
On Publishing Controversy

Norman R. F. Maier and the Genesis of Seizures

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The award of the AAAS Thousand Dollar Prize to Norman R. F. Maier in 1938 for research on conflict-induced seizures in rats was a major event that received appreciable media coverage. However, substantial criticism of Maier's research, spearheaded by Clifford T. Morgan, eventually led to the generally accepted conclusion that the seizures were artifactual and "audiogenic." Unpublished documents have revealed, contrary to the public conclusion of this controversy, that in private Morgan conceded error. Nevertheless, whereas Morgan went on to an important career in experimental psychology, Maier left animal research. The case suggests that it is important to publish controversy and illustrates the power of those working at the core of a discipline over maverick scientists.

Pychological journals are pressed for space as an increasing flow of submissions exerts pressure on editors both to limit the kinds of articles they publish and to be increasingly selective within those ranges. One issue of concern is the utility of publishing critical comments—exchanges of views concerning an experiment, approach, or theory without new data—within a journal otherwise limited to reports of empirical research. The controversy between Norman R. F. Maier and Clifford T. Morgan over the genesis of seizures in rats provides a case study that suggests such articles can play an important role in the development of psychological science. Although rarely discussed today, Maier's research received extensive publicity in its day.

There has been much controversy about the utility of controversy in science (e.g., Vanderplas, 1966; Wenner & Wells, 1990). Many scientists believe that the facts will speak for themselves and that therefore controversy can be counterproductive. However, with increased appreciation of the role of extrascientific factors in shaping the discipline, there has been increased tolerance for such disagreements. Henle (1973) viewed them as playing a useful and necessary role in the development of the science. Ziman (1968) saw a well-fought controversy as a form of cooperation, analogous to two contesting barristers dedicated to the common cause of seeing justice done. Although Boring (1929) found the conclusion abhorrent, he too held that "scientific truth, like juristic truth, must come about by controversy" (p. 99).

The situation is complicated when one combatant works outside of the in-group or the prevailing paradigm of the discipline (see Bennett, 1968; Keller, 1983; Wenner

& Wells, 1990). Both the creative innovator and the crackpot work at the fringes of the prevailing paradigm, and it often is difficult to distinguish one from the other in the early stages of development. The scientific establishment, therefore, must develop a commitment to scientific orthodoxy that makes it hostile to challenges to that orthodoxy. Limiting access to the publication outlets controlled by the scientific establishment is one way in which those who are part of a scientific in-group or who are working within the dominant perspective can help defend that perspective (see Bennett, 1968; Mahoney, 1976). The themes of the importance of publishing controversy and the reaction of the establishment to the outsider are intertwined in the Maier-Morgan story.

The Phenomenon

The research of Norman R. F. Maier and his associates followed in the tradition of Ivan Pavlov, Howard S. Liddell, and others in using animal models for the study of abnormal behavior in humans. The apparatus used was a Lashley Jumping Stand (Lashley, 1930). In this apparatus a rat is placed on a small stand facing two small windows, from which it is separated by a gap. It must jump from the stand toward one of the two windows, in each of which is a card. In a typical learning experiment, when a rat makes a correct choice, the card blocking the window falls over and the rat goes through the window to a food reward. The incorrect card, in the other window, is latched closed, so that if the rat jumps at the wrong card, the animal bumps its nose and falls into a net at the bottom of the apparatus. The choice designated as correct can be based on either location (left vs. right) or the pattern printed on the cards. Rats generally learn such discriminations readily. Under some conditions, however,

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rats resist jumping, preferring to remain on the small platform. Such resistance is overcome by introducing electric shock or a blast of air.

In the typical experiments in Maier's work, rats first were trained to jump to a card bearing a white circle on a black background and not to jump to a card bearing a black circle on a white background. After the discrimination was learned well, the conditions were changed in one of several ways so that the original solution no longer was appropriate and the problem became insoluble. Different procedures were used with different animals. According to Maier (1939), "The essential condition seems to be the necessity of reacting in a situation in which all ordinary modes of behavior have been removed" (p. 79).

Maier and his associates studied two major phenomena. One, which was their primary interest, was the fixation of response when rats were presented with two choices in insoluble problems. Often the rats became fixated on one response, generally a position habit, and it was difficult to change their behavior (see Maier, 1949). The other was the phenomenon of the convulsive seizures, the focus for the present discussion.

