. This question was approached by examining the textbooks that cited Garcia and Koelling (1966) for indications that their findings were declared by the textbook authors to be contrary to claims advanced by Pavlov. In twelve of the 54 texts (i.e., 22.2%), the demonstration of selective associability ("Garcia effect") was presented as contrary to Pavlov's position. Of the 12 books, all but two were published in the 1984–1995 portion of the period examined, the exceptions being the texts by Houston (1976) and Ellis, Bennett, Daniel, and Rickert (1979).

Textbooks that cited Garcia and Koelling (1966) — but did not claim that the Garcia effect constitutes an embarrassment for Pavlov — followed one of three strategies. Some texts presented the Garcia effect in a neutral fashion as merely showing the impact of evolution on species' learning mechanisms (e.g., Domjan and Burkhard, 1986; Klein, 1991), or simply as evidence for a principle of preparedness (e.g., Catania, 1984; Wickelgren, 1977). A second option was to present the Garcia effect as contradicting an impersonal principle such as "basic learning principles" (Tarpy, 1975, p. 276), or "general process assumptions" (e.g., Adams, 1980; Hintzman, 1978; Schwartz, 1978), or "equivalence of associability" (Fantino and Logan, 1979, p. 346). A third stance involved stigmatizing someone other than Pavlov, for instance, B.F. Skinner (Borger and Seaborne, 1982), or a collectivity, such as "traditional learning theory" (Lutz, 1994), or simply unnamed researchers who had investigated classical conditioning but had inquired only into quantitative relations of CS and US and, as a result, neglected qualitative relations (Schwartz and Reisberg, 1991). A common practice was simply to identity something "traditional" (such as traditional learning theory) as the source of error.

Clearly, the misattribution of strong equivalence of associability to Pavlov had some influence on the textbook literature of the psychology of learning, though textbooks exhibited quite a variety of ways of handling the Garcia effect, as the above description indicates. By far the most common method was to present the Garcia effect in a noncontroversial and merely descriptive manner, one that echoes the nonpartisan style of presentation that is commonly adopted by textbooks (e.g., Catania, 1984; Chance, 1979; Hall, 1982; Klein, 1991). One could say that, in comparison to the professional-journal literature of the same period, the textbook literature adopted an expository style favoring description of results and avoiding a judgmental, critical, polemical style and advocacy of partisan interests. After all, the audience to which these publications are directed is a student audience, without prior exposure to the subject and, as a result, without technical convictions that authors have to refute. Nonetheless, almost a quarter of the texts did identify Pavlov as an eminent figure who was embarrassed by the discovery of the Garcia effect.

Interim Summary

The foregoing considerations support the idea that shortcomings of scholarship played a role in formulating and attributing a doctrine of (undifferentiated) equivalence of associability to a "behavior-theory establishment" that was committed to Pavlov's presumptive claims in the period from the late 1960s through the 1970s, when a biological constraints movement positioned itself as a party of opposition. Specifically, I have pointed to instances in which the professional literature displayed two kinds of shortcoming: (a) that which results from quoting out of context, and (b) that which reflects a very limited use of Pavlov's *Conditioned Reflexes*.

Quoting out of context is a familiar complaint in political and legal debate, and it is regarded not just as an inaccuracy, but as a type of dishonesty motivated by a partisan stance on a divisive issue. Aspects of this ethical matter in the Garcia effect controversy have been insightfully handled by Revusky (1977). The subject of partisan scientific interests and of their effect on scholarship has been treated more generally by others (e.g., Mahoney, 1976). Earlier in this article, I employed a theatrical metaphor to characterize the ideologically overheated environment in which the biological constraints movement developed. In the closing section of this paper, I would like to concentrate on some implications that can be extracted from the second shortcoming, namely, the superficial use that was made of Pavlov's *Conditioned Reflexes* in the debate.

The Ordinariness of Misrepresentation

Kinds of Reading

Reading any publication involves a complex set of cognitive operations that vary along dimensions of effortfulness, frequency of use, and other factors that contribute to the outcomes of reading (e.g., Robinson, 1970). It may be useful to fold all of the resultant capabilities into a single continuum that could simply be called subject mastery. The degree of subject mastery that a given way of reading typically produces can be empirically specified by the gradable range of activities in which the reader can competently engage after completing an episode of reading the publication. Practices that engender outcomes at the lower end of that range of mastery are such activities as the following: the quick scan, reading passively, reading only a very small amount of the publication, and reading while distracted, very tired, or sedated. Those ways of reading permit the reader to recall later only a small amount of the content and to produce very little elaboration of the kind that might be required, for instance, to answer a lengthy essay question in a college-course final examination.

