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Chapter 13

# *The Psychology of Controversy*

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Ideally, it might be argued, the psychologist is a superior being, for over all other scientists he has the advantage of being a psychologist. He alone, the argument would continue, knows the human mind without which there could be no science. The work of the exact sciences, as they are sometimes called, involves not only precise observation but also a loose admixture of personal prejudice, ambition and conviction. The psychologist, however, knows the human mind that is both the object and the subject of his work, and is superior to prejudice, to exaggeration, to vanity, and consequently to quarrelsomeness. Thus without these constant errors he serenely pursues his way, at peace with his fellow-workers, his hand alone grasping at fundamental truth with the personal equation of observation accounted for and eliminated by corrections. Psychology would thus be the one perfect exact science.

And it might be, though it is not clear that it could be. Certainly psychology has not been above personal bias. It is true that when psychologists battle they may hurl Freudian explanations of each other at each other. They may rise with scientific magnanimity against oppo-

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nents and suggest that falsification is involuntary and unconscious, or that stupidity is inherited and therefore not a matter of individual responsibility. All this is, perhaps, delightfully scientific, and yet there nevertheless remains in such controversy a seeming lack of objectivity. For instance, some psychologists in writing for publication place the mystic symbol Ph.D. after their names, but none, as far as I know, has yet seen the value of adding the statement of his own I.Q. Stanford University now does that sort of thing posthumously for the great, but it has not yet undertaken a handbook of the living. The classical method of psychology is introspection, yet not the behaviorist, nor the "gestaltist," nor the purposivist, nor the late functionalist, nor even the introspectionist himself has yet succeeded in maintaining clear vision with the eye rotated through 180° to see the mind that is at work. From this point of view we would seem to have a long way to go, and yet I must confess to you, attractive as my picture is, that I am not sure that we want to go, or can go, all that way. The scientific eye sees dimly when it turns through half a circle to look behind itself. The scientist, it seems to me, is limited by certain paradoxes of human nature and the psychologist shares these limitations with other scientists. It is therefore to two of these paradoxes that I ask your attention. The second follows from the first.

## I

The history of science, like Hegel's view of the history of thought, is one long series of theses, set off by ardently advocated antitheses, with ultimate syntheses terminating controversy and marking a step forward. This picture, it seems to me, holds, not only for speculative, philosophical psychology, but also for the most rigorously observational work. Controversy has always been part of the method of science. A judge, or even a lawyer, might accept the statement that controversy, the clash of prosecution and defense, is the fundamental method for getting at truth. However, I do not think that the scientist would be quite so ready to subscribe whole-heartedly to this principle. He expects controversy as part of the scientific "game," but he generally engages in it under the principle that "'I am right and you are wrong." We hear little in science of an able defense of a lost case. Only correct discoveries are held to measure scientific ability. There is little applause for the investigator who, by being brilliantly wrong, prevents his antagonist from being wrong at all, and thus contributes to the truth. Unfortunately this situation makes scientists tend to hold to lost causes, when they might know better, for no other reason—unconscious reason—than that the laurel commands no acclaim when shown to be artificial.

After much thought about the matter, I have come reluctantly to the conclusion that scientific truth, like juristic truth, must come about by controversy. Personally this view is abhorrent to me. It seems to mean that scientific truth must transcend the individual, that the best hope of science lies in its greatest minds being often brilliantly and determinedly wrong, but in opposition, with some third, eclectically minded, middle-of-the-road nonentity seizing the prize while the great fight for it, running off with it, and sticking it into a textbook for sophomores written from no point of view and in defense of nothing whatsoever. I hate this view, for it is not dramatic and it is not fair; and yet I believe that it is the verdict of the history of science.

The paradox then in science would seem to be that the more you fight for the truth the less you see it. If you are always trying to see it, you have no time to fight, and without fighting you get science nowhere; you are just the cautious critic who is afraid to venture research. Perhaps I can improve this doleful picture before it is done. Let us, however, turn our attention first to famous controversies, and, since discretion behooves us, chiefly to those of the dead.

If we are looking for controversy, there is of course the case of Mesmer, who participated in a battle royal with the conservative scientists of Paris at the end of the eighteenth century.[1] Certainly the egotistic, opinionated Mesmer was an uncomfortable person for the scientific contemporary who did not believe in his theory; but, if science is, as is so often claimed, quite impersonal, Mesmer's personality ought to have nothing to do with the question of his demonstration of the truth of animal magnetism. Nobody knows whether Mesmer in young manhood was more conceited than the average of men who later became famous. It is quite clear, however, that, even had he been a modest and retiring person when young, his great theory that an important beneficent power resided in his own person would have made him into the sort of man that his opponents would regard as vain. In fact, conceit was involved in his scientific theory, since the theory had to do specifically with Mesmer. Yet nobody in the various investigating committees or in the Académie des Sciences was objective enough to see this crucial point. They denounced Mesmer, largely, so it seems to me, because of his personality, and thus mingled their own personalities with their scientific criticism.