The convulsive seizures often occurred as the animal leapt from the apparatus. They were characterized by three stages, the first of which lasted approximately 20 seconds and entailed a bout of violent, undirected running. The second, or convulsive, phase, generally lasting from one to three minutes, involved clonic activity of the legs, biting, salivation, urination, defecation, and sometimes ejaculation. The third phase was one of passivity. Righting reflexes were absent, and the animal could be molded into almost any position. Heart rate could drop to 50% of normal.

From the beginning, Maier (1939) recognized that auditory stimulation was a factor in producing the seizures. Convulsive seizures did not occur when electric shock was used to break resistance; the air blast was required. Indeed, one rat convulsed in response to air blasts given outside of the training situation. Maier (1939) observed seizures in some rats tested outside of the training situation either by placing them on a shelf approximately eight feet from the air blast or by jingling keys. However, Maier pointed out that air blasts had been used when testing animals in difficult discrimination tasks for many years without the elicitation of convulsive seizures; seizures occurred primarily with insoluble problems. Thus, he concluded that "the essential condition seems to be the necessity of reacting in a situation in which all ordinary modes of behavior have been removed. In these experiments compressed air was used to furnish the necessity of reacting" (Maier, 1939, p. 79).

Norman R. F. Maier

Norman R. F. Maier (1900–1977) received his BA degree from the University of Michigan in 1923. After a year of graduate work, he studied at the University of Berlin during 1925 and 1926 and completed his PhD at Michigan in 1928. He was a National Research Council Fellow with Karl Lashley at the University of Chicago 1929–1931 and

joined the faculty at Michigan in 1931. The influences on Maier included John Shepard at Michigan; Wolfgang Köhler, Max Wertheimer, and Kurt Lewin in Berlin; and Lashley and Heinrich Kluver at Chicago (see Solem & McKeachie, 1979). Together with Theodore C. Schneirla, Maier authored the classic textbook, *Principles of Animal Psychology* (Maier & Schneirla, 1935). Maier was out of the then-prevalent Eastern corridors of power in psychology (see, e.g., Benjamin, 1977). His Gestalt background and early work on reasoning in problem solving in rats and humans left him out of the mainstream of the psychology of the time, especially Hullian approaches. He was thus subject to criticism from its proponents (e.g., Wolfe & Spragg, 1934). In the words of Hilgard (1950), "Where others might point out continuities, Maier prefers to point out discontinuities" (pp. 129–130). Furthermore, Maier was rather probing and direct in interpersonal contacts, and he developed a reputation for being a difficult man with whom to get along. The notion that a psychological process, such as conflict, could produce so dramatic a response as seizures appeared heretical to the devotees of the very hard-nosed experimental psychology of the day. They preferred to view the response as more reflexive—especially when the reports came from the likes of Maier.

The Prize and the Media

What catapulted this research from the level of being just another rat study into the eye of the media was the awarding of a prize by the American Association for the Advancement of Science (AAAS). The AAAS Newcomb Cleveland Prize, originated in 1923 and formerly called the AAAS Thousand Dollar Prize, is the oldest award given by the AAAS, the leading scientific organization in the United States. Before 1977 the award was given for the best paper presented at the annual meeting; more recently it has been awarded for an article published in *Science*. Only twice in its history have psychologists received the prize: Neal E. Miller and James Olds shared it in 1956, and Norman R. F. Maier received it for a paper on "Experimentally Produced Neurotic Behavior in the Rat," presented at the 1938 AAAS meeting (AAAS, 1980, 1989/1990; Wolfe, 1989).

The awarding of the AAAS prize was an important event within psychology and outside of it. In summarizing the activities of the Psychology Section of the AAAS, Leonard Carmichael (1939) reported that "American psychologists as a whole are especially proud of the fact that this year for the first time in history the American Association Prize of \$1,000 was awarded to a psychologist" (p. 107).

The response in the media was widespread. The work was written up in the *New York Times*, under the headline "Neurosis of Rats Wins Science Prize" ("Neurosis," 1939). The *Washington Post* gave the prize front-page treatment with the more sensationalistic headline "Scientist Who Double-Crossed Rats Into Lunacy Wins \$1,000" ("Scientist Who Double-Crossed," 1939).

