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R. S. WOODWORTH

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CONTENTS

THE HEARING OF PRIMITIVE PEOPLES

AN EXPERIMENTAL STUDY OF THE AUDITORY ACUITY AND THE UPPER LIMIT OF HEARING OF WHITES, INDIANS, FILIPINOS, AINU AND AFRICAN PIGMIES

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CONTENTS

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THE HEARING OF PRIMITIVE PEOPLES

INTRODUCTION

In the pages which follow are presented some data relating to the problem of the hearing of primitive peoples. The study was made in connection with other experiments on the inferior races at the Louisiana Purchase Exposition in 1904. During the period of the Exposition, the writer, in the capacity of Assistant Superintendent of the Sections of Anthropometry and Psychometry, under the Division of Anthropology, in cooperation with Dr. (now Professor) R. S. Woodworth, who was his immediately superior officer, was commissioned to make a study, so far as practicable, of the mental and physical status of the alien races stationed on the Exposition grounds. In the arrangements for the tests, the entire problem of the hearing of these people was assigned to me—the ways and means of testing their hearing, together with the turn and scope the particular study of hearing should take.

When it came to the question of selecting the tests to be made, there was little of historical precedent to aid in making a choice. Dr. Charles Myers, in the only extended report on the hearing of primitive peoples extant, had examined three phases of hearing, namely: (1) The upper threshold of pitch, (2) the acuity for tones of medium pitch, and (3) the perception of interval. In addition to these three tests, a number of others, which might have brought out interesting and instructive results, suggested themselves to me. These related to space perception, tone memory and imagery, and clang preferences. The scope of our work, however, was subject to certain definite limitations.

In all, there were stationed at the Exposition in one capacity or another, something like one thousand individuals of various races, whom it might be possible to measure. There were two of us to carry on the work. One of two alternatives, consequently, must be pursued, (*a*) to restrict the number of measurements which should be made of each individual or (*b*) to confine the measurements to a few representative individuals and races. We chose the first in the hope that with relatively large groups some fairly definite information could be obtained. In consequence of this limitation of the number of measurements, it was thought advisable to confine the number of tests of hearing to three or four. After some consideration and advisement, I selected the three[1] which Dr. Myers had made on the Papuans.

So far as practicable in these tests of primtive peoples, I hoped to be able to present my conclusions in such terms that the data might, if desired at some subsequent time, be reviewed and the measurements compared. I have consequently attempted to express my data in definitely interchangeable units, that is in acoustical units which are wholly objective in character. Much of the work included in this report, for this reason, is wholly a matter of physics. It concerns itself with the graduation of the instruments employed, but, for purposes of a quantitative psychology, this phase of the problem is none the less important.

In this connection, I desire to express my indebtedness of one kind or another, to those who have particularly assisted me in the undertaking and completion of the study herein reported. To Professor Cattell, first of all, I am especially indebted for suggestions relating to the problem as my particular field of research and for many valuable and kindly hints and criticisms in the treatment of

[1] The instruments and methods with reference to the first two named, (1) the measure of the upper threshold for hearing and (2) the determination of the acuity for tones lying within the range of conversational speech, will be fully detailed in the proper place. With reference to the perception of interval, however, a word is necessary in this connection.

The usual method of testing the perception of a musical interval is with the aid of tuning forks. I took to the Exposition two König tuning forks, differing from each other by four full vibrations, the one having a vibration frequency of 512, the other 516. On one of the prongs of the fork of lower pitch I placed a metallic rider, which it was possible to slide up and down and which might be fixed readily by means of an ordinary thumb screw. Thus the pitch of the fork could be raised or lowered without occasioning any wearisome delays. It seemed impossible, however, to arrange the rider in such a way as not to alter the character of the tone which followed independently of its purely pitch character. With the rider attached, the two forks possessed marked characteristics of clang tint by which they could be distinguished, wholly independently of the feeling of a pitch difference. My subjects were repeatedly warned that they should neglect the individual peculiarities in the two tones and render a judgment based only on their recognition of a difference of pitch. It appeared that this even my most intelligent subjects were unable to do, for questionings always showed that the individual peculiarity of the tone of each fork had become fixed during the earlier moments of the test, when the difference in pitch would be so marked as to be easily observed by all the subjects. Such a method of conducting the experiment as that just indicated seems necessary with children and intelligent adults to impress the object of the test.

Since, with intelligent white subjects, it seemed impossible to secure satisfactory data, it appeared unreasonable to presume that anything could be gained from this test on the primitive peoples. This test, therefore, was abandoned altogether and the hearing tests confined to two, (1) simple acuity and (2) upper threshold of hearing.

the results. To Professor Woodworth, it is impossible to express the full extent of my obligations. He performed large numbers of the tests. He, chiefly, was instrumental in making the arrangements with the officials in charge of the several groups for having the natives brought to our laboratories for testing. His searching criticisms, encouragement and interest at all times during the months at the Exposition, and subsequently, in the work of graduating the instruments have been an unfailing source of inspiration. To Professor W J McGee, Chief of the Department of Anthropology at the Louisiana Purchase Exposition, I am indebted directly for my selection to carry on the work. His encouragement in the work also and his assistance and cooperation in having the peoples brought to the laboratories for measurement was of inestimable worth. To S. M. McCowan, Superintendent of the Chilocco Indian School, who had charge of the Indian Ethnological exhibit at the Exposition, I am indebted for the privilege of measuring the Indians of the School, and to Major William Haskell, U. S. A., for the privilege of measuring the Philippine Constabulary soldiery. For assistance in interpreting directions and otherwise aiding the measurements, I was under obligations to Mr. Inagaki, of Tokio, in connection with the Ainu, Reverend S. V. Verner, in connection with the Pigmies, Dr. William Newcomb, in connection with the Vancouver Indians, and to Mr. Cushman, in connection with the Cocopas. Finally, and in a more comprehensive sense, I am indebted to all those who offered themselves as subjects for measurements, the mention of whose names alone would require many pages. Though mentioned in this general way only, my feeling of thankfulness to them is no less sincere.

CHAPTER I

The Peoples

In all, I was able to secure hearing records, which were more or less suitable for use in making various deductions indicating individual and racial differences, from about four hundred individuals. These were distributed as follows: 156 Whites; 63 Indians; 137 Filipinos (Christianized); 10 Cocopa Indians; 7 Ainu from the Island of Hokkaido, Japan; 7 Indians from Vancouver's Island; 6 so-called African Pigmies; and 4 Indians from the region of Southern Patagonia.

The Whites.—The Whites whom we measured were those, for the most part, who strolled through our laboratories primarily to view the exhibits, but offered themselves as subjects for our tests, willing victims to be sacrificed, as we chose, in the interest of the furtherance of scientific truth. We examined altogether about 100 of each sex but many of the records were unavailable for my purposes, either because the subjects were too young to be used in comparative tests or, to be sure, because they had advanced too far in years to make data concerning their hearing of value in comparison with those of younger individuals of other races. Many of the individuals were graduates from colleges and universities, others were school teachers. A number of professional and business men and women helped make up the number. Indeed, for the most part the group was made up of intelligent people.

The Indians.—Except the Christianized Filipinos and Whites, the Indians constituted the most numerous group tested. However, it can not be said that the Indians measured formed a single group, for they were brought from regions as widely separated as the Vancouver Islands and Patagonia. They belonged to approximately fourteen different stocks, as may be seen by the groupings below.

The greatest number of individuals belonged to the Algonkian stock. Next in order came the Shoshones, then the Sioux, Pima, Iroquoian in the order given.

During the whole of the Exposition period there were kept at the Indian School for purposes of exhibition something like 77 Indians. Of this number we tested 27 males and 44 females. Of the males, 14 were full-blooded and 13 mixed-bloods. Among the females, but four were of true stock. The mixed-bloods were in all cases partly

Stock or Family	Tribe	Stock or Family	Tribe
Algonkian	Sac and Fox	Piman	Pima
	Shawnee		Opata
	Pottawattomie		Papago
	Piegan		
	Chippeway	Iroquoian	Oneida
	Kickapoo		Cherokee
	Cheyenne		
		Scattered Tribes belonging to as many stocks	Navajo
Shoshones	Comanche		Pawnee
	Chemehuevi		Silitz
	Hopi or Moki		Muskoki
			Payallup
Siouan	Sioux		Kwaguitl
	Ponca		Cocopa or Seri
	Otae		Tehuelche

white—that is, partially American, Scotch, French, German, Swedish, Spanish or Irish. Not a single individual among the Indian group, so far as we know, possessed a negro strain in his inheritance.

It may well be questioned whether any group of individuals so heterogeneously conditioned as was this, might, with propriety, be lumped and treated as if representative or typical of the race. Indeed, were we dealing with traits in which tribal and stock differences are marked, as for example, anatomical features, such a procedure would be wholly unwarranted. Sensory features, however, are subject to a smaller range of variation. Moreover, these Indians from the Model Indian School came largely from the Indian schools at Haskell, Chilocco, Genoa, Phœnix and Ft. Shaw. Many of the boys and girls were taken from their homes at an early age and boarded at the Indian schools where they were subjected to social habits, intellectual training and industrial occupations which are common to whites. For the most part, they conducted themselves as do the young men and women of our cities. So far as their attitude toward society is concerned, one could not detect anything that would point directly to their immediately native origin. Hearing tests, moreover, look to constitutional differences rather than anything that may be directly influenced by a social veneer, hence the culture to which these boys and girls had been subjected, we might with reason aver, would affect their sensory reactions only very remotely.

The membership of the Model Indian School was made up of individuals belonging to the first twenty-two tribes named above, excluding the Hopi or Moki people who were connected with a concession on the Pike. Obviously, the numbers are altogether too few to bring out tribal differences. All considered, therefore, it seems best to treat the membership of the Indian School as a single cultural group, nor does it seem necessary to indicate with reference to this group such physical and mental traits as mark off the various tribes of Indians aligned under the several stocks or families. Having lived for three or four years directly under the influence and training of an American civilization, the factors which might arise from differences in home life and ancestry are for the most part obliterated.

The Filipinos.—The 137 Filipinos whose hearing was tested all belonged to that branch of the Philippine soldiery known as the Constabulary. They constitute the local police of the Islands, being stationed in squads of eight in the different villages and districts, to preserve order. Inasmuch as the Constabulary is a branch of the local civil service and the remuneration is considerably in excess of that received for other manual vocations, the better element of the citizenship has been attracted to its ranks. Those brought to St. Louis were men in the prime of life, none older than thirty-five years or younger than seventeen. All had attended school to a certain extent at least. None was found to be illiterate or unable to write his name, tribe, and place of residence in the Islands. Many of the men were sons in well-to-do families who had attended the Spanish and parochial high schools and colleges found in the Philippines. Rather indicative of the scholarly habits of many of the younger men was their activity in acquiring our language. It was not uncommon to observe groups of men collected in some place apart with dictionary and grammar, assiduously studying English grammatical forms and usages.

In collecting the group for representation at the Exposition, it appears that the men were drafted in squads of eight from the various Constabulary regiments located in every part of the Archipelago, there being, in fact, eight from the Moro population of the Island of Mindanao. The tribes represented were the Tagalog, Visayan, Ilocano, Bicol, Macabebe, Pampanga, Pangasinan and other less well established tribes. It does not appear that any criteria of stature, strength and intelligence were used in selecting the individuals for representation. Two legs, two arms and two eyes were required. Besides, it was necessary for the recruit to understand enough Spanish to take the orders of the line in that tongue. Other than these, there seem to have been no prerequisites.

The Cocopa or Seri Indians.—The Cocopa or Seri Indians tested were all males. For one reason or another, sickness, timidity, indolence, the women of the tribe could not be induced to come to the laboratories where the measuring was being done. It was chiefly through the instrumentality of Professor W J McGee that the Seri Indians were brought to the Exposition.[1] As the result of a careful and painstaking study of the social habits, customs and physical characteristics of this interesting group of people, Dr. McGee speaks in the following words: "Isolated to a considerable extent on Tiburon Island (in northwest Mexico) these people have successfully resisted the innovations of the white man. To-day they still cultivate aboriginal crops by aboriginal methods. They are said to be of so low a grade of culture that they may be classed as just entering the stone age. Physically, the Seri are a gigantic people, perhaps not excelled in their physical proportions by any other known tribe. Force of circumstances has made them an agricultural people chiefly, though the Cocopa are also given to the chase." Their habitations are extremely crude and primitive. Coarse grass, branches, leaves or whatever may be most convenient are thrown upon a crude framework of poles for a roof, while the same sort of an improvised material serves for walls. Such a habitation serves illy the purposes of protection from either heat or storm. The Seri Indians are not as intelligent as the average of the Indian race. They are inert, unresponsive to new impressions, dull and stupid in the face of an untried problem, and succumb readily to a difficult situation. With the older members of the group, especially, our efforts to make them approach our tests intelligently had been almost baffling were it not for the very able assistance and encouragement of an intelligent native interpreter, a half-blood woman who very ably interpreted our directions to the several subjects, but even with this, in some cases, the task seemed hopeless. Auditory acuity measures, however, being extremely simple, less difficulty was experienced with respect to them.

The Ainu.—The Ainu, four males and three females of whom we tested, are a people of more than ordinary ethnological interest. Surrounded by peoples of yellow skin, scant beard and little body hair with a head covering of straight black hair, the Ainu are white (when free from dirt) and their bodies and faces so profusely grown with a thick coat of hair that they have been popularly described as the hairy Ainu. The hair, too, is brown and wavy rather than straight. They inhabit the Island of Hokkaido or Yezo of the Japanese group. Little is known of the people's origin or ethnic relations. Until recently they had been little disturbed by other peoples, even by the Japanese among whom they dwell. While

[1] Professor McGee made some extensive explorations in the Seri country, which are reported at length in the Annual Report of the Smithsonian Institution for the year 1895-96 (Part I., pp. 1-285).

natively the Ainu are hunters and farmers, those at the Exposition had been under the influence of American missionaries, chief of whom is the famous Mr. Bachelor whose influence with the native Ainu has been remarkable. Through his influence it was that the people consented to leave their native land for the journey to this far off country. One of the Ainu young men had attended Mr. Bachelor's mission school, another had been a servant to another missionary. The father of the household, an old patriarch, had also been converted to the Christian faith, but had never quite surrendered all of his native instincts and superstitions. He was a farmer and bear hunter and still clung to the superstitions attaching to the erection of a bear head at the door of the dwelling to ward off evil spirits and omens.

The Ainu are short and stocky, sluggish in movement, deliberate in action, excessively timid in the face of a novel situation, and, taken all in all, very immature in their mental conceptions and aptitudes. However, they were willing and patient in the tests to a degree to cast reproach upon many of our white subjects. The Ainu were brought to America by Professor Frederick Starr of the University of Chicago. We were much indebted also to Mr. Y. Inagaki, a Japanese student, familiar with the Ainu language, who interpreted our directions to the Ainu subjects. In fact, in many ways, Mr. Inagaki's kindly interest and assistance alone made the tests on these people at all possible. Especially was this true because of their excessive timidity.

Vancouver Indians.—The Vancouver Indians belonged to two tribes, the Kwaguitl and the Nutken. There were present at the Exposition, two members of the first named tribe and five of the latter. Like the Ainu just described, some of these people were interrelated. At least four of the group of seven tested were closely so, though we were unable to discover in all cases the exact character and extent of the consanguinity. There were Atleo, an old man of perhaps 65 years, his two daughters, Ellen, aged 35, and Anna, aged 30, or thereabouts, and a nephew, Jack Curley, aged 28. From a scientific point of view, of course, this was unfortunate.

The physical proportions of the Vancouver Indians are rather less than those of the individual of the Algonkian stock. The Vancouver Indian is shorter and slighter of build, but on the whole, stronger and more hardy. He is certainly more active as well as more alert than the Algonkian. In their native haunts, the men are fisherman, often going miles to sea in open boats in the search of whale and seal, which are captured by skillful rowing and spearing.

The Vancouver women are especially noted for their beautiful blanketry, skill in weaving and dyeing. Some of the men also carve skillfully in ivory and wood. In common with most Indian tribes of the Northwest, the Vancouvers have elaborate ceremonial rites, family legends bound up with the family totem, and certain fiducial customs and habits, which are the sacred possession of the household to which they are attached. The totem and fiducial customs attached thereto pass down as a family coat-of-arms, as it were, by which the tribe is distinguished.

In point of intelligence, the Vancouver Indians surpassed any of the Indians we tested save only those boys and girls at the Indian schools who had for years been moulded by the influences and habits of whites. We, therefore, found these people easy to handle and instruct.

The Pigmies.—The group of people popularly known as the Pigmies whom we tested were made up of individuals from three tribes; three Batwas, two Batsubas and one Cheri Cheri; all were males. Their ages were uncertain. They were, however, boys, almost if not fully grown, though I think none was older than 25 years. It is claimed that no Pigmies had before crossed the Atlantic, and naturally they were of peculiar interest. Only two or three of the natives had ever before left Congo territory. The Pigmies were brought to the St. Louis Exposition by Rev. S. V. Verner, a missionary who had spent some years on the African coast, and had familiarized himself with the Pigmy language and social customs. Mr. Verner related that it required some energetic persuasion to induce these people to accompany him to St. Louis. The Pigmy tribes observed by Mr. Verner in the Congo lived a parasitic existence, following the large Kaffir tribes and feasting on their bounty or refuse. It is related that companies of Pigmies and dogs, intermingled, station themselves at reasonable distances from Kaffir feasts, spying with envious eyes the feasting banqueters. No sooner is an unwholesome piece of flesh cast aside by the Kaffirs than there ensues a scramble of Pigmies and dogs indiscriminately for the rejected prize. Whether all Pigmies stand so low in the scale of social culture we are unable to say, but it is held to be applicable to the group which we tested.

In physical appearance, the Pigmy presents no sign that might lead one to class him as of degenerate stock. Although not exceeding an American boy of twelve years in stature, his bodily proportions are good. Still, he is not robust nor capable of great endurance or extraordinary feats of strength. This inferiority, however, probably has its basis in habit, rather than in any innate physical incapacity. Active in play and frolic, and with a keen sense of humor, the Pigmy is a thorough optimist. He really enjoys life, indeed, takes everything with such a degree of levity, that it was only with considerable effort that we were enabled to have him approach our tests with anything like the seriousness they demanded. Withal, the Pigmy is stupid and dense and apprehends meanings slowly and often incompletely. The hearing tests being very simple in character, however, were understood with a fair degree of apprehension, and I have reason to believe that the data are reasonably representative of the group measured.

The Patagonians—Tehuelche Indians.—I was successful in testing only four men of the group of Indians from southern Patagonia; their ages being respectively, 18, 24, 35 and 55 years. At home, the individuals on exhibition at the Exposition had been employed as herdsmen on the Patagonian prairies. They had learned the use of money and, furthermore the habit of rendering no service, no matter how trivial, without a money consideration. This necessitated a bribe of money before any measurements were possible.

Like many other of the Indian tribes, the Tehuelche are sullen and uncommunicative. Their cultural habits are primitive. Their habitation is a tent made of the skins of the llama or guanaco, sewed together so as to form a considerable sheet. This is then stretched across poles, with the edges spiked to the ground. Within this tent, the family cooks, sleeps and lounges. As there is no vent for the escape of smoke, and the floors are never scoured, filth and grime abound everywhere.

The Tehuelche are horsemen and skilful in the use of the bolo, a triple thong loaded at the end with stone weights—which is thrown great distances with unerring accuracy. They are a large people, both men and women being tall and robustly built. With respect to the four individuals measured by us, no unusual difficulty was experienced in instructing them in the ways of the tests, but I question whether in grade of intelligence, they did not exceed the average of the Patagonian Indian.

PART I

THE UPPER LIMIT OF AUDIBILITY

CHAPTER II

Historical

That considerable individual differences exist amongst persons with regard to the faintest tone that can just be sensed is commonly recognized, but differences in the *range of hearing* are not so readily apparent. Helmholtz first called attention to the fact that the chirp of the cricket is sometimes wholly inaudible to people whose hearing is otherwise uneffected—who have experienced absolutely no diminution in hearing. Even after the fact was known that such individual differences really exist, the experimental determination of the range of such variations long awaited some device which would produce and accurately evaluate the sonorous stimuli.

Experiments with visual sensations present no such difficulties as are encountered by an investigator who works with sound. Among visual stimuli, qualitative characteristics are overt. They stand out in such a way as to be little confused by an individual with a normally functioning visual organ. Differences in color such as those between red and green, yellow and blue, or, indeed, yellow and red, are readily perceived, and at the same time differentiate certain qualitative effects to which there is no analogue in the field of audition. When one is affected with the sensation red, it has been determined once for all time, that the stimulus arises from a disturbance in the ether amounting to approximately 450 billions of vibrations to the second, and that the sensation blue corresponds to a disturbance of approximately 790 billions. But, among the higher orders of pitch, differences in tones as great as an octave are scarcely observed, even when they follow each other in immediate succession. Indeed, among comparatively low pitch values, the perception of tone differences is relatively uncertain. In truth, pitch differences stand out as variations in degree only, while color differences naïvely are differences in kind.

Coming to the question of accurate tone analysis, the physical difficulties are still more involved. What the prism has been able to do for the physicist in assisting him in establishing the wave-length of any ray of light whatsoever, the resonators of Helmholtz and König do only very unsatisfactorily, in helping to fix the components of any tonal compound or establishing the pitch of a given unknown sound. The latter, indeed, must still be accomplished by complicated registering devices. Again, the nature of auditory stimuli is such as to make every unfamiliar tone in nature an almost wholly unknown quantity, which it becomes necessary to establish, empirically, always anew. And with a shrill tone, the empirical method alone suffices to fix the pitch even roughly.

Unfortunately, the importance of knowing the exact character of the stimuli employed has not always been appreciated by investigators in the field of hearing. Especially is this to be regretted in the reports of investigations on the limits or the range of audition. It thus happens that on account of a diversity of statement and lack of precision in the definition of the tones employed, it is wholly impossible to compare the data of different investigators. But they serve to emphasize the futility of any research in the field of hearing unless the physics of the problem involved has first been clearly worked out. Some figures relating to the upper threshold of hearing, given out by different investigators, will serve to illustrate what I have just indicated.

Blake and Appunn[1] who are among the foremost investigators of the upper limit of audibility, think the human ear to be sensative to tones of 50,000 or 60,000 double vibrations to the second. Preyer[2] placed the extreme upper limit at 40,000 vibrations; König,[3] with the use of short sounding rods and a modification of the Galton whistle, got results substantially in agreement with those of Preyer. With a Galton whistle blown by a constant air blast, Zwaardemaker[4] believed he could produce audible tones whose vibration rate exceeded 33,000 to the second. But all these data were called in question by Melde,[5] who pointed out that previous investigations were valueless because of instrumental errors. Melde's objective experiments with different makes of instruments led him to believe that no ear is sensitive to tones above 24,000 double vibrations to the second. Melde's investigations were repeated and elaborated by Schwendt,[6] who reached the conclusion that with the Galton whistle,

[1] *Annal. d. Phys. u. Chem.* **64**: 409. 1898.

[2] "Die Grenzen der Tonwahrnehmung" (English trans.), *Proc. Mus. Assn.*, 1876, pp. 1–32.

[3] *Annal. d. Phys. u. Chem.* **69**: 626–66, 721–38. 1899.

[4] *Arch. f. Ohrenhk.* **35**: 30. 1893; *Ztschr. f. Psychol.* **7**: 10. 1894.

[5] *Pflüger's Arch.* **71**: 441. 1898; *Annal. d. Phys. u. Chem.* **67**: 781–793. 1899.

[6] *Pflüger's Arch.* **75**: 346–64; **76**: 189–91. 1899.

audible tones of greater vibration frequency than 22,000 can not be produced. However, with an instrument modeled after the type of the steam whistle (Edelmann's), Schwendt[7] found that where greater intensities of air blasts might be employed, a tone of the value of 49,000 double vibrations might still be heard. With the same whistle, Edelmann[8] put the upper limit at 50,000 double vibrations. By a singular method Stumpf and Meyer,[9] in which some König forks and an Appunn Galton whistle were graduated by a method of difference tones, believed a greater vibration rate than 20,000 inaudible. Still more recently, after some extended objective experimentation with a Galton whistle of the Hawksley pattern, Dr. Charles Myers[10] concluded, at least so far as the Hawksley whistle goes, that tones above 24,000 double vibrations to the second can not be made of such an intensity as to be audible, nor indeed measured by any known means.

It is scarcely necessary to enter into a detailed discussion of the factors responsible for such wide discrepancies in results as these shown between instrument makers on the one hand and scientific investigators on the other. A review of the literature relating to this work, notwithstanding, forces the conviction, that the variations are due to instrumental differences almost wholly—being complicated by the failure to allow for the physical and physiological factors involved in the tests conducted. Much of the early work suffers from inefficient methods of arriving at the vibration frequencies of the tones that have been employed in making the physiological measurements. A great deal of the later work on the upper limit of hearing is of the same character. If we add to this difficulty the fact that the physiological value of tones for the ear has been ignored, it is not improbable that most of the differences would be accounted for. In keeping with this conviction are the recent investigations of Wien and Zwaardemaker.

Wien[11] and Zwaardemaker and Quix[12] have shown that the ear is most sensitive to tones whose pitches are of the middle values, those lying roughly within the range covered by conversational speech (400 D. V. to 4000 D. V.). Both above and below these values, the ear's sensitivity is found to diminish rapidly. Indeed, as early

[7] *Verhandl. d. Naturforsch. Gesellsch., Basel,* **12**: (1900); *Arch. f. Ohrenkk.* **48**: 1. 1900.

[8] *Annal. d. Phys.* **2**: 469. 1900; *Ztschr. f. Ohrenhk.* **36**: 330. 1900.

[9] *Annal. d. Phys. u. Chem. N. F.* **61**: 760–79. 1897.

[10] "The Pitch of Galton Whistles," *J. of Physiol.* **23**: 417. 1902.

[11] *Pflüger's Arch.* **97**: 1. 1903.

[12] *Arch. f. (Anat. u.) Physiol. (Suppl.)*, 1902, 367–98; also *Ztschr. f. Psychol., u. s. w.* **33**: 407. 1904.

as 1878, Helmholtz (Tonempfindungen) pointed out that the ear's range of sensitivity could not be safely divorced from the factor of the intensity of the tonal elements, but the importance of this fact, as related to the upper and lower thresholds of audition, has until recently received little thought. Scripture and Smith[13] in reviewing the factors involved in the wide variations discovered by those who have investigated the ear's range of sensibility again called attention to the importance of the intensity factor and suggested that perhaps the differences in recorded experimental results might be explained on this basis. Some experiments with the Galton whistle, in which various degrees of wind pressure were employed convinced them that if the intensity of the stimuli could be made sufficiently great, the ear would be found to be sensitive to tones whose vibration frequency exceeded 50,000 or even 55,000 D. V. It must be confessed, however, that their empirical evidence for such a conclusion is not particularly convincing.

In the light of recent experimental results, it would not be over-confident to believe that such wide discrepancies as the figures from different observers show would largely disappear were it possible to reckon and allow for the two factors just indicated—the physical, concerned with graduation and intensity, and the physiological, concerned with the ear's relative sensitivity. Both of these factors are so interrelated in the historical data to be considered, that it is impractical to attempt to separate them.[14] This condition comes about partially at least because no uniform type of instrument has been employed for measuring the upper threshold of hearing. Appunn,[15] Preyer,[16] and Koenig[17] used small tuning forks in which the instruments themselves possess certain physical limitations confining the possible intensities of the tones to very narrow limits. Koenig's "rods" possess the same deficiency. It appears that all of these instruments uniformly were assigned tonal values altogether too high.

[13] See "Highest Audible Tones," *Studies from the Psychological Laboratory of Yale University,* 1894, p. 105.

[14] To Schwendt particularly do we owe a method for evaluating the vibration frequencies of tones, which is wholly independent of the experimenter's auditory sensitivity. As Schwendt remarks, it is a method that may be employed equally well by a person wholly devoid of hearing, and consequently eliminates so far as such a thing is possible, the element of the personal equation. The method consists of an adaptation of the Kundt dust figures to tubes of small bore and sound waves of extremely small extension. See *Annal. d. Phys. u. Chem. N. F.* **61**: 760–69. 1897.

[15] *Annal. d. Physik u. Chem.* **64**: 409. 1898.

[16] *Pflüger's Arch.* **71**: 441. 1898; *Wiedemann's Annal.* **51**: 683. 1894; **52**: 238. 1894.

[17] *Pflüger's Arch.* **75**: 346. 1899.

Melde,[16] who put to experimental test the pitch values assigned to the tuning forks used in Preyer and Appunn's experiments, entirely discredited them. His results were later confirmed by Schwendt.[17] The Appunn fork marked (g^8) 50,000 (double vibrations) was found to have a vibration rate of only 13,157 D. V. The pitches of the remaining forks of the series were overstated to about the same degree.

What has just been said of the tuning forks applied with equal force to the sounding rods of König. His highest pitched rod, according to the optographic measurements of Melde, gave a tone of (f^7) 21,845 D. V., although König had assigned to the same rod a value of 40,000 D. V. Much of the error, no doubt, arose on account of the method used for assigning the pitch values to the different forks and rods. Preyer, König and Appunn believed that forks could be graduated with sufficient accuracy for ordinary scientific purposes, with the unaided ear. According to Preyer,[18] practised musicians can distinguish with certainty a difference in pitch amounting to one-half a vibration between the limits of "C" and "c^2." Were it possible for practised observers to discriminate tonal differences as accurately for all parts of the hearing scale, the graduation of instruments for measuring the upper limit of hearing might well be made by some such subjective method as these investigators employed. But the perception of interval is extremely deficient for tones above "c^3," as the experiments of König[19] with sounding rods and those of Preyer[19] with tuning forks have pointed out. With tones in the sixth octave an interval of a fifth is scarcely observable, while for tones in the seventh octave and above an interval of an octave passes unnoticed even by musical ears. As Schwendt has well remarked, it is not improbable that the production of audible tones of a pitch exceeding 25,000 vibrations to the second, with a tuning fork or rod is a physical impossibility owing to the extreme weakness of tones coming from such sources. Where the forks are made as small as those must be to give a tone above 25,000, the energy given out is much diminished. Schwendt further pointed out that, even were their production possible, no known means exists for determining the vibration rate. The methods in use for determining the vibration frequencies of tones are wholly inapplicable to tones of so small energy value as these. Indeed, Schwendt found the resonance method inapplicable for the forks and rods of a vibration frequency exceeding 15,000.

[18] "On the Limits of the Perception of Tone" (trans.), *Proc. Musical Assn.* 1896–7, pp. 1–32.

[19] Helmholtz, "Tonempfindungen" (4th ed.), p. 147.

By far the most common device for measuring the upper limit of hearing has been some form of the Galton Whistle. This type of threshold-whistle,[20] devised by Sir Francis Galton, is constructed on the model of the closed organ pipe, with vibrating lip and resonance cavity. In a closed organ pipe, Helmholtz and also Lord Rayleigh[21] found that theoretically, at least, the vibration frequency is a definite function of the length of the resonance cavity. Knowing the velocity of sound in air at the temperature prevailing "V_a" the length of the resonance cavity "L," the vibration frequency "N" in double vibrations may be computed directly from the formula

$$N = \frac{V_a}{4L}.$$

In point of fact this theoretical formula is not wholly valid even for closed pipes of relatively large dimensions as has been experimentally demonstrated by Savart,[22] Liscovius and Wertheim[23] and others and, indeed, the formula has been shown to be wholly inapplicable to pipes of small bore.[24] Differences in the pressure of air blast employed, the ratio between the length and width of the resonance cavity, the dimensions and shape of the mouth slit, together with the materials of which the whistles are made, all have been proven to be extremely important factors in determining the pitch of Galton whistles as well as all other closed pipes of this variety.

As the diameter of the resonance cavity increases, the tone deepens but, on the other hand, the pitch becomes more acute as the wind pressure becomes greater. Such considerations make impossible any mathematical formula generally applicable to threshold pipes. It has been found, moreover, that no matter how painstaking and skilful the construction in the attempt to duplicate a threshold whistle differences are certain to result, which make it necessary to graduate objectively each whistle independently.

In the experiments of Stumpf and M. Meyer,[25] in which Galton whistles were used, the graduations were also made subjectively. These investigators depended on the observation of difference tones. By blowing two whistles, whose pitches differed by about 2000 double vibrations to the second, simultaneously, a difference tone resulted whose pitch these investigators believed they could place

[20] This whistle is described in Galton's "Inquiries into Human Faculty," p. 38.

[21] *Phil. Mag.* **22**: 344. 1879.

[22] Wüllner's "Experimental Physik," Bd. 2, p. 324.

[23] *Op. cit.* Bd. 1, p. 886.

[24] Vid. Myers, *J. of Physiol.* **28**: 417. 1902.

[25] *Annal. d. Phys. u. Chem.* **61**: 760–79. 1897.

accurately by ear. Then, by increasing the pitch of each whistle alternately, graduations were made until the point was reached where the tones faded entirely. A similar method was pursued with the König tuning forks and the sounding rods.

Certain serious criticisms, however, have been offered against the method. In the first place, difference tones are difficult to recognize at all times and one is never quite certain whether the difference tone is the difference between the vibration frequencies of the fundamentals of the two tones employed or of their octaves, or, perhaps, of the second upper partials. It depends altogether upon the relative intensities of the various components, factors indeed, of which we are never quite certain in the production of very acute tones. Helmholtz[26] observes that while the method of difference tones is possible with pitches whose vibration frequencies fall below 10,000, for values higher than this the method is extremely uncertain. In point of fact when Stumpf and Meyer's graduations were put to experimental test they were shown to be not at all reliable. Employing a resonance method for evaluating the pitches of the instruments used by these investigators, Melde[27] found that the graduations were not accurate to within 10,000 double vibrations for the shriller tones—those of f^7 and above.

There has been on the market, during recent years, an improved form of threshold whistle devised by T. L. Edelmann.[28] It is asserted that this instrument overcomes the objections put against the older Galton form. Edelmann believes that with his whistle tones of 110,000 D. V. can easily be produced and the vibration frequencies fixed; tones, to be sure, which are far too high for any ear to sense. The mechanism of the whistle is such as to consume considerably more energy in its operation than the ordinary Galton type and it likewise gives out a tone whose intensity is many times in excess of that given out by the Galton whistle. Zwaardemaker and Quix[29] discovered that the upper threshold of hearing might be raised a major third by increasing the intensity of the tone 1,000 times, and Wien thinks that it may be raised at least a full octave if the intensity can be increased 10,000 times.[30]

[26] "Tonempfindungen" (4th ed.), p. 203.

[27] Über die verschiedenen Methoden der Bestimmung der Schwingungszahlen sehr höhen Töne, *Annal. d. Phys. u. Chem.* **67**: 781–93. 1899.

[28] This whistle, together with the methods for its graduation, is fully described by the inventor in an article in *Annal. d. Physik* (N. S.) **4**: 469–82. 1900.

[29] *Ztschr. f. Psychol.*, u. s. w., **33**: 407. 1904.

[30] *Pflüger's Arch.* **97**: 1. 1903.

These conclusions confirm the conviction of Scripture and Smith, referred to above.[31] Exactly in the same direction point some of the experiments of Dr. Chas. Myers, in which an Edelmann whistle with different degrees of wind blast was used. His statement of the facts is so convincing that I quote his own words: "When the wind pressure was 30–35 mm. of water, a very faint quivering note was heard. The note rose gradually until a wind pressure of 140 mm. of water was reached when the tone disappeared. Increasing the pressure 100 mm. the tone was again audible, and, at a wind pressure of 800 mm. of water, the tone was at least an octave higher." With a pipe length of 1.3 mm. and mouth width of 0.75 mm., employing the Schwendt dust figures for evaluating the pitch numbers, Meyers got the following significant figures: When the wind blast, measured by water pressure was 36 mm., the vibration frequency was 5,673.

With a water pressure of 109 mm. the vibration frequency was 10,942
With a water pressure of 680 mm. the vibration frequency was 23,315
With a water pressure of 800 mm. the vibration frequency was 28,332

The last figure exactly corresponds to that given, for the same adjustments, in the chart which the makers sent with the Edelmann whistle used in the experiments to be detailed in this writing. This tone and many much higher were easily heard not only by myself, but also by a majority of my adult subjects. There is no question but that the fundamental objection to the Galton whistle used by Zwaardemaker, Stumpf, Dr. Chas. Myers and others, as well as to the tuning forks and sounding rods, lies in this, that the tones produced are too feeble, not only to cause a disturbance of the lycopodium powder in the dust tubes, but also to reach the physiological threshold of the auditory end organs for tones of the upper pitches.

So much for the literature referring specifically to the physics and physiological factors of the problem. Now, let us look for such sensory differences as have been discovered.

In the light of the sources of error mentioned above, the sensory data thus far collected are almost hopelessly bemuddled.

Zwaardemaker,[32] perhaps, has collected more data of individual and age differences as regards the upper limit of hearing than any one else, but his results are not even comparable among themselves. In some of his experiments, a form of the Galton whistle was employed, graduated by the Stumpf-Meyer method. In still other of

[31] *Loc. cit.*, p. 108.

[32] *Annal. d. Phys. u. Chem.* (N. F.) **61**: 760–79. 1897.

his experiments, the pitch values of the different whistle lengths were fixed by comparing the tones given out with those of the König rods or the König tuning forks. The untrustworthiness of both of these methods of making graduations has been pointed out above. Zwaardemaker's "e^8," a tone which many of his subjects heard distinctly was found[33] to be wholly fictitious.

Regarding the upper limit of hearing of whites, the literature is not at all scant. But even were it possible to separate out the data in which the instrumental defects have been least prominent, it would still be difficult to draw comparisons. Without exception, the distrbution of cases has been omitted in the data presented. Most of the work on the upper range of hearing has been carried on for the purpose of determining age differences. It has been pretty well established that the upper limit of hearing contracts with increasing years of life. Zwaardemaker,[34] Cuperius,[34] Alderton,[35] Myers,[36] and others have contributed rather convincing data on this phase of the problem.

The results obtained are so significant as to justify the inclusion of tables summarizing them:

	Zwaardemaker[37]		*Cuperius*[37]	
Ages	**Whistle Length**	**Pitch**	**Whistle Length**	**Pitch**
Under 10 yrs.	1.22 mm.	e^7		
10–20 "	1.39 "	dis^7	1.08 mm.	f^7
20–30 "	1.39 "	"	1.19 "	e^7
30–40 "	1.58 "	"	1.31 "	"
40–50 "	2.23 "	cis^7	1.39 "	dis^7
50–60 "	2.93 "	h^6	2.08 "	cis^7
Over 60 "	3.03 "	cis^6	3.02 "	cis^6

Alderton reports as follows the examination of 500 individuals with the Gaton whistle:

For children up to 12 years—pipe length on the average .. 1.24 mm.
For adults—pipe length on the average .... 3.03 mm.

Dr. Charles Myers's[36] tests of Scotch children point in the same direction.

The Upper Limit of Audibility among Primitive Races.—So far as I have been able to discover, no measurements of this character save those of Myers and my own herein reported, have ever been

[33] Myers, *op. cit.*

[34] *Arch. f. Ohrenhk.* **32**: 54. 1893; *Ztschr. f. Psychol.* **7**: 11. 1894.

[35] *Arch. of Otology,* **23**: 71. 1894; **25**: 43. 1896.

[36] Report of the Cambridge Anthropological Expedition to Torres Straits, Vol. 2. 1903.

[37] *Ztschr. f. Psychol.* **7**: 10. 1894.

made. In their work on the upper limit of hearing of the native Papuans of the Murray Islands, Rivers and Myers[36] used the Hawksley pattern of the Galton whistle. This was an instrument with an extremely small bore, such as to make it possible to produce very high pitched tones. Dr. Myers, who reports the hearing tests, was inclined to put little faith in the graduations of his instrument, a task which was performed after he had returned to England, so the data relative to sensory differences are given in terms of the length of the cavity of the whistle.[38] His results are presented so as to show at the same time race and age differences. I summarize them in the following table:

	Murray Islanders				*Aberdeenshire Folk*		
	No.	Age	Whistle Length		No.	Age	Whistle Length
Children	2	5– 9 yrs.	2.25 mm.	Children	4	5– 9 yrs.	1.97 mm.
	15	10–15 "	2.07 "		18	10–15 "	1.99 "
Adults	5	16–19 "	2.25 "	Adults	....	....	....
	9	20–29 "	3.00 "			20–29 "	2.22 "
	12	30–39 "	3.17 "			30–39 "	2.63 "
	9	40–49 "	3.53 "			40–49 "	3.19 "
	10	Over 50 "	4.68 "			Over 50 "	3.77 "

It appears from this table that for all ages the upper threshold of hearing of the Papuans is lower than for Scottish people of corresponding years. It is a conclusion that is significant, notwithstanding that the numbers tested for the several ages were small. More of this, however, in connection with the discussion of my own figures.

[38] Dr. Myers employed a number of methods in making the graduations of tonal values corresponding to the different cavity lengths in the tests. Chief of these are the resonance method and optographic method. See *J. of Physiol.* **24**: 417. 1902.

CHAPTER III

The Instrument and Its Graduation

I employed, for measuring the upper range of audibility, the Edelmann modification of the Galton whistle. It is probably too commonly known to require description.[1] It differs from the familiar Galton form, in being modeled after the pattern of the steam whistle instead of the closed organ pipe. In the opinion of Edelmann, this improved pattern possesses some marked advantages over the old form. In the first place, the different parts are constructed separately, allowing of finer work. The whistle cavity itself is a perfect cylinder, which makes it possible to turn it out very delicately on a lathe. Then, the whistle possesses a means whereby the width of the lip opening may be varied, so as to allow for the large quantity of air that must pass through it in producing low pitched tones, and still avoid the air puff with extremely shrill tones. In measurements with the Galton whistle this air puff, which accompanies very high tones, is extremely confusing especially to untrained subjects. Not infrequently a subject states that he is not certain whether he hears a tone or wind only. Dr. Myers[2] observes in connection with his work on the native Papuans in which the older Galton type of instrument was employed, that there was constant confusion between the perception of the sound and that of the air puff, which always accompanied it. While I do not think that this difficulty is entirely obviated by making the mouth width of the whistle adjustable, as in the Edelmann pattern, yet there is no question but that the Edelmann whistle is superior in this respect to the old Galton form. In my own experiments, I had never observed that any subject found difficulty in distinguishing between the accompanying air puff and the tone and, indeed, when the whistle was so far as 25 centimeters from the ear, almost no air puff was ever audible.

[1] Those not familiar with the instrument, I refer to the inventor's (Professor Edelmann's) able description and careful drawings to be found in the *Annalen der Physik* for 1900. 4 Folge, Bd. **2**, S. 469. Those also unfamiliar with the Galton whistle may find a description of this instrument in Galton's "Inquiries into the Human Faculty," 1883, p. 275.

[2] *Op. cit.*, p. 4.

The manufacturers send out with each Edelmann[8] whistle a chart giving the pipe length, the mouth width, and the vibration frequency corresponding to each of some twenty different tones, ranging in pitch from that represented by a vibration rate of 6,000 to that represented by a rate of 50,000 double vibrations to the second. It is claimed that each whistle has been graduated independently and empirically at the factory, and that, for each tone, that mouth width was selected by trial and practise which would produce a note of optimum purity and strength.

In the chart sent out with the whistle which fell into my hands, it appeared that the graduations for those tones lying between e^6 and e^8 had been made with a uniform mouth width of 0.75 mm. With the whistle so adjusted, those tones lying in the region of e^6 were pure, clear and free from that peculiar harshness which results when a considerable quantity of air escapes with the tones. The contrary, however, was true when the whistle cavity was diminished for the production of tones in the region of e^8. These latter tones were decidedly harsh. This harshness was obviously due to the accompaniment of air puffs, which escaped with the tones. They stood out so prominently as to confuse even the most careful subjects, and must have proven a very distracting element to children, and especially to the primitive peoples. It therefore seemed advisable to vary the adjustment from that prescribed in the chart sent out with the instrument even if it would necessitate an entire re-graduation.

After careful experimentation with the assistance of Professor Woodworth, a mouth width was hit upon which give admirable results for all tones from e^5 upwards. Indeed, so free was the tone from wind blasts, that when the threshold range was passed, no auditory stimuli of any character were sensed as coming from the whistle. This mouth width was 0.55 mm. It chanced, however, that the resulting tone was predominantly the first overtone instead of the fundamental, but, since the fundamental tone was inaudible except for vibration rates between ten and fifteen thousand, it was thought not to be a particularly disturbing factor. All the measurements at the Exposition, consequently, except those where the upper limit was found to be extremely low, were made with a constant mouth width of 0.55 mm. This, to be sure, rendered the table which accompanied the whistle entirely worthless. A wholly new set of graduations must be made to meet the new conditions.

[8] The Edelmann whistle used in my tests was kindly loaned to me by the C. H. Stoelting Co., 38 W. Randolph Street, Chicago, for the double purpose of exhibition and experimentation.

To carry on such an extended series of experiments called for more time and more elaborate preparations than it was possible to institute at St. Louis. I therefore allowed this work to await my return to Columbia University after the Exposition had closed.

While collecting the data, the measurements were tabulated in terms of mouth width and pipe length, from which it was possible readily to transcribe them into terms of vibration frequencies, and into musical nomenclature when desirable. The graduation of the whistle was a difficult task. In the work of determining the vibration frequencies corresponding to the different pipe lengths employed in the actual test, several devices were tried with varying degrees of success. It was found especially difficult to devise any rotating system of sufficient speed and delicacy to register satisfactorily disturbances of a sensitive flame for such rapid vibration rates as those from 15,000 to 40,000 D. V. The optical method for registering vibrations used in physical laboratories, however, was tried for some of the lower pitches, but even here the figures were by no means wholly satisfactory, by reason of the inaccuracies in determining the speed of the rotating system, the sliding of parts, etc., etc.

The most satisfactory device for making the graduations, because the most accurate and objective, proved to be the "dust figure" method of Kundt, first adapted to the use of tubes of small bore by Schwendt.[4] The Kundt dust figures are illustrated and described in all general texts on acoustics; hence, they require no elaborate description in this place. The dust figure method is the one, moreover, which is employed at the factory for standardizing the Edelmann whistles before they are sent out. The modifications which are essential to adapt this method to tones as high as those employed for testing the upper limit of audibility are significant as to detail and procedure. It is unnecessary to enter here into a description of the mode of procedure in anything like a detailed way. Something requires to be said, however, with reference to some of the difficulties which are encountered.

One of the first essentials with regard to the dust figure method is that the tubes used for resonance chambers shall be of optimum dimensions. A series of tubes varying in length and bore are consequently necessary to secure satisfactory results. For evaluating the most acute tones, I drew out some very thin tubes to a bore of 2 mm. and a length of 26 mm.—just long enough to allow for the formation of five or six half-wave lengths, that is, for five

[4] *Pflüger's Arch.* **75**: 546. 1899.

crests and as many troughs. It is quite essential that a number of these artificial wave troughs and crests be formed if the work is to be at all delicate, since, in making the actual measurements of the length of the several waves, accuracy is enhanced if the span covered by several half waves is measured with the aid of calipers and the figure thus obtained divided by their number to secure the length of a single wave. Tubes with bores of from 6.0 mm. to 10 mm. and lengths from 100 to 250 mm. were used for tones of the sixth and the lower third of the seventh octaves (c^6–e^7), and still larger tubes for the tones of still lower pitch values. Just enough of the lycopodium powder, which had been previously carefully dried, to cover the bottom of the tube, was evenly distributed along its entire length. The tube was then slightly turned so as to raise the powder to one side. This facilitates the formation of dust figures, the aerial disturbance within causing the dust to fall, and while falling, to collect at the points of rarefaction within the resonance chamber.

In making dust figures, it is necessary that the resonance tubes be kept free from extraneous vibratory influences, else the resulting dust figures will be confused and impossible to interpret. To avoid jars of all kinds, I had the tubes carefully clamped between large pieces of cork, which took up most of the disturbances transmitted to them. Then to make as much as possible of the sound energy leaving the whistle enter the tubes, the whistle mouth was brought as close to the mouth of the resonance tube as possible, and, to further facilitate the movement of air, the ends of the tubes adjacent to the source of sound were flared into a funnel form whose widest diameter was about 15 mm. When everything proceeded favorably, satisfactory dust figures generally resulted in from ten to fifteen minutes, but failures and disasters were frequent. Indeed, much patience and repetition were required to secure perfectly reliable results, frequently as many as six or seven trials being necessary to get anything like a satisfactory measurement. It was essential that each graduation be the average of as many determinations as possible to eliminate chance results. My data, in every instance, are the average of five or more determinations.

Let us assume that the dust figures have formed in the resonance tube. Knowing the distance between two adjacent wave crests, it is a simple matter to compute the vibration frequency of the tone that gave rise to the dust figures. The result is accomplished directly by a simple formula,

$$N = \frac{V_a}{2L}$$

where "N" represents the vibration frequency, "V_a" the velocity of sound in air at the temperature prevailing at the time the measurement is made, and "L" represents one half wave-length, or the distance encompassed by two adjacent wave crests. The "V_a" implies a temperature correction. At 0° Centigrade, under ordinary barometric pressures for this latitude, sound has a velocity of 330.7 M. per second. To secure the corrected value of "V_a" for any given temperature, I used the well-known formula of Kayser and Kirchoff,[5]

$$V_a = 330.7 \sqrt{1 + 0.00376\, t^a}$$

During the summer of 1904, within doors on the shady side of a building the temperature varied (according to the United States Weather Bureau reports[6]), between 17° and 37° C. Our hearing tests, however, were made in a basement where the prevailing temperature was lower, and unfortunately we have no record of this during this period. Still the difference between this and the above, I think, does not amount to a figure to be significant. My corrections were made on the basis of the Weather Bureau statistics.[6]

In the sound-proof room of the Psychological Laboratory of Columbia University where the work of graduation was done, the temperature remained quite uniformly at 22.2° C., making the velocity of sound approximately 344 M. per second.

Temperature variations can not well be neglected in making tests for the upper limit of hearing with such a device as the Edelmann whistle, if results be sought which aim to be more than approximately correct. Taking the lower and upper extremes of temperature, during the days that my hearing measures were made, and making the necessary corrections, a whistle cavity length of 1.3 mm. and mouth width of 0.55 mm. would give a vibration frequency of 40,840 D. V. and 42,054 D. V. respectively. This variation is certainly sufficient to be quite significant, especially in view of the fact that tests on the different peoples were made during different seasonal conditions. With regard to most of the data relating to the Filipinos, the correction was especially necessary, because the season had so far advanced when these people were measured that the rooms had to be heated artificially, and the temperature, which was almost uniformly 22° C. differed markedly

[5] See Wüllner's "Experimental Physik" (1894), **1**: 931.

[6] I was able to secure from the office of the U. S. Weather Bureau at St. Louis temperature and humidity readings taken at each hour of the day, for a period covering that included by the taking of the hearing records. From these figures I calculated the true value of "V_a" for each hearing record made.

from that prevailing during the hot summer weather when the data on Indians and whites was collected.

Conditions at the Exposition made it necessary to employ a wind blast supplied from the hand bulb, which accompanies the whistle. Some such constant pressure device as that of Whipple[7] was contemplated, but there was so much delay occasioned by the failure of the apparatus to arrive and the general equipment of the laboratory to be provided, that many of the tests of the upper range of hearing were made before such an equipment might have been installed. Consequently, to keep the conditions under which the tests were conducted as nearly uniform as possible, the hand bulb method of blowing the whistle was permanently adhered to. Edelmann holds that with a whistle of the type I used, the pitch of the tone is only slightly dependent on the wind pressure employed.[8] This statement, however, can be only partially true, and indeed is applicable only to a certain range of variation about the optimum wind blast for blowing it.

To investigate the influence of a variable wind blast to some extent, I improvised a wind pressure device which allowed of differences in the force of the blast. An ordinary wet spirometer, found in the Columbia University Psychological Laboratory, was weighted to the required wind pressure by loading it with slugs of iron; then, with an ordinary foot bellows, the quantity of air in this reservoir was kept constant. As in all such experiments, a water manometer or U-tube, was inserted in the lead as close to the whistle as convenient to measure the pressure of the air blast passing into the whistle. But before allowing the air to pass through the whistle, it was made to flow through a drying device; a bottle filled with the crystals of calcium chloride, by which the moisture was, so far as possible, removed inasmuch as moisture in the air tends to interfere seriously with the formation of the lycopodium dust figures in the resonance chambers.

The cavity length of the whistle being 1.3 mm. and mouth width 0.55 mm., with a constant wind pressure indicated by 40 mm. of water in the U-tube, the resulting tone was too faint and weak to produce satisfactory dust figures. It sounded of uncertain pitch and was by no means pure.

When the wind blast showed 100 mm. of water in the U-tube, the tone was clear but observably lower than when the whistle was blown by the rubber bulb. With the wind pressure increased to 500 mm. of water, the tone came forth clear and piercing. My

[7] "A Compressed Air Device, etc.," *Amer. J. of Psychol.* **14**: 107. 1903.

[8] *Annalen d. Physik*, 4 Folge, **11.** 1900.

subjective judgment was that the pitch was the same as that produced by the use of the bulb.

The table below shows the result, in terms of wave lengths and vibration frequencies, for a number of different wind pressures employed:

Wind Pressure: mm. of water	Wave Lengths in mm. Average of 5 determinations	Pitch Values D. V.
40	A faint tone—too weak to produce dust figures	
100	16.268	21,330
500	8.33	41,217
1,000	8.724	39,460
Rubber bulb	8.428	40,840

These averages were obtained from the following individual determinations:

Pressure—100 mm. of Water

3 crests measured	23 mm.	Wave length 15.34 mm.
2	17	17.00
2	19	19.00
3	22	14.66
3	23	15.34
		Average wave length 16.248 mm.

500 mm. of Water

5 crests measured	21 mm.	Wave length 8.4 mm.
4	15	7.5
4	18	9.0
6	25	8.34
5	21	8.4
		Average wave length 8.33 mm.

1,000 mm. of Water

4 crests measured	19 mm.	Wave length 8.5 mm.
6	26	8.66
6	23	7.66
5	22	8.8
4	18	9.0
		Average wave length 8.524 mm.

The Bulb

5 crests measured	19 mm.	Wave length 7.6 mm.
4	17.5	8.75
6	25	8.33
6	26	8.66
5	22	8.8
		Average wave length 8.428 mm.

These figures are in general in agreement with those obtained by Dr. Charles Myers, in investigating a similar problem with the

Galton whistle. It is to be noted, however, that a wind pressure of 1,000 mm. of water, gave a tone which is actually lower than that produced by a wind pressure of 500 mm. of water, a fact which does not accord with Dr. Myers's[9] experimental conclusions. His experiments lead him to believe that the pitch of the whistles increases regularly with increase of air blast. The difference in favor of the lower wind pressure found by me, I believe, however, is not significant in that I do not think it exceeds the limits of the accuracy of the method. It is to be further noted that with the bulb, the figures do not differ materially from those with a wind pressure of 500 mm. and 1,000 mm. So far as I was able to determine, the bulb gives approximately a pressure of 800 mm. of water though with a pressure of so short duration it is difficult to evaluate accurately the total force given out, with the means at my command.

Inasmuch as in all my investigations the bulb supplied the wind blast, it is the graduations in which the same source of wind pressure was used that concern us chiefly. Indeed, for this reason, in all of the work of standardizing the instrument, from which the tables to follow were made, the rubber bulb alone was employed. When using the hand bulb in the making of the graduations, an effort was made to reproduce as nearly as possible the conditions as they obtained in taking the original hearing records. That the conditions were more than approximately reproduced and, moreover, that with the use of the bulb, fairly constant conditions can be maintained from day to day, is borne out by the rather uniform character of the data secured. Were one unable to give relatively constant and uniform blasts to the whistle in blowing it with the hand bulb, some marked differences in the vibration frequencies of the tones produced would result, and would show in the dust figures. It is significant that such was not found to be the case, and that no greater differences in the character of the dust disturbances were experienced when the hand bulb supplied the air than when the air came from a uniform and constant pressure source, as is shown in the foregoing tables. To illustrate this point, I shall present some typical measurements in which the hand bulb was employed. As too much space would be required to present the individual measurements for the graduations of the whole series of whistle lengths used in the original tests, I will content myself with two samples selected at random.

In the following series, the length of the whistle bore measured 1.5 mm., the mouth width being 0.55 mm.:

[9] "The Pitches of Galton Whistles," *J. of Physiol.* **28**: 417. 1902.

2 crests measured	9.2 mm.	Wave length	9.2 mm.
12	57.1		9.52
12	52.81		8.8
20	87.34		8.73
14	65.41		9.33
		Average wave length	9.116 mm.
		Vibration frequency	37,560 D. V.

In the next series, the cavity length of the whistle was 2.2 mm., the mouth width remaining the same as in the preceding experiment:

2 crests measured	16.0 mm.	Wave length	10.66 mm.
4	22.3		11.15
9	44.1		9.08
6	31.2		10.04
3	15.4		10.26
		Average wave length	10.45 mm.
		Vibration frequency	30,270 D. V.

These two sets of measurements are in every way typical of all that were made and indicate about the same degree of variation between the individual determinations as was experienced on the average. The same procedure, as in these samples, was followed for every whistle length employed in the original measurements. The accompanying table gives the vibration frequencies corresponding to each whistle length as empirically determined by aid of the Kundt-Schwendt resonance method.

In this table the distance between the lips of the whistle remained uniformly at 0.55 mm. The average wave-length of the several determinations has been omitted in each case since it would afford no information vital to an interpretation of the figures presented:

Length of whistle cavity in mm.	Vibration Frequencies (Complete Vibrations)	Length of whistle cavity in mm.	Vibration Frequencies (Complete Vibrations)
1.2	42,960	2.5	28,048
1.3	40,840	2.6	27,448
1.4	39,220	2.7	26,854
1.5	37,560	2.8	26,264
1.6	36,360	2.9	25,724
1.7	35,100	3.0	25,212
1.8	34,000	3.1	24,754
1.9	33,060	3.2	24,196
2.0	32,180	3.3	23,020
2.1	31,170	3.6	22,217
2.2	30,270	3.8	20,973
2.3	29,508	4.0	18,490
2.4	28,766		

CHAPTER IV

Data Collected on the Upper Limit

During the earlier months of the Exposition, while we were getting our bearings, equipping our laboratories, and installing our apparatus which was somewhat tardy in arriving, we spent our time amusing the public and, incidentally accumulating data on some few tests. For the most part we limited ourselves to a single test and measured as many individuals as we could in this one particular only. In consequence, I was enabled to secure considerable material relating to the upper threshold of hearing. Unfortunately these data had to be secured under somewhat unfavorable conditions, in that the test was always made in the presence of a crowd. The sound room had then not yet been completed and there were many distracting noises that might have tended to distort the results to some extent. On the whole, notwithstanding, I believe the data satisfactory, since a comparison of this material with some secured on whites in the sound room showed no significant differences.

The individuals were tested one at a time. The Edelmann whistle was held twelve inches from the subject's ear and blown; the other ear meanwhile being closed by pressing the tip of the finger into the auditory meatus. At first the pipe was so adjusted that a tone resulted whose pitch was so low as to be easily sensed by all ears. The pitch was then gradually raised until a point was reached where the tone was no longer audible. A reading was then taken and recorded for the last audible sound. Beginning with an inaudible tone, the pitch was now lowered until it could again just be sensed and this whistle length recorded. The average of the two tabulations, if a difference existed—and there usually did—was taken as the measure of the upper limit of the person tested. Each ear was tested. Almost without exception, the right ear was the one first examined.

In the measures on primitive peoples, the procedure was in all respects essentially the same as just outlined, except that the tests were made within a specially constructed booth.[1] Although this

[1] This booth was constructed in one corner of a room of our suite, which had been closed to the public, being set apart by us for making such measurements as required privacy. The dimensions of the booth were approximately six by nine feet, and seven feet in height. On two sides the heavy stone and

room was not completely sound-proof, it did exclude extraneous noises sufficiently well to keep the subject free from distractions. To contribute still further to this same end, the person tested was kept in the dark and his back turned to the experimenter, in order, so far as possible, to exclude visual stimuli and allow the subject's mind to concentrate wholly upon the auditory sensations presented to him. Moreover, the subject was seated in a chair, in as comfortable a position as possible. In other words, I attempted to make the external conditions as suitable as could conveniently be done for sensing the auditory stimuli and consequently securing the most nearly normal results.

By way of check, a record was made of my own hearing immediately following that of each individual tested. Conditions made it necessary for me to make personally the test of my own hearing. Still I do not think that the source of error arising from this method is really significant. Of course, the element of expectancy was something of a factor so that there might have entered into the experiments auditory images which it were easily possible to confuse and mistake for the stimulus at a time when it was really inaudible. I have, however, practically no auditory imagery, at least, I have never been able to observe any in myself, so it is scarcely possible that images of sufficient intensity to be confused with even faint stimuli should have arisen under the circumstances just noted. In any event, the factors concerned with the personal test did not vary from day to day or from hour to hour, and hence are not significant on the whole. In this connection may be mentioned the need for this check, discovered during the time the tests were being made. It frequently occurred that the whistle became partially clogged with bits of rubber, or other matter coming from the inside of the rubber bulb. This clogging had the effect of lowering the pitch of the upper tones from a fifth to an octave, although otherwise no apparent change in the character of the tones resulted, so that had it not been for the personal check-test, the subject would have received an unfairly high rating.

brick walls of the building served to exclude the most penetrating sounds. Although the remaining sides were of wood, the walls were double, the space of about four inches between the two being filled with sawdust. Sawdust to the depth of six to eight inches also covered the roof while the whole booth rested in a bath of sawdust in order to exclude any sounds which might be conducted into it through the cement floors of the building. Entrance was made through a single padded door, and the sole illumination came from an incandescent electric bulb which was suspended over the apparatus. The arrangement was such as to exclude extraneous sounds entirely for all practical purposes.

It is unnecessary to name all the factors and considerations which entered vitally into the making of the tests, but there is still one which requires to be mentioned. This relates to the ear's susceptibility to fatigue. Observers are unanimous in their experience with respect to this point. Professor Seashore[2] has remarked in connection with his audiometer that to be satisfactory for testing children, owing to the presence of fatigue, a device must be employed which will make it possible to complete the test in not to exceed two minutes. Especially with subjects who are unaccustomed to making introspective observations, it is found that tones die out and are inaudible long before threshold values are reached. Particularly is this true where continuous tone devices are employed in making auditory measurements such, for example, as the tuning fork, the sounding rod, or the Galton or Edelmann whistle, blown by some constant and continued air supplying device. For this very reason such devices and means for testing the range of audition are unserviceable. The Edelmann whistle blown by means of the bulb with its short, quick sound obviates all this difficulty. It is a difficulty, too, which can not be overlooked with safety, especially in dealing with children and primitive peoples.

The Measurements.—We shall now turn directly to the data regarding racial differences in the upper limit of hearing. In Table I. are presented the figures representing the averages for both the right and left ears. In the first column are indicated (1) the number of individuals constituting each group examined; and in parallel columns; (2) the averages; (3) the average of the deviations from each average; and (4) the standard deviation, of each group. From these data it is possible, without difficulty, to compute directly any of the different variability coefficients desired, and in consequence, the reliability of each average, and the probability of a difference between any two groups. Owing to the fact that among the Filipinos, the Pigmies, Patagonians and Cocopa, there were no women tested, the data have not been separated so as to show the influence of sex, except that relating to whites.

In tables II. and III. the individual records are distributed, so as to present in parallel columns a picture of the distribution of the individuals of each group examined. These tables represent the figures for both the right and the left ears. In Table IV. is presented a distribution of the cases according to age, for the three most numerous groups measured. These again have been placed in parallel columns to afford a more ready comparison, and to give

[2] *Univ. of Iowa Studies,* **2**: 55. 1899.

in pictorial form the relations which the different groups sustain to each other in this respect. For the smaller groups, the ages will be presented in connection with their discussion.

TABLE I

UPPER LIMIT OF AUDIBILITY

Racial Differences as Shown by Averages

	No.	Right Ear			Left Ear		
		Average	A. D.[3]	S. D.[4]	Average	A. D.[3]	S. D.[4]
Whites	156	32,285 D. V.	2271	2344	33,087 D. V.	1891	2482
Indians	63	31,975 "	2190	2663	31,580 "	2460	3028
Filipinos	97	29,916 "	1755	2180	29,886 "	1737	2089
Cocopa	10	32,123 "	827	977	31,794 "	1408	1566
Ainus	7	28,846 "	1666	1873	29,529 "	2946	3199
Vancouvers	7	28,269 "	1209	1413	27,571 "	819	852
Pigmies	6	33,223 "	2071	2468	34,081 "	3212	3428
Patagonians	3	30,240 "	3240	3551	28,630 "	2366	2592

TABLE II

RIGHT EAR—UPPER LIMIT OF HEARING

Distribution of Individual Cases

Vibration Frequency	Whistle Length	Whites	Indians from School	Christian Filipinos	Cocopa Indians	Ainu	Vancouver Indians	Pigmy	Patagonian Indians
42,960	1.2	1	—	—	—	—	—	—	—
40,840	1.3	0	—	—	—	—	—	—	—
39,220	1.4	3	1	—	—	—	—	—	—
37,560	1.5	8	—	—	—	—	—	1	—
36,360	1.6	9	6	—	—	—	—	—	—
35,100	1.7	10	6	2	—	—	—	1	1
34,000	1.8	15	4	2	1	—	—	—	—
33,060	1.9	18	6	4	2	—	—	1	—
32,180	2.0	26	6	11	1	—	—	1	—
31,170	2.1	20	12	14	4	1	—	1	—
30,270	2.2	18	8	20	—	2	1	1	—
29,508	2.3	6	3	10	—	1	1	—	—
28,766	2.4	6	5	5	—	—	2	—	1
28,048	2.5	7	3	10	—	1	1	—	—
27,448	2.6	3	1	2	—	1	—	—	—
26,854	2.7	3	1	5	—	—	2	—	1
26,264	2.8	2	1	3	—	—	—	—	—
25,724	2.9	1	1	5	—	—	—	—	—
25,212	3.0	—	—	—	—	1	—	—	—
24,754	3.1	—	—	—	—	—	—	—	—
24,196	3.2	—	1	1	1	—	—	—	—
23,020	3.4	—	1	—	—	—	—	—	—
22,217	3.6	—	1	—	—	—	—	—	—
20,973	3.8	—	—	—	1	—	—	—	—
18,490	4.0	—	—	—	—	—	—	—	—

[3] A. D.—Average Deviation.

[4] S. D.—Mean Square Deviation.

TABLE III

LEFT EAR

Distribution of Individual Cases

Vibration Frequency	Whistle Length	Whites	Indians from School	Christian Filipinos	Cocopa Indians	Ainu	Vancouver Indians	Pigmy	Patagonian Indians
42,960	1.2	—	—	—	—	—	—	—	—
40,840	1.3	—	—	—	—	—	—	—	—
39,220	1.4	1	1	—	—	—	—	1	—
37,560	1.5	8	2	—	—	—	—	1	—
36,360	1.6	3	2	—	—	—	—	—	—
35,100	1.7	9	5	—	—	—	—	1	—
34,000	1.8	14	8	1		—	—	—	—
33,060	1.9	18	6	6	1	1	—	—	—
32,180	2.0	9	4	11	—	2	—	—	1
31,170	2.1	11	11	21	3	—	—	2	—
30,270	2.2	5	3	10	1	—	—	1	—
29,508	2.3	2	11	13	1	—	—	—	—
28,766	2.4	5	0	7	—	1	2	—	—
28,048	2.5	2	4	5	—	—	1	—	—
27,448	2.6	—	1	5	—	—	—	—	1
26,854	2.7	—	1	5	—	1	4	—	—
26,264	2.8	—	1	6	—	—	—	—	1
25,724	2.9	—	3	2	—	—	—	—	—
25,212	3.0	—	—	—	—	—	—	—	—
24,754	3.1	—	—	1	—	1	—	—	—
24,196	3.2	—	1	—	—	—	—	—	—
23,020	3.4	—	—	—	—	—	—	—	—
22,217	3.6	—	—	—	1	—	—	—	—
20,973	3.8	—	1	—	—	—	—	—	—
18,490	4.0	—	—	—	—	—	—	—	—

TABLE IV

TABLE SHOWING THE NUMBER OF PERSONS OF EACH AGE OF THE THREE MOST NUMEROUS GROUPS OF PEOPLE MEASURED

Ages	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Whites	5	6	10	8	14	12	14	9	13	11	12	10	19	6	7
Indians (School)	12	11	16	12	4		2	2	1	1		2		1	3
Filipinos	3	1	8	16	20	14	12	7	5	5	2	2			1

Average age: Whites, 23 years, 5 months; Indians, 19 years, 2 months; Filipinos, 21 years, 1 month.

Reference to Table I. shows that of Whites, the records of 156 individuals were included in this study. Owing to some rather significant changes, that occur in the range of audibility during the earlier and later years of life as has been already indicated by the studies of Zwaardemaker,[5] Alderton, and others, it was thought advisable to include in these data only the records of those in early manhood and womanhood, those individuals whose ages ranged

[5] See Alderton, *Arch. of Otol.* **23**: 171. 1894; **25**: 45. 1896; also Zwaardemaker, *Ztschr. f. Psychol.* **7**: 10. 1894, and *Arch. f. Ohrenhk.* **32**: 53; **35**: 299.

from sixteen to thirty years. During these years, no very significant changes have been discovered in the respect just indicated. I excluded also, not only in case of the whites but also among the other races, the record of every individual who had noticed particularly any diminution in his hearing acuity. Obviously those with defective hearing should not be included in any comparative measure of the hearing function, since they form a distinct functional group or species.

The average age of the whites was found to be 23 years and 5 months. About one third of them were older than 25 years, while one fifth only were younger than 20. The majority, therefore, of the Whites used here for comparative purposes were between the ages of twenty and twenty-five years—men and women in the younger years of adulthood, at a period of life most favorable for accurate testing.

On the average, the results show a slight superiority of the left over the right ear, the average for the latter being 33,087 D. V., as against 32,285 D. V. for the former. This is in accord with the observations of Preyer and Fechner, who observe that the left ear is superior to the right in all its functions, due, they believe, to the fact that human beings are left brained. In the case of my own measurements, however, much may have had to do with the order of testing the two ears. During these tests, the right ear was almost invariably first tested, so that a mental element arising from practise may have been responsible for the superiority of the left ear. If instead of the average we take the median as the measure, the difference in result is not changed.

The reliability of the averages, which may be directly computed from the S. D. given in Table I., is such as to indicate that the chances are about ten to one that the true average will not vary from the one given by more than 200 vibrations, which in reality, is within the range of instrumental accuracy; that is, the true average will not be as high as 32,500 vibrations to the second or as low as 32,000.

Reference to tables II. and III. will show that the distributions are fairly normal. Exclusion of the records of all individuals who had experienced some hearing defect has served to eliminate the skewness which might otherwise be looked for at the lower end of the curve.

Indians

So far as I am aware, no study of the hearing of our American Indian has ever been undertaken. There are, to be sure, in the literature relating to this interesting race of people, some general observations of a wholly unscientific character which, for the most part, attribute to the savage of the American forest and plains, a remarkable sensory acuity. The works, for example, of James Fenimore Cooper, contain statement after statement, all purporting that the Indian has ears that hear so keenly that he is able to detect sounds in the forest that are wholly inaudible to the ears of a white man most favorably gifted in this respect. And, indeed, whether from the popular literature relating to Indians, or from a preconceived notion that a savage ought to be superior to civilized peoples in sensory acuity, the opinion generally prevails that the ears of the Indians are very much keener than are those of the Whites. No doubt much of this conception arises from the belief in what is commonly known as the doctrine of compensation. According to this view, if one sense or mental function is lacking, in any respect, the others are the keener to compensate for the loss. Moreover, based on the olfactory sense of the dog, the visual acuity of the hawk and the superior audition of certain of the felines, there has arisen the belief that the senses degenerate under the influences of civilization and higher culture.

