

# Autobiography of Karl Marbe[1]

Karl Marbe (1930)

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## I. WORK IN AND PRACTICAL ATTITUDE

The reason why I am only now responding to the very friendly and complimentary request which came a few years ago from Professor Carl Murchison, that I write my biography for this collection, lies in the fact that precisely in the last few years I have been carrying a particularly heavy load of work, from which I am only now beginning to recover somewhat. In addition to conducting my scientific work and my professorship at Würzburg I taught for five and one-half years (up to October 1, 1931) in the Commercial High School in Nürnberg (High School for Social and Industrial Science) where, as in Würzburg, I represented the subjects of psychology, pedagogy, and philosophy, also belonging to the Senate of the high school. In Nürnberg I was furthermore in charge of the Psychological Institute of the .



Although the Nürnberg position became more and more exacting, I enjoyed my work in it. Although in general a university can certainly give a psychologist and philosopher much more stimulation than a commercial high school can, yet my working in such a high school did have certain special advantages for me.

Immediately after the War I had begun to be interested in the psychology of advertising, which

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arose in the and which, like industrial psychology as a whole, was first made known in my country by Hugo Münsterberg. Already in 1924 there appeared a book on the psychology of advertising by Theodor König, then one of my students, which, as the first German book on this subject, had a large circulation, went through three editions, and had been written originally as a doctor's thesis in my Würzburg Institute. Already in the year 1925 I had written for the journal, *Die Reklame*, an article entitled "Die psychologische Beratung bei der Reklame," and before and after this I had been called upon many times by the most varied concerns for expert opinion in the field of advertising. I had been interested from the start in the aptitude tests from . And even before the War I had [p. 182] been concerned with the practical significance and utilization of psychology. My essay, "Theorie der kinematographischen Projektionen" (, 1910), already aimed at a practical application of psychology. As long ago as 1912, at the Fifth Congress of the German Society for Psychology, in , I presented an extensive summary of discussions of the significance of psychology for the other sciences and in practical life.

It is plain that this practical attitude of mine, which revealed itself in the above-mentioned activities and also in many other writings, some of which are to be mentioned later, necessarily made work in a commercial high school appear desirable. Also, there were many fields of industrial psychology—as, for instance, the question of aptitude for being a merchant and of the psychological analysis of industrial markets—in which, inevitably, commercial high schools were more interested than universities. In Nürnberg I worked particularly on the psychology of advertising, often lecturing there in conjunction with the Professor of Industrial Management, Dr. Alfred Isaac Kurse, on the psychology and technique of advertising.

It is also quite plain, however, that all my efforts in the field of industrial psychology were necessarily much deepened by association with my Nürnberg colleagues. While my book, *Psychologie der Werbung* (Stuttgart, 1927), was still written primarily during a time when I was not working in Nürnberg, my work on the *Psychologie der Wertreklame* and two essays on the psychology of command and obedience come within my Nürnberg period.

Furthermore, I had already worked once before for a time in a commercial high school, namely, while I was at the Akademie für Sozial- und Handelswissenschaften in a. , where I taught from 1905 to 1910. During the first semester of 1909-1910 I worked simultaneously in Frankfort and as regular Professor at the University of Würzburg, where, even before moving to Frankfort, I had been for nine years, first as a Privatdozent, then as an externe professor, and finally as Vice-President of the Psychological Institute and for a year also as the official representative for pedagogy.

In my time the Frankfurter Akademie was at the same time a commercial high school, and even today, although it has become a university, it still counts the tasks of a commercial high school among [p. 183] its most important functions. My interest in questions of industrial science had already been aroused in , although of course industrial psychology did not exist at that time.

The practical attitude which I have mentioned showed itself also in the invention, development, and application of my "Russmethode" (method using smoke of flame for direct recording of voice), to which I devoted a considerable number of articles in the *Physikalische Zeitschrift*, the *Zeitschrift für Psychologie*, and other journals, and which falls within my Frankfort period, although this invention itself originated in purely theoretical considerations. During my earliest activity in Würzburg I had been interested, among other things, in the psychology of language. This interest was perhaps particularly stimulated by a friend who died all too young, the well-known linguist, Albert Thumb, with whom I had published, in in 1901, the essay, "Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung." Later I had published my work, *Ueber den Rhythmus der Prosa* (Giessen, 1907), in which I was able to show, among other things, that the aesthetic impression of a prose work depends essentially upon the rhythm of the work, that this rhythm is different in different works and authors, etc. It seemed a logical step to me then to subject the melody of speech to experiments similar to those relating to dynamic rhythm and to find a technique which suited me for the graphic registration of the melody of speech. In this way I came upon the generally known "Russmethode," which I at once had Eggert, my student at the time, apply to

the melody of speech (*Zeitschrift für* 1908, **49**, 218-237), and which later led Thom and me to the plan of investigating the melody of dialects by this "soot method"—a plan which was, of course, frustrated by Thom's death (1915).

I also treated the "soot method" from the theoretical point of view (cf. Marbe, K. and Seddig, M.: Untersuchungen schwingender Flammen. *Annalen der Physik*, 1909, **30**, Series 4, pp. 579 ff.), and it has also been applied to phonetics. Its invention likewise led me to the most varied practical applications, which were far remote from the field of language. Thus I have shown that it can also be applied to the graphic registration of heart sounds and pathological heart noises, a fact which led to the book, *Klinische Untersuchungen über die Schallerscheinungen des Herzens* (Leipzig, 1911), by my friend the internist Ernst Roos, who, alas, has also died.

[p. 184] Further testimony to my practical attitude is borne by my articles, "Ueber Psychologie und Eisenbahnwesen" (*Archiv für Eisenbahnwesen*, 1924, pp. 729 ff.), "Psychologie und Versicherungswesen" (*Zeitschrift für die gesamte Versicherungswissenschaft*, 1925, **25**, 337 ff.), and "Ueber Strafanstalt und Psychologie" (*Zeitschrift für Psychologie*, 1926, **99**, 375-382). Evidence of this attitude is also given by my actual psychotechnical investigations, such as, for example, the one on aptitude for surgery and dentistry (*Deutsche Zeitschrift für Chirurgie*, 1928, **208**, 289-317; *Deutsche Zahnärztliche Wochenschrift*, 1928, **31**, No.6) and also my little essay on "Psychotechnische und faktische Eignung" (*Industrielle Psychotechnik*, 1928, **5**, 16-20). This attitude likewise explains my wish, which, is continually being reactivated, to obtain practical recognition and practical significance for psychology as extensively as possible in the most varied fields of life and science.

Perhaps my largest accomplishment in this respect has been attained through my effort to bring psychology and jurisprudence into closer contact with each other. The first German psychological legal expert opinion was my testimony in a case of sexual assault in Würzburg in 1911, in which I had to discuss the question of the testimony of children. Soon afterwards came my testimony in the suit resulting from the big railroad accident at Müllheim in , which was argued before the Provincial Court of Freiburg i. At that time I was able to utilize not only the psychology of alcohol but also, and especially, the theory of reaction times, which has been brought up so many times since, and must be brought up, in the psychology of accidents. This case was argued in 1912. Since that time, through my agency and that of others, a mass of psychological expert testimony has been submitted, bearing continually upon new circumstances. Thus, for example, in 1930, as an expert witness in a case in Elberfelde, I was able to treat experimentally, among other matters, the question of whether or not certain impostures and deceptions are possible.

My successful activity in the railroad case which I have mentioned, which led to many other similar juridical activities on my part, I owe, incidentally, to pure chance. The district attorney in the case of the Müllheim railroad accident, who is now Reichsgerichtsrat Justus Bender, is one of my friends. When I heard of the accident I addressed myself to him personally and pointed out [p. 185] that psychology was in a good position for explaining much in this connection; I was then named as the medical expert in this case.