An article in *Life* magazine, appearing the following March, was entitled "Rats Are Driven Crazy by Insoluble Problems" and included 13 photographs ("Rats," 1939). According to *Life*, "From these experiments Professor Maier concludes that many human beings suffer nervous breakdowns when forced to solve problems which have no apparent solutions" ("Rats," 1939). Two authors of published letters to *Life* were generally sympathetic and found generalizations to human behavior quite easy (Phelan, 1939; Wattles, 1939).

Several years later, *Time* magazine covered Maier's attempts to apply his frustration theory to questions concerning the treatment of Nazis after World War II. The article opened, "Norman Raymond Frederick Maier is a man who has made his name and fame by driving rats crazy" ("Cure for Germans?" 1944).

Noted author E. B. White (1939) also saw parallels between Maier's rats and the human situation. He wrote a short story, "The Door," based on an application of Maier's work to urban life in humans. The story entails a man's musings on the changing situations one encounters in life and the frequency with which a "door" that formerly yielded reward becomes fastened shut; life is full of changing reward situations and insoluble problems.

In 1939, after Maier received the AAAS award, the University of Michigan conferred on him its prestigious Henry Russel Award, granted annually to its most promising junior faculty member.

Few attended to the warnings of caution, suggesting some hesitancy on the part of the AAAS committee that made the award. Its citation included qualifications with its praise:

He has produced in rats behavior, the neurotic character of which seems to the committee, and the critical audience which heard his paper, to be beyond doubt. . . . The committee does not feel that the author's analysis of the phenomena is complete, nor does it believe that the small number of rats in which neurotic behavior was experimentally induced is sufficient for generalization or sweeping conclusions, and it gives credit to Dr. Maier for the conservatism he exhibited and for the scrupulous avoidance of applying his discoveries prematurely to the field in which they ultimately will be vastly significant—namely, neurotic behavior in human beings. (Moulton, 1939, p. 93)

The Decline

Maier's work on convulsive seizures is largely forgotten, rarely mentioned in recent reviews. What led to this virtual disappearance? The initial journal reviews of Maier's monograph were considerably less favorable than those in the media. One issue concerned the appropriateness of calling the behavior *neurotic*. Maier later conceded that it was the abnormal fixations, rather than the convulsive seizures, that should be labeled neurotic (Maier, 1943, p. 141). The more troubling criticism concerned the possibility that the seizures were caused by the auditory stimulus from the air blast, with no role played by the conflict situation (see, e.g., Cook, 1940; Karn, 1940).

The most stringent critic was Clifford T. Morgan (1915–1976). Morgan completed his PhD at the University of Rochester in 1939, working with Leonard Carmichael and Elmer Culler. Morgan then accepted a position at Harvard University, where he became a colleague of Karl Lashley and worked in Lashley's laboratory early in his (Morgan's) Harvard career. He then made a war-complicated move to Johns Hopkins University, assuming duties there in 1946.

Morgan and Morgan (1939) reported that convulsive seizures, identical to those reported by Maier, were elicited in rats exposed to the sound of air blasts but without the training regime used by Maier. Later, Morgan (1940) was critical of "the sketchy and unquantitative character of the experimental work (p. 227)." Morgan concluded that "the validity of Maier's interpretations is open to serious question on both logical and experimental grounds" (p. 233).

This led to a long series of articles by a variety of authors during the 1940s. The reader is referred to the reviews of Finger (1947) and Munn (1950) for complete treatments. Maier conducted an active research program during this period, publishing 24 numbered articles in a series on "Studies of Abnormal Behavior in the Rat." Much of this work involved exploration of the determinants of the fixated responses that occurred in situations with insoluble problems (e.g., Maier & Klee, 1941). Other studies were devoted to an exploration of the parameters affecting seizures triggered by auditory stimuli (e.g., Maier & Glaser, 1940). During this period, Maier made only minor modifications of his original views, extended his interpretations to a wider range of situations, and developed a frustration theory to deal with his results (see Maier, 1949).