The figures relating to the upper limit of the hearing of the various Indian tribes represented at the Model Indian School at the Exposition, are given in tables I., II., III. and IV.[6] The hearing of 71 Indians included in this group was taken; 14 full-blooded males and 13 of mixed blood; 4 full-blooded females and 40 of mixed blood. Of this number, 8 were younger than sixteen years so that data relating to them are not included in the general average. Only 6 individuals of the group were older than 25 years (See Table IV.) while but 12 of the 63 were older than 20 years. In other words, 81 per cent. of all the Indians examined were between 16 and 20 years old. The average age of the entire group is only 19 years and 2 months, while that of the Whites with whom they are compared is 23 years and 5 months. It is therefore evident that, if the upper range of hearing progressively decreases from earliest childhood, the Indians are favored in the comparison with the Whites. For the right ear, the tone marking the upper threshold of hearing of Indians is on the average 31,975 double vibrations to the second, with an average deviation amounting to 2,190 vibrations. The average for Whites for the same ear was 310 double vibrations higher. On purely statistical grounds[7] one would be justified in

[6] For general remarks relating to these Indians more specifically consult page 4.

[7] In making these computations, the formulæ commonly used in statistical data for measuring the reliability of a difference were employed. See Thorndike, "Mental and Social Measurements," 1904, p. 139, et seq.

inferring that the chances are almost two to one (exactly 1.91 to 1) that an actual difference exists between the upper limit of hearing of Whites and Indians respectively. The difference however is too small, considering that neither group is very numerous, to point strongly in the direction of a real difference. The age factor, moreover, is of some importance in affecting the data as may be seen when we take the records of those individuals only whose ages run from 16 to 20 years—the figures which encompassed the largest number of the Indians examined. We have, then, 51 Indians and 43 Whites. When this reduced group is taken, the average upper threshold value for Indians, in case of the right ear, is found to differ only slightly from that found for the whole group, namely, 32,080 vibrations. On the contrary, the average for the Whites, when thus limited to those between 16 and 20 years, is increased to 33,587 vibrations. On statistical grounds, therefore, the chances are 10 to 1 that the upper limit of hearing for Whites is 1,000 vibrations in excess of that for Indians, and 400 to 1 that a real difference exists between the upper limit of hearing of the two peoples, on the average. Turning our attention now to the left ear, the difference between the two peoples seems still greater. The left ear too probably gives a figure which more nearly represents the actual state of the organs on account of the practise which the individual had in the experiment, as pointed out above. For this ear, on the average, the Whites hear tones of a pitch amounting to 1,500 vibrations higher than do Indians. Moreover, the probability of a real difference between the two peoples is at least 500 to 1, which, indeed, is extremely high. Although the conclusion is not so positive as though a larger number of each group had been measured, yet the data point in that direction to a degree amounting almost to certainty. Not only do Whites hear tones, which are more acute, on the average, than do Indians, but a glance at the distributions found in tables II. and III. will show that the entire curve for Indians for both ears extends lower than that of Whites. The amount of variability likewise appears to be greater in the case of Indians. As regards the right ear, one white woman only heard a tone higher than any of the Indians, and for the left ear, the best Indian did as well as any White.

Again looking at tables II. and III. from another angle, it may be observed that the relative inferiority of the range of Indians' hearing is not due to a few extreme records, which would have a tendency to distort the figure representing the average. As regards the right ear, the data presented show that 44 out of 67, or 66 per cent. of the records of Indians fall below the average for

Whites, and in case of the left ear, 41 of the 65 records or 63 per cent.

Cocopa Indians—The Seri.—Of this tribe, we were able to make

COCOPA INDIANS

Highest Audible Tone

Name	Age	Right Ear		Left Ear	
Hi	6	32,180	D. V.	30,270	D. V.
Skik	14	31,170	"	31,170	"
Mert	14	33,060	"	34,000	"
El Puck	15	31,170	"	31,170	"
Jack	17	33,060	"	31,170	"
John Roy	18	31,170	"	33,060	"
Joe	20	34,000	"	34,000	"
Jerry	40	31,170	"	29,508	"
Pablo	56	(18,000	")	(21,420	")
Tom	70	(16,000	")		
Average		32,123	"	31,794	"
S. D.		977	"	1,566	"

measurements of ten males only. On account of the variations in age, too, the data are not very suitable for comparative purposes. The youngest member of the group was a lad of six years, while the oldest had passed the age of three score and ten. All of the essential data with reference to these people are presented in detail in the table above. The records of the two oldest men have been enclosed in parentheses to indicate that they have not been included in casting up the averages for the whole. Both Tom and Jerry heard even ordinary conversation with difficulty, so that obviously their range of hearing could not be taken as normal to the group. Excluding, then, these two records, it is seen that the average upper limit of audibility of Cocopa Indians for the right and left ears respectively is 32,123 and 31,794 vibrations (double) to the second, figures which do not differ materially from the averages obtained from the more intelligent Indians at the U. S. Government schools. However, considering that only one of the eight individuals whose hearing records contribute to the average was older than 20, it is probable that the range of hearing of Cocopas in the long run would be found to be less than that of the more intelligent Indians; but even so, the difference would likely be psychological rather than organic.

Compared with the upper limit of hearing of Whites, the Cocopas fall significantly lower. Not only is this true as regards the average result, but age for age, the individual records are found to fall decidedly below the medians for Whites, as may be seen by comparing the distributions of the groups exhibited in tables II. and

III. In case of the upper limit of hearing for the right ear, 6 of the 10 Cocopas stand lower than the median record for Whites and 6 of the 9 in case of the left ear.

Vancouver Indians—Kwaguitls and Nutkens.—Of Vancouver Indians, we tested five males and two females, belonging to two local tribes.

VANCOUVER INDIANS—KWAGUITLS AND NUTKENS

Highest Audible Tone

Name	Age	Sex	Tribe	Right Ear		Left Ear	
Bob	40	Male	Kwaguitl	26,264	D. V.	26,854	D. V.
Charley	28	"	"	28,766	"	28,766	"
Jasper	27	"	Nutken	28,048	"	26,854	"
Curley	21	"	"	26,264	"	28,048	"
Atleo	64	"	"	30,270	"	28,766	"
Ellen	35	Female	"	29,508	"	26,854	"
Anna	30	"	"	28,766	"	26,854	"
Average				28,269	"	27,571	"
S. D.				1,413	"	852	"

With the exception of Atleo—whose hearing record is the best of the group—these Indians were all in the prime of life. They are Indians, moreover, who have only to a small degree been influenced by contact with Whites, inasmuch as their homes are far removed from those parts where civilization has made its march. So far as the data will permit of generalization, therefore, I believe these peoples to be fairly representative of the more intelligent Indian of North America, before his organism has become modified by its adjustment to the social conditions and habits of the invading Caucasians.

In the case of both ears, the degree of variability among the records is fairly small. By reference to the table above, it is seen that the range of all the cases is encompassed by less than 4,000 vibrations to the second; or that part of the musical scale lying between gis^6 and bis^6, *i. e.,* within the range of less than two whole tones. The records for the left ear (exhibited in Table III.) are lower, as a whole, but the difference between the highest and lowest record is still smaller, being only about a semi-tone (1912 D. V.).

It is, however, worthy of note that not a single record of the Vancouver Indians is as high as the average for Whites. In fact, the highest Vancouver Indian record for the right ear is 1,000 D. V. lower than the mean for Whites, while the highest for the left ear is 3,500 D. V. lower. The numbers are too small to apply statistical methods with satisfaction, but there certainly is no question of the evident tendency toward the inferiority of these Indians in hearing range, as compared with Whites. The records of the two women are poorer than those of any of the men, but additional representatives of the women might tend to reverse this result.

Patagonian Indians—The Tehuelche.—We were able to examine only four men of this tribe, the data concerning whom are given below in detail. These Indians represent a grade of culture slightly lower than that of the Vancouvers just considered; a tent-living instead of a house-dwelling people, a nomadic instead of a home-building folk.

PATAGONIAN INDIANS—THE TEHUELCHE

Highest Audible Tone

Name	Age	Right Ear	Left Ear
Cosimero	24	26,854 D. V.	26,264 D. V.
Canjo	35	28,766 "	27,444 "
Senchel	55	Hearing very defective. About 10,000 D. V. for both ears. An acuity defect.	
Boni Farci	18	35,100 D. V.	32,180 D. V.
Average		30,240 "	28,630 "
S. D.		3,551 "	2,592 "

Only three records were included in the average. The number examined is too small to draw any general conclusions. However, with the exception of Boni Farci, the upper limit of hearing of all was found very much inferior to that of the average for Whites. I leave the data without further comment.

Indians as a Whole.—From the foregoing, it is evident that whether taken tribe by tribe or as a whole, on the average as well as individually, the experimental results indicate that Indians do not possess as great a range of hearing as do Whites. If, indeed, we lump the records of all the Indians irrespectively, those who have had the cultural advantages of the Whites—the tribes represented in the group taken from the Indian School—with the smaller groups represented by Indians closer to nature, we find that for the right ear, 60 of a total of 83, or 73 per cent. of the individuals rank below the average for Whites, and 64 of 82 Indians, or 77 per cent. stand lower than the average of Whites as regards the left ear. It is worthy of note too that the better records were made by those Indians who had attended, more or less, the Government Indian Schools. It was my impression also, formed at the time of making the measurements, that the better records were made by those individuals who were, all around, more intelligent and alert. Unfortunately, it had not occurred to me to make record of the degree of intelligence of each person when tested, although it might be nothing more than a personal opinion based upon observation only.

Roughly, this could easily have been done with a fair degree of accuracy, since I was associated with and saw at work each person for the better part of an hour. Had I made such an observation, it would have enabled me to determine in a loose way, at least, how far the differences in the upper limit of hearing between Whites and Indians are psychological and to what extent organic in character. A little further on, we shall have occasion to revert to this subject again.

Filipinos.—In all, 97 Filipinos were tested for their upper threshold of hearing. The data for four, however, were so palpably in error, that the records were rejected. We thus have 93 records which form the basis for the following study. So far as I am aware, no statement has ever been made with reference to the hearing of any of the Filipino people, in any of its aspects. It is doubtful, too, whether so good an opportunity for testing these peoples, and under circumstances so favorable, will soon be found again. If taken in the Islands, it would be no easy matter to collect into a laboratory, for testing, as many as a hundred individuals, representing, as did these, almost every section of the Philippine Archipelago.[8] We feel especially gratified with the results obtained on the Filipino peoples. For the opportunity of making measurements of the Filipinos, I am under obligations to Major Haskell, U. S. A., under whose orders the men came to the laboratory for the testing and without which orders it is doubtful whether the tests could have been made at all. Obligations are also due to Dr. Wilson, of the Philadelphia Museum, who, in the capacity of Director of the Philippine Exhibit, used his influence in our behalf to the extent of recommending that we be given permission to make the measurements.

The men were brought to the laboratories, four at a time, this number being tested in the forenoon and the same number immediately after dinner. It may here be stated that the hearing examinations were but two of a great number made on each individual. The men were ignorant as to the object of the measurements nor was there a hint given of their purpose. Notwithstanding, they approached the tests with a great deal of interest and zeal, and seemed eager to compare their several records with those of their fellows. None of them suspected that there might be such a thing as a racial difference inasmuch as they referred to my record as one, in all respects, comparable with one of theirs.

The men were taken into the sound-room, one at a time, and the test conducted with as great dispatch as possible in order to avoid the flagging of interest and the onset of fatigue.

[8] See page 6 for a more detailed description of these peoples.

It is seen, from the age distributions found in Table IV. (See p. 34) that 4 men were younger than 18 years and 5 older than 25. The oldest of the group was but 30. Were one to select the individuals deliberately with the test of the upper threshold of hearing in view, it is difficult to see how a more favorable lot could have been secured. The mean age was 21 years and 1 month.

Turning now to tables I., II. and III., it is seen that the figure representing the upper threshold of hearing of Filipinos for the right ear was found to be 29,916 vibrations to the second on the average, and for the left ear 29,886 vibrations. The small degree of variability found in the group is at once striking. The average deviation is very much smaller than that for either Whites or Indians (See Table I.). So little variability, no doubt, may be accounted for by the fact that the variation in age is likewise small and, moreover, it may be noted that in mental alertness and general intelligence, these individuals were more nearly on a par than those of any other group measured, and, consequently, all mental factors concerned in the tests would become more largely equalized.

When compared with Whites, the upper limit of hearing of the Filipinos is decidedly lower, not only on the average, but also as regards the general distribution of the individual cases; a fact which stands out in the general distributions found in tables II. and III. The figures show that Whites on the average have an upper limit of hearing higher by 2,369 vibrations for the right ear and by 2,301 for the left. On the basis of mathematical probability, the chances are such as to amount almost to an absolute certainty (10,000 to 1) that, even were the numbers infinitely increased, the upper limit of Filipinos would, on the average, be lower than that of Whites. Indeed, the chances are about 4 to 1 (3.9 to 1) that the difference between the upper limit of hearing of Whites and Filipinos amounts to at least 2,000 vibrations. And, inasmuch as the number measured is sufficiently large to render the data susceptible to fairly accurate statistical treatment, the reliability of these figures may be accepted with a certain degree of confidence.

Something of the standing of the upper limit of hearing of the Filipinos as a whole may be inferred from the distribution of the records as found in tables II. and III. Not only the mode, but the distribution as a whole is found to fall distinctly below that for Whites. Take, for example, the records of the right ear. But 19 of the whole number of 93 (about 20 per cent.) are as high as the median record for Whites; for the left ear, only one Filipino record is as good as the median for Whites.

In the data just presented, were included the records of 13

Filipino students attending American colleges and universities, who were temporarily connected with the Exposition and whom we tested as they chanced to stroll into the laboratories. These records afford an opportunity for testing to what extent the factor of intelligence was instrumental in effecting differences found to exist between the upper limit of Filipinos and Whites. In mentality, I take it, these Filipino students were on a par with the freshmen and sophomores in our American colleges and universities. They ranked, moreover, in point of intelligence at least equal to the Whites with whom the Filipinos are compared above, inasmuch as the Whites were for the most part artizans and tradespeople, with a few who had completed college courses. Below I give the individual records of these Filipino students, both for the right and left ears. Also I give the averages of the thirteen records and the variability.

FILIPINO STUDENTS—MOSTLY TAGALOGS

Highest Audible Tone

Right Ear	Left Ear		Right Ear	Left Ear
31,170 D. V.	33,060 D. V.		32,180 D. V.	32,180 D. V.
34,000 "	31,170 "		32,180 "	32,180 "
31,170 "	31,170 "		35,100 "	33,060 "
32,180 "	32,180 "		30,270 "	28,766 "
29,508 "	28,766 "		33,060 "	33,060 "
33,060 "	30,270 "	Average	31,878 "	31,157 "
30,270 "	30,270 "	A. D.	1,323 "	1,361 "
30,270 "	28,766 "			

Even these intelligent and educated Filipinos, it is seen, have an upper range of hearing distinctly lower, on the average, than do Whites; the difference for the right and left ears respectively being 407 and 1,930 vibrations. Were the numbers of these intelligent Filipinos larger, it would be interesting to note what the probabilities of a real difference are between their upper limit and that of Whites. The data would incline one to believe that they would amount to practically a certainty. It requires to be noted, moreover, that with one exception, the records of the Filipino students were among the best of the Filipinos tested, so that there can be no doubt but that the psychological factor is significant in accounting for some of the differences discovered between Filipinos and Whites, though, to be sure, by no means all.

Army officers, who had done service for some time, in the Philippines, and some teachers who had spent two or three years in the Islands, with whom I conversed, without exception informed me that they, too, had experienced defective hearing in the Philippines. But they attributed this abnormality to the action of the quinine, which they found it necessary to take in large quantities, on account of the prevalence of fevers. Since the natives are immune to malaria and other tropical maladies, and do not use quinine or other drugs with similar medicinal properties, the explanation just noted could not account for the defective hearing of the Filipinos, which, as will hereafter be pointed out, extends not only to a diminution of the range but to an actual lack of acuity as well. It would be interesting to know whether this is common to all dwellers within tropics, inasmuch as the results are in accord with the data collected from the inhabitants of the Torres Straits.[9]

Ainu.—Excepting the Vancouver Indians, the Ainu rank lowest as regards the upper limit of hearing of any of the peoples I examined. Owing to the fewness of the numbers, it is impossible to do more than present the results with a statement of their general tendency, and perhaps this can best be seen from the comparative tables II. and III. It may be noted that all of the Ainu records fall lower than the average for Whites for both the right and left ears. The general tendency seems to indicate an upper limit slightly lower even than that of Filipinos.

THE AINU

Highest Audible Tone

Name	Age	Sex	Right Ear	Left Ear
Yazo Osawa	23	Male	31,170 D. V.	31,170 D. V.
Kutorge Hiramura	38	"	30,270 "	24,754 "
Santukuno Hiramura	53	"	25,212 "	26,264 "
Goro Bete	28	"	29,508 "	32,180 "
Sangea Hiramura	56	"	Less than 10,000 D. V. (both ears). Record not included in the average.	
Ume Osagwa	19	Female	28,048 D. V.	33,060 D. V.
Shutratek Hiramura	33	"	25,212 "	26,264 "
Kin Hiramura	6	"	30,270 "	30,270 "
Average			28,846 "	29,529 "
A. D.			1,666 "	2,946 "

Much of the auditory inferiority of the Ainu is undoubtedly to be accounted for on purely psychological grounds. They seemed an excessively stupid people, ranking next to the lowest of all the primitive peoples collected on the Exposition grounds. Their minds seemed unresponsive and lethargic. They apprehended meanings poorly. Things once apprehended, moreover, held their attention for a moment only when they seemed immediately to relapse into a state of mental indifference. I never could feel quite certain that they were hearing even when they said they were, inasmuch as in the region of the threshold values the number of false statements was exceedingly large.

[9] See Report Cambridge Anthropol. Exp., Vol. 11.

The Ainu have been little studied and little is known of them. So far as I am aware, no scientific measurements of them have ever before been made, so it is quite unfortunate that more of the race were not available for study. It is a race, too, which, according to travelers and missionaries, is fast dying out; hence, in the near future, will no longer be open to scientific observation.

The Pigmies—Batwa, Batsuba and Cheri Cheri.—Of the so-called Pigmies proper, there were at the Exposition only six representatives. I present hearing records of the entire number.

THE PIGMIES—BATWA, BATSUBA AND CHERI CHERI

Highest Audible Tone

Name	Age	Tribe	Right Ear	Left Ear
Shamba	30	Batwa	31,170 D. V.	31,170 D. V.
Malinga	16	"	37,560 "	37,560 "
Bushaba	13	"	32,180 "	39,220 "
Latuna	15	Batsuba	35,100 "	35,100 "
Otabenga	17	Cheri Cheri	30,270 "	31,170 "
Lumme	17	" "	33,060 "	30,270 "
Average			33,323 "	34,081 "
A. D.			2,071 "	3,212 "

The records of the Pigmies are all high. Were the same relative distribution to continue for a hundred Pigmy hearing records, they would be found to possess an upper range of audition superior to that of any other people, including Whites. The general distribution of their results stands higher than that for Whites. (See tables II. and III.) Only one white male was found to possess a range of hearing higher than that of any Pigmy.

How shall we account for this manifest superiority of the Pigmy's hearing? Certainly not on the basis of a superior mental attitude toward the tests, since the Pigmy ranks low in the mental scale—only slightly higher than the Ainu whom we have just considered. Moreover, their interest in the test can not be said to have been especially keen. It was certainly by no means the equal of the Filipinos whose upper limit was found to be especially low. The question of age was a factor, no doubt, since the Pigmies were all boys, but even this does not account for all the difference found. Perchance, something of a relation may exist between a high degree of sensitivity and extreme motility. The Pigmy is a motor individual. Like his next of kin, the negro, in his native haunts, he is perpetually on the move. His reactions to incoming stimuli likewise are direct and excessively overt. Other than this, there is certainly nothing in the Pigmy's environment or mode of living which should tend to develop and cultivate any peculiar aptitude in the way of sensory acuity, with which he seems naturally to be gifted.

CHAPTER V

The Upper Limit of Hearing as Affected by Age and Sex

From the data I was able to collect at St. Louis and some gathered subsequently from children in the public schools, it was possible to select material which will throw some additional light upon the relation that obtains between range of hearing and age and sex. As regards Whites alone, the data represents tests on 385 individuals; 209 males and 176 females, ranging in age from 5 to 65 years.

By consulting the tables and charts which follow (See tables V. to XII.) it will be observed that the records of the individuals have been distributed into *four-year* groups, the first group representing the upper limit of hearing of children between 5 and 8 years inclusively; the second group 9 to 12 years, and so on. Beyond 49 years, the numbers tested were so few that the four-year groupings were dropped and the records lumped. Such a procedure, too, is justifiable on other grounds. With the approach of senescence is met a decrease of sensitivity, in general, in consequence of which many of the data would bear record of a decline in general sensibility rather than a diminution only in the one particular function of hearing piercing tones.

In Table V. the data are so arranged as to contribute information toward the significance of age in influencing the upper limit of

TABLE V

Highest Audible Tone, According to Age

Ages	No. of Cases	White Males and Females		White Males and Females	
		Right Ear. Average	Left Ear. Average	Right Ear. Median	Left Ear. Median
5– 8	41	34,826	34,525	34,000	35,100
9–12	32	34,614	34,939	35,100	34,200
13–16	54	34,418	34,224	34,000	34,000
17–20	40	32,466	32,415	32,480	32,200
21–24	48	33,491	33,025	32,480	33,800
25–28	53	31,557	32,390	32,180	32,200
29–32	31	31,464	32,000	32,180	31,200
33–36	17	28,816	29,046	28,900	29,000
37–40	27	27,512	25,054	26,854	28,100
41–44	12	27,953	29,994	29,508	27,600
45–48	20	27,382	27,741	26,854	27,500
49+	12	26,020	26,188	28,048	25,212

TABLE VI

HIGHEST AUDIBLE TONE, ACCORDING TO SEX

Ages	No. of Cases	White Males		No. of Cases	White Females	
		Right Ear. Average Vibration Frequency	Left Ear. Average Vibration Frequency		Right Ear. Average Vibration Frequency	Left Ear. Average Vibration Frequency
5–8	18	35,180	34,926	23	34,535	34,211
9–12	17	34,500	34,740	15	34,861	35,082
13–16	31	34,207	33,893	23	34,671	34,713
17–20	24	32,103	31,546	16	32,991	32,849
21–24	26	33,069	31,358	22	33,565	33,714
25–28	19	30,834	31,480	34	31,048	32,059
29–32	22	31,105	31,761	9	32,740	32,168
33–36	10	29,316	30,005	7	28,101	28,161
37–40	15	24,142	26,498	12	25,152	27,255
41–44	6	25,316	27,084	6	30,748	31,035
45–48	11	27,435	27,894	9	27,319	27,571
49+	10	25,424	26,244	2	25,224	25,826

hearing. In this table the sexes have not been segregated. The table is made to show differences between different periods of life, both in terms of the average of the individual records of each age group and in terms of the median or middle record. The latter gives the data in such terms that the influence of pathological, or functional, disturbances of hearing is largely eliminated. That is, extreme records which have a tendency to skew the average lose their unwarranted weight. From this table, as well as from others which will follow, something, also, may be inferred as to the relative range of sensitivity of the two ears.

In Table VI. the data are arranged so as to exhibit any sex differences that might be found for the various age groups. Neither in this table nor Table V. has there been worked out, however, any measure of variability, inasmuch as the character of the distributions may be seen in tables VII. to XII.

Tables VII. to XII. present the original data in such form that they may be worked over by anyone who cares to review the question; and, indeed, they show more convincingly than any series of figures representing averages, modes or medians could possibly do, the tendencies of the several groups measured. Perhaps a word of explanation is necessary to an understanding and interpretation of these tables. Taking, for example, Table VII. In the column to the left are given the lengths of the whistle cavity employed in the various hearing measurements, and in the column to the right the corresponding vibration frequencies (double) of the resulting tones. At the head of the several columns are indicated the different age groups; "5–8" indicating that the data in the column beneath are those secured from boys and girls without distinction of sex; and so on for the remaining columns. Again, under the "5–8" caption, the "4" indicates that four individuals were measured in which the whistle cavity had a length of 1.4 mm. or the tone possessed a vibration frequency of 39,220 D. V. (double vibrations), and so on, the "12" indicating that the upper limit of twelve children was marked by a tone of 35,100 D. V.

Extended interpretation and discussion of the data contained in tables V. to XII. are uncalled for in connection with the problem of racial differences, of which this paper particularly treats. A few words, however, are perhaps not out of place.

It will be observed that the number of individual measurements recorded, especially as regards the younger years, is sufficient to make the conclusions fairly definite. From tables V., VII. and XII. it stands out fairly clear that there is practically no shortening in the range of hearing before the age of sixteen years, but that after this age the upper limit falls slowly, having sunk about three-

TABLE VII

UPPER LIMIT OF HEARING

Males and Females (White)—Right Ear

Whistle Length mm.	Ages in Years												Vibrations (Double) Per Sec.
	5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and Over	
1.2													42,960
1.3			1										40,840
1.4	4	3	3		2	1							39,220
1.5	3	3	6		3	2	1						37,560
1.6	4	3	4	3	6	2	1			1			36,360
1.7	12	7	5	5	5	3			1				35,100
1.8	4	6	15	5	6	5	4			1	1		34,000
1.9	7	2	7	5	7	8	2						33,060
2.0	6	4	8	10	5	8	7	2	1		1		32,180
2.1	1	3	4	3	6	6	6	3	2	2	2	1	31,170
2.2		1	1	3	4	3	3	2	4	1	2	1	30,270
2.3				2		3	2		2		1	1	29,508
2.4				2	1	1	3	2	2	1		1	28,766
2.5				2	2	5		2	2		3		28,048
2.6									1				27,448
2.7						2		3	2		1	1	26,854
2.8								2	1	1	1		26,264
2.9						1	1		2				25,724
3.0						2	1		1	2	2	2	25,212
3.1								1	2		1		24,754
3.2									1		2		24,196
3.4						1				2	2	2	23,020
3.6									1		1	1	22,217
3.8									1	1		1	20,973
4.0									1				18,496
Average D. V.	34,826	34,614	34,418	32,466	33,491	31,557	31,464	28,816	27,512	27,953	27,382	26,020	

TABLE VIII

UPPER LIMIT OF HEARING

Males and Females (White)—Left Ear

Whistle Length mm.	Ages in Years: 5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and Over	Vibration Frequency D. V.
1.2					1								42,960
1.3		1				1							40,840
1.4	3	3	1	1									39,220
1.5	5	4	8		2	2							37,560
1.6	3	3	4	1	1		1						36,360
1.7	7	6	6	3	5	2			1	1	1		35,100
1.8	5	5	14	4	2	1	2						34,000
1.9	8	4	8	3	2	4	3		1	1			33,060
2.0	9	2	4	3	1	2	4	1	1	2	1		32,180
2.1	1	2	4	3	5	3	4	1	2		2		31,170
2.2		2	2	2	1	2		3			1	1	30,270
2.3			1		2	1	2	1	1	2		1	29,508
2.4				2	1			1			2	3	28,766
2.5				1	1	1	1	2	1		1		28,048
2.6					1			1	1	2	1		27,448
2.7								1			5	1	26,854
2.8						1			1	1	1		26,264
2.9				1		1		1	1				25,724
3.0									1			1	25,212
3.1									1	1			24,754
3.2									1	1	2		24,196
3.4											1	1	23,020
3.6									1		1	1	22,217
3.8													20,973
4.0									2			1	18,496
Average D. V.	34,525	34,939	34,224	32,415	33,025	32,390	32,000	29,046	25,054	29,994	27,741	26,188	

fourths of an octave on the average by the forty-ninth year. The data giving the median record for each age group disclose the same facts, so that it seems to matter not whether we speak in terms of the average or the median (See tables V. to XII.).

It will be remembered that Alderton, Blake, Galton, Zwaardemaker, Caperius, Myers[1] and others found that by the age of 12 to 13 years, the upper limit of hearing already exhibits considerable shortening. My data necessitate a conclusion quite at variance with that of these earlier experimenters. Nor can I suggest an explanation for this lack of harmony in our experimental results.

A mathematical statement of the probability of a difference in the range of hearing on the average for years of life beyond seventeen, is wholly superfluous in view of the distributions of individual records exhibited in tables VII. to XII. which makes this conclusion absolutely certain. But to explain this shortening in range with

[1] See discussions under Chapter II., page 19, et seq.

increasing years is a more serious question. Whether it is but a symptom of a general insensitivity of the organism as the individual ages or only an atrophying of certain tissues through disuse, which were of service to man in a lower stage of his culture, or whether it may be due to other factors, are questions which await experimental determination. In view of the relative inferiority of the extent of hearing range among primitive peoples, the former of the two suggested explanations perhaps seems the more plausible.

TABLE IX

UPPER LIMIT OF HEARING

Males—Right Ear

Whistle Length mm.	Ages in Years: 5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and Over	Vibration Frequency D. V.
1.2													42,960
1.3													40,840
1.4	2	1	2		1								39,220
1.5	1	2	4		2	1	1						37,560
1.6	2	2	1		3	1							36,360
1.7	6	4	4	4	3	1							35,100
1.8	3	2	8	2	3		3						34,000
1.9	4	1	4	4	4	3	1						33,060
2.0		3	3	6	2	2	4	2					32,180
2.1		1	4	1	2	3	4	2			2	1	31,170
2.2		1		3	2	2	3	2	2	1	1	1	30,270
2.3			1	1			2		2		1		29,503
2.4				2	1		2		2			1	28,766
2.5				1	2	3		1	2		2		28,048
2.6									1				27,448
2.7						1		1	2			1	26,854
2.8								1	1	1			26,264
2.9							1		1				25,724
3.0						1	1			2	2	2	25,212
3.1					1			1	2		1		24,754
3.2											2		24,196
3.4						1				1		2	23,020
3.6										1		1	22,217
3.8													20,973
4.0												1	18,496
Average D. V.	35,198	34,500	34,207	32,103	33,069	30,834	31,105	29,316	24,142	25,366	27,435	25,424	

As to a difference in sensitivity between the two ears, taking the results as a whole, all ages together, nothing like a significant difference stands out in the tables (Table V.). As to a sex difference, it would appear from Table VI. that woman's range of hearing extends slightly higher than man's; the difference is however for each age-group too small to possess any high reliability.

TABLE X

UPPER LIMIT OF HEARING

Males (White)—Left Ear

Whistle Length mm.	Ages in Years: 5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and over	Vibration Frequency D. V.
1.2													42,960
1.3													40,840
1.4	2	1											39,220
1.5	1	2	4										37,560
1.6	2	1	2										36,360
1.7	4	5	5	1	1	1							35,100
1.8	4	4	7			1							34,000
1.9	3	2	5	2	1	2	2						33,060
2.0	2	1	3	1	1	1	2						32,180
2.1		1	2	1	2		2	1			2		31,170
2.2			2	1				3				1	30,270
2.3			1				1			1			29,508
2.4				2	1						2	3	28,766
2.5					1			1	1		1		28,048
2.6									1	2			27,448
2.7											3	1	26,854
2.8										1	1		26,264
2.9									1				25,724
3.0												1	25,212
3.1									1	1			24,754
3.2											1		24,196
3.4												1	23,020
3.6													22,217
3.8													20,973
4.0												1	18,496
Average D. V.	34,926	34,740	33,893	31,546	31,358	31,480	31,761	30,005	26,498	27,084	27,894	26,244	

SUMMARY AND CONCLUSION

Placed in the order of superiority, beginning with the people whose upper limit was found to be the highest, the peoples arrange themselves in the following order:

Right Ear

1. Pigmies.
2. Whites.
3. Cocopa Indians.
4. Indians from the Schools.
5. Patagonian Indians.
6. Filipinos.
7. Ainu.
8. Vancouver Indians.

Left Ear

1. Pigmies.
2. Whites.
3. Cocopa Indians.
4. Indians from the Schools.
5. Filipinos.
6. Ainu.
7. Patagonians.
8. Vancouver Indians.

There is a slight difference in relative order for the two ears, respectively, which may be, and probably is, due to the fact that

TABLE XI

UPPER LIMIT OF HEARING

Females (White)—Right Ear

Whistle Length mm.	Age in Years: 5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and Older	Vibration Frequency D. V.
1.2													42,960
1.3			1										40,840
1.4	2	2	1		1	1							39,220
1.5	2	1	2		1	1							37,560
1.6	2	1	3	3	3	1	1			1			36,360
1.7	6	3	1	1	2	2			1				35,100
1.8	1	4	7	3	3	5	1			1	1		34,000
1.9	3	1	3	1	3	5	1						33,060
2.0	6	1	5	4	3	6	3		1		1		32,180
2.1	1	2		2	4	3	2	1	2	2			31,170
2.2					2	1			2		1		30,270
2.3				1		3						1	29,508
2.4						1	1	2		1			28,766
2.5				1		2		1			1		28,048
2.6													27,448
2.7						1		2			1		26,854
2.8								1			1		26,264
2.9						1			1				25,724
3.0						1			1				25,212
3.1													24,754
3.2									1				24,196
3.4										1	2		23,020
3.6									1		1		22,217
3.8									1			1	20,973
4.0									1				18,496
Average D. V.	34,535	34,861	34,671	32,991	33,565	31,048	32,740	28,101	25,152	30,748	27,319	25,240	

the numbers making up some of the groups were small. The larger groups, Whites, Indians (from schools), and Filipinos, on the other hand, retain the same order, for the two ears, unchanged. It is reasonably certain that Whites have a higher upper limit of hearing than do Indians and that Indians in turn have a wider range of tonal hearing than do Filipinos.

The data, therefore, bring out strikingly and justify a conclusion, amounting, practically, to a certainty that racial differences in hearing exist, at least so far as the upper limit of hearing goes, and that the more cultured people rank most favorably in this respect. So far as the data are comparable, too, my experimental conclusions with Filipinos are strictly in accord with those of Dr. Myers, on peoples of the same race. Dr. Myers's results are stated in terms of the cavity length of a small Hawksley-Galton Whistle and, consequently, his data can not be compared directly with my own, but Myers found that, for the most part, the Papuans whom

TABLE XII

UPPER LIMIT OF HEARING

Females (White)—Left Ear

Whistle Length mm.	Ages in Years: 5–8	9–12	13–16	17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48	49 and Over	Vibration Frequency D. V.
1.2					1								42,960
1.3		1				1							40,840
1.4	1	2	1	1									39,220
1.5	4	2	4		2	2							37,560
1.6	1	2	2	1	1		1						36,360
1.7	3	1	1	2	4	1			1	1	1		35,100
1.8	1	1	7	4	2		2						34,000
1.9	5	2	3	1	1	2	1		1	1			33,060
2.0	7	1	1	2		1	2	1	1	2	1		32,180
2.1	1	1	2	2	3	3	2		2				31,170
2.2		2		1	1	2					1		30,270
2.3					2	1	1	1	1	1		1	29,508
2.4								1					28,766
2.5				1		1	1	1					28,048
2.6					1			1			1		27,448
2.7								1			2		26,854
2.8						1			1				26,264
2.9				1		1		1					25,724
3.0									1				25,212
3.1													24,754
3.2									1	1	1		24,196
3.4											1		23,020
3.6									1		1	1	22,217
3.8													20,973
4.0									2				18,496
Average D. V.	34,211	35,082	34,713	32,849	33,714	32,059	32,168	28,161	27,255	31,035	27,571	25,862	

he examined, possessed a rather noticeably shorter range of hearing than did the peoples of Scotland in whose measurement he employed the same whistle.

Only two factors in explanation of the relative shortness of the range of hearing among primitive peoples have been suggested:

(*a*) Attention was called to the psychological factor concerned with the subject's attitude toward the test, or the subject's relative ability to attend to stimuli. This mental factor is undoubtedly a prominent one. It likewise has its motor aspects and relations. Those individuals who are relatively more alert, and whose range of motor responses likewise is greater, were, on the whole, found to possess a greater range of auditory sensitivity, than individuals less given to motility.

(*b*) The second factor, perhaps, is fundamentally the same as the first. It refers to climatic and geographical influences. It was suggested that, perchance, variations in upper limit of hearing may be induced by factors arising from the different latitudes in which the individuals have been born and have grown up.

Each of the suggested factors just enumerated, however, awaits additional experimental evidence for verification. Unfortunately, I had neither the time nor facilities to enter further into the subject.

Lastly, it was found that age plays an important rôle in shortening the upper limit of hearing. While no perceptible shortening occurs before the sixteenth year, after this age the upper limit gradually drops, shortening almost an octave on the average during the next thirty years.

PART II

AUDITORY ACUITY

CHAPTER VI

Historical

Reference to the literature of auditory acuity from the point of view of this paper implies its consideration from two distinct aspects: (1) the relative acuity of some of the inferior races, and (2) a quantitative auditory measure. With reference to auditory tests on primitive peoples, the literature is indeed meager. If we exclude haphazard statements, and such as have been made by tourists without reference to the physical and psychological factors involved, the classic work of Dr. Charles Myers[1] on the Hearing of the Murray Islanders is about all of consequence that has found its way into print.

In the seven volumes which have been published relating to almost every phase of the life and environment of the natives of southern South America, P. Hyades[2] and J. Deniker encompass the problem of hearing within a single paragraph. The statement is so abbreviated that I quote it in full: "The Fuegians have the sense of hearing particularly developed owing to their conditions of life. Yet, by divers experiments with the watch, with the diapason, etc., we proved that the acuity and the range of hearing, among these people do not exceed that of Europeans especially gifted in this respect. It was observed also that noises such as are habitually disagreeable to us (Europeans), an explosion of a charge of powder, or the hissing of steam in escaping from a locomotive, do not by any means produce an unpleasant effect upon their ears." Such a statement as this, while indicating the direction of a tendency in the gross, does not give us much information on the question in point. The inference is, that the hearing is equal, at least, to the average among Whites. Nothing is said, moreover, as to the method pursued in testing, the individual differences observed, or of the number of persons tested.

[1] "Anthropological Expedition to Torres Straits," Vol. II.

[2] "Mission Scientifique du Cap Horn," Tom VII., p. 209. 1882–3.

In keeping with the statement just noted are such observations as that of Robertson[3] who remarks "As a result of very many observations of an unscientific kind, I could never discover that Kaffirs displayed any superiority to other races in their certainty of hearing." And that of Sir Francis Galton[4] who says "My own experience also, so far as it goes, with Hottentots, Damars, and some other wild races, went to show that their sense of discernment was not superior to that of white men." Giltschenko[5] also relates that the Osset, whom he examined, was found to hear the tick of the watch at no greater distance than other peoples. However, on the open plains and among the hills, the Osset understands spoken words and perceives significant sounds at extraordinary distances. But, as Dr. Myers critically remarks, extraordinary auditory acuity under such circumstances as that just related, where the stimuli possess a conscious significance, no more indicates superior hearing than does the superior ability to analyze clangs give evidence of a more efficient organic hearing sensitivity.

Myers quotes a personal communication from Stanley Gardener, in this connection, which illustrates what is patently true of all sense avenues, namely: that even very intense stimuli, if not significant or familiar, are usually unobserved. Gardener, while in the Maldive Islands, had an American clock hanging in his bungalow. He observed that the natives who had never seen a clock would often approach within two yards of the timepiece without noticing its tick, and even after he had called attention to the sound, they usually experienced considerable difficulty in localizing it. Gardener further relates that frequently he passed behind the natives on the sandy seashore, but that, invariably, they failed to distinguish his booted tread from that of the bare-footed savage.

More forcibly, perhaps, than any *a priori* statement of the matter, these observations emphasize the importance of carefully selected and well devised hearing tests for measuring primitive peoples, such as will not afford undue advantage to the intelligent over the inferior race with which it is compared. In other words, if a comparison of hearing is to be valid, the stimuli must so nearly as possible have the same psychological value for all the individuals in question. Unhappily, this condition is rarely fulfilled, not only as regards auditory stimuli, but those affecting other sense avenues as well. Whether suitable conditions for making comparisons obtained in the tests of Hyades and Deniker, the statement of their conclu-

[3] "Kaffirs of Hindu Kush," 1896, p. 174, quoted from Myers, *loc. cit.*

[4] "Inquiries into Human Faculty," London, 1883, p. 32.

[5] *Biol. Centralb.* 11: 304–318. 1891.

sions leaves us in ignorance. Certainly such statements as those of Galton, Robertson, Gardener and Giltschenke lose all their value when weighed by psychological standards, simply because unequal psychological units of measure were employed in the comparisons. Myers more nearly meets the situation than any of the above in that he employed a metallic click, which possesses comparatively few associations, in some of his measurements, while in others, an ordinary noise produced by a pith ball falling upon an inelastic surface served as a stimulus. The last mentioned instrument consisted of a telescope tube mounted vertically; at the base of which extended a piece of felt, placed at an angle. The pith ball was allowed to drop through the tube, strike the felt below and rebound, falling noiselessly upon some velvet cloth stretched to catch it. The height from which the pith ball fell could be varied so as to change the intensity of the sounds. This device was found unserviceable, however, on account of surrounding noises. Myers and Rivers found considerable difficulty in their work of measuring the auditory acuity of the Murray Islanders, by reason of extraneous noises which so confused the subject under examination that he was frequently unable to tell when the stimulus was present and when not; that is, whether he heard the appropriate sound or not. Later, a Politzer Hörmesser was tried but with almost as little success, though a certain number of measurements were made with the instrument. But in by far the greater number of Myers's tests, use was made of an ordinary stop watch, the acuity of the native being recorded by the relative distance at which he and one of the experimenters could hear the tick. In each test, one of the experimenters stood beside the subject and also listened for the tick. It was thus observed whether or not he heard the tone at a greater distance than did the native; the distance at which the watch or the Politzer acuometer was audible to the experimenter always being the denominator of a fraction standing for the acuity of the native tested. Thirty-five natives were tested for hearing. Although the tests, as has just been indicated, were extremely rough, they served to show that Papuans, on the whole, do not hear as well as do Europeans, though some of the individual Papuans, indeed, heard better than did either Myers or Rivers. On their return to England, Myers compared his own hearing and that of Rivers with the hearing of other Europeans and these comparisons seemed further to confirm the relative inferiority of the auditory acuity of the Murray Islanders.

From the second point of view of my measurements of hearing, the literature is voluminous. Anything like a comprehensive review of all of its phases would, therefore, at once carry us entirely too far afield. Indeed, it would be a bootless task, by reason of the diversity of methods employed and devices used, which it would be wholly impossible to place into any scale of equivalents.

Very considerable work has been done on the continent of Europe in the way of establishing quantitative auditory measures. Something in the same direction has been accomplished in the United States, and Lord Rayleigh, in England, has pretty thoroughly broken the ground in the way of making it possible to determine the energy proceeding from certain sonorous sources. I shall not, at this time, attempt to review the merits of the various devices and instruments that have been put forward for testing auditory acuity. Few of these should concern us in connection with a study of a quantitative measure of hearing, inasmuch as they are at best only semi-quantitative in character. The mere mention of a few such as the following will suffice to illustrate the point in question: The Seashore audiometer;[6] Bryant's and Bentley's[7] phonograph audiometer; the audiometer of De Graffe;[8] the audiometer of D'Arsonval;[9] a device for measuring hearing by R. Panse;[10] an instrument by W. De Bechterew;[11] an *accoumetrie metrique* by Tretop;[12] an apparatus for measuring auditory acuity based on mechanically produced vowels, by Robin;[13] etc. Among such devices, also may be classified instruments such as the well-known Politzer Hörmesser, the watch test, speech and whisper tests; in fact, all those methods and ways for testing hearing which depend on empirically established norms, which are not interchangeable as among different investigators. Such devices have their value in individual laboratories and clinics, where only semi-quantitative results are required and where opportunity is afforded for measuring and establishing, once and for all, the hearing equivalent of the instrument in use. For purposes of investigation, however, they answer but poorly, in that it is never possible for other investigators to review and verify any experimental conclusions reached.

Among the first work done to measure, quantitatively, the intensity of a sound in objective units—at least relatively objective units—was that by Vierordt,[14] in 1878. This investigator concerned

[6] *University of Iowa Studies,* **2**: 55. 1899.

[7] *Science,* **19**: 959. 1894.

[8] *Arch. Int. Laryn.* **15**: 96.

[9] *Arch. Int. Laryn.* **15**: 96.

[10] *J. of Laryngol.* **19**: 534.

[11] *Arch. de Psych.* **5**: 108.

[12] *Bull. de Laryng., Otol., etc.* **8**: 20.

[13] *Bull. et Mem. Soc. d'Anthropol.* **3**: 209. 1902.

[14] Das Mass der Schallstärke, *Ztsch. f. Biol.* **14**: 361. 1878.

himself with the question of the relation between the height of fall of a ball and the resulting sound intensity. Vierordt believed he had established an experimental formula by which could be determined in relative terms the intensities of two succeeding sounds. Vierordt's work was later reviewed by Oberbech,[15] who established experimentally the formula $i = ph^e$ instead of the formula $i = ph^c$ which Vierordt had proposed for expressing the relation between the sound intensity and fall-height; where "*c*" is a constant depending on the construction of the instrument, "*h*" the height of fall, "*p*," the weight of the ball and "*i*" the intensity of sound. This formula was again reviewed in Wundt's Laboratory in 1881 by Tischer who showed experimentally that a relation between the height of fall and the intensity of the resulting sound is only roughly accurate and such as to be impossible of expression algebraically. Furthermore, the relation is one which it is necessary to establish independently for every ball and instrument used. Some such device, were it possible to state the relation between the falling height and the resulting sound intensity, would be admirably adapted for auditory testing but, for the present at least, it lies beyond the possibilities of a quantitative statement.

The methods for measuring the energy of sound in physical units have been numerous. Relatively loud tones have sometimes been employed in determining the ear's sensitivity. By this method the subject is removed to such a distance from the source of sound that it is just no longer audible. Then by calculating the energy of the sound emitted, and knowing the distance between the subject and the sonorous source, the intensity of sound at the ear may be easily reckoned. This method was employed by Toepler and Boltzmann,[16] who were first to determine the absolute sensitivity of the human ear. On the strength of Helmholtz's generalizations, they calculated the quantity of sound energy leaving an open organ pipe, while Wolf pursued the same method by calculating the energy going out from a bottle over which a blast of air was made to pass, giving rise to a loud shrill tone. The method assumes that *all* energy consumed by the open organ pipe or sounding bottle passes over into aerial sound wave energy. Knowing the pressure of the wind blast playing upon an organ pipe or bottle; the quantity of air consumed; the distance between the subject and the tone center; it is simply a matter of arithmetic to determine the maximum condensation of an air wave reaching a subject's ear.

So far as I have been able to discover, only two remaining devices

[15] *Wied. Annal.* **13**: 254. 1881.

[16] See Rayleigh, "The Theory of Sound," 1896, Vol. II., p. 433.

have been employed to arrive at a quantitative *Hörmass:*—the tuning fork and the telephone. The tuning fork has gained prominence as an instrument for testing hearing, I believe, largely because of its efficiency in a functional hearing test. For this purpose it impresses me as being rather a superior instrument. But it is well to bear in mind the distinction between a test's efficiency functionally and its psychological value in testing individual differences. In a functional hearing test, according to the standards set by modern otological practitioners, such devices are required as will measure hearing efficiency for different regions of the tonal scale, and particularly the ear's relative acuity for all variations in pitch found among the tones employed in speech. A scientific test for comparative hearing acuity on the contrary concerns itself with only the relative sensitivity of individuals as regards some one point of the hearing scale. A determination of an individual's functional hearing consequently involves many problems which are found not to enter into a test which seeks only to point out individual differences in the sensitivity of the ears of any group. Continental European otologists, however, employ tuning forks almost altogether both in their tests for general hearing acuity and for locating islands of deafness and other functional disturbances.[17]

To Lord Rayleigh,[18] we owe the credit for first deriving a formula for measuring the sound energy that is given out by a tuning fork in vibration. Rayleigh's formula contemplates a microscopical measurement of the amplitude of the fork's vibration.[19]

[17] See in this connection: Bezold, "Functionelle Prüfung," 1897, S. 121; also *Arch. of Otol.* **25**: 384. 1896; **36**: 37. 1900. M. Paul Robin, "Appareil pour mesurer l'acuité auditive," *Bull. et mem. soc. d'anthropol.* **3**: 209. 1902. Ostmann, "Zur quantitative Hörmass," *Arch. f. Anat. u. Physiol.* 1903, S. 321; also by the same author *Ztschr. f. Ohrenhk.* **51**: 237. 1906. Zwaardemaker and Quix, "Schwellenwerth und Tonhöhe," *Ztschr. f. Psychol. u. s. w.* **33**: 415. 1903. L. Jacobson and W. Cowl, "Darstellung und Messung Schwingungsamplituden," *Arch. f. (Anat. u.) Physiol.* 1903, S. 1. Many others might be mentioned, but these are sufficient to indicate the European interest in the tuning fork as a measuring instrument of the ear's hearing power.

[18] "Theory of Sound," Vol. II., p. 437; also, "Amplitude of a Just Audible Air Wave," *Phil. Mag.* **38**: 365. 1894.

[19] Where "E" is the total energy given out by a tuning fork in motion, its value may be obtained from the following formula:

$$E = \tfrac{1}{4}\, plw \times \frac{dn}{dt} = \tfrac{1}{4}\, pwl \times \frac{\pi^2}{T^2}(2n)^2.$$

Where "l" is the length, "w" the width, and "p" the cross-sectional area of one of the prongs of the tuning fork, "n" the displacement at one of the vibrating ends of the prongs, and "T" the time in fractions of a second, of a single vibration.

For measuring the smallest audible tone when the subject is stationed in proximity to the testing instrument, this method does not suffice in that the displacements of the ends of the prongs are too small for microscopical measurement. Recently, however, Wien derived a formula applicable to a tuning fork, whereby the energy radiated, at any given moment, may be stated in terms of the time of the fork's vibration, if only the initial amplitude be known. This formula is based on the law of the dampening or dying-out rate of tuning forks. The dampening of a tuning fork has been found to follow the law, $a \times e^{-ht}$, where "a" is the initial amplitude, "h" the dampening factor and "t" the time the fork has been vibrating. At any given moment, therefore, the relation between the amplitude of the fork's vibration for the experimenter and the subject, respectively, can be stated by the formula:

$$\frac{a^2(\text{Subject})}{a^2(\text{Experimenter})} = e^{-2h\,(t_{(\text{Subject})} - t_{(\text{Experimenter})})}.$$

With such a formula, it is necessary, only once for all time, to determine the dampening factor "h" for each fork employed, in order to have a quantitative hearing measure at all times serviceable for use.

By computing the energy proceeding from a tuning fork, Lord Rayleigh[20] found that for a tone of 512 vibrations (double) a condensation of 4.6×10^{-9} sufficed to excite a just audible tone, in case of a normally hearing subject. According to Zwaardemaker[21] and Quix, the quantity of the energy necessary to excite a just audible tone of the same pitch is 1.30×10^{-5} ergs. Wead got the figure $1{,}100 \times 10^{-8}$ ergs. Wien with tuning forks placed the value at 612×10^{-8} ergs. These quantities, it will be noted, are small in the extreme, and such as to baffle all except the most delicate and painstaking measurements.

Work with the telephone has been very meagre, if we except some experiments of a purely physical sort directed toward determining the sensitivity of instruments for speech transmitting purposes.

Ferrais,[22] in some experiments published as long ago as 1877, found that an electrical current of 7×10^{-9} amperes (528 D. V.) was sufficient to produce a sensation of hearing. Preece[23] found the minimum electric current audible in a telephone to be

[20] *Phil. Mag.* **38**: 365. 1894.

[21] *Ztschr. f. Psych.* **33**: 416. 1903.

[22] *Atti della R. Acad. d. Sci. di Torino,* **13**: 1024. 1877.

[23] *Brit. Assoc. Report,* Manchester, 1887, p. 611.

6×10^{-13} amperes. Tait[24] found for a tone of approximately 500 D. V. a current 2×10^{-12} amperes only was required to excite an auditory sensation of tone. And Lord Rayleigh,[25] as a result of some careful and extended experiments, placed the value of the minimal current for a sensation of hearing in the telephone, at 7×10^{-8} amperes, for a tone of 512 D. V. Lord Rayleigh presented a means for evaluating the actual energy given out by a telephone in use. For a tone of 512 D. V., the quantity of energy given out by the telephone for a tone of threshold value was practically that which he received when the tuning fork was the auditory excitant, namely, 4.6×10^{-9} ergs per sq. cm. area at the opening into the ear. Rayleigh's method for translating the electrical potentials employed in producing a tone of threshold value into sound intensity units is rather complex, involving some difficult mathematical calculations. Essentially, however, it consisted in a measure of a telephone plate's amplitude of excursion, from which was derived a law showing the relation obtaining between these and changes in electrical potential. Although Lord Rayleigh found that a telephone plate registers differences in electrical current as small as it is possible to measure with a galvanometer of more than ordinary sensitivity, and that, therefore, the change in electrical potential in the telephone is at once a measure of the differences in the intensity of the sound leaving such an instrument, he suggested no means for evaluating concretely intensities of sounds in terms of the excursion of the telephone plate alone. This work remained for Max Wien.[26] Wien based his computations on the assumption that a telephone plate, in an instrument of the unipolar variety, where the edges are perfectly clamped, executes movements which are comparable to those of circular clamped membranes. Wien derived a formula whereby the resulting intensities of the tones may be directly computed, when the extent of the oscillations of the center of the telephone plate is known. According to this formula, it is possible to express the condensation of a sound wave leaving a telephone, at any point in space, directly as a function of the amplitude of the plate's excursion at its middle point, or indeed, the same may be said of the energy of a sound wave at any distant point. The formula, I have neither the inclination nor ability to verify. But Wien, like Lord Rayleigh, in the work of actual experimentation, based his computations on the assumption that the degree of electrical poten-

[24] *Edin. Proc.* **9**: 551. 1878.

[25] "The Theory of the Telephone" and "Minimum Current Audible in a Telephone," *Phil. Mag.* **38**: 295; also 285. 1894.

[26] "Die Empfindlichkeit des menschlichen Gehörorgane, *Pflüger's Arch.* **97**: 41. 1903.

tial is directly a measure of the intensity of a sound as it leaves a telephone. This, however, seems to be not wholly a settled principle. It is a question which has only recently again been opened in the psychological laboratory of the University of Michigan,[27] so, perhaps, this may serve to explain some of the unusual results which Wien reports.

For a tone of 500 D. V., Wien obtained a value in amperes of 3.6×10^{-11}, as the current strength essential to produce a just audible sound in a telephone, whereas, Lord Rayleigh had worked out the value to be 6×10^{-9} amperes. Wien found, moreover, for a tone of pitch 3,200 D. V., that a pressure difference in the air wave, at the ear, amounting to only 1.4×10^{-11} cms. is sensed by our organs of hearing.[28]

In some experiments, to determine roughly the relation between the intensity of tones necessary to hearing and the hearing of speech, Wien [29] discovered that even if the condensation value of a sound necessary to excite a just noticeable sensation of hearing had to be increased 10,000 times, the hearing for speech is only slightly interfered with. One of Wien's subjects, who was hard of hearing, but who could hear loudly spoken words, required an increase in the intensity of the sound necessary to just excite an auditory sensation, of 10,000,000 over that for Wien's own ear and those of some of his fellow workers in the laboratory. I am unable to criticise Wien's data in this regard, on account of the many factors involved in his elaborate apparatus, which are not clearly described in his published results. His figures are, nevertheless, far in excess of those which I have been able to secure with partially deaf subjects, as will become apparent from the distributions of subjects on the basis of their keenness of auditory sense, to which I shall have occasion to revert frequently in the pages which follow.

[27] See "The Telephone and Attention Waves," G. L. Jackson, *J. of Phil., Psych., etc.* **3**: 602. 1906.

[28] Professor Webster's "phone," an elaborate mechanism for generating tones of determinate vibration frequency and intensity, also utilizes a circular membrane as the sonorous source. But the instrument, though possessing commendable qualities, is not adapted for auditory acuity measurements under circumstances which make it necessary for the subject to be near the instrument. See Webster, Boltzmann-Festschrift, 1904, p. 866.

[29] *Loc. cit.*, p. 34.

CHAPTER VII

The Instrument for Measuring Auditory Acuity

In the selection of a device for the measure of auditory acuity, there were certain definite limitations which governed the choice to be made. Almost of necessity, the tests would have to be made in buildings and rooms more or less open to the public, and, if it be remembered that there was scarcely a room at the Exposition through which hundreds of people did not pass daily, it will not be difficult to apprehend that any auditory test which depended upon air conduction and a variation of the distance between the subject and the source of sound, as a measure of relative acuity, was at once removed from the realm of choice. In this connection it may be well to recollect also that, to a large extent, we were dependent for subjects upon these very conditions which limited the effectiveness and scope of our work—the crowds of people who passed through our rooms day after day and offered themselves as subjects for experiment. Of course, this precluded the possibility of making measurements in the night or, perchance, on Sundays when there was a maximum of quiet on the World's Fair Grounds. The strikingly advantageous feature attaching itself to a study of some problem at a place where crowds congregate rests in the fact that the study can be made more extensive. Consequently, several very significant phases of the problem of auditory acuity which suggested themselves to me had to be neglected. Chief among these was the problem of the relative acuity of different races for significant and non-significant sounds. This was very unfortunate, too, it seems to me, in that sounds have significance for us largely because they have functional—social and economic—values. We shall never be able to state definitely the relative standing of the various races, with reference to auditory acuity, until experimental evidence can be presented based on measures which have taken into account the significance which *meaning* has for sensitivity. Indeed, the barriers cast about me were such as to restrict the selection of devices for measuring the hearing to those which conduct the sound directly into the auditory meatus. Thus, it became a question of one of some three or four types of apparatus.

The first method suggesting itself is the one employed quite generally by European continental otologists—the tuning fork.

A tuning fork is permanently fixed into some substantial base, such as concrete, marble or lead, in order that there may be little or no alteration in the time, intensity or character of its vibration, the initial impulse remaining the same during the successive weeks or months that the fork may be used for testing. Thus mounted, the fork is placed in a sound-proof box, the cover of which can be lifted for giving the initial impulse to the fork. Close to the fork's tone center is fixed a funnel from the apex of which leads a rubber or lead pipe which soon bifurcates, allowing an equal quantity of sound to pass into each. One may lead to the subject's ear, and the other, perchance, to the ear of the experimenter. Knowing the initial amplitude of the fork and its rate of dampening, it is a simple matter of computation to arrive at a formula by which a subject's auditory acuity can be reckoned directly from the length of time he is able to hear the sound. Or, having once for all determined the dying out rate of any fork in question, it is equally easy to formulate a rule for determining the subject's auditory acuity in terms of the lengths of time that the experimenter and the subject, respectively, are able to hear a tone as it dies out—irrespective of the fork's initial amplitude. (See p. 60 for these formulæ.)

Wherever it is possible to be certain of the faithful and intelligent cooperation of a subject in the performance of a test, there can be no question but that the tuning fork method simply and adequately meets all requirements. But these are conditions that are not often fulfilled even with adults, much more rarely so with children of intelligent parentage, still less so in the case of most children as they are found in the public schools, and almost not at all in case of tests on primitive peoples. The language difficulty in communicating to a subject such directions as are essential to an understanding of the *modus operandi*, the lack of interest in the procedure, indifference as to its outcome, fluctuation of the attention, especially in the presence of very faint stimuli, and frequently willful deceit, all are factors which make uncertain results that depend on a subject's statement of his own subjective experiences. For these reasons, not to speak of the peculiar perceptive difficulties, illusory effects, hallucinations, etc., attending the centering of attention on relatively pure and continuous tones, the tuning fork device was rejected.

Some consideration was also given to a phonographic method of communicating auditory stimuli, first suggested by Bentley.[1] This method, to be sure, possesses the marked advantage of allowing the use of significant stimuli, such as spoken numerals instead of meaningless tones or noises. By the use of certain carefully selected[2] words possessing vowel and consonant combinations, whose tone and intensity values have been determined, it might be possible

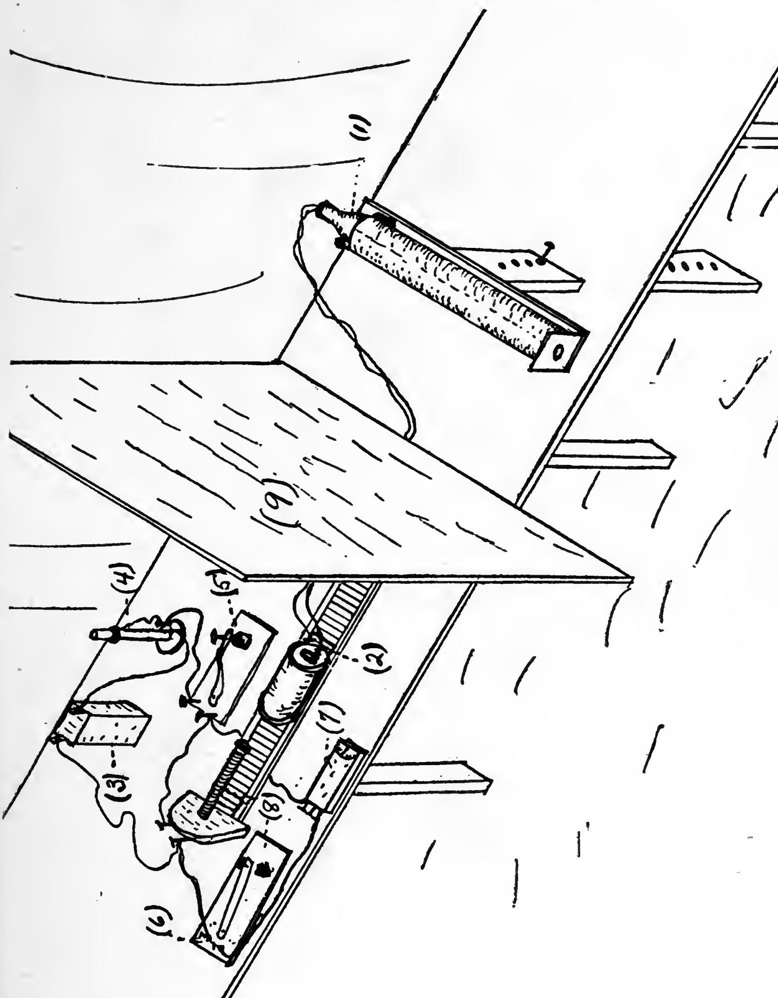
[1] *Science*, N. S. **19**: 959. 1894.

[2] See Andrews, *Amer. J. of Psych.* **15**: 26. 1904; Wolf, "Ohr und Sprache"; Bezold, "Functionelle Prüfung."

not only to measure the acuity but the range of the ear's functioning. Difficulty, however, is experienced if one seeks to state his measurements in quantitative terms. Moreover, it is impossible to secure a hard, phonographic record that will retain its tonal character after any considerable use. Certain individuals, too, experience considerable difficulty in hearing a phonographic reproduction, as many do in understanding conversation over a telephone. Such objections to the use of a phonograph audiometer as have just been outlined, make its use for measuring primitive peoples of questionable value.

The device found to be most serviceable, though not entirely free from objections, was a form of the telephone. The telephone device was favorably considered, chiefly because Wien had shown how the intensity of the stimuli transmitted to the ear from the telephone receiver can be directly and objectively measured. If such a consummation was within the range of probability, it was hoped to employ in the extended research of the auditory acuity of the different races which I was called upon to make, some means whereby a definitely quantitative statement might be made of the results obtained. It is in this latter particular that the classic work of Dr. Charles Myers on the Papuans is defective, together with practically all other data on hearing heretofore published. With the use of the telephone receiver, the character of the stimulus I chose to employ was such as is produced by the opening and closing of an electric circuit, of which the telephone forms a part. In other words, it is a metallic click which, to be sure, is a sound whose components are not in any definite harmonic relation, and, consequently is what is characterized as a noise, in contradistinction to a tone. Much, notwithstanding, may be said in favor of a sound of such a character. In the first place, since its range of stimulation is large, it is more easily heard, and less fatiguing than relatively pure tones, or even clangs whose tonal elements form some sort of a harmonic series. Noises, too, are more tangible. They have more character; they possess various phases and elements to which the mind can attach itself during the successive moments that they are being held in the focus of attention. Then too, they have much in common with spoken language, in that all of the consonants are sounds whose character can be expressed as inharmonic. However, in that they are non-significant and carry no meaning, to that degree they are relatively less adaptable for general auditory tests. This is a condition that I was able to discover no means of avoiding altogether, although, to a certain extent, the end was attained by the manner in which the stimuli were presented to the subject.

The apparatus, as a whole, is somewhat of a departure from others which have been employed in auditory acuity tests, hence, a more or less detailed description of its several parts is advisable. The parts of the apparatus in gross[3] were (1) a telephone receiver



connected by long leads to the poles of (2) an ordinary spark coil (inductorium). The telephone receiver fitted into one end of a long pasteboard tube, at the other end of which was attached a cushion with a central perforation, against which the head and ear were

[3] The numerals refer to the accompanying figure.

to rest snugly and comfortably. This tube measured a meter in length and 8 centimeters in diameter. To the primary poles of the inductorium were attached leads connecting it with (3) an ordinary lead accumulator (storage battery). In this primary circuit were (4) an ordinary resistance coil; (5) a "make and break" device; (6) a shunt circuit in which was placed, (7) a Weston volt-ammeter, and (8) a switch which allowed the current to be sent through the "make and break" key or through the shunt circuit and the volt-ammeter, as might be desired. Between the transmitting device and the telephone receiver was interposed (9) a large double walled pasteboard screen. The entire device ready for operation is illustrated diagrammatically in the figure.

The Induction Coil.—For bringing about changes in the electrical potential of the secondary circuit, through the telephone, a type of spark coil was employed which permitted the sliding of the secondary coil along a graduated scale farther and farther from the primary coil. The range of graduation covered 100 centimeters. (See diagram.) As is well known, by removing the secondary coil from the primary, the strength of the current in the secondary circuit is progressively decreased. With a relatively weak current, *e. g.,* with a voltage of about 2 and amperage of 0.5, in the case of my own ear, the secondary coil must be removed from the primary on an average to a distance in the neighborhood of 75 cm. before the click ceases to me audible. Thus, it is seen, a considerably wide range is afforded within which the extremes of audibility of any group may find a place, and, obviously, a sufficiently wide distribution is given to the data for the purpose of making comparisons.

The Storage Battery.—It is a difficult matter to secure a type of wet or dry electrical cell which will give a perfectly uniform current, even during the few moments that a hearing test might be given. On this account, I induced the Exposition company to purchase for me lead storage batteries (accumulators). One of these cells was placed in the circuit at a time; the other, in the meantime, being free for charging. When charged to its full capacity, each cell possessed an electromotive force of about 2.2 volts. However, the potential rapidly fell to about 2.0 volts, through internal leakage, but there it remained, with the usage I gave the cells, for several days. It is held that accumulators do better work when not used to their full capacity. Some resistance was therefore always placed in series with the primary induction coil and the battery. Almost uniformly, throughout the entire series of tests, the current passing through the primary circuit measured 0.5 amperes and 1.9 volts. The current was measured by means of the Weston volt-ammeter, placed in the shunt circuit, before taking the measure of the auditory acuity of every subject. The object of the long leads between the telephone receiver and the secondary coil, was to remove the subject so far from the noise of "make and break" key as could conveniently be done, within the limits of the sound booth.[4] Thus, almost any sound that might arise from the opening and shutting of the key—the faint spark or the noise attending the mechanical manipulation of the instrument—was beyond the hearing of the subject under examination.

The "Make and Break" Apparatus.—In making and breaking an electric circuit with as strong a current as the one I employed in these measurements, unless particular precautions are observed, a spark occurs which is so loud as to be distinctly audible at a considerable distance from the instrument, and indeed in any part of the sound booth. To avoid this distracting feature, a mercury dip "make and break" key was devised. This, in all essential respects, did not differ from the ordinary telegrapher's key, except that in place of the hard contact, a platinum point was made to dip into a mercury bath on closing the circuit, which prevented the noise of contact. And, to prevent a spark, the surface of the mercury was covered with an extremely thin coating of sweet oil. The whole arrangement was such as to be manipulated, generally, without the least accompaniment of sound. But to make precautions doubly certain, a large screen was interposed between the transmitting device and the subject. This screen served, in addition, to shut off the subject from the view of the experimenter, making it impossible for the latter's movements to be seized upon as cues to the character of the stimuli which were being presented, or for his presence to prove a source of distraction.

Max Wien and Lord Rayleigh had their subjects hold the telephone receiver snugly against the ear. By making the span between the diaphragm of the telephone and the tympanum of the ear air tight, they believed all energy given out by the vibrations of the former would be transmitted directly into the ear to the tympanum and the ossicles. I am not certain that this method does not give rise to a molecular transmission of sound through the bones of the skull. I desired to avoid bone conduction, if possible, and therefore placed, at the far end of the pasteboard tube away from the telephone receiver, a soft leather cushion against which the ear might rest easily and yet be shut practically air tight into the tube.

[4] For a description of this sound booth, I refer the reader to the foot-note on page 30.

My experience has been, too, and that of those whom I have questioned, that when the ear is pressed snugly against any hard surface or object, such as a telephone receiver, small distracting sounds result which very much interfere with the perception of faint stimuli. They give rise, particularly, to confusing illusions of hearing. This is especially true of those inexperienced in introspection. The noises just referred to, no doubt, are due to molecular disturbances arising from the rubbing of the head against the hard substance, the tremors of the hand in holding the instrument and no doubt from other sources as well. To avoid all these furnished additional reasons for the leather cushion and the removal of the subject's ear to a distance from the telephone receiver.

The Telephone Receiver.—Of considerable importance in a test of this character is the form and make of telephone receiver employed. The large type is preferable on account of the additional room for windings on the solenoid; for the greater the number of turns of wire on the solenoid, the more sensitive is the instrument to electrical changes; the quicker is the response; and the more sensitive is it to weak electrical currents. A telephone receiver with a single spool of wire surrounding a central magnet which acted over an area of less than a square centimeter on the center of the telephone plate was what I selected. But in place of the central magnet, I installed a piece of extremely soft iron, which would retain relatively no magnetism when not in use. I did this, first, to avoid the error due to changes in the magnetic character of permanent magnets from day to day, and, secondly, to do away with the factor of self-inductance, which so frequently enters as a distributing element when telephones are employed for delicate work. It were better to have no core at all, but the effect of the central core is to simplify the character of the tones given out and, consequently, it can not safely be dispensed with.

It has frequently been observed that differences in the tension, with which the cap is turned on to an ordinary telephone receiver, decidedly influence both the intensity and the character of the tones that are given out. To obviate differences in this respect, I decided to fix permanently the telephone plate to the instrument I employed. Thus the distance between the soft iron core, or temporary magnet, and the diaphragm of the telephone would remain constant throughout the whole series of tests. In the instrument as purchased, the plate rested upon a metallic base, the cap at the same time serving to hold the plate firmly upon its base, and to retain the active parts within the hard rubber casing. I removed the cap entirely as it would be of no service in the testing of hearing, and fitted over the diaphragm a small steel ring which I fastened firmly and permanently by means of screws to the metallic frame work of the instrument, thus causing the telephone plate to retain the same position and tension at all times.