The facts can be restated briefly. Mesmer was a physician in Vienna. As a physician he could not ignore the obvious influence of the celestial bodies upon human well-being, a fact well known then although not now. Mesmer asked himself how these remote bodies can act upon human beings from a distance, and it occurred to him that the astrological influence might be mediated by magnetism, a force that is capable of penetrating space. This was Mesmer's first theory, and if he had, then and there, written a book about it we could have put him down with all those persons, from the Greeks to the present day, who have held and advanced theories without resort to experimental proof. Mesmer, however, in good scientific fashion, used his happy thought as the basis for

1. In general on F. A. Mesmer, see Binet, A., and Féré, C., "Le magnétisme animal," 1887, Eng. trans. 1888, Chap. 1; Moll, A., "Hypnotism," various eds., Chap. 1.

an experiment. He got himself magnets and tried passing them over the bodies of his patients with remarkable effects; the persons were benefited by the magnets or even cured of diseases. "By the magnets"—it is a proper phrase to have used, and yet so often the supposed analysis of an effect into its causes turns out to be wrong. Presently Mesmer met a Swiss priest who was practicing the same kind of therapeutics as Mesmer, passing his hands over the bodies of his patients *without* the magnets. Mesmer had fixed upon the wrong cause of his effect.

There were two things for Mesmer to do. The more probable thing—so I am obliged by the history of science to conclude—would have been for Mesmer to have denounced the priest or at least to have tried to prove that he had concealed magnets up his sleeves, to have come out more vigorously in favor of magnets, insisting that they were essential to the true Mesmeric method, and to have enlarged, by the advertising that dogmatic assertion in the face of controversy gives, his medical practice. Mesmer, however, did the improbable thing: he discarded the magnets. It would have been lucky for all the people who have wrangled over this matter for a century after him, if he had discarded the word "magnetism" also. Unfortunately Mesmer had begun with the notion that a mysterious influence without contact is likely to be magnetic, and the physiologist, van Helmont, a century earlier had expounded a theory of animal magnetism. So Mesmer called his new therapeutic means "animal magnetism," allowing the implication to stand that the influence was something like mineral magnetism. He now knew that he could cure people of certain diseases, or make them think themselves cured, which, in certain cases, is the same thing from the physician's point of view. Others could not effect these cures. Undoubtedly Mesmer's personality and his growing confidence in his power were the reasons for his power, but Mesmer did not know this. Not all minerals are magnetic; why should all persons be magnetic? Mesmer came to believe that he, unlike most other persons, was magnetic, and thus capable of influencing others. All this happened before Mesmer left Vienna for Paris in 1778.

Up to this point, it seems to me, we have nothing more than the account of the genesis of a scientific discovery that is without reproach. Mesmer's personality is irrelevant to the scientific fact. The Church opposed him; the scientific academies ignored him; his followers worshipped him; but none of these things matter. He had discovered hypnotism, that is to say, he had discovered the state of hypnosis, had arrived at a vague notion of its therapeutic significance, and was possessed of the practical means of inducing the state, although he had an incorrect theory as to the nature of the means. Thus Mesmer occupies a definite position in the history of the knowledge of hypnosis. Without him knowledge could not have advanced as it did advance, for it was the travelling mesmerists who interested both Elliotson and Braid, it was Elliotson who interested Esdaile and many others in England, it was Braid who started Liebault on hypnotic work and Liebault who convinced Bernheim who began the Nancy school. Mesmer, ordinarily neglected, occupies the important place at the beginning of the genetic chain of events. That his knowledge of the conditions, the nature, and the effects of hypnosis was incomplete is a situation that applies to almost all scientific discoveries at first; moreover our knowledge of these facts is still incomplete.

After Mesmer went to Paris it is not quite so clear that his discoveries can be divorced from his personality. Here he developed further the conditions of hypnosis, the mysterious baquet with its rods of iron that the patients held (iron, of course, because of its magnetic properties), the circle of sitters about the baquet connected by cords or hands (a circuit, because of magnetic analogy), the subdued light, the soft music, the hocus-pocus of Mesmer's speech and his magician's attire. It is no wonder that the scientific world was disgusted, but my question is whether this disgust interfered with its perception of the truth. Mesmer was the talk of Paris. There was a large band of enthusiastic disciples. The scientists appointed investigating committees, which investigated and found, so it is always said, "against Mesmer." Actually there was no denying the phenomena; all the committees did was to disapprove Mesmer's theory which he had formally embodied in twenty-seven propositions, and in particular to deny the identity of this influence with mineral magnetism and the existence of "a responsive influence between the heavenly bodies, the earth, and animated bodies." The view then developed that Mesmer, since he was not using mineral magnetism, was employing some secret force that he would not divulge. His disciples, who thought that they had been promised this secret, finally turned against Mesmer because he would not reveal it. Mesmer was discredited, driven from Paris by public opinion, and died shortly afterward. He had, of course, no secret to reveal. Everybody, the committees and his disciples, knew all that he did, but could not realize that a man can know how to use a power without understanding its nature.