Meanwhile, much of Morgan's work entailed a systematic exploration of the auditory stimuli that could trigger seizures (e.g., Morgan, 1941; Morgan & Gould, 1941; Morgan & Waldman, 1941). Morgan and his associates held that because the auditory stimuli appeared critical for the occurrence of convulsive seizures, they should be called *audiogenic seizures*, a term that Maier never accepted.

Various other investigators entered the field (e.g., Griffiths, 1942; Humphrey & Marcuse, 1944). A skirmish broke out between M. E. Bitterman and Frank W. Finger, with Bitterman generally supporting Maier and Finger supporting Morgan (e.g., Bitterman, 1944, 1946; Finger, 1945).

The end result of this research and publishing activity was the perception of a resolution in favor of Morgan's position regarding the primacy of auditory stimuli in the genesis of convulsive seizures. Finger (1944) wrote that "the conclusion has gradually evolved that the pattern occurs as a relatively reflex reaction to direct sensory stimulation (primarily of an auditory nature), and has little immediate significance to the study of 'conflict'" (pp. 414–415) and of "the banishment of the phenomenon from the realm of the neuroses" (p. 416). Elsewhere, Finger (1947) concluded that "the majority of investigators

now prefer to regard the behavior as something other than conflict-induced experimental neurosis" (p. 204). Later, Bevan (1955) noted that "the widespread appeal of the Morgan interpretation of audiogenic seizures has resulted in little consideration, except by Maier and his students, of the possible significance of psychologically specified variables in the etiology of these convulsions" (p. 190). Although purporting to avoid the ensuing controversy, Marx and Hillix (1973) discussed Maier's work as "an interesting and important illustration of the necessity for the combination of analytic thinking and control as a basis for interpretation" (p. 13). In the view of Garner (1976), Morgan "demonstrated, in a series of papers, that the effect was not due to a neurotic conflict, but rather to high auditory frequencies, inaudible to man, in the blast of air" (p. 410).

Subsequent Careers of Morgan and Maier

Before the controversy, Maier had been viewed as the senior, successful, although controversial, psychologist—the coauthor of *Principles of Animal Psychology* and numerous other publications. Morgan and Finger were recent PhDs (F. Finger, personal communication, September 11, 1990). The careers of Maier and Morgan took very different routes after the controversy. Morgan became an extremely successful experimental psychologist, administrator, and publisher. He was at the heart of the power structure in experimental psychology. In Maier's surely overstated view, "He built his reputation on his refutation of Maier" (Maier, 1967b).

Morgan served as chair of the psychology department at Johns Hopkins and was credited with building the department in its modern form. After leaving Johns Hopkins, Morgan served on the faculties of the University of Wisconsin; the University of California, Santa Barbara; and the University of Texas. Morgan became a part of the establishment in experimental psychology; he was elected to the Society of Experimental Psychologists before his 31st birthday. Later, he became the leader of the Psychonomic Society, serving as its first chair and building its stable of journals. Morgan became a highly successful writer of textbooks, beginning with his *Physiological Psychology* (Morgan, 1943), published, like Maier's early books, by McGraw-Hill. His *Introduction to Psychology* was so successful that it made him independently wealthy and caused him eventually to drop other commitments and essentially become a professional writer (Garner, 1976).

Maier, by contrast, left the field of animal psychology to find success in industrial psychology. Maier appears to have been positively attracted to work in industrial psychology and to the opportunity for work in a new "laboratory" (R. Heyns, personal communication, February 27, 1990; A. Solem, personal communication, February 9, 1990). Thus, it would appear unfair to conclude that the rejection of his animal research was the only factor in Maier's change in research focus. However, it is

clear that Maier believed that he had been forced to change research areas (Maier, 1966b).

Maier's perception of his treatment by psychologists was charged with the controversy-related emotion of which Boring (1929) wrote (A. Solem, personal communication, February 9, 1990; S. Wapner, personal communication, August 10, 1989). Maier noted that before he had received the AAAS prize, he had been on various committees and had been runner-up in an election for president of the Midwestern Psychological Association. After that, he never was even a runner-up, and his committee invitations declined. He opined, "Of course all this naturally is maneuvering behind your back but I think there must have been a lot of hostility toward it" (Popplestone, 1967, p. 15).