Among the practices that engender outcomes at the other end of the continuum is an open-minded, *sympathetic reading* of all (or most) of the publication while alert and after requisite preparation for such reading. It involves accepting — at least provisionally — the author's explicit statements, with intent to understand the viewpoint that the author's message expresses. Prior familiarity with writings by the same author is an effective preparation for this kind of reading. Sympathetic reading can enable the reader to paraphrase the primary theme of the publication and to identify major and minor components of the author's conceptual system. Also among the results of sympathetic reading is the ability to imagine how the author *might* have responded to a problem or question on which no explicit position was taken in the publication. Attaining such competence requires, not just a suspension of partisan suspicion and doubt, but a willingness to explore the outlook of the author attentively and without reservation.

Sympathetic Reading and the Normal Scientific Work of Specialists

Sympathetic reading takes a lot of time and effort. These are resources that a discipline or specialty encourages, channels, and rewards its practitioners for applying to important disciplinary issues and questions. Nonetheless, there are differences among the learned disciplines in regard to where this resource is directed. Sympathetic reading is a much-used tool in disciplines in which canonical works continue to be examined carefully and subjected to current tools of interpretation: in the study of the history, the philosophy, and the literature and arts of different societies, civilizations, or historical periods — in other words, the humanities in toto — those works that have attained inclusion in a canon are periodically given sympathetic readings, because they are the primary objects of interest and, therefore, merit close study in developing new interpretations and then appraising them. By contrast, in scientific specialties, a canon can hardly be said to exist because of the rapid obsolescence of scientific literature (e.g., Price, 1970).

Moreover, work in the sciences, particularly in the material sciences, displays a branching pattern: overall disciplinary growth is accompanied by the emergence of specialties that — while remaining within the parent discipline — pursue subjects, phenomena, interests, and problems that are different enough from the parent issues and subjects to be developed in a semi-autonomous fashion. Branching development creates interests, topics, methods and findings that are distinct, in greater or lesser degree, from the disciplinary origins from which the specialty has evolved (e.g., Toulmin, 1972, pp. 399–404; Ziman, 1987).

A consequence of specialization is that well-developed specialties emerge if they can mobilize an intensive application of resources to highly circumscribed examinations. That characteristic is part of Kuhn's (1963) analysis of the "secret" of the progress of the sciences, and I take that model of microscopically intensified exploration for granted in the current discussion. If Kuhn's account is assumed, one can understand why well-developed scientific specialties are unlikely to promote sympathetic reading of disciplinary classics: the resources of any such specialty have become concentrated upon the highly specific issues and problems that differentiate specialty from the parent discipline and now make up its normal scientific work. Exceptions to this pattern of division and isolation occur, though infrequently: for instance, in the period of formal education of the young scholar, some amount of "exposure" to the classics is arranged. But, even in those cases, reading of a few classics is likely to be selective and guided by channels of relevance to present-day, concrete issues in the normal work of the specialty or sub-specialty. Indeed, as specialties in a discipline undergo a process of divergence from the main trunk, the pursuit of interests, methods, and viewpoints that depart from those to which the founding figures were committed makes many of the claims, methods, and concerns of the founders less and less relevant to current undertakings. In some cases, an acknowledged classic may have come to be thoroughly misunderstood (see Observer, 1976, for a compelling example from the history of psychology) as a result of historical dismantling of the background assumptions operative at the time of its composition.

Accounting for the Misrepresentation of Pavlov

Two related pieces remain to be added. The first involves pointing out that the scholarly lapses that have been described above can be interpreted as a natural outcome of the development of psychology in the twentieth century. That is, a consequence of the successive subdivision of the discipline of psychology into a collection of specialties is that the classics — those in the discipline as a whole, such as James's *Principles of Psychology*, and those in a specialty, such as Pavlov's *Conditioned Reflexes* — are unlikely to be read sympathetically except by newcomers to the discipline. The influence of that feature may *itself* be a historically situated phenomenon (see autobiographical accounts by Clark Hull [1952/1968, pp. 145, 154–155], Fred Keller [1994], and B.F. Skinner [1976, pp. 298–301]). Some of the classics will be carefully read by those who are in the specialty concerned with the history of psychology as a whole; but it is evident that, when such historians of psychology read *Conditioned Reflexes*, they are not reading as practitioners of the psychology of learning, the specialty in which this work has the status of a classic.

Secondly, the classics are occasionally consulted, because of the service that they provide to specialists: as I noted earlier, examination of a classic permits the reader to obtain quotations that can be used to suggest that a given doctrine is widely accepted; in turn, that state of affairs supports the inference that a finding which disconfirms the quoted passage is an important finding. In the case of a doctrine that is under criticism, the quote enables the critic to show that the doctrine under criticism "is no straw man" (Seligman, 1970, p. 407). For such roles, the selected writing need not be examined in depth: it is sufficient merely to find incriminating passages.

Such selective, "unsympathetic reading" may be motivated by defensive contingencies that have become influential in a partisan atmosphere. It was in such an atmosphere that the biological constraints movement developed, with predictable effects upon scholarship. The current article has documented an example of the same: as the biological constraints movement developed in opposition to an alleged Pavlovian establishment, Pavlov was represented as the proponent of a doctrine which he could not have endorsed, if the principal claims of the current paper are sound.

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