My first general publication on legal psychology was my *Grundzüge der forensichen Psychologie* (München, 1913); my most recent work in this field is my little book on the Halsmann case (, 1932), in which I discussed in detail the essential significance of the psychological expert, of which I had already frequently spoken. In my work, *Der Psycholog als Gerichtsgutachter im Strafund Zivilprozess* (, 1926), I presented a survey of the legal activity of psychologists to date.

I attach particular value to my work on the psychology of accidents, to which I have devoted several essays. This work is perhaps best known through my little book, *Praktische Psychologie der Unfälle und Betriebsschäden* (Munich and Berlin, 1926), and again reveals my practical point of view. On the basis of the data of a large insurance company I was able to

show that the probability that a given person will have an accident varies directly with the number of accidents he has already had within a given time. Furthermore, on the basis of data from the German railways I was able to show that the probability that a given workman will cause damage will likewise vary directly with the number of times he has already caused damage within a certain period of time in the past. It was also possible to show that those people who frequently suffer accidents are the same ones who repeatedly cause them. Psychological analyses and experiments showed that the psychological prerequisites for the causation of damage and for special liability to accident are, on the whole, one and the same. These facts led me to the concept of the "*Unfälle*" ("accidenter") and to the recognition of the deep significance of the psychology of personality in the avoidance of industrial damage and accidents of all kinds.

Naturally, however, in my book on accidents, the psychology of personality in general also had to be discussed. I had already devoted attention to this subject in my essay in the *Festschrift für Robert Sommer* in the year 1925 (*Zeitschrift für die gesamte Neurologie und Psychiatrie*, 1925, **94**, pp. 359 ff.) and later treated it again and again, as in my detailed review, "Persönlichkeit und Aussage," given at a congress of the *Kriminalbiologische Gesellschaft* in Munich (*Mitteilungen der Kriminalbiologischen Gesellschaft*, 1931, **3**, pp. 89 ff.) I attach particular value to the concept of the momentary personality [p. 186] (*momentane Persönlichkeit*) and to the fact that, in spite of the great significance of the congenital factor and of the deeper levels of personality, the individual is nevertheless a different person as it were, each moment. The theory of modifiability (*Umstellbarkeit*), which is of great practical significance, which can be treated experimentally, and which is of importance to the psychology of accidents, among other subjects, my associates and I have discussed again and again. Individuals vary very widely in their ability to adapt themselves to new situations. To many people repeated adjustment of the personality seems desirable and pleasant. To others it is unbearable. I reckon among the more important results of my researches the establishment of the theory of modification (*Umstellung*), which I was able to use, among other places, in an article on homesickness (*Archiv für die gesamte Psychologie*, 1925, **50**, 512-524).

My practical attitude is again shown in my pamphlet, "Eignungsprüfungen für Rutengänger (Munich, 1927), and also found expression in the invention of various pieces of apparatus and series of experiments, of which my apparatus for shifting sectors (*Sektorenverschiebungsapparat*) has probably come to be best known. This apparatus, which I invented as a young physician during my stay in the Leipziger Institut of Wilhelm Wundt, and which I discussed in 1894 in the *Zentralblatt für Psychologie*, Volume 25, pp. 811 ff., makes it possible to change the sectors of a rotating disc during the rotation. I had heard that the Physikalisch-technische Reichsanstalt in was working on the solution to this problem, so I plunged into the question and completed my apparatus and published it considerably sooner than the Reichsanstalt finished its efforts. Today both sets of apparatus, which, incidentally, are based upon quite different principles, are widely used.

Always, however, my practical efforts have been closely related to my interests bearing on theoretical psychology. This connection, to my mind, is necessary. "The psychologist who stops his studies when they lead him to practical attitudes and practical measures incurs the just reproach of narrow-mindedness. But one who aims only at practical results lapses into a wretched technicalistic psychology (*Laboranten psychologie*)." This was approximately what I said at the Kongress der Deutschen Gesellschaft für Psychologie (at [p. 187] that time the Gesellschaft für experimentelle Psychologie) in (1925), amid the general approval of my auditors.

## II. BASIC ATTITUDE ON PURE THEORY. STUDENT PERIOD. ENTRANCE INTO THE FACULTY OF THE

The practical attitude which I have strongly emphasized so far, and which, of course, was associated with pleasure in practical results, developed very slowly in me and at present seems

to be receding again. When, at the age of 18, I passed my matriculation examination in my native city of i. and then served there as a volunteer for one year, during which I was registered as a student but could not study, my orientation was at first a purely theoretical one. Pure science was my ideal. At that time all practical activity seemed to me inferior. At first I had intended to study Germanistics and modern German literature, and during my first semester in after my period of military service I attended lectures primarily in philology, literature, and philosophy. At that time I was interested particularly in Old Norse, Middle High German, and Sanskrit. I was introduced to philosophy by the famous philosopher, Alois Riehl, who was then teaching in and with whom I had a two-hour seminar on basic philosophies of life. With the newer psychology, then only little known in general circles, I was occupied only incidentally in that I made the personal acquaintance of Hugo Münsterberg, who was at that time active as a Privatdozent in , and also attended his one-hour lecture course on hypnotism. Incidentally, it was something quite new to have university lectures on this subject, and Münsterberg was probably one of the first to lecture on hypnosis in an institution for higher education.

Toward the end of my student period in Freiburg I asked my instructor in Germanistics, the philologist, Hermann Paul, who was also then working in Freiburg and who later became very famous, where I ought to go to continue my studies, as I had no desire to go on studying at home indefinitely. Paul advised me quite decidedly to go to Halle, where the professorship in Germanistics was held by Sievers, whom he esteemed very highly and who later became known to the psychologists through his investigations (to my mind entirely erroneous), or rather his assertions, on the psychology of language and of music.

[p. 188] Sievers and his students taught that every author is characterized by a special voice quality (*Stimmqualität*) and that absolutely every work created by man in the field of sound, whether language or music, is dominated by certain sound constants (*klangliche Konstanten*) which are of basic importance in the character and effect of the sound product (*Klangwerk*) and show large individual variations. Sievers claimed that it was possible, on the basis of difference in voice quality alone, to decide whether a given piece of prose or poetry was to be ascribed to one author or two different authors. Never, however, did Sievers conduct real experiments with which he might support this theory, which at first found many adherents but then decidedly lost in interest. At the Kongress für Aesthetik und allgemeine Kunstwissenschaft in in 1914 I first made Sievers' acquaintance. Not only I but also the psychologist, Carl Stumpf, and others opposed his theory at that time.

I did not follow Paul's suggestion that I transfer my studies to . I wanted not only to study but also to enjoy the world and life, and therefore, at the end of April, 1890, I went for one semester to , which had been described to me as one of the loveliest of the German university towns. Here I continued my studies in philology, but also came in touch with Götz Martius, a student of Wundt's, who was a Privatdozent at at that time and who had been mentioned to me by Münsterberg. At that time Martius was making all sorts of reaction experiments, in which I helped him and which increased my interest in modern psychology. I also attended his seminar on Spinoza, which deepened my philosophical interests. I also began at that time to read other philosophical classics besides Spinoza, and I continued this reading during the subsequent vacation and during my whole student period.

