



Dr. Adler at his desk when he wrote his autobiography (1977).

DEAR DOCTOR

*The delightful story of how Mortimer Adler
got his Ph.D.—in his own words.*

During those last years at Columbia, many letters passed between Bob Hutchins and me, those he wrote me always addressed “Dear Doctor.” I do not know whether that was simply a humorous acknowledgment of my claim to being a scholar of sorts or an oblique reminder that I had not yet earned the title. The reminder I did not need. It came in no uncertain terms from Professor Poffenberger, head of the Psychology Department in which I had been teaching since 1923. Poff said, gently but firmly: “Mortimer, you’ve been around now for almost five years. It’s about time you got your Ph.D.” That meant finishing a piece of “experimental” research, writing a dissertation, and defending it in the final oral examination that all doctoral candidates had to go through. Some years earlier, I had passed the preliminary written examinations, which consisted mainly in a day-long ordeal of answering questions about every aspect of the science of psychology—its history and its present state.

In earlier pages, I may have given the impression that psychology as a subject was of little interest to me. Far from it! The study of the human mind and of human nature was from the beginning and always has remained one of my main interests and a field in which I have done a great deal of thinking, lecturing, and writing. What bored me or irked me, because I thought it so often trivial and insignificant, was the so-called “experimental science” of psychology, whether the experimental work was done in line with the structuralist, or introspective, approach represented by Wundt at Leipzig and Titchener at Cornell, the behaviorist approach of Watson at Johns Hopkins, the functionalist approach of Angell at Chicago and Yale, or the dynamic school of psychology that had been developed at Columbia under Woodworth. The theoretical framework and the conceptual apparatus of these various schools did not seem to me adequate to the task of understanding the nature of man and the acts of the human mind, nor did their experiments do anything but confirm this inadequacy.

The very fact that the science of psychology was divided into conflicting schools of thought showed, as Professor Boring pointed out, that psychology had not resolved its ambiguous relation to philosophy. That, after all, was the focus of my deep interest in the subject: the philosophy of man and of the human mind—philosophical, not experimental or scientific, anthropology and psychology. Nevertheless, I was sufficiently diligent as a student to have passed with flying colors the written examination covering the various contemporary schools of psychological thought. Only one of the books I studied for that examination stands out as a work worth reading again and again—William James’s two-volume

Principles of Psychology—and it is included in *Great Books of the Western World* precisely for that reason. The writings of Freud are also included; I had begun reading his books and papers early in my career, but in the 1920s, academic psychologists ostracized Freud. There was no mention of him or his work in the preliminary Ph.D. examination.

The work I had done as an undergraduate in neurophysiology, and the wider reading I had subsequently done in physiological psychology, together with the questions raised by the James-Lange theory of the emotions, determined the direction of my first efforts to do the laboratory research required for a Ph.D. To collaborate in this research, I was fortunate in having a classmate, George Schoonhoven, who was well trained in physiology. We proposed to test the hypothesis that the emotions fell into two main groups according to their affective tone—the unpleasant emotions of fear and anger on the one hand, and the pleasant emotions of hunger and sex on the other. Since, according to the James-Lange theory, the emotions consist in widespread bodily changes, we proposed to measure the physiological reactions that occurred simultaneously when, under laboratory conditions, we induced fear and anger, or hunger and sex. The pupillometer that Arthur Rubin had used for his Ph.D. research was still available. Schoonhoven rigged up the pupillometer so that we could take a continuous reading of pupillary dilations and contractions and register them on the smoked surface of a moving kymograph. The subject was hitched up to the pupillometer in a small room that had to be absolutely dark for the purpose. A sphygmomanometer was attached to his chest to register changes in respiration on the kymograph; electrodes were attached to his wrists to register psychogalvanic changes; the apparatus needed to measure blood pressure was attached to his arm, and a blood sugar test to measure adrenaline discharge was made at the end of the experiment.

On the physiological side, the hypothesis being tested involved a division of the emotions into two groups of visceral reactions—the unpleasant and relatively intense emotions consisting of reactions innervated by the thoracico-lumbar segments of the sympathetic nervous system, the pleasant and milder emotions consisting of reactions innervated by the cranio-sacral segments: the first group of bodily changes would include pupillary dilation, accelerated respiration, heightened blood pressure and blood sugar, positive psychogalvanic response, and so on. Supposedly, the milder, pleasant emotions would involve an opposite set of bodily changes. To test this, we first had our subjects—all students in my experimental psychology class—suffer anger, shock, and fear. Anger was pro-

duced by my kicking the subject's shin under the table on which the pupillometer stood; shock by Schoonhoven's firing off a revolver behind his head; and fear by wrapping around his neck a young boa constrictor which George had borrowed from the zoology lab, the extremities of which he held firmly in his hands.