Maier was told that he had been nominated as a member of the Society of Experimental Psychologists on many occasions and had more votes than others but that he was kept out by a blackball system that enabled a single member to veto a membership (Popplestone, 1967, pp. 15–16). Maier recalled various other occasions on which negative comments or actions affecting him had been relayed by friends (Popplestone, p. 15). He experienced difficulty in placing his graduate students (Popplestone, p. 16).

Maier reported difficulties in getting his papers published in journals of the American Psychological Association (APA). He did note that the frustration papers had been published, but he reported many editorial hassles and "nasty letters that I would get from the editors" (Popplestone, 1967, p. 16). Maier recalled that he eventually had to resort to the less competitive and less prestigious journals published by Carl Murchison. He recalled, "It was this type of control over the journals that forced me to change research areas" (Maier, 1966b).

The awarding of a prize should provide a moment of glory and a sense of accomplishment for a hardworking scientist. Such was not the case for Norman Maier. He reflected in an interview, "I think nationally it did me harm" (Maier, 1967b). Elsewhere in the interview, he opined, "The winning of the AAAS prize was probably the worst thing that happened to me." As summarized by his former student, Allen Solem (personal communication, February 9, 1990):

Maier was disillusioned, I know by the rejection of the dominant S-R school of thought of Gestalt theory and more specifically the pervasive jealousy and envy incurred by the Cleveland AAAS and the subsequent Russel awards. . . . He said little about it; however on one or two occasions he said the AAAS award was the worst thing that could have happened to him because of the ill feeling it generated.

Discovery of the Role of Auditory Stimuli

There is an interesting twist beneath the surface of the story of the initial discovery that some rats displayed convulsive seizures in response to the air blast alone. Maier's first monograph appeared in 1939. In it, and in his work

in progress at the time (e.g., Maier & Glaser, 1940), Maier used jingling keys to elicit convulsions in some experiments. Morgan and Morgan's article claiming an auditory basis for the seizures appeared in the June 1939 issue of the *Journal of Comparative Psychology*. According to Maier (1966b), he received a letter from Morgan in March 1939 asking for details concerning the production of convulsions. Maier told Morgan of his subsequent studies and stated that one could produce convulsions in some rats simply by jingling keys. Morgan and Morgan's article on audiogenic seizures, with air blasts as the stimuli, was received in the editorial office on April 26, 1939. Maier continued,

By paying for publication he beat my completed work to print. The tests he ran could have been done in a day. He had more than two weeks to make his tests and to write the four-page paper after receiving my letter. Immediate publication in those days was rare. Why did he perform such a limited experiment and hurry to publication? Naturally, when his paper appeared I reacted to his failure to mention my letter in which I had shared a year's research.

In an oral interview, Maier indicated, "It was a very good way of discrediting me" (Poppstone, 1967, p. 15). In another interview, Maier (1967b) stated, "The thing that I think was very unethical was when he paid for publication to beat mine to press. Because I sent him parts of it."

A different perspective on this story is presented by Morgan's coauthor, Jane Hildreth (formerly Jane Morgan). She recalled an experiment she did as a senior honors student at Rochester that required delivering powdered food through a glass tube in a Skinner box. She cleaned the tubes by washing them and drying them with compressed air. One night, when there were rats in the room as she cleaned the tubes, she noted that three of the four rats displayed seizures. She rushed upstairs to her husband "and burst into the room, announcing with some trepidation that we had 'Maier' seizures 'with no frustration'" (J. Hildreth, personal communication, November 25, 1990).

According to a variation on this part of the story, Maier gave a colloquium at Rochester as his work was in progress during the 1938–1939 academic year. His work generated immediate skepticism from the Rochester psychologists. Morgan triggered seizures by running compressed air through a glass tube and confirmed the finding the night of the colloquium (E. Stellar, personal communications, October 9, 1990, and January 4, 1991). Finger has recalled Morgan informing him that it was he (Morgan) who wrote to inform Maier of his finding of seizures in rats outside of the conflict situation (F. Finger, personal communication, September 11, 1990).

It is difficult to reconcile or decide among these alternatives. A check of the submission dates of articles in the 1939 *Journal of Comparative Psychology* suggests that Morgan's article did receive early publication. The Morgan and Morgan article does appear more preliminary than Morgan's later, detailed investigations; it is brief and

does not even include a complete description of the stimulus used.