During the following Winter Semester, 1890-1891, I studied in , having meanwhile decided to become not a germanicist or a literary historian but a philosopher. I still remember quite definitely that, upon my arrival in , I wrote to a friend that I had now decided to devote myself entirely to philosophy, which had always interested me. How I came to this decision, however, is hard to state in a few words. Certainly I did not make it in the course of a few days or weeks. Rather it undoubtedly matured quite gradually. At that time I considered myself a young man who was rich or at least very well-to-do, and in a certain sense I really was. [p. 189] For being rich consists not in having an income of \$30,000 or more but in having an income which exceeds one's needs. At that time not a soul dreamed of the losses of income which we Germans were to incur as a result of the War and inflation. And the thought that I would ever have to or wish to live on the income of a professor was quite foreign to me. The reason why I turned first to the study of philology and literature lay in the fact that one of my teachers at the

Gymnasium at had especially aroused my enthusiasm for these things. Philosophy, in which another teacher had interested me, I had regarded only as absolutely necessary to the intensive study of literature. It gradually interested me more and more, however, and all other scientific interests receded proportionately into the background. Thus my subsequent intention of becoming a philosopher did not represent a real transfer to another field, but rather only a gradual redirection of my interests. Also, when I came to , I naturally was not thinking of choosing psychology as my special subject. At that time in there was probably no one who held the view, which I stressed strongly later on, especially at the Congress of Psychologists in in 1921, that psychology should be separated from philosophy as a distinct field. And when, in as previously in and , I did do some work in the new psychology, which at that time we called experimental or even physiological psychology, I did this only because, in my opinion, this, too, belonged to philosophy.

My psychological studies were conducted under the guidance of Hermann Ebbinghaus, who was at that time Professor Extraordinary in . I studied philosophy under the famous historian of ancient philosophy, Eduard Zeller. I also attended additional lectures, in part regularly and in part as an auditor, in order to further my general culture, which I regarded from the first as fundamental for a philosopher.

In the summer of 1881 I returned to , where I studied mainly with the three regular professors of philosophy, the Kantian Jürgen Bona Meyer, J. Neuhäuser, the authority on Aristotle, and Wilhelm Bender. Bender was an evangelical theologian who had been moved out of the evangelical-theological into the philosophical faculty because of his very advanced views. Through Neuhäuser, who, incidentally, was a scholasticist and thoroughly clerical in his orientation, [p. 190] and through student friends of mine in the field of classical philology I was stimulated to extend my philosophical reading to ancient philosophy and to do it more in the original. I cannot say that, except in this one respect, I received very much stimulation from the three teachers named above. It would be more accurate if I said that the fact that I did not allow them to frighten me away from the study of philosophy shows that my interest in philosophy was very active at that time.

At that same time in I also attended the big laboratory course in physics under Heinrich Hertz, the discoverer of the electric wave, and his assistant at that time, Philipp Lenard, later the famous Heidelberger physicist, while I frequently used my vacations, which I usually spent in , to work in a private chemistry laboratory there. In I also attended the lectures in physics and chemistry by Hertz and Kekulé. It was thus not until this second period, which lasted up to the time of my doctorate examination (1893), that I began to study the natural sciences.

During this period I again worked with Martius, under whose guidance I had, as a student, already written an article on the fluctuations in visual sensations (*Philosophische Studien*, 1893, **8**, 615-637) and in whose private institute I also wrote my doctor's thesis, "Zur Lehre von Gesichtsempfindungen, welche aus successiven Reizen resultieren" (*Philosophische Studien*, 1894, **9**, pp. 384 ff.).

The article on the fluctuations of visual sensations bore upon a problem treated several times by students of Wundt under the head of fluctuations of attention. I was able to show experimentally that these fluctuations are not really fluctuations of attention, but rather fluctuations in sensation. That a philosopher should hand in as his dissertation an experimental study of successive stimuli and visual sensations at that time caused some surprise and shaking of heads. It did not do me any harm, or at least not much, in connection with my receiving the degree, for I received it with the designation "*insigni cum laude*," not frequently given in at that time. I was examined for an hour each by Jürgen Bona Meyer and Neuhäuser in philosophy. Just before this examination I had to take the *Magisterexamen* (master's examination) in mathematics, natural sciences, history, and classical philology, each for one-half hour. This utterly senseless arrangement, which kept many [p. 191] people from taking their degrees in , and by which of course any half-way thorough knowledge of the subjects was

not to be expected, has meanwhile been quite abolished. In classical philology I was examined by the famous philologist, Bücheler, who put before me the *Ethics* of Aristotle, which I was easily able to master, as I had read more difficult writings of Aristotle.

Perhaps I would have handed in, in , not an experimental but a purely philosophical piece of work as a dissertation if I had been more strongly attracted to the teaching of the three regular professors of philosophy mentioned above. Perhaps I might not have remained in at all if I had not been held by certain personal relationships to which I will revert further on.

During my second stay in I also studied more closely the writings of W. Wundt, to whose *Grundzüge der physiologischen Psychologie* my attention had already been directed by Münsterberg in . Wundt's combination of medical and specifically physiological, psychological, and general philosophical interests, and his great literary productivity impressed me tremendously at the time. I read many other modern philosophical authors also during my student period in . It was always, however, the association of universality with specific scientific achievement which I valued particularly then. A comprehensive view over the whole realm of science and considerable positive research and achievement in special fields seemed to me to be the distinguishing marks of a great philosopher. I was not yet capable then, since I still lacked independent judgment, of reaching my later opinion that W. Wundt frequently erred as a positive investigator. Another whom I counted among the most universal and important thinkers of the new era was Eugen Dühring, whose writings I have probably read *in toto*, who was active in political economy, mathematics, and natural science, and of whose weaknesses I was likewise not aware at that time. That I was still woefully lacking in universality I did recognize, however. To acquire it more fully than I had yet been able to do and to work first of all in the field of modern psychology in a scientific way was my next plan.

I therefore went for a year to , where I found much stimulation in Wundt's Psychological Institute, in which I prepared many problems for my future research, and where, incidentally, I was inscribed as a medical student and now also studied anatomy and zoölogy.

[p. 192] Returning to for a third stay, I worked there in the Zoölogical Institute. I also wrote my faculty habilitation thesis (*Habilitationsschrift*), to the contents of which I had already given much thought in . My two studies and the little article, already mentioned, on the apparatus for shifting sectors (*Sektorenverschiebungsapparat*) were related to more comprehensive studies in the field of physiological and psychological optics. In connection with these studies and especially with my dissertation I developed my habilitation thesis, "Theorie des Talbotschen Gesetzes," which was later published in *Philosophische Studien* (Volume 12, pp. 279 ff.), and whose contents were again utilized later in the article, "Theorie der kinematographischen Projektionen," likewise mentioned above. I showed that Talbot's law and all the facts connected with it can be deduced from very simple physical and mathematical considerations.

With this piece of work I entered the faculty in Würzburg as a Privatdozent in the entire field of philosophy. In order to enter I had to discourse upon a purely philosophical theme, the Freedom of the Will, assigned to me three days in advance, and to defend twelve theses, proposed by me, which belonged almost exclusively to philosophy proper, and which, I must admit, make a very childish impression upon me when I read them through again today.

The reasons why I chose to teach in Würzburg were personal. In and I had made the acquaintance of a group of young, active psychologists and philosophers to whom I was indebted for much stimulation. Among my fellow students in were Arthur Wreschner and William Stern, who are at present working in the Universities of Zurich and , respectively. Among those studying in Leipzig at that time, with many others whose acquaintance I made, were the present Professor of Pedagogics and Philosophy in Freiburg i. Br., Jonas Cohn, the present Professor of Psychology in Turin, Friedrich Kiesow, and also the Pedagogical Therapist, Theodor Heller of Vienna, famous today. I also established a very stimulating relationship with Ernst Meumann, who at his death was professor in and who, in my period, was Wundt's second assistant. I owed most, however, to my association with the young Privatdozent, Oswald Külpe, who in my time was acting as first assistant in the Leipzig Institute.