Up to this point the experiment was a great success. The kick on the shin, the revolver shot, and the cold coils of the boa constrictor all elicited the same set of violent physiological reactions, as we expected. The visceral content of fear and anger appeared to be exactly the same state of excitement and stress. If there is any difference in the psychological content of these two intense emotions, it must lie in the cognitive and conative contexts—what is being perceived and what impulses the perceptions set in motion, recoiling and running away from what is perceived as dangerous, as contrasted with aggressive behavior toward what is perceived as aggressive. If hunger and sex are emotions of a different sort from fear and anger, not only should their impulses to action be different, but their visceral content should also be different. In putting that part of the hypothesis to the test, we faced the difficulty of inducing the appropriate emotions under controlled laboratory conditions. For sex, I persuaded the students who volunteered as subjects to bring to the darkroom some girl with whom they had established at least a minimal degree of intimacy. When George had strapped the subject to all the pieces of apparatus that we used, I instructed the young lady to enter the darkroom and engage in mild forms of fondling accompanied by affectionate speech. The only result we obtained was embarrassment on the part of the subject, hardly mild, and the recorded visceral reactions were the same as those of fear and anger. A similar thing happened when we tried hunger by asking the subjects to starve themselves in the twenty-four hours before they came to the darkroom. Then, after they were strapped up, we passed a hot cup of coffee and a redolent bacon and tomato sandwich under their nostrils, only to elicit pain and anger on their part, the frustration, not the satisfaction, of hunger.

We should have known, of course, that it was impossible at that time to measure the visceral pattern of satisfied sex or hunger under laboratory conditions. In the case of sex, that is no longer the case. My guess at the time was that the visceral content is exactly the same in all violent emotional excitement, whether the emotion is called fear, anger, or sexual passion by the person experiencing it. It is experienced differently because of the differing perceptions and the differing impulses to which they give rise. I wish we could have tested that conjecture; but in addition to difficulties that we did not know how to surmount, George Schoonhoven fell ill, and

we could not go on with our work together. We made a preliminary report on our findings, and that won us membership in Sigma Psi, the equivalent of Phi Beta Kappa for graduate students in the laboratory sciences.

When, to the great sorrow of all his friends, George died of cancer, I decided not to go on alone to complete the work we had begun. That left a hole to be filled. A few years later, I hit upon an easier piece of research to do for the Ph.D. It would be empirical, if not experimental in the laboratory sense of that term; it would involve tests and measurements, and it would call for statistical computations and graphs—all the paraphernalia needed for a dissertation in the Psychology Department at that time. Professors Abbott and Trabue, at Teachers College, had constructed a “poetry appreciation” test by taking verses written by Shelley, Keats, Wordsworth, or Tennyson, and spoiling each in three different ways, one version spoiling the meter, one spoiling the rhyming scheme, one spoiling the sense. The original and the three spoiled versions were then submitted to students to number in the order of their preference—from most liked to least liked. By testing groups of students, different in age, in intelligence, or in scholastic background, correlations might be obtained between these factors and the appreciation of excellence in poetry. That, at least, was the theory behind the work of Abbott and Trabue, and since it was then regarded as a competent piece of psychometric research, I saw no reason why I could not submit a similar piece of research for my Ph.D., substituting music for poetry in the construction of the tests.

Douglas Moore was then a young instructor in the Music Department in Columbia, and when I told him my idea at a Faculty Club luncheon one day, he volunteered to produce the musical equivalent of the Abbott and Trabue poetry test. Gifted composer that he was, he easily turned out amusingly spoiled versions of passages from musical classics—one version spoiled by being made musically dull, a second by being made musically sentimental, and a third by being made musically chaotic. Douglas Moore constructed two series of tests, in the first of which the originals to be spoiled were drawn from Chopin, Bach, Beethoven, and Wagner; and in the second of which he used Mozart, Rarneau, Brahms, Weber, and Chopin. In addition, he persuaded friends of his at the Aeolian Company to record his playing of these pieces of music on duo-art piano rolls that I could use on the Aeolian player piano. The Aeolian Company obligingly sent one of their player pianos around to the various schools and colleges in the metropolitan area, at which I had permission to conduct the tests.

Over a period of two years, I accumulated a vast pile of raw data in the form of test results. All that remained was to score the papers on which students had registered their preferences, work out statistical correlations of the results with other supposedly relevant factors, construct tables and charts, and write the dissertation itself. I found myself either too busy or too bored to do much of this busy work, so I hired two of my students to do the scoring and my sister, Carolyn, who had graduated from Barnard and was working for her own Ph.D. under Professor Boas in the Department of Anthropology, to do the necessary statistical computations. A girl who had been a classmate of my wife at Barnard and was now working with her at R. H. Macy's department store volunteered to construct the graphs or charts that an orthodox Ph.D. dissertation had to include in order to look right. As for the dissertation itself, I had examined so many of them that I knew exactly how one had to be written: an introductory chapter stating the problem, followed by a description of the method and the materials devised to solve it; then a series of chapters summarizing the findings, accompanied by tables, charts, and graphs; finally, a chapter or two stating the writer's interpretations of his findings and the conclusions he could draw from them. Once all the data was in hand and the statistical work had been done, there would be no difficulty in writing the dissertation. In fact, I did it in twenty hours at the typewriter, turning out seventy-seven pages between 9:00 AM. one day and 5:00 A.M. the next.