It seems likely that Morgan wrote to Maier. It is possible that Morgan wrote after his wife found and he confirmed the occurrence of audiogenic seizures, but perhaps without mentioning the finding directly. Maier might have informed Morgan of the methods to produce audiogenic seizures and might have perceived Morgan as having published the idea Maier had given him when, in fact, Morgan had discovered it independently. This scenario has the advantage of reconciling the apparently conflicting versions from generally reliable sources and suggests that this aspect of the controversy stemmed from a failure of communication. This, however, is only speculation.

A Resolution Kept Private

A more probing analysis of the controversy relies on archival materials but begins with an article by Maier and Longhurst (1947) designed to refute the conclusion of Morgan and Waldman (1941) that the seizures should be viewed as audiogenic. Maier and Longhurst, who provided a fairly detailed breakdown of their results, found seizures on 20.3% of the tests in the experimental group, which had been trained in a soluble problem and tested in an insoluble problem, compared with 7.6% in the control group, which had had no training in a soluble problem. Four times as many individual rats in the experimental as in the control group proved susceptible. However, on average, the control animals received longer exposure to air blasts. Maier and Longhurst concluded that their findings were "definitely contrary to the view which makes auditory stimulation the sole cause of the seizure" (Maier & Longhurst, 1947, p. 409).

Morgan (1948a) wrote a critique of the Maier and Longhurst article and submitted it to the *Journal of Comparative and Physiological Psychology*, the journal in which the Maier and Longhurst article had been published. The manuscript was forwarded to Maier by editor Calvin P. Stone (1948a), with a cover letter dated February 5, 1948. In the letter, Stone appeared favorably disposed toward Morgan and skeptical of Maier, noting that Maier's paper had been "carefully studied" by Morgan. Stone suggested that a joint early publication of Morgan's paper and Maier's reply could be arranged. Stone noted of Morgan, "He would like to have you read it with a view to preparing a short paper that would comment on and answer it, *so far as is possible* [italics added], in light of your work and available data." Stone hoped that such publication would "illuminate the problem, *to show the limitations of the studies already done* [italics added], and to highlight for other researchers the intricacies of crucial experiments in this field."

Although Morgan (1948a) was conciliatory in his article, he offered criticisms of the Maier and Longhurst article that he believed "raise doubts whether their experiment does, in fact, prove or support the hypothesis" (p. 2). Morgan first offered some technical criticisms of Maier's handling of his data. He then raised issues con-

cerning the equivalence of the intensity and duration of exposure to air blasts in the two groups. Next, he turned to an issue concerning the relation between the latencies to seizure and the number of previous seizures. Morgan noted that if five or more rats had their first seizure after 35 seconds, he could account for the results in terms of the greater length of exposure in the experimental group.

Maier (1948b) replied quickly; his cover letter is dated just five days after Stone's. He agreed to the joint publication "since the questions raised are the type of reactions that will continue if they are not expressed." Maier added, "I see no point in communicating with Morgan personally. His position is perfectly clear and since many others share his views it seems best to reply in print." In his succinct three-page reply, Maier (1948a) addressed the issues raised by Morgan. Maier presented a further analysis of his data to show that not even one, let alone five, of his rats could be eliminated on the basis of Morgan's argument concerning the latency to seizure. He pointed out that the orientation of the animals in the jumping box was such that it took the experimental animals further from, rather than nearer to, the air blast. Maier concluded, "I am glad to grant Dr. Morgan a contrary bias and I hope it gives him equal comfort. The unbiased new generations can react to the findings without 'losing face', [*sic*] and I hope they will find that each point of view contributed to the final solution of the problem" (p. 3).

Morgan's replies provide the focal points of the story. The following are excerpts from Morgan's (1948c) letter to Stone:

As you no doubt realized, I seem to be clearly off the beam on the principal point of my criticism of the Maier-Longhurst paper. . . . The long and short of it is that I have had the wind taken from my sails. . . . So, as you might guess, I am not now particularly anxious to have the two papers published. . . . I do not believe the publication of these papers would particularly enlighten journal readers.

On the other hand, it is my error which makes me want to withdraw the paper, and it is only fair, I think, to leave the decision with Dr. Maier and yourself.