Külpe had uncanny industry and was, even then, better read than I [p. 193] have ever become. The fullness of his reading, which he was continually extending, spread not only over psychology and philosophy but also over the most varied other scientific fields. Külpe, with whom I ate lunch regularly in and whom I also saw much of otherwise, both inside and outside of the Institute, was always accessible for all questions I asked and for the explanation of all problems I attacked. The appreciation which I accordingly and inevitably felt for Külpe soon showed itself to be mutual, although I realized even then that he and I were quite different sorts of people and represented quite different types of young scholars with scientific interests.

During my third sojourn in , Külpe had been called to Würzburg as a regular professor (*Ordinarius*). And in view of our friendly relations and of everything which I owed him it was of course natural that I should seek my field of activity in the place in which he had gained a decisive influence upon instruction and investigation. Also, in view of our relationship and the lack of another private instructor, it was quite agreeable to Külpe that I should come to Würzburg, especially since he was making an attempt (in which he also succeeded) to establish a psychological institute in Würzburg and since he knew that he could be sure of my help and practical collaboration in this.

It should be clear from the details of this chapter that the practical attitude was really quite foreign to me at first. At most, one might see certain modest beginnings of a practical point of view in the fact that I helped Martius in a practical way to build his apparatus for the investigation of reaction time to sounds—a fact which he explicitly emphasized (Martius G., *Philosophische Studien*, 1891, 6, 403) and in the fact that I invented in Leipzig my apparatus for shifting sectors.

### III. LATER LIFE AND WORK UP TO THE PRESENT TIME

My first period of work in Würzburg, lasting for nine years, was the most pleasant period of my academic career. The teaching went very well. During the first semester (the Summer Semester of 1896) I gave a one-hour public course on Arthur Schopenhauer and, during the second semester, a private colloquium on the general history of philosophy. The next semester I taught Ethics. It was only gradually that I undertook the teaching of psychology also. The [p.194] teaching and my successes gave me much pleasure. At that time, already, as I do today, I attached particular importance to expressing myself clearly and understandably, while at the same time presenting as much material as possible. Külpe, who was himself a popular and respected teacher, was far from envying me my effectiveness; on the contrary, he seemed to rejoice in it almost more than I did.

Already, during my third stay in , I had devoted myself to a certain type of teaching with great pleasure. I not only helped Martius in his work in his private institute but also prepared many friends and other students who were working toward their degrees in for the doctor's examination. The situation was that everyone whose main subject was not philosophy (and it was but rarely that a philosophy major came to at that time!) had to be examined for half an hour in philosophy during the doctor's examination. Philologists, chemists, in short, everyone who wished to obtain the doctorate, thus also had to undergo an examination in philosophy. Since many students were naturally not at all suited to this field and since most of them had not attended philosophical lectures and moreover had not even covered the field, a philosophical coach for the philosophical portion of the doctor's examination was much in demand at that time. Since it was already pretty well known what the three regular professors of philosophy usually asked in the doctor's examination, and since they were also willing to have the students take a special, not too large field as the subject in which they wished to be examined, I did not find the coaching, which I regarded as a sort of sport and for which I never accepted pay, too hard.

Today, also, teaching has undeniable attraction for me, but my manifold scientific interests and other tasks somewhat limited the pleasure of instructing. My lecture courses today I usually enjoy only while I am on the platform, though I am as enthusiastic about teaching in the Institute and in seminars as I was before.

My scientific activity during my first stay in Würzburg was expressed in a series of publications on psychological optics, in which I entered partly into opposition with Götz Martius. I also pointed out at this time in an essay in Pflüger's *Archiv für die gesamte Psychologie* (1903, 100, pp. 551 ff.) that Talbot's law and all facts connected with it are valid and must be valid in the [p. 195] field of acoustics also. That during my first stay in Würzburg I also did research in the psychology and the esthetics of language I have already stated above.

In my work, *Naturphilosophische Untersuchungen zur Wahrscheinlichkeitslehre* (, 1899), I approached a very different field. In school I had already become familiar with the elements of the calculation of probability. In the work mentioned above, I treated, as d'Alembert had done before me, the problem of the agreement of experience with the expectations to be cherished on the basis of the so-called a priori consideration of the probabilities. These studies I resumed in my two-volume work to be mentioned later, *Die Gleichförmigkeit in der Welt* (München, 1916-1919), in which I was also able to utilize my work with Thumb, *Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung* (Leipzig, 1901), mentioned above, and other later publications of mine connected with it.

In my work with Thumb, belonging in my first Würzburg period, it was shown for the first time that the association reactions of a rather large number of subjects in response to the same stimulus word were to a large degree the same. There are always the most preferred, next preferred, etc., reactions, and finally some which are purely idiosyncratic (*auseinanderfallend*). There was also revealed the law of familiarity (*Geläufigkeitsgesetz*) according to which, on the average, the more familiar a reaction is, i.e., the larger the number of subjects, relatively, in whom it occurs, the more rapidly it occurs. These and allied facts have found their way into many other writings and have also led to the so-called association test, since it has been found that it is possible to draw certain conclusions from the fact of whether a subject does or does not conform to the general regularity of the process of association. Linguistically the work showed that the words which in the history of language have influenced each other in the direction of linguistic analogy formation are at the same time those which show themselves to belong together on the basis of the association experiments.

My strongest influence, however, during my first stay in Würzburg was probably exercised through my work, *Experimentellpsychologische Untersuchungen über das Urteil (Eine Einleitung in die Logik)* (, 1901). Studies in logic had shown me that the logicians have widely varying opinions on the psychological nature [p. 196] of judgment. I therefore determined to investigate the nature of judgment from a purely psychological point of view. I requested several trained subjects, under quite definite conditions set by myself, to make judgments and to give accounts of the conscious processes which occurred while they were judging. The large amount of material which I obtained in this way showed that all the prevailing psychological theories of judgment, including W. Wundt's—that it is an analysis of a total concept into its parts—were erroneous. In fact, I came to the conclusion that no psychological criterion of judgment existed. Many judgments of the utmost practical importance seem to take place quite reflexly, as it were, or automatically. The merchant or the mathematician who adds 2 and 7 and obtains the answer 9 carries out this judgment process without the presence, or need for the presence, of experiences particularly characterizing judgment. Many psychological considerations and experiments which had paved the way for this work also led me at that time to the concept of "conscious set" (*Bewusstseinslage*). It appeared that a reduction of the whole conscious life to sense perceptions, memory images, and feelings of pleasantness and unpleasantness, such as was the rule at the time, was entirely inadequate, and that quite different experiences exist which are not, or are not yet, accessible to analysis, and which I designated as conscious sets. In my work at that time I discussed the conscious set of doubt, of uncertainty, of expectation, of surprise, of agreement, of recognition, and many other conscious sets. The "feelings" of tension and relaxation and other experiences described by Wundt as feelings were accordingly classed in my work under the head of conscious sets. More obvious conscious sets, too, such as the recollection of conversations, were deduced by me from my experimental records. Others have subsequently used the expressions "consciousness" (*Bewusstheit*) and "thoughts" (*Gedanken*) for the whole or parts of what I designated as conscious sets.

This essay, in which the consciousness of understanding, also, was explicitly discussed on the basis of experiments and which dates the beginning of my opposition to Wundt, who until then had always been very fond of me, is the first work in which the psychology of thinking is systematically and experimentally treated. That its results are by no means all negative, as someone recently stated them to be, is already shown by my theory of conscious sets. [p. 197] My methodological procedure was likewise entirely new. That there cannot, however, be a positive criterion of judgment follows from the fact that the concept of judgment must be conceived as a logical and not as a psychological one. As I have explicitly shown in the essay referred to, whether conscious phenomena should or should not be treated as judgments depends upon the meaning (*Sinn*) we attribute to them, but not upon their psychological structure. This meaning may be but is not necessarily given in consciousness in the form of conscious states (*Beswusstseinslagen*). Also, the value of a judgment for science and for practical life is in no way dependent upon the conscious processes which represent the judgment, but solely upon whatever we wish to express through the judgment, although of course this desire for expression need not necessarily be really conscious at every moment. The merchant into whose calculation the proposition "twice two is four" blends quite automatically is, of course, making a judgment just as truly as is the A-B-C-scout (*Abc-Schütze*—first grader) who painfully works out this proposition.