The question of Mesmer's personality comes in here because we wonder whether he merited rebuke. The technique of the baquet was certainly an aid to the technique of hypnotizing, and Mesmer in a sense made this discovery. But was he sincere? Did he believe that all this mystery was an aid to animal magnetism, or did he induce it quite consciously to attract the crowds? The concept of sincerity is a dangerous one. Psychologists could well do without it, substituting the notion of dissociation. At any rate, it seems to me that psychologists who have thought about the problems of personality will have to agree with Mesmer's defenders that Mesmer at least thought he was sincere; and who but a psychologist could undertake to distinguish between a man's sincerity and his belief in his sincerity? However, the conviction or the exoneration of Mesmer hardly matters to us. What about the scientists who repudiated him? They shut their eyes to an important scientific discovery because they could not stomach the conditions of its demonstration. Mesmer was a nuisance. He was a propagandist and a demagogue, and, behold, the whole world had gone after him. Moreover he was making money out of his discovery. He was vain and opinionated, and had even achieved that summit of conceit of making the new force a property of his own person. It is thus no wonder that the scientists repudiated him, and it is also no wonder that the use of hypnosis passed from the hands of scientists to charlatans for nearly half a century. This is the scientific dilemma that I am discussing: does science preserve its purity and thus retard its progress by shutting its eyes to partial truths, and does it thus sometimes cut off its nose to spite its face?

I have dealt with Mesmer at length because I want you to be quite clear as to my problem. I could now go on with other instances from the history of our science keeping you here until early morning, unless, in spite of being psychologists, you should develop free wills and leave. As it is, my love of determinism is too great to risk such an *experimentum crucis*. I shall be brief with my other cases.

The history of Mesmer was repeated with John Elliotson in the forties of the last century, except for the fact that there can be no doubt of Elliotson's complete sincerity, that is to say, of the complete integration of his personality, with no divided knowledge about what he claimed to be the truth.[2] Elliotson was a physician of exceptional native ability who was a member of the faculty of University College in London in the thirties of the last century. Nowadays we should call him a radical. He was always, to the resentment of his colleagues, advocating some new idea, like the use of the stethoscope, just invented, of which they said, "It's just the thing for Elliotson to rave about," or the maintenance of a hospital in connection with a medical school, an idea which, however, he advocated successfully. He made some important contributions to *materia medica*, and did not hesitate to ridicule the fallacies of current medical dogma. He was too ardent to be tactful, and consequently he was disliked by most of his colleagues. In 1837 Elliotson acquired the inheritance of Mesmer by witnessing the demonstration of a travelling mesmerist. Within a few days he was mesmerizing the patients of the new University College Hospital and getting what he regarded as beneficial therapeutic results. He was urged to desist on the ground that he was injuring the reputation of the Medical School, but he refused on the opposite ground that truth is more important than a reputation. Within a year the Council of University College had passed a resolution forbidding "the practice of mesmerism or animal magnetism within the Hospital," and Elliotson had resigned from the Hospital and from University College never to enter either again. He kept up his crusade. No medical journal, would print his papers so he founded the *Zoist* as an organ of free speech about new things, especially mesmerism. He was denounced. Medical men would not associate with him. He lost his practice. Feeling ran into intimate channels and he also lost most of his personal friends. Yet Elliotson kept on. Mesmeric hospitals sprang up all

2. In general on John Elliotson, see Bramwell, J. M., "Hypnotism, Its History, Practice, and Theory," 1903, 3-30.

over England. He had a group of supporters, but the group did not include many of the reputable medical practitioners of his day.

How far this controversy penetrated into the emotional lives of its participants is illustrated by the following instance. Like Mesmer, Elliotson saw in mesmerism mostly a therapeutic agent, but it was also obvious that the new state might be used as an anesthetic—in those days just before the discovery of the modern anesthetics. In 1842 Ward, a surgeon, amputated a leg of a patient under mesmeric trance.[3] The patient had been suffering excruciating torture from the least motion of an ulcerated knee-joint, and could sleep little. A mesmerist, Topham, one of Elliotson's disciples, found that he could give this patient rest by mesmeric sleep. Later Ward amputated the leg at the thigh after Topham had mesmerized the patient, and tried, in the course of the operation, bruising the cut end of the sciatic nerve. The patient remained in relaxed sleep and denied all memory of the operation afterward.

Ward then reported the case to the Royal Medical and Chirurgical Society of London. The report aroused a storm of protest. Marshall Hall, whom we now honor for the discovery of reflex action, described mesmerism as "trumpery which pollutes the temple of science," and fell back on his own theory, arguing that the report was false because it did not show that the sound leg twitched reflexly when the other leg was cut. Eight years later Hall informed the Society that the patient had confessed to collusion, although the patient then signed a deposition stating that the operation had been painless. Other members at this first meeting of the Society contended that,

> if the account of the man experiencing no agony during the operation were true, the fact was unworthy of their consideration because pain is a wise provision of nature, and patients ought to suffer pain while their surgeon is operating.

At the next meeting of the Society, after violent discussion, it was voted to strike from the minutes the statement that such a paper had been read.[4]

Well? Intolerance does not beget tolerance. That is all. Hypnosis may

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3. The full description of the Ward case and of the action of the R. M. C. S. upon it are given in a little pamphlet by John Elliotson, "Numerous Cases of Surgical Operations without Pain in the Mesmeric States; with Remarks," 1843. The "remarks" are numerous and caustic. The pamphlet I have seen was published in Philadelphia, but I think it was also printed in London.