Before I tell the rest of the story, I must confess that I had little or no interest in this Ph.D. project; in fact, little or no interest in getting a Ph.D. I had not yet read William James's telling attack on the Ph.D. octopus in American institutions of higher learning, but if I had been acquainted with it at the time, I would have given it to Professor Poffenberger as expressing my reasons for not thinking it necessary to get a Ph.D. I had been teaching the subject for five years and had demonstrated in the preliminary written examination my knowledge of it. Why did I need to do some trivial piece of research, have it published, and get awarded a Ph.D. for it in order either to go on teaching or to win advancement in rank and increase in salary? I realize, of course, that Poff would have listened to me patiently, been tolerant of my complaints against the system, but he would also have told me that I had to do it whether I liked it or not. He was so insistent on my conforming to the requirements that he even conspired to help me conform by maneuvering enough credits on my graduate school record to fulfill the course requirements (I had cut some of the graduate courses that I had registered for, and so received only attendance credit for them, which was not sufficient for the purpose).

In addition, I had never taken the examinations in French and German which were among the requirements for a Ph.D. in psychology at Columbia. On this score, I must confess a profound disinclination on my part to become competent in foreign languages. I had passed my French courses in college, but I did nothing to maintain or improve my ability to react that language. I began the study of German, but found its irregular verbs and its peculiar word order so annoying that I gave it up. The secretary of the Psychology Department, a few months before my oral examination, called my attention to the fact that my records showed that I had not passed my qualifying examinations in French and German. She, too, was willing to conspire, and said she would not mention this to Professor Poffenberger if he did not specifically ask her a question about it.

The morning of the oral examination finally came. It was held in the Trustees Room in Low Library and attended by four professors from my own department, together with three or four from other departments. The dissertation I had submitted bore the title "The Experimental Measurement of the Appreciation of Music." Professor Woodworth sat at the head of the long conference table, chairman of the meeting. He opened it by a startled exclamation as he looked at the matriculation parchment in front of him, which contained the candidates record. "The candidate," he said with a smile, as if it could not possibly be true, "does not seem to have passed his French and German examinations." Then, with another, even gentler, smile he added: "Let's do something about that here and now. You, Professor Garrett, ask him a question in German, and you, Professor Lecky, ask him a question in French." Garrett asked me what time it was, and I replied, "*Zehn Uhr*"; Lecky asked me how I felt, and I replied, "*Trés bien*"; and Woodworth, with a final smile of benign content, said, "Examination passed!"

Since I felt that the dissertation itself was not worth two full hours of questioning, I diverted the attention of the examiners from it by proposing a theory of pleasure and displeasure in the aesthetic experience. The theory contended that pain had no sensory opposite, and that displeasure was not the opposite of pain, but the opposite of pleasure as an affective response that had no specific sensory basis. I argued for this contention on the physiological grounds that we have specific nerve endings for pain, but none for pleasure. The theory was novel enough not only to get everyone's attention, but also to set my examiners to quarreling among themselves about it. This used up most of the two hours, and after returning to the Board Room, which I had been asked to leave while my examiners

discussed the merits of my dissertation and its defense, I was told that I had passed but that my examiners recommended that the title of the dissertation be changed to “Music Appreciation: An Experimental Approach to Its Measurement.” It was published under that title as Number 110 in the *Archives of Psychology*, edited by Professor Woodworth, and its preface expressed, not fully enough, my debt to all the persons who did the real work on it—Douglas Moore who wrote the music, the technician at the Aeolian Company who made the recordings, two students of mine, Richard Fitch and Sigmund Timberg, who scored the papers and tabulated the results, and, last but not least, my sister Carolyn, who did or supervised the statistical computations, graphs, and charts.

That morning in April 1929, when I finished writing the dissertation a little before 5:00 A.M., I did not go to bed, but lay down for a brief nap until the morning milk and paper arrived. While breakfasting, I looked at the *New York Times* and, on the first page of the second section, found the announcement that Robert Maynard Hutchins had just been elected president of the University of Chicago at the age of thirty. I can recall vividly the thought that jumped into my head the moment after I felt a surge of exuberant gaiety at this announcement. Why, I asked myself, had I drudged through this tiresome Ph.D. business when it might no longer be necessary for me to have that union card for academic advancement? Then, almost as quickly, I remembered the repeated salutation “Dear Doctor” in the letters Bob Hutchins had written me. I might just as well go ahead and justify the epithet, even if I regarded it as having little significance. 

From Chapter 6, *Philosopher at Large: An Intellectual Autobiography* (1902-1976) Collier Books, Macmillan (1977)

EDITOR’S NOTE

For those of you who may be interested, upon request we will send you a copy of Dr. Adler's Ph.D. dissertation.

Please note that this pdf, is 9.4 MB in size and may be too large for the capacity of your e-mail program.

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