This essay led Külpe to assign to Mr. H. J. Watt and Mr. A. Messer further subjects in the field of the psychology of thought. I was somewhat vexed by this, perhaps somewhat unjustly, and did not like to see the psychology of thought, which at the time lay very close to my heart, being led into fields in which I could not make my full influence felt. When, in the Spring of 1905, before Watt and Messer had yet completed their work, I responded to a call to Frankfort, K. Bühler, too, came to Würzburg, and worked on the psychology of thought afresh, entirely under Külpe. Also, Külpe assigned his students still further studies in the psychology of thought. Finally he, too, first in the Fifth Congress of the *Gesellschaft für experimentelle Psychologie* (1912) in Berlin, then in the *Internationale Monatschrift für Wissenschaft, Kunst, und Technik* (also in 1912), and finally in his philosophical book, *Die Realisierung* (Leipzig, 1912), took a stand in regard to the psychology of thought. He finally spoke of the "monarchistic arrangement" of our consciousness and said: "The Ego sits upon the throne and carries out governmental acts. It observes, perceives, and ascertains." Although I had previously intended to do so, I did not work specifically any more on the psychology of thought, but in an essay (*Fortschritte der Psychologie und ihrer Anwendungen*, [p. 198] 1915, 3, 27) I did reject, sharply and on the basis of explicit reasons, the course which the psychology of thought was taking under, after having previously repeatedly expressed my concern to him verbally, without success even though without actual contradiction on Külpe's part. For this essay, which had been directly stimulated by the avowed performances of Külpe and which appeared shortly before his death (1915), Külpe never forgave me.

In this essay I also emphasized my independence of Külpe in this field. Much as I owe him, I have never been his student. I never attended a single lecture of his and never took part in the laboratory work which he directed. I was never his assistant and never worked in fields which he had suggested to me or in which he had previously worked himself. And if I have ever been independent in anything it has been in having arrived independently at the idea of applying the method of systematic introspection (*Selbstwahrnehmung*) to the study of logical thought. Although Külpe acted as a subject in my experiments on the psychology of thought, this fact showed only that he was interested in this work, as in all my work at that time.

Taking over ideas from other people to develop them has never been my interest. It is only in independent work that I really feel at ease; from the start I have emphasized the attainment of new facts, trying at the same time to pursue the study of psychology in as exact a way as possible. I was animated by this striving when I tried to place the psychology of thought upon an experimental basis. When a younger psychologist wrote that the striving after precision was the weakness of my school, I gladly accepted this reproach, since I regard it as the highest praise by which our work can be distinguished. I necessarily disapproved, however, when

another psychologist, in the course of a scientific discussion, attempted to disparage the direction of my researches by using the expression "fact finding" (*Tatsachenforschung*). In my opinion, theories have a place in science only in so far as they explain facts or have a heuristic significance for the obtainment of new facts.

I naturally regret very much that I displeased Külpe in the essay I have mentioned in the *Fortschritte der Psychologie und ihrer Anwendungen*. Perhaps after all I should have been more cautious in my choice of words, although, before having it printed, I had the essay read through by my wife and by a professional psychologist [p. 199] who now holds a regular professorship, and although I took into account, to a large extent even though not completely, the mitigations of expression which were suggested to me. For the rest, Külpe, when I read him the most important parts of the manuscript of my essay, *Experimentelle-psychologische Untersuchungen über das Urteil*, with which I desired to found a systematic psychology of logical thought, was at first very enthusiastic. The fact that he then turned aside from me with his school was undoubtedly due to the circumstance that, when Külpe was further developing the psychology of thought, I was no longer in Würzburg but in Frankfurt, and that he now yielded to other influences than mine.

One of the reasons why I found my first stay in Würzburg very satisfying was that, in agreement with Külpe, I was able to stimulate independently some work by students, to guide it, and to lead it to conclusion. The fact that the number of auditors at my lectures declined after a few semesters and only gradually increased again I cannot regard as evidence against my statement that I was a successful teacher.

This decline is to be explained as follows. I was baptized and brought up as a Catholic and bore a name which had a very good sound in Catholic-ecclesiastical circles. An uncle of mine was a Central representative in the Reichstag and in the Bavarian Diet. I myself had gone beyond any Catholic or even generally religious attitude, since my school days. My name, however, attracted at first a large number of incipient Catholic theologians to my lecture room. These soon perceived, however, that I was not, as they had probably at first expected I would be, a representative of the Catholic attitude toward the world.

My leaning toward complete freedom also became known far and wide through an occurrence which is not without interest. In a public lecture on the problem of causality, I had occasion to discuss in great detail the cosmological evidence of God, and also to characterize briefly the other arguments for God. In this connection I contested the stringency of these arguments (in the Kantian manner) without otherwise entering into the question of the existence of God. In March, 1898, one of the many rural and priestly representatives then belonging to the Diet made a long, spready speech culminating in the assertion that a teacher in one of the Bavarian upper schools had said before his class, "We do not need [p. 200] to discuss the evidence for the existence of God, for there is no God." This reputed statement was discussed in detail everywhere by the speaker himself and by several of his colleagues. It was said that evidence of the existence of God had been suppressed, and that there had been dishonesty, pedagogical tactlessness, and other nice things. Only v. Landmann, the minister of public worship and education, who likewise was close to the centrist party, maintained that such debates never led anywhere. Since the aforementioned attacks of the representatives bore upon my person and my lecture on the problem of causality, he then had me make a written report on the occurrence. At the same time, all available students of mine were questioned on the content of that lecture. This questioning led to the finding that neither this statement nor any statement of similar meaning had been made in my lecture. Thus, all the gossip in the Diet resolved itself into idle talk at the cost of the Bavarian taxpayers, and the instigator of it, after being sharply reprimanded, was obliged to make a public acknowledgment of his mistake. One of my auditors had told a clergyman about the content of the lecture in question, and then, through the spreading of gossip (according to the well-known facts of the psychology of rumor),

there developed a statement which was entirely nonsensical and almost the opposite of what I had said. Although the matter was thus settled very favorably to me, I was nevertheless branded in the eyes of many who had originally been well disposed toward me. Auditors with Catholic tendencies all removed themselves from me at first. They were replaced by others, and in the course of the following semesters confirmed Catholics also again appeared at my lectures.

During my first stay in Würzburg, I also began to work into the field of pedagogy. Even in my attention had been called, through talks with Meumann, to the possibility of applying modern psychology to pedagogy. Also, Külpe had suggested to a student by the name of Johann Freidrich a thesis on the effects of duration of work and pauses in work on the mental achievement of school-children. This study later appeared in Volume 13 of the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane* (1897, pp. 1 ff.). I had also obtained some information from other experimental-pedagogical studies. At that time I still knew very little about these matters, however. It was not until I came under the influence of Meumann's book (*Vorlesungen zur Einführung in die experimentelle [p. 201] Pädagogik und ihre psychologischen Grundlagen*, 1st ed., Leipzig, 1907) that I acquired a stronger interest in psychological questions in the field of education and instruction. And later I not only assigned to my students many dissertation topics relating to psychological pedagogics but I also worked in this field again and again. Thus, in the study already mentioned on psychotechnical and practical ability, I showed to what a great extent the achievements of children in the same grade at school depend upon their chronological age. Finally, in association with Ludwig Sell (*Zeitschrift für Psychologie*, 1931, **122**, pp. 177 ff. and pp. 188 ff.), I also studied the dependence of school achievements upon the environment and upon the occupation of the parents of the children.