Morgan sent Maier a copy of his letter to Stone. He wrote to Maier:

I was quite wrong on the matters of fact in which I attempted to criticize you. I am now convinced that you have done the nearest thing to a crucial experiment, and I am almost convinced that there is no other explanation than "conflict" for the differences which you report. It was a shame, however, that I had to put you to so much trouble, and I am very grateful for your kindness and effort.

As you can see, I feel that the original plan of publishing the "discussion" and "reply" is not particularly good—since I was wrong in my premises. (Morgan, 1948b)

Stone (1948b) wrote to Maier asking "Would you object to our following his suggestion in respect to dropping the plan for publication of his discussion and your

reply to his discussion?" Stone added, "I, however, am willing to go ahead with the publication if in the opinion of *both of you* [*italics added*] this will make a useful contribution to our readers."

Maier (1948d) replied to Stone that he was "willing to let the issue rest." Maier suggested that Morgan might write a paper stating that Morgan had raised certain questions and Maier had supplied him with relevant information. Maier indicated that he would want to see such a paper before publication and raised the possibility that it might be signed jointly. On the same date, Maier (1948c) forwarded a copy to Morgan noting that "the paragraph on the alternative that you write a paper incorporating your questions and our data is merely a suggestion and is not meant to make you feel obligated."

On April 5, Stone (1948c) returned the manuscripts to their authors. Morgan wrote to Maier, "The suggestion in the second paragraph of your letter appeals to me: writing one paper for joint signature." Morgan (1948d) suggested that it "might have a healthy effect on our public to show some substantial agreement." He noted that "I cannot do it right now, however, for I have some other pressing commitments, but I will try to get a draft to you within, say, about a month." That paper never was written.

Maier (1949) did, however, include a footnote in his book, *Frustration: The Study of Behavior Without a Goal*, noting, "Morgan, whose views are reflected by Finger, has informed the author that he wishes to withdraw from the controversy in the light of the Maier-Longhurst study" (p. 136). However, it appears that Morgan resisted public acknowledgment of his revised position. According to Finger (personal communication, September 11, 1990), "When I later asked Cliff if he had abandoned ship, he said that he had not, but that life was short and that Maier's interpretation was unworthy of continued attention. His silence, he assured me, was not to be interpreted as an assent." Sometime later, Maier (1966a) reflected,

I feel the suppression of these papers was unfortunate because Morgan terminated his interest in the subject but the literature is left with the impression that giving the seizures the name "audiogenic" explains them. The problem of investigating the causes and nature of the conflict therefore has been dropped as an interesting area of investigation.

On the Publication of Controversy in Psychological Journals

There can be little doubt that the development of understanding of the genesis of seizures would have been better advanced if the Maier-Morgan exchange had been published. A very dramatic and powerful phenomenon has virtually disappeared from recent literature. It would be difficult to argue that the failure to publish the Maier-Morgan exchange was the only factor leading to the disappearance of the phenomenon from the literature or to the problems later experienced by Norman Maier. However, it is likely that additional research would have been conducted and the public and private resolutions of the

controversy might have been brought to accord. The case suggests that the publication of controversy can be important.

The issue of the publication of comments on recently published articles is covered in the new *Editor's Handbook: Operating Procedures and Policies for APA Publications* (APA, 1992). According to that manual, the editor is under no obligation to publish such material. Should the editor decide to publish such a comment, however, he or she should inform the original author so that a reply can be prepared. All comments should be refereed. The exchange should end after a comment and a reply.

In order to determine the actual policies in force, I wrote to the editors of the APA journals that publish primarily empirical contributions. I received 15 replies. Of these, 11 reported that they did publish such material; several editors referred me to particular instances within their journals. None reported a policy that would absolutely prohibit such publication. In 4 cases, the editors reported that there was no policy against such contributions but that no such articles had been published.

