Twitmyer and the Conditioned Response
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those of Bernard, although he did not specify Bernard's work on psychic secretion. Bernard, although he cited certain observations of Colin, seems not to have mentioned cases where their results did not agree.

It appears then that Bernard, as well as Bidder and Schmidt, had made experimental demonstrations of psychic secretion in the early 1850s, Bernard working on salivation and Bidder and Schmidt on gastric secretion. Neither they nor their contemporaries seemed greatly surprised or even greatly interested in these demonstrations. Certainly they did not attempt to carry them farther.

Pavlov apparently never knew of Bernard's experiments.[22] In another paper of 1904, Pavlov did refer to "the beautiful adaptation of [the salivary] glands to external stimulations, as had already been foreseen by Claude Bernard.[23] In this case, however, he referred only to work showing that the amount and quality of saliva is appropriate to the substances put into the mouth.

From this brief survey, it is clear that the phenomenon of conditional reflexes had been observed several times during the nineteenth century. Furthermore, it had been given an explicit associationistic interpretation by Erasmus Darwin a century before Pavlov offered a similar interpretation. Nevertheless, it is also clear that much was left for Pavlov to do. The earlier investigators demonstrated already established conditional responses; they did not attempt to prove their interpretation by trying to establish new associations to stimuli which are not likely to be related to food under natural circumstances. This is undoubtedly one of the reasons that the earlier work attracted less attention than that of Pavlov. When a dog salivated to food, that didn't seem news; when it salivated to a bell, that was news. Since they did not attempt to establish new associations, the earlier workers did not produce any information about the actual progress of the formation of conditional responses. Only a thoroughgoing program of investigation, such as that begun by Pavlov, could show how fertile this field could become.

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TWITMYER AND THE CONDITIONED RESPONSE

In the preceding Note,[1] Rosenzweig shows that the salivary reflex was well known before Pavlov; that numerous investigators had noted an increased flow of saliva when food was seen, smelled, recalled, thought of, or imagined; and that some (Bernard and possibly Dumas) had observed the arousal of the reflex by non-food stimuli—by movements associated

[22] M. A. B. Brazier (ed.), *The Central Nervous System and Behavior,* 1959, 170.
[23] Pavlov, *Lectures on Conditioned Reflexes,* 1928, 61.

[1] *Supra,* 628.

with the giving of food, and by habit alone.[2] Neither Bernard nor Dumas saw the significance of their observations or followed them further. As far, therefore, as the conditioned salivary response is concerned, Pavlov is the discover of it. By showing that a dog's salivary reflex could be established to a bell—to an associated stimulus that was not a food—he supplied the *experimentum crucis* that established it.

Rosenzweig limited his review to the salivary reflex—probably because Pavlov's early work on conditioning was concerned primarily with it. By so doing, Twitmyer's study of the knee-jerk, in which the discovery of the conditioned response was announced and the phenomenon first described, escaped his attention.

Pavlov reported his results in 1904; Twitmyer reported his, in his doctoral dissertation submitted to the University of Pennsylvania, in 1902.[3] The purpose of this Note is to invite attention to Twitmyer's study and to consider the reasons for its obscurity.

Twitmyer's study was undertaken to determine the variability of the knee-jerk. His discovery of the response, that was later to be called "conditioned" by Pavlov, was made accidentally. As a preparatory signal to warn his subject (*S*) that the stimulus-hammer was about to be dropped upon the patellar tendon, he sounded a bell. One day, while adjusting the apparatus for a new series of trials, he accidentally tapped the bell. Though the stimulus-hammer had not been released nor the patellar tendon struck, the knee-jerk followed. His *S* reported that the reaction was involuntary. Regarding this unexpected outcome, Twitmyer wrote:

> Two alternatives presented themselves. Either (1) the subject was in error in his introspective observation and had voluntarily moved his legs, or (2) the true knee-jerk, or a movement resembling it in appearance, had been produced by a stimulus other than the usual one.[4]

He immediately turned from his main problem to determine which of these alternatives was correct. An extended series of experiments upon 6 *S*s yielded the following results:

> Knee-jerks without taps on the tendons were obtained from all the subjects after a large number of preliminary experiments had been performed in which a bell was struck 150° before the blow fell on the tendon. These responses were not the result of voluntary effort on the part of the subject. Attempts to inhibit these kicks were wholly unsuccessful. The movement displayed the characteristic jerky or explosive appearance of the true knee-jerk. . . . No difference in the form of the graphic records of the new response and those of the normal knee-jerk was observed. . . . Preliminary experiments with both the tap of the bell and the blow on the tendon were necessary before kicks could be obtained with the bell alone; the number varied

[2] *Supra*, 629 631 f.
[3] E. B. Twitmyer, *A Study of the Knee-Jerk*, 1902.
[4] Twitmyer, *op. cit.*, 24 f.

from 150 to 238 trials. With an increase in the number of experiments performed, the regularity of response with the bell alone was greatly increased.[5]

Though he found no differences in the graphic records of the two responses, Twitmyer suggested, anticipating future experiments, that the response might not be exactly the same, that differences between them might be detected if more adequate methods, a faster running kymograph, for example, were employed in recording the responses. He concluded his report with a statement that he intended to continue his investigation of the new phenomenon.