4. I have omitted all mention of James Braid, the reputed discoverer of hypnosis, on account of lack of time. See Braid, J., "Neurypnology," 1843, reprinted 1899. Braid also met opposition, but he did not break with the medical profession because he refrained from criticizing it, because he laid no claim to a peculiar personal power but sought to explain hypnosis in normal physiological terms, because he avoided the word "mesmerism," because he opposed Elliotson, and because Elliotson attacked him. See my remarks on this situation in *Amer. J. Psychol.*, 1927, *39*, 83-86.

not be the ideal surgical anesthetic, but it is a great deal better than none, as Esdaile, inspired by Elliotson, was in the same years proving in hundreds of cases in India, and against opposition almost as strong as was to be found in England.[5] The medical men almost let the world suffer on in surgical operations for an indefinite period. They might have done so but for the fortunate discovery of the anesthetic effects of nitrous oxide in 1845, three years after Ward's use of mesmerism, and of ether, and chloroform a couple of years later.[6] Against these anesthetics there is a story of similar opposition and of the contention that anesthesia interferes with God's plan for the universe; but I have cited enough instances of this sort.

In fact the history of science is full of such examples. Elliotson made them his text when, after much opposition, he was finally invited in 1846 to deliver the Harveian Oration before the Royal College of Physicians. He could begin most aptly with the story of the opposition to Harvey's discovery of the circulation of the blood.[7]

In modern psychology we have so far been spared the violent controversy that engages public attention, except in psychic research, which represents today a case almost exactly like that of mesmerism. It seems impossible to undertake psychic research without emotion, and the emotions of the investigators are present in part because it is an egotistic hypothesis. Like mesmerism it claims that a peculiar power is localized only in certain individuals, and it defines this power in terms of its effects and omits the causal term that is necessary to every scientific correlation or fact.

However, although modern psychology lacks these dramatic controversies that enlist the lay public on one side or the other, it is lacking neither in controversy nor in intolerance. I trust that I am still treading safe ground if I ask you to recall with me the famous controversy between Wundt and Stumpf about the tonal distances.

Into the elaborate intricacies of this controversy we cannot enter, nor do we need to do so. As is well known, musical interval follows a law like Weber's Law. A given interval is divided into two equal portions by a stimulus which is, in vibration rate, the geometric mean of the stimuli for the two extremes. Stumpf, with a musical background, believed that musical interval bore a close relation to the simple sensory properties of tones. Wundt, basing his view on experiments in the Leipzig laboratory by his pupil Lorenz, regarded sense-distances as less closely related to musical interval. Lorenz's results showed that observers in bisecting a tonal interval tended toward the arithmetical mean and not the geometric. About this difference the controversy waxed.

We should perhaps bear in mind the fact that the difference in

5. The secondary source for James Esdaile is Bramwell, *loc. cit.*

6. For an interesting account of the controversy that the discovery of anesthesia aroused, see Smith, C. A. H., *Scient. Mo.*, 1927, *24*, 64–70.

7. Elliotson, "The Harveian Oration," 1846.

question is small with respect to the tonal distances involved. On the other hand—and this was Wundt's ground for assurance—these seemingly small differences were large with respect to the scatter of the judgments, much larger than modern statisticians are accustomed to require. Stumpf, however, could not accept this view. For one thing he appealed to the extreme case as a *reductio ad absurdum*; if tonal distance is directly proportional to vibration rate, as Wundt claimed, then a major second like $c^3$-$d^3$, must include the same distance as the entire octave, $c$-$c^1$, three octaves below. This proposition seems so manifestly absurd that we can understand why Stumpf felt that Lorenz's results must be capable of being explained away, as he undertook to do argumentatively, in part by questioning the meaning of Lorenz's observers in judging tonal distance and the degree to which they were influenced by musical relationships.

Wundt had espoused Lorenz's results by publishing some of them in the third edition of the "Physiologische Psychologie" in 1887.[8] Lorenz's paper came out in 1890.[9] Then followed controversy, altogether of 141 pages. Each published thrice. First Stumpf printed sixty-seven pages, in which he reprinted portions of many of Lorenz's tables and sought to reinterpret them.[10] There was almost no personal invective in the paper; nevertheless it is hardly impersonal to reprint another man's results and, in a paper almost as long as the original, argue elaborately to opposite conclusions. It is easy to imagine Wundt's feelings when the significance of the observations was thus called in question. Wundt, therefore, replied with a paper which included some personal advice to Stumpf.[11] Stumpf's rejoinder adopted more nearly Wundt's tone.[12] It was called "Wundt's Antikritik." Then Wundt printed "Eine Replik C. Stumpf's."[13] Finally the controversy closed in verbal exhaustion with Stumpf's "Mein Schlusswort gegen Wundt"[14] and Wundt's "Auch ein Schlusswort."[15] The discussion became less calm as it progressed. The two final *Schlusswörter* dealt each almost as much with the psychological problem of how the other psychologist conducted argument as with the psychological problem of tonal distances.

This controversy must be read to be appreciated, but I can perhaps give you the flavor of it. As I have said, Stumpf in his original criticism had little to say to which Wundt or Lorenz could have objected, except that they were wrong and should have drawn exactly the opposite conclusions. The only definite resort to the method of psychologizing opponent psychologists that I have found in the entire paper is this:

8. Wundt, W., "Physiologische Psychologie," 1887, *1*, 428*f*.
9. Lorenz, G., *Philos. Stud.*, 1890, *6*, 26-103.
10. Stumpf, C., *Zsch. f. Psychol.*, 1890, *1*, 419-485.
11. *Philos. Stud.*, 1891, *6*, 605-640.
12. *Zsch. f. Psychol.*, 1891, *2*, 266-293.
13. *Philos. Stud.*, 1891, *7*, 298-327.
14. *Zsch. f. Psychol.*, 1891, *2*, 438-443.
15. *Philos. Stud.*, 1892, *7*, 633-636.