Still less did I understand, during my first teaching period in Würzburg, of the history of pedagogy, which I had not yet studied at all. The historical study of pedagogy and with it my general pedagogical interest were well set on the road, however, by an external circumstance. The Würzburg professor of pedagogy, Lorenz Grassberger, who has become known through his work on education in ancient times, had retired from his teaching in the year 1901. The position could not immediately be filled. The history of pedagogy absolutely had to be given, however, at least for one semester of the year. At the end of the Winter Semester of 1901-1902, Professor Martin Schanz, then rector, known through his studies on Plato and through his history of Roman literature, came to me and urgently requested me to give a course in the history of pedagogy. My objection, that I knew nothing about the subject, was not allowed to prevail, and finally I consented. Immediately and during the subsequent vacation and during the Summer Semester of 1902 I read pedagogical classics and textbooks almost day and night, and in the same Summer Semester I gave a four-hour course in the history of pedagogy which, while certainly not the work of a trained professional, was nevertheless quite adequate.

In , to which I moved, as stated, in the year 1905, I had the opportunity, with large means placed at my disposal, to establish a Psychological Institute and a notable philosophical seminar. In addition to working on my soot method (*Russmethode*) I was busy with quite different problems there too. Specifically, I began in to work on my philosophical essays, which I published under the title of "Beiträge zur Logik und ihren Grenzwissenschaften" [p. 202] in the years 1906-1912 in the *Vierteljahrschrift für wissenschaftliche Philosophie und Soziologie*.

During my stay in Frankfort, where I also took a very active part in the upper-school affairs of the Academy, which was then evolving into a university, and where, as already stated, I was planning many practical applications of my soot method, my practical tendencies began, for the first time, to develop strongly—tendencies which subsequently, during my second stay in Würzburg, were even more strongly brought out through my legal involvement, the influence of American industrial psychology, and other agencies.

In , too, I stimulated various scientific studies by students. Here, too, I found satisfaction in

teaching, although the satisfaction was not as strong as it had been during my first, and was again during my second, stay in Würzburg. The auditors of the new upper school (*Hochschule*) consisted of young commercial high school students, high school teachers, high school superintendents, physicians, society ladies, and people from many other circles. Although I was able to be effective through my suggestions in the Institute, it was nevertheless very difficult [*sic*] to offer in the lectures something which should be equally satisfying to all the different elements of which my audience consisted.

I must not neglect to state that, in , at the age of 38, I married the painter, Milly Fries, the daughter of a manufacturer, who has very zealously continued her activity to this day, and who since her marriage has been exhibiting under the name of Milly Marbe-Fries. Although my marriage has remained childless, it has nevertheless been a very happy one.

Having returned to Würzburg in the year 1908, I worked (besides doing much other writing of which I have already spoken in part) particularly on my two-volume work, *Die Gleichformigkeit in der Welt* (Munich, 1916-1919), in which I discussed from the most varied points of view the problem of uniformity, to which I had been led by the consideration of probability and by studies on association. It had already been shown in my essay with Thumb that, the reactions of subjects in association experiments are concordant to a very large extent, and now I was able to show in addition that the behavior of human beings, when they are subjected to similar conditions, reveals striking similarities or uniformities to a greater extent than it was previously assumed to do.

[p. 203] During my second stay in Würzburg I also wrote my essay on animal psychology, "Die Rechenkunst der Schimpansin Basso im Frankfurter Zoologischen Garten" (*Fortschritte der Psychologie und ihrer Anwendungen*, 1917, 4, 135-186). This animal had brought thousands and hundreds of thousands of people into the zoo to admire its arithmetical skill. Before her lay a pile of little tablets upon which the numbers from 1 to 10 had been written. Then the caretaker said, for example, "Basso, how much is 10 minus 8?" whereupon the chimpanzee took a tablet into her hand upon which stood the number 2. The caretaker thought that he had systematically instructed Basso in arithmetic, but then became convinced that, after all, her achievements were due not to instruction but rather to thought transference. The superintendent of the zoo was not able to account for the behavior of the chimpanzee. And when, while I was temporarily staying in , I had occasion to see Basso at work, I too was completely at a loss. Through systematic investigations which the superintendent of the zoo permitted me to conduct, I was finally easily able to show, however, that with his questions the keeper combined involuntary and unconscious movements, on the basis of which Basso figured correctly. Thus what was here revealed was essentially the same as what had been established previously by Pfungst when he was studying Clever Hans, the calculating horse of Mr. von Osten in .

Subsequently I have used this study, in which I also treated more general questions in animal psychology, again and again as a point of departure of the emphatic statement that it is only through systematic experiments and not through mere observation that apparently mysterious performances of human beings and animals can be solved.

I have tried in many other writings, also, to reduce occultistic statements to their proper scope by means of an exact scientific attitude. I treated in this way the problem of the wishing rod (*Wünschelrute*), among other problems. Unfortunately, however, I cannot maintain that I have achieved anything in a practical way by my fight against occultism. The mob's craving for sensation, especially in periods of economic depression such as we have had to experience in since the War, is far too strong to be shaken by the sober expositions of a scholar. I found out, inevitably, in this as in other fields, that faith is much stronger than science.

It was not until the time of my second stay in Würzburg that I [p. 204] attained a following on a larger scale. A large number of studies of the most varied kinds issued from my Institute here. Slips of the pen, the problem of instruction in languages, the testimony of witnesses, the psychology of lying, musicalness, the psychology of the deaf and dumb, and many other quite

different matters were investigated by my students under my direction. Some of these studies have appeared in the five volumes of my *Fortschritte der Psychologie und ihrer Anwendungen* (1913-1922). Other studies have been printed in other journals, mainly in the *Archiv für die gesamte Psychologie*, in the *Zeitschrift für Psychologie*, and in the *Psychotechnische Zeitschrift*. About 90 doctors' theses and other publications by my collaborators have come out of my Würzburger Institute, not including the many studies by my assistants. My own studies have been published in psychological, philosophical, natural science, legal, and medical journals. Many studies by me which are not mentioned in the present article have been listed under "Marbe" in the "Psychological Register," edited by Professor Carl Murchison. Many have not been mentioned there either.

I have certainly come to realize more and more clearly that fame is not an advantage, but rather a disadvantage, and that in particular to flirt with the practical calls forth a mass of inquiries, correspondence, and studies which one would often be very glad to do without. Therefore, as already indicated above, my practical ambitions have gradually been receding. At present I am again turning toward philosophical problems, so far as other unavoidable tasks permit.

#### **IV. SUPPLEMENTARY REMARKS. PERSONALITY. YOUTH**

We may make acknowledgment when, as I have done, a scholar continually turns toward new questions and when he inspires efforts here and there and works on a great variety of problems. This type of activity can undoubtedly also be criticized, however.

A professor on the staff of one of the three high schools in which I was working once criticized a habilitation essay before us because it gave the impression of being a piece of occasional writing rather than a study resulting from systematic work. In a certain sense, I can say that all my writing has been occasional writing (*Gelegenheitsschriften*). Almost always I have been guided by certain notions or ideas which I obtained in the course of my reading, conversation with scholars or men of practical life, or other experiences, [p. 205] and I have never been able to decide to study a field for the sake of writing an essay or a book about it. If some few of my writings have resulted from stimulation by a third person, I have responded to this stimulus only when I believed that these writings offered me the opportunity of expressing in action, or at least of publishing, ideas that were in my mind.

It is clear that I cannot treat a subject successfully unless it interests me strongly, and I have no talent for what Herbart calls an even distribution of interest over everything such as is involved in large systematic studies. Today I have even reached the point of basing my study of scientific literature almost solely upon my own ideas and investigations and, of course, also upon the requirements of my teaching. Clearly, the urge to investigation, the wish to have an effective influence upon science and life, and the aim of being able to offer my students the material they need have become the exclusive, or almost the exclusive, deciding factors in my work.