He did not, however, keep this promise; that much is a matter of historical record. The experiments, upon which his doctoral dissertation was based, were the only ones he ever reported upon this phenomenon. He had one of the most, if not the most, important experimental discoveries of his day and generation in his hand and he let it slip through his fingers! Why did he discontinue his work on this problem? Why were his findings ignored when reported? Why have they remained buried in obscurity ever since? The answers to these questions are matters of opinion, but the following facts are pertinent.

Twitmyer's dissertation was privately published. A small number of copies were deposited at the University library for exchange with a group of associated libraries and a few were presented to friends and professional acquaintances. Its circulation was consequently small and its readers few in number. The title of his dissertation, *A Study of the Knee-Jerk,* was not particularly exciting nor attention-compelling; it did not indicate that the study was concerned with anything new or especially important. It is doubtful, moreover, whether any of its readers at that time would have attached any great significance to his results. His study, being privately printed, was not listed in any of the annual indexes of psychology,[6] hence, as far as the psychological world at large was concerned, it was almost as though his study had not been reported.

To make up for the dissertation's lack of publicity, Twitmyer presented his work at the annual meeting of the American Psychological Association in 1904. The title of his paper, "Knee-jerks without stimulation of the patellar tendon,"[7] was a little, though not much, better than that of the dissertation. William James, president of the Association that year, pre-

[5] Twitmyer, Knee-jerks without stimulation of the patellar tendon (abstract), *Psychol. Bull.,* 2, 1905, 43 f.

[6] It is not listed in the *Psychological Index,* in the annual bibliographies published by the *Zeitschrift für Psychologie,* nor in Twitmyer's bibliography published in *The Psychological Register,* 3, 1932, 507.

[7] *Op. cit., Psychol. Bull.,* 2, 1905, 43 f.

sided at the meeting at which Twitmyer reported. His report, though presented before the elite of American psychology, fell dead. Not one of his hearers commented upon it after his presentation. The most important paper, as we now know, of that and many succeeding meetings of the Association was followed by—to Twitmyer—embarrassing silence![8] Irwin writes, in his necrology of Twitmyer, that "Twitmyer's own recollections of this occasion were always mingled with feeling of disappointment at the failure of his auditors to express interest in his results, of which he recalled no discussion whatever."[9]

We have here the answer to our first question. Twitmyer did not continue his investigations of the new response because he received no encouragement to do so. The silence and indifference of his audience were prejudicial to further work. Twitmyer had just recently received his doctorate. He had his position in his University and in psychology to win and to hold. He could ill afford to continue work on a phenomenon in which no one but himself was interested. He turned, therefore, to problems approved by his confrères: to the psychological clinic and to the study and treatment of defects of speech, to which he devoted the remainder of his life.

Regarding the remaining questions—Why was Twitmyer's work ignored? Why has it remained obscure?—Irwin is of the opinion that "the intellectual climate [Boring's *Zeitgeist*] in which lived the American psychologist of the first years of this century" was responsible.[10] This is an easy answer and it has the virtue of closing the questions. Much more than an unreceptive *Zeitgeist* is, however, involved; at least more positive and concrete answers are available.

Twitmyer's first report (his dissertation) was, as we have seen, bound to obscurity by the very nature of its publication. His second attempt to gain a hearing—his paper before the American Psychological Association—fared no better. Indeed, in one respect, it fared worse as it not only failed to win the recognition he desired but in addition the indifference of his audience discouraged him from continuing work upon the problem. Though an abstract of his paper, which, as Irwin observed, "presents per-

[8] A good chairman, after throwing the paper open for discussion, would, particularly in the case of a young man giving his first report, have asked the first question to thaw the audience's reticence and to start the discussion rolling. Had James done that, the audience's reaction might have been different. Had Twitmyer received a spark of encouragement, he would have continued his investigation. Had he done that, 'conditioning' might have had its effective beginning in America instead of Russia. "Of all sad words . . . the saddest . . . it might have been."

[9] F. W. Irwin, Edwin Burket Twitmyer: 1873-1943, this JOURNAL, 56, 1943, 452.