This extension, however, certainly does not amount to as much as it should according to Wundt, who here, as he so often does, has exaggerated a correct idea into another that is falsely inverted with respect to it.

Wundt in his reply studied to be calm. He said twice that he would test Stumpf's conclusions "sine ira et studio," without anger and vehemence. Such a statement, however, carries a latent as well as a manifest content. When the orator says, "I will call my opponent neither a liar nor a fool," he is not doing just exactly what he says he is doing. There is not the least doubt at all that Stumpf had hurt Wundt's feelings, and that Wundt was thus moved to many of his remarks, including his final sentences, which read as follows:

Stumpf knows, I hope, as well as I, that whoever would further the psychology of tone must have something more than musical experience. However, it can do no harm, I believe, if he will strengthen himself in this conviction by the result which he now achieves (as the consequence of this criticism). Somewhat sooner then will this polemic also have for him the further result, that he will learn to value, not only as the best but also as the most useful virtue for a scientific researcher, this: to be just toward others, to be severe toward himself.

This peroration does not seem to me to clinch the problem of the tonal distances. I suppose, however, that Wundt thought it did.

I have not time to cull numerous examples from the remaining four articles. You can imagine what was said after Wundt has thus advised Stumpf "without anger and vehemence." To Wundt's personal advice about being just toward others and severe toward himself, Stumpf, everything considered, replied quite calmly. He said, "Wundt is accustomed to imprint on his polemics a kind of moral stamp. . . . It is distasteful to me to make many words about the matter." Wundt reiterated that he found nothing in Stumpf's rejoinder "from beginning to end but distortion and fictions. 'I have studied these things and you have not!' Upon these words I restrain myself from judgment," he concluded.

Finally Stumpf, who had been consistently the more reluctant to pursue the personal side of this controversy, was goaded in his *Schlusswort* into a frank characterization of Wundt's polemical method. He wrote:

I abstain from a detailed rejoinder to the new voluminous reply of Wundt. For it, which pours out his expression of blind thoughts, any word would be too much. Those, however, who wish to compare, point for point, his new article with mine and especially with the earlier one upon which it is based, will find therein for themselves, as in his preceding article, the same mixture of untrue assertions, of confusions, of mutilations of the course of my thought, of obscure imputations and negligences, of infirm evasions, of fallacies of every kind, and of frequent assurances of the incapacity and ignorance of his adversary.

Each of the first six items of the list Stumpf supported with long footnotes, omitting only citations of his own alleged incapacity and ignorance.

Stumpf started the controversy, but Wundt made it personal. It is plain that Stumpf was drawn into this aspect of it with reluctance, and that, being more tender-minded than Wundt, he felt it keenly. More than thirty years later, in writing a short account of his life and thought, the affair still rankled. He devoted to the controversy a paragraph, which he placed in the biographical half of his article.[16] Stumpf regarded the controversy as an event in his life more than as a psychological contribution; but Wundt, the tough-minded, made no reference to this little affair in his "Erlebtes und Erkanntes."

Now I feel that most of you will be disposed to condemn this controversy and to blame Wundt the more for the part he took, and yet I believe that there are not so many of us who, on the next occasion when our work is attacked in print, will in reply studiously avoid trying to make our antagonist seem to our readers like the fool that we believe him to be. We have not yet solved my fundamental dilemma; we have only illustrated it.

However, before I discuss the major issue, let me point out that controversy of this kind is not limited to Germany. In the nineties there was the American controversy about reaction times, with Titchener and Baldwin the chief protagonists.[17] Titchener was upholding the Leipzig view that the muscular reaction is always about one tenth of a second shorter than the sensorial, provided you have subjects so well practiced that they can assume the two attitudes at will. Baldwin was contending that people are of different types and that some react more quickly in a sensory manner and some in a motor manner. Baldwin thought that Titchener was misrepresenting the truth by selecting subjects that would fit his theory. Titchener thought that Baldwin had wandered from the straight scientific path in concerning himself with a problem of human nature instead of the scientific problem of the generalized human mind. Of course, as Angell and Moore showed eventually, both were right; yet neither seemed to be able to see how the other was right, obvious as the matter is now. If Baldwin wanted to work with individual differences in true American fashion, what matter if Titchener thought that personal idiosyncrasies are not the problem of science. If Titchener got his difference with *general* practice (not, of course, with practice for giving the desired result), why did Baldwin mind that training in the direction of attention should counteract the effect of natural modes of attention? Yet each was so sure of his view that neither ever seemed in publication to understand the other. Each, like Wundt and Stumpf, made a moral

16. Schmidt, R., "Philosophie der Gegenwart in Selbstdarstellungen," 5, 1924, 218.

17. The chief controversial papers are: Baldwin, J. M., *Psychol. Rev.*, 1895, *2*, 259-273; Titchener, E. B., "Mind," 1895, N.S. *4*, 74-81, 506-514; Baldwin, *ibid.*, 1896, N.S., *5*, 81-89; Titchener, *ibid.*, 236-241.

judgment. Titchener thought Baldwin unscientific because he used subjects untrained, in the Leipzig sense, to precise observation. Baldwin thought Titchener unscientific because he closed his eyes to a problem of the natural world.