So much for the instrument proper. But something needs to be said of the arrangements of the parts in testing. On one side of the screen stood the experimenter, with the induction coil, ''make and break'' key, the alternator, the ammeter-voltmeter in the shunt circuit, before him—over all of which was suspended a small incandescent electric bulb which furnished the illumination. On the other side of the screen sat the subject—his head resting easily against the leather cushion, from a perforation in the center of which the sound entered the ear being tested; the other ear being meanwhile stopped with cotton.

CHAPTER VIII

The Graduation of the Instrument

In this section a question will be considered which is very largely a matter of physics in that it is concerned with the graduation of the telephone apparatus employed in all my hearing acuity tests. The question is of vital importance. Indeed, the exact graduation of the instrument employed in a hearing test should be the chief concern of an experimenter, and particularly is this true, when an attempt is made to state conclusions in terms of interchangeable units. Studies of hearing acuity in which the telephone method has been used, and in which there has been an attempt to translate the auditory acuity of subjects measured into terms of physical units, have been few indeed. Ferrais,[1] Preece[2] and Tait[3] were satisfied to record their conclusions in terms of the fraction of an ampere required to produce an auditory sensation. On the other hand Rayleigh,[4] Wien,[5] Kempf-Hartmann[6] and Jackson[7] have translated their data into terms of ergs (centimeter-gram-seconds). Kempf-Hartmann and Jackson employed a reflecting mirror device for measuring the excursion of the telephone plate, a method which permitted of readings down to an excursion of 0.2 mm. only. Wien and Rayleigh's writings, therefore, alone are of interest as being historically connected with that of mine.

Lord Rayleigh reported some experiments on the sensitivity of the telephone as an instrument for producing delicate shades of difference in the intensity of tones, while Wien employed a telephone in his experiments on the relative sensitivity of the ear for tones of different pitch values. Lord Rayleigh, by measuring microscopically the excursion of a telephone plate under varying conditions of electrical current, discovered that a telephone of the unipolar variety is capable of registering differences in sound intensity as small as a galvanometer of the same number of windings of wire in the solenoid is capable of recording. By inference Lord

[1] *Atti della R. Acad. d. Sci. di Torino,* **13**: 1024. 1877.

[2] *Report Brit. Assn.* (Manchester), 1887; 611.

[3] *Edin. Proc.* **9**: 551.

[4] *Phil. Mag.* **38**: 285, 295. 1894.

[5] *Annal. d. Physik,* **4**: 456. 1901; *Pflüger's Arch.* **97**: 1. 1904.

[6] *Annal d. Physik,* **8**: 481. 1902.

[7] *J. of Philos., Psychol., etc.* **3**: 602. 1906.

Rayleigh concluded that the current changes are at once proportional to the force of the resulting sound waves and that to measure the intensity of a sound wave leaving a telephone it is necessary only to have a measure of the strength of the electrical current used in generating it. It was this extreme sensitivity of the telephone, which places it on a par with the galvanometer, that influenced me to employ it in the measurements of the hearing of primitive peoples. Wien, after verifying Lord Rayleigh's results with several telephones, derived a formula whereby the energy emitted by a telephone plate in vibration, is stated in terms of the extent of the excursion of its middle point.

In a hearing test it is always desirable to have the unit of measurement as accurately defined as possible. Acoustically, the unit is defined as the quantity of sound energy passing a square unit surface (usually 1 sq. cm.), perpendicular to the line of a sound wave's progression, in a unit of time (1 second). Likewise, the intensity of a sound at any distance from a sonorous body is expressed in the following algebraical formula:[8]

$$E = \tfrac{1}{2} p.\, a. \times \left(\frac{2\pi}{L}\right)^2 \times A^2,$$

where "*E*" is the intensity of the sound in ergs; "*p*" the density of the medium through which the sound passes; "*a*" the velocity of sound's propagation in air at 0° C.; "*L*" the length of a single sound wave; and "*A*" the amplitude of the sound wave's vibration.[9]

In the formula, the "*A*" is a factor whose value it is always difficult to ascertain. Either the quantity of sound energy leaving the sounding body must be known or the degree of condensation at some point distant from the source must be measured.

Wien[10] used both methods, employing the latter as a check to the former. In the determination of the value of "*A*" at a distance from the source of sound the condensation was measured by observing the effect of a sound wave on a rubber membrane stretched across a resonator pitched to respond to the tone of the sounding source. But the method can not be said to be very accurate. It is simpler to express the intensity directly in terms of the quantity of energy leaving a sonorous center—if this quantity can be de-

[8] Rayleigh, "Theory of Sound," Vol. II., p. 468.

[9] The factor indicating the decline in sound intensity with distance from sound source has been purposely omitted, for the reason that the rate of decline is still an unsettled question. The best authorities seem to favor a decrease directly as the distance. See Webster, Boltzmann-Festschrift, 1904, p. 866.

[10] *Pflüger's Arch.* **97**: 30. 1903.

termined—and to compute from this the amount passing any given area at a point removed from it.

Instead of using the quantity of energy, it is customary in acoustics to express the force of a sound wave in terms of its condensation. For the convenience of the reader in the discussions which follow, I shall employ both figures.

Helmholtz established certain formulæ, whereby the intensity of sound emanating from circular vibrating membranes, with firmly clamped edges, can be calculated. One of these formulæ was adapted to a membrane of the character of that found in a telephone receiver by Wien,[11] if the instrument is of the unipolar type, in which event it is presumed to emit longitudinal sound waves of the sinus variety. The formula expressed algebraically is,

$$\Delta = 0.147 \times \frac{K}{c^2} \times \frac{(2\pi N)^2 \times R^2 \times a}{d}$$

Where "Δ" represents the condensation; "K" a constant, the correction for specific heat; "c" the rate of propagation of sound in air at the temperature obtaining when the measure is made; "N" the vibration frequency of the tone emitted; "R" the radius of the telephone plate, freely vibrating; "d" the distance between the telephone and the ear of the observer; and "a" the maximum extent of the excursion of the middle point of the telephone plate. Of these factors, the value of "K" may be computed once for all from the familiar formulæ for temperature corrections;[12] the values of "c," "d," "N" and "R" also may be either directly measured or computed, leaving only the value of "a" for various sound intensities to offer any serious problem. This value necessarily must be measured independently for every variation in the intensity of tones employed. The derivation of the formula involves some complicated mathematical deductions which I shall not attempt to elaborate. Its value and general utility in a hearing acuity test where conditions make it advisable to employ a telephone for producing the auditory stimuli, are obvious. The hearing data to be hereafter presented have been computed from it. For purposes of pointing out individual differences, however, its validity is inessential, inasmuch as in any case the figure representing the condensation is directly proportional to the amplitude of excursion of the middle point of the telephone plate.

If, instead of the condensation, it is desired to know the actual quantity of energy passing a square centimeter area perpendicular

[11] *Loc. cit.*, p. 46.

[12] Wüllner's "Experimental Physik," Bd. 1, s. 928. 1894; and *Phil. Mag.* **38**: 256. 1894.

to the line of a sound wave's progression at any distance from its source, it may be found directly from the figure giving the condensation. According to Lord Rayleigh,[13] the sound energy passing a unit area is equal to:

$$E = \frac{\rho a_t^3}{2k} \times \Delta^2$$

Where "ρ" is the density of air (.00129 gm.) "a_t" is the velocity of sound in air as above, "Δ" is the condensation and "k" is a constant representing the correction for specific heat.

Conditions were wholly unsuited for graduating the hearing instrument at the Exposition, but I felt that no inaccuracy would result in case the graduations were made at some subsequent time, if only the electrical conditions might be reproduced to correspond exactly with the originals for each hearing measure taken. In making the original records of the hearing acuity of the different individuals, therefore, I registered the data in terms of the position of the secondary coil as regards its distance from the primary of an induction circuit also making record at the same time of the character of the electric current that I was employing. The latter record was made in both amperes and volts, a Weston volt-ammeter being used for the purpose.

The problem of the measurements of the excursion of the telephone plate is one of no small concern. Indeed, it is a task of great difficulty and one that must be approached with patience and more than ordinary precision of method, if the error of measurement is to be kept within workable limits. Like Lord Rayleigh and Max Wien, I made these measurements with the aid of a compound microscope. The method was as follows: To the center of the telephone plate was fixed a finely drawn out glass tube, to serve as an object upon which to focus the microscope. In order to clarify the image in the microscope, the tip of the glass tubing was first dipped into some red ink, which caused the red point to stand out clearly in the field of view. The tube was then attached to the telephone plate by setting it into a drop of shellac, which when dry presents a hard and inelastic adhesive, and which will transmit vibrations in exactly the same form in which they are received. At first an attempt was made to do the measuring with an ordinary compound microscope by mounting the telephone receiver in such a way as to allow the glass tubing to extend horizontally under the ocular of the microscope. But on account of the constant vibratory movements which the glass rod executed, even in the absence of an electric

[13] "Theory of Sound," Vol. II., p. 469.

current, the use of the vertical type of microscope had to be abandoned. I then selected a Bosch and Lomb demonstrating microscope which I mounted horizontally into a mass of plaster of Paris, and into the same mass I mounted the telephone receiver with the glass rod extending downward in such a way that the tip of the tubing was in the microscopic field of view. The microscope was fitted with a No. 1 ocular and $\frac{1}{12}$ objective, which gave a magnification varying only by a small fraction from 500 diameters. The ocular was supplied with a micrometer scale, making it possible to make readings which might be translated directly into micromillimeters.

Even with the precautions just indicated, it was found that satisfactory measurements in a down-town building were impossible, owing to the jars and vibrations arising from traffic on the streets below, which affected the movements of the glass filament mounted upon the telephone diaphragm. I therefore removed the entire apparatus to a place in the country, wholly free from all external disturbances.

It had not occurred to me, when measuring the hearing of the peoples at the Exposition, that for the threshold of hearing of an individual with ordinary acuity, an excursion of the diaphragm of the telephone would suffice, which measured no more than 0.00000016 centimeters; a distance, indeed, so small as to exceed optical possibilities of measurement, and necessarily, beyond the measuring capacity of the most powerful microscopes. This, then, was a serious situation. Except for the lower acuity values, the measurement of the excursion of the telephone membrane would be either wholly impossible or accurate within the region of a large probable error. Moreover, when the limits of the powers of the compound microscope are exceeded, other, reflecting, devices are even more useless. A way to circumvent this difficulty finally presented itself to me—what may be designated as a method of extrapolation. This method consisted in making microscopical measurements for those positions of the secondary coil where it was possible to do so accurately with the electrical currents as they obtained at the Exposition, while the hearing records were being made, and, for other positions of the secondary coil, to increase the electric current to the point where it was possible to make satisfactory microscopic measurements. This method was pursued for every variation in electric current that was employed in the hearing tests.

In the first column of the table on page 81, are tabulated these measures of the excursion of the telephone plate for the different positions of the secondary coil, when a current strength of 1.9 volt and 0.5 ampere was employed. Under such conditions of current it will be seen it was possible to make microscopic measurements for positions of the secondary coil from 1 to 12 only, the figure "1" here indicating that the secondary coil is removed from the position 0, or that in which the primary coil is wholly within the secondary, by one centimeter. The position 10 indicates that the secondary coil is removed by 10 centimeters from the zero position, and so on. Beyond the reading 12, the diaphragm excursions were so small that they could not be accurately evaluated from the readings on the micrometer scale. In point of fact for the position 12 the excursion amounted to 0.000045 centimeter only, which extended over only about one fifth of a single space on the micrometer scale of the eye-piece of the microscope, and is about as small as can be measured with much certainty.

Each figure given in the table is the average of at least five independent readings. Frequently, too wide discrepancies appeared in the separate readings, in which event the measurements were repeated until a certain degree of harmony was found among them. It is unnecessary to give in detail the several individual measurements from which the averages given in the table were made, and inasmuch as some twenty different strengths of electrical current were used in the original hearing records, the tabulations of all would alone cover too many pages. The inconstancy in electrical current arose from the fact that it was impossible to keep the storage batteries, which supplied the current, up to full strength from day to day. No inaccuracy, however, need result from this except that which might be occasioned by certain errors of observation in different microscopical measurements, which need not, in any case, exceed 10 per cent., as will appear from the data which will shortly be presented. Only a very few of the hearing records taken were found to be so poor that a current intensity of such strength was required as that represented by the 8 or 10 position of the secondary coil in the induction circuit. Since the excursion of the center of the telephone plate for positions of the secondary coil further removed than 12 could not be measured directly under normal current conditions as they obtained at the Exposition ($V = 1.9$; $A = 0.5$), the value of the excursion for these subsequent positions had to be derived indirectly, as suggested above.

First, I increased the intensity of the current in the primary circuit to a voltage of 10.0 and an amperage of 0.5; and repeated the measurements as before. This stronger electrical current was drawn from the main of an electric light conduit, resistance in the shape of incandescent lamps being placed in series with this circuit to reduce it to the required strength. With a current of this intensity, it was possible to continue the measurements until the secondary coil reached the position 19. It was found that the excursions of the telephone plate for a current strength of 10 volts were approximately seven times (6.72) those when the voltage was 1.9. The readings for this current intensity are to be found in the second column of the table on page 81. If now the microscopical readings of the excursion of the middle part of the telephone plate for this strength of the electrical current (10 volts) is divided by this figure (6.72) the readings should correspond with those obtained from the weaker current. It is significant that in no case does a result so obtained differ by more than 10 per cent. from its corresponding one to be found in the first column. This indicates that the method gives results which are accurate, at least, to within 10 per cent. With this stronger current, for the first position of the secondary coil, the oscillations of the glass tip were so violent as to extend too far beyond the micrometer scale of the ocular to be read. Beyond the position, 19, the extent of excursion on the contrary was again too small for measurement. For positions beyond this point the strength of the electrical current had again to be increased; the current in the primary circuit being raised to a voltage of 107 and an amperage of 1.1. This was about as powerful a current as I felt justified in sending through a wire of the dimensions of that with which my primary coil was wound, owing to the danger of burning off the insulations. The microscopic readings with this current intensity are to be found in column three of the table on page 81. Excursions of the telephone plate corresponding to positions of the secondary coil nearer than 12, it was again impossible to measure, inasmuch as the extent of its oscillations exceeded the limits of the micrometer scale. At the other end, microscopic measurements were possible to the position of 64 of the secondary coil, that is, until the secondary was removed from the primary coil to a distance of 64 centimeters. There remained about twenty-one of the hearing records of whites and one of Indians in which the secondary coil was beyond this point. The excursions for the remaining positions of the secondary coil, therefore, had to be gotten by some other method. Two suggested themselves.

The first of these subsidiary methods was based on the experimental conclusions reached by Lord Rayleigh[14] and Wien[15] to the effect that a telephone is as sensitive to changes in electrical potential as an ordinary galvanometer is capable of measuring, especially as regards small currents. In keeping with this method, I selected

[14] *Phil. Mag.* **38**: 287. 1894.
[15] *Pflüger's Arch.* **97**: 11. 1903.

ten intelligent subjects—boys 12 to 14 years old—for whom I determined the average current intensity necessary to produce a tone of threshold value for all positions of the secondary coil from 10 to 75 centimeters removed from the primary. If, according to Lord Rayleigh,[16] the excursion of the middle point of the telephone plate is directly proportional to the electromotive force, the excursion of the telephone plate for positions of the secondary coil where microscopical measurements are impossible may be computed.

In detail, the mode of procedure was as follows: The secondary coil was placed 10 centimeters removed from the zero position and the current passing through the primary circuit uniformly without resistance ($V = 5.0$; $A = 0.5$) was reduced by placing resistance in the circuit until the threshold of hearing was reached for each of the subjects. A record was then made of the number of ohms of resistance required.[17] Then, in order, the same procedure was followed for other positions of the secondary coil up to 75. It seems scarcely necessary to present all these data in detail. In point of fact, the method was not extremely accurate, due no doubt in a large measure to variations in the attentive capacity of my youthful subjects from moment to moment, hour to hour, and from day to day. In general, however, the data secured were such as to justify a favorable conviction relative to the accuracy of the main method employed, for while the variations between the two methods were considerable, the figures gotten from the latter method followed along the same line as those from the first.

The second subsidiary method was based on the assumption that the law of the decrease of the intensity in the secondary circuit of an induction coil holds uniformly, as the two circuits are separated more and more widely. That is, by determining the ratios of the excursion of the telephone plate for the several positions of the secondary coil nearer to the primary than 64 centimeters, and thus determining the rate of the fall, it was assumed that with a method of extrapolation the excursion of the telephone plate for positions of the secondary coil farther removed from the primary than 64 centimeters could be calculated with a fair degree of accuracy.[18]

If we take the telephone excursions as they appear in the third column of the table on page 81, where the strongest electrical potential prevailed, and divide the excursion of the telephone plate for

[16] *Loc. cit.*, p. 287.

[17] The resistance coils used were those of a standardized box, kindly loaned to me from one of the physical laboratories.

[18] For positions of the secondary coil 16 and beyond, the primary was wholly outside the secondary coil, and consequently it would be unreasonable to believe that there would be any change in the law of fall for these positions.

the position of the secondary coil, 44, by that for the position 42, and that for the position 46 by the excursion for the position 44, etc., until the last measurable position 64 is reached, the general fractional rate of decline in excursion is found. I present the separate figures:

.00012 /.00013	0.93
.00011 /.00012	0.92
.000096/.00011	0.87
.000086/.000096	0.91
.000079/.000086	0.92
.000069/.000079	0.87
.000064/.000069	0.93
.000058/.000064	0.91
.000052/.000058	0.90
.000049/.000052	0.94
.000045/.000049	0.92
Average rate of decline	0.91

As stated above, if it is assumed that the rate of decline for the last five positions of the secondary coil is the same as for the eleven just preceding as indicated in the data given, it becomes necessary only to multiply the excursion 0.000045 centimeter of the 64 position by 0.91 to have the excursion for the position 66, and hence the excursion of the telephone plate for this position would be 0.000041 centimeter under the conditions of current as they obtained in the readings of the third column. Multiplying 0.000041 centimeter by 0.91 we get 0.000037 centimeter as the excursion of the telephone plate for the position 68. Continuing the same process, the excursion for position 70 is 0.000034 centimeter; for position 72, 0.000032 centimeter, and for position 75, allowing for the increase in interval, 0.000028 centimeter.

It may be noticed that for the positions of the secondary coil removed, respectively, 10, 11 and 12 centimeters from the primary, microscopic readings were secured for three different current intensities. The figures given in the table for each of these positions of the secondary coil, moreover, are the averages of at least 10 independent microscopical readings. Especial care was observed at these positions in order to afford an accurate basis for determining the ratio of excursion of the telephone plate for the three intensities of electrical current used. In Series II., where the voltage was 10, amperage 0.5, the excursion of the telephone plate on the average was 6.72 times as large as in Series I., where the voltage was 1.9, amperage 0.5, and in Series III., where the voltage was 107, amperage 1.1, an excursion on the average resulted which was about 27.8 times as large as in Series II., and just about 187 times as large an excursion as in Series I. When allowance is made at these positions (10, 11 and 12) for differences in current strength, that is, in case the telephone plate's excursion for positions 10, 11 and 12 of

TABLE XIII

EXCURSIONS OF THE TELEPHONE PLATE FOR DIFFERENT CURRENT STRENGTHS

Position of the Secondary Coil	Current Strength Amp.=0.5 Volt.=1.9	Current Strength Amp.=0.5 Volt.=10.0	Current Strength Amp.=1.1 Volt.=107.0	Position of the Secondary Coil	Current Strength Amp.=1.1 Volt.=107.0	Position of the Secondary Coil	Current Strength Amp.=1.1 Volt.=107.0
	cms.	cms.	cms.		cms.		cms.
1				21	0.0011	54	0.000069
2	0.06			22	0.00092	56	0.000064
3	0.011	0.079		23	0.00077	58	0.000058
4	0.0036	0.023		24	0.00064	60	0.000052
5	0.0015	0.009		25	0.00052	62	0.000049
6	0.00072	0.005		26	0.00049	64	0.000045
7	0.00032	0.0024		27	0.00037	66	0.000041
8	0.00023	0.0014		28	0.00034	68	0.000037
9	0.00015	0.0013		30	0.00030	70	0.000034
10	0.000093	0.00061	0.016	32	0.00024	72	0.000032
11	0.000064	0.00045	0.013	34	0.00021	75	0.000028
12	0.000045	0.00028	0.0081	36	0.00019		
13		0.00023	0.0067	38	0.00017		
14		0.00019	0.005	40	0.00015		
15		0.00015	0.0043	42	0.00013		
16		0.00011	0.0034	44	0.00012		
17		0.000094	0.0029	46	0.00011		
18		0.000073	0.0021	48	0.000096		
19		0.000063	0.0017	50	0.000086		
20			0.0014	52	0.000079		

the secondary coil, in Series II. are divided by 6.72 and in Series III. by 187, the results appear as in the following summary:

TELEPHONE PLATE'S EXCURSION

Position of the Secondary Coil	Series I	Series II/6.72	Greater or less than in Series I	Series III/187	Greater or less than in Series I
10	0.000093 cm.	0.000094 cm.	1.0 per cent.	0.000085 cm.	8.7 per cent.
11	0.000064 "	0.000068 "	6.2 "	0.000069 "	7.8 "
12	0.000045 "	0.000042 "	6.7 "	0.000043 "	4.5 "
Average difference between series			4.6 "		7.0 "

Series II. and III. have 10 readings in common, for positions of the secondary coil from 10 to 19. Comparing Series II. with Series III., that is, by dividing the excursions of the telephone plate in Series III. for positions 10, 11 and 12, to 19 of the secondary coil by 27.8, we get the results in the summary below.

From the data just exhibited, it is possible to determine what degree of accuracy may be looked for with reference to the method of extrapolation here employed. It is seen that the average difference in result between the microscopical measurements of the telephone plate with the electrical current as it obtained at the Exposition (voltage 1.9; amperage 0.5) and the next stronger current

TELEPHONE PLATE'S EXCURSION

Position of the Secondary Coil	Series II	Series III/27.8	Greater or less than Series II
10	0.00061 cm.	0.00058 cm.	5.0 per cent.
11	0.00045 "	0.00046 "	2.2 "
12	0.00028 "	0.00029 "	3.6 "
13	0.00023 "	0.00024 "	4.4 "
14	0.00019 "	0.00018 "	5.3 "
15	0.00015 "	0.00015 "	0.0 "
16	0.00011 "	0.00012 "	9.1 "
17	0.00094 "	0.00010 "	6.4 "
18	0.000073 "	0.000076 "	4.1 "
19	0.000063 "	0.000060 "	4.8 "
		Average difference between two series	4.6 "

(voltage 10; amperage 0.5) was only 4.6 per cent. Between the weaker electrical current and the most powerful one employed in the work of graduation (voltage 107; amperage 1.1) the average difference in the result of microscopical measurements amounted to 7.0 per cent. and for the 10 different readings for identical positions of the secondary coil, when the second and highest electrical currents were employed, the average difference in result was only 4.6 per cent. It is likewise worthy of note that the difference between the readings for the same position of the secondary coil, but with different electrical potentials, in no case exceeded 9 per cent. It is, therefore, safe to assume that the microscopical readings for the excursion of the telephone plate as recorded in the third column of the table on page 81, are reliable within 10 per cent. at the outside. Indeed, it is also safe to assume that the corrected figures representing the actual excursions of the telephone plate for the current strengths used in the measurements of auditory acuity do not differ from the true values by more than 10 per cent.

Knowing the excursion of the telephone plate caused by an electrical current of 107 volts, 1.1 ampere for every position of the secondary coil as appears in column III. of the table on page 81 and knowing also from readings of three identical positions of the secondary coil the excess in excursion produced by the current intensity just noted, over that normally obtaining when the hearing records were made (voltage 1.9; amperage 0.5) it is simply a matter of arithmetic to determine the actual extent of excursion of the telephone plate corresponding to each of the original hearing records made. By dividing the values found in column III. of the table (page 81) by 187, the corrected values corresponding to a current strength of 1.9 volts; 0.5 ampere, are secured for every position of the secondary coil from 1 to 75. These are given in Table XIV. below. Inasmuch as an intensity of sound represented by a position of the secondary coil nearer to the primary than 14 centimeters

TABLE XIV

Table Showing the Excursion of the Telephone Plate (a), the Maximum Condensation (Δ) and the Energy (E) in Ergs, Corresponding to Every Position of the Secondary Coil

Position of Secondary Coil	Excursion of Center of Telephone Plate "a"	Condensation of Air Wave Leaving Instrument "Δ"	Energy of Sound Wave Per Sq. cm. at Instrument "E"	Position of Secondary Coil	Excursion of Center of Telephone Plate "a"	Condensation of Air Wave Leaving Instrument "Δ"	Energy of Sound Wave Per Sq. cm. at Instrument "E"
	cms.		ergs		cms.		ergs
75	1.6×10^{-7}	1.4×10^{-9}	5.0×10^{-8}	34	1.1×10^{-6}	1.04×10^{-8}	2.8×10^{-6}
72	1.7 "	1.5 "	5.6 "	32	1.3 "	1.2 "	3.5 "
70	1.8 "	1.6 "	6.5 "	30	1.6 "	1.4 "	4.9 "
68	2.0 "	1.8 "	8.0 "	28	1.8 "	1.6 "	6.4 "
66	2.2 "	1.9 "	9.0 "	27	2.0 "	1.8 "	8.0 "
64	2.4 "	2.1 "	1.1×10^{-7}	26	2.4 "	2.1 "	1.1×10^{-5}
62	2.6 "	2.3 "	1.3 "	25	2.8 "	2.5 "	1.6 "
60	2.8 "	2.5 "	1.6 "	24	3.4 "	3.0 "	2.3 "
58	3.1 "	2.8 "	2.0 "	23	4.1 "	3.6 "	3.3 "
56	3.4 "	3.0 "	2.3 "	22	4.9 "	4.3 "	4.5 "
54	3.7 "	3.3 "	2.8 "	21	5.9 "	5.3 "	7.0 "
52	4.2 "	3.6 "	3.3 "	20	7.3 "	6.5 "	1.05×10^{-4}
50	4.6 "	4.0 "	4.0 "	19	9.3 "	8.2 "	1.7 "
48	5.0 "	4.5 "	5.0 "	18	1.1×10^{-5}	1.00×10^{-7}	2.5 "
46	5.6 "	5.0 "	6.3 "	17	1.4 "	1.3 "	4.3 "
44	6.3 "	5.5 "	7.5 "	16	1.8 "	1.6 "	6.5 "
42	7.0 "	6.2 "	9.5 "	15	2.3 "	2.0 "	1.0×10^{-3}
40	8.0 "	7.1 "	1.3×10^{-6}	14	2.8 "	2.5 "	1.6 "
38	9.1 "	8.1 "	1.7 "				
36	10.0 "	9.1 "	2.1 "				

was not required for any subject tested, values for positions of the secondary coil nearer to the primary than this were omitted from the tabulations.

It will be observed that while the secondary coil was nearer to the primary than 27 centimeters readings were made at intervals of one centimeter, but for positions of the secondary coil more distant, graduations were made only at intervals of 2 centimeters. This plan was followed since obviously the fall of electrical potential along the induction circuit is not directly as the distance. Indeed, it falls very much more rapidly than this, and while the change from graduations at intervals of one centimeter to graduations at intervals of two centimeters does not equate the steps, it does serve to keep the records from being too widely dispersed at the upper end of the curve. The matter of inequality in steps, however, injects no inaccuracy into the final results, so long as basically the values of each position are accurately determined. And, since each position in these tests receives its value in *ergs* of sound energy emitted or *centimeter-seconds* of *condensation* of the sound wave, the question of the number of steps to be made in the tests acts as a matter of convenience in manipulation only. It would make but an insignificant difference in the average of any group whether graduations had been made at intervals of 1 or 2 or 5 centimeters.

The excursions of the telephone plate (a), corrected as outlined above for an electrical current of 1.9 volts, and 0.5 ampere, appear in the second column of Table XIV., on page 83. In the third column are given the values of the condensations (Δ) computed from Wien's formula[19] already given, namely,

$$\Delta = 0.147 \times \frac{k}{c^2} \times \frac{(2\pi N)^2 \cdot R^2 \cdot a}{d}.$$

[19] These formulæ call for certain measurements relative to the values of "k," "c," "N," "R," "a," "ρ," and "d," respectively. "k," which represents the temperature coefficient, is a constant found from the well-known formula of Keyser (*Phil. Mag.* **38**: 256. 1894). "c" stands for the velocity of sound in air at the temperature prevailing when the graduations were made. In the experiments under consideration its value was taken as 340.2 M. The radius of the free vibrating area of the telephone plate measured 2.24 cm. The several values of "a," the excursion of the telephone plate at its middle point, appear in the second column of the table. The distance between the ear and the center of the telephone plate "d" throughout the entire series of hearing tests was uniformly 100 cm. However, inasmuch as the ear was sealed into one end of a tube and the telephone into the other, both to a large extent air tight, a value of one centimeter was assigned to this distance. Indeed, it is improbable that the loss of energy in a sealed tube would be any considerable quantity for such a short distance of propagation. The value of "ρ," the temperature coefficient, is 0.00128. "N" equaled 500 D. V.

In the fourth column are given the values in ergs (centimeter-gram seconds) of the quantity of sonorous energy passing a square centimeter area perpendicular to the line of the sound wave's progression. The latter are computed from Lord Rayleigh's formula already given, namely,

$$E = \frac{c^3 \rho}{2} \times \Delta^2 \text{ ergs.}$$

CHAPTER IX

Method of Conducting Test

All the measurements for simple auditory acuity were made in the sound room,[1] and consequently under conditions, the most favorable for quiet that it was possible to secure at the Exposition. In order to obtain a correct estimate of an individual's hearing, it is very essential that the test be conducted under circumstances as favorable as possible. It is difficult to keep the attention focused on faint stimuli, no matter through which sense avenue they are received, but faint auditory stimuli are especially elusive. Fatigue of the auditory sense organ results after a moment of stimulation, so that even under the most favorable adjustment of external conditions, it is not possible to continue an auditory test longer than four or five minutes, even with a subject of far more than average intelligence. Hallucinations enter as very disturbing factors. It is a common observation with those who have made tests of the hearing of both children and adults to find that the individual vigorously asserts that he hears the sound used as a stimulus even though the source has been entirely removed. This is especially true of continuous tones, or such as follow in a rhythmical order, as the ticking of a watch, or the regular falling of water drops.

Faint auditory sensations are likewise subject to decided illusory effects. Any extraneous noises are apt to be interpreted as the tone to which the subject is expectantly attending. Such noises as are produced by the scraping of the feet in walking over an earthen, brick or cement floor are especially likely to be heard as the stimulus to which the subject is directing his attention, if a metallic click is the stimulus employed. Noises from the street act in the same way. Those things which tend to be distracting elements with adult Whites are bound to be emphatically so in tests on primitive peoples and younger children, whose power of attention is weaker, and who are, consequently, more easily distracted and are unable to single out one from among a number of somewhat similar stimuli which they will hold at the focus of attention.

To overcome so far as possible the error which might creep in, on account of the elements just enumerated, a check method sug-

[1] For a description of this booth the reader is referred to the foot-note on page 30.

gested by Professor Cattell was introduced. This consisted in having the subject interpret and give back what he received. The test in consequence is a little more than a measure of pure sensation—if a measure of pure sensation is ever possible. Instead of presenting the "makes and breaks" in rapid succession without reference to number or manner, as is ordinarily done with the telegraph key, or the *"make and break"* mechanism of the Seashore audiometer, I gave the stimuli in groups or rhythms; that is, a series might be presented in which two clicks in rapid succession would be followed by a pause of two seconds, the two clicks repeated and so on. Or, the series might be given in singles or in groups of three clicks followed by a rest of a couple of seconds. Graphically, the series might be represented as follows: - - , , , - - , , , - - , , , - - , , , etc., in which the dashes represent the order of the stimuli and the commas, pauses or silence. A group of three clicks would be represented by the following scheme: - - - , , , , - - - , , , , - - - , , , , - - - , , , , etc., and a rhythm of single clicks by the following: - , , , , - , , , , - , , , , - , , , , - , , , , etc. I pursued no set order of presenting the series, so that it might not be possible for the subject to anticipate the answer which he would be expected to give.

The number of clicks to a group, I found, had to be limited to three. I discovered that many of the primitive peoples were unable to count the stimuli following each other as rapidly as did these with any degree of facility in groups larger than two or three. Children, too, I afterward learned, can catch a rhythm of three but experience difficulty with one of four or five.

Such a method of presenting the stimuli, as has just been indicated, in a manner supplies the deficiencies arising from the necessity of employing non-significant stimuli in testing hearing. Indeed, to a considerable degree, the test is one of which the elements are significant; that character being attached to them by the counting operation. The test, moreover, has other advantages. It does away with the necessity of depending upon the cooperation of the subject—a necessity present in almost all other methods for measuring hearing. It presents something tangible, as it were, for the attention to attach itself to. The subject, when the ordinary methods are followed, finds that as the sensation becomes increasingly more faint, it is impossible for him to organize his mental processes to the extent of arriving at a certain conviction as to whether he actually hears the tone or not. Where the method of *reproduction* is followed, this element of uncertainty does not, in any way, enter into the results.

In all cases, I chose as the threshold that point where the subject failed to indicate accurately the character of the stimuli presented. In the neighborhood of the threshold values, I made it a point to vary the character of the stimulation frequently without changing its intensity. This was done to make certain that the subject's responses were not pure guesses. It was found, however, that in case of almost all persons, there is a subconscious evaluation given to such estimates which are wholly valid. When the subject stated that he was uncertain as to the character of the sensation, whether the ticks came in twos, threes or ones, he was told to express a judgment and it so happened that frequently the subject judged correctly every variation made with a given intensity although he asserted that his estimations were in every case pure guesses.

CHAPTER X

RESULTS

THE nature of the data as regards auditory acuity is such as to make them somewhat difficult to present intelligibly. These data relating specifically to auditory acuity are given both in terms of the condensation of the sound wave leaving the telephone and the actual energy in ergs (centimeter-gram-seconds) exerted by the sound wave over a square centimeter of surface at the same point. Since the energy required to produce a tone of threshold value, in an ear of ordinary sensitivity, amounts to less than a ten-millionth part of an erg, it is at once clear that the data we are working with are comprehensible, popularly, only in mathematical terms. The data to be presented, however, are to be employed for comparative purposes only, so that the absolute values are, relatively, of insignificant moment. To know that one group of people possesses a hearing sensitivity two, or ten, or one hundred times as great as another, does not call for an understanding of the absolute unit used, if only the unit remains constant from one series of measurements to another, and besides, possesses the additive character. The figures representing the condensation as well as the actual quantity of sound energy being dispersed fulfill both requirements, and hence no inaccuracy will result if the reader ignores the fractional factor altogether, and thinks of the results in terms of the first part of the figure only, *e. g.*, in terms of "1.4" instead of "1.4×10^{-9}." And in the eyes of many readers such a method of interpreting the complex data which follow, will contribute both to their clearness and ease of comprehension.[1]

In Table XV. are presented these figures in detail, indicating the race; number of cases included in the average; the average result, stated in terms of atmospheres of pressure, and ergs (centimeter-gram-seconds); the average deviation from this average; and the standard deviation. From the latter, it is possible to compute directly any other measures of the variability that may be desired. The data are given for both the right and the left ear. A word, perhaps, is necessary concerning the make-up of the respective groups. For a test of this kind, as was noted in connection with the

[1] For details relating to the physical graduation of the instrument employed in the acuity test, the reader is referred to Chapter VIII.

data on the upper threshold of hearing, already presented, it was thought best to reject those individual hearing records where there was definite reason to believe that the case was one of pathological hearing. In pursuance of this, therefore, I rejected in this series of measurements the data from every person who consciously had experienced difficulty in hearing. Obviously, so long as the rejections follow some consistent plan, no inaccuracy need result and I do not

TABLE XV

Auditory Acuity

In Terms of the Condensation of the Sound Wave Leaving the Instrument and also in Terms of the Energy in Ergs Leaving the Same

	Right Ear					Left Ear			
	No. of Cases	Average $\Delta = 10^{-9}$	A. D.	S. D.	Average $E = 10^{-7}$ ergs	Average $\Delta = 10^{-9}$	A. D.	S. D.	Average $E = 10^{-7}$ ergs
Whites	151	5.6	3.85	5.02	7.9	7.2	5.06	5.09	13.2
Indians (School)	64	7.5	3.63	5.18	14.2	8.5	4.09	5.79	18.2
Filipinos	137	24.2	14.57	18.30	147.8	26.6	14.46	17.81	178.6
Cocopa Indians	10	7.3	2.86	3.49	16.01	9.0	5.30	6.34	30.2
Vancouver Indians	7	10.01	5.01	5.69	38.1	10.03	5.95	6.66	36.5
Patagonian Indians	3	12.3	8.43	8.97	38.1	17.0	8.59	9.57	73.0
Ainu	8	18.1	11.64	16.38	82.7	17.1	8.71	11.36	73.4
Pigmies	5	10.3	3.55	4.44	26.8	7.5	1.24	1.32	14.2

Δ = condensation of sound wave at the instrument.

E = energy in ergs of sound wave leaving the instrument.

A. D. = average deviation.

S. D. = standard or the mean square deviation.

TABLE XVI

Auditory Acuity

In obtaining the averages given in this table, the lowest 25 per cent. and the highest 25 per cent. of each group were omitted, leaving only the middle half.

	Right Ear				Left Ear		
	No. of Cases	Average $\Delta=10^{-9}$	A.D.	Average $E=10^{-7}$ ergs	Average $\Delta=10^{-9}$	A. D.	Average $E=10^{-7}$ ergs
Whites	78	3.9	0.922	3.84	4.9	1.33	6.06
Indians	33	6.1	1.40	9.61	5.0	1.03	6.31
Filipinos	81	16.5	5.29	68.73	18.8	6.02	89.21

Δ = the condensation of the sound wave at the instrument.

E = the energy of the sound wave in ergs per square centimeter area leaving the instrument.

think that the rejection of the records of some twenty individuals, for the reason indicated, influenced the character of the results otherwise than to raise the average for each group to an appreciable extent. For such large groups as those of the Whites, Indians, or the Filipinos, a sifting of the records, perhaps, would have been unnecessary, but in case of some of the smaller groups, with fewer than ten individuals, any one specially poor record would

TABLE XVII

RIGHT EAR

Showing the Distribution of the Individuals with Respect to Auditory Acuity

Intensity of Sound, $\Delta = \times 10^{-9}$	Whites	Indians (from School)	Filipinos	Cocopa Indians	Van-couver Indians	Pata-gonian Indians	Ainu	Pigmy
1.4	1							
1.5	3							
1.6	4							
1.8	7							
1.9	6	1						
2.1	5	1						
2.3	9	2						
2.5	6	1	3					
2.8	9	3	1					
3.0	13	2	2					
3.3	9	2	3	1	1			
3.6	9	2	3					
4.0	7	4	2	2			1	
4.5	6	4	2				1	
5.0	6	5	2			1		
5.5	7	4	2				1	
6.2	6	3	4	4	1			2
7.1	5	4	5			1		
8.1	2	7	6		1			
9.1	4	5	8					1
10.4	6	4	7		1			
11.9	5	4	7	1			1	1
14.1	3	2	9		1			
15.7	0	1	10					
18.1	2	0	9		1			1
21.1	1	1	8				1	
25.0	2	1	8			1	1	
29.9		1	7		1			
36.5			6				2	
43.4			5					
52.5			5					
64.8			6					
82.3			3					
99.9			2					

affect the standing of the group to such an extent as to make the figure for the average wholly unrepresentative and false. Yet, in spite of corrections and eliminations, it is quite evident that all pathological cases were not excluded, as appears from a comparison of the data presented in tables XV. and XVI. In Table XVI., I

TABLE XVIII

Left Ear

Showing the Distribution of the Individuals with Respect to Auditory Acuity

Intensity of Sound, $\Delta = \times 10^{-9}$	Whites	Indians (from School)	Filipinos	Cocopa Indians	Van-couver Indians	Pata-gonian Indians	Ainu	Pigmy
1.4	1							
1.5	2							
1.6	3							
1.8	3							
1.9	4	1						
2.1	4	1						
2.3	5	1						
2.5	7	3		1				
2.8	7	2						
3.0	8	3						
3.3	11	4	4		1			
3.6	12	4						
4.0	8	5	2		1			
4.5	8	6	2					
5.0	7	5	4	2	1			
5.5	6	4	3					
6.2	8	2	4	2			1	2
7.1	5	3	4			1		1
8.1	5	3	5	1			1	
9.1	5	3	7					2
10.4	4	2	6					
11.9	5	2	6				3	
14.1	4	2	8		1	1		
15.7	3	1	12		1			
18.1	4	1	11	1	1		1	
21.1	4	1	9	1				
25.0	2	2	9				1	
29.9	3	1	7		1	1		
36.5			7					
43.4		1	6				1	
52.5		1	8					
64.8			4	1				
82.3			3					
99.9			3					
127.2			2			1		
162.5			1					
214.9				1				

rejected the 25 per cent. of the records at the top and the same number at the bottom, leaving one half of all the records distributed normally about the median. Were the distribution of cases normal, such a method of procedure would affect the average result only to a very slight degree. It will be noted on the contrary, however, that the average has been shifted upward decidedly in the case of every group. In fact, this sifting has the effect of almost doubling each average, thus reducing the quantity of energy necessary to excite an auditory sensation, on the average, by one fourth.

In tables XVII. and XVIII. are shown the general distributions of the individual records, as they appear in the several groups.

These distributions have been made in parallel columns to facilitate the study of comparisons. The tables, likewise, show the character of the curves that the hearing records of the several groups offer, and in addition, present them in such form that group differences are directly apparent and recognizable. Such a form of distribution makes unnecessary the use of graphic curves.

In Table IV., p. 34, are given the distributions of the individuals of the three most numerous groups according to age. All the data relating to the smaller groups will be presented in detail in connection with the discussion of their several individual results. Again, making a cursory review of the data in Table IV. the fact is revealed that in case of the Whites, Indians, and Filipinos, the age lines were rather closely drawn. I accepted the record of no individual whose age exceeded 30 years or fell below 16. This was done as in the case of the data relating to the upper threshold of hearing, in order to secure, so far as practicable, homogeneous groups. The average age of the Whites selected was found to be 23 years and 5 months; of Indians, 19 years and 2 months; and of the Filipinos, 21 years and 1 month. For an acuity test I do not think this difference in average age is such as to be very significant.

We shall now proceed to a consideration of the data in detail:

Whites.—Whites, such as those selected for these tests, individuals in the prime of life, men and women who have never experienced any difficulty in hearing, according to these experimental data are able to sense and interpret on the average, a stimulus produced by the action of an air wave, amounting to a pressure difference of 5.5×10^{-9} atmospheres or 7.5×10^{-7} ergs. This indeed is a pressure difference smaller than it is possible to secure in the most rarified *vacua.*[2]

It is rather difficult to compare this value with the figures which have been obtained by other observers, largely because of the different experimental conditions under which the tests have been made. In the first place, I have been unable to find data based on the measures of more than a dozen individuals, and, indeed, since the range of individual differences in hearing acuity is as 100 to 1, within a single group, any discrepancies which exist may very plausibly be accounted for because of the paucity of numbers constituting the groups compared. Again, Wien[3] has shown that the ear's sensitivity for tones is a function of their pitch, and that its sensitivity for different pitched tones varies within rather extreme limits. The vibration number of the tone used is, consequently, no

[2] See Rayleigh, *Phil. Mag.* **38**: 300. 1894.

[3] *Pflüger's Arch.* **97**: 1. 1903.

doubt, also an important factor in accounting for discrepancies noted.

The vibration frequency of the tone I employed I could not definitely establish. After repeated assays to fix the dominant tone by comparison with other tones of known vibration frequencies, I finally selected 500 double vibrations as being most nearly correct. But, although I think the dominant tone did not vary far from 500, there is no question but that some very pronounced over-tones present were quite effective in favoring acuity. Wien's[4] figures for a tone of approximately 400 D. V. was 1.2×10^{-10} ($E = 8.0 \times 10^{-11}$ ergs) for his own ear, when a telephone instrument furnished the sound. The figure was practically the same with a tuning fork and the Helmholtz's resonators, namely, 8.0×10^{-10} ($E = 3.6 \times 10^{-9}$ ergs).[5] Wien's results, however, make the ear to be almost one hundred times more sensitive than the experimental results of previous observers had led them to believe. For example: Toepler and Boltzmann,[6] who, according to Lord Rayleigh, were the first to make an experimental determination of this question found, with tuning forks, that the value of a sound wave's condensation at the ear, to be just audible, was 6.5×10^{-8}. This figure differs but slightly from Lord Rayleigh's own conclusions from experiments with Wolf's bottle[7] where $\Delta = 4.1 \times 10^{-8}$ for a tone of 2,730 D. V. The same writer found $\Delta = 4.6 \times 10^{-8}$ when a tuning fork[8] of 512 D. V. was employed as the source of sound.

Professor Wead,[9] also employing tuning forks found for a tone of c^2 that $\Delta = 7.1 \times 10^{-9}$ ($E = 1.1 \times 10^{-6}$ ergs) was still audible. P. Ostmann[10] with tuning forks (256 D. V.) places the threshold at $\Delta = 2.1 \times 10^{-8}$ ($E = 8.0 \times 10^{-6}$ ergs). These latter results are pretty much in accord with some recent experiments of Zwaardemaker and Quix,[11] who find that tones from a tuning fork of pitch c^2 in which $\Delta = 1.5 \times 10^{-8}$ ($E = 5.4 \times 10^{-6}$ ergs) might still be heard. More recently still,[12] they secured a somewhat smaller figure $\Delta = 7.1 \times 10^{-9}$ ($E = 1.3 \times 10^{-6}$ ergs). Lord Rayleigh's results from his telephone experiments lead him to think his previous figures placed the sensitivity of the ear too low, since some of his

[4] *Pflüger's Arch.* **97**: 33. 1903.

[5] Dissert. s. 46, 1888; also, *Wied. Annal.* **36**: 849. 1889.

[6] See Lord Rayleigh, "Theory of Sound," II., p. 433.

[7] *Proc. Roy. Soc.* **26**: 248. 1877.

[8] *Phil. Mag.* **38**: 270. 1894.

[9] *Amer. J. of Sci.* **36**: 36. 1883.

[10] *Arch. f. (Anat. u.) Physiol.* 1903, S. 321.

[11] *Arch. f. (Anat. u.) Physiol.* 1902, S. 393.

[12] *Ztschr. f. Psych.* **33**: 401. 1904.

subjects were able to still hear when $\Delta = 1.1 \times 10^{-9}$ ($E = 2.8 \times 10^{-8}$ ergs). The first figure is about the same ($\Delta = 8.88 \times 10^{-9}$) as has more recently been gotten by Webster[13] with his "*phone.*"

It was noted above that Wien's figures are from 40 to 100 times smaller than my own and those of the other investigators. Zwaardemaker and Quix[14] attribute Wien's excessive sensitivity of the ear to the fact that the telephone receiver was held snugly against the ear and that hearing was assisted by molecular bone conduction in addition to the molar sound energy passing over the ossicles.

From the character of my own data it is easy to explain differences in auditory acuity as great as 20 times, such as have been obtained by different observers, where experiments have been limited to a few subjects. Among my white subjects, although not a single individual had ever observed any diminution in his hearing function, the person with the best acuity required about 400 times less energy to just excite an auditory sensation than did the one who heard most poorly. And, indeed, the individual with the keenest ears heard about 90,000 times as well as did the poorest among Filipinos, although in conversing with these Filipinos it was not possible to detect any hearing deficiency. Wien[15] reported two cases of individuals who are still able to hear loud speech but whose hearing is from one to ten million times poorer than normal. Ostmann[16] concluded that a dimunition of hearing of one half or one-third is of slight consequence. The range of efficiency in hearing among normally hearing people is a question which, to my knowledge, has never before been investigated in this way. Ordinarily, it appears unreasonable to believe that in speech the human voice covers such wide latitudes of intensity that a person can speak 300 or indeed 1,000,000 times as loud at one time as at another and yet not be speaking appreciably loud. Our difficulty, perhaps, arises from comparing a hearing test such as the one under consideration, with the ordinary visual acuity tests in which the units are the angles subtended by light rays coming from opposite parts of the letters. These tests obviously are incomparable. But only recently von Kries[17] has shown that the minimum intensity of light necessary to excite the eye amounts to only 1.3 to 2.6×10^{-10} ergs. To see an object, 5.6×10^{-10} ergs is essential, about the same quantity of energy that Wien discovered necessary for sound, and about $\frac{1}{50}$ the

[13] Boltzmann-Festschrift, 1904, p. 874.

[14] *Ztschr. f. Psych.* **33**: 408. 1904.

[15] *Pflüger's Arch.* **97**: 37. 1903.

[16] *Arch. of Otology*, **34**: 207. 1905.

[17] *Ztschr. f. Psychol. u. s. w.* **41**: 393. 1907.

quantity necessary to excite a sensation of sound on the average according to my own experimental results. Between the intensity 2.6×10^{-10} ergs and that of ordinary moonlight, the difference is millions, and between the intensity of moonlight and that of sunlight, there is again a difference of at least 100,000. It is such differences with respect to the eye which have their counterpart in the field of hearing. Indeed, extremely great differences in the intensity of tones are not so commonly noticed as one would think. The singing of a thousand voices, though noticeably louder than that of a single singer, certainly does not appear 1,000 times as great. Under certain atmospheric conditions in the quiet country, it is possible to hear, quite distinctly, a human voice at a distance of two miles. If the loudness of the voice when at a distance of 11,000 feet be compared with that when audible only 10 feet from the speaker, some conception may be had of differences of intensity amounting to at least 10,000 to 1 and perhaps 1,000,000 to 1. In the light of such comparisons the figures showing the range of sensitivity of the normal ear are not exceptionally striking.

It will be noted that not only in the case of Whites but in those of the records of all of the groups, the average deviations are extremely large. Such of necessity must be the case, however, when the unit of measurement is extremely small, such as are physical units of sound. Although such fineness of measurement is not essential, it can not well be avoided. Our units are fixed beforehand and with these we are measuring physiological and psychological conditions as they are found among individuals. Part of the large average deviation, indeed, may also be explained by the disproportionately large number of the cases found below the mode in the curve, which arises from the inability which we experience to mark off accurately the normal from the pathological in any functioning. In a way, I sought to eliminate some of the error arising from this source by presenting an average of the mean cases only, as appears in Table XVI. But, even under such restrictions, the average deviation amounts to about one third of the average result in each of the several groups. From this it appears that so far as the hearing function goes, individuals do not distribute themselves so as to conform closely to the laws governing the normal frequency curve.

Indians.—(For a more detailed description of this group, see Chapter II.) The 64 Indians tested for simple auditory acuity, whose records are here presented, are the same whose records formed the basis of the study on the upper threshold of hearing for Indians, as already presented. Included in this group are the records of 14 full-blooded males and 13 mixed bloods, with 4 full-blooded females and 33 mixed bloods. Five of those tested were over 25 years; 81 per cent. were between the ages of 16 and 20 years. (See Table IV., p. 34.)

It must be recalled that these Indians included under the general caption "Indian" were those only attending the Model Indian School at the Exposition, who, previous to their coming to St. Louis had been in attendance, for some considerable time, at various Indian schools throughout the United States. As has already been pointed out, in habits and culture they are to be distinguished from the Indian of the forest and plain, hence, my reason for grouping together all these Indians from the schools representing a number of different tribes. For the same reason I have chosen to consider separately those tribes representing Indians who came from their natural habitats, and who, therefore, more nearly constitute what might be called representatives of the typical Indian.

For the right ear, the figure representing the condensation of a sound wave which on the average is required to just excite the organ of hearing of the Indians at the Model Indian School, is 7.5×10^{-9} ($E = 14.2 \times 10^{-7}$ ergs) and for the left ear 8.5×10^{-9} ($E = 18.2 \times 10^{-7}$ ergs). The figure for the right ear is 1.34 times larger than that for the corresponding ear of Whites, and for the left ear 1.18 times larger. On account of the relatively large average deviations these differences are not so significant as at first hand one might suppose. Still, the mathematical probability of a difference in favor of Whites is not unimportant. On the basis of the data, the chances are nearly 200 to 1 in favor of the superior auditory sense of Whites for the right ear, and 6 to 1 in case of the left ear. Arguing from Table XVI., where the individual records included in the average are restricted to those lying about the mean, the superiority of the hearing of Whites over Indians is more strikingly brought out, especially as regards the right ear. The average for Whites shows a keenness of hearing which is just about two times that for Indians. In the averages of the left ear, however, the size of the difference between the two groups is lessened, a condition, no doubt, due to the fewness of the individuals comprising both the groups under comparison.

Where the individual records are so widely dispersed as are these, instead of grouping themselves rather closely about the mode, the character of the distribution, as a whole, is really more significant in the way of indicating group differences than the figures representing the averages of the two groups to be compared. A comparison of the hearing of Indians and Whites respectively can easily be made by reference to tables XVII. and XVIII., where the individual records have been distributed according to relative position. It is clearly apparent that the modes of the distributions of the Indian hearing records—if such a term as "mode" is really applicable to such a form of distribution—fall rather decidedly lower than do those in the curves of Whites. Moreover, it is seen that the general distributions of the hearing records for Whites stand distinctly higher than do those for Indians for both ears, demonstrating that Whites as a whole hear better than do Indians, although many of the Indians, to be sure, possess ears that are more acute than the average acuity for Whites. Of the 64 Indians, however, the ears of 13 only, or about 20 per cent., rank as high as the median record for American or European ears as regards the right ear, and for the left, 24 Indians hear better than the median hearing record for Caucasians, or about 38 per cent. Taken all in all, therefore, the data point rather decidedly toward a superiority of the hearing of Whites over that of Indians; such Indians at any rate as constitute the groups here considered.

The numbers are rather small to indicate reliable sex differences; 27 men and 37 women. But the average acuity of the men, for the right ear, amounted to $\Delta = 7.4 \times 10^{-9}$ ($E = 1.38 \times 10^{-6}$ ergs); of the women for the same ear it was $\Delta = 7.5 \times 10^{-9}$ ($E = 1.42 \times 10^{-6}$ ergs). For the left ear, the figures for men and women respectively were $\Delta = 8.5 \times 10^{-9}$ ($E = 1.83 \times 10^{-6}$ ergs) and $\Delta = 8.4 \times 10^{-9}$ ($E = 1.78 \times 10^{-6}$ ergs). If we should argue from this group alone, therefore, sex differences in hearing, among Indians, do not exist.

The Cocopa (or Seri) Indians.—(For a more detailed description of these people, turn to Chapter II., page 6.) Of the Cocopa Indians I was able to secure ten hearing records, all of males. Owing to the fact that the number is so small, I shall present the data relating to the hearing of the various individuals in detail:

AUDITORY ACUITY

Name	Age	Right Ear		Left Ear	
		($\Delta = \times 10^{-9}$)	($E = \times 10^{-7}$ ergs)	($\Delta = \times 10^{-9}$)	($E = \times 10^{-7}$ ergs)
Skik.......	14	6.2	9.5	8.1	17.0
El Puck..	15	6.2	9.5	6.2	9.5
Hi.........	8	4.0	4.0	5.0	6.3
Jack.......	17	6.2	9.5	5.0	6.3
Mert.......	14	11.9	35.0	(64.8)	(1050.0)
John Roy	18	6.2	9.5	2.5	1.6
Joe........	20	14.1	49.0	18.1	80.0
Jerry......	42	3.3	2.8	6.2	9.5
Pablo......	55	(64.8)	(1050.0)	21.1	110.0
Tom........	70	(214.9)	(8260.0)	(214.9)	(8260.0)
Average.		7.28×10^{-9}	16.1×10^{-7}	9.02×10^{-9}	30.2×10^{-7}

It will be observed that the individual differences are considerable, which makes the average of relatively little value as a figure to represent the hearing efficiency of the group. Of the ten hearing records made, I rejected, as being palpably pathological, two for the right ear and two for the left; those which have been enclosed in parentheses. Even with the elimination of these records, not only on the average but in almost every individual case the records for the Cocopas are below the median record of Whites for both the right and the left ears. The single exceptions to this statement are that of Jerry, whose record for the right ear is slightly superior to the median record for the Whites, and that of John Roy, who hears slightly better with his left ear than the median of Whites. Some of the auditory deficiency manifested by these people is undoubtedly mental, but it is improbable that all can be attributed to this factor, inasmuch as some of the individuals tested were fairly bright young men and, moreover, took a decided interest in the hearing test. I questioned some of the men as to their apparent hearing deficiency, with the result that the difficulty was attributed to ear afflictions, from which it appeared almost every individual had suffered in time past. These were said to be due to exposure to storms and inclement weather. But why the Cocopa Indian should particularly be a victim to the inclemency of the weather, it is hard to imagine, inasmuch as he dwells in a tropical land, where the climate has a tendency to be arid. And besides, the Cocopas' ears are usually very completely protected by the dense mat of tarred hair which is allowed to grow long and hang loosely about the head, thus covering the ears almost completely.

The number of individuals measured was too few to speak definitively, but it seems fairly safe to assume that Cocopas do not have auditory acuity which is equal to that of Whites. It is almost certain that it is not superior.

The Vancouvers (Nutken and Kwaguitl) Indians.—(For a description of these people, see Chapter II., page 8.) I was able to secure records of the auditory acuity of all seven of these Indians, from the southern portion of Alaska, who were present at the Exposition. I shall present the data relating to this group in detail, as shown in the accompanying table.

On the average, it is seen that these Indians have an acuity of hearing only about one half as great as do the whites (see Table XV., p. 90). Considering the large amount of variation among the records, the averages for right and left ears respectively do not differ to any significant extent. The records of the two women show an acuity strikingly poor, though the records are probably not repre-

AUDITORY ACUITY

Name	Age	Tribe	Right Ear		Left Ear	
			$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs	$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs
Bob	40	Kwaguitl	3.3	2.8	5.0	6.3
Jasper	27	Nutken	14.1	49.0	18.1	80.0
Jack Curley	21	"	6.2	9.5	4.0	4.0
Charley Newell	28	Kwaguitl	8.1	17.0	3.3	2.8
Ellen	35	Nutken	18.1	80.0	15.7	64.0
Anna	30	"	10.3	28.0	14.1	49.0
Atleo	64	"	(29.9)	(2300.0)	(29.9)	(2300.0)
Average			10.01	31.05	10.03	34.3

sentative of the Vancouver Indian women as a whole. Atleo, an old man, made a poorer record than the women, as did also Jasper, but Jasper had experienced a hearing difficulty at one time, although when the test was made he did not believe his hearing in any way defective.

One of the seven had an acuity equal to that of the median of Whites with the right ear, three with the left, but, on the whole, the hearing records of the Kwaguitl and Nutken Indians are low, making it plausible to believe that the Alaskan Indians as a class possess an auditory sensitivity decidedly less acute than do Americans and Europeans.

Patagonian Indians—The Tehuelche.— (For a description of these people see Chapter II., p. 10.) I was able to make tests on four of the Indians of this tribe only, all being men. The data relating to their auditory acuity, in detail, follow:

AUDITORY ACUITY

Name	Age	Right Ear		Left Ear	
		($\Delta = \times 10^{-9}$)	($E = \times 10^{-7}$ ergs)	($\Delta = \times 10^{-9}$)	($E = \times 10^{-7}$)
Cosimero	24	7.1	13.0	7.1	13.0
Canajo	35	5.0	6.3	29.9	230.0
Senchel	55	(127.2)	(4300.0)	(127.2)	(4300.0)
Boni Farci	18	24.9	160.0	14.1	49.0
Average		12.3×10^{-9}	59.7×10^{-7} ergs	17.01×10^{-9}	97.3×10^{-7} ergs

For neither ear, is there a single record of the hearing of these Indians which is as good as the median record for Whites. On the average, their auditory acuity is less than one-half as good as that of Whites for each ear. Moreover, these were not old men nor were they in any respect prematurely aged. On the contrary, they were sturdy, vigorous, and in good health. Indeed, so far as I was able to learn, there was no apparent physical reason why their hearing should not be good. We may not assume, however, that a group as small as this is representative of the Patagonian natives.

Resumé of the Hearing of Indians.—If, now, we group the native Indians together and consider them as a single group, we have altogether twenty records. (Table XVII.) It is thus seen that only two of the twenty native Indians have auditory acuity records equal to the median for the right ear of Whites, and three for the left ear. While even yet the whole number measured is not large, it certainly is sufficiently great to justify the rather positive inference that, on the whole, Indians hear less well than do Whites. Furthermore, if we compare this group of native Indians with those who have been in attendance at the U. S. Government Schools, it will be noted as striking that the more intelligent Indians—those who have been subjected to the influences of civilization—have a better auditory acuity than do those who have been closest to nature and a natural life. So far as our American Indians of the plains are concerned, therefore, it can not be averred that their senses deteriorate with increased contact with civilization.

Filipinos.—(See Chapter II., page 6, for a description of these people.) Of the Filipinos, I measured the hearing of 137 individuals. As will be seen by reference to Table IV., the Filipinos were all men in the prime of life, in fact, young men in their teens or just beyond twenty. A more favorable group of individuals for testing it would be difficult to find, and especially is this true when we remember that for the most part they were also rather decidedly above the average of native Filipinos in intelligence and social culture. The group had been selected somewhat on the basis of hearing before reaching the United States. In choosing the men for army service those men had been rejected whose auditory acuity was discovered to be too low. Just what was the nature of the auditory test I was unable to learn, but from the information which I could glean from the men and officers in charge, and from requirements in other particulars, made of those enlisted in the Filipino service, I would judge that nothing more was required in the way of hearing acuity than ability to understand military directions spoken in an ordinary tone of voice.

The relative position occupied by the Filipinos as regards their auditory acuity may be seen by reference to the data in tables XV., XVI., XVII. and XVIII. (pages 90–92). In terms of the average result of the group, for the right ear, the condensation of a sound wave (Δ) must equal 24.2×10^{-9} ($E = 160.0 \times 10^{-7}$ ergs) and for the left ear 26.6×10^{-9} ($E = 171.0 \times 10^{-7}$ ergs) to excite an auditory sensation such as is required to interpret the stimulus in the hearing test herein employed. It may be noticed that the figures for Whites are 5.6×10^{-9} ($E = 7.7 \times 10^{-7}$ ergs) and 7.2×10^{-9} ($E = 12.0 \times 10^{-7}$ ergs) for the right and left ears respectively. These are rather extraordinary figures, in that they indicate that on the average Filipinos possess a sense of hearing which is only about one twentieth as keen as Whites, taking as a basis of comparison the acuity of the right ear of the two groups. This means that Filipinos on the average require 21 times as much energy to excite an auditory sensation as do Whites. For the left ear the sonorous energy required is about 14 to 1 in favor of the Whites. The difference between the two groups is so marked that there does not appear the slightest chance of its obliteration, however large the groups of Filipinos and Whites respectively might be made. To be sure, the average deviations for Filipinos are large; but no larger, proportionately, than are the corresponding deviations for Whites.

Applying statistical methods to the data, for the two groups, to ascertain the relative mathematical certainty of a difference in their hearing, it is found that the chances in favor of a difference between the groups are practically infinite. The chances are 300 to 1 that the difference in condensation of a sound wave required to excite hearing, between the two peoples is at the least 15.00×10^{-9}, or 300 to 1 that the hearing acuity of Filipinos is only one ninth that of the Whites for both ears. *Exactly* the same differences appear if we select only the median cases—the 50 per cent. of the cases distributed equally about the median—as will appear by reference to Table XVI. (p. 90). Referring now to tables XVII. and XVIII., it is seen that, for the right ear, nine Filipino hearing records only, and for the left ear but six of a total of 137, are as high as the median record of Whites.

When the hearing was being tested at the Exposition, both Professor Woodworth and I noticed that the Filipino peoples were doing very poorly indeed. At first, there was a disposition to attribute it to a defect in the working of the instrument. But this was found to be untrue, since a comparison of my own hearing record, which always immediately followed that of each Filipino tested, showed the testing device to be working normally.

I am at a loss to account for this remarkable difference between the auditory acuity of the Filipinos and Whites. One point of interest has already been referred to in connection with the discussion of the upper limit of hearing of these people. It was remarked in that connection that some of the Americans who had resided in the islands for two or three years had observed that their own hearing was very poor while in the Philippines, a fact which they believed to be attributable to over-dosing with quinine, a drug which they found it necessary to use freely to ward off tropical fevers. But, as was then stated, Filipinos are immune to attacks of these febrile diseases. They use no quinine or other drug with like property, so the explanation of auditory inferiority which takes into account the use of quinine, is unsatisfactory so far as it relates to the natives. Were the relative inferiority confined to this one hearing test alone it might be interpreted as at least in part due to the Filipinos' inability to react to the test from one cause or another. But a like unfavorable difference, it will be recalled, was found to obtain with reference to their upper limit of hearing. The inferiority probably extends to all phases of hearing.

In order to determine to what extent the records from this test would correlate with those for the upper threshold of hearing, I worked out the Pearson coefficient of correlation[18] for all the Filipino records, employing both methods—for relative position and for average difference. By the first method, the coefficient of correlation between auditory acuity and the upper limit of hearing of Filipinos was + 0.2907. By the second method, the coefficient of correlation between the two amounted to + 0.5408. The amount of correlation is certainly fairly large. And, taking into consideration an instrumental error amounting to between 5 and 10 per cent. in each series, the degree of correspondence is about as great as might be looked for where the measures are of quantities that are as variable in nature as are those of any sensory test.

Looking at the distribution of the records of individuals in the two tests respectively we discover that of the 25 individuals who ranked highest in auditory acuity, 18 are among the 25 highest in the upper limit test, and of the 25 individuals who ranked lowest in auditory acuity—*i. e.*, did most poorly—21 are also among the 25 poorest in the test for the upper threshold of audibility. The character of the coefficient of correlation together with the figures showing the correspondence in the cases of the records at the extremes, although they indicate that some definite correlation exists between hearing acuity and the limit of hearing, do not show a point for point correspondence such as to justify one test for both functions in a purely functional hearing test. The results furnish evidence, it seems to me, in support of the theory that in its ultimate analysis, pitch discernment is closely related to the factor of intensity.

The relation that exists between auditory acuity and intelligence

[18] $r = \Sigma xy / n\sigma_1\sigma_2$ in which r is the required coefficient; x and y the deviations of an individual from the averages of the two series of measurements; n the number of individuals; σ_1 and σ_2 the standard deviations of the two series of measurements.

was likewise investigated to a certain extent. In connection with other tests and measures, we had the people whom we measured at the Exposition perform certain intelligence tests, which were more or less simple in character. Among the intelligence tests was a simple "form test" which we believed, from observation, to correlate roughly with intellectual ability. The test consisted in selecting certain blocks, cut into geometrical forms, which were arranged in random order, and placing them into holes of corresponding shapes which had been cut into a board. A record was made (1) of the time required to perform the operation as well as (2) of the accuracy with which it was done. Taking the time required to perform this test by Filipinos and their auditory acuity, the Pearson coefficient of correlation was + 0.238. While, therefore, the correlation between intellectual ability, as measured by this intelligence test, and auditory acuity is not very considerable, it does show that intellectual ability is a factor that must be reckoned with in sensory measures, even though the tests be as simple in character as were those of auditory acuity, and especially is this true in the case of tests on primitive peoples.

I had an opportunity to test the factor of intelligence as regards its relation to auditory acuity a little farther, in the case of the Filipinos. Among other Filipinos tested for hearing were fourteen students, who were in attendance at various American universities and colleges. I separated the hearing records of these Filipinos from the others in order to compare them with those of Filipinos of the more humble walks of life. These student records for the hearing of the right and left ears, respectively, I will present in full:

FILIPINO STUDENTS—AUDITORY ACUITY—INDIVIDUAL RECORDS

	Right Ear		Left Ear	
	$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs	$\Delta = \times 10^{-9}$	$E = \times ^{-7}$ ergs
1	2.5	1.6	1.9	0.9
2	2.8	2.0	2.1	1.1
3	3.0	2.3	3.3	2.8
4	2.5	1.6	3.3	2.8
5	3.0	2.3	3.3	2.8
6	3.3	2.8	4.0	4.0
7	3.3	2.8	4.0	4.0
8	3.3	2.8	4.5	5.0
9	3.6	3.3	4.5	5.0
10	3.6	3.3	4.5	5.0
11	4.5	5.0	4.5	5.0
12	4.5	5.0	6.2	9.5
13	15.7	64.0	15.7	64.0
14	15.7	64.0	36.5	330.0
Average........	5.0	11.6	6.40	31.5

Of these records of Filipino students, it will be seen that all except the last two are above the median hearing record of Whites, in case of both the right and the left ears. But it has been questioned whether, after all, this difference in hearing between the students and the common native Filipinos is really a matter of intelligence at all but rather due to the fact that these individuals had been longer in the United States and hence had become somewhat more acclimated than had the soldiery, who came to St. Louis directly from the islands. It is difficult to conceive, though, how a climate so different from that in their natural habitat, in which they and their forefathers had lived for centuries, could be effective in bettering a sensory quality. Indeed, the argument would sound more plausible were we to reason conversely that their hearing become poorer in the United States because of their longer sojourn here.

Ainu.—(For a more complete description of these people, see Chapter II.) The composition of the Ainu group was not the most favorable for an auditory acuity test. In addition to the too great variation in ages among the peoples, it appears that to some extent the different individuals of the group were interrelated. In testing a related group such as this, consequently, there is some possibility that what is really being measured is a family characteristic rather than a racial trait. I shall present the records, however, as I secured them:

Ainu—Auditory Acuity

Name	Age	Right Ear		Left Ear	
		$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs	$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs
Yazo Osawa	23	5.5	7.5	8.1	17.0
Kutorge Hiramura	38	21.1	110.0	25.0	160.0
Goro Bete	28	4.0	4.0	6.2	9.5
Sangea Hiramura	56	4.5	5.0	11.9	35.0
Kin Hiramura (Daughter of Sangea)	6	25.0	160.0	11.9	35.0
Shrutatek Hiramura (Wife of Kutorge)	33	11.9	35.0	18.1	80.0
Ume Osawa (Wife of Yazo)	19	36.5	330.0	11.9	35.0
Santukuno Hiramura	53	36.5	330.0	43.4	450.0
Average		18.12	122.7	17.06	102.7

In making up the averages for this group, I included the records of all the individuals examined, in spite of the fact that they presented wide variations. In case of a doubtful record in making up the averages of larger groups, such as the Whites or the Filipinos, I observed the rule laid down by Professor Cattell of rejecting the record of any individual if its divergence from the average exceeded three times the average deviation. Such a record is more likely to be an instrumental error or to be a case of pathological functioning than a question of simple deviation from the average. Such a procedure in the case of the records of the Ainus, however, would be difficult to follow.

Of the eight records of hearing for the right ear, five were very poor and only three fair in comparison with those of Whites, while for the left ear, seven of the eight are relatively poor, though for this ear the average shows slightly higher than that of the right. None of the Ainu people, in case of either ear, stand as high as the median records for Whites, for corresponding ears. While it is not permissible to become dogmatic from so small a sampling, it seems probable enough that the average of a large group of Ainu people would show about the same relative inferiority with respect to auditory acuity as was discovered among the few examined. At the time of making the examinations, and again some months later, I inquired carefully of each person whether he or she had ever observed that his hearing was defective, but the replies were invariably in the negative. There is no way in which the question can be investigated other than by the method of observation and selection, but I am convinced that the inferiority of the Ainu in respect to this sense is due in no small part to their sluggishness in reacting to impressions. So sluggish and unresponsive are they, that a stimulus of more than ordinary intensity is required to arouse them, and it seems not unnatural that weak auditory stimuli should fail to break over the threshold of consciousness. Confirmatory of this opinion is the fact that of the eight individuals tested, the three who were the most intelligent and alert of the group were likewise the three who possessed the best records of hearing acuity.