[10] Irwin, *op. cit.*, 452.

fectly clearly the essentials of the discovery, a spark surely sufficient to fire the imagination of any one who was adequately prepared for it,"[11] was published in the proceedings of the meeting, it did Twitmyer's cause little good. Abstracts are not listed in bibliographical indexes and abstracts in the proceedings of yesterday's meetings are rarely read. A scholar, as Rosenzweig exemplifies in his review, would search in vain for references to Twitmyer's work. Unindexed abstracts of papers of 50-odd years ago and necrologies are not sources the historian could reasonably be expected to examine. Surely, the *Zeitgeist* is not responsible for the obscurity of Twitmyer's reports. Twitmyer, and he alone, is accountable for that. He never really published his results; never really promoted them. Had he 'christened' his discovery, given it an intriguing name, such as 'conditioned reflex' instead of calling it simply a "new phenomenon,"[12] had he published his reports in a periodical with world-wide circulation whose papers were listed in bibliographical indexes, had his dissertation come to Pavlov's attention, and had he, despite discouragement, pushed his investigations forward and effectively reported them, the history of this experiment would have been very different.

Pavlov and Twitmyer announced their discoveries at approximately the same time and in the same *Zeitgeist.* Though Pavlov made his against the sounding board of the Nobel Prize, it did not fare much better than Twitmyer's; but Pavlov's survived whereas Twitmyer's did not. Something more than a *Zeitgeist* is required to explain this difference and that something is, as we believe publicity and promotion. 'Salivating to a bell by a dog' was news, after it had been vigorously promoted; but 'jerking of the knee to a bell by a man' was not 'news' when promotion was lacking. One does not hide one's light under a bushel if one wishes it to be seen; publicity and promotion are necessary to bring it to the fore.

Twitmyer had little ability, experience, or inclination along promotional lines. He 'missed the boat,' not because of an unfavorable *Zeitgist*, but because he was a young, inexperienced scholar, not a promoter. He did not know how to promote his discovery and he could not withstand discouragement. Pavlov, on the other hand, was experienced in the promotional arts, and he utilized them to their fullest extent. He gave his phenomenon a distinguishing name, "conditioned response," and, despite initial discouragement, he continued his experiments and reports. He could, of course, afford to do this as he was a man of established position and

[11] *Idem.*

[12] There is much in a name, if only as a convenient handle for indexing and reference.

reputation, and he was also a man who could not be discouraged by an adverse intellectual climate. He persisted in his 'promotion' until he had created a *Zeitgeist* favorable to his work. History is replete with similar instances.[13]

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ESTIMATING AVERAGE ERROR FROM THE STANDARD AND CONSTANT ERRORS

Occasionally one wishes to determine the average error (AE) from data for which the constant error (CE, bias) and the standard deviation (SD) or the standard error (SE) have already been computed.[1] One reason for this might be the desire to compare one's results with those of early workers who rather commonly employed the AE as their measure of randomness. Where only a rough comparison is needed, the task of going back to the raw data and calculating the AE seems unnecessarily arduous, and an easy way to estimate this quantity would be a help. The present Note describes a method.*

Handbooks of statistics[2] tell us that for a normal distribution, the average deviation from the mean (AD) is $SD(2/\pi)^{1/2}$. This value is obtained by integrating $y \cdot x$ over one-half of the distribution and dividing by the area under the normal curve for that half. The present method follows the same procedure, except that the averaging is done from the correct value of x rather than from the mean value. Knowing the CE, the difference between the correct value and the mean value of the variable, and knowing the SD (or the SE), we can readily determine the AE for a normal distribution. The SE and the SD are related, as Woodworth reminds us, by $SE^2 = SD^2 + CE^2$.

For convenience in making the derivation, let us express our data

[13] Cf. E. G. Boring, The problem of originality in science, this JOURNAL, 39, 1927, 70-90.

*This work was stimulated by a study of sound-localization conducted under Research and Development Contract NObsr-72627 between the Bureau of Ships and the Defense Research Laboratory of The University of Texas. A brief but frantic search of the easily available literature failed to uncover a method of relating our standard errors to the average errors of earlier investigators. The present paper should help to make the formula more readily available to other workers in psychophysics than it has been (if it exists at all in the literature).

[1] R. S. Woodworth, *Experimental Psychology*, 1938, 396. Woodworth distinguishes between the SD computed from the mean, and the SE, the RMS error computed from the correct value, the 'Standard' of psychophysics, or the 'target.'

[2] See *e.g.* R. S. Burington and D. C. May, *Handbook of Probability and Statistics*, 1953, 84.