Nor is controversy of this sort limited to the ancients of the late nineteenth century. I could have taken my examples from the present decade, but I have thus far forborne because it must be hard for you to believe that my remarks carry with them no whit of praise or blame. Perhaps I can briefly make my point that the styles have not greatly changed by a few citations in which I obscure the source.

Only a few years ago one psychologist complained, in a long critical article, about the practice of a colleague, who, he asserted, would praise the work of his friends and condemn similar work by others. This controversy is full of instances germane to my subject-matter, but I shall content myself by citing only the closing sentence of the paper I have mentioned. It reads:

> We live close to one another with our similar problems, which approach today as nearly as does all the community of work. If here, as in a thickly planted forest, conflicting growth occurs, it is a thing of the natural order. If, however, it falls out as in a horse-race, where someone uses the whip in order to lash the noses of the neighboring horses, then I must raise a protest against it in the name of fair play.[18]

Recently a psychologist, usually very conservative in his utterances, actually likened a colleague to "a soap-box evangelist." Within the year another psychologist has said, in print, of still another: "To the charge of misunderstanding must now be added the charges of misreading, misinterpreting, and misquoting," and then, like Stumpf in his *Schlusswort* to Wundt, has proceeded in two pages to document these items. It sounds scandalous, that a scientist should not only misread and misinterpret, but actually misquote. Yet I doubt if either author is less well-intentioned than the other.

You may say, of course, that all this is but the scientific "game," that it is the way things are done. I submit, however, that these expressions are not mere stylistic conventions of writing; often there is even more real feeling than the words express. Wundt's moral prescriptions for Stumpf in 1891 were still disagreeing with him in 1924. We have all known psychologists who were supposed not to be able to meet each other socially lest something should happen. Most of us know what it is to feel bitter about published criticism, especially when it is personal; yet, if science is the dispassionate search for truth by the empirical method, can it flourish in the face of passion?

Let us go back to Wundt and Stumpf. The argument is, so far as it dealt with the tonal distances, very evenly balanced. Titchener said that

18. The quotation is as literal as anonymity permits.

he read the controversy three times, and decided twice for Wundt and once for Stumpf.[19] Since we still do not know the correct answer to their problem, we might say that the chances are even for either of them being right; and thus the chances are even for either being wrong. It was a battle of giants; why discriminate? But, if scientists are seeking only for the truth and not to prove themselves right, then there are even chances that Stumpf would have convinced Wundt, or Wundt Stumpf, and only a twenty-five per cent chance that each would have convinced the other and have thus continued the controversy. I make this ludicrous use of the elementary principles of probability in order to show you how certain you would have been from the start that neither was going to be convinced. They both could not be right; each knew that; each, as a psychologist, knew about human fallibility and prejudice even in the pre-Freudian days, and could therefore realize that there was a good chance of his being somewhere in the wrong. Plainly there is a perseverative tendency in scientific thinking.

It would be easy now to draw the moral that the scientific value of an investigator varies inversely with his emotionality in scientific matters, but I do not believe that such a conclusion would be true. It is not only the lesser men who quarrel. The great are particularly adept at it, and the lesser may perhaps only be copying them. Rather it seems to me that we have a true dilemma, that the drive that urges men to laborious research and to the braving of public criticism with their conclusions, is the drive which perseveres and makes them persist against criticism. Thus the same thing that drives them toward the truth may also keep them from it. We still face, then, the uncomfortable picture with which I began. However, before I attempt even an incomplete solution, I want to deal briefly with the second of my scientific paradoxes, which I promised you long ago.

## II

This second paradox is that new movements in psychology, and presumably in thought at large, are most obviously negative. That which claims to be progress, that which is presently accepted as progress, is nevertheless most patently an undoing of the progress of the past. How then is there any real progress in what appears on inspection to be a regress? The answer, I think, is psychological, but before I come to it, let me try to establish my point about the negativism of progress.

Recently I have tried to show for psychology that trite historian's point that nothing which is supposed to be new is ever really new.[20] The course of scientific thought is gradual, as it is in individual thought. In

19. Titchener, "Experimental Psychology," 2, ii, 1905, 242.
20. *Amer. J. Psychol.*, 1927, *39*, 70-90.

the individual it is hard to distinguish imagination from memory; careful scrutiny of a creative imagination seems to reveal little that is brand-new. So it is in scientific thought. The ideas occur as the result of individual thinking, or the facts are found as the result of experiment, both are put forward, and nothing much happens. Then, perhaps many years later, someone comes along, sees relationships, puts things together, and formulates a great theory or founds a great movement. Often the formulator or founder is not even the compounder, but another man, who because of his personality or because of the times in which he speaks, has the capacity for gaining attention. So he originates, as we say, a step in progress, lending his name to a theory or a school, and it is left to dull historians to discover and reiterate the fact that de Moivre discovered the Gaussian law and Charles Bell the Müllerian doctrine of the specific energy of nerves. Founders are generally promoters, in science as elsewhere, and we have therefore here to consider the mechanisms of public attention.