No effort was made to differentiate the sexes in the tests, though it will be seen from the data that the average acuity of the women is considerably less than that of the men. The record of Kin was high also, and perhaps should have been omitted in making up the average, but she seemed to understand the procedure and to react as intelligently to the questions put to her as the average of her people, and for this reason her record was included with the others.

Pigmies.—(For a description of these people see Chapter II., page 9.) It would, perhaps, be better to consider this group as one of native African Negroes, rather than of a particularly primitive or aboriginal race. Some of the number, at least, were not Pigmy at all: they belonged to a type of large red negro found in the central Congo district. Yet even as regards Negroes who have been little influenced by the habits and arts of civilization, they will give instructive information. I shall give the data relative to their ages and tribal connections as it was given us together with the hearing records which I was able to make, in the following table:

PIGMIES—AUDITORY ACUITY

Name	Age	Tribe	Right Ear		Left Ear	
			$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs	$\Delta = \times 10^{-9}$	$E = \times 10^{-7}$ ergs
Shamba............	30	Batwa	6.2	9.5	6.2	9.5
Malinga...	16	"	11.9	35.0	9.1	21.0
Bushaba	13	"	18.1	80.0	7.1	13.0
Latuna	15	Batsuba	9.1	21.0	9.1	21.0
Lumme	17	"	6.2	9.5	6.2	9.5
Average...........			10.31	31.0	7.54	14.8
A. D...............			3.55	20.8	1.24	7.6

In contrast with the records of the Ainu just considered, it is seen that the Pigmies present a rather homogeneous group so far as auditory acuity is concerned. For both ears the average deviations are small. This probably is due, at least in part, to the fact that the individuals were about of an age, and differed little temperamentally from one another. It will be remembered that for the upper threshold of hearing the Pigmy records were all high, and if the same relative distribution were to follow, were the number increased indefinitely, Pigmies would possess an upper threshold for hearing superior to that of any other race, not being inferior to even Whites in this respect. An equal degree of superiority was not attained by Pigmies in auditory acuity, although for the left ear (see Table XV.) the average acuity of Pigmies is slightly higher than that of the same ear for Indians who next approach Whites in keenness of hearing; the record for the right ear falls below that of both Whites and Indians. Little significance perhaps may be attached to an average measure, where the numbers measured are so few, but the character of the distribution of the group seems to indicate a decided inferiority for the hearing of the Pigmies as compared with those of both Whites and Indians. The curve of their hearing falls perceptibly lower, the average being relatively higher owing possibly to the fact that none of the Pigmies possessed anything in the way of an organic hearing defect, which might tend to lower the standing of the group. An explanation of the differences found between the comparative records of Whites and Pigmies in the upper threshold of hearing and for simple acuity respectively might be given on an intellectual basis. In the test for the upper threshold of hearing, the stimuli are of longer duration. Moreover, they require no interpretation, and consequently the feeling of hearing the sound, which really has only a subthreshold value, may be more easily accomplished, than the actual interpretation, which the counting of the stimuli, implied in the simple acuity test, necessitates. I put this forth as suggestive only.

CHAPTER XI

Summary and Conclusion

It is very difficult to compare the foregoing results with those of Myers in the same field, by reason of the differences in the method employed in collecting data. In the classic work of Myers, on Papuan hearing, several different devices for testing the hearing of the primitives were employed.[1] And to such an extent were these different measures therefore confused that it was necessary for Myers to report all the data he collected in terms of a personal fraction in which the hearing of one of the members who made up the expedition was the denominator, while that of the subject constituted the numerator. Of the 35 Islanders who were examined for auditory acuity by Myers, the hearing of seventeen only was reported and of these twelve were children, five only being adults. Of the children five could hear as far as Myers; seven were clearly inferior; and of the adults examined, all possessed a very low acuity. Although consequently I can speak only in general terms, Myers's conclusions do not appear to differ essentially from my own in this, that they point out clearly the obvious superiority of Whites over primitive races in the keenness of their hearing sense.

With my smaller groups, as has been repeatedly stated, the number examined is insufficient to do more than indicate a general tendency of the group within the region of a large probable error. Especially is this true of such peoples as the Vancouvers, the Pigmies, and the Cocopas, where it is difficult to predict with any degree of probability the character that the hearing curve of the peoples as a whole might assume, inasmuch as the records are so scattered—some being fairly high; others extremely poor. But in the case of such groups as the Indians, Filipinos and Whites, the number of measurements is sufficient to give at least the general character of a complete distribution of the race as a whole.

Taking the results of all the groups examined for what they are worth their standings respectively are as follows, as regards the acuity of the right ear:

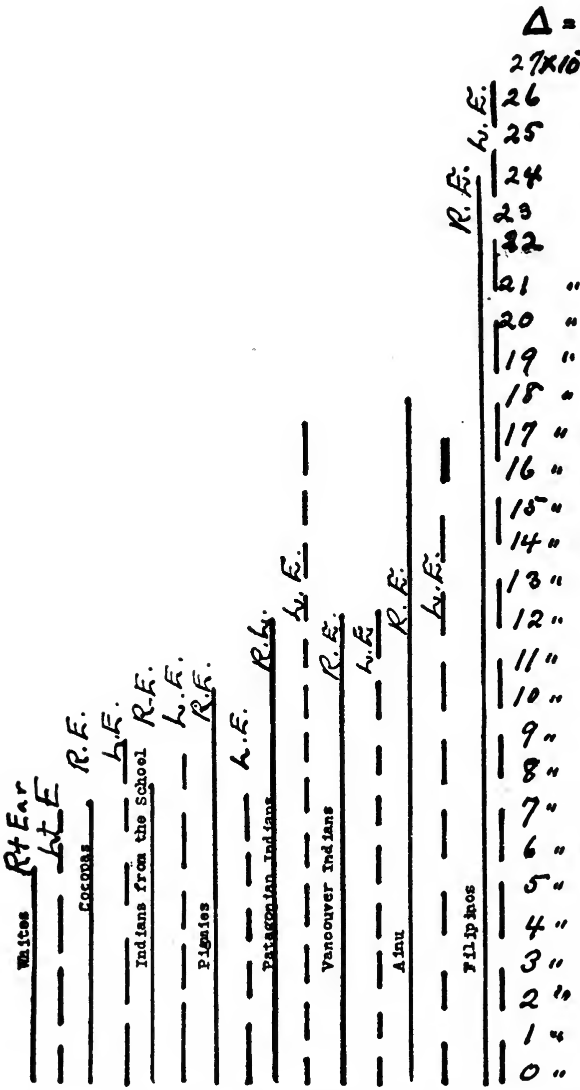
Whites; Cocopas; Indians from the School; Pigmies; Patagonian Indians; Vancouver Indians; Ainu, and lastly, Filipinos.

For the left ear, the order is slightly changed, Whites and

[1] See Report of the Cambridge Anthropological Expedition to Torres Straits, Vol. II.

Filipinos, however, still retaining the positions at the two extremes; the order from the most acute people to the least acute being:

Whites; Pigmies; Indians from the School; Cocopas, Vancouver Indians; Ainu; Patagonians and Filipinos.



It will be observed that the relative positions of the three most numerous groups, namely Whites, Indians from the School, and

Filipinos remain unchanged. Indeed, they retain in respect to each other about the same relative position for both the right and left ears, and also, when the basis of comparison is that of absolute units of hearing, instead of relative position. To summarize the various comparisons which have been made in connection with the data relating to the several groups, we may show the following ratios indicating the relative keenness of the hearing sense of each group as compared with that of Whites:

		Right Ear		Left Ear
Whites—Cocopas	Ratio	7 to 5	Ratio	9 to 7
Whites—Indians (School)	"	9 to 5	"	8.5 to 7
Whites—Pigmies	"	10.5 to 5	"	7.5 to 7
Whites—Patagonians	"	12 to 5	"	17.5 to 7
Whites—Vancouver Indians	"	10 to 5	"	10 to 7
Whites—Ainus	"	18 to 5	"	17 to 7
Whites—Filipinos	"	24 to 5	"	26.5 to 7

Preyer, Fechner,[2] Bezold and others have observed that in hearing tests, the left ear in general is more acute than the right. Miss Nelson,[3] on the contrary, found that in both men and women the right ear was the better. The left ear, it will be remembered, was found to be superior with respect to the tests for the upper threshold of hearing. In case of the ears of each of the larger groups, my own experiments in general confirm the observations of Miss Nelson as opposed to those of Fechner and Bezold. The acuity of the left ear not only of the three larger groups but in three of the five smaller ones, is clearly inferior to the right, the Pigmies and Ainus alone being exceptions. When making the measurements of the upper threshold, it will be recalled that it was stated that almost invariably the right ear was first tested. In consequence, I believed the superior upper limit of the left ear to be due to the effect of practise in hearing shrill tones. But this explanation will not apply to the case of the acuity test. Instead of testing invariably one particular ear first, the process was alternated—the right and left ears alternately being first tested in successive subjects. Practise effects could not, therefore, have been operative in causing the average for acuity of one ear to be higher than the other. It is, indeed, more probable that the causal factor is organic rather than psychological.

The one fact standing out most prominently as a result of these measurements is the clearly evident superiority of Whites over all other races, both in the keenness and in the range of the hearing sense. The evidence is so clear and striking as to silence effectually the contention that the hearing function, inasmuch as it is of rela-

[2] *Poggendorf's Annal.* (4th series) **3**: 500.
[3] *Psych. Rev.* (suppl.) **12**: 280. 1905.

tively less utility in the pursuits attending modern social conditions than those surrounding the life of the savage, has deteriorated and is degenerating. On the contrary, they are more nearly in keeping with the advanced positions taken by modern dynamic psychology, to the effect that not only the intellectual but sensory possibilities are to be stated in terms of the variety of motor response of which the individual is capable. Other things being equal those individuals or races possessing the greatest complexity and variety of reactions to elements in their respective environments likewise will be gifted with keener and more acute sensory mechanisms.

If all discrimination of data coming to the senses must finally be stated in motor terms, as most psychologists would have us think, then those peoples whose social activities call for the greatest complexity of response will, of necessity, possess keener senses along those lines in which the social media call for closer discrimination. This motor aspect of a sensory function also serves, to a certain extent, to explain a rather startling auditory inferiority on the part of some of the natives of tropical lands. In these regions of warmth, where lack of thrift and indolence are fostered by nature's bounty, in its luxuriance and plenty in the way of food, in its relative immunity from exigencies calling for protection and shelter, adaptive activities are found at their lowest ebb. Contrast these conditions with those of higher latitudes, in which the individual is in constant strife to keep himself in harmony with his surroundings. And the ear plays no insignificant rôle in this endless round of readjustment. Roughly, and in general, the data on hearing were found to correlate with motor versatility as regards the different races.

Then again the more involved a test, the more probable is it that differences in the degree of intelligence of the subjects tested will be effective in modifying in an unfavorable direction the performance of the less gifted group. It has already been indicated that the test for auditory acuity which I employed was more than a simple sensory test, inasmuch as it required an interpretation of the stimuli presented to the ear, and for this reason it was believed that some of the differences between the acuity of the several peoples tested might be attributable to the obvious fact that striking differences in mental alertness obtained among the different races. But to what extent the mental factor was responsible for the degree of auditory inferiority in such a race as the Filipinos, it is impossible to tell with any degree of certainty from the data at hand.

Only two factors have been indicated to account for differences in auditory acuity found among primitive races, and between primitive races and Whites. That there are many others, some perhaps more significant and vital than those pointed out, the writer only too well appreciates. But the field is new. And indeed the conditions surrounding the taking of the measurements herein reported were not as favorable for making an intensive study of the factors entering into hearing as the importance of the problem warrants. Still this is not the phase of the study that I care to unduly emphasize. As significant as the problem of racial differences in hearing is for genetic psychology, and the writer feels this importance keenly, it was to the method employed in testing hearing, particularly, that it was desired to call attention. Psychology as a science has advanced to that point where quantitatively exact methods of research ought to be emphasized, as well for evaluating functions as for equating differences between individuals or among races. Such methods too are demanded as are possible of reinstatement, and offer data in a nomenclature more specific and determinative than normatively established units of measurement can give. If consequently the methods that have been used to obtain the results herein presented succeed in accentuating the need for more exact objective methods of research in experimental psychology, the writer's purpose will have been attained as effectually as by the recognition of the light that has been thrown on the problem of the hearing of the inferior races.

STUDIES IN DEVELOPMENT AND LEARNING

CONTRIBUTIONS FROM THE DEPARTMENT OF PSYCHOLOGY AND CHILD STUDY IN THE FITCHBURG NORMAL SCHOOL, MADE BY THE ADVANCED CLASS OF 1907, AND EDITED

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CONTENTS

PREFACE

The students of the advanced or four years course in the Fitchburg Normal School are required to write a thesis during the last year on some subject connected with psychology or child study. They usually collect data and treat them statistically. This year the data, consisting largely of a series of measurements and tests made upon the six hundred children in the training school during the last five years, were of more value than usual, and it was thought best to print parts of a number of the theses, with an introduction and supplementary comments by the head of the department. Only those parts of the theses that are of general psychological and pedagogical interest are included. Complete theses would doubtless be of interest to some who are interested in knowing the value of thesis writing as a method of training elementary teachers, but to have published in full would have made the monograph too large and detracted from its interest to psychologists. All unnecessary details therefore, together with some suggestions of practical applications, are omitted. With very few minor exceptions the language of the students is unchanged. All the theses were accompanied by references, but as most of them were incomplete, including only material well known to psychologists, they are omitted. The editor when necessary has prefaced each thesis with an explanation of the tests on which it is based, and followed each with brief *comments*.

It may be of interest to remark that the past year nearly all of the advanced class, instead of taking a general topic for study, took a concrete case of a child backward in one or many lines and tried to improve him, accompanying the teaching by reading and by carefully kept records of what was done and the results. This studying of individual children in order to teach them more effectively proved to be of much more value and interest than the mere studying of individuals without expecting to do anything for them.

E. A. Kirkpatrick.

Fitchburg, Mass.,
November, 1908.

VI.

STUDIES IN DEVELOPMENT AND LEARNING

THESIS I

Physical Tests and Measurements

By Lillian G. Myers

Editor's Explanation.—The data regarding physical development summarized in the tables that follow were taken about the first of October of each year by the Normal students under the direction of the head of the Department of Child Study. One or two students usually took charge of each instrument and made the tests and measurements of all the children as they passed along the line. The ordinary clothing at that season of the year was worn by the children except that the boys were asked to remove their coats while being weighed and measured. At first shoes were also removed, but later this was given up. The head of the department tried to secure accuracy in measurement but of course there were slight errors due to the personal equation of those taking the measurements at different times. Larger errors, due to the mishandling of an instrument, sometimes occurred but were usually discovered and corrected before many measurements were made. Although not quite as accurate as measurements made by experts the sources of error are not large enough to vitiate the general results. In the grip test an adjustable dynamometer of the Smedley type was used, which gives a higher record especially for small children than the ordinary non-adjustable instrument. The tests of chest expansion and of lung capacity are not always correct indications of the strength of individual children, especially of the lower grades because they do not know how to empty and fill the lungs to the extent that they are capable of if they only knew just how to do it. Sometimes a determined effort to contract the chest results only in rigidity or actual expansion. Better results are usually obtained by letting the children imitate other children or the experimenter than by telling them what to do and urging them to do their best.

Thesis.—(As the facts discussed in this thesis are of a familiar character only the table is here reproduced.)

TABLE I

PHYSICAL MEASUREMENTS AND TESTS

Ages	Girls								Boys							
	6	7	8	9	10	11	12	13	6	7	8	9	10	11	12	13
Number	40	40	40	40	40	40.	30	10	40	40	40	40	40	40	30	10
Weight	20	22.9	25.1	26.8	29	32.5	37.6	41.3	21.4	23.5	25.1	27.5	30.3	32.4	34.6	38.5
Height standing	113.5	120.8	125.9	130.3	136.3	142	147.9	154	115.4	121.8	126.2	130.4	135.6	139.7	144	148.[illegible]
Height sitting	61.4	65.3	67.5	69.2	72.4	73.4	75.5	80	62.7	65.2	67	68.7	70.7	72.4	73.5	75.7
Breadth of head	14.1	14.1	14.1	14.2	14.3	14.4	14.5	14.7	14.2	14.3	14.5	14.6	14.5	14.6	14.8	14.8
Breadth of chest	18.3	18.8	19.4	20.1	20.4	21.1	22	22.4	18.9	19.5	20	20.9	21.3	21.6	21.7	21.8
Breadth of waist	16.5	17.6	18.3	18.7	18.9	19.3	19.5	19.2	16.6	17.5	18.7	19.7	20.2	20.3	20.2	19.3
Girth of head	50.6	51.7	52	52.4	52.7	52.7	53	52.6	51.3	51.7	52.6	53.2	53.3	53.4	53.4	53.9
Depth of chest	14.6	14.9	15	15.7	15.8	16.2	16.9	17.2	15	15.4	15.5	16.2	16.4	16.7	17.2	17.8
Chest expansion	5.0	5.6	7.2	6.4	7.4	7.9	8.4	8.3	5.2	6.0	7.2	7.4	7.6	8.6	8.1	8.5
Lung capacity	.67	.92	.95	1.11	1.20	1.41	1.50	1.34	.80	1.07	1.18	1.43	1.54	1.72	1.87	1.8
Grip, right	9.9	12.1	13.4	13.8	14.9	18.3	20	22.5	12.2	14.5	15.8	18.8	18.9	20.9	22.2	21.6
Grip, left	10.1	12.4	13.3	14.3	15.2	17.2	19.9	20.6	12.3	14.1	14.9	17.5	18.5	20	21.7	20.7

Editor's Comment.—These tables may best be compared with those of Professor Hastings in his "Manual of Physical Measurements" since the instruments and methods used were similar. With very few exceptions these tables are higher for both boys and girls at all ages than those of Professor Hastings though in most cases where there is no difference in the instruments and the mode of taking the measurements, the difference is very slight. His tables are based on more children but these have the advantage of being based in a large measure upon the same children at different ages.

The differences in height and weight which, according to the tables, place eastern children about one year in advance of western in those respects is partly, but perhaps not wholly, accounted for by the fact that our children retained their shoes, while those measured under the direction of Professor Hastings did not.

In height sitting the differences are slight, also in breadth of head and girth of head, but in breadth of chest and of waist they are somewhat greater, while in depth of chest the difference is very marked amounting at nearly every age to two or more cm. This can not be accounted for by any difference in clothing or mode of measurement so far as we know. It seems to indicate a marked physiological difference between eastern and western children.

Chest expansion and lung capacity correspond pretty well, considering the difficulty of making accurate tests of children who have had little or no practice in controlling the muscles of chest and lungs.

The difference of from two to six kilograms in the strength of forearm, as indicated by grip, can not be wholly accounted for by the adjustable dynamometer used by us, for our records are somewhat higher than those of the Chicago Child Study Department where the same instrument was used. The fact that many of our children had taken the test one or more times may have given them some advantage. In one other respect our results are peculiar, *i. e.,* in the relative strength of right and left hands. In our tables the figures for the left hand are relatively high at all ages, and up to nine years of age are absolutely higher than for the right hand. Others have found the right hand slightly superior at six and markedly so after twelve.

It was hoped that these tests, carefully chosen for their close relation to development and health, would be a valuable means of diagnosing the condition and progressive development of individual children, but this hope has been realized only in part. In averages of the measurements of a number of children, errors due to slight difference in clothing and to the personal equation of different persons who make the measurements, and to the variation in the intelligence and intensity of effort on the part of the children in the strength tests, are likely to cancel each other. In individual cases, however, the figures recorded for the same child in successive years may often be misleading. This is true of the vital strength tests and the measurements of breadth of chest and waist. In such measurements as those of girth of head and breadth of head, although the probable error of measurement is not great, yet the change from year to year is slight and may be less than the error of measurement.

If the same person made all these measurements and tests year after year in the same way, the records would doubtless be very significant of the actual development of individual children, but where different persons have handled the instruments and directed the efforts of children who have not practiced the tests, the records can not be implicitly relied upon as showing variations in the individual though any marked individual variations from the normal are clearly shown, and the averages are fairly reliable.

In the case of measurements of height and weight the facts are somewhat different, for the changes from year to year are well above any probable error due to the personal equation of the measurers or to slight variation in clothing (if the measurements are made at the same time of the year).

THESIS II

Development of Auditory and Visual Memory

By Mary J. Conway

Editor's Explanation.—The data regarding memory discussed in this thesis were taken each year at about the same time by the head of the Department. Cards with from three to nine figures of good size were used for the visual test and similar series of spoken numbers for the auditory test. In both cases the time occupied was between one and two seconds for each digit. In the lower grades series of from three to six digits were given, while in the upper grades series with two or three more digits were also given. The same number of digits was given two or three times. In marking, no credit was given except for series that were correctly reproduced in the proper order. Averages were not made but each pupil was credited with the highest number of digits that he reproduced correctly every time that many digits were given him. This was taken as his standard memory span while variations from this standard, due to fluctuating attention and other causes were indicated by plus or minus the excess or deficiency. For example a boy who reproduced six digits every time that number was shown, but failed once on five digits and succeeded on one of seven and one of eight digits was marked $6 + 1 + 2 - 1$. Such extreme variation as this was of course rare. Had four or five series of each number of digits been used the standard alone would probably have been a good indication of the individual mental span. The children were always very much interested in this test, and though pains was taken to prevent them from beginning to write before the series was complete or from looking on the paper of some other child, yet a few incorrect records due to these causes were doubtless included. Some errors were probably also made by the student teachers who looked over the children's papers and recorded the results. Such sources of error however would not apply to one age or sex more than another and hence would not affect the comparative averages.

Thesis.—The results of the tests taken, when tabulated, show what has been proved by other tests and what is learned by ordinary observation, that the memory span, or the power to reproduce impressions just received, increases with age to a marked degree.

These same tests had been taken on the students at the Fitchburg Normal School and the average of 103 students was 6.3 for the auditory and 7.3 for the visual record in the first test, and the average of eight who took the test a second time is 7.2 for the auditory and 8.5 for the visual. These show a slight superiority over the record of the children in the grades.

Older pupils have the advantage in a memory test, because no test that can be given is so new to them as to the child. In another test in this school where some adults who were unacquainted with Greek and some children were tested with the Greek letters, the adults' record did not show much gain over the children's. Even in this case, although the letters were unfamiliar to the adults, still they could see their resemblance to other symbols more readily than the children and they still had the advantage.

Development of memory is really a training of the mind, in the power of concentrating the attention and of associating the new with the old.

TABLE II

INCREASE IN MEMORY SPAN

Ages.	6	7	8	9	10	11	12	13	14
Auditory									
Boys, No.,	8	56	63	56	62	51	40	26	18
Average,	3.8	3.9	4.2	4.6	4.9	5.5	5.1	5.6	6.0
Girls, No.,	15	54	71	65	79	53	38	30	5
Average,	3.6	4.1	4.3	4.8	5.0	5.3	5.5	5.8	5.0
Visual									
Boys, No.,	10	48	62	58	66	52	19	26	7
Average,	3.1	3.8	4.0	5.0	5.6	5.9	5.4	5.0	5.8
Girls, No.,	14	48	63	74	71	53	40	25	3
Average,	3.4	3.6	4.5	4.9	5.5	6.0	6.1	6.3	6.0

Table II. shows the average memory span of children of different ages, age six meaning more than six and less than seven, and the same for other ages. Fig. 1 shows the same as the table, except that allowance has been made in the figure for the " variations " from the standard records (see above, p. 3). One third of each variation is added to or subtracted from the standard.

With the boys from six to nine the auditory memory is better than the visual, with the girls from six to eight the same is true showing what has been proved by other tests that younger children remember better what they hear, probably because of the fact that before a child enters school he receives most of his ideas through the auditory sense, while the experience that the child gets in the school room teaches him visual language, and this learning of words and numbers visually gives him a tendency to represent things visually.

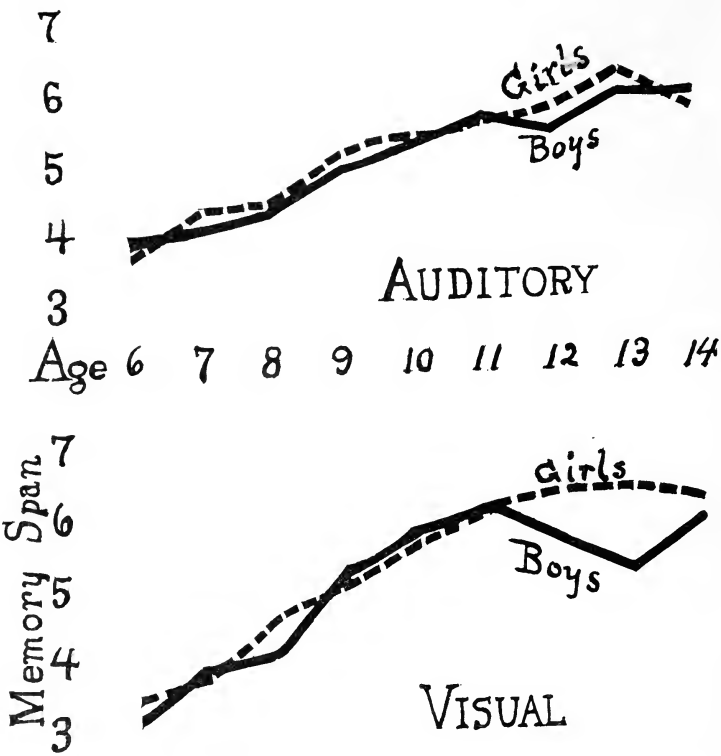

FIG. 1. Increase in Memory Span.

From the ages of eight and nine to fourteen the visual memory is better than the auditory. In the tests taken upon Normal School students the same is true.

The auditory memories of girls from six to eleven are better than those of boys, from eleven to twelve both records are the same, from twelve to thirteen the record of the girls is better than the boys again, but at fourteen the curve of the boys is above.

In the visual curve the girls record is higher than the boys except from eight to nine where the boys' record is better. From ten to fourteen the record of the girls is very much better than of the boys.

Generally speaking the curves seem to indicate that the memory span or immediate memory of girls is better than that of boys.

The preceding table and curve show the average memory span of children, not all of whom were the same at the different ages. An attempt was also made to trace the gain of the *same* children from one year to the next—which could be done to some extent, as three or four successive tests had been made on a good number of them. The records for each individual for the three or four successive tests were examined, and if there was a gain from one year to the next it was marked *plus* and if there was a loss it was marked *minus*. The results are shown in Table III., which gives the average gain both in the "standard" record (see above, p. —), and in the record when allowance is made for the "variations." The numbers under each age give the gains in the year preceding that age.

TABLE III

MEMORY SPAN

Age.	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14
Auditory														
Girls, No.,		3	6	11	6	31	5	25	4	15	3	15		
Standard,		.3	.5	1.1	.7	—.1	1.6	.5		.2	2.0	.6		
Variation combined,		.3	.3	1.4	1.7	.1	1.2	.3		.4	2.1	1.3		
Boys, No.,		5	3	25		25		12	2	8	4	9	1	5
Standard,		.2	1.0	.8		.9		1.1	1.0	.9	.3	—.1	2.0	.8
Variation combined,		.3	1.1	.9		.6		.9		.3	.3	—.1	1.7	.3
Visual														
Girls, No.,	2	5	5	15	7	25	8	18	3	12		16	3	2
Standard,	1.0	3.0	.4	.9	.1	1.0	1.0	1.0	1.3	—.3		—.3	—.7	.5
Variation combined,	1.0	2.7	—.1	1.0	.3	1.0	1.0	1.0	1.4	—.5		0	—.1	.7
Boys, No.,		3	3	20	5	17	7	10	3	13	6	11	3	2
Standard,		1.3	—.3	1.1	1.4	1.5	1.3	1.3	1.3	.3	—.2	—.6	—.7	1.0
Variation combined,		1.1	1.1	1.1	1.2	1.5	1.1	1.3	1.6	.4	—.4	—1.0	—.6	.7

In the auditory memory of the girls there is a marked gain to nine and a half, then at ten there is a loss with a gain from twelve to thirteen.

In the visual memory of the boys there is a very marked loss at thirteen the same as with the auditory.

In the visual memory of the girls there is a loss between eight and nine but a more marked loss at twelve.

In the visual memory of the girls there is a loss between eight and nine but a more marked loss at twelve.

In each case there seems to be a difference of from one to three years in the time when this retarded growth of memory comes to the boys and girls, the retarded growth coming earlier in the girls in each case.

This corresponds with the studies which have been made of the bodily growth of children, since there is a diminished rate of growth in girls about ten and in boys about twelve with a rapid growth afterward.

This matter of memory span is very important in the learning of spelling; the fact that some children fail repeatedly in their spelling may be because those children have a poor visual or auditory memory.

If we remember that there is a limit to a child's mental grasp we shall be careful about the length of directions which we give him and will not think a child stupid because he does not take in very many directions at once. In giving a dictation lesson the number of words dictated at a time should be determined by the child's age and mental grasp.

The primary teacher should remember the fact that the younger pupils are more ear minded than eye minded.

Since an impression is more lasting if it is received through two senses at once, the teacher should see that the pupils receive both visual and auditory impressions of facts as much as possible. In fact these principles are applicable to everything we teach.

Editor's Comment.—With many, and probably most persons the number of things that can be held in mind at one time, or that can be grasped and immediately reproduced, is very definitely limited, at least for any particular kind of mental content. The determination of such limit for an individual at a particular stage of development is therefore a practicable and important means of measuring certain forms of his individual mental ability. These tests indicate that such limits or standards may be determined by only a few tests, since a large number of children can remember just so many digits, and uniformly fail when more are given. It is probable that most tests of *individual* mental ability would better be directed toward determining the limit of power under usual conditions, instead of averages of a large number of experiments. Variations from these limits should not be fused with the ordinary limits by averages but kept separate and interpreted as signs of variation, which is such a marked feature of some lives and comparatively rare in others.

THESIS III

The Development of Children in Quickness of Perception and Movement

By Sadie E. Lamprey

Editor's Explanation.—The Perception Motor test consisted of making with a pencil one hundred marks in fifty squares in each of which was the figure 1, 2 or 3 to indicate the number of marks to be made. Students supervised the tests of individual children, recording the time in seconds from an ordinary watch, the children being encouraged to work as rapidly as possible. Since this test was made at the same time as the other tests, about a month after school began, many of the first grade children were not familiar with figures. Those who were not were taught how many marks the figures told them to make and when they could tell correctly how many marks they were going to make in various indicated squares the test was begun. In general the errors were so few as not to be worth while keeping account of. Children who could not learn or were so slow that they could not complete the test in five minutes were excused, their cards being marked "x." There were only a few such children and in every case they were children who were not capable of doing successfully the regular school work. Children sometimes stopped to rub out an extra line they had made or to see what some one else was doing, but were always reminded by the student in charge to go on marking as fast as possible. The sources of error due to such cases as these were large in the case of the smaller children, but much less for the larger children.

Thesis.—The data upon which this thesis is based consist of the complete records of ninety boys and ninety-five girls who were tested at least four times. Besides these there were records of about two hundred and fifty children, who had taken the tests a less number of times. As an aid in getting at the conclusions, this supplementary list was sometimes used as will be explained later.

In one set of tabulations the results of *all* the children taking the first test were tabulated according to age and sex; here the comparison is between *different* children at different ages. In the other tabulations the *gain* of each child over his own previous record is the basis of tabulation. The two tables agree fairly well as to the periods of greatest gain.

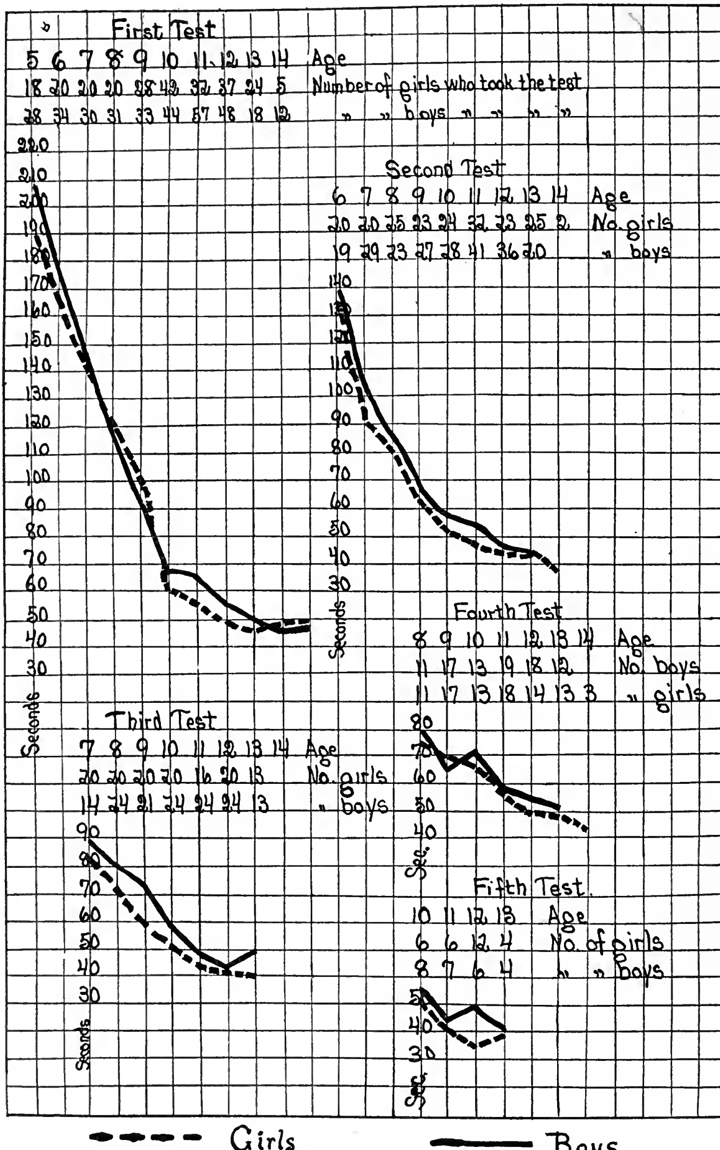

FIG. 2. Time Occupied in the Perception-motor Test at Different Ages, and at First to Fifth Tests.

The progress in rapidity according to this test may be divided for each sex into three periods, the time of greatest growth, the time of next greatest, and the time of least. With the girls the first period extended from five to nine, the second from nine to eleven, the third from eleven to fourteen. With the boys the first period extended from five to nine, the second from nine to twelve, and the third from twelve to fourteen. The complete arrest of growth with the girls came from twelve to thirteen, a year younger than with the boys. The average of the 335 boys of all ages was 86 seconds, while of the 246 girls was 78 seconds, showing that girls were better in this test than boys. The only ages in which the boys did not require more time than the girls were those of thirteen and fourteen.

The results of the second test correspond in a general way with those obtained in the first. The ages of the boys ranged from six to thirteen, and of the girls from six to fourteen, though the number taking the test at fourteen was only two.

The most important thing to be noted is the fact that the girls failed to improve from eleven to thirteen and especially from twelve to thirteen. With the boys this failure in improvement began at the age of twelve, but the data do not cover the fourteenth year, so that this can not be traced any further.

An interesting thing in comparing the averages of the first two tests for the corresponding years is the fact that with the practice gained in the first test came added ability which carried over the interval of a year so that children taking the test the second time were superior to those of the same age taking it the first time.

The total average for the 194 girls taking this second test was 66 seconds; for the 223 boys, 70 seconds.

The third test which included children of the ages seven to thirteen showed a curve something like those of the first and second tests. In the case of the girls the arrest came at the formerly found period—from eleven on. With the boys there is a loss from the age of twelve to thirteen. The average time required by the 129 girls was 58 seconds, by the boys 61 seconds. There was a slight gain in the ability of the children of different years in the third test over the ability of those in the corresponding years in the second test, though the difference was not as great as was that between the first and second tests.

The curves for the fourth and fifth tests are given, though the number of pupils in comparison with those in the first are so few that the results are less reliable.

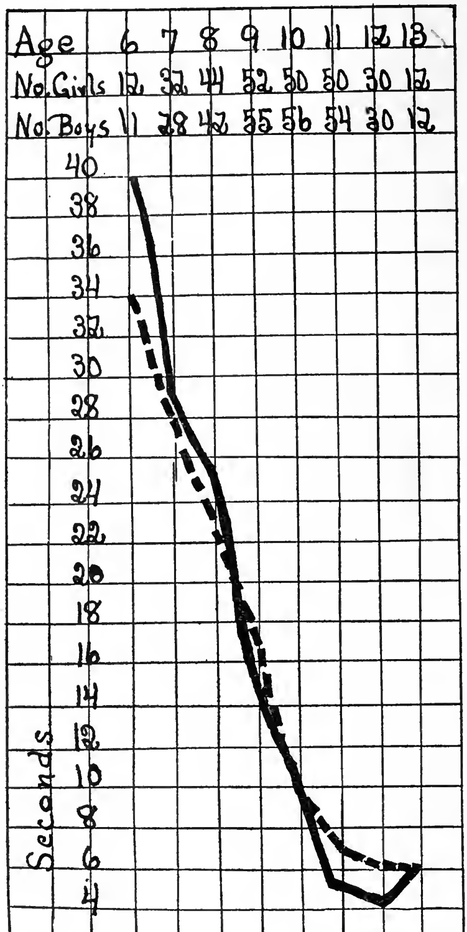

FIG. 3. The Perception-motor Test. Gains of individuals from year to year. Broken line for girls, solid line for boys.

In the second tabulation the result sought was the gain of the children over what they were themselves a year before.

With both boys and girls the gain was much greater in the earlier years than in the later. From eleven on, the girls practically made no gain. This slackening of development in the boys came from twelve on. Both sexes suffered actual loss at one period: the fourteen-year-old girls over those of thirteen, and the fifteen-year-old boys over those of fourteen. The greatest gain in both sexes of one age over the preceding was of those of six over those of five, the gain of the girls being 35 seconds, and of the boys, 39 seconds.

In order to find out if practice actually did influence the results of the successive tests, the gain in time of boys and girls in the second test over the first, in the third over the second, and the fourth over the third, was reckoned and divided by the number in each case taking the test. The average gains for the boys were respectively 21, 15 and 13—for the girls 20, 14 and 11. Practice must therefore have aided.

Other experiments show that with continuous special practice the improvement in this perception motor test is rapid. These experiments were made upon a group of normal school students and upon two children. As a result of the practice, the decrease in time after ten trials was 18 per cent. for the normal school students, 20 per cent. for the seven-year-old girl, and 25 per cent. for the five-year-old boy.

TABLE IV

TIME FOR THE PERCEPTION-MOTOR TEST, REPEATED ANNUALLY

5	6	7	8	9	10	11	12	13	14	Age in years
					Girls					
18	20	20	20	28	42	32	37	24	5	Number taking test 1
194	140	118	95	62	57	50	47	48	47	Average time
	20	20	25	23	24	32	23	25	2	Number taking test 2
	132	91	79	61	52	47	45	44	37	Average time
		20	20	20	20	16	20	13		Number taking test 3
		85	74	62	51	43	42	40		Average time
			11	17	13	18	14	13	3	Number taking test 4
			68	64	56	46	41	39	34	Average time
					6	6	12	4		Number taking test 5
					54	42	35	37		Average time
					Boys					
28	34	30	31	33	44	57	48	18	12	Number taking test 1
206	144	116	89	68	63	56	49	46	46	Average time
	19	29	23	27	28	41	36	20		Number taking test 2
	135	102	86	68	57	52	46	44		Average time
		14	24	21	24	24	24	13		Number taking test 3
		88	80	73	57	46	41	49		Average time
			11	17	13	19	18	12		Number taking test 4
			77	56	61	48	45	43		Average time
					8	6	7	4		Number taking test 5
					56	45	48	40		Average time

The two children later took the tests four times a day with few omissions, for four months. The first seventeen days the girl made a great improvement, the time required changing from 43 to 30 seconds. During the fourth month the results of the tests varied little. This seems to point to the same fact that has been mentioned, namely, that at first practice brings rapid improvement in speed or rapidity of movement, but that progress decreases as the limit of speed is approached.

The tests with the boy were incomplete, but the thing of greatest note was the effect of interest in his work. When that lagged, the time required for the test was correspondingly increased. For instance, the time record after a month's practice suddenly dropped from the lowest record—106 seconds to 185 seconds, which was 15 seconds higher than the initial record.

Editor's Comment.—This test seems to be a pretty good one for establishing a norm for children of each age and grade, considerable variations from which would indicate the possession of exceptional characteristics in general. This was particularly true for younger children, the backward ones always being slow in this exercise.

As in all other tests, however, special practice quickly makes greater changes than years of development and general practice in perception and movement. This accounts also for the fact that the greatest improvement is from the first to second grade where the children are becoming familiar with numbers and with the manipulation of a pencil.

It is interesting to note that we have here indications that the first repetitions have more effect than later ones even when they are a year apart.

It is also interesting to note in this and several other studies of this series that figures based on changes in the same children from year to year are of the same general character at different periods as those that have been inferred from determining the difference between different children of all ages. The agreement is not, however, complete and the figures based on the changes in the same children at different ages are undoubtedly the more significant when the data are reliable, a few cases being equal in significance to many upon the usual basis.

THESIS IV

The Development of the Artistic Sense

By Grace L. Seaver

Editor's Explanation.—Data for the study of individual progress in this and the two following theses were secured by taking samples of the children's best work twice a year about a month after school began and a month before it closed. These specimens were deposited in a pasteboard box upon which was the individual child's name. The children knew of these boxes and tried to have as good a specimen of their work as possible to put in them. The covers or portfolios were made by folding a piece of drawing paper to enclose the other work. The pupil placed on the outside of this portfolio his name, grade, the date and whatever decorations he chose. The " designs " on these portfolios constituted the data upon which this study of drawing and artistic development was made.

Thesis.—There is, in the school where these drawings were done, a system by means of which a sample of each child's work in all the departments is placed semi-annually in portfolios which the children make for this purpose. Thus in many cases it is possible to study the cover designs drawn by an individual child at the age of six, six and a half, seven, seven and a half, and so on until he is thirteen or fourteen years of age. In many cases the series were, through various causes, incomplete, but after eliminating those not worth considering, there were left the portfolios of one hundred and thirteen children, fifty-three boys and sixty girls. From these drawings and designs it has been possible to extract some general and particular truths regarding the development of the artistic sense of the child.

It must be understood that in this work the children had no help or suggestions from the teacher, and relied wholly upon their own ingenuity in decorating the covers of their portfolios. They also had perfect freedom in their choice of materials and models for their designs.

A description of a few of these portfolios may help to give a better idea of the problem of development in drawing as it was presented to me.

The first cover I examined had at the top a border of squares colored with red and blue crayons. The next had lines across the corners, forming triangles which were filled in with the bright red crayon. Still another had an inch border of bright orange on all four sides of the paper. This I found to be a typical form of decoration, especially with flat washes of paint. The colors were in many cases very crude and combined without any regard to harmony.

Sometimes a portfolio would be completely covered with scrolls, flowers, etc., with no attempt at design. One had a fanciful border of stars. Many had the word "Portfolio" at the top, the child's name at the bottom and some drawing in the middle of the page. I found the following things represented: a bunch of grapes, birds of various kinds, a foot-ball, flags, blackboard with arithmetic examples, a squash, a pine-tree, pictures which illustrated stories and other equally diverse objects.

These drawings were studied from a three-fold standpoint, (I.) that of color, (II.) design or form, (III.) arrangement and general artistic effect.

Regarding color, the generalizations were based upon data as to brilliancy, particular tone used, combinations of color, and choice of neutral tints through the medium of ink, pencil or brush. Under form there were four typical divisions, objects from nature, geometric forms, those associated with some activity or recent experience, and printing or lettering.

The last set of statistics in regard to arrangement were put on the basis of comparative rank or degree of excellence. The letters A, B and C stand respectively for good, fair and poor, A meaning good, and C poor.

I first made a list of the names of all the children. The space after each name contained divisions for all the half-years between the ages of six and thirteen, inclusive. In each division I noted the color, design and rank of the child's drawing for that particular month (the tests being taken every June and October.) This made it easy to obtain the averages for the whole, and also to make studies of the development of individual children.

I

With both boys and girls a marked preference for bright colors is shown in the lower grades, which decreases steadily as the child becomes older.

It is noticeable that with the boys the use of bright colors remains at about the same per cent. until October of the ninth year. (It should be stated, perhaps, that in obtaining these per cents, five tenths of a number, or over five tenths, was reckoned as an additional per cent.) The work of the previous June showed that over one half the colors used by the boys were bright, that is, not grayed or softened in any way, while in October of the same year only three tenths of the colors were bright.

A corresponding decrease in the per cent. of girls who made use of bright colors, also appeared in the ninth year. The decrease for the nine-year-old girls from June to October was twelve per cent., while for the boys of the same age there was a decrease of twenty per cent., in the use of bright colors.

About the same number of colors was used by both sexes, but the girls chose more as they grew older, while the boys used more in the four earlier years. This confirms the theory that the attention of girls is drawn more to color as they begin to think of matters of dress, while as the boys grew older they left the color for pen and ink work and printing.

Up to October of the ninth year, the data, for all the half-years showed that in every case more girls used bright colors than hues. Commencing with that October, however, the reverse was true for all the following half-years. More girls grayed their colors, using tints and shades, and securing more artistic effects. This was not true of the boys, for only in the eleventh and thirteenth years, did the majority of the boys use grayed tones rather than brilliant colors.

As to particular colors used, both boys and girls seemed to prefer red and green, using these colors not only for flowers, autumn leaves, and sprays of berries, but also in their original drawings. Blue came third in the list of those most used by the boys, then yellow and orange, while violet was the least popular.

The colors chosen by the girls, in order of preference, are as follows: green, red, yellow, blue, violet and orange.

For the boys, the highest per cent. for the choice of red came in the ninth year, for green in the seventh and eighth, for blue in the eighth, while the six-year-olds showed the highest per cent. for violet. As this last-named color was so little used by the children, I did not attribute the choice of it by those in the first grade to any particular liking for that color. Probably the high average was rather due to the promiscuous use of all the colors in their crayon boxes, as most of the children were not satisfied unless they used them all. The change which is brought about in this direction as the child progressed through the grades, is clearly shown by a study of the development of individuals.

In the lowest grades the girls used red, green and violet more frequently than any other colors. As they grew older, blue came to be a favorite, then yellow, and finally, in the twelfth year, orange took the lead.

As the portfolios were made twice a year, it was possible to note the development from October to June, and also the effect of the summer vacation upon the child's ability to draw.

In June more bright colors and more hues were used by the children of all grades than in October.

In all cases the neutrals were more often found in the fall portfolios than in those made in the spring. A possible explanation of this might be that after vacation the children do not have as many ideas of designs to be worked out in color. After having used crayons and paints more or less during the year they are more ready to apply color to their cover designs in June. At the ages of twelve and thirteen, where the pupils had more decided preferences and could remember better how they had used their materials, they did more color work in October than they did in June.

The per cents for the use of neutrals (ink, gray, black and white, etc.) increase at a fairly uniform rate, until, in the thirteenth year, seventy-five per cent. of all the colors used by both sexes are neutral tints. This is partly explained by the fact that in the higher grades the children turned much more to the use of lettering. They seemed to develop a sense of the fitness of things, and decorated their covers with appropriate designs, and with printing, instead of the various objects which were characteristic of their earlier years.

With the boys, the use of neutral mediums had been steadily increasing up to the ninth year, but then, in October, the average showed a jump of from thirty-five to fifty-six per cent. The per cents then continuel to increase until, at the age of thirteen, we find all the boys using these materials in preference to color.

Girls, as well as boys, selected neutral mediums in the upper grades, though as has been stated, the girls still clung to the use of color, while the boys dropped it somewhat as they grew older.

II

Regarding the objects represented in the decoration of the portfolios, the curves for both sexes showed, as the child progressed through the grades, a decided increase in the use of models from nature. In studying the statistics I found that the October of the ninth year, which was mentioned before, marked a drop in the per cent. of nature forms drawn by the boys. It might be inferred that this caused the decrease in the use of brilliant colors. After the drop the average per cent. remained about the same in both cases, never returning to the higher figure. The girls also used nature forms less after they reached the ninth year. There was, perhaps, a little higher average for the use of nature specimens in June than in October.

Geometric forms, such as squares, circles and triangles, appeared in nearly half of the boys' portfolios in the two lowest grades, while in the eighth grade, none were used. This choice in the first years of school-life was probably due to the daily use of cardboard forms for "busy work."

In general, not as many girls as boys made use of the circles, squares and other precise forms, but the per cent. of those who did use them decreased at about the same rate for both sexes. One of the girls' papers showed a pretty arrangement of diamonds and circles in a border effect.

Decorations and designs made up from drawings of things associated with the child's life and work were a study in themselves. They varied in the different grades from houses and steam-engines in the lower, to Greek frets and lotus-flower borders, in the higher classes. This style of design gives more scope for originality, and I found that the children took the associated objects to draw from more as they grew older, until at the age of thirteen nearly half the boys and a correspondingly large per cent. of the girls chose this method of decoration. Through all the grades, the girls drew more associated objects than did the boys.

The October designs showed more of this kind of work. Sometimes they were related to activities of the summer, such as games, boating, seashore amusements and the like. In June I noticed flags, wreaths and other decorations connected with Memorial Day. But even this tendency did not bring the average for associated objects up as high as it was in October.

For the boys, the highest per cents for the lettering were in June of the eleventh and thirteenth years, but in October of the eleventh year there was a decrease of nearly forty per cent., and in October of the thirteenth year there was a decrease of thirty-four per cent. In fact, the curve for the lettering was very irregular.

The girl's portfolios for the last two years showed that very many of them selected lettering in preference to any other form of decoration. Indeed, in the thirteenth year three fourths of all the children printed on their covers. One was very neatly done in a beautiful tone of brown, with a fine line of bright orange around the letters in the words "Portfolio of My Best Work." Below were painted two books and at the very bottom the girl's initials in a monogram.

III

The rank for both boys and girls grew steadily higher till at the age of thirteen very few were marked C. The choice of materials and subjects probably had some effect upon the rank, as most of the printing was excellent, while the flowers so often drawn on the covers by the lower grade pupils were not in many cases deserving of any higher mark than C.

However, the average rank did not increase in excellence as regularly as one would expect from the growing power of drawing which proper training in the grades should bring. I attributed this variation in rank somewhat to a fact which was corroborated by the individual studies, namely, that each half-year many of the children attempted something new which was as hard for them as that which they had done the year before. If at every test they had tried the same thing, doubtless the rank would have been bettered accordingly.

One thing is to be noted in the per cents of those whose covers ranked excellent. In every case, with the exception of the six-year-olds, the per cent. of boys marked A was higher in June than in either the previous or the following October. For the girls, the per cent. marked A was higher in the fall for four different years. This uniform change in rank from spring to fall would seem to indicate an increase of ability during the school year and a loss of it during the summer. Taken all together the boys received better marks than the girls.

One significant fact was noticeable throughout the work. Both sexes show much greater similarity of choice in the earlier years than they do later on in their school-life. In the higher grades the individuality becomes more marked, and there is a greater diversity of selection.

Another fact is perhaps worthy of notice. The age of nine, for both sexes, marks a change in the per cents in several particulars. This may be due to the subjects included in the drawing-course at this time, or may be due to the natural development of the child at this period.

Some of the general truths brought out by a study of these covers might be applied to the teaching of drawing in the grades.

Interest is an important factor in drawing. Many principles which are sometimes taught in abstract ways, could be made more instructive if presented in connection with things associated with the child's life and pastimes.

Children have a natural love for bright colors, but during the early years a child needs training in color perception, more especially regarding combinations of color; they may also be led to prefer the softer tones to the very brilliant colors.

They should be given only one or two colors, to use at one time, thus avoiding poor combinations. The use and effectiveness of neutrals may be taught early by giving the pupil one bright color to be combined with black, white or gray.

Pupils in the higher grades should be led to an appreciation of the possibilities of color, and encouraged to choose this medium for original work, as well as the pen and ink or pencil. The printing

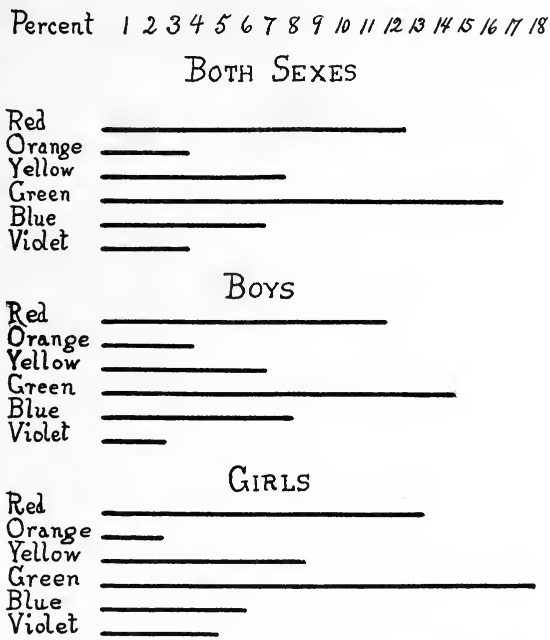

Fig. 4. Line Chart showing Per Cent. of Various Colors used in all the Designs taken together.

may be made very effective in color and with training the child may learn to delight in its use.

Little children may learn how to make border and surface designs by the use of common everyday forms. This later shows its influence and effect on the space divisions and rhythms of the original designs taken up in the higher grades.

A review in September of the principles of drawing taught the year before is necessary as the children do poorer work and apply their knowledge to less advantage in October than in June.

TABLE V

Colors and Designs on Portfolios

Data. Girls

Age in Years	6		7		8		9		10		11		12		13	
Time of Test	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.
Total number of girls,	17	27	40	40	45	45	51	48	46	42	32	27	19	10	8	7
Colors																
Total number of colors used,	34	45	67	69	84	66	66	74	57	44	35	31	19	17	8	8
Number bright colors,	23	26	40	39	47	24	27	14	14	9	7	2	0	0	1	2
Per cent.,	68	58	60	57	56	36	41	19	25	20	20	7			13	25
Number of hues,	5	9	17	21	25	14	18	27	20	12	18	6	1	4	0	0
Per cent.,	15	20	25	30	30	21	27	36	35	27	51	19	5	24		
Number neutrals,	6	10	10	9	12	28	21	33	23	23	10	23	18	13	7	6
Per cent.,	18	22	15	13	14	42	32	45	40	52	28	74	95	76	88	75
Objects																
Total number of objects used,	17	27	40	40	45	45	51	48	46	42	32	27	19	10	8	7
From nature,	9	13	25	21	16	14	20	16	10	7	13	7	2	2	1	0
Per cent.,	53	48	63	53	36	31	39	33	22	17	41	26	11	20	13	
Geometric forms,	6	9	5	4	10	3	9	4	3	1	1	1	3	0	0	0
Per cent.,	35	33	13	10	22	7	18	8	7	2	3	4	16			
Associated,	1	5	5	9	9	12	9	13	17	13	9	14	3	3	1	2
Per cent.,	6	19	13	23	20	27	18	27	37	31	28	52	16	30	13	28
Lettering,	1	0	5	6	10	16	13	15	16	21	9	5	11	5	6	5
Per cent.,	6		13	15	22	36	25	31	35	50	28	19	58	50	75	71
Rank																
Number ranked A,	2	4	12	6	13	5	12	15	20	20	19	14	15	3	7	7
Per cent. ranked A,	12	15	30	15	29	11	24	31	43	48	59	52	79	30	88	100
Number ranked B,	7	9	9	5	11	15	27	13	9	9	6	9	2	4	1	0
Per cent. ranked B,	41	33	23	13	24	33	53	27	20	21	19	33	11	40	13	
Number ranked, C,	8	14	19	29	21	25	12	20	17	13	7	4	2	3	0	0
Per cent, ranked C,	47	51	48	73	47	56	24	42	37	31	22	15	11	30		

TABLE VI

COLORS AND DESIGNS ON PORTFOLIOS

Data. Boys

Age in Years	6		7		8		9		10		11		12		13	
Time of Test	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.	June	Oct.
Total number of boys,	7	13	29	28	37	40	41	42	43	35	33	31	21	16	13	7
Colors																
Total number of colors used,	13	24	60	63	69	57	57	55	57	52	34	34	22	19	13	8
Number bright colors,	8	15	42	39	40	33	30	18	19	12	4	2	2	5	0	0
Per cent.,	62	63	70	62	58	58	53	33	33	23	12	6	9	26		
Number of hues,	5	5	7	9	20	6	7	6	7	4	11	6	1	1	0	2
Per cent.,	38	21	12	14	29	11	12	11	12	8	32	18	5	7		25
Number neutrals,	0	4	11	15	9	18	20	31	31	36	19	26	19	13	13	6
Per cent.,		17	18	24	13	32	35	56	54	69	56	76	86	68	100	75
Objects																
Total number of objects used,	7	13	29	28	37	40	41	42	43	35	33	31	21	16	13	7
From nature,	4	8	18	14	15	15	14	6	7	3	7	7	0	3	0	1
Per cent.,	57	62	62	50	41	38	34	15	16	9	21	23		19		14
Geometric forms,	3	3	5	9	4	3	4	6	9	5	1	5	3	0	0	0
Per cent.,	43	23	17	32	10	8	10	15	21	14	3	16	14			
Associated,	0	2	3	1	11	9	11	7	8	7	3	10	6	5	3	3
Per cent.,		15	10	4	30	23	26	17	19	20	9	32	29	31	23	43
Lettering,	0	0	3	4	7	13	12	23	19	20	22	9	12	8	10	3
Per cent.,			10	14	19	33	29	55	44	57	67	29	57	50	77	43
Rank																
Number ranked A,	0	4	11	5	9	8	12	10	22	17	20	16	15	10	13	6
Per cent. ranked A,		31	38	18	24	20	29	24	51	47	61	52	71	63	100	86
Number ranked B,	3	2	5	6	9	8	11	15	9	9	8	11	4	4	0	0
Per cent. ranked B,	43	15	17	21	24	20	26	36	21	27	24	35	19	25		
Number ranked C,	4	7	13	17	19	24	18	17	12	9	5	4	2	2	0	1
Per cent. ranked C,	57	54	45	61	51	60	44	40	28	27	15	13	10	13		14

Editor's Comment.—The discussion of the above data is so complete and clear that further explanation is unnecessary. It is worth while, however, to emphasize the value of such data of which as yet little has been collected or published. The children being entirely free from the constraint of authority, but with a motive to do their best, chose and executed according to their own ideas, as influenced of course by training, special events, and the example of companions. Since the tables are based largely upon the work of the same children from year to year they are good indications of the way in which the artistic sense and ability develop in individual children under the influences to which those children were subjected. The development of a science of education would be greatly helped by giving pupils an opportunity and motive to freely do work according to their own ideas in the various lines of school work, and preserving such work year after year as data for determining just how children do develop in interest and effective power under the influence of a given school system and the social conditions of the locality. Boxes for the preservation and alphabetical filing of such records can be made at an expense of not over five dollars per hundred, and if the school population is stable, the labor of filing the papers is not excessive. In our own schools the labor of keeping the files was great in proportion to the number of complete papers obtained, because pupils frequently changed from our district to other parts of the city. If such records were kept, there are plenty of specialists who would be glad to work up the data.

THESIS V

Development of Penmanship

By Grace Emogene Stockwell

Thesis.—My interest in this study lies in the importance of legible writing as a means of expression. My aim is to find the changes that occur from year to year during school life.

Through the statistics and facts I have gained I hope to present the changes that occur, both general and individual, and yearly and half yearly. In gathering these records, I had an excellent opportunity, for the children of the Edgerly school select specimens of their best work which they would like to have preserved, each October, one month after school has begun, and each June—one month before school closes. Thus in many cases, there are complete sets of each child's writing selected at half yearly periods from the second through the eighth grades.

I classified the writing of the pupils as to general appearance, slant regularity, neatness, and individual letters, grading them as excellent, good, poor or very poor.

In all there were ninety-eight sets of which the greater number were complete from October, 1902, to October, 1906, thus giving four June records and four October records. A few were incomplete because the child was absent at the time of selection; others because the child did not enter at the lowest grade or left before he reached the higher grades.

The statistics gathered from these records are reasonably accurate. The fact that the records were not of special writing lessons but of language or spelling work, and that the child himself chose what he considered a specimen of his best work, makes them of special value.

It must be taken into consideration that the work was done under conditions impossible to render exactly similar, and this is the cause of any slight inaccuracy that may appear in the records.

A great deal depends upon the teacher, the special teacher for each different year undoubtedly caused a difference in the strength of the motive to do good work.

The first comparison was made of the records from October to June embracing the period of time spent by the children in the school.

The results show that in general appearance and regularity, the

TABLE VII

CHANGES IN WRITING. GENERAL COMPARISONS

		Oct. to June			June to Oct.			Oct. to Oct.			June to June		
		Improved	Same	Lost	Improved	Same	Lost	Improved	Same	Lost	Improved	Same	Lost
General appearance	Number	145	133	31	63	137	45	107	106	26	99	91	30
	Per cent.	46	43	10	25	55	18	45	43	10	45	41	13
Slant	Number	90	167	52	49	153	43	85	105	51	81	108	31
	Per cent.	29	54	17	20	62	17	35	43	21	36	49	14
Regularity	Number	150	123	36	66	125	54	105	98	38	105	86	34
	Per cent.	48	39	11	26	50	22	43	40	15	45	39	15
Neatness	Number	131	143	35	69	119	57	109	96	36	85	111	24
	Per cent.	42	46	11	28	48	23	45	39	14	38	50	10
Individual letters	Number	116	152	41	63	134	48	101	103	37	92	93	35
	Per cent.	37	49	13	25	54	19	41	42	15	41	42	15

greatest number improve, a second class remain the same, while a very few lose. In the slant of the writing much the greatest number retain the same slant, about one half of the rest improve, that is, bring the slope of their letters nearer a recognized form, while nearly as many lose, that is, fall away from the standard slope, sometimes sloping their letters both right and left. In neatness and formation of individual letters, the greatest number remained the same, nearly as many improved, while a few lost.

The next comparison was from June to October, the period of time spent mostly in summer vacation.

These results show that in every respect, in general appearance, slant, regularity, neatness, and individual letters, there is a standstill, more than one half remaining the same, while of the rest few more gain than lose.

I then made yearly comparisons from October to October and June to June. In the October to October comparisons, it must be noted that the children received their practice in writing before the summer vacation and that the specimens of writing were obtained after the summer vacation.

In general appearance the greatest number improved, a close second remained the same, a very few lost.

In slant the greatest number remained the same, nearly as many improved, a few lost.

In regularity the greatest number improved, nearly as many remained the same, a few lost.

In neatness a large proportion improved, nearly as many remained the same, a very few lost.

As to individual letters nearly equal numbers improved and remained the same, a few lost.

The next comparison was of the June to June records. In this case the summer vacation came before the practice and the specimens of writing were selected after a year of work.

This time in general appearance and regularity the greatest number improved, those who remained the same came a close second and a few lost.

In slant and neatness the greatest number remained the same, nearly as many improved and a few lost.

In individual letters nearly equal numbers improved and remained the same and a few lost.

I then made individual comparisons from year to year, classing the girls and boys separately. These comparisons noted whether the writer remained constant or lost and the age at which he remained constant or lost.

TABLE VIII

CHANGES IN WRITING

GIRLS' INDIVIDUAL YEARLY COMPARISONS

Constant

	Age	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14
Number of comparisons		8	22	40	65	78	88	84	80	79	51	41	29	24	10	8
General appearance	Number		1	1	2	6	10	9	11	14	16	9	5	1	1	
	Per cent.		4	2	3	7	11	10	13	17	31	21	17	4	10	
Slant	Number				3	3	10	14	15	7	6	5	5	2	1	
	Per cent.				4	3	11	16	18	8	11	12	17	8	10	
Regularity	Number		1	1	2	8	6	12	13	11	14	7	6	2	1	
	Per cent.		4	2	3	10	6	14	16	13	27	17	20	8	10	
Neatness	Number			2	3	4	2	9	13	16	15	16	5	5	1	
	Per cent.			5	4	5	2	10	16	20	29	39	17	20	10	
Individual letters	Number	1	1	3	3	6	4	12	10	15	16	10	4		1	
	Per cent.	12	4	7	4	7	4	14	12	19	31	24	13		10	

Negative

	Age	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14
Number of comparisons		8	22	40	65	78	88	84	80	79	51	41	29	24	10	8
General appearance	Number			2	2			1	1	5	5	2	2	1		
	Per cent.			5	3			1	1	6	9	4	6	4		
Slant	Number		1	1	3	14	11	10	12	14	7	4	3	1		
	Per cent.		4	2	4	17	12	11	15	17	13	9	10	4		
Regularity	Number			1	1	2	4	2	1	8	6	2	3	1		
	Per cent.			2	1	2	4	2	1	10	11	4	10	4		
Neatness	Number				2	1	2	5	1	5	3		1			
	Per cent.				3	1	2	5	1	6	5		3			
Individual letters	Number				1	1	4	2	1	3	7	4	1			
	Per cent.				1	1	4	2	1	3	13	9	3			

In the girls' comparisons I found the following results: First, those who remained constant. In general appearance of the writing the greatest per cent. remained constant at eleven and one half years. The standstill began at nine and one half and lasted until twelve and one half.

In slant the highest per cent. remained constant at twelve years. The standstill began at nine and one half years and lasted until twelve and one half.

In regularity the greatest per cent. came to a standstill at eleven and one half years. The standstill began at ten and lasted until twelve and one half.

In neatness the standstill began at ten and lasted until twelve and one half while the highest per cent. came at the age of twelve.

So it can be seen that in general, in the case of the girls, the majority stop their development of writing at about nine and one

TABLE IX

CHANGES IN WRITING

BOYS' INDIVIDUAL YEARLY COMPARISONS

Constant

	Age	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14
Number of comparisons		8	22	40	65	78	88	84	80	79	51	41	29	24	10	8
General appearance	Number	1	1	1	6	10	6	9	11	10	9	7	9	7	3	
	Per cent.	12	4	2	9	12	6	10	13	12	17	17	31	29	30	
Slant	Number			2	4	7	10	9	13	5	5	11	6	8	3	5
	Per cent.			5	6	8	11	10	16	6	9	26	20	33	30	62
Regularity	Number	2	2		6	10	6	10	7	7	6	6	6	3	4	
	Per cent.	25	9		9	12	6	11	8	8	11	14	20	12	40	
Neatness	Number		1		3	6	7	11	14	11	9	13	10	5	3	
	Per cent.		4		4	7	7	13	17	13	12	31	30	20	30	
Individual letters	Number	2	2	1	6	10	6	9	10	13	6	9	5	3	4	1
	Per cent.	25	9	2	9	12	6	10	12	16	11	21	17	12	40	12

Negative

	Age	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14
Number of comparisons		8	22	40	65	78	88	84	80	79	51	41	29	24	10	8
General appearance	Number			1		1	2	1	4	6	1	6	2	3	1	
	Per cent.			2		1	2	1	5	7	1	14	6	12	10	
Slant	Number	1	2	2	5	8	9	9	7	4	6	4	4	1	1	
	Per cent.	12	9	5	7	10	10	10	16	5	11	9	13	4	10	
Regularity	Number			1		3	5	4	5	7	2	2	5	3		
	Per cent.			2		3	5	4	6	8	3	4	17	12		
Neatness	Number		1	1		2	5	4	4	3	5	1	4	4		
	Per cent.		4	2		2	5	4	5	3	9	2	3	16		
Individual letters	Number			1		1	4	3	3	4	4	2	3	4	2	
	Per cent.			2		1	4	3	3	5	7	4	10	16	20	

half years of age; the number of those who stop developing gradually increasing until the ages of eleven and one half and twelve when the per cent. is largest, then decreasing in number until the standstill is virtually ended at twelve and one half or thirteen.

For those girls who lost, the highest per cents came at these ages—in general appearance at eleven and one half, the losses coming between eight and twelve and one half; in slant—at nine and eleven, the losses coming between nine and twelve and one half; in regularity at eleven and one half, the losses coming between eleven and twelve and one half; in neatness at eleven, the losses coming between ten and eleven and one half; in individual letters at eleven and one half, the losses coming between nine and one half and twelve.

In general the greatest per cent. of girls lost at the ages of eleven and eleven and one half, the losses coming between the ages of nine and twelve and one half.

So it can be seen that between the ages of nine and twelve and one half girls either lose or come to a standstill in the development of writing.

Comparing the boys' records with those of the girls, I find that while a large per cent. of both either lose or come to a standstill at about nine years of age, that period is ended with the girls at about twelve and one half, and improvement begins again while the boys continue longer in their standstill or losses. Moreover the per cent. of girls who lose or remain the same is much smaller than the per cent. of boys.

Probably this is due partly to the fact that girls are naturally more painstaking in their work than the average boy; also to the finer coordination in the girls' muscles than in the boys. But this subject will be taken up later.

The fact that after the age of twelve and one half the girls again show signs of improvement while the boys continue to lose or remain the same is explained in this way; as girls grow older they write well because good writing is asked for and praised while boys follow their other interests and cease in their efforts for improvement.

In order to understand fully the difficulties a child has to overcome, and the full significance of his development of this process, it is necessary to go back to fundamental principles.

Writing involves complex muscular movements. All the different factors fit each other perfectly. Children's nerve centers are far from perfectly developed, so it follows that the movements of the muscles do not cooperate perfectly, and we who have not known this have sometimes wondered why children can not seem to direct their movements in writing.

Writing must be developed by trial after trial with the mind concentrated upon the result obtained and not upon the movement itself.

It should be realized that the development of coordinated movements is the important point in teaching a child to write, and not merely some particular method.

At first the child's movement is not easy. His attention is not free for he has to study the form of the letters. His movements are cramped and jerky and lack organization. This irregularity refers back to lack of organization in the brain.

That a regular coordinated movement of the muscles is necessary for good writing is shown by the fact that after the summer vacation, during which these movements have ceased, there is comparatively far less improvement than after the year of work and practice. But it is wonderful that loss after vacation is not greater than it is, for the majority hold their own and more improve than lose.

We may explain this perhaps by the fact that the rest and the recreation of a vacation gives new vigor to the wearied muscles even as the lack of coordinated movements takes away from their regularity.

Editor's Comment.—Educationally these results are important in that they confirm the common opinion of teachers that after a certain age children usually cease to improve and perhaps deteriorate in their writing, and emphasize the importance of determining just why this is. In my opinion, school methods are largely, but not wholly, responsible. In the early stage of writing only are the children learning the visual forms of letters, while in the later stages they are forming motor habits. Unfortunately teachers have failed to recognize clearly these two phases of learning to write, and they have also failed to realize that a habit formed under these conditions of copybook practice will almost surely not carry over and function under the condition of expressing thought while writing.

On the theoretical side the data are interesting as indicating what is also suggested by several other studies of this series, *i. e.*, that there is a decrease or increase in the rate of development in various forms of physiological and mental functioning correlated more or less closely with the rate of growth of boys and girls in height and weight. Perhaps, as claimed by C. W. Crampton in a recent number of the Psychological Clinic, these changes are not dependent upon growth, but like growth are indications of the degree of physiological maturity in relation to the attainment of pubescence.

THESIS VI

The Development of Language

By Elizabeth S. Smith

The original papers upon which this study was based were not sufficiently numerous and uniform to permit the drawing of general conclusions of value regarding sentence structure, length of sentence, etc. Hence, they are not printed.

THESIS VII

Characteristics of Children as Viewed by Teachers

By Mabel Josephine Spalter

Editor's Explanation.—This thesis is based upon reports of teachers, in training, regarding the conduct, ability, success in studies and most marked characteristics, of individual children.