The journal in question, the *Journal of Comparative Psychology*, has published a few commentaries at various times (e.g., Hirsch, 1957; LaFleur, 1943, 1944; Schneirla, 1942, 1943; Tinbergen, 1957). However, such articles appear to have disappeared from the journal. I perused the last 30 years of the *Journal of Comparative Psychology* and the *Journal of Comparative and Physiological Psychology (JCPP)* and located no such articles. According to the current editor, Gordon G. Gallup, Jr., the journal "does not normally publish such material," although there is no set policy against it. During Gallup's tenure as editor, only one such article was submitted, and it was rejected for substantive reasons (G. G. Gallup, Jr., personal communication, July 13, 1992). Of the journals closest in content to the *Journal of Comparative Psychology*, *Animal Behaviour* regularly publishes such comments, and *Behavioral Neuroscience* includes a "Technical Comments" section (e.g., Dworkin & Dworkin, 1991; Roberts, 1991).

The comments of Stone's successor as editor, Harry F. Harlow (1961), provide an interesting perspective:

When I took over as editor of *JCPP*, Dr. Calvin Stone gave me some detailed fatherly advice, suggesting that I would do well not to publish apparatus articles which were not backed by an experiment and not to publish controversies. In both cases, in eleven years of editing, I deviated once and I regret both deviations. I am convinced that Stone was right. . . . I have consistently denied many requests to publish rebuttals and corrections, other than corrections of a mechanical error type (p. 1).

Controversy and the Maverick Scientist

It seems clear that Maier was regarded as a maverick, outside of the mainstream psychology of his time. This perception may have been started with his Berlin experience working with Gestalt psychologists and further developed with his view that problem-solving behavior was qualitatively different from normal learning. The seizure

and fixation work, which Maier also interpreted as suggesting a discontinuity, surely exacerbated this perception.

It is difficult for the objective student to confirm or disconfirm Maier's perceptions of prejudice against him. In his own view, "No one came out and attacked me, it was always done by the students" (Poplestone, 1967, p. 14). The majority of psychologists have papers rejected, are not appointed to major committees, are not elected to major office, and fail to get elected to the Society of Experimental Psychologists. Many of Maier's papers on convulsive seizures and fixations were published in APA journals.

On the other hand, Maier was an internationally recognized scientist who produced over 200 articles and over a dozen books. His work was creative and original. Research trends since Maier's departure from the field have been away from the stimulus-response (S-R) reflexology he opposed toward a cognitive emphasis compatible with his views. No longer does it seem heretical to suggest that learning and problem solving may reflect the action of different processes. Clearly, Maier was out of step with his time—ahead of it in many respects. However, perusal of his 1967 Faculty Biographical Data Sheet (Maier, 1967a), *New York Times* obituary ("Norman Maier," 1977), and *American Psychologist* obituary (Solem & McKeachie, 1979) reveals none of the fellow elections, honorary degrees, or national and regional offices one would expect of a scientist of Maier's stature. It is difficult to explain these lacunae if Maier's perceptions were pure paranoia. There appears to be at least some validity to his perceptions of prejudicial treatment.

Eliot Stellar recalled that Morgan regarded those outside of the Eastern establishment as being in the bush leagues. "Maier was in the bush leagues, not only because he was at Michigan, but also because his concept of conflict leading to seizures was thought to be on the border of real science" (E. Stellar, personal communication, June 23, 1992). In his unpublished textbook, another member of the Eastern group, Frank Beach, dismissed the seizures as due to auditory stimuli and used the case as an example of the misidentification of the effective stimulus for behavior (see Dewsbury, 1990).

Maier (1960) summarized his perception of the operation and machinations of behavioristic psychologists in an article entitled "Maier's Law," which appeared in the *American Psychologist*. According to Maier's law, "If facts do not conform to the theory, they must be disposed of" (p. 208). Maier delineated several ways in which psychologists dispose of unwanted facts, providing graphic examples of each. The first was to give the phenomenon a new name, which thereby recasts the observations in a form compatible with the theory. Another way of disposing of facts is to omit them from reference books. Still another is to fail to report disturbing facts, such as the number of rats failing to learn discrimination problems.

At one level, Maier's law is a rather bitter presentation of a scientist disillusioned with the treatment of his and others' findings. At another level, however, it can be read as an insightful treatment of the first defense of

establishment scientists against anomalies promulgated by maverick scientists (see Kuhn, 1970). Maier was exploring the dynamics of the resistance of mainstream scientists to outsiders and their novel approaches. His own case may provide insight into the operation of these dynamics.

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