With respect to scientific movements there seems to exist something like Newton's third law of motion: action equals reaction. You cannot move—in the sense of starting a movement—unless you have something to push against. The explanation of this law, I think, lies in the relation of movements to public attention. Science can actually, by the empirical method, so I am disposed to believe, lift itself by its own boot straps, but the result is not what we call a "movement" because motion can be defined only with respect to a frame of reference. A movement must move with respect to something, and progress must move away from something, if the movement is to command observational attention. It is therefore the business of the founders of new schools, the promoters and propagandists, to call persistent attention to what they are not, just as one political party is forever emphasizing the short-comings of the other.

Thus we see that movements are founded upon controversy, and that all we have been saying about the effect of controversy on controversialists applies also to the schools. A school may be flexible and disposed toward change and growth in all directions except those against which it has set itself. Here it is hardened by its own drive. A movement cannot move backwards and persist, and the question as to which direction is backwards is decided by the opposition which brought the movement into being. Moreover the drive forward leads to an over-estimation of the distance moved. The negativism of progress is thus essential to observed progress.

Now let me illustrate.

The greatest foundation within modern psychology is Wundt's promotion of experimental psychology itself. The question is often asked: Did Fechner or Wundt found experimental psychology? Fechner came first and may be its father, but Wundt is certainly its founder. Fechner with his psychophysics was trying to found, not experimental psychology, but a spiritualistic metaphysics. Wundt, from within physiology, arrived at his view from a study of the relationships of the sciences, in a day when physiology was as self-conscious as psychology is now.

In the interests of the new movement Wundt had to overcome many obstacles. He had to write a scientific handbook for experimental psychology. He had to get himself a chair of philosophy and pervert it to experimental practices. He had to found a laboratory, a real laboratory of rooms with instruments in them. He had to get the experiments going, and then to found a journal for their publication. To make his point it was necessary for him, in all sincerity, to exaggerate. The new experimental science must be exhibited to the world as a lusty infant with none of its organs missing. Thus Wundt, when experimental results were lacking, resorted in his handbook to speculation to fill the chapters. There was certainly an over-emphasis on apparatus, peculiarly psychological apparatus. If psychology was an independent science, it must have apparatus to distinguish if from philosophy, and special apparatus to distinguish it from physiology.

All this we can readily understand because we ourselves are still of this self-conscious school of Wundt's. The struggle to separate psychology from philosophy in American universities is still not quite yet over. The habit of writing complete text-books in the face of incomplete knowledge still persists. There is still, I believe, a tendency to collect and exhibit much psychological apparatus without regard to the immediate needs of research. If you do not know what it is like to be on the inside of a new movement, consult therefore your own minds.

Yet this movement for a scientific psychology was largely negativistic. It was primarily directed against philosophy. It was a long time before Wundt had done any experimental work equal in importance to Fechner's, and yet Fechner thought he was working in experimental philosophy. The experimental work of the sixties and seventies was performed mostly by physiologists. Of course, we say now that the final result has demonstrated the positive nature of the original idea, although there remain philosophers who do not agree. I do not believe, however, that the present outcome reacts upon the situation of sixty years ago. Whatever has happened since, there was a chance then that experimental psychology might prove sterile. But it is difficult to argue clearly where our own prejudices are involved. Let us consider the movements within psychology.

In the nineties there was the school of *Gestaltqualität*. It was a reaction against the current elementarism, although it did not itself avoid elementarism as successfully as does the modern *Gestalt*. The chemical combination of sensations was obviously inadequate for the explanation of perceptions. Nevertheless the form-qualities, the founded contents, the superiora, and the act of founding turned out to have no empirical definition and the movement failed. Or did it not fail, but live on to be reborn in *Gestaltpsychologie*? The answer does not matter. My point is that it would not have been a movement if it had not been directed against something.

So it was also with Külpe's school of imageless thought. The very word "imageless" is a negative term. The movement was nothing more than a protest against sensationism. It is easy to say this now, but what of the enthusiasm of the Würzburgers for the *Bewusstseinslagen* and *Bewusstheiten*? They did not think that their movement was negative. They thought they had discovered a new kind of mental stuff. You have only to read the controversial literature to see how the love of self-preservation sustained each side.

In America we used to have functionalism. It was a revolt of the colonial psychologists against Germany, their mother-country. The controversy between Titchener and Baldwin was a phase of the whole. Germany was more philosophical and America more practical, as America's rôle in the history of mental tests has shown. Chicago functionalism was the explicit movement, but I think it was but symptomatic of what was quietly going on all over America, except in some protected places like Ithaca, where Penelope still remained faithful to the marriage vow. Functionalism centered attention upon the individual and the individual organism. Leipzig could still work with the generalized human mind; in Chicago, and in Columbia too, they had *minds*. I think of this revolt as the most radical since Wundt's original heterodoxy, and I also recall that the explicit functional movement itself was largely negative and got little further along positively than did the school at Würzburg.

In those days the opposite of functionalism was structuralism, but nobody—except perhaps some graduate students—ever called himself a "structuralist." Titchener adopted the phrase "structural psychology" and abandoned it long before it went out of use. No, the functionalists had to have something definite to push against, and it was they only who talked about "structuralists."