I mentioned above that, as a student, I regarded universality and achievement in special fields as the principal earmarks of the great philosopher. Undoubtedly, I have achieved a certain modest universality in that I have extended my studies, more than it is customary to do, over the most varied fields. To deepen and broaden this universality, however, as Herbert Spencer and Wilhelm Wundt did, for example, and on the basis of this to progress to systematic works on a grand scale was not granted me, because I lacked this even distribution of interest. The wish, too, to bury myself in subjects which held my interest, even when they were not of general interest, hindered my development into a universal philosophical author. When I had once set myself a problem, my desire was to exhaust it to the extent of my ability, and to touch upon a thousand questions, gaining only a literary mastery over them, has never appealed to me. And, lastly, I have also recognized that the authors of large systematic works have hardly ever been able to avoid superficialities in detail which I have always personally strenuously repelled, although I do not on this account reject work of a more encyclopedic trend.

When Külpe saw, in Leipzig, that I kept on working again and again on the construction of the apparatus for shifting sectors (*Sektorenverschiebungsapparat*) and the outlines of my theory of Talbot's law, he remarked, without meaning to reprove me, shaking his head, that that type of activity was impossible to him. And indeed he and I were quite different scientific personalities.

[p. 206] I ultimately noticed that I was much too critically minded by nature to make a universal writer on philosophical subjects. In the early period of my activity I wrote a large number of reviews and also some very sharp critical essays. As I have already stated above, my critical attitude brought me into opposition with Martius, Wundt, and also Külpe. Other oppositions, too, have arisen through my critical attitude. Although today I avoid polemics and no longer enjoy the writing of criticisms, I undoubtedly still have, even now, a very critical attitude toward others, and also, if I am not very much mistaken, toward myself. This critical attitude, too, was a hindrance to the writing of large systematic philosophical works.

And, in conclusion, writing itself, even to this day, has never come as easily to me as it does, and must, come to the productive writer on philosophical subjects. Rather, I am still inclined, even now, to reflect on almost every word which I write down or dictate.

In speaking of my critical manner, I am far from regarding it as a pure asset. "You always emphasize only what separates us and never what unites us," the philosopher, Heinrich Rickert, once said to me when I had personal association with him for a time and had explicitly criticized his book, *Die Grenzen der Naturwissenschaftlichen Begriffsbildung (Part I)*, in the *Zeitschrift für Philosophie und philosophische Kritik*, and had taken a critical attitude toward his views in other places also. He was not far wrong in his observation, and in it he showed, perhaps unintentionally, that critical endowment also constitutes a weakness to a certain extent.

Just as I could not manage to write large systematic philosophical works, I could not manage to write philosophical textbooks or other kinds of textbooks, although I consider the writing of good texts, which, in the subjects I have taught, are decidedly lacking, in my opinion, as exceedingly meritorious work. To begin with, in view of my personality, I would find the writing of a textbook much too tedious. Historical problems, too, and also historical researches, have never fascinated me, although I have done much teaching of the history of philosophy and of pedagogy.

My desire was always to do concrete research, and, after attaining my practical attitude, to influence not only science but also practical life. When I had once made a subject the object of my research, however, I never ceased my investigation when my research led me beyond the borders of the subjects I officially taught in the [p. 207] high school. Thus I took particular pleasure in making my soot method (*Russmethode*) useful in as many ways as possible. And thus also my studies in uniformity have led me far beyond psychology and philosophy proper and have made it possible for me to be influential in many varied fields, as for example in sociology. And it is solely because of my urge to keep on investigating when I was possessed by ideas on uniformity that my two-volume work, *Die Gleichförmigkeit in der Welt*, which is, of course, neither a systematic philosophical structure nor a textbook, became so enormous. In this I was also led into the field of mathematics. That I influenced several great mathematicians through this work could easily be proven. But that here, precisely in connection with mathematics, I was frequently guilty of errors should not be concealed but rather emphasized. In my latest essay, *Der Strafprozess gegen Philipp Halsmann* (, 1932), I did not hesitate to use legal questions also, particularly in connection with Austrian jurisprudence, as subjects of my discussions.

It is possible to advance the cause of philosophy by large systematic works and by textbooks, but also, of course, by criticisms and by attacking special problems, as I tried to do particularly in my book on uniformity in the world and in my essays in logic and kindred sciences. If, however, I compare my philosophical writing with everything else that I have written, it occupies



only a modest space, especially in comparison with psychology. That I have applied myself so extensively to psychology is not due, any more than was my transition from the history of literature to philosophy, previously discussed, to very definite resolutions. Psychological questions simply gradually came to interest me more than the actual philosophical ones. And the more I worked myself into the field of psychology the stronger my interest in this subject became.

Naturally, however, the form of my activity was determined not only by my innate personality but also by the stimulation that I received from all sorts of directions and which I obtained particularly from scholars with psychological interests. In addition to those cited above, I must mention here Alfred Binet, in whose Institute I worked immediately after my doctorate examination during a long vacation in , and with whom I came into friendly relation. Incidentally, even today I regard myself as a psychologist only in that I have worked primarily in the field of psychology. Just at present, [p. 208] as I have already stated, I am again engaged in philosophical studies. That I may ultimately adopt quite different subjects as the object of my interest, however, is by no means out of question on principle so far as I am concerned. I have long recognized, however, not only through scientific psychology but also on the basis of ordinary common sense, that my capacities are not as many sided as, in the frenzy of youth and in my first enthusiasm for science, I had foolishly taken quite for granted that they were. When I began to study, I did not even consider the question of capacity at all, and at that time I overvalued the importance of the will in relation to achievement to an almost unbelievable extent.

There are many other things, too, done or not done in the past, which I am compelled to criticize today. Although I certainly made my studies cover many fields, I nevertheless omitted important matters. Thus, for instance, I regret today that I did not study psychiatry more thoroughly from the start. While I was a student in Bonn, the well-known psychiatrist, C. Pelmann, was giving well-liked lectures on borderline states, which he also discussed in his book *Über die Grenzen zwischen psychischer Gesundheit und Geistesstörung*. It did not occur to me, however, to attend either these or other lectures on psychiatry. It was not until later that I made myself familiar with psychiatric topics, utilizing for this familiarization (not until a few years ago, either) a rather long period of study in an asylum for the insane. Undoubtedly, it would have been much better for my development if I had earlier occupied myself more with psychiatry. It is true, however, that in my time other young psychologists were not thinking either of the importance of psychiatry for our field. I have also regretted very much that during my entire student period I did not come closer to jurisprudence, with which I did not come into more intimate touch until the time of my expert testimony in the Müllheim railroad accident case and in which, since then, mainly under the guidance of my highly esteemed Würzburg colleague, the criminologist, Friedrich Oetker, I have done considerable work. While I was studying, my opinion of jurisprudence, Heaven knows why, was much too low. Today I do not doubt that this important field would have held me spellbound if I had turned to it in the beginning, and that I might very well have found fulfillment in the study of jurisprudence too.