Thesis.—My main thought in this thesis is to find out to what extent different teachers judge the same children in the same way, and, where a difference of opinion occurs, to what it is due.

Under conduct, I used good, fair, and poor, as the three heads under which to correlate the one hundred and eighty-three papers. There were in the majority of cases from three to six reports concerning each child, so my standard of "complete correlation" was that every teacher had judged the child in the same way; and of "incomplete correlation" that some difference in the opinion of the teachers was shown, as where, perhaps, two reports out of three agreed, or three reports out of four or five.

The reports upon "Means of Influence" were very hard to classify, owing to the many different ways in which teachers seek to influence their pupils. Praise and affection were used most frequently by teachers.

It was especially hard to classify the characteristics of children.

The most prominent characteristics noticed with the boys were "self control," "an interest in work," "a desire to learn," "restlessness," "stubbornness" and "pleasantness," while among the girls "shyness," "willingness" and "inattention" prevailed. The following are typical individual records.

B. F.

In the first grade this boy seemed shy and sneaky, proved himself untrustworthy and would not try to do well.

In the second grade he still continued to be mischievous, sly and lazy.

He is now in the third grade and still has the spirit of contrariness, but many times does little helpful things.

A. G.

This boy in the lower grades was inclined to be mischievous, needed a firm hand and could only be influenced by an interest in his work. Now he is doing much better, by having been made an officer in the school where self government is prominent.

E. M.

This little girl in the first grade was very slow in her work, but thought herself quite smart and always wanted to be first without any effort. She was out a great deal from school because of sickness and perhaps that partially accounts for her being slow in her work.

In the second grade she was slow in her work but always wanting to be first.

Now in the third grade she is very sensitive, and easily discouraged if reprimanded for anything, however slight.

E. F.

One teacher thought this girl silly, a giggler, but earnest in her work, while the next teacher attributed her silliness to nervousness.

M. M.

In the lower grades this girl was thought to be very lazy and idle but in the fifth grade the teacher reported her as learning very easily but lacking persistence which perhaps accounts for her seeming idleness.

L. H.

This boy was very silly and giggled incessantly. One teacher reported that this giggling had ceased but the next teacher thought environment had caused the laughter, for the lad suddenly showed a natural ability which had lain dormant and was on the alert, showing great improvement in all his work.

It would seem from these statements that the teachers appealed to different qualities which called forth various actions and responses from the children.

I compared the work of nine children who had complete correlation and three with whom there was no correlation in different subjects.

In composition work I found that only one of the nine showed a decrease in the standing of her work, and of the three the standing was variable—first a decrease, then increase, and decrease again.

In the perception motor test six out of the nine, having complete correlation, did it in a shorter time each year but the remaining three were irregular—one year it took a shorter time and then perhaps the next year a longer time. From the three with whom no correlation was found only one could shorten the time each year while the other two lengthened the time one year and shortened it the next.

In physical measurements one of the nine and one of the three showed uneven development.

In writing only one of the nine did not show progress—the others went from poor to either good or excellent. With the three, whose correlation was incomplete, progress was also shown.

In auditory and visual tests of memory all but one of the nine and one of the three showed an increase in ability to remember dictation.

From these papers I should say that the teachers do judge children, to a great extent, in the same way, and from the preceding reports I should think that they judge them quite correctly.

There is more complete correlation with boys than with girls.

I talked with the different supervisors about different children and found that a child with perfect physical growth showed complete correlation oftener than a child whose growth was backward or stunted.

TABLE X

Correlation in the Reports of Successive Teachers of the Same Children

Girls

	No. of Reports						Complete		More than ½		Less than ⅔		None		
Grades	1	2	3	4	5	6	No.	%	No.	%	No.	%	No.	%	Total
Conduct,	12	16	23	19	16	23	46	42	33	30	3	2	27	24	109
Influence,		35	8	3	2		3	6	1	2			44	91	48
Ability,	18	21	22	20	28	3	34	33	33	25	3	2	42	38	112
Good in what,	13	23	14	7	2	10	22	32	13	18	2	3	32	46	69
Poor in what,		26	19	4	5		16	29	7	12			31	57	54
Characteristic,	20	28	29	8	9	2	12	12	4	4			80	83	96

Boys

	No. of Reports						Complete		More than ½		Less than ⅔		None		
Grades	1	2	3	4	5	6	No.	%	No.	%	No.	%	No.	%	Total
Conduct,	5	4	18	27	10	10	26	35	23	31	5	6	20	27	74
Influence,		33	23	3	7	8	5	6	1	1	1	1	67	90	74
Ability,	13	25	23	2	9	1	38	52	13	17	5	6	17	23	73
Good in what,	20	24	16	13	16	3	20	21	18	17			54	58	92
Poor in what,	29	20	6	12			13	19	3	4			51	77	67
Characteristic,	17	25	14	3	6	4	11	16	7	10	2	3	49	71	69

Editor's Comment.—Data of this character, consisting in part of reports by young teachers in practice, are not well suited to statistical study; but some of the individual reports are interesting and the fact that in general children about whom the reports of teachers agree are more likely to show consistency in mental and physical tests than those whose reports vary from year to year is very significant.

THESIS VIII

The Curve of Learning

By Abbie F. Munn

Editor's Explanation.—This experiment for studying habit formation was devised by Professor Lough, of New York University, who very kindly furnished the blanks for the test. (He would be glad to have others cooperate in the same test, that norms for practice curves may be established.)

TEST SHEET

1.	T	O	P	E	S	N	I	D	R	M	H	C	Q	L	G	B	J	K	F	A
2.	P	T	K	O	F	G	A	E	S	Q	N	L	I	D	B	R	M	H	C	J
3.	C	F	L	S	B	G	A	P	K	H	M	R	D	I	N	T	E	O	J	Q
4.	J	A	N	T	B	I	Q	R	O	L	E	K	P	S	M	C	G	D	F	H
5.	E	J	O	T	D	I	N	S	C	H	M	R	B	G	Q	A	F	K	P	L
6.	T	C	E	F	A	D	K	O	Q	J	R	P	I	G	M	S	H	I	N	B
7.	G	B	M	S	A	H	N	T	C	I	O	P	D	K	Q	E	F	L	J	R
8.	H	T	I	S	R	K	Q	L	P	M	O	A	G	B	F	C	E	N	D	J
9.	D	G	J	M	P	S	A	E	H	K	O	R	B	T	F	I	L	N	Q	C
10.	K	H	O	F	A	G	M	J	P	E	B	Q	S	R	I	N	D	L	C	T

KEY SHEETS

I	II
A—X	A—O
B—U	B—I
C—F	C—G
D—L	D—N
E—Y	E—J
F—M	F—V
G—B	G—A
H—W	H—H
I—Z	I—D
J—E	J—R
K—R	K—E
L—D	L—Z
M—H	M—W
N—A	N—B
O—V	O—M
P—J	P—Y
Q—N	Q—L
R—G	R—F
S—I	S—U
T—O	T—X

The material for the test consists of (1) a test sheet with ten lines of letters in mixed order, and (2) a key sheet, in which the twenty letters used in the test sheet are arranged in a vertical column and opposite each is printed some other letter. The idea of the test is that the letters in the second column of the key sheet are to be substituted respectively for the corresponding letters in the first column. The procedure was as follows: Only one line of the test sheet was exposed to view at once. A blank sheet covered all the lines below the line in use at any moment, and the lines that had already been used were folded under and so concealed. The key sheet was kept in sight all of the time. Having before him the key sheet and the first line of the test sheet, the person tested, at a given signal, began writing beneath each letter of the test sheet the letter corresponding thereto in the key sheet; thus, writing X beneath each A of the test sheet, U beneath each B, etc. Having no previous acquaintance with the key sheet, the person tested had, at the beginning of the experiment, to refer continually to the key sheet in order to determine what letter to write beneath each letter of the test sheet.

As soon as one row of letters was written, the time in seconds was recorded, the edge of the sheet upon which they were written folded under, the second row of letters exposed, and the experiment continued. Each line of letters is called a ''trial,'' and the ten lines done at one time constitute a ''test.'' After one or more trials the subject notices that the first column on the key sheet is in alphabetical order and then knows just where to look for the required letter. After a greater or less amount of practice most of them learned also what letter was opposite each letter of the alphabetical series so that it was not necessary to look on the key sheet at all. When the learning was partly complete, a few students wasted time in trying to think what letter to write instead of looking at once on the second sheet, and thus took a longer time than when they first began; but this was not a general source of irregularity.

Directions for the experiment were given the normal school students in class and the experiments performed in their rooms. They were asked to have the conditions as nearly the same as possible and to take the time as accurately as they could with a watch. Most of them had a classmate keep the time, but a few kept it themselves. The ''standard series'' consisted of one test a day, at the same time of day, but several groups of students were asked to take more tests, and at different intervals, as is indicated in detail in the thesis. Tests of children were made under the immediate direction of Misses Lane and Munn.

In order to study interference effects, persons who had already practiced with key sheet I. were required to change to key sheet II.

Thesis.—The aim of this study was to find a standard curve of learning, the variations in this curve resulting from the different methods of taking the tests; the conditions under which most progress was attained and those where least progress was made.

As a means for this investigation, I made a study of one hundred and twenty papers, from as many individuals, showing the results of a habit formation experiment. The greater part of these papers were received from normal school students, all women; some few however were obtained from children of a third, a seventh and an eighth grade.

Each individual, with a few exceptions noted below, went through a series of twenty tests, each "test," as explained above, consisting of ten lines or "trials." In different series, the tests were differently distributed in time. The individuals participating in the various practice series, mentioned below, were in all cases different individuals.

The first group comprises individuals who took one test a day for twenty successive days. This group I call my standard or regular series, and the curve of learning resulting from these papers I call the standard or regular curve of learning.

Fig. 5 represents the results of the regular series, taken by twenty-three normal school students. On this chart are five curves, A, B, C, D, E.

Curve A represents the record made during ten trials or one test; curve B the record of the second ten trials or test two; curve C shows the records of the first trials of the twenty tests; curve D the results of the tenth trials of the twenty tests; curve E the averages of the twenty tests.

Discussion of Regular Series.

Curve A shows steady gain first half, little gain last half; former gain 6.8 seconds, latter gain 2.8 seconds—entire gain 9 seconds.

Curve B. Here the gain is more even, the first and second halves of curve varying little. The entire gain was 6.2 seconds.

Curves C and D both show great gain, first half less gain toward the end. As the practice continues, the rate of progress diminishes

Curve E is the important curve of all, for it shows the average of all the tests. The total gain made during the practice was 28 seconds, the gain first half was 21 seconds, gain second half was 7 seconds. Gain first half was three times the gain of second half.

Before leaving this set of papers it may be of interest to notice one or two of the individual papers. For this study I chose the two papers which showed the most marked contrast, one the paper of the individual who made the greatest gain, the other the paper belonging to the one whose gain was the least. Fig. 6 represents the former gains; Fig. 7 the gains of the latter.

By comparing these two curves, it is evident that the natural ability as far as quickness is concerned varied greatly. One was extremely slow at the beginning while the other was quick.

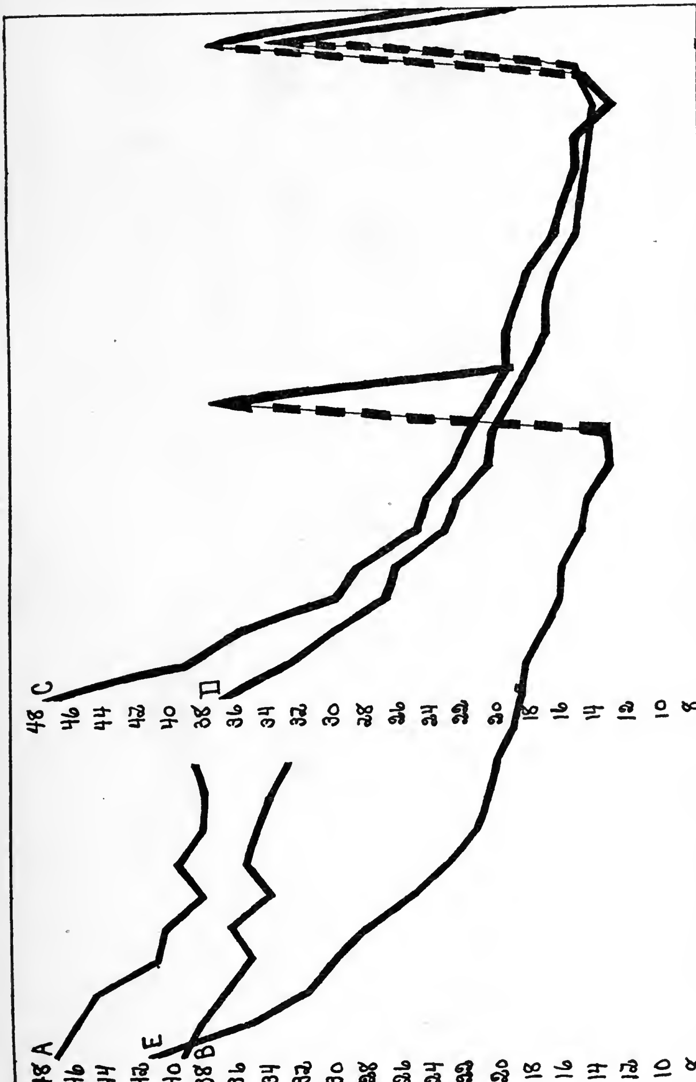

FIG. 5. "Regular Series." Twenty Trials with the A–X Key, and One with the A–O Key. Average Results of 23 Normal School Students. Curve A gives the results of the 10 successive trials composing the first "test"; curve B gives the results of the 10 successive trials composing the second test; curve C gives the results of the first trials of the 20 tests; curve D gives the results of the tenth trials of the 20 tests; curve E gives the averages of each of the 20 tests.

The subject who was the slowest in doing the tests at the beginning made more rapid and greater gains throughout the entire series than did the one whose first test was done in the least time. Fig. 6 also shows that the limit in the rate of progress had not been reached by the reagent while Fig. 7 shows the opposite to be true. The gain made by the reagent whose results are shown on Fig. 6 was 46.6 seconds,

FIG. 6. Individual Making the Greatest Gain.

3.3 times as much as that made by the other reagent whose gain was 13.2.

After the first five or six trials of a test there is usually a loss of a second or two, this loss however is frequently made up by the following trial and almost without fail before the end of the test.

The longest time taken by any individual for the first trial was 90 seconds, the shortest time was 30 seconds. For the last trial the longest time was 35 seconds, the shortest was 7 seconds.

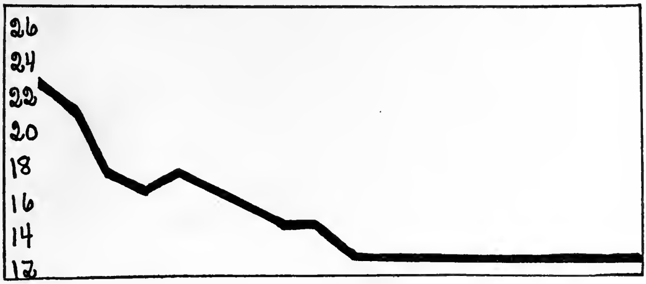

Fig. 7. Individual Making the Least Gain.

Fig. 8 represents the results of one of the special series, in which ten tests were taken one Saturday and ten on the Saturday following. These tests were taken by four normal school students.

This curve is less regular than the curves of the "regular series" shown in Fig. 5, but there are no great gains or losses, save for the one loss which is noticeable between the two periods of the exercise.

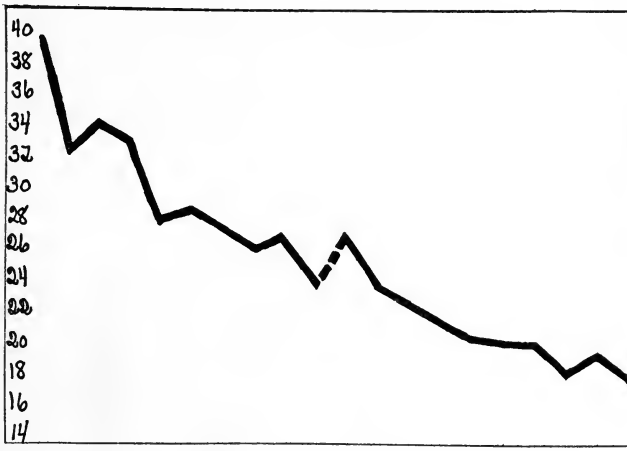

Fig. 8. Ten Tests a Day, on Two Days a Week Apart.

These losses too are not permanent for by the second test after the interval they are more than reclaimed.

From these curves too we notice that the greater gains are near the beginning of the series, that as the practice continues the gains decrease. The gains made throughout all these curves are pretty uniform. The average gain made during the entire exercise was 20.7 seconds. The gain the first half was 14.4 seconds; gain last half was 6.3 seconds.

Here we find the gain made during first half of series to be twice the gain of the second half, while in the case of the regular series the gain in the first half was three times the gain in the second half.

We find that the week's interval between the two periods of practice caused a slight set-back in the rate of speed but the loss was only temporary and easily regained.

In another experiment, a series of 17 "tests"—each consisting of 10 lines of the test sheet—was executed on the same day and without intermission between the tests. This experiment was tried on 4 normal school students. The average result is shown in Fig. 9.

FIG. 9. Seventeen Tests Without Intermission.

On comparing this curve with that of the "regular series" in Fig. 5, we find them alike in that the gains are in both instances near the beginning of the series. They are unlike in other respects. The curves of the regular series are even and gradual, while those of the continuous practice series are much more irregular.

	Gains, Fig. 5	Gains, Fig. 9
Entire gain	28	14.4
Gain in first half	21	17
Gain in second half	7	—3 (loss)

From the above comparisons it can readily be seen that the process of learning gradually counts for more than learning quickly; that short periods of practice in learning are more effective and beneficial than the long extended ones.

Fig. 10 shows the results of another series, in which five tests were taken at each of four different periods on the same day; there being two morning periods and two afternoon periods. Normal school students were the subjects of this experiment.

The curve indicates that four times as much gain was made during the first half of this series as in the last half. The gain of the first half was 17 seconds while that of the last was but 4 seconds, making the entire gain 21 seconds.

From the perusal of this chart one new significant point is gained, namely, that work in the morning is more effective than the afternoon work. If we apply the above to school work, it follows that the harder work of the day should be a part of the morning program, rather than of the afternoon one.

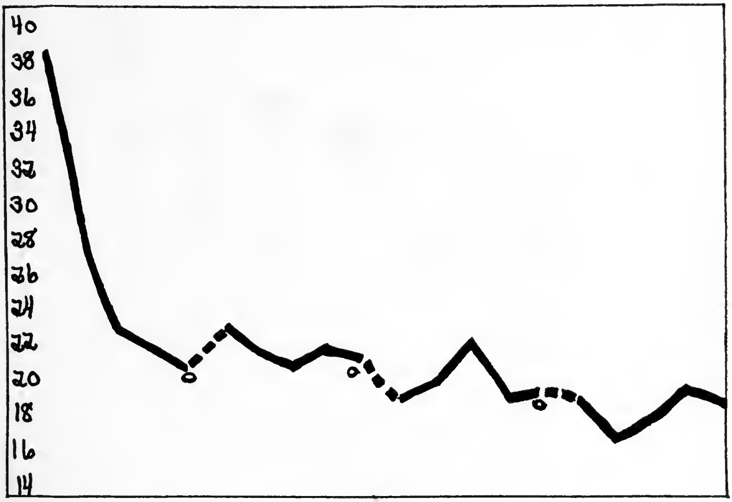

FIG. 10. Four Periods of Five Tests Each, on the Same Day.

Fig. 11 shows the results of a series of twenty tests taken in groups of five on four successive days; this was tried on 4 normal school students. Here the general character of the curves is regular. There are no losses which are not regained. These curves are more nearly like those of Fig. 5 than any previously considered.

The entire gain made was 31 seconds, gain first half was 24 seconds, gain last half was 7 seconds. The first gain was over three times last gain.

Comparing the results of Fig. 11 with those of Fig. 5, we find that the gain made by the former which was 31, was more than that made by the latter.

This shows that continuous practice periods, if not too long, are of value in that there is no time for "forgetting" to enter in.

The carrying out of this idea in the work of the lower grades would be advantageous, for the little ones easily forget if drills are not frequent.

The series in which the greatest gain was made was one in which the tests were taken twice a day, two at each period, for five successive days. Four normal school students took part in this experiment.

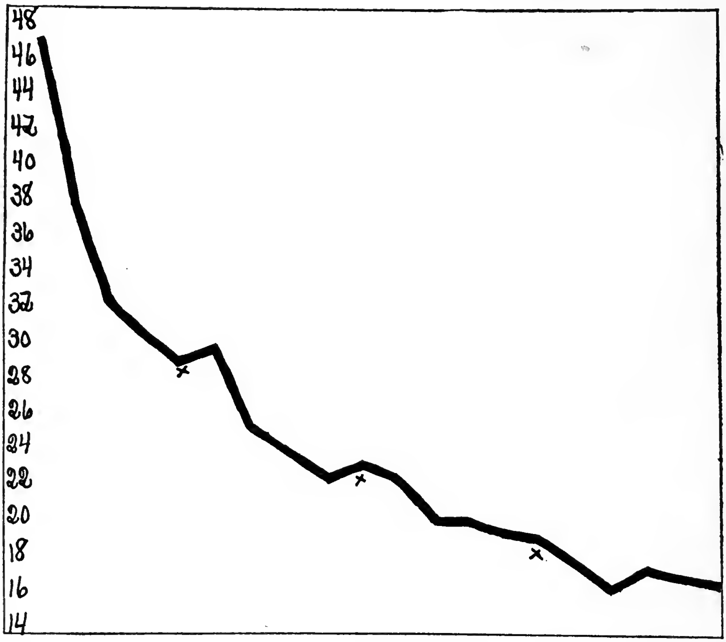

FIG. 11. Five Tests a Day on Four Successive Days. The end of each day's practice is indicated by a cross below the curve.

The results, as seen in Fig. 12 (average of the average results), show unusual uniformity up to the tenth test, from there on the curve is much less regular.

The gain made during this series of tests was 39 seconds, the gain first half was 32 seconds, the gain second half was 7 seconds, the former gain being over four times the latter gain.

Comparing the above results with the corresponding results of the regular series, we find that this special group gained more through the first half than did those who took the tests in the regular way. This however may be in part accounted for by the fact that the initial rate of speed of this special group was much slower than was the rate of speed attained in the regular tests at the beginning, thus affording more chance for gain on the part of the special group.

Taking two tests twice a day for five days appears to be more effective than taking them one a day for twenty days or five a day for four successive days.

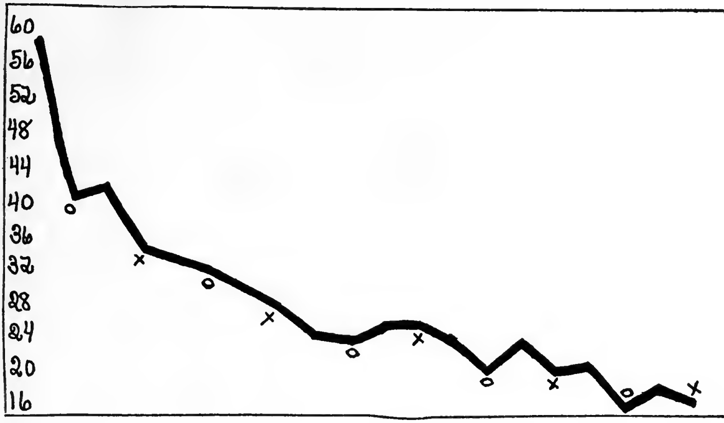

FIG. 12. Two Tests Twice Each Day, on Five Successive Days.

Beginning with Fig. 13 we have the tests taken first with the a–x key, then the a–o key, that the effect of the interference may be noticed, and the part it plays in the practice determined.

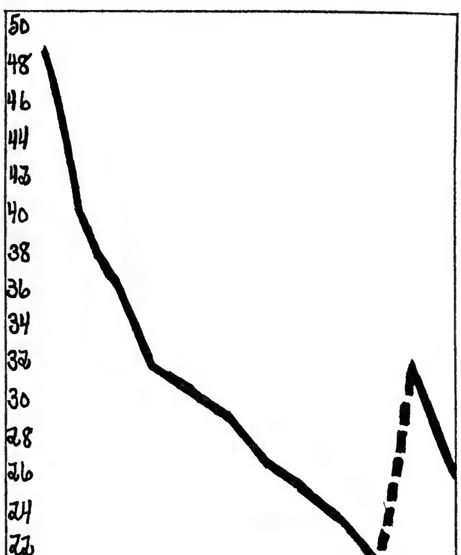

FIG. 13. Interference.

Fig. 13 shows the results of 10 a–x tests, then an interval of one week followed by one a–o test, then another a–x test. The interval of one week is represented in the curve by the broken line, and the line that follows shows the result of the a–o tests.

The practice and knowledge gained in doing the a–x series of ten tests aided much in doing the tests with the a–o key. Practice in doing or learning one thing helps in the mastery of other things of a like character.

Groups of papers were received where ten tests with the a–x key were taken followed immediately by 8 tests with the a–o key. In contrast to this group there was another set of papers of the same number where an interval of one week came between the two series.

Table showing results of papers where there was no interval between a–x and a–o series:

1st trial	a–x	43	seconds	Difference of 10 seconds
1st trial	a–o	33	"	
10th trial	a–x	21	"	" —2 "
10th trial	a–o	23	"	=8 "

Table showing results of papers where an interval of one week came between the a–x and a–o series:

1st trial	a–x	58	seconds	Difference of 21 seconds
1st trial	a–o	37	"	
10th trial	a–x	23	"	" —4 "
10th trial	a–o	27	"	=17 "

The above tables show that though the amount of gain was more where the week's interval came between the two series, the proportionate gain varied but one second. This shows that the short interval of one week had slight if any effect.[2]

So far, the results reported have been from adults. The first series with children consisted of 10 tests with the A–X key, taken, one each day in the morning, by six children from the seventh and eighth grades; the average age was 11 years, 7 months.

The gain made by the children during the 10 tests was much greater than that made by the normal school students ("regular series") in the same number of tests. The gain made by the stu-

[2] In other words, practice with the use of the A–X key decreased the time for the first trial with the A–O key by 10 seconds in one group and by 21 seconds in the other group, while the difference between the tenth trials of the A–X and the A–O series was in the first group 2 seconds and in tne second group 4 seconds—which indicates that the practice effect is proportionally the same in the two cases.

dents in the first ten tests of the regular series was 21 seconds; that made by the children in the same number of tests was 48 seconds, more than twice as much.

Gain in first five tests—adults	16 seconds
Gain in first five tests—children	34 "
Gain in second five tests—adults	5 "
Gain in second five tests—children	14 "

From the above we see that the gain of adults, in the first five tests of the "regular series," was three times as much as in the second five tests; while the gain made by the children in the first five tests was twice their gain in the second five.

The children began their tests at a much lower rate of speed than did the normal school students, the average time for the first "trials" or lines by the students being 47 seconds, and by the children 88 seconds. The best records among the children were, however, about as good as the best records among the normal school students.

Tests were also taken after school, at four o'clock, by six children from the seventh and eighth grades. The average age of these children—11 years, 7 months—was the same as the average age of the children in the preceding group, who were tested in the morning.

From the following table we find that there is an average loss of 10 seconds when the tests are taken at night:

	Tests taken A.M.	Tests taken P.M.
Gain in first half	34 seconds	25.7 seconds
Gain in second half	14 "	12.5 "
Total gain	48 "	38.2 "

This indicates that with the children, as well as with adults, the morning work is of more value.

Besides the results received from the children of the seventh and eighth grades, I also had some papers from twelve little children of a third grade. I took the tests myself with these children, taking six of the children for the tests in the morning and six children for the tests after school. The average age of these little ones was eight years.

Charts were also plotted to show these results, the chart representing the tests taken in the morning is not on exactly the same basis as others because one or two of the little children were unable to do the entire first five tests, some only doing the first three trials in the time at our disposal.

With the little ones it took some time for them to learn what they were expected to do and how to do it, but once this part was understood, their gains were rapid.

In studying Fig. 14 we find that the gains were not as gradual as they might have been, the very great gains came at the beginning but toward the end the gains were of a more equal length. We find the greatest gain to be at the first of the curve—a gain of forty seconds. As the practice continues the amount of gain decreases. The total gain made by the children taking the tests in the morning was 138.1 seconds.

During the tests taken after school an average gain of 108 seconds was made, being 30 seconds less than the morning gain.

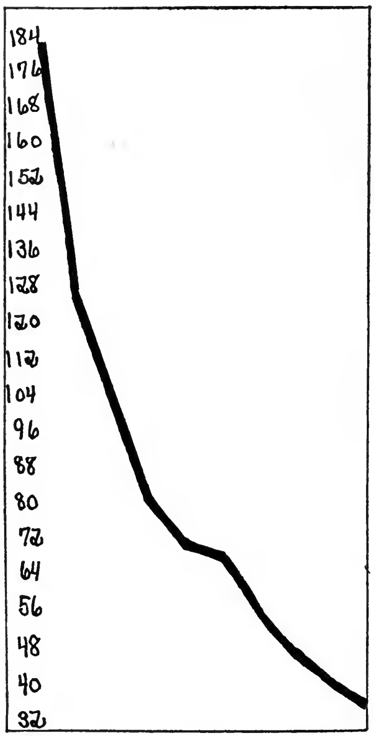

Fig. 14. Ten Tests taken A.M. on Children of Third Grade. Average results.

From studying Fig. 14 one can readily see how enormous are the gains made by the children as compared with those made by the normal school students.

If we consider some of the individual papers of the children we find that in many places there is evidence of no real gain whatever, but this period of standstill is not truly one of no gain, for after these resting periods, as we may call them, great gains are frequently made and also kept. It seems almost as though we might call these periods of assimilating, for the acceleration which follows shows that some learning must have been going on or otherwise the sudden gains would not have ensued.

It was intensely interesting to watch the little ones as they were taking the tests. They were all greatly interested in doing the exercise and were especially anxious to know the progress they were

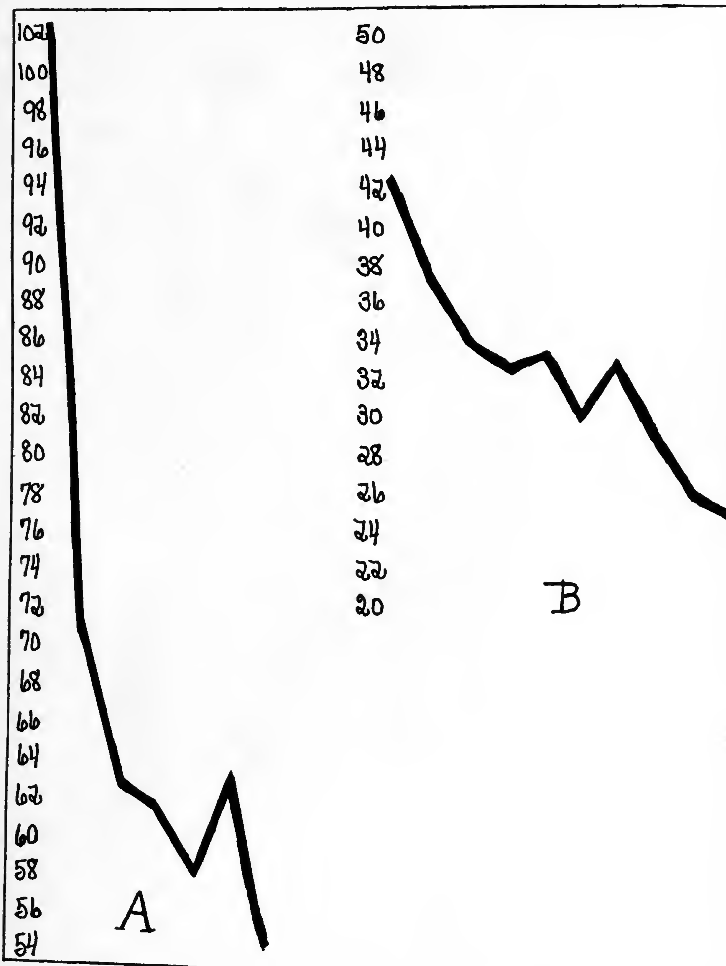

FIG. 15. Practice Curves of Elderly Persons. A shows the results obtained, in 7 tests, by an individual 72 years old; B shows the results obtained, in 10 tests, by an individual of 60 years.

making and how it compared with that made by their friends. Encouragement did much in raising the record and the trying to outdo their friends held the interest of the children and proved the best incentive to doing the work.

After an afternoon spent almost entirely in drawing, the tests were taken, and the weariness of the children influenced the rate of progress greatly. Their interest in the doing of the tests was much less than it previously had been and the gains they made interested them little. It was only with great coaxing and encouragement that they were able to be kept long enough to finish the tests.

In two instances, children having a headache could not work nearly so quickly as they had been accustomed, and one little girl who had a hard cold was unable to do more than three trials of one test, and to do this amount she took as much time as she usually would require to accomplish the ten trials.

These instances show that the physical condition of a child, especially, has much influence on his mental ability.

Having tested the normal school students and some few children, I was interested to know how tests of older people would compare with the previous tests of children and students. This study I could not carry very far, for subjects were hard to find. However, the tests were taken in the "regular" way—one test a day—by two elderly individuals, a gentleman of seventy-two years, and a woman of sixty years. Fig. 15 shows the results obtained by these two. Curve A is very similar to the corresponding curves on the charts which represent the results of the children's tests, while curve B is more nearly like the corresponding curve in Fig. 5.

After a period of five months or so, during which time no tests were taken, the subjects who had previously taken the tests were asked to try one test more, of ten trials, that it might be seen whether the learning was permanent or not, and if so to determine where it was the most so.

This was done and the following table shows the result.

From this table we find by comparing results of the first trials before the interval with the first trials after the interval that in every case save two there was a gain at the beginning of the period after the interval, showing that the knowledge gained from practice five months previous still in part remained.

In the two instances where no gain was made during first trial after the interval over first trial before interval we have good proof that the forgetting played an important part.

It is not surprising that this is the result where all the tests were taken at the same time or even where they were taken four different periods on the same day.

TABLE XI

	1st Trial	Best Av.	Last Av.	Last Trial		1st Trial	Last Trial	Av. of Test after Interval
1. General test	47.6	13.4	13.4	13.8	Interval, five months	33.6	21	28.4
2. All test, one period	53.7	21.1	31.7	37.5		60.7	33.7	40.8
3. Four different periods, same day	44	14.9	17	15		45	28	31.4
4. Ten tests, two successive Saturdays	53.5	18.2	18.2	17.7		41	29	33.7
5. Two tests, twice a day, five successive days	90.6	17.1	17.1	15		42.3	31	34.3
6. Children's tests, 7th and 8th grades, A. M.	88.8	14.8	20.6	19.3		51.2	26.2	35.5
7. Children's tests, 7th and 8th grades, P. M.	66.5	22.5	22.9	18		36	24.2	27.5
8. Children's tests, 3d grade, A. M.	23.9	24	25.9	24.5		68	33	42.8
9. Children's tests, 3d grade, P. M.	25.0	35.5	35.5	22		86.7	55.7	61.7

If we compare the averages after the interval with the last ones before the interval we may, I think, form a just estimate of how great a part the long interval played. Where all the tests were taken at one period, as well as where they were taken from different periods of the same day, it would be more accurate, it seems, to compare the best average of these two groups with the average after the interval.

Considering the groups, of the normal school students, we find the loss caused by the interval to be rather more marked where the tests were taken all at one period, and less marked where the tests were taken daily.

With the children's tests we find that where the tests were taken in the afternoon by the seventh and eighth grade children the interval caused the least effect. With the little children who took the tests in the afternoon the interval caused the greatest effect.

From observation of those taking the tests, as well as from written statements from many of them, some of the conditions which influenced progress were made apparent.

1. Physical condition of subject, most noticeable in the nervousness which followed inability to find a certain letter in quick time; headache was accountable in several instances for lack of power to work quickly.

2. Temperature of the room—if the room was warm, work was much slower than usual; if too cold the same result was noticeable.

3. Interruption of any kind barred progress. This was especially true with the children. Once their minds were off their work, it was hard for them to concentrate themselves upon it again for some time.

4. "Mind-wandering"—thinking of outside things—caused decrease in rapidity of action.

5. If subjects were in a hurry, for any reason, the work was slower than usual.

6. Weariness from school work made a great difference in the records.

7. If an unusually strong effort was put forth to do the work quickly, without fail undesired results would follow.

Brief Summary of Results

1. The greater gains in the process of learning to do something are at the first of the practice.

2. Periods of morning work are more effective than the afternoon periods.

3. Children work much slower to begin with than do adults, but the gains made by them are greater. The gains of the adults, however, are more even and uniform than those of the children.

4. Short and frequent periods of practice are more valuable than long extended ones.

Editor's Comment.—The test sheet and key used in the above experiments are reproduced above that others may use them if they so desire. The exercise has proved very serviceable, both as a means of making a simple research and for illustrating a number of truths taught in the psychology class.

As a research, while revealing little that is entirely new, it helps to confirm and emphasize, and suggests some truths that have not as yet received sufficient attention. The comparison of the learning curves of children, adults and the aged is interesting, but the questions of greatest importance raised by the study are those concerning the number of repetitions at one time and the length of intervals between practice that are most favorable to rapid and permanent learning.

To what extent an individual curve of learning and fatigue is typical of all learning by that individual is also a matter of great theoretical and practical importance.

THESIS IX

Fatigue in Habit Formation

Experiment by Marian F. Lane

The same test-sheet and key sheets which were used in the preceding study and described on p. 36 were also employed in the study of fatigue.

This discussion is too extensive to be quoted in full, and parts given alone would not be clear without considerable explanation. In general, the results are what might be expected; such as decrease in rate of improvement, or irregularity in the record where a number of tests were taken at one time and usually less rapid improvement in the afternoon than in the morning. Even in a single test there seems to be evidence of fatigue, for in the sixth to eighth trial there is usually little or no improvement, sometimes a loss. The poor record made after a drawing lesson indicates that the fatigue is largely local rather than general, and perhaps mainly motor. There is probably no actual inability to maintain the rate but decreased tendency to do so.

The experiment is a good one with which to illustrate to a class the phenomena of fatigue as well as those of learning processes and habit formation.

THESIS X

Ways of Learning Visual Forms

By May N. Hills

Thesis.—I placed before the pupils of the first, third, sixth and eighth grades and before the normal school students five meaningless figures based on geometrical forms. I asked the pupils to study the figures, but did not suggest any particular way of studying them. I allowed ten minutes. Then I took away the figures and asked the pupils to draw them. After the drawing, I asked the pupils to answer the following questions:

1. How did you learn the figures?
2. Did you move your hand or any part of your body in the shape of the outline while studying?
3. Did you associate the figures with any familiar shape or object?
4. Did you study the parts of the figures separately or try to think of words which would describe the parts?

From the first grade, of course, I received only oral answers, which could not be tabulated, but I learned much about the characteristics of little children. I marked the papers received from the third, sixth and eighth grades and normal school students as to the general appearance of the drawings they had made and then as to the perfectness of the details in the drawings. In marking the papers, I tried to keep one standard of excellence for *a*, another for *b*, and another for *c*, without regard to grade or sex. After marking all the papers, I found the per cent. receiving *a, b* and *c* respectively, as to the general appearance, and then as to the more detailed representation.

Next I found the per cent. of correspondence between the general appearance and the detail—that is, what per cent. of those who got *a* in the general, got *a* in the detail also.

My next problem was to find how pupils learn. Very often a teacher places a lesson—spelling, for instance—on the blackboard and tells the class to write each word five times or else she gives them no direction for learning. So it seemed important to try to know something of the natural ways in which children learn. For if we, as teachers, can appeal to a natural method, it saves much energy and time for both teacher and pupil.

After reading the answers for both the children and the students, I was able to make four classifications: first, those who learned by moving the hand or some part of the body in the direction of the outline of the figure to be learned; second, those who associated the figure or a part of it with some object or figure already familiar; third, those who tried to learn the figures as wholes; and fourth, those who analyzed the parts, learning only a part at a time.

According to Table XII., it is seen that as to the general appearance of the figures the normal school students received a higher mark by only a few per cent. than the pupils of the eighth grade; and also it may be noticed that the increase in ability to remember the general appearance is gradual, but with more variation between the sixth and eighth grades, 47 per cent. of the normal school students receiving *a*; 30 per cent. of the eighth grade; 27 per cent. of the sixth grade, and 26 per cent. of the third grade. In the third and eighth grades the boys did the best, but in the sixth grade the girls took the lead. This difference between the ability of the girls and boys is even more noticed as to the detailed appearance of the figures.

Fifty-seven per cent. of the normal school students received *a* as to the detailed correctness of their drawings; 13 per cent. of the eighth grade; 11 per cent. of the sixth grade, and 8 per cent. of the third grade. Here the difference in ability in remembering many details between the normal student and the pupil of any grade is quite marked.

Table XIII. shows the per cent. of students and pupils receiving *a, b, c,* as to the general appearance, who also received the same mark in detailed appearance.

Table XIV. I found the most interesting and instructive. This shows the method by which each child learned the figures. Nearly all the students and pupils used a combination of two or three methods and several used all the methods. The normal school student depended the most upon association in remembering the figures, 92 per cent. using this method, while 37 per cent. consider it the most important method of learning; but the children consider the learning of wholes as wholes the most important. For example, one typical normal school student writes:

"In order to place the figures better in my mind I at once thought of their likeness to other things. Upon looking at the first one, I noticed that the upper, lower and left hand sides were straight lines put together so as to form a square. The fourth side made me think of a crude drawing of a human face. The second figure looked like an Indian tent; the third one like a semicircle on an axe; the fouth one like writing, and the fifth like an oak leaf."

Let us next notice what importance was assigned to the motor element in learning. Eighty-eight per cent. of the third grade, 80 per cent. of the sixth grade, 76 per cent. of the eighth grade and 40 per cent. of the normal school students used this method. Nearly all the children said that they moved their finger on their desk or in the air while learning. When they thought they could draw it, they looked away from the figure and tried to draw it with the finger. This method seemed most natural to them, but as the children advanced in age and in grade they gradually lost the motor method of drawing, or rather of learning; and when we come to the normal school student, we find that the only form of the motor element existing is the moving of the eyes around the figures just as the child moved the finger. There may be two causes for this; first, the natural instinct toward motor learning may be less strong as the child grows older; second, this natural instinct may have been repressed so often through the discipline of the school room that the older students do not have so strong a tendency to use this method. Only 9 per cent. of the normal school students consider this method the most important; 16 per cent. of the eighth grade; 24 per cent. of the sixth grade, and 40 per cent. of the third grade.

As mentioned before the normal school students consider the learning by association the most important method, but as we come down through the grades we find the method decreasing in popularity. Ninety-two per cent. of the normal school students use it; 86 per cent. of the eighth grade; 68 per cent. of the sixth grade, and 48 per cent. of the third grade. While 41 per cent. of the eighth grade consider it the most helpful method, only 5 per cent. of the third consider it the most helpful. In comparing the drawings with the methods used, I found that those who used this method—association—modified their images so that often the drawing looked more like the figure with which it was associated than like the original form. For example, an eighth grade boy said that the second figure "looked just like a pine tree" and his drawing did look decidedly like a pine tree.

All the children of the third grade used the method of learning by wholes to some extent. Ninety-four per cent. of the eighth grade and 75 per cent. of the normal school students used it, while 55 per cent. of the third grade considered this method most helpful; 45 per cent. of the sixth grade; 32 per cent. of the eighth grade, and 30 per cent. of the normal school students.

This fact may be given as one reason why so much smaller proportion of the children received *a* as to the details of their drawings than received *a* as to the general appearance. Naturally children see the whole thing at once, instead of analyzing. Only 8 per cent. of the third grade used the method of analysis, while 57 per cent. of the normal school students used it.

After several days had passed since the students and children had seen the original figures, I asked them to draw the figures again from memory. Then I asked them which method helped the most in remembering the figures. These papers I did not tabulate but from them I learned many facts. Those who considered the motor element of learning very important the first day they drew the figures said that the motor element did not help them so much when they had to remember the figures for several days. The normal school students considered this method "a quick method of learning but the easiest to forget." While many who did not realize that association helped them the first day say that it helped them to remember for a longer period. One student writes:

"The first three figures which I associated with a familiar object came back readily to-day, but the others which I had learned by hand tracing did not come back so readily."

It is interesting to notice the combinations of methods used and the results obtained from various combinations. Nearly all the students and pupils who received an *a* in both general and detailed appearance used a combination of three or four methods, while those who were marked *c* rarely used more than one or two methods. The normal school students obtained the best results by studying the figure as a whole first and then spending much time in associating it with other known figures and analyzing it. The children of the third grade obtained the best results by studying the figure as a whole carefully and then tracing it with the finger many times. When they thought they could draw it, they looked away and tried to trace it in the air or on the desk; if they couldn't do it, they studied it some more.

As the children grow older imagination or association seems to take the place of the motor element; and the tendency to reason and analyze grows with age. These facts too often pass unnoticed in the school room. Many teachers who do good work with older children fail with the little pupils because they do not realize the importance of the motor learning to the undeveloped mind. And the reverse is equally true. The motor process of learning must be used but not so constantly that as the child grows older he will lose the power to analyze and to reason.

TABLE XII

Showing Perfectness of Representation

Grade	Normal Students	Eighth			Sixth			Third		
Sex	f.	m. & f.	m.	f.	m. & f.	m.	f.	m. & f.	m.	f.
Number of papers,	150	30	20	10	26	15	11	24	14	10
Number marked *a* in general appearance,	74	9	7	2	7	4	3	6	4	2
Number marked *b* in general appearance,	52	10	9	1	8	4	4	5	3	2
Number marked *c* in general appearance,	29	11	4	7	11	7	4	12	7	5
Per cent. marked *a* in general appearance,	49.3	30	26	20	27	26.6	27	26.1	28.6	22.2
Per cent. marked *b* in general appearance,	30.4	33.3	33.3	10	30.8	26.6	36.3	21.7	21	22
Per cent. marked *c* in general appearance,	20.1	36.6	40.7	70	42.3	46.6	36.3	52.1	50	55.5
Number marked *a* in detailed appearance,	92	4	4	0	4	2	2	2	1	1
Number marked *b* in detailed appearance,	39	12	9	3	8	4	4	3	3	0
Number marked *c* in detailed appearance,	20	14	7	7	15	9	5	18	10	8
Per cent. marked *a* in detailed appearance,	57.4	13.3	20	—	11.5	13.3	18.1	8.7	7.1	7.1
Per cent. marked *b* in detailed appearance,	25	40	45	30	30.8	26.6	36.6	13	21.4	—
Per cent. marked *c* in detailed appearance,	17.5	46.6	35	70	57.7	59.9	45.4	78.2	71.4	56.8

TABLE XIII

Showing Per Cent. of Correspondence Between General and Detail

Grade	Normal Students	Eighth			Sixth			Third		
Sex	f.	m. & f.	m.	f.	m. & f.	m.	f.	m. & f.	m.	f.
No. papers marked *a* in general and detail also,	69	4	4	—	3	1	2	2	1	1
No. papers marked *b* in general and detail also,	29	7	7	—	4	2	2	2	2	0
No. papers marked *c* in general and detail also,	18	11	4	7	10	6	4	11	8	3
Per cent. papers marked *a* in gen. and detail also,	92	44.4	58.2	—	42.8	25	66.6	33.3	25	50
Per cent. papers marked *b* in gen. and detail also,	55.3	70	77.7	—	50	50	50	40	66.6	—
Per cent. papers marked *c* in gen. and detail also,	70.7	68.1	36.3	100	99	85.7	100	91.2	87.5	60

TABLE XIV

SHOWING THE METHOD OF LEARNING

Method	I	II	III	IV
Per cent. of normal school students using,	40	92	75	57
Per cent. of eighth grade using,	76	86	94	22
Per cent. of sixth grade using,	80	68	100	18
Per cent. of third grade using,	88	48	100	8
Per cent. of normal school students helped most by,	9	37	30	22
Per cent. of eighth grade helped most by,	16	41	32	7
Per cent. of sixth grade helped most by,	24	20	45	3
Per cent. of third grade helped most by,	40	5	55	0

Method I. is by motor tracing.
Method II. is by association.
Method III. is by wholes.
Method IV. is by analysis.

Editor's Comment.—This and the two studies following are representative of forms of study that can be made and that should be made frequently by intelligent teachers who are not satisfied with the mechanical learning of lessons, but who wish to economize the time of their pupils and help them to gain the power to direct their activities to the best advantage.

The results of this study suggest that probably movements are of greatest importance when habits are to be formed, but that recollection of specific things after an interval is best insured by associative memory.

THESIS XI

Methods of Learning Visual Forms

By Edna L. Battles

Thesis.—To test the methods of learning in the different grades, I drew three figures. The first figure was composed of a circle, a triangle, a rectangular shape which was pointed at one end, and a figure resembling an oak leaf and having five points. The second figure was made up of the same parts differently arranged. The third figure was entirely different. The island of New Guinea was taken as a basis for the form. This island was taken because it was thought that the pupils had probably never studied it to any great extent nor learned to draw it, so that they would not be familiar with its shape. It was simplified somewhat but the general shape was preserved. The dividing lines between the Dutch, German and British territories were put in the figure, also the two largest rivers. Two marks indicating capitals and two dots indicating cities were also added.

The tests were taken in the second, fourth, sixth and eighth grades. Three tests were taken in each grade. In the first test the pupils were asked to learn the figure and were allowed to learn it in the way they chose; in the second test they were asked to look at the figure, and then to shut their eyes or look away and see if they could see a picture of the figure; in the third test the pupils were asked to learn the figure by drawing it on the desk with their finger. In each test, they were then given four minutes in which to study the figure; after which time they were required to draw it from memory. The tests were given in the afternoon; in nearly every case (except where there was no recess) they were given directly after the afternoon recess.

I went over the tests, marking them as to general form, detail and proportion. The *general form* was marked with the letters from *a* to *g,* according to the correctness of the form; *a* being used when the form was correct; *b, c* and *d* as the forms were less correct; *e* when there was no resemblance to the original form; *f* when only a part was drawn, and *g* when nothing was drawn. In grading the first two figures for *detail,* the circle, the triangle, the rectangular form, the shape of the leaf and the number of its points each counted as one. The details of figures 1 and 2 thus numbered five. In figure 3 each dot representing a city, each river, each boundary line and five parts, either points or curves of the figure each counted as one, making the number of details equal thirteen. In marking the proportion of parts, the letters ran from *a* to *e*.

The tables are made on the standard of twenty-five boys and twenty-five girls in each of the four grades used. Table XVI. gives the distribution of the total marks for general form and proportion in each test for each grade.

TABLE XV

	Test I						Test II						Test III						
Grade	*a*	*b*	*c*	*d*	*e*	*f*	*a*	*b*	*c*	*d*	*e*	*f*	*a*	*b*	*c*	*d*	*e*	*f*	*g*
Boys																			
II.	2	8	10	23	6	1	2	3	13	20	9	3	5	7	13	12	8	5	
IV.	4	11	15	14	6		5	11	16	12	6		6	7	11	20	6		
VI.	7½	11¼	17½	11¼	2½		7½	17½	15	7½	2½		8¾	8¾	20	7½	5		
VIII.	18	16	13	2	1		17	16	13	4			25	14	9	2			
Girls																			
II.	1	6	10	20	12	1	2	4	7	22	12	3	4	7	9	17	11	1	1
IV.	5	8	14	22	1		3	11	14	19	3		7	7	15	15	6		
VI.	7½	12½	15	12½	2½		15	17½	12½	5			10	7½	18¾	10	3¾		
VIII.	23¾	13¾	11¼	1¼			26¼	16¼	2½	3¾	1¼		22½	18¾	7½	1¼			

In the second table, 7 was substituted for *a,* 6 for *b,* 5 for *c,* etc. The number of *a*'s was multiplied by 7, the number of *b*'s by 6, etc. The results were added and the total was placed in the table. The number of details which were right in each test were also added and put in the table.

TABLE XVI

Grade	Test I Child's own Method		Test II Imagination Method		Test III Motor Method		Total	
	Detail	Gen. Form and Proportion	Detail	Gen. Form and Proportion	Detail	Gen. Form and Proportion	Detail	Gen. Form and Proportion
	Boys							
II.	62	224	60	210	78	224	200	658
IV.	102	243	113	247	105	237	320	727
VI.	118	260	105	270	120	259	343	789
VIII.	135	298	143	296	180	312	458	906
Total	417	1025	421	1023	483	1032	1321	3080
Total	1442		1444		1515		4401	
	Girls							
II.	59	211	63	203	68	219	190	633
IV.	106	244	104	242	122	244	332	730
VI.	114	260	115	293	133	260	362	813
VIII.	151	310	146	313	165	313	462	936
Total	430	1025	428	1051	488	1036	1346	3112
Total	1455		1479		1524		4458	
	Boys and Girls							
II.	121	435	123	413	146	443	390	1291
IV.	208	487	217	489	227	481	652	1457
VI.	232	520	220	563	253	519	705	1602
VIII.	286	608	289	609	345	625	920	1842
Total	847	2050	849	2074	971	2068	2667	6192
Total	2897		2923		3039		8859	

The table shows, in regard to *detail,* a great improvement from the second to the fourth grades, no great change from the fourth to the sixth grades, and again a great improvement from the sixth to the eighth grades. In test II. the boys of the fourth grade actually surpassed those of the sixth; but this result is perhaps accidental, as it is due to the great success of two or three boys in the fourth grade—who ranked as high in the number of correct details as the best of the eighth grade—combined with the very low rank of two or three boys in the sixth grade.

The gain in *general form and proportion* is usually gradual from grade to grade. It totals less, from the second to the eighth grades, than the gain in number of correct details. The number of correct details is doubled and a little over, while the general form and proportion is not very much better in the eighth grade than in the second. The younger children get the general form of the figure much better than they get the details. They see anything as a whole, and have not yet learned to look for the parts. One important phase of the primary teacher's work is to lead the child to see the details in the things around him.

One very interesting thing is the number of times that the figures were reversed. Figures 1 and 2 were often reversed and usually it was after one or the other of them had been learned before. There were most reversals in Grade IV., there being very few in the other grades.

In regard to the relative success of the *three methods of learning* used respectively in tests I., II. and III., we find from Table XVI. that, for all the children taken together, the motor method gave better results than either the method in which the children were directed to close their eyes and imagine the figure or the method of the child's own choosing. This superiority of the motor method is much more marked in correctness of details than in general form and proportion. The importance of motor methods in school work is indicated by these results.

Looking at the results of each grade separately, we see that the motor method of learning is by far the best for the second grade, while the imagination is very poor. The importance of the motor method was brought very forcibly to my mind in giving the tests in this grade. When the children were learning their figures by the choice or motor methods, a great many of them moved their whole body in the direction in which they would move their fingers when drawing the figure. In the fourth and sixth grades the imagination tests begin to gain prominence, showing that the children are gaining the power to form images.

The question arises, Would it not be better to lead the pupils to learn by a method which is found to be best for children in that grade than to allow them to learn as they choose? It would seem from the results of these tests that it would be much better to direct them as to the way they should study, especially in the low grades. The choice tests do not give the best results, as perhaps one might expect, but it was where some specified method was prescribed that the best results were obtained. If the children had better results when some specified method was used, with only one trial, does it not seem probable that there would be still greater improvement if it was continued?

The individual child, however, should be studied. Some children are greatly helped by having the method of learning suggested. For instance, there was a boy in grade II. who advanced from *e* in the choice test to *a* in the motor test for correctness of general form. He also gained in details and proportion although not so much. That boy certainly was helped, that time at least, by having the method of learning suggested to him. Of course there might have been other things which combined to make it better. *The motor test was the last test given,* and as this was probably the first time that the children in this grade had ever done anything of the kind they had grown more accustomed to doing it in the last test and so could put their minds more entirely upon it. Another child would be greatly hindered in its development if forced to learn by some specified method. There was one girl in grade II. who fell, in general form, from *b* in the choice test to *f* in the imagination test; and her loss in detail and proportion was nearly as great. This time, at least, the girl was hindered in her learning by having to learn by the imagination method. She however ranked quite high in the motor test.

While the tabulated results show that the majority of the children gained in the motor test, there were very few cases where there was a great difference. There was one boy in Grade II. who gained from *f* in the imagination test to *b* in the motor for general form; in details he gained from 1 to 4, and in proportion from *e* to *b*. This shows again the great value of the motor method of learning for some individuals, especially young children.

Although this study is made of the learning of visual forms and seems very narrow, it may be applied to many phases of the school work. In the map drawing in the higher grades it should be remembered that it is important that the children shall have plenty of practice in drawing the maps and that they shall have an image of the map. In the manual training department, a knowledge of how to use the tools will not suffice. There must be practice in using them. Also in the study of the sciences, we find this recognized and see that in a great many schools, the pupils try the experiments themselves instead of merely getting an image of the results from descriptions which they read in books or which the teacher gives them. When the small children are learning to read, it helps to impress it more deeply upon their minds if when they are learning a new word, they learn to speak it, as well as get an image of its form. Thus we find that in all the school work, when possible, the children should "learn to do by doing."

THESIS XII

How Children Study

By Martha Josephine Baldwin

Thesis.—The following questions were sent to teachers in the grades and high school to be answered by the pupils:

1. How long did you spend on this lesson?

Was it a study period?

2. Were you interrupted at all?

3. How did you try to learn the lesson?

4. How did you know you had your lesson?

The pupils were not allowed to see the following question until after they had finished the preceding ones?

5*a*. Did you try to learn the words of the book?

b. Did you write down any part?

c. Did you use an outline?

Papers were received from the sixth, seventh, eighth and ninth grades, and from the four classes of the high school, altogether numbering four hundred and five: two hundred and thirteen girls and one hundred and ninety-two boys.

The lessons had been prepared the day before and the questions were answered upon one special lesson: history in the sixth and seventh grades and in the freshman and sophomore classes, geography in the eighth, grammar in the ninth, and German in the junior and senior classes. Two typical papers are given on the followingpages.

Girl. Subject, Grammar.

Ninth Grade. Age, Fourteen.

1. I spent three quarters of an hour on the lesson.

It was a study period.

2. I was not interrupted.

3. I tried to learn the lesson by first reading it, then learning some of the rules by heart.

4. I knew I had my lesson by reciting it to myself.

5*a*. I did not try to learn the words of the book.

b. I wrote a little down.

c. I did not make an outline for any part of it.

Boy. Subject, History.

Freshman. Age, Fifteen.

1. I studied my lesson fifty minutes.

It was not a study period.

2. My dog barked and I had to go to speak to him.

3. Concentrated myself for the length of time stated.

4. I answered the questions which I expected Miss B would ask us.

5*a*. Partly.

b. I did not.

c. I wrote out part of the lesson.

The papers received were studied for the characteristics shown by the answers to the questions. Then I obtained from the high school teachers, records of the pupils' standing received in the studies concerning which these papers were written for the previous eight weeks. The system runs in A, B, C, D; A being the highest mark.

There are twelve different methods of study used. They divide into two great heads, the word methods or studying simply words, and the thought method, illustrated by one quotation which says "I tried to understand the lesson as I studied it." Under the first head are included reading, reading and reciting, reading and writing, reading by sections, reading and using other books, and learning by heart. The thought method includes reading for story, reading to understand, concentrating self, trying to answer questions, finding important facts, and merely spending time required. According to Table XVII. it is found that 82 per cent. of the children used the word method of learning as shown above, leaving a very small per cent. whose answers indicate that they tried in any way to understand the content of the text. A larger per cent. of girls than of boys studied by the thought method.

TABLE XVII

HOW DID YOU STUDY YOUR LESSON?

		Grades: Grammar				Grades: High				Ages									
BOYS		VI	VII	VIII	IX	IV	III	II	I	10	11	12	13	14	15	16	17	18	19
Word	Number	11	16	32	22	34	5	24	8	3	16	15	31	20	21	17	18	8	1
	Per cent.	73	67	86	57	92	100	100	73	75	76	56	76	83	78	94	90	89	100
	Time	20	18	44	43	49	69	71	97	27	20	23	55	17	46	47	45	54	75
Thought	Number	4	8	5	17	3			3	1	5	12	10	4	6	1	2	1	
	Per cent.	27	33	14	43	8			27	25	24	44	24	17	22	6	10	11	
	Time	25	20	30	34	85			45	20	23	19	43	54	41	90	56	56	
GIRLS																			
Word	Number	12	17	30	24	51	12	28	9	3	16	27	24	47	23	24	10	8	1
	Per cent.	92	23	23	33	18	92	100	100	100	84	82	75	91	26	92	100	100	100
	Time	27	19	27	40	60	69	78	46	30	24	21	39	45	62	58	48	62	65
Thought	Number	1	4	7	8	9	1				3	6	8	5	6	2			
	Per cent.	8	77	77	67	82	8				16	18	25	9	74	8			
	Time	15	30	20	40	63	69				17	30	63	53	48	65			

TABLE XVIII

How Did You Study Your Lesson?

Boys	VI	VII	VIII	IX	T.	IV	III	II	I	T.	10	11	12	13	14	15	16	17	18	19	T.
Reading	8	10	8	9	35	17	1	1		19	1	11	9	9	9	8	5	1	1		54
Reading and reciting		4	18	6	28	8	1	1		10		1	8	12	6	6	4		1		38
Reading and writing			1	1	2									2							2
Reading by sections	3	1	3	4	11	9	1			10	2	3		4	4	4	2	2			21
Reading and using other books		1	1		2		2	22	8	32		1		2	1	3	5	15	6	1	34
Learning by heart			1	2	3									2			1				3
Reading for story																					
Reading to understand				1	1									1							1
Concentrating self	2	3	4	3	12	3			2	5		4	4	4		2	1	1	1		17
Trying to answer questions	1	3		3	7						1	1	3			2					7
Finding important facts	1			4	5								1	2	2						5
Spending time required		2	1	6	9				1	1			2	3	2		2	1			10
	15	24	37	39		37	5	24	11		4	21	27	41	24	27	18	20	9	1	192
Girls																					
Reading	3	14	2	7	26	16	3	1	1	21	3	5	9	3	16	6	2		2	1	47
Reading and reciting	1	2	12	5	20	12	1		1	14		3	5	7	12	3	3		1		34
Reading and writing			2	4	6				1	1			1	3	2			1			7
Reading by sections	7		7	4	18	15	6	2		23		8	5	7	8	7	5	1			41
Reading and using other books	1	1	3	2	7	7	2	25	6	40			4	2	7	7	14	8	5		47
Learning by heart			4	2	6	1				1			3	2	2						7
Reading for story						1				1				1							1
Reading to understand			1	1	2	3				3				3	1	1					5
Concentrating self	1	1	3	3	8	2				2		2	3	1	2	1	1				10
Trying to answer questions		2	3	4	9							1	2	3	2	1					9
Finding important facts						3	1			4						3	1				4
Spending time required		1			1								1								1
	13	21	37	32		60	13	28	9		3	19	33	32	52	29	26	10	8	1	213

TABLE XIX

How Did You Know You Had Your Lesson?

	Grades										Ages										
	Grammar					High															
	VI	VII	VIII	IX	T.	IV	III	II	I	T.	10	11	12	13	14	15	16	17	18	19	T.
Boys																					
Could tell story	1				1						1										1
Could recite lesson	3	8	25	17	53	34	2	6	9	51	2	7	13	22	23	14	12	3	7	1	104
Could answer questions	3	3		3	9		2			2		3	3	2	1	2					11
Could write lesson		3		1	4	2				2			3	1	1	1					6
Felt sure								2	2	4						1	1	1	1		4
Studied hard	7	9	3	2	21	1		1	1	3	3	3	8	6	1	1	1	1			24
Thought it was learned	1	1		7	9		1	2	5	8	1	1	2	2	1	6	1	2	1		17
Was not positive		4	5	3	12	6	2	2	6	16		3	1	5	3	4	2	5	1	4	28
Did not have lesson			1	1	2	1		1	1	3			1	1		1	1	1			5
	15	28	34	34		44	7	14	24		7	17	31	39	30	30	18	13	10	5	200
Girls																					
Could tell story			2		2		1			1			1	1			1				3
Could recite lesson	4	6	24	21	55	30	12	11	5	58	1	10	15	20	25	19	14	4	4	1	113
Could answer questions	6		3	5	14	7				7	2	3	2	4	3	4	3				21
Could write lesson			1	7	8	1				1				4	4	1					9
Felt sure								2	3	5						1	1	3			5
Spent time required	3	8	2	1	14	2	1	2		5		7	7		1	2	1	1			19
Thought it was learned	2				2	3	2		1	5	2			1	2	1	1		1		8
Was not positive			6	2	7	3		1	1	5		3	3	1	2	2		1			12
	15	14	38	35		46	16	16	10		5	23	28	31	37	30	21	9	5	1	190

There are two of the twelve divisions which take the lead. The first is reading, which is used in the case of twenty-five per cent. of both boys and girls, being more popular with the boys. The other prominent method is that of reading and using other books. Twenty per cent. of all the papers show the use of this method, which is employed more by the girls than by the boys. In contrast with the simple reading method, this is used much more in the high school than in the grades, especially in the junior class. This is doubtless due to the fact that the papers came from German classes where the translation was done by the use of the German dictionary. In the ages, this method is used very much by the older pupils.

TABLE XX

How Did You Know You Had Your Lesson?

Boys		VI	VII	VIII	IX	IV	III	II	I	10	11	12	13	14	15	16	17	18	19
Tests	Number	7	14	25	21	36	4	6	9	3	10	19	25	25	17	12	3	7	1
	Per cent.	47	67	74	62	67	57	43	38	43	59	62	64	63	56	67	23	70	20
No tests	Number	8	7	9	13	8	3	9	15	4	7	12	14	5	13	6	10	3	4
	Per cent.	53	33	26	38	33	43	57	62	57	46	38	36	37	44	33	77	30	80
Girls																			
Tests	Number	10	6	30	33	38	13	11	5	3	13	18	29	32	24	18	4	4	1
	Per cent.	67	43	79	94	83	81	63	55	60	57	64	94	87	80	77	45	75	100
No tests	Number	5	8	8	2	8	3	5	5	2	10	10	2	5	6	3	5	1	
	Per cent.	33	57	21	6	17	19	37	50	40	43	36	6	13	20	23	55	25	

Table XIX. shows how the pupils knew they had their lessons. Table XX. shows the same, only in different divisions, which are two, one in which the pupils tested themselves in some definite way and one where no definite tests were employed. It was found that higher marks were obtained by those using some sort of test. Boys of thirteen and sixteen and girls of thirteen and fourteen used tests more than those of any other age.

Of the nine classes of tests represented by the headings and quotations of Table XX. the most prevailing is the second, which is the proof by reciting either to one's self or to some one else by topics. Fifty-six per cent. of all used this test and it seems more popular among the girls. It is used more in the high school and most in the freshman class. The ages thirteen, fourteen and fifteen are the ones that used it most.

On comparing the results of my inquiry with the standing of the pupils in their studies, it was found that the average time spent in obtaining the highest mark was sixty minutes, the girls spending more time than the boys and the juniors and the seniors using the most. The preparation was at home, usually the pupils not being interrupted. The largest per cent. of those who obtained the highest mark used the simple reading method, with nearly as many referring also to other books.

In obtaining the mark B, the average time spent was sixty one minutes, the boys spending more time than the girls, and the freshman class required more than the others. Here, also, the lessons were prepared at home in the afternoon with no interruptions. The method most used was that of reading and using other books.

A little more time was spent in obtaining the mark C, that is, sixty nine minutes and the boys spent more time than the girls. This preparation was also at home and in the afternoon, although the boys of the higher classes studied in the evening. They were not interrupted to any great extent and forty-four per cent. used the methods of reading and using other books, which applies more to the girls.

The greatest interest in the marks centers in the lowest one, for those receiving D were all boys, and the average time spent was one hundred and fifty minutes. They all studied at school with recitations going on around them and they were interrupted. The common method was reading, over and over again.

In drawing conclusions from these studies, the first which comes up is that the children study words rather than thoughts, that they study in a mechanical sort of way, which enables them to say they have studied the lesson and spent the time required. They read the words over and over, and doubtless get more confused the more they read.

This seems to me a great fault. Children should study to understand what they read and it will prove a quicker and surer method. This is one problem for teachers to solve, and it is certainly a very important one.

G. Stanley Hall says that at least three fourths of the time spent by a boy of twelve in trying to learn a hard lesson out of a book, is time thrown away, not in deliberate idling, but through unconscious mind wandering, lack of concentration, the unwise attempts of memorizing words of the text without proper assimilation of thoughts.

One most serious side of the problem is that most of the loss is experienced by boys and girls who are trying hard to master the lesson.

THESIS XIII

An Experimental Study of Musical Learning

By Mary G. Gilles

Thesis.—This study was made to ascertain the different methods employed in learning and remembering a series of tones and the best method of doing the same.

The reagents were nine normal school students, two instructors, and eight children from grades five to eight, and two high school students, making a total of twenty-one. Of this number nineteen have had practice, varying from considerable to a very little, in either playing or singing or both, one sang by note only and one could neither sing or play, and had no knowledge of notation, the keyboard or the relation of one tone to another though he could discriminate tones higher or lower than a given tone.

The experiment was made in four ways, the instrument used being a piano.

I shall refer to the different parts of the experiment as test I., II., III. and IV. Test I. consisted of a series of fifteen notes played to the reagent, who reproduced them from memory on the piano. Test II. comprised a series of fifteen notes which the reagent read from the score and reproduced from memory on the piano. Test III. contained a series of fourteen notes. These the reagent read and played from the score until he could reproduce them from memory. Test IV. was made up of a series of thirteen notes. They were played to the reagent who followed the score at the same time, completing the test by reproducing from memory.

The reagent was not limited as to time or number of trials but he attempted to reproduce as soon as he thought he could do so.

The children often tried to reproduce before they were able to do so, seeming to depend upon the trial and success method, while the adults, in most instances, did not attempt to reproduce from memory until they were fairly sure they could reproduce correctly, yet after reproducing they were not always sure that they had done it correctly.

It was noticeable that the first and last tones were reproduced more quickly and more accurately than the intervening ones, by both children and adults. This fact has been found to be true in all memory tests.

The children almost invariably began with the feeling that they couldn't do what was asked of them. They underestimated their ability and in many cases said they had reproduced the tones incorrectly simply because they thought it beyond them to do it. However by encouraging them and naming some of their little friends who had taken the tests, and said it was great fun, I succeeded in overcoming this fear.

Several of the adults and the children noticed the scale form in some of the series of tones given, and in that way recognized the series in less time than they would have otherwise.

Six of the adults failed to recognize a series as correct after playing it so, and three failed to recognize it as wrong when it was wrong. Six of the children were troubled in a similar way and made the error more than once. Four played a series wrong, thinking it right, and made this mistake from one to four times.

Three distinct methods were used by the reagents, sound, symbol and visual. Combinations of these three were used to some extent.

The symbol method involved the use of syllables, numbers and letters. The visual included a visual image of the keyboard or a key as representing a given tone, the position of the notes on the staff or their relative position when representing tones.

It is readily seen that test I. affords a greater opportunity for the use of the sound method than any other test. Test II. is more favorable for the use of the visual method. In tests III. and IV. there is occasion to use both the sound and visual methods equally.

Table XXI shows the number of children and adults that used each method. The children used the sound method the greatest number of times, twelve, and the adults the sound and visual methods, each twelve times.