We have this same phenomenon in behaviorism. For years the American tendency has been to have two behaviorists growing where one grew before. Any number of psychologists have been willing to call themselves behaviorists and to be proud of it, but they missed badly a definite opposition to set them off. Words have been coined for the opponent school, words like "introspectionism" or "introspectionalism," but I have never heard anyone apply such a term to himself. Someone once suggested "Titchenerism," which had the advantage of seeming to indicate at least one Titchenerist definitely. My point is that all along behaviorism has been seeking an enemy so that it could disprove the charge that it is fighting windmills, for it must fight something; it is a movement.

I know it is not fair to leave behaviorism so casually, but I must do so. Behaviorism is not new; this has been shown more than once. Yet Watson is right in thinking that he founded it. He could not have founded it if it had been new; it would not yet have been ready to found. It denies consciousness as the subject-matter of investigation, and therefore the so-called introspective method for investigating it. In this it is negative. It goes on investigating what is left, bereft of an enemy since many of those whom it woos for enemies would also investigate the same problems. It is unfortunately limited by its parental inheritance, for it cannot get over trying to translate consciousness and the sensory quale into behavioristic terms, as it has already translated association into the conditioned reflex. Respect for parents may be laudable and yet hinder the free development of youth. Behaviorism is already past its prime as a movement, because movements exist upon protest and it no longer needs to protest. Had it been less successful it might have lived longer as a movement and a shorter time as a method.

*Gestaltpsychologie* is in the same box with behaviorism. Born at the same time its development was hindered by the war, so that it is now less mature. Its infant cries of protest against an unkind world still persist. Everybody must know now what *Gestaltpsychologie* is not. It is not elementaristic or associationistic. It eschews the vague concepts of the past, like attention and attitude, and cultivates new vague concepts in their place, like insight, closure, and level. When Wertheimer and Koffka were describing it, they worked largely in negative terms. There is no general positive content of *Gestaltpsychologie* with which anybody disagrees. Still the voice cries in the wilderness, whereas the kingdom of God is already with man. *Gestaltpsychologie* was not new in 1912; it was quite ready to be founded. It is now a movement. Presently, I think, it too will become simply psychology.

I am now ready to form a conclusion.

I believe that I have shown that movements and the rise of schools are a form of controversy, often one-sided because directed against no particular antagonist. Thus, as controversy, the movement introduces all the psychological advantages and disadvantages of personal controversy.

Discussion is relevant to scientific work, but controversy is more than discussion. It involves emotion; and passion, while of itself irrelevant to scientific procedure, enters to prejudice reason and to fix the debaters more firmly in their opinions. If it were possible, scientific discussion should be dispassionate, not only in form but in spirit, for otherwise progress toward the truth is hindered.

Since the controversy of a movement is apt to be less personally pointed, especially when there is only one active party to the quarrel, participation in a movement may have the advantage of blinding the scientist less than participation in a personal controversy. On the other hand, movements, in so far as they are blind, have the further disadvantage of lending to blindness the social support of the group within the school.

As psychologists, we cannot, however, afford to condemn controversy, be it ever so emotional. If we could read out of the body scientific every investigator who lost his temper with an opponent and kept it lost, we should read out those very men who, because of their drives or prejudices or whatever we like to call that conative component of their personalities, had made the positive contributions to the science. Research is something more than a habit and it requires something more than patience. It requires, among other things, an irresistible urge, bolstered up, I think, not so much by curiosity, as by egotism. This urge may carry one to the truth, beyond it, or even directly away from it. Vision and blindness are here alike, for both are attention, and attention to one thing is inattention to another. The same urge helps and hinders progress.

Must the truth then forever transcend the individual? Is the stage of science like the court of law, where attorneys contend and only the judge speaks the truth? This is the view of research that I find so personally abhorrent and yet seem forced to accept.

There is, however, an incomplete solution for the dilemma. A scientist should, I think, cultivate dissociation. Too much has been said in favor of the integration of the personality, and too little in favor of dissociation. The scientist needs to be a dual personality. He needs to be able to become the prosecutor or the judge at will. He can then stand off and evaluate himself at times, and perhaps even arrange things so that the prosecuting personality will fare more happily when it returns to dominate his person. But I would not have him be the judge too often, for then the assured, prejudiced, productive personality might get "squeezed out," and science would be the loser.

I recommend this dissociation, not because it will make us happier, not merely because it is fun to be the judge as well as the prosecutor, but because I have no expectation that it could be so complete that there would be no interaction between the two personalities. I should hope for a tempering of the prosecutor by the judge so that there would really be more vision and less blindness, and so that psychology would benefit thereby. Then we should have less futile controversy, fewer people devoting their lives to lost causes, even more candid and thus more fruitful discussion, less talk and more research.

I have asked you to-night to play the judge with me. I think it is important for psychology, still so talkative a science, that we should all be practiced in being judge as well as prosecutor. Do I dare in closing to point you a moral, as Wundt so ungraciously did to Stumpf? If there is any precept that comes out of all this talk, it is rather that we should beware of precepts. Psychology needs both judiciousness and effective prejudices; and I cannot resist the impression that we shall do well to cultivate and welcome both.