So far, I have spoken only of my studies and my scientific work [p. 209] and never of *not* studying and *not* working. The reader would receive an entirely false picture of my personality, however, if I did not also bring out that I have not been, and am not, oriented toward science as exclusively as might appear from the discussion thus far. Just as, when a student, I chose the not for scientific reasons but because I wished to live by the beautiful , so during my student period and later I also had a great many other extra-scientific interests. For decades I was particularly absorbed in hunting. Even as a schoolboy I was sometimes taken along by hunters and later I spent a large part of my life in hunting. Apart from the fact that I followed hunts and accepted invitations here and there, I rented, sometimes alone and sometimes with other hunters, lovely hunting grounds (especially on the upper Rhine), in which I felt extremely happy, since unhindered wandering in the midst of beautiful nature, but above all the passion for hunting, had a strong hold upon me. For months I lived on the hunting grounds, where I thought almost exclusively about game and dogs. Here I often used practically to forget that I was also a scholar and a professor, and it was a great annoyance to me to receive letters and packages here which reminded me of my profession. Temporarily, to shoot well seemed much more important to me than to do good scientific work. And often, when in the Fall I returned

with my guide from the hunt and we were both so laden with partridges, pheasants, and wild ducks that our upper bodies had become almost invisible, or when I had shot a fine buck or a rare piece of game, I was prouder than I could have felt if I had invented perpetual motion. But a strange thing happened. About ten years ago I had so many professional duties for a time that I did not have a chance to hunt, and all at once I had lost all of my great interest in hunting. And yet formerly I would never have thought it possible that I would be doing scientific work and even giving lectures longer than I would be hunting.

I had the same experience with many other interests. As a schoolboy I did a good deal with music. I learned to play the piano and at the age of 16 began to play the violin too. Toward the end of my school period I also took courses in the theory of music, in harmony, and in counterpoint. Then and later I hardly missed a concert which seemed to me at all important. But these music interests too, although they have certainly not been completely [p. 210] lost, have given place, in the main, to others. For a few years I was very enthusiastic about difficult mountain and glacier climbing, which I later regarded as superfluous. Toward the end of my schooltime I felt very sympathetic toward the drinking customs, the societies, and other practices of German students, and my school work at that time was much less important to me than the imitation of these things, which at that time were current among the pupils of the upper high school classes in university towns. But by the time I had finished my military service I was no longer enthusiastic about these student activities. In , it is true, I joined the so-called Bonner Kreis, in which I established many relations of personal value, but this club was not a student society in the usual sense. It was rather a club with many decades of existence, officially quite unknown and externally quite inconspicuous, made up of young people with serious scientific interests, particularly classical philologists, who were held together solely by a certain tradition and not by any sort of statutes or regulations of that kind, and who, within the widest limits, permitted each individual to pursue happiness in his own way. The very advantageous scientific stimulation which I received here and the friendly relationships which I established, and which, as I indicated above, partly were influential in directing my studies into the field of ancient philosophy, have led me back to Bonn again and again.

I cannot, of course, name here all the places and persons to whom I owe scientific inspiration. I have already mentioned my colleagues in Nürnberg and . Other sources have also been cited. By far the largest debt, however, is the one I owe to the , in which to this day, in all the faculties, I have found men who were interested in my problems and from whom I have learned much. Many advantageous relationships have been established by correspondence and in national and international congresses. My students and assistants, too, have encouraged me again and again. Among the latter may be cited Mr. F. E. Schultze, now regular professor in Königsberg, Mr. W. Peters, who migrated with me from Frankfort to Würzburg and who is now regular professor in Jena, Kurt Koffka, well known in America too, who was my assistant in Würzburg while I was teaching simultaneously in Frankfort and in Würzburg, O. Sterzinger, now professor in Gratz, my Nürnberg assistant, Ludwig Sell, and my present excellent assistant in Würzburg, [p. 211] the instructress (*Privatdozentin*), Maria Schorn. I must not forget, either, my technician, Joos, who has been with me for many years, who helped me particularly with the apparatus for the soot method, and who also migrated with me from to Würzburg.

When I was speaking of hunting and other matters, just now, I mentioned that my interests have often changed. This is not a striking fact in and for itself. I do believe, however, that this tendency to shift is stronger in me than in most other scholars. Clearly, I do not come as close to being all of one piece as do some of my colleagues, and I am ruled more strongly than they are by a certain restlessness, as is shown in my manifold activity, always attacking new problems, and in my disinclination to write large systematic treatises covering entire fields. It cannot be said, however, that I easily abandon views or experimental findings which I regard as correct, or that in general I am particularly inclined to vacillation in judgment. Clearly the changes in question relate particularly to personal values, which I assign sometimes to one and and [*sic*] sometimes to another scientific or extra-scientific activity. That the frequent change in my interests was and is accompanied by a change in my knowledge is obvious. While during the first part of my academic career I took great pleasure in often conducting practice classes in the translation of Greek authors, I have now, because of the regression in my knowledge of the Greek language, long felt myself incapable of this, and while as a student in Bonn, as the only

pupil of the well-known Sanskritist, Jakobi, I had to translate a text from German into Sanskrit once a week, I have now forgotten even the letters of the Sanskrit alphabet. I do not see any special disadvantage in these and similar instances of forgetting. We do not learn and work in order to retain everything. But the personality which is ours at a given time is essentially dependent upon those earlier experiences which have long sunk into the subconscious, as I have stated again and again in my writings on personality.

Although, of course, the history of populations runs a strictly logical (*kausal*) course, yet nevertheless historical development is also dependent upon what we call chance occurrences. Who would deny that the World War and misfortune that it has brought to was caused to a large extent by the politicians and other persons who then happened to be at the helm in and elsewhere? Who [p. 212] would deny that, in all the great decisions in history, accidents, that is, factors which in themselves seemed to be unimportant, have played a rôle? The same thing happens in the life of the small individual human being. And thus it has been with me, too. For, although my development was surely determined essentially by my innate personality, yet other factors, such as are called accidents, have also been decisive.

I was born in on , as the only child of my parents. My father, like my mother, was of German descent and was conducting in a business which consisted primarily of the exportation of French merchandise into and other countries. A few years after my birth, however, my father became ill. A few years later he found himself compelled to give up his business activity and to move to his home city of i. to rest. And a few years after that, when he was only 37 years old and I was only eight, my father died in . If he had stayed well as long as I, we would not have moved to for decades, if at all, and my whole development would thus have been different. My father said to my mother before he died that he thought I ought not to become a merchant, but that I ought to study. Well-educated people, he said, had a much lovelier life than business people, who were compelled to struggle day and night. Thus it came about that, after I had spent a few years in the elementary public school, I was sent to the Bertholdsgymnasium there, which, except for the public continuation school (*höhere Bürgerschule*) was the only upper school in . I was well pleased with this arrangement, since other people, though not my mother, had suggested to me that the high school student, the university student, and the member of a learned profession was a much higher being than a business man. When I had entered the "Gymnasium," however, I was told that I must work hard, or I would be sent to the continuation school. I am not able to judge today whether it was so especially fortunate for me that my father expressed that wish and that during my upper-school life I was not so careless or lazy that I was not immediately taken out of the "Gymnasium" and transferred to the continuation school. But I am definitely convinced that my entire development hinged essentially upon the type of education which I was given. This type, that is, the humanistic, undoubtedly tended to turn me away from practical life and to develop [p. 213] the theoretical attitude of which I spoke above and which was balanced only to a certain extent later on. A more detailed discussion of the course of my school life and adolescence is not likely to be of interest here. The only thing I would still like to emphasize here is the idea that my tendency to go my own way in science and not to attach any special value, in general, to the treatment of topics of the day has probably also been favored by the fact that I never had any brothers or sisters and that hence an essential basis has been lacking for adapting myself to others.

I have also failed in such adaptation in that I have failed to read the autobiographies of other psychologists that are already available before starting to write my own. It did occur to me, however, that in his valuable work, for which he deserves many thanks, Professor Murchison was not only collecting biographies but also seeking to further the psychology of personality, a thing which can certainly be done most successfully if each author proceeds quite independently. I shall study the remaining sections of the book with all the more pleasure on this account after my lines have been printed.

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**Note**

[1] Submitted in German and translated for the Clark University Press by E. Marion Pilpel.

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