The adults used visual memory more than the children, which supports the statement made by Colegrove that "Visual stimuli usually make the strongest appeal to adults, excepting during sleep and repose." There is a great tendency as age increases to use the visual memory. Only in one instance did a child under thirteen use visual imagery, while all the adults, with the exception of one, made more or less use of it. The children used the symbol method to a greater extent than the visual memory. They used it to a greater extent than the adults, probably because in school they are taught to read notes by means of symbols, which would naturally influence them to remember not by the relative position of one note to another, but in terms of another and more familiar system of remembering.

From the table it follows that both the adults and children com-

TABLE XXI

METHODS USED BY CHILDREN AND ADULTS IN LEARNING A SERIES OF MUSICAL NOTES

	I			II			III			IV							
	No.	T.	Tr.	No.	T.	Tr.	No.	T.	Tr.	No.	T.	Tr.	T. T.	A. T.	T. Tr.	A. Tr.	T. No.
Children																	
	10	198	107	10	76	41	10	56	33	10	58	43	388		224		40
S.	6	68	41				3	14	11	3	15	16	97	8.0	68	5.6	12
Sym.				4	24	16	3	27	12	2	15	11	66	7.3	39	4.3	9
V.	1	55	30	2	18	7	2	8	6	2	11	7	92	13.1	50	7.1	7
S. and Sym.	1	15	9							1	8	4	23	11.5	13	6.5	2
S. and V.	1	20	8										20	20.0	8	8.0	1
Sym. and V.	1	40	19	2	21	12	1	2	1	1	5	3	68	13.6	35	7.0	5
S. Sym. and V.				2	13	6	1	5	3	1	4	2	22	5.5	11	2.7	4
Adults																	
	11	87	34	11	59	21	11	48	20	11	91	39	285		114		44
S.	5	37	14	2	4	4	4	7	5	1	3	3	51	4.2	26	2.1	12
Sym.				1	25	2				2	22	2	47	15.6	4	1.3	3
V.	3	36	13	5	18	10	1	2	3	3	37	20	93	7.7	46	3.8	12
S. and Sym.	2	7	3	1	3	2	2	8	4	2	14	8	32	4.5	17	2.4	7
S. and V.	1	7	4	1	1	1	2	7	5	2	10	5	25	4.1	15	2.5	6
Sym. and V.				1	8	2	2	24	3				32	10.6	5	1.6	3
S. Sym. and V.										1	5	1	5	5	1	1	1

T. = Time in minutes. Tr. = Trials. T.T. = Total time. A.T. = Average time. T.Tr. = Total trials. A.Tr. = Average trials. S. = Sound. Sym. = Symbol. V. = Visual. T.No. = Total number.

pleted test III. in the least time and with the least number of trials, the adults taking less time than the children and fewer trials in either case, although there is no marked difference.

Both adults and children had the poorest record in test I., it taking the children more than twice as long as the adults and three times the number of trials.

Of the three principal methods of learning, the symbol method required the least time for the children and the auditory for the adults.

It will be noticed that the symbol method took the least time for the children and the greatest for the adults, the latter even requiring mo time than the former. This is the only instance in which the adult requires more time than the children. It would seem from this that time is lost if we try to fit things into a system with which we are not very familiar, but if we use the system of remembering with which we are most familiar time is gained, and the use of the trial and success method is eliminated.

With both children and adults, this symbol method required the least number of trials, and the visual the greatest, which seems to indicate that we can depend upon our visual memory the least in memory of musical notes, and that when we have a system into which to fit things our power of recall is surer. The children seem to use this method of remembering more than the adults.

Table XXII. represents the number that used different methods, of those who play by ear or do not, and those who play from memory or do not. For convenience I will refer to them as groups a, b, c and d. Three children were placed in group a and seven in group b. It is more than probable that most of those in the latter group could play by ear if they tried, but had never attempted to do so before this experiment. Seven adults were placed in group b and four in group a.

The children in groups a and b used the sound method to the greatest extent. The visual method was not used at all by those in group a, and was made considerable use of by those in group b.

The adults in group a used the sound method the greatest number of times, while those in group b used the visual.

Both children and adults belonging to group b used the visual method more than those of group a.

All the children and eight adults were placed in group c, and the remaining three adults were placed in group d. Group c contains those who play from memory, and group d those who are unable to do so.

Both children and adults in group c used the sound method the

TABLE XXII

METHODS OF LEARNING A SERIES OF MUSICAL NOTES

	Children								Adults							
	No.	S.	Sym.	V.	S. Sym.	S. V.	Sym. V.	S. V. Sym.	No.	S.	Sym.	V.	S. Sym.	S. V.	Sym. V.	S. V. Sym.
I. Play by ear,	3	2			1				7	4		1	2			
Do not play by ear,	7	4		1		1	1		4	1		2		1		
Play from memory,	10	6		1	1	1	1		8	4		2	2			
Do not play from memory,	0								3	1		1		1		
II. Play by ear,	3		2					1	7	2		3	1	1		
Do not play by ear,	7		2	2			2	1	4		1	2			1	
Play from memory,	10		4	2		1	1	2	8	2		4	1	1		
Do not play from memory,	0								3		1	1		1		
III. Play by ear,	3	1	1				1		7	4			2	1		
Do not play by ear,	7	2	2	2			1		4			1		1	2	
Play from memory,	10	3	3	2			1	1	8	4		1	2	1		
Do not play from memory,	0								3		1	1		1		
IV. Play by ear,	3	1	1				1		7	1	1	1	2	2		
Do not play by ear,	7	2	1	2	1		1		3	1		2				
Play from memory,	10	3	2	2	1	1		1	8	1	1	2	2	2		
Do not play from memory,	0								2			1		1		

S. = Sound. Sym. = Symbol. V. = Visual.

greatest number of times and those of group d the least, substituting the visual for it.

From a survey of the whole, it seems that the auditory memory is most essential to musicians, and if they are lacking in this, adults rely upon the visual memory, and the children upon a system with which they are more familiar and into which they can fit the thing to be remembered, for example, the use of the symbols.

As age increases there is a great tendency to use the visual memory. Visual memory is better for the older students because it is employed more by them.

The motor memory is important in committing to memory. In several cases I noticed a movement of the fingers or the lips, and in some cases the symbols were said aloud or were sung.

The usual practice method of musicians seems to be without doubt the best method of learning.

There were many individual peculiarities. One boy took note of the highest tone played, which showed that he reasoned about it, taking less time, fewer trials, and having to hear it the least number of times of all the children. His record too was better than that of many of the adults.

Another child belonged to a family, several of which were very fine musicians. She herself played well, but could not play by ear, as could none of her relatives. It took her fifty-five minutes to complete the first test, thirty trials, and she was obliged to hear it played forty-nine times. Then in order for her to get it, I had to offer suggestions, and call her attention to the relation of one tone to another. One particular tone, a, she couldn't place, and although she played the series twice correctly, she didn't realize it. Finally she became assured that it was correct by comparison, listening intently to the series after having played it herself.

Of the two who could not play the piano, I noticed a similarity in the manner of learning. They were given just enough instruction to enable them to perform the tests. They arranged the notes or tones into three groups, but not by measures, as one would naturally think. A series of tones, one following the other in succession, formed one group, then the repetition of two tones determined the end of a second group, and the third group included the remaining tones. They repeated these groups several times, one more than another, and made no attempt to play from memory until quite sure they could remember it. Both depended on the symbols largely.

One of these two I have just mentioned, f, took twenty-five minutes for the completion of the test, while it took one very proficient in the art of music only a minute. In another test f took

TABLE XXIII.

INDIVIDUAL RECORDS

					I.					II.					III.					IV.				
	Age	Amount of Practice	Play by Ear	Play from Memory	S.	Sym.	V.	T.	Tr.	S.	Sym.	V.	T.	Tr.	S.	Sym.	V.	T.	Tr.	S.	Sym.	V.	T.	Tr.
Children																								
1.	13	2½ yrs.	No	Yes	1			5	3		1		10	9		1		3	3	1			6	6
2.	9	1 yr.	Yes	Yes	1			18	11	1	1	1	5	3		1	1	2	1		1	1	5	3
3.	10	7 mo.	No	Yes	1			15	7		1	1	5	3	1			4	2	1			5	4
4.	11	3 yrs.	Yes	Yes	1			10	9		1		8	4	1			6	6	1			4	6
5.	10	1½ yrs.	No	Yes	1			8	5			1	15	6		1		20	8		1		10	8
6.	14	1 yr.	Yes	Yes	1	1		15	9		1		3	1		1		4	1		1		5	3
7.	13	3 yrs.	No	Yes			1	55	30		1	1	16	9			1	6	5			1	7	5
8.	13	14 mo.	No	Yes	1			12	6		1		3	2	1			4	3	1	1		8	4
9.	14	3 yrs.	No	Yes	1		1	20	8			1	3	1			1	2	1			1	4	2
10.	11	3 mo.	No	Yes		1	1	40	19	1	1	1	8	3	1	1	1	5	3	1	1	1	4	2
Adults																								
a.	21	little	Yes	Yes	1			5	2			1	7	2	1			4	1			1	5	5
b.	21	little	No	No	1		1	7	4			1	3	1	1		1	5	3			1	17	10
c.	22	little	No	Yes			1	7	4			1	4	3			1	2	3			1	15	5
d.	21	none	No	No	1			25	8		1	1	8	2		1	1	6	2	1	1	1	5	1
e.	35	much	Yes	Yes	1			2	1	1			1	2	1			½	1	1	1		4	3
f.	44	none	No	No			1	25	6		1		25	2		1	1	18	1					
g.	21	little	Yes	Yes	1	1		2	1	1			3	2	1	1		2	2	1			3	3
h.	21	much	Yes	Yes	1			2	2	1		1	1	1	1		1	2	2	1		1	2	1
i.	22	much	Yes	Yes	1	1		5	2	1	1		3	2	1	1		5	2	1	1		3	1
j.	21	some	Yes	Yes	1			3	1			1	2	2	1			1	1		1		2	1
k.	21	much	Yes	Yes			1	4	3			1	2	2	1			2	2	1		1	3	3

S = Sound. Sym. = Symbols. V. = Visual. T. = Time. Tr. = Trials.

eighteen minutes, while the musician required only thirty seconds.

Not age, but training is probably the secret of whatever greater ability the older classes possess. It is true that system is the secret of memory. But system depends on the discovery of distinctions unnoted by the unsystematic, which constitute threads of connection between details. It elevates the association between these details from a mere serial association by contiguity to a simultaneous association in which similarity plays a most important part. The kind of connection that makes all these details one thought may absorb them so that in recall their separate existence is lost. System is the secret of remembering, but also of forgetting whatever it finds inconvenient or unnecessary.

THESIS XIV

Incidental Memory

By Isabel Wallace

Thesis.—Much of what we remember has been learned without conscious effort. During the first few years of his life, when more is learned than during any later period, a large part of the knowledge gained is acquired without volition on the part of the child. Neither does the acquisition without effort cease at the end of these few years.

The data for this thesis were taken in connection with an illustrated lecture on Hiawatha. The lecture was given in the main hall of the normal school, and it was attended by the normal school students and the pupils from the fourth to the eighth grades. Questions were asked which had nothing to do with the lecture itself, but were of things purely incidental.

The questions about the lecture were asked about four weeks after the lecture was delivered. Therefore what was remembered at that time would probably be retained much longer. I received eighty papers from the normal school students, and two hundred and sixteen from the children from the fourth to the eighth grades.

The first question was: "When did Mr. Kempton lecture at the normal school? Give the date and the hour." The normal school students had a much higher per cent. of correct answers than the grade pupils.

The following give an idea of the answers received:

Normal School Pupils:

"December 21, from about half past ten to twelve o'clock. I thought about the lecture for a moment and the remembrance came to me that it was just before the vacation. I thought of the day on which Christmas came, then counted and found that Friday came the twenty-first."

"Mr. Kempton lectured on Hiawatha the Friday before the Christmas vacation—Dec. 21, 1906. The time was 10:30 in the morning. I remembered the day because Howard was married on Tuesday the twenty-fifth and the Friday before was therefore the twenty-first."

"Mr. Kempton lectured on Hiawatha Dec. 23, 1906. The lecture was on Friday and came at 10:40 A. M.

"He gave it on Friday afternoon between four and five. I remember it was about five because I did not have to wait long after the lecture until supper."

"It was Wednesday before the Thanksgiving recess. I remember because we went home that day."

An eighth grade boy: "Mr. Kempton lectured Dec. 28, 1906, at half past two in the afternoon."

A fourth grade boy: "Before Christmas."

The normal school pupils acknowledged that they used reason as well as memory in answering questions. They thought first that the lecture came before a vacation and then they calculated the date. Some did not think of the right vacation, while one thought that supper rather than dinner followed the lecture. The answers of the normal and the eighth grade pupils include more details than the answers of the children in the lower grades.

The second question was: "What color was his suit?"
The normal school students:

"His clothes were of dark mixed goods."

"I think his clothes were black."

An eighth grade boy: "His suit was blue."

A sixth grade girl: "His suit was gray."

A fourth grade boy: "Dark."

Several said that they could not see the color of his suit because the lights were turned out and some of the reasons for the answers were peculiar. For instance, "He wore a dark suit such as all men wear." "He has always worn black." These indicate that some relied on their previous knowledge and not upon their memory of the particular individual and day.

The third question was: "From what did he read?"
The normal school pupils:

"He read from a book which had limp covers, for he folded one side of the book back."

"He read from a book about three fourths of an inch thick."

"He read from small sized note paper."

An eighth grade girl: "He read from a book."

A sixth grade boy: "He read from the poem."

A fifth grade girl: "He read out of a little book of Longfellow's poems."

The fourth question was: "Who managed the lantern?"
The normal school students:

"A lady managed the lantern."

"The man who managed the lantern has acted in the same capacity here before."

"Mrs. Kempton managed the lantern."

A boy in the fifth grade: "Mrs. Kempton."

A fifth grade boy: "Mr. Alexander managed the lantern."

A fourth grade boy: "Mr. Thompson managed the lantern."

A fourth grade girl: "The minister from the C. C. church."

The per cent. of the sixth grade children who were able to answer the question was very low. (The children in this grade had been told by their teacher not to look around the hall.)

The fifth question was: "Describe the clothing of the person who managed the lantern."

The normal school pupils:

"She had on a light waist and a black skirt."

"She wore a white waist and a dark skirt."

"A striped waist and a dark skirt."

"A light cape and no hat."

An eighth grade boy: "A light waist and a blue skirt."

Another: "A plaid waist and a dark skirt."

A sixth grade girl: "She wore a gray suit."

A sixth grade boy: "He had pants, vest, jacket, necktie, collar and watch."

A fourth grade boy: "She had pretty clothes on."

There is a great variety in the answers to this question because the person stood at the back of the hall where she was out of the range of vision of most of the children. Unless they saw her enter the hall, they were obliged to turn around to see her. Owing to the fact that the hall was darkened, it was necessarily difficult to obtain an idea of the clothing. Many of the answers are so general that they would apply even if the pupil had not seen Mrs. Kempton.

The sixth question was: "Tell me about any change in the lighting of the hall."

The normal school pupils:

"The hall was darkened by the shutters over all the windows. During the first part of the lecture, the electric lights were on. When Mr. Kempton was ready for the pictures, the lights were turned out. They were on again for the audience to go out."

"When we went in, the hall was as usual. Then the dark curtains were let down. After Mr. Kempton had read a few moments, the lights were turned on until he was ready to show the pictures, and then they were turned off again. After the pictures, the curtains were raised."

An eighth grade girl: "The curtains were pulled down and the doors closed."

A seventh grade girl: "It changed from red to green."

A seventh grade boy: "The curtains were drawn and the electric lights lighted."

A sixth grade girl: "The lights were put out."

A fifth grade girl: "Sometimes it was dark, at other times light."

A fifth grade boy: "Darkened."

The girl who answered that it changed from red to green must have been thinking of something entirely different. It was noticeable that the normal school pupils had a knowledge of the order in the changes made in the lighting. This knowledge was not contained in the answers from the lower grades, but almost everyone knew that the hall was darkened during the time when the pictures were shown.

The seventh question was: "Tell anything you can about Mr. Thompson (principal of the normal school) during the lecture." The normal school pupils:

"Mr. Thompson introduced the speaker, going upon the platform: He also helped to pull the curtains."

"Mr. Thompson helped lower the curtains and introduced Mr. Kempton. After it was over, he raised some of the curtains. He asked Mr. Alexander if he had anything to say to the Edgerly pupils."

An eighth grade boy: "He pronounced the name of Hiawatha."

Another: "Mr. Thompson was quiet and sat near the stage."

Eighth grade girl: "Mr. Thompson introduced Mr. Kempton."

A seventh grade boy: "He spoke."

The eighth question was: "Tell me anything you can about Mr. Alexander during the lecture."

The normal school pupils:

"Mr. Alexander had charge of the pupils from the Edgerly. He gave an announcement to the Edgerly School children at the close that school would begin again after the Christmas vacation."

"Mr. Alexander stood in the back of the room watching the children and when they were making a disturbance he spoke to them quietly. I remember that he watched a particular group of boys and spoke to them."

A girl in the eighth grade: "He placed the people in their seats."

Eighth grade boys: "Mr. Alexander was at the head of it all."

"Mr. Alexander was a chairman."

A seventh grade girl: "He saw that nobody was rude."

A sixth grade boy: "He helped run the machine."

The children seemed better able to answer this question than the previous one. They are more familiar with Mr. Alexander and are more accustomed to watch him because he is principal of the Edgerly School.

The ninth question was: "What kind of a day was it?"

The normal school pupils:

"It was a dark, rainy day. It was very slushy. I remembered that because I found it hard walking when coming to school." "Pleasant and sunny and quite cold." "A misty day. I remember because I was uncertain about taking an umbrella, but finally sent it back to my room. Also very slippery and slushy under foot, for one of the girls fell down and got her coat wet."

An eighth grade boy: "It was slippery."

An eighth grade girl: "It was dark and damp."

A seventh grade girl: "It was muggy."

A fifth grade boy: "It was rainy."

A fourth grade girl: "It was cold."

Any of the answers could be applied to this particular day, as the weather was decidedly unsettled. It is difficult therefore to decide whether the pupils actually remembered the conditions of the weather on this particular day or not.

The tenth question was: "Was there anything except the lecture?"

The normal school pupils:

"The children and the normal school students sang Christmas carols before the lecture. Miss Perry gave out the names of the songs and played the accompaniments."

An eighth grade boy: "There were colored pictures."

A sixth grade girl: "There were moving pictures."

A fourth grade girl: "We sang Christmas songs."

The eleventh question was: "Did anyone come in the front door during the lecture?"

The normal school pupils:

"Dr. Chalmers and a few others came in during the lecture." "Two men and some ladies came in the front door."

An eighth grade boy: "Ernest Seton Thompson came in the front door."

A fifth grade boy: "Dr. Chalmers and Mr. Kratzer came in during the lecture."

When anyone came in the front door, the contrast between the darkened hall and the light corridor attracted attention. This is probably the reason that many knew that some people came in during the lecture. Those who were able to mention names had definite knowledge. The boy who said that Ernest Seton Thompson came in the front door may have referred to Principal Thompson.

The twelfth question was: "Were there any decorations in the hall and what was there on the platform?

The normal school pupils:

"There was a white screen, a drawing table, a chair and a lantern on the stage."

The hall was decorated with autumn leaves."

"There were the decorations from the last party."

(These last two show that the individuals were thinking of some previous lecture.)

"Across the front of the platform a large white curtain was stretched. To the right of the curtain was a drawing table used by Mr. Kempton to rest his book and his light upon. There was a dark red curtain upon the platform when the moving pictures were shown. There was a fern on a stand, a desk and a chair."

(The curtain was white and this person must have been thinking of some other occasion.)

A seventh grade girl: "A chair and a glass of water on a little stand."

(She must have had in mind another lecture where the lecturer had a glass of water as there was none on the platform in this case.)

In preparing the tables those who acknowledged that they knew nothing about the questions were grouped with those whose knowledge was clearly incorrect. The other papers were grouped together as having knowledge of the question.

As the answers cited in this paper indicate, the normal school students have answered at greater length than the pupils in the grades. They have given more details and many gave accounts of the ways in which they remembered. The greater command of language enabled the normal school pupils to express themselves more fluently and with a greater degree of accuracy. The pupils in the higher grades have given more details than those in the lower ones.

Reason as well as memory was used in answering some of the questions. This was especially true of the normal school pupils and to some extent of those of the grades. The questions where reason was unquestionably used by many are the first, for many acknowledged that they calculated in order to find the date; the sixth, because in order to show stereopticon views we know that the hall must be darkened and also the twelfth to some extent, because there must have been something on which the lecturer could place his books or papers.

Previous knowledge was used in the second, where an answer which could be applied to any man was given; in the fourth, for many knew that Mrs. Kempton often manages the lantern for Mr.

Kempton; in the fifth where general answers were given; in the sixth, for many know that the hall is always darkened for a stereopticon lecture; in the seventh because many, especially normal school students, know that Mr. Thompson usually introduces the speaker; and in the eighth because Mr. Alexander usually has charge of the pupils at a lecture.

Some used direct association as in answering the question, "From what did he read?" Some evidently answered from a desire to write something in spite of the fact that they had no knowledge of the question.

The statistical result of the study is presented in tables XXIV. and XXV., which show the per cent. of boys and of girls of each school grade, and the per cent. of normal school students, whose answers to each question showed knowledge of the fact.

The question which was answered with the greatest degree of accuracy by all the grades was the third, and next came the sixth and ninth. The questions which were answered better by the lower grades than the higher were the third, the tenth, and the eleventh. There is no regular decrease according to grades in any of the questions. Girls are seen to have more ability to remember incidentally than boys.

Normal	8th Grade	7th Grade	6th Grade	5th Grade	4th Grade
$79\frac{1}{6}$	$67\frac{7}{12}$	$64\frac{1}{4}$	$53\frac{1}{6}$	$65\frac{1}{3}$	$61\frac{2}{3}$

This table shows the final total averages and it indicates that there is no very marked difference in the per cents. The per cent. of the normal school pupils is the highest, but the decrease in passing to lower grades is not regular. Considering that the normal school pupils and the higher grades have greater ability to reason and also a greater amount of knowledge upon which to rely, they show no remarkable gain in the amount remembered incidentally. The differences in incidental memory seem to be due to natural individual differences.

For instance one person of my acquaintance has a remarkable power to remember the clothing of individuals. This person is able at the close of the day to describe the clothing of every teacher, and of many of her acquaintances. Seemingly after a mere glance she will describe in detail the clothing of a person whom she meets on the street. In marked contrast to this person is another who seldom notices a person's clothing unless something unusual attracts. This latter person is more apt to notice what a person says than what he wears.

Another acquaintance can tell many of the peculiar habits of her associates. Various attitudes, tricks of expression, favorite phrases, and different mannerisms are associated with her mental images of the persons with whom she comes in contact. She is able to tell just what certain persons have done during the day and the attitudes they have assumed under various conditions.

One person acknowledges that he is remarkably defective along the line of incidental memory. He can drive all day and not be able

TABLE XXIV

Boys

	Grades				
	8	7	6	5	4
Number of papers,	25	25	19	17	19
Question I.					
Per cent. with knowledge,	16	0	5	0	10
Per cent. with no knowledge,	84	100	95	100	90
Question II.					
Per cent. with knowledge,	88	72	74	88	68
Per cent. with no knowledge,	12	28	26	12	32
Question III.					
Per cent. with knowledge,	96	92	100	100	84
Per cent. with no knowledge,	4	8	0	0	16
Question IV.					
Per cent. with knowledge,	84	80	68	77	63
Per cent. with no knowledge,	16	20	32	23	37
Question V.					
Per cent. with knowledge,	52	28	32	47	38
Per cent. with no knowledge,	48	72	68	53	62
Question VI.					
Per cent. with knowledge,	76	68	84	71	63
Per cent. with no knowledge,	24	32	16	29	37
Question VII.					
Per cent. with knowledge,	56	44	63	47	48
Per cent. with no knowledge,	44	56	37	53	52
Question VIII.					
Per cent. with knowledge,	60	64	43	59	63
Per cent. with no knowledge,	40	36	57	41	37
Question IX.					
Per cent. with knowledge,	74	84	95	100	95
Per cent. with no knowledge,	26	16	5	0	5
Question X.					
Per cent. with knowledge,	32	48	43	53	63
Per cent. with no knowledge,	68	52	57	47	37
Question XI.					
Per cent. with knowledge,	80	68	43	83	74
Per cent. with no knowledge,	20	32	57	17	26
Question XII.					
Per cent. with knowledge,	60	40	48	48	37
Per cent. with no knowledge,	40	60	52	52	63

to tell the color of the horse! He even was so unfamiliar with his daughter's best coat that he sent it off in a missionary box. These are a few instances of the remarkable differences in the memories of individuals.

In a study supplementary to the one about the lecture, quotations were put upon the blackboard for the normal school students and for the pupils in the seventh and eighth grades. The quotations were on blackboards which were seen by the pupils every day. Nothing

TABLE XXV

Girls.

	Grades					Normal
	8	7	6	5	4	School
Number of papers,	21	32	26	17	15	84
Question I.						
Per cent. with knowledge,	53	3	0	6	6	60
Per cent. with no knowledge,	47	97	100	94	94	40
Question II.						
Per cent. with knowledge,	95	88	63	88	87	87
Per cent. with no knowledge,	5	12	27	12	13	13
Question III.						
Per cent. with knowledge,	100	97	89	100	100	86
Per cent. with no knowledge,	0	3	11	0	0	14
Question IV.						
Per cent. with knowledge,	95	78	43	54	60	84
Per cent. with no knowledge,	5	22	57	46	40	16
Question V.						
Per cent. with knowledge,	90	63	34	66	20	59
Per cent. with no knowledge,	10	37	66	34	80	41
Question VI.						
Per cent. with knowledge,	95	94	96	94	100	95
Per cent. with no knowledge,	5	6	4	6	0	5
Question VII.						
Per cent. with knowledge,	72	38	27	59	74	79
Per cent. with no knowledge,	28	62	73	41	26	21
Question VIII.						
Per cent. with knowledge,	74	53	62	59	47	89
Per cent. with no knowledge,	26	47	38	41	53	11
Question IX.						
Per cent. with knowledge,	95	91	96	88	94	100
Per cent. with no knowledge,	5	9	4	12	6	
Question X.						
Per cent. with knowledge,	43	66	85	59	74	85
Per cent. with no knowledge,	57	34	15	41	26	15
Question XI.						
Per cent. with knowledge,	48	78	69	71	74	69
Per cent. with no knowledge,	52	22	31	29	26	31
Question XII.						
Per cent. with knowledge,	32	75	44	18	21	57
Per cent. with no knowledge,	68	25	56	82	79	43

was said about the quotations and no questions in regard to them were asked. They were left upon the boards about four weeks, after which they were erased and the pupils were requested to write them from memory.

The quotations were chosen with reference to the age and ability of the pupils. The one for the normal school pupils was:

"Character is higher than intellect; a great soul will be strong to live as well as to think." *Emerson—The American Scholar.*

That for the eighth grade was:

"The deed I intend is great, but what as yet I know not." *Ovid.*

That for the seventh grade:

"Be merry all, be merry all,
With holly dress the festive hall,
Prepare the song, the feast, the dance,
To welcome merry Christmas."

Spencer—Joys of Christmas.

Although the last quotation is longer than the previous one, on account of the rhyme and rhythm it is more easily learned.

From the normal school pupils I received seventy-seven papers, from the eighth grade twenty-nine, and from the seventh grade forty-three. Very few had the quotations absolutely correct. The highest per cent., fourteen, was found in the eighth grade; the next highest, seven, in the seventh grade; and the lowest, six, among the normal school students.

The highest per cent., eighteen, who gave the idea of the quotation, was found among the normal students; the next highest, fourteen, in the eighth grade, and the lowest, four, in the seventh grade. Ideas, not words, appealed to the older people.

Some gave quotations which they had seen elsewhere. Many of the normal school students told just where the quotation was written.

This is a mechanical memory by which one remembers the exact position of a word in the dictionary or of a paragraph upon a printed page.

Although much that might be remembered incidentally might and would prove of value, the power or ability to discriminate that which could be used later is most worthy of cultivation.

THESIS XV

Children's Ideas of Right and Wrong

By Fannie G. Stearns

Thesis.—Having been placed in close contact with children during the last two years, I have become interested to know what the child's conception of right and wrong might be. In order to make a more intelligent study of the subject, I decided that by questioning children of grades one and two, five and six and nine, I might gain some insight into their opinions regarding right and wrong.

The questions asked of each child were these:

What is the worst thing a girl can do?

Why do you consider this wrong?

What is the worst thing a boy can do?

Why do you consider this wrong?

In all, over three hundred and fifty papers were received, and in answering the above questions the children gave their ideas concerning what they believed to be wrong for their own and the opposite sex. In some cases, particularly with the younger children, the same faults were mentioned for both sexes, but those who did this were in the minority.

The answers were carefully tabulated according to age and grade, and those having something in common, such as drinking, smoking, swearing, lying and cheating were placed in groups. The grouping according to age was as follows—from five to seven—from ten to twelve—and from thirteen to sixteen years of age. For convenience these periods will be considered as childhood—intermediate years—and adolescence.

The following are answers typical of each period. A boy six years of age said: "The worst thing a girl can do is to run away because mother would call and call and the girl wouldn't hear and mother would cry. The worst thing a boy can do is to set the house on fire because some one might not know and be burned up." A boy eleven years old wrote: "Lying is the worst thing a girl can do, because it is wrong anyway. To smoke or swear is the worst thing a boy can do because gentlemen never smoke or swear." A girl of fifteen years wrote: "The worst thing a girl can do is to disobey her parents. If a girl does just the opposite to what her parents want her to, she will most likely get into bad company and will be out on the streets acting rude and disorderly."

The principal faults named were classified in five groups. The total number of answers included in these groups was 513 and those mentioned came in this order: bad habits, including drinking, smoking and swearing, 168; disobedience, 102; having low social habits, 83; and stealing, 73.

Deceit was considered by girls as the worst fault for their own sex, while for the opposite sex they named bad habits, drinking, smoking and swearing. These two are doubtless the most common faults in each. Girls do not consider deceit a very bad fault for boys and place it fifth in the list for them. It is generally acknowledged that girls are more deceitful than boys and it is also conceded that it is the worst fault in women. That deceit is so prominent in their minds as to be avoided may be due in part to the fact that their elders warn them against it continually, and in part to natural tendency.

Girls name low social habits and disobedience as the next worst fault to be avoided by themselves, while for boys they put them in this order—disobedience and low social habits. That girls do not mention low social habits as being as bad for boys as for themselves is probably because this vice in boys is not emphasized so much by older people.

Girls place drinking, smoking and swearing as the fourth worst fault for themselves. As a rule they do not indulge in these vices, but when we do find a girl who has stooped so low we are stricken with horror. It is the unusual that surprises us most and so girls even though they seldom see these vices in their own sex are impressed with the fact that they are to be dreaded in girls.

The girls believed that stealing was the fourth worst fault for boys but placed it fifth for themselves.

We will now consider what vices are worst according to the opinions of the boys. They say that drinking, smoking and swearing are the worst for both boys and girls. In this they seem to make no distinction as to sex. Boys say that the second worst fault for themselves is cheating while for girls it is deceit.

Stealing seems to hold a very prominent place in the opinion of boys. They place it second for themselves and third for girls, while girls place it fifth for themselves and fourth for boys.

Vandalism stands as the third vice boys consider for themselves. It is scarcely mentioned for girls. Evidently the boys think of this as belonging only to themselves. The term vandalism is used to include fighting, hurting people and defacing property. It may be that boys do consider this as the third worst vice for themselves, but I think it was given this high position because of the stress school authorities had laid on defacing property just previous to the writing of these papers by the children. Many papers showed plainly the influence of this recent experience.

The following answer is typical of many given in a school where this vice had been particularly emphasized. "The worst thing a boy can do is to go to school and then deface the building by cutting it and marking it up. I consider this a wrong thing because the city gave the children the free use of the school and the children ought to treat the city in the way the city treated the children."

According to boys, disobedience is equally bad for both sexes while they consider deceit worse for girls than for themselves.

We found that girls gave low social habits a prominent place as something to be avoided by both their own and the opposite sex. On the other hand boys place it at the end of the list for boys and girls. That boys give it this obscure place is probably due to the fact that their attention has been called to it less than is the case with girls.

We find that, as to the three periods of development, drinking, smoking and swearing was denounced by the girls as being a worse vice in intermediate years than in the two other periods. Their beliefs during childhood and adolescence regarding this vice are about the same. Girls from five to seven years of age do not realize the meaning of these habits. They see older boys and men smoking and it means practically nothing to them. They see an intoxicated person and the sight serves merely as an amusement. When they have reached adolescence, other vices which affect them more closely, seem worse.

The conception of drinking, smoking and swearing as the worst things a child can do reaches its climax in the opinion of boys from the ages of ten to twelve years. During childhood it is less than half as prominent and in adolescence it ranks much lower than during intermediate years. This may be due to the fact that during childhood it does not generally enter into his experience. As he reaches intermediate years he is easily influenced by what he sees and hears and it seems an astounding wrong and a vice to be abhorred. Later the experience becomes common among his friends. He desires to become a man and considers that the outward signs make this an enviable condition. This is particularly true of smoking and swearing. The commonest of these habits is smoking and so it no longer seems wrong or a fault, but an incident, an event in becoming a man.

Disobedience as the worst fault in the child's opinion is most prominent from five to seven years for both boys and girls. Little girls however think it worse than little boys. And have we not found it true in our experience with small children that girls rather than boys think it a greater sin to disobey? This fault holds rather an insignificant place during intermediate years and adolescence, and is practically the same for both boys and girls.

Girls during the intermediate period do not consider being deceitful a bad fault and at this age many use deceit when they think it is for their advantage. In childhood they consider lying worse than they do in intermediate years. This may be explained by the fact that small children probably consider lying the same as disobedience.

Deceit for boys is believed to be worse by themselves during adolescence. At this age they seem to realize that a person whose word can not be relied upon will not get along in the world for no one will place sufficient faith in him even to give him work.

When we consider the five worst faults for boys and girls of all ages we find that girls believed that low social habits was worst for themselves. The figures show that in childhood they consider this worst. That this fault should be most prominent during childhood in the opinion of girls seems almost impossible, but it is to be explained in this way. Of all the faults mentioned by girls from five to seven years of age running away was considered the worst. Running away was included in my classification under the general term social habits, hence the high place that low social habits holds in the minds of girls from five to seven.

This having been disposed of we may safely say that low social habits as the term is commonly used stand highest as that to be avoided in the opinion of girls during adolescence rather than in childhood or intermediate years. With boys this stood highest during intermediate years and lowest during adolescence.

Stealing was not mentioned at all by girls from five to seven and stood highest from thirteen to sixteen years. That it is highest during adolescence seems to show that at that period in life girls come to realize what it means to be looked upon as a thief. Stealing is looked upon by boys too as being worst during adolescence.

Girls from five to seven believe the vice to be guarded against most is disobedience, from ten to twelve bad habits—drinking, smoking and swearing and from thirteen to sixteen deceit. With the exception of bad habits, considered worst during intermediate years, the faults that they named as being worst for themselves are probably the ones they yield to most.

Boys from five to seven believe disobedience to be the worst vice, from ten to twelve and from thirteen to sixteen bad habits. Boys from thirteen to sixteen give stealing a very prominent place also. I believe that boys, too, indulge most largely in those vices which they say they believe to be the worst for themselves.

As to the reasons why the things named were worst the answers given seemed to divide themselves into four classes, namely, the social, the law abiding, the character and the punishment groups.

From the results obtained by a comparison of the four groups it seems that the largest number of children, of all ages and both sexes together, are prevented from wrong doing by the fact that if they are guilty of certain faults it will prevent them from developing a good character. Many answers in this group gave the idea that certain acts were wrong for children not so much because of the doing of the single act but because of what it would lead to.

A boy twelve years of age wrote: "The worst thing a boy can do is to steal. I consider this a very bad beginning for a small boy." While a girl of fourteen years says: "I think the worst thing a girl can do is to cheat. A person who cheats once will cheat every time they get the chance."

For children, of all ages and both sexes counted together, those motives classed as law-abiding were very nearly as prominent. In this division were included reverence, home training, wrong and the Ten Commandments. Typical answers grouped under each of these particular headings may prove of interest here.

"I think the worst thing a boy can do is to say or do anything against his mother because your mother is the best friend you have. If anything goes wrong with you she will protect you."

"The worst thing a boy could do would be to deliberately rob any person. That boy ought to be fined and imprisoned because a boy who has had a proper training at home ought to know better."

"The worst thing a boy can do is to go with girls and swear. I consider this wrong because girls are very giddy and swearing is very wrong."

A girl of twelve years writes: "The worst thing a girl can do is to swear because it is taking the name of God in vain. The worst thing a boy can do is to steal because it says in the Bible "Thou shalt not steal."

Next in prominence was the social group which includes public opinion, reputation, approval and desire to please. A girl of thirteen years influenced in her moral ideas by public opinion wrote: "I think the worst thing a girl can do is to go around with a tough crowd for it will make her name quite known and she will have to work very hard to enter any decent society." A boy of fourteen years of age believed that the worst thing a boy could do was to get into bad company because it would give him a bad reputation.

A little girl influenced in her opinion of what is wrong by the approval of older people said she thought the worst thing a girl could do was not to be good because mama wouldn't like her.

The following answer might be classed under desire for approval or desire to please. "The worst thing a boy or girl can do is to whisper in school because teacher don't want you to."

Although it has been stated that the groups showing the motives most prominent, in the minds of children of all ages and both sexes together, were first—character, second—law-abiding, third—social and fourth punishment, it may also be stated that the first three were about equally prominent while the fourth ranked far below.

We will now separate boys from girls, still massing all ages together, and compare the motives expressed by boys and by girls. Girls are influenced first by the social motives while with boys obedience to law is the first requisite. I believe that both have named the things which do influence them most in their actions. Who can show us the person who has observed children widely and who will not say that girls are influenced in their actions first by what people will think of them, while boys are influenced most by whether they are keeping within the law or not, not merely law as laid down by our courts but by the moral law.

With boys social motives come second while with girls law abiding motives hold second place.

As to the advantage of possessing a good character both boys and girls agree and both would strive for it equally. This is probably the result of moral training together with their innate goodness. In this we in part agree with John Locke, the English philosopher, who said that people are naturally good and that they develop other than in the right way owing to contact with vice in the world.

Punishment as a prevention of wrong doing holds an equally small place with both sexes.

We will now see what place these four motives hold in the minds of children during childhood, intermediate years and adolescence.

The motives placed in the social group are most prominent in childhood and least prominent during intermediate years. They stand very much higher in girls than in boys during this period. We found that girls of all ages placed these higher than boys. Many people believe this is due to the fact that girls are trained in such a way as to make them more sensitive to public opinion. But that during childhood the social motives stand 67 per cent. in girls and only 19 per cent. in boys points to the conclusion that girls are naturally more sensitive than boys in this respect, for I do not believe that up to the time boys and girls are five years old their training in this respect has been any different.

In girls the law-abiding motives are equally prominent in childhood and intermediate years and less so in adolescence. That is, girls come to realize in adolescence the importance of having a good character. By girls punishment as a preventative of wrong doing was scarcely mentioned either in childhood or adolescence but was most prominent during intermediate years. This may be due to the fact that parents think they find more occasion to punish children of this age and act according to their belief. It may be explained in part also by the fact that to children of this age immediate punishment for their sins seems worse than some consequence that although it might not happen for a longer time would really be more disastrous and far reaching in the end. This would be realized by children in adolescence while in early childhood they think it much worse not to be liked than to be punished.

In regard to the ages when conformity to law seems most necessary, boys and girls agree very closely. Regarding punishment the prominence in the different ages agrees for both sexes. In childhood, however, boys place it higher than girls.

During childhood, intermediate years and adolescence girls are kept from wrong doing by the opinion of others, conformity to stated rules and the necessity of having a good character, respectively. During the same periods of life boys are guided by conformity to stated rules during the first two and in the third by the necessity of possessing a good character.

In conclusion let us see how parents and teachers may take advantage of the child's ideals and appeal to him along the line of his own motives for avoiding wrong. Justice is the primary virtue of all races and not until the coming of Christ did the world ever hold the conception of mercy and not sacrifice. So to a child justice is the primary virtue and the great ideal. To his mind it means doing according to his reasoning. So if we can meet the child on his own moral ground, win him through his own conceptions, encourage the good motive and thwart the low motive we have won our child.

Let us first consider his childhood. The motive and ideal here is naturally simple and easily reached. We found that girls from five to seven years of age are influenced most easily through the social motives—desire to please and approval.

When the children enter the school for the first time we must influence them in such a way that they will wish to please us. In order to do this we must win their affection. We can express our approval of their little efforts to do right and teach them the kind of reputation we wish them to covet by means of stories such as Joseph and other Bible heroes and Baucis and Philemon.

The teacher whose pupil said it was wrong to whisper because teacher don't want you to, had appealed in some way to a strong motive in the little one.

The little boys are also strongly influenced by obedience to law be that law ever so simple. The boy who said he must not drop chalk because it was wrong was loyal to the teacher's law. So it would seem that they are willing to take our superior knowledge for granted and abide by our laws and advice.

Next we will consider the intermediate period, for all children must pass through the three stages—childhood, intermediate years and adolescence. The motive appealing to girls during intermediate years has changed to the law abiding although the social motives still remain strong. The girl twelve years of age who says it is wrong to swear because it is taking God's name in vain has grown some since the period when personal approval was her motive for avoiding the wrong act. In a few years more she will be influenced by all the new feelings and ideals of adolescence and we must recognize the bridge between the two and not swing too far either way. We have a right to appeal to our girls in the fifth and sixth grades by examples of the lives of noble men and women who have sacrificed themselves to obey the higher law of conscience or the voice of God. Stories from the Greeks and Romans may prove invaluable in these grades.

The boys and girls of this intermediate period agree, so that any well founded system of law in which the children have some part, that is recognized as universal law, will appeal strongly to both sexes.

Now we come to the great period of change known to the psychologist as adolescence. It is the most trying period of a child's life. Everything changes—body, mind and spirit—and we find our girls and boys leaving previous ideals behind and reaching toward the great ideal of manhood and womanhood. In both sexes the strong motive is character building or an appreciation of the great dignity and worth of life. Here we may in our eighth and ninth grades and in the high school introduce simple studies in "Every-day Ethics."

It is as though a plant grew up to the light and then found no food in its welcome rays. The children are seeking after character and they need more than example, they need instruction in the fundamentals of good character. The girl who thought it was wrong to cheat because it led to the habit of cheating was on the right road. The teacher could easily influence that girl along other lines by appealing to the truth she had discovered for herself that one wrong act leads to another or that one step in the wrong direction makes it easier to take the next.

TABLE XXVI

The Worst Fault, According to Girls

The numbers opposite each fault mentioned at the left give the per cent. of girls, of each age, who condemn this fault most strongly in girls and in boys.

	Worst Fault of Girls				Worst Fault of Boys			
Age	5–7	10–12	13–16	Total	5–7	10–12	13–16	Total
Number of girls judging,	*33*	*46*	*104*	*183*	*33*	*47*	*38*	*118*
Drink,			2	1		8	10	7
Smoke,		6	1	2				
Swear,	9	30	5	12	19	40	8	24
Group total,	**9**	**36**	**8**	**15**	**19**	**48**	**18**	**31**
Form bad habits,			4	2			10	4
Keep bad company,		4	2	2			3	1
Be bold,			2	1				
Be out late,		2	4	3				
Run away,	28			5	22	2	5	8
Kiss a boy,		2		1				
Be untidy,		2	3	2				
Be impolite,			1	1				
Be saucy,		16	6	7		2	7	4
Group total,	**28**	**26**	**22**	**24**	**22**	**4**	**25**	**16**
Be deceitful,		2	4	3				
Lie,	16	12	19	16	9		3	4
Cheat,		4	12	8			7	2
Group total,	**16**	**18**	**35**	**28**	**9**		**10**	**6**
Steal,		2	15	10		15	29	15
Group total,		**2**	**15**	**10**		**15**	**29**	**15**
Torture,			1	1				
Commit murder,			1	1			3	1
Take animal life,						2		1
Deface property,			1	1				
Slap people,	3			1	3			1
Throw dirt,	3			1	3			1
Fight,						15		5
Group total,	**6**		**3**	**5**	**6**	**17**	**3**	**9**
Disobedience,	19	6	9	10	22	8	5	11
Disrespect to parents,			4	2			7	2
Tease mother,	3			1				
Whisper,	19			3	19			5
Drop crayons,	3			1	6			2
Play truant,						6		2
Group total,	**44**	**6**	**13**	**17**	**47**	**14**	**12**	**22**
Lose self respect,			1	1				
Sin,		2		1				
Jump rope,		4		1				

TABLE XXVII

The Worst Fault, According to Boys

The numbers opposite each fault mentioned at the left give the per cent. of boys, at each age, who condemn this fault most strongly in boys and in girls.

	Worst Fault of Boys				Worst Fault of Girls			
Age	5–7	10–12	13–16	Total	5–7	10–12	13–16	Tota
Number of boys judging,	*32*	*52*	*91*	*175*	*31*	*54*	*52*	*137*
Drink,		6	8	6		3	6	4
Smoke,		34	24	23		11		5
Swear,	19	18	12	15	19	9	12	12
Group total,	**19**	**58**	**44**	**43**	**19**	**23**	**18**	**21**
Lie,		2	4	3	12	11	16	13
Cheat,		2	3	2			14	5
Group total,		**4**	**7**	**5**	**12**	**11**	**30**	**18**
Steal,	3	16	25	19		22	26	18
Group total,	**3**	**16**	**25**	**19**		**22**	**26**	**18**
Disobedience,	19	2	6	7	12		2	3
Disrespect to parents,			4	2			4	1
Desert parents,		2		1				
Play truant,		2		1		5		2
Whisper,	12			2	6			1
Not look on book,					3			1
Be lazy,		2		1		7	8	6
Group total,	**31**	**8**	**10**	**13**	**21**	**12**	**14**	**15**
Have bad thoughts,						2		1
Gamble,			1	1				
Deface property,	6		8	5				
Kill a person,		6	1	2		2		1
Fight,		2		1		5	6	5
Have matches,	6			1				
Set fire,	3			1				
Hurt people,	19			3	19			4
Get on track,					6			1
Go out in rain,	3			1	13			3
Group total,	**36**	**8**	**10**	**14**	**38**	**9**	**6**	**14**
Be out late,		2	1	1		5	6	4
Go with girl,		4	1	2				
Go with boy,						3	4	3
Kiss a boy,						2		1
Be saucy,		2	1	1		4		1
Run away,	9			2	6			1
Group total,	**9**	**8**	**3**	**5**	**6**	**14**	**10**	**10**
Sin,		2		1		5		2

TABLE XXVIII

Why Faults Are Wrong, According to Girls

The numbers opposite each reason indicated at the left give the per cent. of girls, of each age, who assign this as the reason for the badness of the worst faults of girls and of boys.

	Faults of Girls				Faults of Boys			
Age	5–7	10–12	13–16	Total	5–7	10–12	13–16	Total
Number of girls judging,	*34*	*43*	*108*	*185*	*34*	*49*	*47*	*130*
Public opinion,	20	18	15	17	20	16	11	15
Reputation,		9	16	12		10	15	9
Approval,	18	2		3	12			3
Desire to please,	32			6	32			8
Group total,	**70**	**29**	**31**	**38**	**64**	**26**	**26**	**36**
Reverence,		2	6	4			2	1
Home training,		7	11	8			7	2
Wrong,	26	7	5	9	29	4	6	10
Ten commandments,		25	3	8		14	4	6
Group total,	**26**	**41**	**25**	**28**	**29**	**18**	**19**	**21**
Character,			12	6			19	6
What it leads to,	3	16	29	21		30	36	24
Group total,	**3**	**16**	**41**	**29**		**30**	**55**	**31**
Punishment,		12	2	3	5	24		11
Group total,		**12**	**2**	**3**	**5**	**24**		**11**

TABLE XXIX

Why Faults are Wrong, According to Boys

The numbers opposite each reason indicated at the left give the per cent. of boys, of each age, who assign this as the reason for the badness for the worst faults of boys and of girls.

	Faults of Boys				Faults of Girls			
Age	5–7	10–12	13–16	Total	5–7	10–12	13–16	Total
Number of boys judging,	*32*	*55*	*86*	*173*	*31*	*53*	*50*	*134*
Public opinion,	3	9	10	9	10		28	12
Reputation,		5	7	5		2	22	8
Approval,	15			3	10			2
Group total,	**18**	**14**	**17**	**16**	**20**	**2**	**50**	**23**
Reverence,			6	3		17	4	8
Home training,	12	7	11	10	6	14	10	10
Wrong,	46	5	4	12	46	10	10	19
Ten commandments,		18	2	7		4		1
Group total,	**58**	**30**	**23**	**32**	**52**	**45**	**24**	**40**
Character,		7	13	9				
What it leads to,	12	29	39	33	3	29	26	22
Group total,	**12**	**36**	**52**	**40**	**3**	**29**	**26**	**22**
Punishment,	9	11		5	16	21		12
Group total,	**9**	**11**		**5**	**16**	**21**		**12**
Humanitarianism,			1	1				
Prudence,			3	2				
Virtue,		12		2				
Example,					6			1
Group total,		**12**	**4**	**4**	**6**			**1**

Theses XVI and XVII

Thesis XVI., by Frances Denis Smith, and Thesis XVII., by Grace I. Davis, were based on the study of individual children. Space can be spared to quote only the definitions given by the little girl studied by Miss Davis for several years.

When she was five, she defined school thus:

"School is made of brick and it is big."

At ten she says: "School really is a place to learn. You have to go to school until you are fourteen and when you have examinations, that's what shows how much you know. I have just had mine and I got E in them all."

At five—"Bee is a bird that I never see."

At ten—"Bee is larger than a fly and buzzes louder. It has one eye right in the center of its face and a hair horn each side of it. It has yellow on its tail and it will sting, anyway the one I did see did."

At five—"Lady is a growed up woman."

At ten—"A lady is anyone that knows good manners and uses them, that is what my father says is a lady."

At five—"Water is what we drink and you can't pick it up and you can't hold it in your hands at all. It runs way off with the dam and it can swim and float along and it is water."

At ten—"Water is a liquid. It isn't white but it is the color of glass and it looks like glass. It comes from the clouds and makes rivers. Rivers have whirly places that drown people. We could not live without water."

At five—"Dogs have four legs. Some have long hair and some have not. Some are big dogs and some are just little pups."

At ten—"A dog is a little four footed animal and knows more than any other animal I know. St. Bernards save people."

At five—"A fish has six wings, two eyes and a mouth, and a flat back and some are gold fishes and have a head—about a hundred inches long and it flies into the water and has two cuts in the sides of the head and these cuts shake all the time."

It might be well to add that when she formed this definition she had a globe containing gold fish before her.

At ten—"A fish has a long fin on its back and three on the underside and lives in water. It has a place cut on each side where it breathes and its mouth is most always moving."

At five—"A chicken is a little hen—wears white stockings and is good for white eggs."

At ten—"A chicken is a little hen, some people call them fowls. The hens lay eggs and if they are not too lazy hatch them into chickens."

At five—"Pride is to feel funny."

At ten—"Pride, well people that are proud think they are smart and make a lot of motions and people don't like them."

At five—"Moss is the most like grass or hair of anything I know of."

At ten—"Moss is damp green stuff. It doesn't grow as high as grass. Spanish moss is gray like gray hair and grows long and is stringy and hard to break."

At five—"Man, first God borns them and they are little and cry and grow until a hundred and seventy years if they don't die before, sometimes they do."

At ten—"A man is a master. A fellow is not a man until he is thirty-five or forty years old. Some men wear glasses, some men are five feet, six inches tall, some are fat and some are slim. Men have short hair and some of them have mustaches. You can tell a man from a woman by his dress and hat and he looks cross."

THE INACCURACY OF MOVEMENT

WITH SPECIAL REFERENCE TO CONSTANT ERRORS

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CONTENTS

THE INACCURACY OF MOVEMENT

INTRODUCTORY

THE student of the psychology of movement is, to say the least, not hampered by the novelty of his subject. Ever since the days of the muscle sense controversy investigator after investigator has interested himself in the subject of movement until a considerable body of motor psychology, in turn acclaimed and condemned, has developed. The present study is not directly concerned with the implications of movement, nor with the relation of movement to mental processes with which an analytic psychology is largely concerned. As an experimental investigation it grew out of a number of interesting and not at once explicable observations of constant errors in exercises on the accuracy of the perception and reproduction of arm movements. The accuracy of movement has frequently been the subject of special study, from different points of view and not infrequently with varying or inconsistent results. As has often been pointed out, this inconsistency is partly due to the extreme complexity of the sensations aroused by the movement of the parts of the body usually employed—chiefly the upper and lower limbs and the eyes. Introspective analysis of the sensation of movement is exceedingly difficult. Coming, as it does, from a great number of sources—the muscles, ligaments, tendons, articular surfaces and skin—and closely associated as it is with the spatial order of other senses, particularly that of vision, it seems to present a highly complex fusion, the components of which do not readily yield themselves to the efforts of introspective discrimination.

Further, as will be more fully developed in a later chapter, the process of recognition and judgment of extent seems to consist, first, of a reference of the movement to a familiar and rather loosely defined group, followed by its approximate recognition as this or that movement. Many of the constant errors and illusions of movement may be found to depend on the nature of this process. A third source of discrepancy in comparative results is shown by the present experiments to lie in the methods of control and record used. The present experiments seem to indicate that every movement as judged tends to fall into its proper place in the objective scale of magnitude as determined on some other basis than the intensity or extensity of the accompanying sensations, or of the stimulus, though not entirely without reference to these factors. Investigation of the relations and interdependencies of these objective characteristics and of the influence any one of them may exert on the judgment of any other are not without interest. Not a few such researches have been conducted, but there are still questions that have not been satisfactorily answered, sources of error that have not been sufficiently regarded, contradictory results that have not yet been cleared up, and interesting phenomena which seem to have escaped observation. Theories as to the more fundamental character of judgments of time or of extent have not been tested in any direct way, illusions of over and under estimation have not been satisfactorily accounted for, statements of the "law of forgetting" for spatial and temporal magnitudes can not yet be generalized, no convenient apparatus for recording simultaneously the extent, speed, duration and force of movements of any considerable magnitude has yet been described.

The experiments to be reported in the following chapters were conducted with the following chief things in mind:

1. The desire to find the most satisfactory method of recording and studying voluntary movements of the limbs, and to construct a simple apparatus which would measure, simultaneously, all the attributes of any single movement.

2. The determination of the relations existing between the perceptions of extent and of duration or speed.

3. The analysis of the basis of the positive and negative constant errors involved in the phenomenon of the shifting or periodic "indifference point."

4. Investigation of the extraordinary positive constant errors produced by the force of impact.

5. The testing of the "duration" hypothesis in the constant errors of the Loeb illusion.

6. The need for further data on the memorability of spatial and temporal perceptions.

7. The unsatisfactoriness of most expositions of the basis of the judgment of equality or difference in the case of space magnitudes.

The experiments were performed in the Psychological Laboratory of Columbia University during the years 1907–9, under the directions of Professors Cattell and Woodworth. The writer wishes also to express his obligation to Professor T. L. Bolton, of the University of Nebraska, under whom he received his first scientific training and to whom he owes his first interest in the psychology of movement.

CHAPTER I

Methods of Studying Movement

In this chapter will be considered some of the methods employed by various other investigators in this field, and a piece of apparatus especially constructed for the present research will be described—an instrument which seems in many ways to possess advantages over the apparatus heretofore used. In the succeeding chapters will be reported the experimental results of the investigations undertaken, with a discussion of their significance for the general problem of movement.

(*a*) Extent of Movement

The study of method, always important to the success of an experimental investigation, has special significance in the psychology of movement. The divergent results of different investigators in this field seem more frequently to indicate the peculiar influence of the method employed than to display the character of the perception of movement as such. The first and most comprehensive study of method in the field of movement, that of Cattell and Fullerton,[1] was concerned chiefly with the investigation of the reliability and influence of the various psycho-physical methods, the mode of judgment and of record. These authors conclude that "the method of average error—in which the observer makes one stimulus as nearly as possible like another—is in many cases the most convenient of methods,"[2] and this method of average error was employed throughout the present study. But the chief divergence between the results of different authors seems to have been caused by differences in the objective, instrumental methods employed, and these methods require more thorough investigation before many results can be completely interpreted.

The controversy over the question of rectilinear and curvilinear movement seems to be still undecided. Külpe[3] and Angier[4] insist that all the work on rectilinear movement is worthless, and that the whole matter must be worked over, using movements of the curvilinear type—rotations of a single joint in a single plane, while Woodworth[5] has clearly pointed out that "the force of this objection is

[1] "On the Perception of Small Differences," 1892.

[2] *Ibid.*, 151.

[3] "Outlines," 341.

[4] *Zeit. f. Psychol.*, **39**, 430, 1905.

[5] "Le Mouvement," Paris, Doin, 1903, 89.

more apparent than real." The matter need not concern us here, since we are not interested in the statement of the absolute quantitative accuracy of either the perception or the reproduction of movements, nor in a determination of individual differences, nor in any attempt to give topographical location to the source of the sensations of movement. In only one case, that of the experiments on the degree of contraction, would the type of movement used suggest any possible source of error for the problem set. But since other experimenters have shown the Loeb illusion to occur with curvilinear as well as with rectilinear movement, even here the type of movement is irrelevant to the topic under consideration.

The methods of active and passive movement have also frequently led to somewhat different results, but it has been generally recognized that the two situations are qualitatively different, and this disparity of method has seldom led to erroneous interpretation. In fact this has happened only in cases in which the block method, next to be discussed, was employed in the active movements and the peculiar error characteristic of this method allowed to pass unexamined.

The traditional method of controlling the extent of a movement to be judged is by impact of the moving member or the carriage against an upright. The possibility of error in the use of this method has already been suggested incidentally by Titchener[6] and by Segsworth.[7] The latter regrets that no other method is possible unless shadows, photography or some other such optical apparatus be employed. The objections made to the method are that groups of other sensations, consisting of contact, pressure and resistance, are brought about, and "complicate the judging of the pure motion sensations."

The present study will show[8] that this method introduces a large positive constant error, which is a function, in part, of the force of impact against the block, and the magnitude of which causes a corresponding increase in the variable error. The only other principal method which seems to have been previously used is the "free" method, by which the subject makes a movement which is self-controlled as to extent and time. Then having made this free and predetermined movement, another movement is made which is to be equal to the first. The faultiness of this method is apparent. What the subject tends to do is to endeavor to make two movements according to a mental standard, which may even be so standardized as to be expressed in inches or millimeters. It is not, in this case, a matter

[6] "Exper. Psychol.," Vol. II., Pt. 2, 260.

[7] *Amer. Jour. of Psychol.,* **6**, 369, 1894.

[8] Chapter II.

of the reproduction of a previous movement. Indeed the perception of extent hardly enters except as the subject is required, after having made the two movements, to indicate their relative magnitude—to guess at his probable error. In this case it is at all events hard to secure well-distributed and uniform records.

From still another point of view there are two methods, both of which have been used by different investigators. These may be called the continuous and the successive methods. With the continuous method, the starting point of the second movement coincides with the terminal point of the first one. The two movements are thus not only made with a different degree of contraction of the muscle, but in some cases different or additional muscles are brought into play. This would of course be no objection from the point of view of one who adheres to the joint sense theory. But as a matter of fact a constant error is here introduced, the nature of which will be pointed out in the next chapter. The other method also presents difficulties. Kramer and Moskiewicz claim that in reproducing from an identical starting point, a tendency to grope for the same terminal position results, and the feeling of movement reduces to a feeling of position. This tendency is clearly present in the case of some subjects. But it seems that one method or the other must be used, for no other practical alternative has yet been suggested.

The apparatus later to be described is designed to eliminate many of the errors arising from this diversity of methods.

(*b*) Time of Movement

Aside from reaction experiments, fewer studies have been made of the time of movement than of its extent. This has been chiefly on account of the difficulty of conveniently recording and controlling the time of movements of any considerable magnitude. Intrinsically the subject is of great interest. By means of the instrument devised for the present study the duration of a movement, its speed at any point in its course, as well as its extent, are graphically recorded. Moreover the movement may be as much as a meter in length, although extremely small movements are recorded with equal accuracy.

It may be well to point out the methods heretofore employed for the registration of this type of movement. The first investigators were Camerer[9] and Vierordt.[10] The subject was required to rest his fingers on the top of a brass rod, which was hinged at one end. The other end bore a writing point which recorded the movement on a horizontal rotating drum. This drum was turned by hand and the

[9] "Versuche üb. d. zeitl. Verlauf d. Willensbewegung," Diss., Tübingen, 1866.

[10] "Zeitsinn," Tübingen, 1868, 33.

time of the movement calculated on the basis of a time line afforded by an induction apparatus. With this method the extent of the movements studied was limited to a very few millimeters. Binet and Courtier[11] worked with rather limited writing movements, using an Edison electric pen. The needle of this pen, actuated by an eccentric at the rate of about 11,000 times per minute, pricked holes in a roll of paper. Interesting results were suggested, but on account of mechanical difficulties nothing very definite could be stated. The rate of 11,000 punctures per minute was too rapid to allow any accurate calculation. And if the rate was lowered the needle caught in the perforations, tearing the paper and interfering with its own regularity. Leuba,[12] in an unpublished study, describes an instrument to be carried by the index finger, the slightest movement of which makes a contact which is broken by lifting the finger into the air. The time is recorded on a kymograph drum. No experimental results have yet been reported, but since the mechanism involves a reaction time at the termination of the movement it seems probable that the natural course and character of the movement would be disturbed by this additional feature.[13] Jaensch[14] worked with a hollow pen holder containing a spring connected with a Marey tambour. The pen was pressed down by the subject at the beginning and end of each movement, thus recording the time of the movement on the drum by jerks in the line but necessitating a reaction time and a distracting performance at each end of the movement. Cattell and Fullerton[15] have described the instrument at present used in the Columbia laboratory in connection with a Hipp chronoscope. The beginning of a movement closes a circuit which is broken at the completion, thus registering the time from the beginning of the movement to the moment at which the circuit is broken. The chief difficulty in the use of this instrument and of the Witmer modification of it employed by Gault[16] will be pointed out in Chapter V.

(*c*) Force of Movement

In most of the work done on this subject—so far as I have been able to learn, in all except the work of Cattell and Fullerton[17]—the

[11] *Revue Philosophique*, **35**, 664, 1893.

[12] Fifteenth Report, Am. Psychol. Ass., 1906, 218.

[13] Leuba has since exhibited, before the American Psychological Association, December 20, 1908, a device for recording independently the extent and duration of forearm movements.

[14] *Zeit. f. Psychol.*, **41**, 257, 1907.

[15] *Op. cit.*, 103.

[16] *Am. Jour. of Psychol.*, **16**, 357, 1905.

[17] *Op. cit.*, 66.

Plate I

force of movement has been a direct function of the extent. This is especially true of the vast amount of work that has been done on the ergograph. Under these conditions it has been impossible to say how far the judgment was concerned with the pure force or energy of the movement and how far it had to do with the perception of extent as a secondary criterion. Consequently in constructing the instrument about to be described provision was made for the study of the force of movement under conditions which allow the perception of force to be made independently of the perception of extent.

(*d*) The Apparatus and Method of the Present Study

As a foundation for the instrument the Cattell-Fullerton apparatus for the study of extent of movement was used. This has already been described by these authors as consisting of "a brass plate one meter long, graduated to millimeters and grooved for the wheels of a small brass carriage (see Plate 2). Along the scale is a wire, carrying an indicator (*i*) which is moved by a bar (*b*) attached to the carriage. Between the front and back wheels of the carriage, and parallel with the track, is a ring (*t*) into which is inserted the finger used in moving the carriage. . . . The carriage may be moved alone or used to raise any weight (*w*) attached to the cord."[18] For the purposes of the present experiments a number of modifications have been made. In order to more completely eliminate the noise made by the moving carriage, wood-fiber wheels have been substituted in place of the original metal ones. When a little machinist's oil is placed in the grooves of the track, the car now runs smoothly and noiselessly. In place of the original uprights for controlling the extent of the standard movements, a sound hammer (*h*, Plate 1) is arranged in circuit with the car and the indicator (*i*) which slides on the wire. The contact made by the bar running out from the carriage and the brass indicator completes an independent electric circuit which runs through the hammer magnet. The stroke of the hammer serves as a signal for the stopping of the movement. The constant error of impact, later to be pointed out, is thus avoided, and whatever reaction time is involved is included in the original time and extent of the movement.

For recording the duration of movements the following device is employed. To the top of the carriage is attached a signal magnet (*m*) which controls the vibrations of an enlarged Pfeil time marker (*s*). The magnet circuit is interrupted by means of a reed oscillator (*v*), vibrating at the rate of ten times per second. This gives ten main vibrations of the time-marker per second (c^1). But in order

[18] *Op. cit.*, 35.

to make interpolation easier and more accurate a thin piece of rubber is glued on the face of the magnet core (*m*). This produces a rebound (c^2) of the spring of the time-marker in the middle of each main vibration. The tenths-of-a-second curve, produced, when the carriage is moved, by the simple vibrations, is thus transformed into a twentieth-of-a-second curve, each tenth being represented by a large deflection of the tracing point, and each intervening twentieth by a somewhat smaller deflection. By interpolating within these twentieths, the time of a movement can be determined to within one hundredth of a second.

The writing point (*p*) of the time marker consists of a thin brass extension terminating in a piece of flexible gelatine. The record is made on a smoked paper (*x*) stretched on a horizontal frame which slides underneath the track from the side of the apparatus on which the operator stands. This frame is made of well seasoned wood, and is prevented from warping by means of a thin steel lining running along both sides. Each side and end of the frame consists of four layers—first a layer of wood, then in turn a layer of cork, another layer of wood, and finally the steel lining. Over the frame is tacked a foundation of cardboard, which serves to support the glazed paper while it is being attached and smoked. The paper is stretched out over the frame and fixed in place by strips of cardboard. Thumb tacks through the cardboard and into the cork layer of the frame are easily inserted or removed. After the paper is thus fixed in place on the frame the smoking is easily accomplished by moving the inverted frame above the camphor flame.

For studying the perception and reproduction of the force of movement, the carriage is made to pull against a pair of coiled springs (*r*, *r'*), placed below the box which supports the track at the proper elevation for making convenient movements of the carriage. These springs are so adjustable that the force may be varied independently of the extent, but may be correlated with it empirically, and in a relation unknown to the subject. Thus the first or standard movement may be made against one spring at a given degree of tension, while the second movement may be made against a different spring, against the same spring, or against both. A pulley attachment (*a*) provides for the use of weights instead of springs if such an experiment is desired.

We may thus secure, simultaneously, a graphic record of the duration, speed, extent, and force of a given movement, along with an indication of any irregularities that may occur in its performance. The method of procedure is simple enough. By closing a convenient key (*s*, Plate 2) on the table before him the operator sets the time

8o

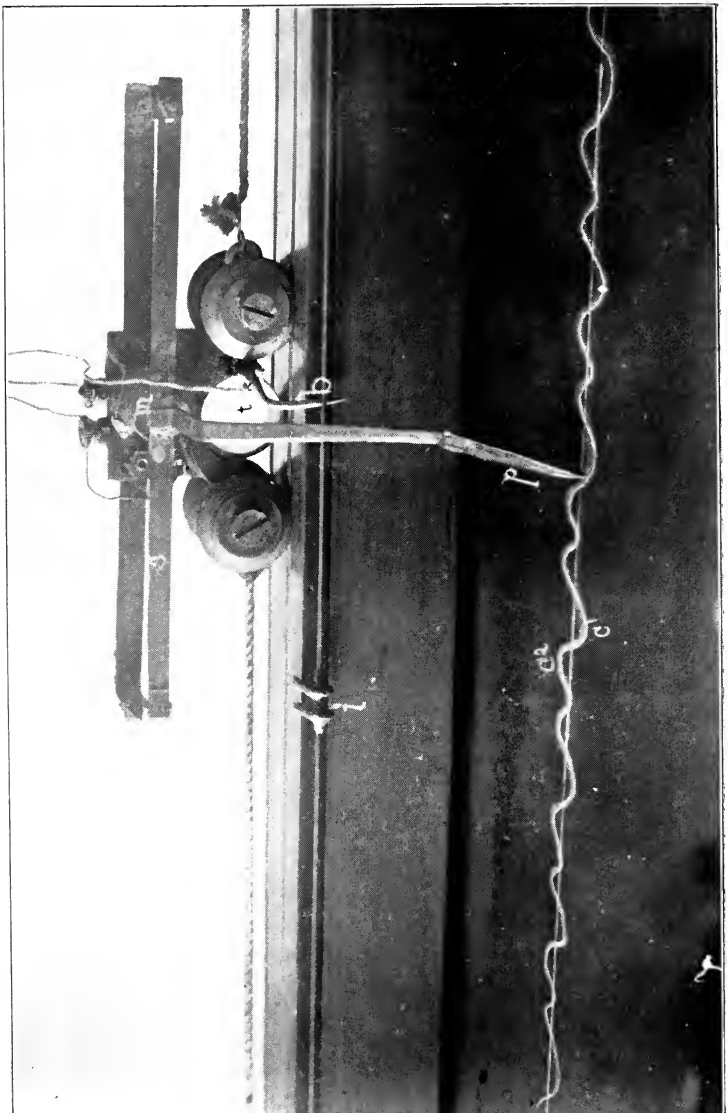

PLATE II

marker in vibration. At a signal from the operator or at an independently chosen moment, the subject begins his movement. At the sound of the hammer signal he stops the movement, and the hammer circuit is broken by the operator by throwing another switch (s') near at hand. Before the carriage is returned to the starting point the magnet circuit is also broken. During the movement the writing point has traced the compound time-curve on the paper. As the carriage is returned, the writing point traces a straight line which divides the previously inscribed record in such a way that the tenths of a second may be read off on one side of the line. For the rebounds of the Pfeil spring, when the current is off, come beyond the straight line, registering tenths of seconds. But the rebounds when the circuit is closed are of smaller amplitude, and come only to the straight line without crossing it. These vibrations are ignored when counting in tenths, but when counting in twentieths, both the vibrations (c') reaching beyond the line and those extending only to it (c^2) are regarded.

CHAPTER II

The Illusion Produced by Impact

In a series of experiments performed previous to those reported in this series, the traditional method of controlling the extent of the movement to be judged by blocking it by means of an upright was employed. It was soon observed that the impact of the moving car against the upright produced a large constant error in the reproduction of the movement. This constant error was always positive, and was frequently so great as to astonish the operator and to cause him to suspect that the observer was not paying the least attention to the experiment. But careful observations of half a dozen subjects showed that the illusion was present in all cases and experiments were made to test the direction, amount and persistence of the error.

In Table I., for Observer Lk., typical results are shown for free and blocked movements. In the case of the free movements the standard was in each case a spontaneous movement the extent of which was determined by the subject. A standard movement was made and then this standard reproduced as nearly as possible. The standard movements were deliberately varied between 75 and 350 mm. The error in per cent. was then calculated for each movement, and movements between 75 mm. and 125 mm. grouped under column 100 mm., movements between 125 mm. and 175 mm. under column 150 mm., etc. Thus, in Table I., the first column under each heading (100, 150, etc.) shows the average per cent. error for movements ranging around the magnitude indicated by the heading as central tendency, and not deviating from this magnitude by more than

TABLE I

Free and Blocked Standards

Observer Lk.

		100		150		200		250		300	
		%	mm.	%	mm.	%	mm.	%	mm.	%	mm.
Free.	A.E.	23	23	14	21	14	28	10	25	9	27
	C.E.	+ 18	+ 10	+ 10	+15	+11	+ 22	+ 7	+18	— 4	—12
	V.E.	*12*	*12*	*13*	*20*	*10*	*20*	*12*	*30*	*8*	*24*
Blocked.	A.E.	155	155	110	165	60	120	30	75	15	45
	C.E.	+155	+155	+110	+165	+60	+120	+30	+75	+15	+44
	V.E.	*28*	*28*	*27*	*41*	*15*	*30*	*13*	*33*	*9*	*27*

25 mm. The second column gives the error in mm., found by multiplying the central tendency by the average error in per cent. In the trials recorded in the lower part of Table I. the subject was simply directed to move along the track until his movement was blocked. The upright was then removed and the movement was continued, in an endeavor to make the two extents equal. The table gives the gross average error, the constant error and the variable error, in both mm. and in per cents of the standard, fifty trials being made of each magnitude under each type of movement. In both cases the "continuous" method was used—the terminal point of the first movement serving as the starting point of the second. Any other method would here interfere with the illusion. Thus if the car had been returned to the initial position and the second movement made over the same stretch of track, the illusion produced by the impact would tend to be partially corrected by the more careful and precalculated movement back to the starting point.

The upper half of Table I. gives the records for the free movements. The constant error for observer Lk. is seen in this case to be slightly positive except for the largest movement, where it becomes negative. It never becomes greater than 22 mm. and the variable error, except in one case, is less than 25 mm. The lower half of the table gives the results for the blocked movements, in which the subject started to move along the track, knowing that at some point he would be blocked by the upright, but being in no case aware of the point at which the block was to occur. In these experiments the constant error is always positive, and becomes from two to eight times as large as in the case of the free movements. Indeed, in the case of the 100 mm. and 150 mm. movements, the positive constant error is larger than the original standard, meaning that the reproduced movement was more than twice as long as it ought to have been. As a consequence of this large constant error we come to deal with quantities of much greater magnitude than with the free standards, and the variable error becomes correspondingly larger, becoming now as large as 28 per cent., whereas before it never exceeded 15 per cent. It is obvious that under such conditions we are not studying the normal accuracy of movement, but are measuring the effect of impact on the perception of extent.

That this is true is shown conclusively in Table II. The purpose of these experiments, on another observer, was to discover in what degree the illusion is a function of the force of impact. At the bidding of the operator the observer started with the intention of moving one foot, two feet or three feet as the case might be. By this procedure the speed of the movement was varied quite uniformly, since

TABLE II

INFLUENCE OF FORCE OF IMPACT

Observer Hl.

Blocked at	10 cm.			20 cm.			30 cm.	
Intent to move.	1 ft.	2 ft.	3 ft.	1 ft.	2 ft.	3 ft.	2 ft.	3 ft.
Speed.	68	100	110	32	120	138	103	155
A.E. of speed.	3	2	6	9	7	5	9	8
C.E. per cent.	+138	+174	+171	+100	+158	+166	+90	+132
V.E. per cent.	*30*	*42*	*41*	*24*	*38*	*32*	*24*	*28*

large movements tend to be made more rapidly than smaller ones, and the variations of speed would of course make a corresponding variation of the force of impact against the upright. In some cases the movement was blocked at 10 cm., at 20 cm. or at 30 cm., at the option of the operator. As a matter of fact, a chance order was adopted throughout, care being taken that in the long run the same number of each kind was given. This number, as in the previous experiment, was 50. But the movement was not blocked in all cases. In 50 cases for each magnitude the subject was allowed to actually make the movement of 1 foot, 2 feet or 3 feet, which was his original intention. Then after the regular interval he went on to reproduce this movement. The records for these trials are given in Table III.

TABLE III

SHOWING AVERAGES IN MM. OF FREE MOVEMENTS (1) INTENDED TO EQUAL 1 FT., 2 FT. AND 3 FT., AND AVERAGES OF REPRODUCTIONS (2) OF THESE FREE STANDARDS, WITH ERRORS IN PERCENTAGE

Observer Hl.

To move	1 ft.	2 ft.	3 ft.
Av. 1 (mm.).	217	371	485
Av. 2 (mm.).	259	385	454
A.E. per cent.	19	4	6
C.E. per cent.	+21	+6	—6
V.E per cent.	13	12	11

After having made these experiments, the actual speed at the various points of blocking, under the different conditions, was computed on the basis of ten movements of each of the standard magnitudes. The speed, in each case, is given in terms of mm. passed over during the twentieth of a second preceding and the twentieth of a second after the particular point of blocking in question.

Examination of the tables discloses several points of interest. Thus, in Table II., reading across on the level of any one block point, as at 20 cm. under 1 foot, 2 feet and 3 feet, the positive constant error is seen to increase directly with the force of impact as indicated in terms of speed or velocity, + 100 at speed 32, + 158 at speed 120 and + 166 at speed 138. Whether this increase is proportional or not can not easily be made out, because, since the continuous method was used in the reproductions, the second movements were in each case subject to the negative error pointed out in the chapter on the influence of the degree of contraction (Chapter VII.). This of course means that the positive error is in all cases really greater than it appears from the record, since, in addition to producing a positive error, it has counteracted the normal negative error.

The speed curve of ordinary movements of a given extent has been found to be rather uniform and typical.[1] The movement begins gradually and increases in velocity until about the middle of the extent, slowing down again as it approaches the end. "In all cases the middle point of the extent coincides almost exactly with the mid-point of the duration." Moreover, the maximum speed attained in executing a normal long movement is higher than that of an equally normal movement, made under the same circumstances but of less extent. The average speeds of the movements in the present experiments were found to be for the 1-, 2- and 3-foot standards, 70, 103 and 113 mm. respectively, per tenth of a second. Thus, when the observer intended to make a movement of 2 feet, the speed at 10 cm. averaged 100 mm. per .1 sec., with a M.V. of only 2 mm. At 20 cm. the movement had attained a speed of 120 mm. with a M.V. of 7 mm., while at 30 cm. the speed was decreasing as the movement approached its goal, averaging, at this point 103 mm., with a M.V. of 9 mm. The 20-cm. point would thus seem to be approximately the mid-point of the movement, although the subject felt himself to be making a 2-foot (about 60 cm.) movement. If we refer to Table III. we find this to be really the case, since the average attempt to make a 2-foot movement averaged a little over 37 cm., and half of this extent does not take us far short of the 20-cm. point. Similarly the one-foot movement is slowing down at 20 cm., while the 3-foot movement is still increasing in speed at 30 cm.

Reading across under the corresponding columns of the three sections of Table II., the constant error seems to be rather independent of the speed at the different block points. Thus in the 2-foot column, when blocked at 10 cm., the C.E. is + 174; blocked at 20 cm., with higher speed (120), the C.E. is only + 158, while at the 30-cm. block, although the speed is still 103, the C.E. is but + 90. Similarly, in the 3-foot column the blocks at 10, 20 and 30 cm., with increasing speeds of 110, 138, 155, the C.E. decreases

[1] Binet and Courtier, *op. cit.*, 664.

through + 171, + 166, + 132. Although the errors are always positive, and strikingly so, in these vertical columns the greater error may occur when the speed is least. This seems to indicate that the same or a greater impact may mean, nevertheless, a smaller illusion, according as it stands near to or far from the end of the movement, but that it always means an illusion. The greater the amount of the movement already accomplished, the smaller the illusion. The indication seems to be that a movement checked by the block method half way towards completion is not the same thing mentally as a movement half as large as the original one, but at the same time a unit, beginning and ending under control. The first movement contains a variety of elements of distraction, chief of which are the original intention and the effect of impact.

The magnitude of the illusion seems thus to bear no exact mathematical relation to the force of impact, but to be highly complicated by other factors when such are present. But, other things being equal, the dependence seems to be direct and proportional. It is at least sufficiently clear that some method other than that of the block should be used in the study of movements. Consequently, in the experiments to follow, the signal method, already described (Chapter I.), is to be used. This method eliminates the elements of distraction and illusion, while at the same time enabling easy variation and control of the magnitude of the standard extent. All movements studied are then unitary movements, and can be properly compared with any other free movement.

There seems to be no possibility of making a general statement that will express in quantitative terms the effect of practise on the magnitude of an illusion of perception. Thus in recent contributions we find these two statements: "Practise affects the variable error but not the constant error"; "Practise decreases the magnitude of an illusion." Now a constant error is an illusion. Illusion takes place when an experience is taken to be what it is not. In these constant errors of movement we have just such a situation. An extent is estimated to be what it is not, and this seems to signify that some internal event, process or effect is also misjudged. It may be of interest to observe the effect of practise on the illusion of impact. Table IV. shows the result of seven days' practise, by another observer, without knowledge of results, and of seven days' later practise with knowledge. The procedure here was the same as in the former experiments, except that after the seventh day the observer was told immediately after each reproduction whether his second movement was "too short," "right" or "too long." (This was only in the case of the blocked movements.) No statement of the amount of the error was made. Each record in the table is the average of five trials for the particular magnitude on the day in question. Through the first week the constant error is seen to have increased quite uniformly from day to day, practise, in the sense of repetition, instead of decreasing the illusion having just the reverse effect. The V.E., however, remained, on the whole, rather constant.

TABLE IV

EFFECT OF PRACTISE ON THE IMPACT ILLUSION

Observer V.

Average of Five Trials for each Magnitude.		100 mm.		150 mm.		200 mm.		250 mm.		300 mm.		1907
		Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Dec.
Without Knowledge.	% C.E.	13	89	29	63	25	75	19	31	40	27	26
	V.E.	*11*	*20*	*10*	*14*	*8*	*23*	*9*	*10*	*7*	*7*	
	C.E.	24	171	41	111	15	51	10	35	7	22	27
	V.E.	*9*	*30*	*10*	*25*	*15*	*11*	*8*	*3*	*4*	*9*	
	C.E.	14	137	20	94	21	69	12	37	—13	33	28
	V.E.	*17*	*40*	*8*	*13*	*6*	*3*	*10*	*16*	*6*	*8*	
	C.E.	37	150	37	83	11	56	30	47	9	28	29
	V.E.	*14*	*16*	*7*	*24*	*10*	*6*	*7*	*11*	*7*	*6*	
	C.E.	30	154	28	92	27	48	43	43	26	41	30
	V.E.	*13*	*27*	*2*	*21*	*4*	*15*	*8*	*11*	*13*	*10*	
	C.E.	59	99	47	85	29	53	19	43	20	29	31
	V.E.	*19*	*25*	*7*	*15*	*8*	*5*	*11*	*12*	*6*	*10*	
	C.E.	43	123	50	97	43	63	28	47	15	34	Jan.
	V.E.	*18*	*44*	*5*	*26*	*6*	*19*	*5*	*5*	*7*	*10*	1
With Knowledge.	C.E.	37	63	44	60	32	31	30	2	13	7	2
	V.E.	*13*	*27*	*8*	*29*	*17*	*12*	*8*	*11*	*8*	*14*	
	C.E.	19	45	20	45	17	27	5	13	4	— 9	3
	V.E.	*6*	*32*	*13*	*10*	*11*	*10*	*5*	*10*	*16*	*8*	
	C.E.	10	74	34	43	26	7	15	5	5	— 5	4
	V.E.	*9*	*23*	*6*	*22*	*6*	*8*	*6*	*6*	*5*	*4*	
	C.E.	26	54	13	31	13	1	13	—11	16	— 6	5
	V.E.	*12*	*29*	*1*	*8*	*6*	*5*	*5*	*9*	*10*	*7*	
	C.E.	16	70	11	19	6	12	12	— 1	6	— 4	6
	V.E.	*8*	*26*	*6*	*12*	*5*	*4*	*4*	*16*	*3*	*12*	
	C.E.	16	63	24	21	19	18	23	8	6	— 5	7
	V.E.	*10*	*13*	*18*	*11*	*8*	*15*	*9*	*5*	*5*	*9*	
	C.E.	24	47	35	25	21	6	12	— 8	4	—10	8
	V.E.	*9*	*18*	*6*	*12*	*3*	*11*	*6*	*11*	*8*	*9*	

During the second week the effect of knowledge was simply to shorten all reproductions. For the shorter movements the C.E. thus became smaller but remained positive throughout, while the previous positive error for the long movements became decidedly negative.

Apparently the effect of practise, as well as of knowledge, is not to decrease the illusion, but to provoke a deliberate shortening of the reproductions against the observer's own judgment. The C.E. for

TABLE V

EFFECT OF PRACTISE ON THE IMPACT ILLUSION

Observer Chr.

	Average of Five Trials for Each Magnitude	100 mm.		150 mm.		200 mm.		250 mm.		300 mm.		1907.
		Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Fr.	Blk.	Dec.
With Knowledge.	% C.E.	31	224	12	141	24	157	9	75	—1	69	1
	V.E.	6	24	4	44	13	14	11	9	7	17	
	C.E.	13	214	25	90	28	113	—5	52	2	43	2
	V.E.	6	20	13	18	9	29	8	21	6	20	
	C.E.	7	99	—4	52	10	25	—3	22	—1	9	3
	V.E.	14	18	10	19	13	4	6	12	7	5	
	C.E.	—3	39	0	54	0	40	0	17	10	2	4
	V.E.	13	15	17	30	12	13	7	10	16	8	
	C.E.	2	88	9	24	7	35	—4	18	—7	13	5
	V.E.	10	42	8	17	18	34	9	19	7	8	
	C.E.	1	52	1	37	—1	3	—14	4	2	—3	6
	V.E.	13	40	14	8	10	5	4	4	6	11	
	C.E.	7	64	2	46	—10	31	—5	25	3	2	9
	V.E.	13	40	11	16	5	7	12	11	7	14	
	C.E.	—12	18	—12	33	—2	10	—5	9	—1	1	10
	V.E.	6	23	7	16	7	13	5	6	7	9	
	C.E.	—11	45	—9	31	—1	10	—1	5	—9	0	11
	V.E.	11	27	9	14	7	11	4	6	2	9	
	C.E.	—25	27	1	29	2	26	—2	2	—11	0	12
	V.E.	10	12	15	11	6	11	8	7	3	7	
	C.E.	—23	21	—6	8	—3	8	—1	—6	—4	—14	13
	V.E.	4	16	12	8	7	9	6	8	3	6	
	C.E.	6	27	1	18	4	9	—4	4	3	3	14
	V.E.	11	21	6	19	4	8	8	8	5	3	
	C.E.	—1	16	20	44	10	19	0	7	—15	1	16
	V.E.	4	19	9	13	12	13	4	5	3	3	
	C.E.	—4	26	—2	28	2	19	2	—4	5	—5	17
	V.E.	8	11	6	14	5	12	10	7	8	10	
	C.E.	—5	1	9	9	3	5	—2	—7	1	1	18
	V.E.	5	5	12	7	8	10	17	4	4	5	

the long movements, which was less proportionately than that for the shorter ones, was shortened sufficiently to be transformed into a negative error. Here again the V.E. remains little changed throughout. Table V., for still another observer, for two weeks, with corrective knowledge from the beginning, and five daily trials for each record, for both free and blocked movements, shows much the same effect. The variable errors remain practically unchanged, the positive constant errors for the blocked movements become quickly reduced, while the constant errors for the free movements, beginning as positive, soon become almost entirely negative. The real illusion still persists, and the deliberate attempt to correct it miscarries in producing an opposite error for the free movements. Another experiment, on a fifth observer for fourteen days shows the same persistence of the illusion and the same disastrous effect of the deliberate corrective attempts.

The effect of these corrective attempts on the reproduction of free movements is not unlike the suggestive results of Solomons'[2] experiment on two-point discrimination, and seems to throw some light on the nature and basis of the judgment of extent. Solomons' experiment demonstrated the susceptibility to suggestion of the "judgment of twoness" and its lack of connection with judgments of area, position, etc. These facts seemed to indicate that the judgment is at bottom but a matter of simple association. "We learn that a certain kind of sensation means two points, just as we learn that certain marks mean the letter H, that another group of sensations means "book," etc. In the experiment referred to the two-point and one-point touches were purposely made to differ in two other features—mode of application and locality—the two-point touch being made by a sharp blow, in one area, the one-point being applied more by pressure and always in another area. After a period of practise the conditions were reversed—"the double points now pressed down and in the place where the single point was formerly applied, while the single touch is made with a blow and in the place where at the start the double touch was made." Under these circumstances the judgment was reversed—two is called one and one two. "The peculiarities of the sensation due to the method of application and the locality, have completely superseded those due to the number of points, as a basis for the judgment." His conclusion is that any cutaneous sensation may give rise to a perception of two contacts if the past experience of the individual has established the proper associations, and that there seem to be reasons for supposing that the same holds for other cutaneous judgments—position, area, etc.

Our present experiment affords indications of a similar associative and empirical basis for the judgment of extent of movement. In the beginning of the experiment the movements of the subject were made on some already present basis of comparison—a certain

[2] *Psychol. Rev.*, **4**, 246, 1897.

movement in one region of the arm's total possible swing was felt as equal to a certain other movement. Introspectively the basis satisfied the demands of the experiment. But objectively the impact disturbance induced striking discrepancy in the judgments of equality. So long as this discrepancy entailed no serious consequence the old system of criteria persisted and the error went unperceived. But as soon as the subject became aware of the large constant error in his reproductions, the desire for objective equality led to a transformation of the basis of judgment. The old signs of magnitude could no longer be relied on. At the end of the second week this new basis had become fairly well established and the accuracy of reproduction of the impact movements approximates the original accuracy of the free movements. The effect of this newly established basis on the judgment of free movements is significant. The correction which is appropriate in the case of impact movements is not restricted to these only, but is carried over into the other situation. This is most clearly shown in Table V., but appears also in the case of the larger movements in Table IV. Movements in the first part of the arm's swing, the standard extents, are the same. But the scale of criteria of extent in the further portion of the arm's swing has been shifted downward, an objectively shorter movement having been learned to be the equivalent of the standard extent. When these standards become free movements the newly acquired scale continues to be utilized. Since no correction was made in the case of these free movements, we may suppose that this scale would in its turn persist until objective necessities, awareness of error, or, in case the impact movements were dropped out, the gradual reassertion of the older and more firmly established system, led to modification in one direction or another. Such results, along with those of Solomons, not only tend to lead to an empirical theory of space perception, but persuade one to go the empiricist one better. Judgments of extent of movement do not seem to be dependent on an anatomically conditioned topographical relation between points on sensitive membranes (joint linings) and points in external space, or on any fixed serial order of stimulations of skin, tendon or muscle. As was the case with the judgment of twoness—the judgment of equality of extent seems to be at bottom a matter of simple association—those movements are judged to be equal which have been *learned to be* equal—any sensation quality which adequately identifies or differentiates a given movement being sufficient to serve as basis for the judgment of the equality or difference of this movement and any other movement with a similarly adequate and equally well learned sensation quality.[3] The significance of this associative basis of equivalence will be further discussed in Chapter VII.

The cause of the impact illusion is not very clear. Three contributory factors seem to be present: (1) the original intention, (2) the irradiation of the stimulus, (3) the shock of impact, as a sensation in its own right.

1. *The Original Intention.*—This first factor is probably an important one. The subject sets out to make a movement of, say, two feet, and is blocked at 10 cm. Now the process of preparing for, innervating and beginning a movement of two feet is not just like any other experience. It requires a particular attitude, a particular more or less widely spread adjustment, and a particular operation of visual and motor imagery. It seems quite likely, then, that in the reproduction, the observer tends not so much to move over the distance he was allowed to go before, but to repeat the original performance—to take the same general attitude, and make the same innervation. And, since there is no block in the way, the reproduction tends to approximate the original intention of the first movement, and the resulting error is always positive. This explanation might be sufficient if the illusion occurred only in such cases. But in the case of the four subjects in which there was no such explicit intention we find the same error manifested. In all four of these cases the observer was simply told to move his finger along the track, knowing that at some point his movement would be blocked. The shock of impact might be expected at any moment, and the only intention present was to keep on moving until the shock came. And this mild intention is certainly inadequate to account for a positive constant error of 155 per cent. of the standard.

2. *Irradiation.*—The irradiation of the stimulus of the shock of impact may have caused the articular surface to be stimulated farther on, at points where it would have been stimulated had the movement actually been of greater magnitude. Strict adherents of the joint sense as the basis of judgments of extent of movement might find here a possible explanation of the illusion. Or the irradiation need not be conceived as restricted to the articular surfaces. Tensions, strains, compressions and various local signs of a qualitative or intensive kind are doubtless provoked in adjacent and outlying regions of the muscles, tendons and skin as well, and these, being ordinarily associated with greater movements, may assist in producing the present illusion.

3. *The Sensation Itself.*—The illusion may come under the gen-

[3] Messenger (*Psych. Rev.*, Monograph 22, 1903) seems to find a similar basis for the perception or recognition of number.

eral head of the phenomena of fusion, the shock of impact fusing with the perception that is uppermost in consciousness, increasing its sensory elements and thus the apparent magnitude of its object. The influence of a secondary stimulus in producing an apparent increase in a primary stimulus is a common experience. In the case of vision this influence has been found to be proportional to the intensity of the secondary stimulus.[4] We found this to be in general the case in this illusion. The effect of such an influence is always "the tendency to fusion of two or more sensations which are simultaneously experienced." The extent of a movement and the force of a blow may seem at first thought to be not only incommensurable but incapable of summation, but both are equally perceptions of magnitude, and Woodworth has shown that "there is a certain amount of correlation between the extent of the preliminary movement and the force of the blow."[5]

Whatever explanation we prefer, the significance of the illusion in the study of the accuracy of movement is clear. Thus in Angier's recent study[6] he finds that passive movements are more accurately perceived than are active movements. Earlier investigators found the reverse to be true. Now from the description of the method used in his experiments, it appears that Angier's results, in the case of active movements, were subject to this error produced by impact. It is then quite conceivable that the error of the active movements should be unfairly increased until it exceeded that of the passive movements, although under similar or equally favorable circumstances just the reverse might have been obtained. Münsterberg[7] used the same method for controlling the standard in his experiments in sense memory, and all but one of his subjects showed extremely large positive constant errors. In the case of the smallest magnitudes, 5 cm., this positive error was nearly always 100 per cent. or slightly less, decreasing rather uniformly with increase in the standard magnitude. The statement of the amount of error under such circumstances can not be said to express the fidelity of the memory for sensations of movement. Even if the C.E. is eliminated, the V.E. will be too great by virtue of the greater magnitudes involved. Besides, one is, in such an experiment, measuring not only the memory for extent as such, but at the same time the rate of decrease in vividness of the illusion.

[4] H. J. Pearce, "Law of Attraction in Illusion," *Psychol. Rev.*, **11**, 43, 1904.

[5] "Vol. Control of Force of Movement," *Psychol. Rev.*, **8**, 350–9, 1901.

[6] *Zeit. f. Psychol.*, **39**, 430, 1905.

[7] *Beiträge*, **4**, 69–88.

CHAPTER III

The Indifference Point

By the "indifference point" is meant the point in a scale of magnitudes at which there is no constant error of estimation. When estimates or reproductions of such magnitudes are attempted the general rule is that the smaller are judged or reproduced too large while the greater are underestimated. At some mean magnitude only the variable error is found, and judgments at this point are consequently more precise. The region about this mean magnitude has been called the "indifference" point, more properly, the region of indifference. The phenomenon of the "indifference point" seems to have been first observed in experiments on the time-sense. Vierordt,[1] writing on the basis of Camerer's experiments, found that "there is an unexceptionable law that small intervals are overestimated and reproduced so on the kymograph, whereas longer times are inevitably shortened." Vierordt also states that the "indifference point" is not absolutely fixed, but varies with different individuals and at different times in the same individual. "It depends especially on the conditions of the experiment, as well as on the sense investigated." But the "conditions of the experiment" were not specified, and it will be seen later that Vierordt himself was misled by disregarding them.

From the time of Vierordt the long array of investigators of the time-sense set themselves the problem of the constant error, and sought to find an indifference point which would be the true one. Höring,[2] Kollert,[3] Estel,[4] Glass,[5] Nichols,[6] Schumann[7] and Stevens,[8] in turn, found regions of indifference, but at varying points in the scale, *e. g.*, Höring at about .5 sec., Kollert at about .8 sec., Glass at 2 to 5 sec., Nichols at about 1 sec. and Stevens .7 sec. on one occasion and 3 sec. on another. Periodically recurring "indifferent points" were asserted and denied, and numerous attempts made to relate the unit of periodicity to various bodily processes, such as

[1] "Zeitsinn," 1868, p. 17.
[2] Dissertation, Tübingen, 1864.
[3] *Phil. Stud.*, **1**, 78, 1882.
[4] *Ibid.*, **2**, 37, 1884.
[5] *Ibid.*, **4**, 423, 1887.
[6] *Amer. Jour. of Psychol.*, **3**, 453, 1890.
[7] *Zeit. f. Psychol. u. Sinn.*, **2**, 294, 1891.
[8] *Am. Jour. of Psychol.*, **13**, 1, 1902.

breathing, pulse, swing of leg, etc. These points will be more fully referred to after the present experiment is described. But the futility of attempting to relate the I.P. (as we shall hereafter designate the "indifference point") to the temporal periods of organic processes should have become apparent as soon as it was found to be a characteristic of all our judgments of serial magnitudes, both temporal and non-temporal. Vierordt had suggested that "a similar relation is to be found in our spatial judgments," but since the temporal relations of motor processes play so large a part in our spatial judgments, it may well have been supposed that the constant error in the case of space magnitudes is "simply the consequence of the rapidity of movement—hence a phenomenon of temporal estimation as well."[9]

But the I.P. is also found in judgments of weight, force and brightness, as well as in those of time and extent. In all these fields, again, there is little agreement among investigators, though there is usually a tendency to speak of the I.P. as though it were in each case some absolute and fixed region. In the estimation of force the I.P. is variously placed at from 200 to 1,600 grams. Cattell and Fullerton, using seven observers, with a series ranging from 200 to 1,600 grams, found the I.P. to be in all cases between 400 and 800 grams. Wreschner, studying the perception of lifted weights, found an I.P. at 1,200 grams and generalizes by saying that high intensities weaken in the memory while low ones are strengthened, a certain moderate intensity remaining unchanged, in both one-hand and two-hand experiments. Leuba,[10] experimenting with memory for brightness intensities, finds a striking difference between the ratios at the lower and upper ends of the scale. "There seems to be a natural tendency to shift the sensation held in memory towards the middle of the scale of intensities." In other words, low lights are overestimated while high ones are under-rated. The recent work of Lewis[11] gives some evidence in confirmation of Leuba's results.

All experimenters on the extent of movement seem to have found regions of indifference, flanked above and below by negative and positive constant errors. And although these I.P.'s all differ among themselves, it has still been the custom to refer to the indifference point as thought it were an absolute something. Thus Kramer and Moskiewicz and Jaensch surmise that there is such a thing as a "most favorable" extent, as well as a "most favorable"

[9] Külpe, "Outlines," 343.

[10] *Amer. Jour. of Psychol.*, **5**, 370, 1892.

[11] Johns Hopkins Studies, No. 2. *Psych. Rev.*, Mon. Supp., No. 40, 55, 1909.

time. Schneider,[12] working with distances ranging from 70 to 100 mm. finds an I.P. at 90 mm., Delabarre[13] locates it at about 300–400 mm., Falk[14] at 70–80 mm., and Münsterberg at 100–200 mm.,[15] while Cattell and Fullerton[16] find it to be 100 for one observer, 300 for another and 600 for a third. It was the disparity of these results in the extent of movement which suggested the present experiment, for while differences of a fraction of a second in the case of experiments on time may be considered quite possibly due to individual, mechanical and experimental conditions, the difference between 70 mm. and 600 mm. within the possible range of horizontal arm movements calls for some other explanation.

That no such explanation has been suggested is shown by the fact that in the most recent and thorough review of the subject of movement these disagreements are merely stated,[17] without being brought under any general law. There is still the tendency to treat the I.P. as a fixed magnitude of yet undetermined location, without specific regard to the series in which it occurs, although Vierordt long ago remarked that the actual magnitude of the I.P. depended on the "category" in which it was placed.

The present attempt to demonstrate the general law for the appearance of the I.P. phenomenon grew out of experiments on the accuracy of reproduction of active and passive movements. In this experiment small magnitudes were employed, the particular lengths ranging from 43 to 100 mm. In nearly every case, both for the active and the passive movements, a constant error was found, which was positive for the small magnitudes and negative for the relatively large. The region of indifference was found between 60 and 75 mm., falling at about the middle of the series. These figures, taken alone, tend to confirm Falk's statement. But another series of experiments with the Cattell-Fullerton apparatus, using a range of 100–300 mm., resulted, in several subjects, in a quite uniform I.P. at about 200 mm., thus agreeing with Münsterberg, who used a similar apparatus with about the same magnitudes. Nevertheless, the subjects of Cattell and Fullerton, attempting to reproduce from memory the extents used in their experiments, 100, 300, 500 and 700 mm., showed an I.P. between 300 and 500. The inference that the difference here found was purely a function of the series seems

[12] "La Memoire des Mouvements Actifs," Diss. Juriew, 1894.

[13] "Bewegunsemfindungen," 89.

[14] "Räumschätzung mit Hülfe von Armbewegungen," Diss. Dorpat, 1890.

[15] *Beitrage*, **2**, 159.

[16] *Op. cit.*, 51.

[17] Woodworth, "Le Mouvement," Chapt. VI.

obvious. Yet in the field of the time sense as well as in the field of movement, there had been, since the days of Vierordt and Camerer, attempts to find the real "indifference point," each investigator using such series limits as seemed to suit his convenience, apparently with no suspicion that the I.P. might be purely a function of the upper and lower limits of the scale of magnitudes in the particular experiment.

In order to test this suspicion in the field of movement, the following experiment was made. A standard series of magnitudes was chosen, ranging from 10 mm. to 250 mm., and this series was divided into three sections, A, B and C. The magnitudes of section A ranged from 10 mm. (increasing by increments of 10 mm.) to 70 mm. Those of section C, from 70 to 250 mm., with increments of 30 mm., and those of section B, from 30 to 150 mm., with increments of 20 mm., thus overlapping the inner ends of sections A and C. There were thus seven standard magnitudes in each section, and the three sections, A, B and C represented respectively the lower, middle and upper regions of the total scale (*S*) originally selected. The standards were made by cutting a narrow slit of the desired length in one strip of cardboard and pasting this strip upon another such strip as foundation. This formed a furrow which served as adequate guide for the stylus of a Delabarre pendulum planchette, or for the blunt point of a heavy carbon pencil which might be held between the thumb and fore-finger while the forearm rested on the planchette board. The furrow was carefully rubbed with graphite, thus affording a smooth and noiseless track.

Experiments were now designed with the following purposes in mind. (1) To see whether a periodic I.P. could be found within the total series (*S*), by working with its special sections, finding an I.P. in A, one in B and another in C. (2) To see, for instance, whether the same absolute magnitude might be, under one circumstance an I.P., under another circumstance affected with a positive constant error, or again, with a negative constant error. (3) To ascertain whether the gradual extension of the series limits would be accompanied by a corresponding change in the position of the I.P. (4) To find whether any magnitude in the series evinced any constant error when estimated out of relation to a series or section. (5) To learn whether or not the C.E.'s occur, even in serial experiments, when the separate trials are distributed over a considerable period of time.

Procedure.—A Delabarre pendulum planchette, with a 14-foot radius, was suspended over the outer edge of a writing table of the twenty-one standards, a total of 1,050 movements for each subject. Three subjects were used, all being graduate students in psychology, two of them ignorant of the purpose of the experiment, and all three ignorant of the results from day to day.

TABLE VII

ERROR OF REPRODUCTION

Obs.	Trial.	30		50		70		90		110		130		150	
		C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
R.	1	+ 1	4	+ 1	8	—8	10	—10	8	—20	11	—29	7	—30	11
	2	+ 5	5	+ 5	9	+2	9	— 7	10	—15	9	—27	8	—30	10
	Av.	+ 3	4.5	+ 3	8.5	—3	9.5	— 8.5	9	—17.5	10	—28	7.5	—30	10.5
B.	1	+ 8	6	+ 9	11	+3	13	— 3	9	—10	12	—14	12	—15	12
	2	+12	6	+16	10	+5	13	— 1	14	— 9	9	—11	12	—14	14
	Av.	+10	6	+12.5	10.5	+4	13	— 2	11.5	— 9.5	10.5	—12.5	12	—14.5	13
C.	1	+ 6	7	0	7	+3	8	— 1	8	— 6	10	—14	14	—14	14
	2	+ 7	9	+ 3	8	+5	9	— 2	8	— 5	14	—16	11	—17	14
	Av.	+ 6.5	8	+ 1.5	7.5	+4	8.5	— 1.5	8	— 5.5	12	—15	12.5	—15.5	14
Av.		+ 6.5	6.2	+ 5	8.8	+1.7	10.3	— 4	9.5	—10.8	10.8	—18.5	10.7	—20	12.5

TABLE VIII

ERROR OF REPRODUCTION

Obs.	Trial	70 mm.		100 mm.		130 mm.		160 mm.		190 mm.		220 mm.		250 mm.	
		C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
R.	1	+14	9	0	10	—12	10	—23	13	—37	11	—53	15	—68	19
	2	+29	12	+11	12	0	10	—17	12	—30	16	—55	15	—73	15
	Av.	+22	11	+ 5.5	11	— 6	10	—20	12.5	—33.5	13.5	—54	15	—70.5	17
B.	1	+10	11	+10	9	— 1	14	—16	15	—16	14	—25	14	—38	18
	2	+17	12	+14	8	— 1	12	—19	14	—17	12	—32	19	—42	18
	Av.	+13.5	11.5	+12	8.5	— 1	13	—17.5	14.5	—16.5	13	—28.5	16.5	—40	18
C.	1	+12	8	+ 8	11	+ 3	14	— 8	14	—15	11	—33	18	—45	15
	2	+17	11	+14	15	+ 7	11	— 2	14	—12	14	—32	18	—47	16
	Av.	+14.5	9.5	+11	13	+ 5	12.5	— 5	14	—13.5	12.5	—32.5	18	—46	15.5
Av.		+16.5	10.5	+ 9.5	10.8	— .6	11.8	—14.2	13.7	—21.2	13	—38.3	16.5	—52.2	17

Tables VI., VII. and VIII show the results for sections A, B and C for the three subjects, including the constant error and variable error of both the first and the corrective trials, and the average constant error and variable error of the two trials, for each subject, along with the grand averages for all three subjects. The unit throughout is the millimeter.

The results are thoroughly clear and uniform. In every section employed we find the relatively small magnitudes reproduced too great, and the greater magnitudes reproduced too small, both in the first and in the corrective attempts, while there is a region of in-comfortable height. The observer sat with his left forearm on the board, with the carbon pencil held between thumb and forefinger. The position of the hand was made constant by inserting the end of the little finger in the pen shaft of the board. The observer wore the blind mask throughout or worked with closed eyes. The board swung an inch and a half clear of the table, on which lay several large sheets of smooth white paper. One of the standard strips was now placed underneath the board and the point of the carbon pencil inserted at the beginning of the guide slit. The cards of varying magnitudes were always placed so that the center of the board, when the pendulum was at rest, was directly over the halfway mark of the slit. The observer could now trace the path with a minimum of interference and adjustment, and with practically a horizontal swing of the forearm, the actual movement being thus rectilinear, a compound of elbow and shoulder joint flexion. After tracing the slit, the carbon point was brought back to the initial position for the respective magnitude and after an interval of two seconds an effort was made to reproduce the magnitude, the cardboard strip having been removed by the operator and the carbon point writing on the smooth white paper. After another interval of about two seconds, a second attempt was made, in which the observer estimated the probable or apparent error of his first trial and tried to reproduce the original magnitude more exactly.

TABLE VI

Error of Reproduction, First and Second Trials and their Average

Obs.	Trial.	10		20		30		40		50		60		70	
		C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.	C.E.	V.E.
	1	+2	*2*	+ 2	*5*	0	*6*	—4	*6*	—6	*6*	—13	*8*	—16	*7*
R.	2	+4	*3*	+ 6	*5*	+5	*6*	+1	*8*	—3	*8*	— 6	*8*	—12	*8*
	Av.	+3	*2.5*	+ 4	*5*	+2.5	*6*	—1.5	*7*	—4.5	*7*	— 9.5	*8*	—14	*7.5*
	1	+5	*2*	+ 7	*5*	+3	*6*	+2	*5*	+1	*5*	— 3	*8*	— 8	*8*
B.	2	+6	*3*	+10	*5*	+5	*5*	+5	*5*	+4	*6*	— 1	*8*	— 3	*7*
	Av.	+5.5	*2.5*	+ 8.5	*5*	+4	*5.5*	+3.5	*5*	+2.5	*5.5*	— 2	*8*	— 5.5	*7.5*
	1	+3	*4*	+ 2	*4*	+2	*6*	—5	*6*	—2	*6*	— 6	*8*	—12	*9*
C.	2	+1	*3*	+ 2	*4*	+4	*6*	+1	*7*	+1	*9*	— 6	*7*	—10	*8*
	Av.	+2	*3.5*	+ 2	*4*	+3	*6*	—2	*6.5*	— .5	*7.5*	— 6	*7.5*	—11	*8.5*
Av.		+3.5	*2.8*	+ 4.8	*4.6*	+3.2	*5.8*	0	*6.3*	— .8	*6.6*	— 5.8	*7.8*	—10.2	*7.8*

At a given sitting but one section, A, B or C, was used, each of the seven magnitudes being presented in chance order, but no magnitude being repeated until all of the others of the series had been presented. Twenty-five trials for each magnitude were taken, making, with the corrective attempts, fifty reproductions for each of difference at about the middle of each section. Moreover, in the case of the small magnitudes the positive error in the corrective attempts is seen to be greater, in 24 averages out of the 26 in which the first error was positive, than the first error. The difference ranges in section A from 1 to 3 mm. (with average of $2\frac{1}{4}$, in B from 1 to 7 mm. (with average $3\frac{1}{2}$), and in C from 3 to 15 mm. (with average $7\frac{1}{2}$).

But in the case of the larger magnitudes we find that only in 10 cases out of the 37 averages in which the constant error is negative is the corrective error still more negative than the first. Of these cases, 7 come in section C, which contained the greater magnitudes. Thus, in 27 averages out of 37, although the constant error was always negative, the correction was positive, just as in the cases of the positive constant errors. These corrective attempts were introduced in order to see if there might not be an attenuation of accuracy, by virtue of the mere repetition of the process of judgment, the positive errors being thus made more positive and the negative more negative. But this is not the case. Instead all errors tend, on the whole, to become more positive, the tendency being more pronounced, however, in the case of errors already positive. It is probable, consequently, that this effect has nothing to do with the particular illusion which we are studying. The phenomenon is probably simply a "warming up" effect, the second movement being easier than the first by virtue of the motor inertia having already been overcome. This would tend to make the corrective movement really greater than it appeared. In the case of the first attempts, then, this fact would also have some bearing. It would mean that the negative errors, so far as actual judgment is concerned, are really greater, that is, more negative than they seem to be in measurement by an objective scale, and that the positive errors, similarly, are not really quite so great as measurement shows them to be.

Turning now to the real point of the experiment, we find in section A (10–70 mm.), for the three subjects, an average I.P. at about 40 mm.; in section B (30–150 mm.), an I.P. at about 75 mm., and in section C (70–250 mm.), an I.P. at about 125 mm. By some singular coincidence the ratio of the approximate I.P. to the upper magnitude of each section is in every case almost exactly one half. It is apparent that in extent of movement and in time of movement, so far as time is a function of extent, we can find an I.P. at whatever point we choose. Given the series of magnitudes with which we are to work, we may be quite certain that our region of indifference will fall at about the mid-point or region of the particular scale. Thus in the present experiment we find that 70 mm., which is always underestimated in section A, falls within the region of indifference in section B, and is always overestimated in section C, etc.

TABLE IX

Error of Reproduction for Isolated Magnitudes

		10 mm.			70 mm.			250 mm.		
		1st.	2d.	Av.	1st.	2d.	Av.	1st.	2d.	Av.
R.	C.E.	— .6	— .3	— .5	+1.5	+4.6	+3	— .7	—2.4	—1.5
	V.E.	*1.6*	*1.5*	*1.5*	*6.6*	*6*	*6.3*	*12.3*	*15.3*	*13.8*
	+	11	13	24	11	13	24	11	13	24
	—	14	12	26	14	12	26	14	12	26
B.	C.E.	+ .3	+3	+3	+2	+2	+2	—3	—5	—4
	V.E.	*1.8*	*1.5*	*1.7*	*4.2*	*5*	*4.6*	*12*	*11*	*12*
	+	24	23	47	14	14	28	10	8	18
	—	1	2	3	11	11	22	15	17	32
C.	C.E.	— .2	— .3	— .3	—2	+2	0	—2	—4	—3
	V.E.	*1.5*	*1.3*	*1.4*	*6.7*	*6.3*	*6.5*	*19*	*23*	*21*
	+	14	13	27	12	13	25	12	14	26
	—	11	12	23	13	12	25	13	11	24

A second experiment, performed as a check to the preceding, shows the facts still more clearly. About four months after the sectional records had been taken, the three magnitudes, 10 mm., which had always been affected with a positive constant error, 70 mm., which fell now into a positive error, now into the I.P. and now into a negative error, and 250 mm., which was always underestimated, were used singly and on occasions several days apart, after the same method. Table IX. shows the results for the three subjects, giving the constant and variable errors for the first and the corrective trials, and their average, together with the number of positive and negative errors in each case. Comparing these constant errors with those of the same magnitudes in the previous experiment, the effect of inclusion in a series is evident. The magnitude 10 mm., always overestimated from 2 to 6 mm. in section A, is here slightly underestimated (from .2 to .6 mm. in the case of the two subjects who knew nothing of the purpose and results of the experiment), and it will be seen that this constant error is a purely chance one, since the number of positive and negative errors is almost equal—51 positive and 49 negative. The magnitude 70 mm., with average constant error of — 10 mm. in section A, + 1.7 mm. in section B, and + 16.5 mm. in section C, when taken by itself has a constant error that is purely accidental, the + and — cases being here also almost equally divided, 25 + and 25 — in the case of C., 24 + and 26 — in the case of R., and 28 + and 22 — in the case of B., with average constant errors of 0, + 3 and + 2. The magnitude 250 mm. shows the same results, although the calculated constant error is slightly negative in all cases (grand average, — 2.8 mm.). The + and — cases show the same equal and chance distribution, in the cases of C. and R. 50 + and 50 —. Only in the case of B. is there the slightest deviation from this rule. But even here the constant error for 10 mm. is only + 3 mm. as against + 5.5 mm. in section A, that for 70-mm. only + 2 mm. as against + 13.5 mm. in section B, while that for 250 mm. is — 4 mm. as against — 40 mm. in section C.

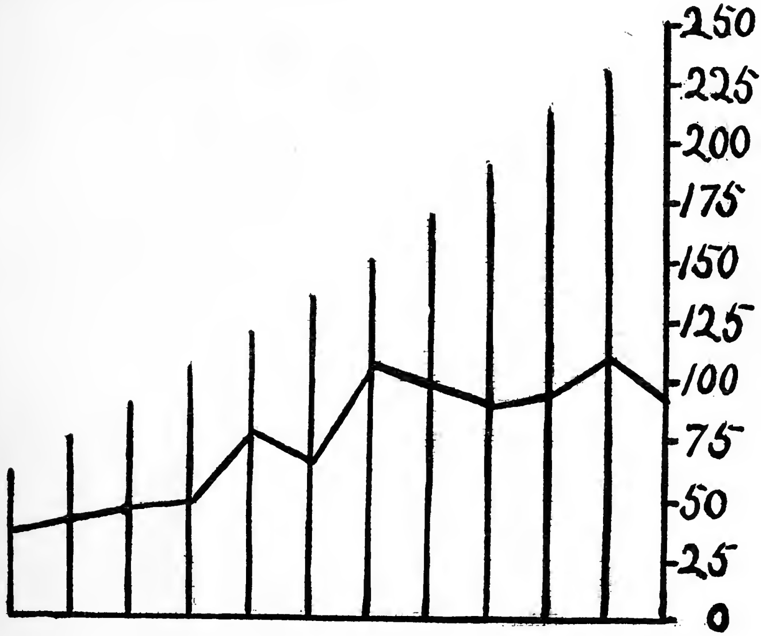

FIG. 1. Showing the Rise of the Indifference Point with the Extension of the Series Limits.

This conclusion is further justified by an interesting variation of the experiment, performed with another subject (Hp.) who was ignorant of the purpose and previous conduct of the experiment as well as of the general subject of the indifference point. A set of standard magnitudes was prepared ranging from 10 mm. to 60 mm. by increments of 10 mm., from 60 mm. to 150 mm. by increments of 15 mm., and on to 250 mm. by increments of 20 mm. The standards of series 10–60 were now given and reproduced in chance order, five trials being made of each magnitude, a total of 30 trials. At this point, without the knowledge of the subject, the next magnitude (75) of the series was introduced, and again five trials made of each standard. Then the next magnitude was introduced and similar tests made for series 10–90. This process was continued until all the higher magnitudes of the series had been introduced, the last set thus consisting of five trials for each magnitude of the total series 10–250. This made a total of 690 movements. All these trials were made at a single sitting, so that the subject worked with a gradually expanding series, the lower limit of which remained constant while the upper limit increased by regular and approximately equal steps.

The results are shown in Fig. 1. In set 1 the series breaks, as usual, approximately half way between the two extremes, giving an I.P. at 35 mm., with positive constant errors below and negative above. In set 2 the I.P. rises to about 40 mm., in set 3 to 45 mm., in set 4 to 50 mm., and similarly throughout the whole experiment (disregarding the exceptional height of the I.P. for range 0–150), each new standard, as it is introduced, being found to influence the apparent magnitude of every other. The general effect of this influence is to increase all positive errors. This increase shows itself in three ways: (1) When the C.E. was positive from the beginning, this error is seen, on the whole, to increase with the introduction of each new standard. Thus the positive C.E. for 10 mm., at first 8.4, becomes as great as 13.6, that for 20 mm. increases from 3.8 to 14.8, and that for 30 mm., from 1 mm. to 10.8. (2) Constant errors which were in the beginning negative undergo a transformation in the course of the experiment. Each decreases in magnitude through an indifference point of its own, below which it emerges as a positive error. This is shown in the case of all standards from 40 mm. through 120 mm. (3) As a result of these transformations the region of indifference varies about a constantly augmenting magnitude which ranges from 35 mm. to 100 mm., but goes in two cases as high as 110 mm. and 120 mm. In general, the indifference point rises in response to the extension of the series at its upper limit, and lies, in all cases, at a point which represents roughly the median of the total group of magnitudes.

The results of all three experiments are consistent, and afford the following answers to four of our introductory questions. (1) A periodic I.P. can be found within the total series (S) by working with its special sections (A, B and C). (2) The same absolute magnitude may be under one circumstance an I.P., under another, effected with a positive C.E. or again with a negative C.E. (3) The gradual extension of the series limits is accompanied by a corresponding shift in the region of indifference. (4) No magnitude evinces any C.E. when estimated out of relation to a series or group of which it is a member. (5) The reply to the fifth question is to be found in the tables of Chapter V., in which are shown the effect, on the direction and magnitude of errors of reproduction, of lengthening the interval between standard and reproduction. This procedure has the effect of extending the period of time within which the individual members of the series occur. We have found that in the case of 2-sec. intervals, with such series as were used in the present experiment, the influence of the judgments of one magnitude persists throughout the experiment, affecting in a very definite manner the judgments of all other magnitudes. Our present query is concerning the persistence time of such an influence. In the experiments of Chapter V. intervals of 2, 5, 10, 15 and 30 seconds were introduced between the standard and the reproduction. These experiments afford somewhat definite answer to our question. In Table XII., recording the errors in reproduction of extent for observer W., the I.P. is found in all series up to the 15-sec. interval. When intervals of 30 sec. are employed the C.E.'s are all considerably negative and the group effect is not found. In Table XV., recording the error in the reproduction of duration for the same observer, the I.P. appears after 2-sec. and 5-sec. intervals, but not beyond, while in similar experiments on observer H. (Table XVI.) the I.P. is found only after intervals of 2 sec. While these are but incidental observations and can not be said to serve as basis for any adequate quantitative statement, there is evident indication that the constant errors do not so much represent transformations in a memory image of the stimulus in question as they do the effect, on a present judgment, of the persistence of the mental set involved in a previous judgment. If the interval between the two judgments is sufficient, the first disposition is soon dissipated and is no longer adequate to affect the second performance. That the C.E. is not due to the transformation of a memory image is apparent from the fact that in this case no C.E. appears.

I conclude then that the phenomenon of the indifference point, so far as it occurs in our spatial judgments, and in our temporal judgments so far as they are a function of the extent of movement, is of purely central origin, and that its position depends entirely upon the range or limits of the magnitudes in question.

The suspicion is a natural one that the varying and contradictory I.P.'s found by the investigators of the time-sense were due to differences in the limits of the series of magnitudes employed. And if the actual range of magnitudes used be but tabulated alongside the I.P.'s obtained, the suspicion is strikingly sustained. In many cases the range of magnitudes actually used is not clearly stated, nor is it always clear how much of the total range was used at a given sitting. Such data as could be found from those who particularly studied the constant error have been tabulated in the following.

TABLE X

RELATION OF I.P. TO RANGE OF INTERVALS

Investigator	*I. P., in Seconds*	*Range, in Seconds*
Vierordt.		
Subject N.	1.5	.5 to 5.8
" H.	1.4	1 to 3.5
" V.	3	.6 to 3.5
Touch.	2.5	.25 to 5
Hearing.	3.5	.5 to 8
Spontaneous movement.	5	.2 to 65
Höring.	.5	.3 to 1.4
Kollert.	.8	.4 to 1.8
Estel.	Multiples of .75	Used different sections of range 1.5 to 8 at different parts of day.
Glass.	2 to 5	.7 to 15
	3	.7 to 9
Stevens.	.7	.2 to 3

One would scarcely pretend to submit these figures to a thoroughgoing comparison, because the methods and apparatus were extremely varied, and the subjects as well as the operators were all different. Moreover, the influence on the I.P. need not be supposed to be directly proportional to the quantitative shift in the series limits. Nevetheless the table is suggestive. Vierordt, for instance, states, on the basis of the averages quoted in the above table, that the I.P. "depends especially on the sense under investigation," and finds that for touch the I.P. falls at 2.5 sec., for hearing at 3.5, and for the time of spontaneous movements at 5 sec. But another fact should be observed which Vierordt failed to point out, viz., that for touch the range of intervals employed was .25 to 5.0 sec., for hearing .5 to 8 sec. and for movement .2 to 65 sec. In the light of the results we have just reported it is extremely probable that the variation of the I.P. is much more a function of the range of intervals employed than of the "sense under investigation." For in every case the region of indifference approximates the middle of the range.[18]

[18] In the case of the spontaneous movements only 216 out of 1,708 movements were over 20 seconds in duration, and only 444 of the upper third of his scale contrast with 757 movements in the lower third. The I.P. would probably have been still higher here if both ends of the scale had been equally employed.

Similarly, Höring, with a range of .3 to 1.4 sec., finds his I.P. at .5 sec., Kollert, with a range of .4 to 1.8 sec., finds a higher I.P., and Stevens, with range .2 to 3 sec., finds it to be at .7 sec., while Glass, using range .7 to 9 sec., finds it at 3 sec., although for range .7 to 15 sec. it rises as high as 5 sec. With these facts in mind little stock can be taken in the reports of rhythmic, oscillating, periodic indifference points, unless it is explicitly stated just what range of intervals was employed, not merely in the experiment as a whole, but at each sitting from which results are used. Estel, who found periodic I.P. in his total scale, employed different sections of this scale at different sittings. Shaw and Wrinch,[19] who used the method of successive reproduction, thus partially eliminating the effect of range, do not state the order in which the different intervals were used. Negative errors were found for .5, .75 and 1.5 seconds. Records on four subjects, using intervals ranging from .9 to 10 seconds, did not agree.

Theoretical.—The cause of this tendency to positive and negative constant errors at the two extremes of a scale of magnitudes has always remained obscure. At first thought it seems to be but an instance of Fechner's time-error, with an unexplained difference in direction at the two ends of the scale. But that the illusion is not based on the temporal positions of the two stimuli is clear from the fact that it does not occur when the magnitudes are taken singly, although the temporal positions of standard and reproduction remain unchanged. Schumann attributed the phenomenon, in the case of time, to mechanical sources of error in the apparatus. But this could hardly explain the same tendency in judging intensity of light (Leuba, Lewis), or the size of squares (Kennedy) or visual lines (Münsterberg). Vierordt's suggestion was that it might be due either to the peculiarity of the sense organ or to the accompanying psychical processes. Delabarre,[20] working with extent of movement, offers various explanations, none of which seem adequate. The first suggestion is the lack of proper and sufficient current control in performing the short movements. The reproduction of the short movements is thus rougher, and the moving member can not be stopped at will. This is probably the case with the very small movements, but possibility of current control would seem to be a rather fixed physiological factor and not to vary up and down the scale of extension. This it must do if it is to account for the fact that a relatively large movement, underestimated in one series, is overestimated when placed in another series. The same criticism

[19] Univ. of Toronto Studies, 1, 105, 1900.

[20] *Op. cit.*, 89.

also applies to his statement that these small movements are unusual and hence overestimated. Actually, movements so small as writing movements are not at all unusual, and are made, moreover, with considerable precision, while we found movements of a foot or more being overestimated. Slower speed for the long movements is also suggested. While this might apply to the greater extents, it has no value for the cases of the other senses, in which the time element is not involved. In the same way objection must be made to the theory that the effect of the constant change and renewal of the motor impulse is to increase the apparent magnitude of the large movements. Besides, all these points apply to the standard movement as well as to the reproduction, and do not make clear why one should be affected in judgment, the other not.

Wundt considers the tendency to overestimate small articular movements and to underestimate large ones as a tactual analogue of the similar illusion present in the estimation of visual angles. "This comes under the general principle that a relatively greater expenditure of energy is required for a short movement than for a more extensive one, because it is relatively more difficult to begin a movement than to continue it after it is started."[21] If it were merely a case of estimation in terms of an objective unit, or even a case of the comparison of two arbitrarily given standards, Wundt's suggestion might suffice. But in all these experiments the method of reproduction has been used. The observer's task was not the comparison of one standard with another, but the reproduction of a given normal stimulus, and it was this reproduction that showed the constant error, and there is no reason for supposing the "relative difficulty" of a short movement to interfere with the attempt to reproduce the same short movement. The same "relative difficulty" is present in the reproduction as in the standard.

By far the most complete discussion of the positive and negative errors and the resulting indifference point in the field of movement is to be found in Woodworth's chapter[22] on the subject. The possible efficient causes are here analyzed into motor factors, sensory factors, emotional factors and factors of attention and association. Under motor factors are included the facts of dynamogenic stimulation and fatigue. If we suppose that the first small movement acts as a stimulant to the motor system, the reproduction would be expected to show the effect in being somewhat more easily made, hence probably correspondingly larger than necessary. And in the case

[21] Wundt, "Outlines," 3d Eng. ed., p. 139, 1907.

[22] "Le Mouvement," Chapt. VI.

of the negative errors it might be supposed that the longer movements, instead of acting as stimulants, actually have a fatiguing effect, so that their reproductions are somewhat more difficult and fall short. But this last supposition is shown to be "undoubtedly false," from the fact that a single contraction, of even maximal force, does not fatigue, but acts as a stimulant to the motor apparatus. Besides, the illusion does not become more pronounced in the course of the experiment, as should be expected on the basis of fatigue. Woodworth admits that the motor factor probably helps to produce the positive error, but insists that this factor alone is insufficient to explain any illusion of perception, since it still requires to be shown why the error, once made, is not perceived and allowed for.

Moreover, in the light of the present experiment, and the quoted results of many others, neither the stimulating effect of the small movements nor the fatiguing effect of large ones, nor both together, would suffice to account for the fluctuation of the indifference point with the change of series limits. It is hardly possible that a 70-mm. movement, taken in one series, should be physiologically stimulating, in another of slightly different range, fatiguing, while in still another it should be physiologically indifferent. For the same reason the exciting or depressing emotional effect of the preceding movement, while it might be conceived as contributing to the total effect if the indifference point were found to be fixed, can hardly be supposed to possess the same variability as is found in the indifference point.

To Woodworth, "the sensory factor in the genesis of the constant errors and illusions may seem the only one worth recognizing." Under sensory factors he includes the lack of adequate sensory evidence for finer discrimination of movements, and an inequality of peripheral sensation, shown in the fact that "the longer and stronger the habitual movements of a member, the less felt are its movements." While these factors seem to be of value in accounting for asymmetrical errors and in comparison of movements in different directions or by different parts of the body, they do not seem to have any direct bearing on the question of the variable indifference point. Nor do they apply to the same phenomenon in other senses, in which the factor of movement is not present.

The fact seems to be rather, that the phenomenon of the indifference point and the so-called positive and negative time errors result from purely central factors. The general law seems to be that in all such estimates we tend to form our judgments around a mode or central tendency of the series. Toward this mean each judgment tends by virtue of a mental set corresponding to the particular scale or series in question. This is practically the equivalent, for judgment, of Leuba's "Law of Sense Memory"—"There is a natural tendency in us to shift the sensation held in memory towards the middle of the scale of intensities."[23] But our own results seem to indicate that the phenomenon is one of direct perception rather than of memory as such. If it were due, as Wreschner, Leuba and others have supposed, to changes in the memory image during the interval between the standard and the comparison or reproduction the same effect should be present when a given magnitude or intensity is investigated alone—out of relation to a group or series. The present experiments show that this is certainly not the case for extent of movement and probably not for time. Moreover the results of the experiments on reproduction of time of movement indicate that it occurs only when the interval between standard and reproduction is short. This is at least not the most favorable condition for changes in the memory image. Besides, if the illusion is due to such changes, it is not clear why the behavior of the memory image should be so dependent on the general range or group in which the impression happened to occur. It is true that memory images undergo changes, which depend chiefly on the period of their duration. But the phenomenon of the indifference point can not be brought under the law of these changes. The constant errors flanking the indifference point seem rather to be errors of direct perception and their generalization should be a law of perception.

In the case of immediate estimation at least it is not so much a law of memory as a law of judgment, and in the case of immediate reproduction, a correlative law of automatic tendency in performance. It is the operation in judgment of the law of habit and adaptation. Just as a group of diverse and varying movements directed towards a given end gravitate toward an average performance which will economize effort and yet accomplish the end of the activity, so the act of judgment, in the interest of mental economy —and especially the motor process of reproduction, when that method of registration is employed—tends toward an average estimate. It is probable that even when single, unserial stimuli are received or movements made, they are "apperceived" into pretty definite mental sets or sense categories. Thus our movements do not, in their extent and force, form a serial scale or continuum, but fall apart

[23] *Op. cit.*

into rough groups, with rather indefinite limits but with rather definite central tendencies—such groups, for instance, as writing movements, eating movements, dressing movements or various sets of trade and professional movements depending on one's habits and customary occupation. And the constant errors found by Delabarre, Loeb and others when movements in such various directions and of different "category" are compared, are probably due to just these central factors of the judgment of magnitude as much as to any anatomical or physiological facts.

What we have here is somewhat different from the contrast experience. The apparent magnitude of one member is not conditioned so much by its general relation to other members of the series or by the effect of an immediately preceding member as by its rather specific relation to the central tendency or mean or average of the series. It is not the phenomenon of contrast. In fact it is just the reverse, for the law of contrast would tend to make the small magnitude seem still smaller in the presence of the large and the large seem correspondingly greater. Nor can it be classified as a phenomenon of adaptation of attention in which expectation or surprise are supposed to result in constant error of estimation. Adaptation of attention toward the group as a whole would lead to a situation of contrast. The small magnitude would surprise by its unexpected shortness and be underestimated in reproduction, while the constant error for the greater magnitudes would be positive. This again is just the reverse of what we actually find in the present experiment.

This *law of central tendency* may be illustrated in the case of judgments of extent of movement in some such way as the following. Suppose *AC* to represent a scale of magnitudes and *B* to represent a value in the central region, between *A* and *C*. In the attempt, now, to reproduce a given magnitude, every point in the series may be said to exert an attraction on the moving member, by virtue of the automatic character of motor habit—the thing once done tends in the future to carry itself out to completion whenever it is initiated. As a result of this "attraction" the tendency to reproduce a magnitude larger than *AB* is partly inhibited by the tendency to make one less and vice versa. *B* thus becomes the "indifference point," and *AB* the magnitude whose reproduction will be least disastrously affected by the motor habit. In the case of the other senses, though not reenforced by this motor law, the law of *central tendency in judgment* prevails nevertheless to sufficient extent to complicate our measurements and to keep us supplied with "problems."

For the same rule holds, as researches referred to in the first part of the chapter have shown, in cases in which errors of reproduction are not present, cases, *i. e.,* in which the judgment is purely a matter of comparison of sense stimuli—visual lines, visual angles, duration, weight, force, brightness—all show the same phenomenon. The *law of central tendency,* in such cases, produces results analogous to many cases of "preperception." The hunter who mistakes the clump of stubble for a rabbit is the classical example. He is mentally set for "rabbits" and is not engaged in an experiment on sensible discrimination. Hence small differences are disregarded. Anything which roughly approximates the form of a small animal is adequate to provoke the judgment "rabbit." In ordinary life we are not concerned with small differences, we are more occupied with averages, types, central tendencies, general resemblances. It is this fact which frequently permits even a crude counterfeit to pass undetected. Now it appears that this daily habit carries over even into our deliberate experiments on sensible discrimination. Each impression leaves a mental set which tends more or less to assimilate a succeeding impression, just as the set corresponding to or induced by the idea "rabbit" tends to assimilate the clump of stubble. Any stimulus not *too* different is likely to appear identical, even though practised scrutiny with knowledge of results might make the discrepancy apparent. Now it is easy to see why the resulting mental set of the series of magnitudes, light intensities, for example, should correspond to the central tendency of the series. In this region there are, in both directions, magnitudes of general resemblance. In the case of the extreme magnitudes, however, the resemblances run in one direction only. In the central region the stretches or ranges of resemblance overlap and intensify each other, magnitudes within them are mutually taken for each other, and the resultant is a mental set for the central tendency. Every magnitude tends to be assimilated by this set and made to appear less different from the central tendency than it really is. The degree of this assimilation is measured by the amount of difference required to do violence to the mental set in a single instance. Thus, in the case of the hunter, the degree of assimilation is measured by the deviation in contour, shading and general appearance sufficient to provoke a judgment of "not rabbit," in the inexact discriminative processes of a hunter intent on game. Just less than this amount of difference will ordinarily go undetected. Similarly, in the case of light intensities, a rather definite degree of assimilation affects magnitudes on both sides of the indifference region. This amount of difference is ignored in the process of. discrimination. This degree of assimilation is measured by the C.E. This of course will be negative for magnitudes above the region of indifference, since this amount of difference is unnoticed. For the same reason the C.E. will be positive for magnitudes below the region of indifference, *i. e.,* magnitudes at either extreme will tend to appear more nearly equal to the central tendency than they really are. The region of indifference represents the range of magnitudes any of which satisfies the general mental set induced by the series as a whole—they can be roughly substituted for each other without detection.

In this sense the illusions may be said to be due to the effect of expectation, except that in this case expectation results in assimilation instead of in contrast. The words "mental set for the central tendency" simply mean that we are adjusted for or tend to expect the average magnitude, and to assimilate all other magnitudes toward it, to accept them in place of it.

CHAPTER IV

Relation between Extent and Duration

Many hypothetical attempts have been made to simplify the judgment of magnitude in the case of movements by reducing the various perceptions of extent, time, force, speed and position to terms of one or more of these factors. Thus it has frequently been conjectured that the judgment of extent is based on the perception of duration or on the force of contraction, that force is measured in terms of extent or speed, etc. The most frequent suggestion has been that which would make the estimation of extent a function of the perception of time. Loeb[1] was led to this supposition by the observation that some subjects sought to attain greater accuracy by counting during the execution of the movement. But the records of such subjects showed no superiority over those in whom the tendency was not remarked. Kramer and Moskiewicz[2] proposed the same reduction as a result of their experiments on the Loeb illusion already quoted. They supposed that, especially in the presence of such different sensation complexes as were involved in their experiments, the only quality common to the two movements was the element of time and that the durations of these intospectively equal extents would be found to agree. The conjecture, however, was not put to the test. Similarly Jaensch,[3] working on the same problem very recently, finds that times agree much more closely than extents though the errors are not always in the same direction. In fact he finds the time differences to be as small as one could expect even in deliberate attempts to reproduce durations, and concludes that "we hold stretches to be equal the retrospective times of which are equal." Even when the movements were made from the same initial point the times were almost equal and seemed to serve as criteria when visual data were excluded. Külpe, while not adhering to a strict duration theory, says that "as a general rule the apparent magnitude of a distance is proportional to the length of time required for movement across it."

On the other hand we find an array of objections accumulated by Woodworth[4] which seems to discredit completely the foregoing hypotheses. Chief among these objections are the following:

[1] *Pflüger's Arch.*, **41**, 124, 1887.

[2] *Op. cit.*, 125.

[3] *Zeit. f. Psychol.*, **41**, 257–279, 1905.

[4] "Le Mouvement," Ch. IV. "Accuracy of Voluntary Movement," 77 ff.

1. The time of movements may be extremely varied without entirely destroying the approximate equality of their extents.

2. The results obtained by Cattell and Fullerton show that extent can be judged better than time.

3. Although the constant error when one movement is made faster than another is in the direction of compensation it is not sufficient for it.

4. If we judged by time alone the difference between long and short or fast and slow movements would have no meaning for us aside from terms of visual space or of force.

5. There is no *a priori* reason for believing any one perception to be more fundamental than others. The sensations of movement are varied enough to afford each sort of judgment a sensory basis of its own. Introspectively there seems to be no inference from one perception to another.

But Woodworth points out the need of more and crucial experimental data on the precise relation between the two factors and suggests the three following methods of procedure:

1. Confusion of the supposed primitive perception. The other, if derived, should, under such circumstances, be less accurate than ordinarily.

2. The method of correlation or of incidental observation of one factor while the observer is occupied with the other.

3. Separate accuracy tests. No purely derivative perception should be found to be more accurate than the more primitive perception on which it is based. It should, indeed, be less accurate, since the process of derivation would probably introduce additional errors.

The first method here suggested has frequently been applied and has resulted in considerable irregular but incommensurate disturbance of the presumably derived perception. The chief difficulty with the second method seems to have been that of arranging apparatus which would register simultaneously the extent and duration of movements of any considerable magnitude. The method appears not to have been employed until the experiments of Jaensch, and these were under restricted conditions and with unsatisfactory apparatus. The apparatus used in the present research is peculiarly adapted to such procedure, and in the present chapter experiments will be reported in which the method was followed with th[illegible] servers. The results of Cattell and Fullerton,[5] who fo[illegible] as in ception and reproduction of extent to be more [illegible]hin a group

[5] "Small Differences," pp. 103–116.

of time have been quoted as an illustration of the third method. But certain disadvantages in the apparatus used in these observations seem to make further experiments desirable. In the experiments reported by these authors the duration of the movement was not completely under the control of the observer. His task was to reproduce, by a movement of 50 cm., the time of a previous movement of the same extent. It was not a question of stopping a controlled movement at the expiration of a given period, but of reaching a certain fixed point at such a time. If the speed was miscalculated no correction could be made, since, if the distant point were not yet reached at the proper time it was necessary to continue the movement beyond the time felt to be sufficient, while, if the point happened to be passed too soon, the record was already made. Under these conditions it would seem that perception of speed rather than perception of duration was under investigation. Moreover these writers report data for only $\frac{1}{4}$, $\frac{1}{2}$ and 1 second. With the duration more perfectly under the control of the observer the perception and reproduction of time might conceivably be found to be more accurate than under the conditions just described. With the present apparatus such conditions are easily fulfilled. As the car moves along the track the duration is recorded accurately and graphically at every point in the progress of the movement. Variations and errors of speed need not interfere with the execution of duration, and the recording of the time is quite independent of the extent passed over. With these favorable conditions experiments on the perception and reproduction of duration were performed on two subjects and these also will be reported in the present chapter. In securing the results to be given later we are thus applying the second and third methods indicated by Woodworth.

On the basis of factor explicitly studied and method followed the experiments may be divided into five groups.

1. Determination of the accuracy of perception and of reproduction of extent. In these experiments four observers were used. On three, W., H. and Bt., the magnitudes ranged from 100 mm. to 400 mm. This total range was broken up into six sections, viz., 100–150, 150–200, 200–250, etc., and 75 trials within each section were made. A trial consisted in (*a*) the execution of a standard movement the magnitude of which was controlled by the signal from
-und hammer; (*b*) the attempt, at the word of the operator, to
[2] *Op. cit.* the extent of this standard; (*c*) after having completed
[3] *Zeit. f. Psy*... a guess, indicated by the word "more" or "less,"
[4] "Le Mouvement," which the error probably lay. With these

three subjects the continuous method was used, the terminal point of the standard serving as the starting point of the reproduction. On subject L. the successive method was employed, the initial points of the two movements being the same. A wider range of magnitudes was thus made possible. The movements used varied between 150 mm. and 650 mm., and the total range has been divided into five sections, viz., 150–250, 250–350, etc., 75 trials being made within each section. In the calculation of error the separate magnitudes were collected under the heading of the section within the limits of which the standards fell and the per cent. error calculated for each movement. These errors were then averaged to secure the average per cent. error for the group, and this error was analyzed into constant and variable errors. By this method it has been possible to avoid the distracting and complicating features involved in earlier methods of controlling the magnitude of the standard movement. There is no illusion of impact and yet the extent is completely under the control of the operator. At a comfortable rate of movement the reaction time in stopping the movement at the signal is exceedingly short. An important advantage lies in the fact that however delayed the reaction may be the space passed over in its execution is included, both introspectively and objectively, in the standard extent and no allowance need be made for it. Not only is the illusion of impact absent but the errors arising when the standards are free movements determined in their extent by the observer himself are not present.[6]

2. Determination of the accuracy of perception and reproduction of duration. These experiments were made on three subjects, W., Bt. and H. The method followed was precisely that of Ex. 1, except that the observer tried to reproduce the duration of the standard movement instead of its extent. In these movements the observer endeavored to move at an approximately constant speed, and this fact enabled the operator to vary the standard duration in the same way in which he varied the extent in the other experiments, by changing the position of the slide, the contact of which with the prong projecting from the car completed the sound hammer circuit and thus gave the signal for stopping the movement. Having executed the standard movement, the observer, at the command of the operator, went on to reproduce its duration by the continuous method. The total range of durations used was between 1 sec. and 3½ sec., and has been divided, for purposes of calculation, into five sections, viz., 1–1½, 1½–2, etc. The calculation of error was performed as in the case of extent: the per cent. error of each trial within a group

[6] See Chapt. II.

being averaged to secure the average error for that section, and this error analyzed into constant and variable errors. Within the limits of each section 75 trials were made. In each case, as in the experiments on extent, the observer, after his attempt at reproduction, guessed as to the probable direction of his error. During the course of the experiment frequent intervals occurred which were below or beyond the range of durations chosen. These were not included in the table.

These two experiments are calculated to bring us one step nearer our conclusion. If we find that the error of perception and reproduction for time is greater than that for extent, as suggested by Cattell and Fullerton, we may be justified in concluding that extent is not judged in terms of time. If the error for time is less than or equal to that for extent, we may still be in some doubt.

3. Incidental observation of the relation between the durations in the experiments in which the observer tried, in his most natural way, to reproduce the extent. Since the instrument records graphically not only the extent but the duration there is no difficulty in securing such a correlation. The observer devotes his attention to the extent of his movements. The observer having made his reproduction and guessed as to the direction of his error, we can determine two facts concerning the process: (*a*) whether the times or the extent of the two movements agree more closely, (*b*) whether or not the subsequent introspective impression indicated by the judgment of "more" or "less" agrees more often with the objective relation of the extents or with that of the times.

Further, the agreement of the durations secured in this way can be compared:

(*a*) With the accuracy of the reproduction of extent. If the times are perceived and reproduced less accurately than the extents we shall not be justified in supposing the perception of time to be the more fundamental of the two. On the other hand, if the times agree more closely than the extents we may be justified in further consideration of the theory.

(*b*) With the accuracy of the perception and reproduction of time intervals when such performance is the explicit and deliberate attempt of the observer. If the agreement of the records incidentally secured is less complete and uniform than in the case of those explicitly performed, we may infer that the judgments of extent were not made in terms of duration. If there is no considerable deviation in the results secured by the two different methods, the theory of the more primitive character of the judgment of time, as proposed by Loeb, Kramer and Moskiewicz and Jaensch, while not demonstrated, is at least made exceedingly plausible.

4. Incidental observation of the relations of the extents in the experiments in which the observer sought to reproduce the times. This procedure is rendered easy by the same features of the apparatus that make the preceding observations possible. In the observer's attempts to reproduce durations we have the extents also recorded, both of the standard and of the reproduction. (*a*) If we find the extents to disagree more than the durations we can draw no conclusion at all except that the perception and reproduction of the time is clearly not a function of the sense of extent of movement. (*b*) But if we find the extents to agree equally with or more closely than the times, it is possible that in reproducing time one seeks to make equal extents at the same speed, and that there is some direct sense for speed aside from the conscious relation of extent and duration.

5. The experiments constituting the fifth group were suggested by the procedure of Cattell and Fullerton in their analysis of the total error into error of perception and error of execution. The objective measurements may be supposed to reveal the basis of the perception involved in the total performance, consisting of the estimation of the standard and the attempt at reproduction. The guesses as to the probable direction of the error we may expect to give certain additional information on the same point. Of course the reproduction is always *intended* to be equal to the standard, and at the final moment the observer feels it to be so. Otherwise he would correct it, if it fell, short, by prolonging it. Or if the second movement happened to "get away" from him and this error was recognized, the consciousness of failure would result in a judgment of "more." *A priori,* then, one might be led to expect all guesses to be "more." As a matter of fact we get guesses of both "more" and "less." If they turn out to be approximately equal in number, no matter what the actual error, we may treat them as merely chance guesses. But if one type considerably exceeds the other we may suppose that its direction was determined by an actual retrospective difference in either the extents or the durations. Thus, if we find guesses of "more" in excess and on consulting the objective relations of the movements we find a tendency to a positive constant error in the times while the errors of the extents are indifferently distributed or are negative, we would not be unjustified in assuming that the direction of the guesses was determined by the perception of time rather than by that of extent. Further inference would depend on the nature of the task which the observer was explicitly trying to accomplish in the trials concerned.

Certain other points descriptive of the experiment as a whole may now be indicated. In all trials visual criteria were eliminated, either by having the observer work with closed eyes or by allowing him to wear a blind mask. In the beginning of the experiment the ears were plugged in order to exclude the secondary criteria afforded by the noise of the apparatus or by the sound of the vibrating reed interrupter which controlled the magnet of the time marker. But very early in the experiment improvements in the running gear of the car made the operation of this apparatus noiseless, and at the same time the interrupter was muffled by the use of boxing and padding. This was probably an unnecessary precaution, however, for the vibrations at the rate of 10 per second can scarcely serve as criteria for duration.

TABLE XI

Reproduction of Extent. First Column, Deliberate. Second Column, Incidental

Observer W.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	8	12	12	18	14	15	9	18	13	13	16	19	12	16
	C.E.	− 1	+ 1	+ 9	+ 5	+16	−10	0	−11	− 1	−10	−12	−19	− 2	− 7
	V.E.	*8*	*11*	*10*	*19*	*14*	*13*	*9*	*15*	*13*	*12*	*11*	*7*	*11*	*13*
	Trials	15	25	15	3	15	13	15	22	15	15	15	7	90	85
5	A.E.	15	11	14	10	23	14	18	5	19	9	12	14	17	11
	C.E.	+ 1	+ 6	+ 5	+ 5	+23	− 2	+ 5	+ 1	− 4	− 7	+ 1	−14	+ 5	− 2
	V.E	*15*	*10*	*14*	*9*	*16*	*13*	*17*	*7*	*18*	*6*	*11*	*7*	*15*	*9*
	Trials	15	19	15	10	15	15	15	14	15	16	15	8	90	82
10	A.E.	13	35	20	19	22	23	13	12	12	13	17	20	16	20
	C.E.	− 2	+ 32	+10	+10	+13	+13	+ 5	+ 2	− 1	− 7	−15	−20	+ 2	+ 4
	V.E.	*12*	*22*	*19*	*18*	*20*	*22*	*11*	*11*	*12*	*10*	*10*	*6*	*14*	*15*
	Trials	15	15	15	25	15	13	15	15	15	17	15	11	90	96
15	A.E.	20	19	10	12	15	16	15	17	16	11	12	12	15	16
	C.E.	+12	+ 11	+ 4	+ 1	+ 7	+ 4	+ 3	+ 6	− 2	− 1	− 8	−12	+ 3	+ 2
	V.E.	*19*	*18*	*10*	*11*	*14*	*14*	*11*	*14*	*16*	*11*	*11*	*5*	*14*	*12*
	Trials	15	21	15	17	15	19	15	14	15	11	15	4	90	86
30	A.E.	26	18	20	14	16	21	23	18	25	17	47	33	28	20
	C.E.	−25	+ 2	−17	− 4	−26	−14	−23	−12	−25	−12	−47	−33	− 27	− 12
	V.E.	*14*	*19*	*17*	*13*	*9*	*14*	*7*	*17*	*10*	*14*	*4*	*8*	*10*	*14*
	Trials	15	20	15	17	15	18	15	18	15	13	15	2	90	88
Av.	A.E.	16	19	15	15	20	18	16	14	17	13	21	20	18	17
	C.E.	− 3	+ 10	+ 2	+ 3	+ 7	− 2	− 2	− 4	− 7	− 7	−16	−20	6	8
	V.E.	*13*	*16*	*14*	*14*	*15*	*15*	*11*	*13*	*14*	*11*	*9*	*7*	*13*	*13*
	Trials	75	100	75	72	75	78	75	83	75	72	75	32	450	437

In order to secure information concerning the influence, on the accuracy of reproduction, of the time interval elapsing between the standard and the attempt to repeat it, five different intervals were used throughout all the experiments, 15 trials for each section, both for time and for extent, being made with intervals of 2 sec., 15 with intervals of 5 sec., and similarly with intervals of 10 sec., 15 sec. and 30 sec., these constituting, for each section, the 75 trials mentioned above. The results on the influence of the time interval will be brought together in Chapter VI.

TABLE XII

Reproduction of Extent. First Column, Deliberate. Second Column, Incidental

Observer H.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	25	25	19	12	19		16	15	13	12	10	9	17	15
	C.E.	+25	+25	+19	+ 9	+19		+13	+ 5	+10	— 5	+ 8	— 6	+16	+16
	V.E.	*14*	*13*	*10*	*9*	*9*		*10*	*14*	*11*	*11*	*10*	*7*	*11*	*11*
	Trials	15	12	15	9	15		15	11	15	14	15	23	90	69
5	A.E.	33	21	20	18	25	24	23	15	10	11	11	10	20	17
	C.E.	+33	+11	+20	+15	+22	+10	+23	+15	+ 7	+ 4	+ 2	+ 1	+18	+ 9
	V.E.	*14*	*24*	*10*	*13*	*13*	*22*	*11*	*8*	*8*	*10*	*11*	*10*	*11*	*15*
	Trials	15	15	15	11	15	11	15	16	15	18	15	14	90	85
10	A.E.	38	15	22	34	22	22	29	17	15	14	9	11	23	19
	C.E.	+38	+10	+22	+30	+22	+12	+29	+13	+14	+14	+ 6	— 7	+22	+11
	V.E.	*11*	*9*	*14*	*20*	*9*	*17*	*9*	*10*	*8*	*12*	*7*	*10*	*10*	*15*
	Trials	15	4	15	5	15	9	15	23	15	24	15	20	90	85
15	A.E.	25		28	20	27	15	24	13	16	12	20	12	23	14
	C.E.	+25		+25	+19	+27	+11	+24	+ 4	+13	— 9	+20	—12	+22	+ 3
	V.E.	*17*		*15*	*9*	*12*	*10*	*9*	*14*	*12*	*10*	*7*	*7*	*12*	*10*
	Trials	15		15	11	15	23	15	15	15	13	15	11	90	73
30	A.E.	18	12	30	5	21	12	29	11	21	10	9	7	21	10
	C.E.	+ 7	— 7	+28	+ 1	+21	— 1	+29	+10	+16	+ 3	+ 4	— 1	+18	+ 1
	V.E.	*16*	*8*	*19*	*5*	*18*	*12*	*17*	*8*	*14*	*10*	*9*	*7*	*16*	*8*
	Trials	15	5	15	3	15	13	15	21	15	26	15	16	90	84
Av.	A.E.	28	18	24	18	23	18	24	14	15	12	12	10	21	15
	C.E.	+26	+10	+23	+14	+22	+ 8	+24	+ 9	+12	+ 7	+ 8	— 5	19	9
	V.E.	*14*	*16*	*14*	*11*	*12*	*15*	*11*	*11*	*11*	*11*	*9*	*8*	*12*	*12*
	Trials	75	36	75	39	75	56	75	86	75	95	75	84	450	396

So far as possible the experiments were performed in an order which would distribute the results of practise. All the subjects had made a great many preliminary trials before the records presented in the present chapter were taken. In general but one or two time intervals were used at a single sitting, and the various magnitudes were taken in a chance order, except toward the end of the experiment, when it became necessary to give definitely chosen magnitudes in order to secure the requisite number of trials for each section of the total range.

The results for each observer, for both extent and duration, are shown in full in Tables XI. to XVII. The records on separate accuracy are given in each table along with the errors for the same factor, for W., H. and Bt. when the observer's attention is fixed on the reproduction of the other factor. Thus in Table XI. are given the A.E., C.E. and V.E. for observer W., for the perception and reproduction of extents—in the first column under each section

TABLE XIII

Reproduction of Extent. First Column, Deliberate. Second Column, Incidental

Observer Bt.

Interval	Error	mm. 100–150		mm. 150–200		mm. 200–250		mm. 250–300		mm. 300–350		mm. 350–400		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	25	31	17	23	22	9	16	26	12	20	22	13	19	20
	C.E.	+23	+30	+10	+19	+ 8	— 3	— 2	—10	— 5	—20	—21	—13	+ 2	+ 1
	V.E.	*14*	*16*	*17*	*18*	*22*	*13*	*16*	*23*	*12*	*6*	*9*	*9*	*15*	*14*
	Trials	12	12	11	14	10	10	10	11	10	6	10	10	63	63
5	A.E.	26	26	27	30	8	16	16	13	19	28	27	17	21	22
	C.E.	+23	+22	+25	+27	+ 6	—16	+13	+ 3	—17	—16	—27	—17	+ 4	0
	V.E.	*23*	*19*	*21*	*19*	*6*	*9*	*15*	*12*	*12*	*15*	*11*	*9*	*15*	*14*
	Trials	11	16	10	10	10	5	10	6	13	5	10	11	63	53
10	A.E.	58	41	35	14	32	16	10	17	14	21	23		29	22
	C.E.	+51	+37	+31	+10	+32	+ 9	— 4	—16	—10	—20	—23		+13	+ 4
	V.E.	*38*	*26*	*21*	*10*	*17*	*15*	*13*	*7*	*13*	*9*	*10*		*19*	*13*
	Trials	11	18	10	6	10	11	14	8	13	11	10		68	54
15	A.E.	87	45	56	27	28	16	15	15	13	22	25	26	37	25
	C.E.	+87	+41	+56	+ 4	+26	— 2	+ 8	— 9	— 4	—11	—25	—26	+25	— 1
	V.E.	*32*	*35*	*29*	*26*	*17*	*16*	*13*	*8*	*14*	*18*	*12*	*6*	*20*	*18*
	Trials	10	10	10	11	10	4	10	15	9	8	10	10	59	58
30	A.E.	73	27	50	37	30	18	17	16	13	21	30	32	36	25
	C.E.	+73	+21	+49	+37	+27	+ 7	+ 9	+16	— 1	+7	—30	—32	+21	+ 9
	V.E.	*42*	*25*	*40*	*26*	*27*	*17*	*15*	*10*	*13*	*21*	*7*	*7*	*26*	*18*
	Trials	5	10	5	6	5	8	5	6	6	8	8	12	33	50
Av.	A.E.	54	34	37	26	24	15	15	17	14	22	25	22	28	23
	C.E.	+51	+30	+34	+14	+20	— 1	+ 5	— 3	— 7	—12	—25	—22	24	15
	V.E.	*29*	*24*	*26*	*20*	*18*	*14*	*14*	*12*	*13*	*14*	*10*	*8*	*18*	*15*
	Trials	48	66	46	47	45	38	49	46	51	38	48	43	287	264

when the attempt is to reproduce extent, in the adjacent column when the attempt is to reproduce the durations, the extent being recorded incidentally. In the case of observer L. only trials for the reproduction of extent were made, and the table gives, alongside these errors, the corresponding errors for the durations of the same movements. In all the tables the records are grouped under their appropriate sections, and the results for the different time intervals are indicated separately. At the foot of each table the results for the particular sections for all five time intervals are averaged, while on the right of each table are brought together the results of each section for the particular time intervals. Reading across at the foot of the table from left to right we have given the influence of the magnitude on the amount of the error, while reading down the

TABLE XIV

REPRODUCTION OF DURATION. FIRST COLUMN, DELIBERATE. SECOND COLUMN, INCIDENTAL

Observer W.

Interval	Error	1–1.5 sec.		1.5–2 sec.		2–2.5 sec.		2.5–3 sec.		3–3.5 sec.		* 3.5–4 sec.	1–3.5 sec. Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	Per Cent.	
2	A.E.	8	22	10	15	11	16	11	18	11	28	48	10	20
	C.E.	− 1	+14	+ 1	+ 4	0	− 1	− 6	− 4	− 7	−28	−48	− 2	− 3
	V.E.	8	21	10	14	14	16	10	16	9	4	17	10	14
	Trials	15	24	15	18	15	28	15	13	15	6	8	75	89
5	A.E.	15	34	18	22	13	19	8	19	8	27	22	13	24
	C.E.	+12	+23	+ 3	+11	+ 2	− 7	− 0.2	− 9	− 4	−24	+ 7	+ 3	− 1
	V.E.	10	28	18	18	17	18	8	17	7	14	22	12	19
	Trials	15	18	15	12	15	24	15	23	15	11	9	75	88
10	A.E.	26	35	24	19	12	21	8	20	7	16	20	16	22
	C.E.	+26	+29	+24	− 1	+ 4	+15	+ 2	+ 5	+ 2	+ 2	−14	+12	+10
	V.E.	11	29	11	19	12	20	7	19	9	16	16	10	21
	Trials	15	8	15	20	15	15	15	18	15	15	13	75	76
15	A.E.	27	29	13	24	10	36	11	22	12	18	26	15	26
	C.E.	+23	+20	+ 9	+17	+ 4	+21	+ 4	−22	+ 9	− 1	−21	+10	+ 7
	V.E.	17	30	12	19	9	35	12	18	10	18	17	12	24
	Trials	15	8	15	8	15	11	15	5	15	17	18	75	49
30	A.E.	18	29	13	25	8	18	7	17	10	29	19	11	24
	C.E.	+ 7	+ 8	+ 0.2	+13	− 1	+12	+ 7	+ 5	+ 3	+13	− 5	+ 4	+10
	V.E.	16	26	13	22	8	17	5	12	10	27	19	10	21
	Trials	15	3	15	10	15	10	15	3	15	15	14	75	41
Av.	A.E.	19	30	16	21	11	22	9	19	10	23	27	13	23
	C.E.	+14	+15	+ 8	+ 9	+ 2	+ 8	+ 1	+ 5	+ 0.4	−14	−15	5	10
	V.E.	15	27	13	18	11	21	8	16	9	16	18	11	20
	Trials	75	61	75	68	75	88	75	62	75	64	62	375	343

* Incidental.

column on the right the figures indicate the influence of the lengthening of the time interval on the accuracy for the total range of the standard magnitudes. The figures in the lower right hand corner of each table represent the average errors of the total range regardless of the difference in time interval. We are now ready to analyze the results according to the principles outlined above.

Taking up the five experiments in the order in which they have been described, we have first to consider those on the accuracy of perception and reproduction of extent of movement when the standard magnitudes are neither free movements nor movements terminating in impact, but controlled movements, the magnitudes of which are determined by their arrest on the part of the subject at the sound hammer signal. The results for the four observers are to be found in the first columns under each section in Tables XI., XII., XIII. and XVII.

TABLE XV

Reproduction of Duration. First Column, Deliberate. Second Column, Incidental

Observer H.

Int.	Error	1–1.5 sec.		1.5–2 sec.		2–2.5 sec.		2.5–3 sec.		3–3.5 sec.		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	23	31	15	15	9	9	7	11	7	12	12	16
	C.E.	+23	+31	+11	+15	+ 5	+ 2	+ 5	+ 2	— 2	—12	+ 9	+ 8
	V.E.	*6*	*10*	*12*	*13*	*8*	*9*	*6*	*16*	*6*	*7*	*8*	*11*
	Trials	15	18	15	34	15	26	15	9	15	3	*15*	90
5	A.E.	26	22	20	15	26	12	15	9	14	10	20	14
	C.E.	+26	+22	+19	+14	+10	+ 2	+15	— 3	+12	+ 5	+17	+ 8
	V.E.	*9*	*12*	*10*	*10*	*11*	*12*	*8*	*8*	*10*	*7*	*9*	*10*
	Trials	15	23	15	30	15	19	15	8	15	7	75	87
10	A.E.	14	22	26	14	29	14	23	9	22	17	23	15
	C.E.	+ 5	+15	+26	+10	+29	+ 8	+23	+ 9	+20	+ 8	+20	+10
	V.E.	*14*	*19*	*17*	*13*	*17*	*12*	*8*	*12*	*11*	*15*	*13*	*14*
	Trials	15	17	15	22	15	29	15	7	15	5	75	80
15	A.E.	19	23	23	19	11	14	8	9	31		19	13
	C.E.	+13	+18	+23	+16	+ 8	+ 8	+ 7	— 4	+29		+16	+ 9
	V.E.	*15*	*18*	*13*	*15*	*12*	*12*	*5*	*9*	*18*		*12*	*12*
	Trials	15	23	15	30	15	26	15	6	15		75	85
30	A.E.	33	16	26	17	16	16	17	14	33		25	13
	C.E.	+33	+13	+26	+13	+10	+13	+ 8	— 2	+30		+21	+ 9
	V.E.	*23*	*13*	*11*	*14*	*14*	*15*	*15*	*13*	*18*		*16*	*14*
	Trials	15	15	15	24	15	16	15	8	15		75	63
Av.	A.E.	23	23	22	16	18	13	14	11	22	13	20	15
	C.E.	+20	+20	+21	+14	+12	+ 7	+12	+ 1	+16	+ 0.3	16	8
	V.E.	*13*	*14*	*12*	*13*	*12*	*12*	*8*	*12*	*13*	*10*	*12*	*12*
	Trials	75	96	75	140	75	116	75	38	75	15	375	405

For W. the gross average per cent. error for the several sections varies between 15 per cent. and 21 per cent., with an average, for the total range, of 18 per cent., and increases with the magnitudes in rather close approximation to Weber's law, the D.L. being between $\frac{1}{6}$ and $\frac{1}{5}$. The C.E.'s are not large in the final average, though they range from — 16 per cent. to + 7 per cent. in the separate sections, the average regardless of signs,[7] for the total range being 6 per cent.

[7] In calculating the final average C.E.'s signs have been disregarded in all these tables, since it is the absolute magnitude rather than the direction of these errors which is significant as a measure of accuracy.

The phenomenon of the indifference point is found here, but is seen to be due to the constant errors in the cases in which the intervals were rather short, no positive errors occurring in the individual averages beyond the interval of 15 sec. With these short intervals

TABLE XVI

REPRODUCTION OF DURATION. FIRST COLUMN, DELIBERATE. SECOND COLUMN, INCIDENTAL

Observer Bt.

Interval	Error	1-1.5 sec.		1.5-2 sec.		2-2.5 sec.		2.5-3 sec.		3-3.5 sec.		Averages and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	31	23	26	21	11	16	22	23	20	14	22	19
	C.E.	+29	+19	+26	− 4	0	−10	−20	−20	−20	−14	+ 3	− 6
	V.E.	*17*	*19*	*9*	*21*	*11*	*16*	*13*	*20*	*9*	*10*	*12*	*17*
	Trials	10	18	10	10	10	15	10	5	10	10	50	58
5	A.E.	30	34	26	18	15	24	20	26	10	30	20	26
	C.E.	+20	+25	+24	+ 6	+ 3	+11	+ 2	−15	− 6	−30	+ 9	− 1
	V.E.	*19*	*33*	*16*	*22*	*15*	*21*	*21*	*24*	*10*	*9*	*16*	*22*
	Trials	10	13	10	12	16	20	10	14	10	11	56	70
10	A.E.	52	42	18	41	36	21	16	21	18	32	28	31
	C.E.	+52	+40	+17	+18	+36	− 3	+11	−20	− 3	−32	+22	− 1
	V.E.	*24*	*29*	*17*	*37*	*7*	*20*	*15*	*18*	*18*	*7*	*16*	*22*
	Trials	12	18	10	16	10	15	10	8	10	5	52	62
15	A.E.	51	37	32	17	18	24	13	21	12	32	25	26
	C.E.	+51	+35	+27	+13	+12	−14	+ 1	+ 4	− 9	−32	+16	+ 1
	V.E.	*30*	*22*	*17*	*13*	*13*	*20*	*13*	*21*	*10*	*15*	*17*	*18*
	Trials	16	24	10	19	10	13	10	4	10	2	56	62
30	A.E.	48	75	62	24	37	26	15	18	18	23	36	34
	C.E.	+48	+75	+62	+24	+37	+ 6	− 3	− 6	−18	− 5	+25	+19
	V.E.	*25*	*26*	*29*	*14*	*12*	*24*	*15*	*20*	*6*	*23*	*17*	*21*
	Trials	10	7	10	9	10	10	10	7	10	2	50	35
Averages.	A.E.	42	40	33	24	23	22	17	22	16	26	26	27
	C.E.	+40	+39	+31	+11	+18	− 2	− 2	−11	−11	−23	20	17
	V.E.	*23*	*26*	*18*	*21*	*12*	*20*	*15*	*21*	*11*	*13*	*16*	*20*
	Trials	58	80	50	66	56	73	50	38	50	30	264	287

the effect on judgment of the total range of magnitudes has time to operate. In the case of the longer intervals the separate trials are distributed over so great a period that the group influence is not found to be effective, the C.E.'s being all negative. The final variable errors range from 9 per cent. to 15 per cent., averaging 13 per cent., and increase in rough accord with Weber's law, though there is indication, in the fourth and sixth sections, of a tendency toward a slower rate of increase.

Turning to H.'s table we find remarkable similarity in the results. The A.E.'s tend to be slightly larger than for W., ranging from 12 per cent. to 28 per cent., with a final average of 21 per cent. as over against 18 per cent. for W. The V.E.'s are identical for the second, fourth and sixth sections, and in no section is the difference more than 3 per cent. The final average V.E. for H. is 12 per cent., as over against 13 per cent. for W. Only in the case of the C.E.'s is there great deviation. H. has no negative C.E.'s, hence no indifference point occurs. It may be noted that this observer is the writer and that he had already performed the experi-

TABLE XVII

REPRODUCTION OF EXTENT. FIRST COLUMN, EXTENTS. SECOND COLUMN, DURATION

Observer L.

Int.	Error	mm. 150-250		mm. 250-350		mm. 350-450		mm. 450-550		mm. 550-650		Average and Trials	
		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.		Per Cent.	
2	A.E.	16	26	9	19	12	17	5	14	5	13	11	18
	C.E.	—12	+15	— 6	+10	—10	— 2	+ 0.3	— 8	— 2	—13	— 6	+ 0.6
	V.E.	*11*	*20*	*7*	*15*	*5*	*17*	*5*	*10*	*4*	*6*	*6*	*14*
	Trials	15		15		15		15		15		75	
5	A.E.	13	16	8	13	13	14	9	13	6	15	9	14
	C.E.	— 1	+ 8	— 6	+ 1	—13	—10	— 8	—12	— 3	— 9	— 6	— 4
	V.E.	*13*	*17*	*6*	*13*	*5*	*10*	*6*	*7*	*5*	*10*	*7*	*11*
	Trials	15		15		15		15		15		75	
10	A.E.	13	12	12	12	12	19	12	19	9	20	12	17
	C.E.	—10	+ 4	— 9	—11	—11	—13	—10	—13	— 9	—20	—10	—10
	V.E.	*7*	*14*	*10*	*10*	*10*	*14*	*9*	*9*	*6*	*6*	*8*	*11*
	Trials	15		15		15		15		15		75	
15	A.E.	14	24	11	6	6	21	5	18	3	17	8	20
	C.E.	— 5	— 8	— 8	— 3	— 3	— 9	— 3	—14	— 0.5	—14	— 4	—11
	V.E.	*14*	*20*	*9*	*6*	*6*	*16*	*5*	*12*	*3*	*14*	*8*	*16*
	Trials	15		15		15		15		15		75	
30	A.E.	14	20	7	16	16	18	9	14	8	26	12	18
	C.E.	— 7	+ 5	+ 1	—13	—13	— 9	— 9	—14	— 6	—14	— 7	— 7
	V.E.	*11*	*19*	*7*	*11*	*11*	*17*	*5*	*7*	*5*	*20*	*7*	*15*
	Trials	15		15		15		15		15		75	
Av.	A.E.	14	20	9	12	12	18	8	16	6	18	10	17
	C.E.	— 7	+ 5	— 6	—10	—10	— 9	— 6	—13	— 4	—14	7	5
	V.E.	*11*	*18*	*8*	*7*	*7*	*15*	*6*	*9*	*5*	*11*	*7*	*13*
	Trials	75		75		75		75		75		375	

ments of Chapter IV. The knowledge of the usual tendency to constant errors in such trials seems to have operated, though quite unintentionally, toward correction. The C.E.'s range between + 8 per cent. and + 26 per cent., averaging 19 per cent. as against 6 per cent. for W. With this observer all the errors increase considerably more slowly than Weber's law would require, and somewhat more rapidly than the square root law of Cattell and Fullerton suggests.

The errors in the case of Bt. are still larger. The A.E's ranging from 14 per cent. to 54 per cent., averaging 28 per cent., the C.E's ranging through — 25 per cent. to + 51 per cent., averaging 24 per cent. and the V.E's varying between 10 per cent. and 29 per cent., and increasing much more slowly than the square root law. The final average V.E. for Bt. is 18 per cent., as against 13 per cent. for W. and 12 per cent. for H. The phenomenon of the I.P. shows itself after all intervals.

The results secured from observer L., by the successive method, appear in Table XVII., in the first column under each section and show slightly greater accuracy of reproduction than was found in the preceding cases. The size of the final averages, however, is likely to be misleading here. The A.E. and V.E. increase with the magnitude at a rate approximating the square root law, and since the upper limit of the range of standard magnitudes is much higher here than in the case of W. and H. the final average errors are expressed, in per cents, by smaller figures. But if we inspect the lower part of the range which is common to all the tables, the errors are found to be only slightly less. For L. the A.E.'s for the total range vary between 6 per cent. and 14 per cent., averaging 10 per cent., the C.E.'s are all negative and range from — 4 per cent. to — 10 per cent., averaging 7 per cent., while the V.E.'s, lying between 5 per cent. and 11 per cent., average 7 per cent. The absence of an indifference point is to be explained by the fact that no effort was made to produce a series effect—that is to say, the magnitudes used at a single sitting or taken on a single record sheet did not vary over the total range but lay within a few adjacent sections.

The results of the second group of experiments, on perception and reproduction of duration, are indicated in Tables XIV.–XVI. in the first columns under each section. For W. the A.E.'s for the separate sections range from 9 per cent. to 19 per cent., averaging 13 per cent., and increasing with the magnitude of the standard in close agreement with a cube root law. The sectional C.E.'s range from + 1 per cent. to + 14 per cent., averaging 5 per cent. Of considerable interest is the fact, already mentioned in another connection, that an indifference point shows itself only when the time intervals are so short as to allow the judgment to be influenced by the group effect. The variable error ranges through 8–15 per cent., averaging 11 per cent. and increasing with the magnitude with an approximation to a cube root law. In the table for H. the A.E.'s increase according to Weber's law, ranging between 14 per cent. and 23 per cent. and averaging 20 per cent. The C.E.'s are positive, varying from 12 per cent. to 21 per cent. with an average at 16 per cent. No indication of an indifference point is present except after the 2-sec. interval. The V.E. seems to follow Weber's law, being remarkably constant in terms of per cent. and averaging 12 per cent. for the total range as over against 11 per cent. in the case of W.

In the case of Bt. again, all errors are greater, and the constant errors change their sign at the indifference point for this particular range of magnitudes—about 2.5 sec. All errors increase too slowly for even a square root law, the final averages being, A.E. 26 per cent., C.E. 20 per cent., V.E. 16 per cent.

These two experiments completed, we have complied with the conditions of the third method proposed by Woodworth. We now have separate accuracy tests for extent and for such durations as are ordinarily employed in the execution of such extents, and are in position to compare the records. Bringing together the final averages for the three subjects we have the following.

TABLE XVIII [8]

	W.			H.			Bt.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent	18	6±2	*13±.6*	21	19±1.7	*12±.6*	28	24±3.8	*18±1.5*
Time	13	5±1.3	*11±.7*	20	16±2	*12±.9*	26	17±2.8	*16±1.2*

If these averages are taken at their face value the results are just the reverse of those of Fullerton and Cattell. With the single exception of H.'s V.E. all the actually obtained errors are less for time than for extent. But the differences in the case of the V.E.'s of W. and Bt. are exceedingly small, and in the case of Bt. quite within the limits of error of the averages compared. The same is true of the C.E.'s of W. and H., while H.'s V.E.'s agree. Positive inference as to the greater accuracy of perception and reproduction of time would be insecure, although the results tend on the whole, to suggest such an inference. The separate accuracy tests, then, yield little information as to the probable basis of the judgment of extent. If the errors in the case of extent had been smaller than those for time there would have been reasonable certainty that the

[8] In Tables XVIII., XIX. and XX. the reliability of the average V.E. and C.E. is given in terms of mean square error. The measure of variability was calculated from the formula $(A.D. \times 1.25)/\sqrt{n}$, in which A.D. = the average deviation of separate V.E.'s of Tables XI.–XVI. from their final averages, and n = the number of determinations of the V.E. under different conditions of magnitude and interval. In most cases this number was 30. The chances are thus about 2:1 (more exactly, 68:32) that the true final average does not differ from that obtained from our figures by more than this mean square error. Thus in Table XVIII. the chances are 2:1 that W.'s V.E. for extent lies between 12.4 and 13.6, or similarly that his V.E. for time is not less than 10.3 or greater than 11.7. See Thorndike, "Mental and Social Measurements," Science Press, New York, 1904, pp. 59 and 139.

judgment of extent was not made on the basis of the duration of the movement. On the other hand, had the accuracy for time been greater than that for extent, there would have been reason for supposing the temporal judgment to be the more fundamental. Perhaps as reasonable a conclusion as any from the figures as they stand would be that the judgments of the time and the extent of our movements are identical.

This can be decided finally only by the methods of correlation next to be considered. Of the three methods proposed by Woodworth the first and third yield results somewhat favorable to the hypothesis, through the following facts:

(*a*) Disturbance of the time element has been found, by other investigators, to confuse the perception of extent.

(*b*) The present experiment seems to suggest the possibility of slightly greater accuracy of perception and reproduction of time, though the evidence is slight. But the experiment affords positive evidence that the accuracy for time is not less than for extent.

We have now to consider the agreement of the durations when the observer is attending to the extents, in the cases of all four subjects. The records for W., H. and Bt. are to be found in Tables XIV.–XVI., in the second columns under each section heading, parallel with the errors when the durations themselves are the object of attention. For W. the gross errors, ranging from 19 per cent. to 30 per cent., average 23 per cent., the final C.E. is 10 per cent. and the V.E.'s, varying between 16 per cent. and 27 per cent., average 20 per cent. For H. the A.E., C.E. and V.E. are considerably lower, averaging 15 per cent., 8 per cent. and 12 per cent. For Bt. the gross errors range through 22 per cent. to 40 per cent., averaging 27 per cent., the final C.E. is 17 per cent. and the V.E.'s, varying between 13 per cent. and 26 per cent., average 20 per cent. The results for L. are indicated in Table XVII., parallel with his records for extent. In the calculation of these results a slightly different method was used. The durations did not range widely, since there seemed to be a tendency to make all the movements in a time between the limits of .8 and 1.5 sec.[9] Consequently, in calculation the durations were not distributed under sections but the actual durations employed in making movements of a given extent section were compared and the error computed for the durations under each extent section. In the two columns then we have the error for extent and the error for duration for the same movements. The

[9] This tendency may stand in some relation to the idea of a most comfortable interval advanced by Jaensch in the case of movements, and by others in general studies of time sense.

A.E. for duration is 17 per cent., the C.E. 5 per cent and the V.E. 13 per cent. We are now in position to compare the accuracy for extent with the agreement of the durations incidentally secured. We may bring together the results as follows:

TABLE XIX

	W.			H.			Bt.			L.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent	18	6±2	13±.6	21	19±1.7	12±.6	28	24±3.8	18±1.5	10	7± .8	7±.6
Time	23	10±1.8	20±.9	15	8± .9	12±.9	27	17±2.8	20±1.3	17	5±1.5	13±.9

Two features of this table seem to me to afford basis for positive inference of considerable reliability. First, if we take the V.E.'s alone, we get results decidedly the reverse of those of Table XVIII. In that table the errors for extent were never smaller than for time. In the present table they are never larger. On the contrary, the V.E. for time is greater by 50 per cent. in the case of W., by 100 per cent. in the case of L. With Bt. the V.E. for time is greater, but the probable error is larger here than with the other subjects. In H.'s case the V.E.'s for extent and time under all conditions average exactly the same and have about the same probable error. It should be noted that H. is the writer and that in all the observations on him he was aware of the procedure and purpose of the experiment. This fact was felt introspectively to give a certain prominence to temporal relations even in reproductions of extent, while in reproductions of duration attention was called to spatial relations more than would be the case with a naive subject. Bt. also knew something of the purpose of the experiment, having listened to a short preliminary report. The rather close agreement in the case of these two subjects, in contrast with the striking differences in the case of the other two, is probably indicative of the difference in attitude resulting from the knowledge of the experiment and the unnatural prominence given to the incidental factor in each judgment. Comparing Tables XVIII. and XIX. we may say, with considerable certainty, that, although deliberate times fall out somewhat more accurately than extents, incidental times are subject to greater V.E.'s than their corresponding extents. There must, therefore, be some regulation of the extent independent of the incidental regulation of duration.

We have yet to observe the C.E.'s of Table XIX. In the case of W. the C.E. for time is greater than that for extent, agreeing with the relation of the V.E.'s and confirming the results of the preceding paragraph. In the other three cases the C.E.'s are smaller for time. In the case of these three observers the C.E.'s throughout the experiment are smaller for the incidental magnitudes than for the deliberate (for extent, see Table XX.). With these three subjects the C.E. seems to be more intimately bound up with the process of deliberate reproduction, or of attention to a certain magnitude. When extent is attended to it is subject to a greater C.E. than when its reproduction is incidental to the reproduction of a time magnitude. The C.E for this deliberately reproduced extent is, moreover, greater than that of the time magnitudes incidentally employed. Similarly the deliberate reproduction of time is affected with greater C.E. than the incidental reproduction of either time or extent. Although this relation does not hold for subject W., its prominence in the results of H., Bt. and L. seems to indicate a certain separation between the magnitude attended to and the other. This separation would argue for separate processes of judgment for the two magnitudes, extent and time.

We have yet to compare our incidental measurements of duration with those yielded by the deliberate attempt to reproduce time, in the case of observers W., H. and Bt. These records are to be found in Table XX., along with the corresponding records for extent.

TABLE XX

	W.			H.			Bt.			L.		
	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.	A.E.	C.E.	V.E.
Extent—												
deliberate	18	6±2	13±.6	21	19±1.7	12±.6	28	24±3.8	18±1.5	10	7± .8	7±.6
incidental	17	8±1.7	13±.8	15	9±1.3	12±.6	23	15±2.2	15±1.2			
Time—												
deliberate	13	5±1.3	11±.7	20	16±2	12±.9	26	20±3.5	16±1.2			
incidental	23	10±1.8	20±.9	15	8± .9	12±.6	27	17±2.8	20±1.3	17	5±1.5	13±.9

In the earlier description of this experiment the criterion for our conclusion was stated in these words: "If there is no considerable deviation in the results secured by the two methods (deliberate and incidental), the theory of the more primitive character of the judgment of time . . . is at least made exceedingly plausible." But "considerable deviations" are present. Only in the case of H. is there approach to agreement, the V.E.'s here being equal. The significance of the smaller C.E. for incidental time, shown here in the case of H., Bt. and L., has already been discussed. But the V.E.'s for W. and Bt. are much greater for incidental than for deliberate time, almost twice as large for W. and a fourth again as large for Bt. If duration were used as the basis of the judgment of extent, we should expect as great accuracy in the case of the incidental times as in the case of the deliberate. Except for the observation on the writer, this is not found.

Summing up the results of the experiment up to this point, we may say that though the argument is not overwhelming, the balance of evidence seems to show that while deliberate times are somewhat more accurately reproduced than deliberate extents, incidental times show an error greater than either, and this fact, along with the character of the C.E.'s points to separate processes of judgment for the two magnitudes. There is at least no justification for the attribution of more fundamental character to either judgment.

There is still another method of handling the data afforded by these experiments which may be supposed to throw some light on the amount of interdependence between the judgments of extent and of duration—the method of correlation. Ignoring the actual magnitude of error, we may regard each particular error solely from the point of view of its direction and correlate the errors for extent, in the case of a standard and its reproduction, with the incidental error or difference in the corresponding durations. Similar correlations may be made between the errors for time (in the attempt to reproduce duration) and the incidental differences of the corresponding extents. Such correlations have been made, for both cases, and for all observers, by the method of unlike signs. Thus each error in the case of reproduction of extent was classified as + or —, and the errors of the incidental times classified in the same way, regardless of their magnitude. These signs were then compared, in the case of each separate trial, for each magnitude and the per cent. of unlike signs computed for each observer. The same calculation was made for the reproductions of duration. From the resulting per cent. of unlike signs we are able to get the approximate coefficients of correlation given in Table XXI.

TABLE XXI

GIVING PER CENT. OF UNLIKE SIGNS (U) AND CORRESPONDING PEARSON COEFFICIENTS ($r = \cos \pi U$) OF CORRELATIONS BETWEEN ERRORS OF EXTENT AND ERRORS OF DURATION

Observer.		W.	H.	Bt.	L.
Reproducing extent	per cent. U	43	31	21	32
	r	.22	.56	.79	.54
Reproducing time	per cent. U	40	32	26	
	r	.31	.54	.67	

We may discuss the two groups separately:

(a) Correlations between deliberate extents and incidental durations. If in these observations the judgment of extent is based upon the perception of duration, we should expect high positive correlation—that is, the direction of the extent errors should correspond closely to the direction of the differences in time. In fact, if the speed of all the movements was equal and uniform we should expect perfect correlation, and a reproduction occupying more time than its standard would also cover more space. The only factor tending to reduce the positive correlation would be variation in speed. If the standard and reproduction were made at different rates and the perception of duration were more fundamental, the reproduction, if slower, would be shorter, if faster it would be longer, than the standard, and the errors for extent would be greater than for (incidental) time. We have found just the reverse to be true (Table XIX.). Consequently, if the perception of duration is here serving as the basis of judgment, we must suppose that the speed is practically equal and uniform in the case of standard and reproduction. In which case very high positive correlation is required. A chance relation will be indicated by $r = 0$ in which case per cent. $U = 50$. The actually obtained correlations are, as a matter of fact, positive and rather high in three cases, in the other case positive though rather low, the four *r's* being .22, .56, .79 and .54, averaging .53. As far as these figures go there seems to be a pretty strong tendency for errors in extent to correspond to errors in duration.

(*b*) Correlations between deliberate durations and incidental extents. There are two conceivable ways of approaching these data:

(1) Let us suppose the perception of time to be fundamental and the perception of extent derived. Then in the reproductions of durations we have no right to expect close correspondence of the incidental extents except in so far as there is, on the part of the observer, an habitual tendency to make movements agree in both respects. At any rate, we should expect the extent errors to be greater than those for duration. But we actually do find almost as high correlation in this case as in the preceding section, the three *r's* being .31, .54 and .67, averaging .51, as against .53 in the reproductions of extent. Moreover, if we compare the accuracy of the deliberately reproduced durations with the incidental variations of the extents (Table XIX.) we find the V.E.'s to be indistinguishable and the C.E.'s in two cases (H. and Bt.) actually smaller for extent, while for W. the C.E. for extent is slightly larger than for time, though the probable errors of the two do not allow them to be at all sharply distinguished.

(2) Let us suppose, for sake of comparison, that the perception of duration is based on that of extent. We should thus expect, on the same argument as that outlined in section (*a*), high positive correlation between extent and time. And, as already pointed out, we get a coefficient (.51) practically as high as in section (*a*), (.53). Following out the argument of section (*a*), we find indications of a dependence of the time judgment on the perception of extent.

The method of correlation, then, affords no very conclusive evidence on the question of primitiveness. All the coefficients show is that there is a considerable positive correlation between extent and duration, no matter which factor the observer is deliberately trying to reproduce.

The result of the observers' guesses as to the probable direction of their errors have yet to be presented. The per cent. of right guesses for all the different cases is shown in Table XXII., which shows the correspondence of the guesses for both factors in each experiment.

TABLE XXII

Task.	Factor.	W.	H.	Bt.	L.
		Per Cent. Right.	Per Cent. Right.	Per Cent. Right.	Per Cent. Right.
Times to be equal.	Times.	46	52	61	
	Extents.	49	56	65	
Extents to be equal.	Extents.	59	54	64	60
	Times.	53	58	63	56

The observer guessed with respect to the deliberate magnitude only, but the table gives the correspondence of these + or — guesses with the + and — variations of both the deliberate and the incidental magnitudes.

We have found (Table XIX.) that in reproductions of extent, extent falls out more accurately than time. Table XXIII. shows that in three cases out of four the guesses agreed more closely with the actual relations of the extents than with those of the durations. There is here a suggestion that the subsequent guess, and, supposedly, the initial judgment, were made on the basis of the factor explicitly attended to (extent). However, the same relation holds in the experiments on reproduction of time. Even here the guesses correspond more closely with the actual relations of the extents, although we saw (Table XX.) that there is no clear difference in accuracy of reproduction. The percentages of right cases are not high, as 50 per cent. would indicate only chance relationship. Moreover, the reliability of a statement of per cent. of right guesses lying within quite a wide region above and below 50 per cent. is not great. The reliability of any one of the figures in Table XXII. may be calculated from the formula $\sigma = \sqrt{pq/n}$, in which p = proportion of cases in which the event occurs (per cent. of right guesses), and q = proportion of cases in which the event does not occur (per cent. of wrong guesses). In all these cases n, the total number of guesses, is about 400. This gives a probable error of about 1.7 per cent. for each of these figures, which gives a low reliability to the differences actually shown. But the fact that in six out of the seven cases the guesses correspond more closely to the actual errors of the extents than to those of the times is unfavorable to the hypothesis that it is the perception of time on which the judgment of extent is based. Another argument in the same direction is the fact that the proportion of right guesses in experiments on the reproduction of extent is greater, for all observers, than the proportion of right guesses in the experiments on reproduction of duration.

These considerations seem to have additional significance when taken in connection with the comparison of the accuracy of reproduction for deliberate and incidental extents. Extents are seen to agree as closely when the observers are reproducing time as when they are attending to the extents, though it is not true that times incidentally measured are as accurately reproduced as those deliberately made. Instead of finding judgments of extent dependent on the perception of time we find indications of the more primitive character of the judgment of extent. At least the same argument which in experiments 1 and 2 excluded time as a factor of the judgment of extent now leads us to conceive the possible importance of the perception of extent in the process of reproducing duration. The judgment, it is true, may not be expressed in spatial terms—it may be, on the other hand, that the fundamental perception is of speed or rate of movement, and that the agreement of the extents is merely incidental to the reproduction of the speed. Reproduction of speed would call for nearly equal force or energy of contraction, and result in the production of a movement tending to agree in all respects—speed, extent and duration. That movements do tend to agree in all their attributes and that observers tend not so much to reproduce particular characteristics as to repeat the previous performance in its entirety we have already seen in Chapter II. And in the measurements of speed in in that chapter, for just such movements as those used in the present experiment, we found deviations of only 2 per cent. average in movements at the rate of 100 mm. per second. No such per cent. accuracy is found for either extent or time. Comparison of the agreement of the incidentally reproduced extents with that of the deliberately reproduced durations leads to pretty much the same situation. The V.E.'s are nearly equal, the A.E.'s deviate in different directions for the different observers, and then only by 4 or 5 per cent.

The idea of a direct sense for velocity is not a new one, having been suggested by Woodworth and asserted by Jaensch. That the perception of extent is influenced by the speed of the movement was long ago demonstrated by Goldscheider, who found the limen to decrease as the rapidity of movement increased. After the sensations of strain and resistance, the sensations attending different velocities seem to afford the closest approximation, in the perception of movement, to a graduated intensive series, and the actual existence of such a series is easily observed introspectively. Intensive differences in the sensations localized chiefly in the elbow joint can be distinctly felt in making the same forearm movement at different velocities. The fact that the limen decreases with higher rates of movement indicates the intensifying effect of speed on a subliminal stimulus, and if, as introspection shows, changes in velocity exert this intensifying influence on supraliminal sensations as well, we have provided a thoroughly adequate basis for direct perceptions of speed, without reference to the elements of either extent or duration. Although, physically, the calculation of velocity involves a relation of distance and duration ($V = S/T$), the judgment in consciousness may not be made in terms of such a formula. The assertion of a "special sense" for speed must not be misunderstood nor carried too far. It is a "special sense" in the same way that the perception of the position of our limbs is mediated by a "special sense." The statement means merely that among the manifold qualities, intensities and interrelations of sensations afforded by movements of the limbs, certain have become, through long experience, associated so intimately with speed differences that the passage from quality, intensity and interrelation of sensation to judgment of speed no longer follows a round-about path of inference which may or may not once have been necessary, but is direct and immediate—an empirical system of speed signs has been evolved. Given a movement in process of execution and the possibility of knowing the position of my limb at a given point, I am able to tell, apparently from the intensity of certain movement sensations at the point in question, the approximate or relative speed of my movement, without reference to the actual or subsequent extent of the movement or to the time elapsed since its initiation. Indeed the speed up to the time of judgment may have been irregular. The present speed may have begun only a moment before my judgment is made, and may bear no relation at all to the total extent or duration of the movement. The anatomical source of the sensations on the intensity, quality or interrelations of which the judgment is based need not concern us in this connection.

CHAPTER V

Memory for Extent and Duration

While performing the experiments on the relation between the extent and duration of movements occasion was taken to observe the effect on accuracy of the interval elapsing between the standard movement and the attempt to reproduce it. Among the many researches on sense memory that have been reported several include experiments on extent and direction of movement and on the movement sensations involved in lifting weights. Less attention has been given to the question of memory for time intervals. A brief review of the chief studies in which memory for magnitudes in the perception of which the sense of movement is involved will serve to introduce the results of the present experiments on this point.

Cattell and Fullerton,[1] in studying memory for lifted weights, employed the method of right and wrong cases on ten observers, using a normal weight of about 100 grams and a difference of 8 grams. Seven intervals were used, ranging from 1 to 61 seconds. The approximate probable error did not seem to increase so long as the interval did not exceed 9 seconds, but beyond this point increased by about one third, but again remained rather constant for the intervals of 15, 31 and 61 seconds. "The memory image seems to last up to 9 seconds, after which the observer does not so much compare the sensations as decide on the approximate intensity of each sensation separately and compare the decisions." Lewy[2] investigated memory for the length of visual lines ranging from 20 to 200 mm., using 9 intervals between 1 and 60 seconds. The error was found to increase with the length of the interval, slowly up to 10 seconds and then more rapidly, though it was greater at 1 second than at 2. Th. Schneider[3] worked with curvilinear movements of the hand, rotating on the wrist joint. The standard was given by the method of "impact" and 6,000 experiments were made on three subjects by the method of average error of reproduction. The magnitudes lay between 70 and 100 mm. and the intervals varied from ½ to 15 minutes. Up to 2 minutes the error remained quite constant (about 3 per cent.), then increased slowly to 5½ per cent. after an interval of 15 minutes. Delabarre[4] made a few preliminary tests on the influence of time

[1] "Small Differences," p. 147.

[2] *Zeit. f. Psychol. u. Physiol. d. Sinn.*, **8**, 230, 1895.

[3] "La Memoire des Mouvements Actifs," Diss., Juriew, 1894.

[4] "Bewegungsempfindungen," 105.

interval. He found 4 seconds to be the most favorable period, though wide deviations ranging to 29 seconds are said to have made no apparent difference. Jastrow[5] found memory for both visual and tactual extents to be extremely accurate and to be almost as faultless after a lapse of several days as after a few minutes. Münsterberg[6] reports experiments by Slatopolski on memory for vertical movements (5 to 50 cm.) of the arm. Intervals of 1 to 60 seconds were employed on four subjects, the standard being given by the method of "impact." The average error of reproduction, which varied from 10 per cent. to 100 per cent. with the magnitude of the standard, was found to decrease until the interval of 10 seconds was reached, after which it increased again, being about the same for 1 minute as for 1 second.

Landau[7] performed a great many experiments on memory for the extent of active and passive movements, using three observers, and testing accuracy of recognition after intervals ranging from 10 seconds to 6 minutes. He claims to have found the error to be quite uniform for intervals of less than 1 minute, but to increase considerably after 3 or 4 minutes. His tables, however, show a pretty regular decrease in the percentage of right cases, from about 73 per cent. at 10 seconds to a chance relationship at 5 minutes. Weber,[8] Courtier,[9] Vaschide[10] and Beaunis[11] have also reported more or less complete experiments on memory for extent.

On the question of memory for time intervals the early experiments of Paneth[12] are the only ones I have been able to find recorded. He found that the "sharpness of the memory image" of such intervals decreases so little in 5 minutes that no change could be detected. Larger intervals were not tried. Kennedy, reviewing the experimental work on memory up to 1898, concludes that "while memory for words, pitch, space, etc., falls off rapidly in respect to accuracy as the time interval increases, memory for time itself, so far as has been investigated, shows almost no diminution of accuracy as the time interval increases."[13]

Extent.—In the present series of experiments memory for extent was studied in the case of four observers. With three of these the

[5] *Mind,* **11,** 552, 1902.

[6] *Beitrage,* **4,** 69–88.

[7] *Wissench. Rev.,* 1896.

[8] Wagner's "Handwörterbuch der Physiol.," 3, p. 2.

[9] "Drit. Int. Cong. f. Psychol.," 1896, 238.

[10] *Ibid.,* 454.

[11] *Rev. Philos.,* **25,** 369.

[12] *Centralbl. f. Physiol.,* **4,** 81–83, 1890.

[13] *Psychol. Rev.,* **5,** 483, 1898.

standard magnitudes ranged from 100 mm. to 400 mm., and the continuous method was used, the terminal point of the first movement serving as the starting point for the second. With the fourth observer the magnitudes ranged from 150 mm. to 600 mm. and the "successive" method was used, the arm being returned to its initial position after passing over the standard distance, the two movements being thus made over the same stretch of track. Five different intervals were used, viz., 2, 5, 10, 15 and 30 seconds, the interval being in each case the time between the termination of the standard and the beginning of the reproduction. The signal from the sound hammer served to determine the magnitude of the standard. The intervals were measured by the swings of a seconds-pendulum, the "Now" of the operator being the signal for the beginning of the second movement. After the reproduction the observer guessed as to the probable direction of his error by judging whether it was "greater" or "less." The car was then returned to the starting point and the next trial made. In order to avoid fatigue in the extended arm in the case of the long intervals a horizontal desk-like shelf was placed along the track on the observer's side, at a height which allowed the hand and wrist to be supported while the car remained in position. The finger could thus be raised from the car and the feeling of cramp relieved.

In the calculation of error the magnitudes have been arranged in 5 or 6 groups—100 mm. to 150 mm., 150 mm. to 200 mm., etc. In the case of three subjects 15 magnitudes, falling between the upper and lower limits of each group, were given for each interval, making 75 trials of each interval for observer L. and 90 for each interval with W. and H., making totals of 375 trials for L. and 450 each for W. and H. In the case of Bt. 50 trials for each interval were given, making 250 trials. The per cent. error for each trial was calculated and the individual errors averaged to get the group average. This final error was then analyzed into constant and variable errors. The grand average for the total range, for each interval, was then computed and is indicated in the following curves.

The particular errors for each observer may be found in the proper table in Chapter IV.

The results from all four subjects are quite uniform. A statement of the general tendency will depend chiefly on which one of the three measures of error is chosen. In all three cases the gross average error of reproduction increases in general, with the length of the interval. All three show an increase of gross error after 2 sec., which either falls slightly or remains constant at about 15 sec. After this point the curves for the A.E. no longer agree, those for

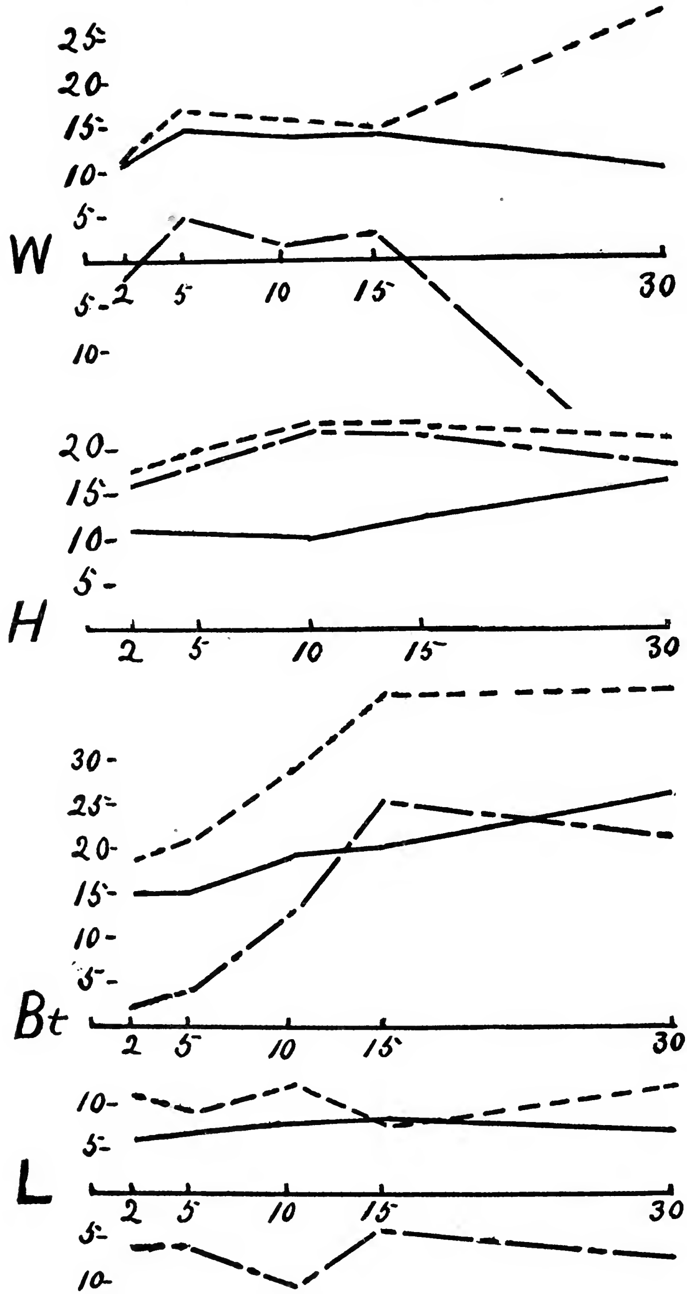

Fig. 2. Memory for Extent. Curves showing increase in error with increasing interval between standard and reproduction.

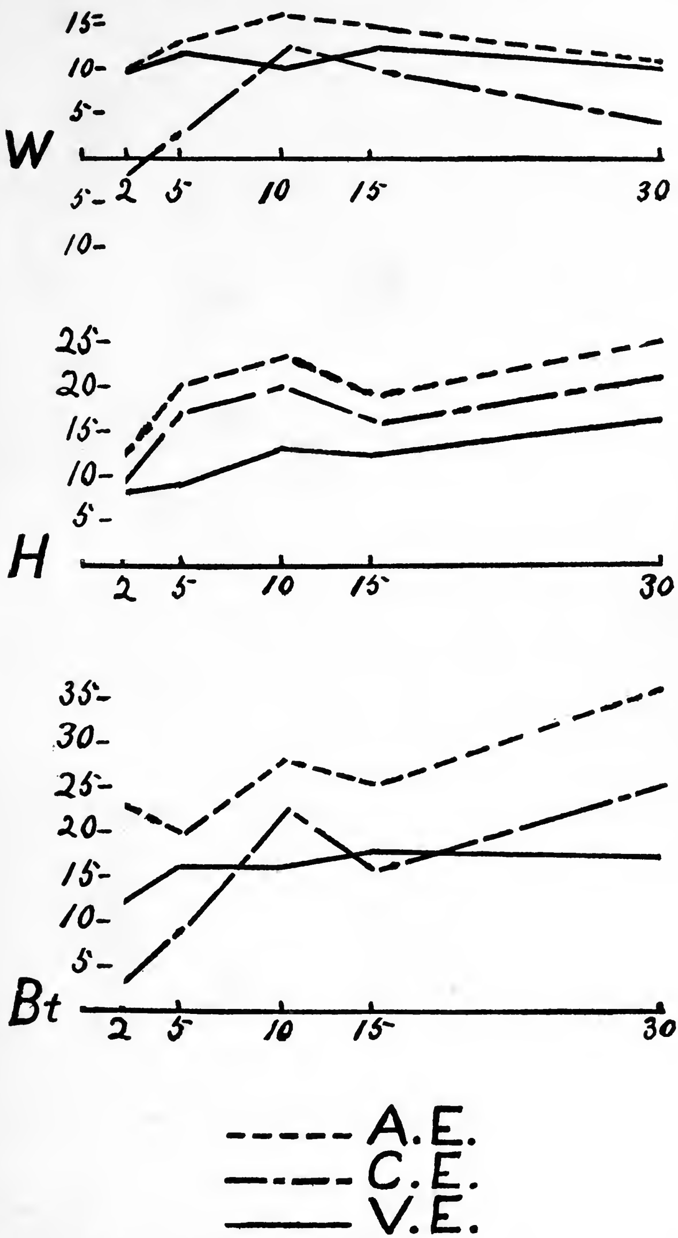

FIG. 3. Memory for Duration. Curves showing increase in error with increasing interval between standard and reproduction.

H. and Bt. remaining on about the same level, that for L. rising to its previous maximum, while that for W. rises considerably. But these effects all appear to be due to the constant error, the curves for which are seen to have the same general direction. The C.E. becomes more positive up to 10 or 15 sec., and then falls again, becoming either a smaller positive error or, as in the case of W., considerably negative. The variable error, however, seems to be only slightly, if at all, affected by the increase in time interval. For L. the level is practically uniform, for H. the only considerable increase is at 30 sec., while for W. the errors at 2 sec. and at 30 sec. are slightly lower than the level of the other three points; Bt.'s V.E., however, increases regularly, from 15 per cent. at 2 sec. to 26 per cent. at 30 sec. Since these tests were extended over a considerable period of time the changes in the constant error may easily enough be due to factors other than the variation of the time interval. Usually the trials made in a given sitting were for one or at most three intervals, the memory problem being rather incidental to the experiment proper. Under the circumstances the variable error is the most reliable measure of accuracy. From its relative constancy we may conclude that, within the limits of the investigation, the accuracy of reproduction, as measured by the variable error, is not influenced by changes in the time interval. This conclusion seems to be further confirmed by the fact that the proportion of right to wrong guesses does not decrease as the time interval lengthens. Such slight change as does occur is on the whole in the reverse direction, the proportion of right to wrong guesses increasing slightly for the longer intervals (see Table XXIII.). In the case of Bt., indeed, this increase is rather striking.

Time.—The procedure in the experiments on memory for duration was the same as in those on extent. The standard magnitudes ranged from 1 sec. to 3½ sec., and have been classified under five groups. The calculation here was the same as in the case of extent. The subjects were W., H. and Bt., and 75 trials were made for each interval between the standard and the reproduction. The continuous method was used, the standard duration being determined as before, by the checking of the movement at the signal from the sound hammer. At the word of the operator, after the appropriate interval, the observer went on to reproduce the duration of the standard movement, and guessed as to the probable direction of his error. The results are shown in curves 6, 7 and 8, and in the appropriate tables in Chapter IV. Here again the three observers agree pretty closely. A.E.'s and C.E.'s increase up to 10 sec. Of course the determining factor here is the C.E. for changes in the

A.E. merely reflect changes in the C.E. In all three cases the C.E. drops at 15 sec., rising again with H. and Bt., to the maximum at 30 sec., but dropping still further in the case of W. The V.E. undergoes little change. That for W. remains on practically the same level throughout, agreeing with the results of Paneth's experiments. With Bt. and H. there is, however, a rather uniform, though slight, decrease in accuracy, the V.E.'s increasing from 12 per cent. to 17 per cent. with Bt. and from 8 per cent. to 16 per cent. with H.

TABLE XXIII

PROPORTION OF RIGHT TO WRONG GUESSES WITH INCREASING INTERVAL

		2 sec.	5 sec.	10 sec.	15 sec.	30 sec.
Extent.	L.	1.2	0.9	1.8	1.5	1.9
	W.	1.1	1.8	1.7	2.2	0.8
	H.	1.0	1.2	1.0	1.3	1.5
	Bt.	2.0	1.4	2.4	2.7	3.3
Time.	W.	1.0	0.7	0.6	0.8	1.1
	H.	0.8	0.7	1.6	1.6	1.1
	Bt.	0.7	3.0	1.7	1.8	1.5

In these two cases the loss of accuracy accords, on the whole, with the "law of forgetting," as it is usually stated, the error increasing rapidly at first and then more slowly. Here, as in the case of extent, the proportion of right to wrong guesses fails to indicate any decrease in accuracy of memory (see Table XXIII.). For W. the proportions remain quite constant, for H. there is a considerable increase in the proportion of right guesses as the interval is lengthened, while for Bt. the minimum is at 2 sec., the maximum at 5 sec., the proportion for the greater intervals remaining equal.

CHAPTER VI

Influence of the Degree of Contraction

One of the points on which various writers have differed is a phenomenon first noted by Loeb.[1] He found that short movements executed in different portions of the possible range of rotation of a joint are not estimated with equal accuracy. In his experiments the movement made under conditions of less contraction of the acting muscle was underestimated as compared with one made under a greater degree of contraction. This fact was used by Loeb in support of the theory of innervation. He held that the objective shortness of the second movement was to be explained by supposing that, by virtue of the partial contraction already involved in making the first movement, further contraction was more difficult. As a consequence the same innervation produced a smaller change, but since equal innervations were made or intended, the two movements appeared of equal length.

Külpe denies the validity of Loeb's figures and ascribes the supposed phenomenon to "erroneous evaluation of experimental results."[2] Delabarre[3] suggests that the results are probably reliable and explains them on the basis of the supposed greater difficulty of the movement under greater degree of contraction. But this feeling of greater difficulty does not, for Delabarre, involve innervation feelings. However, he finds the illusion to occur only under considerable differences of contraction. Angier[4] finds it not to occur under any circumstances. Kramer and Moskiewicz[5] find the illusion always to occur, and in varying positions and directions of movement. They explain it by the principle of unfamiliarity, the theory being that in the unfamiliar position the hand makes slower movements which are thus made shorter in order to be equal to longer movements made more quickly. Since the sensation complexes are different in the two cases the comparison must be made on the basis of some common quality. The only common quality present is the duration. Consequently these investigators surmise that the durations of the two movements are equal when the extents appear to be,

[1] *Pflügers Archives*, **46**, 1–46, 1890.
[2] "Outlines of Psychology," p. 342.
[3] "Bewegungsempfindungen," p. 90.
[4] *Zeit. f. Psychol.*, **39**, 430, 1905.
[5] *Ibid.*, **25**, 101–125, 1901.

but they made no attempt to support their theory by actual measurement of the time.

Woodworth[6] takes exception to the form of the preceding generalizations. His results show that of the introspectively equal segments of the total excursion, the objectively greater extents do not occur at the beginning but in the middle, while the movements at both extremes are overestimated, *i. e.*,—are shorter in execution. Woodworth accepts Delabarre's suggestion of the relative ease of performance. Myers[7] accepts Woodworth's form of the illusion and Delabarre's explanation.

The following experiments were performed in order to confirm either Loeb's or Woodworth's results or to show that no constant errors are to be found, and to test the "equal duration" hypothesis

TABLE XXIV

EXTENT OF SUCCESSIVE MOVEMENTS INTENDED TO BE EQUAL

		mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.	mm.	% A.D.
I.	R.	321	*8*					257	*9*					223	*7*
	W.	260	*12*					279	*13*					240	*21*
	B.	232	*9*					272	*6*					232	*7*
II.	R.	203	*13*			201	*7*			175	*6*			166	*4*
	W.	207	*15*			246	*10*			228	*10*			196	*10*
III.	R.	162	*13*	156	*12*	134	*10*	123	*8*	110	*10*	115	*11*	99	*6*
	W.	139	*16*	158	*13*	166	*13*	149	*12*	131	*11*	126	*10*		
	B.	105	*14*	122	*9*	137	*15*	129	*2*	116	*9*	95	*7*	89	*7*
	Bt.	156	*15*	177	*19*	143	*11*	132	*9*	108	*9*	103	*8*		
	V.	158	*8*	171	*10*	161	*2*	139	*8*	126	*11*	116	*10*		
	C.	136	*10*	140	*13*	140	*10*	121	*8*	111	*9*	124	*8*		

TABLE XXV

DURATION OF MOVEMENTS WHOSE EXTENT IS SHOWN IN TABLE XXIV

		sec.	% A.D.	sec.	% A.D.	sec.	% A.D	sec.	% A.D.	sec.	% A.D.	sec.	% A.D.	sec.	% A.D.
I.	R.	3.13	*9*					2.97	*11*					3.14	*10*
	W.	1.38	*38*					1.32	*35*					1.39	*35*
	B.	1.12	*13*					.96	*10*					.92	*9*
II.	R.	2.28	*19*			2.13	*9*			2.08	*7*			2.24	*15*
	W.	1.34	*37*			1.14	*33*			1.16	*41*			1.35	*42*
III.	R.	2.16	*24*	2.09	*20*	2.07	*21*	2.08	*21*	1.93	*18*	2.17	*18*	1.97	*27*
	W.	1.10	*35*	1.20	*35*	1.15	*33*	1.04	*55*	1.00	*32*	1.08	*35*		
	B.	1.12	*14*	1.03	*15*	1.00	*15*	.97	*9*	.91	*12*	.78	*10*	.86	*9*
	Bt.	.89	*8*	.95	*12*	.88	*14*	.87	*13*	.83	*11*	.85	*12*		
	V.	.80	*15*	.76	*17*	.70	*11*	.68	*10*	.68	*15*	.67	*9*		
	C.	.80	*11*	.75	*12*	.76	*13*	.70	*13*	.71	*11*	.75	*11*		

[6] "The Accuracy of Voluntary Movement," p. 79.

[7] "Experimental Psychology," p. 73. Longmans, 1909.

of Kramer and Moskiewicz. The experiment consists of three series, in all of which the extents were recorded in millimeters and the durations in hundredths of a second.

In Series I. the total excursion was divided into three movements of introspectively equal extents, in Series II. into four such movements. In Series III., after a few preliminary trials in order to get the total number of movements into the single excursion, the subject made six (in two cases seven) successive movements, here again of apparently equal length. In cases R., W. and B. the length of the interval between movements was left entirely to the preference of the subject. In cases Bt., V. and C., each movement was made at a signal by the operator. In this way it was possible to vary the interval between movements and thus avoid any tendency to mere rhythmical performance on the part of the subject. Series I. and II. show the average results of 10 trials and Series III. of 20 trials for each subject, a total of 930 movements. Tables XVIV. and XXV. give the average movement in each position, in millimeters, and its average deviation in per cent. for both extent and duration. Table XXVI. gives the average of the first movements and the positive or negative deviation (A.E.) in per cent. of each of the successive movements from this average, both for extent and duration as well as the variability (V.E.) of all the movements from their average.

Extent.—In Series I., for both W. and B., the middle segment is longer than either the first or third. In Series II., for W., both the second and third segments are longer than either the first or fourth. In Series III., except for subject R., and one additional instance, the first segment is shorter than either the second or third and in two cases than the fourth segment, while beyond the approximate middle of the excursion the segments decrease again, still more rapidly than they increased at the beginning. Thus in all but one subject the results correspond with those obtained by Woodworth. The divergence in the case of R. is probably due to the fact that of the six subjects he was by far the tallest and had the longest arm. The range employed did not constitute his maximum excursion, and from the position assumed before the apparatus the first part of the total swing was the part not used. The degree of contraction of any one muscle or single set of muscles does not afford adequate basis for generalization, even disregarding the fact that somewhat different sets are likely to be employed in the execution of different segments. It should also be noted that these results are just the reverse of what one should expect if the judgment of extent were based on the angle of rotation at the joint. For the same angle, reproduced at

TABLE XXVI

	Extent.										Time.								
	Observer.	Standard. Unit, 1 mm.	Per Cent. Error.						A.E. Per Cent.	V.E. Per Cent.	Standard. Unit, 1 sec.	Per Cent. Error.						A.E. Per Cent.	V.E. Per Cent.
I.	R.	321			—20			—31	26	13	3.13			— 5			+ 0.3	3	2
	W.	260			+ 7			— 7	7	5	1.38			— 4			+ 0.7	2.5	2
	B.	232			+17			0	9	8	1.12			— 5			—18	12	8
II.	R.	203		—10		—14		—18	14	9	2.28		— 7		— 9		— 2	6	4
	W.	227		+ 8		+ 0.5		—14	7	6	1.34		—15		—13		+ 0.7	9	8
III.	R.	162	— 4	—17	—24	—32	—29	—39	24	15	2.16	—3	— 4	— 4	—10	+ 0.5	— 9	5	3
	W.	139	+13	+19	+ 7	— 6	— 9		11	9	1.10	+9	+ 5	— 5	— 9	— 2		6	5
	B.	105	+16	+30	+23	+10	—10	—15	16	13	1.12	—8	—11	—13	—19	—30	—23	15	10
	Bt.	156	+12	— 8	—15	—31	—34		20	16	.89	+7	— 1	— 2	— 7	— 4		3.5	3
	V.	158	+ 8	+ 2	—12	—22	—27		14	13	.80	—5	—12	—15	—15	—16		12	6
	C.	136	+ 3	+ 3	—11	—18	— 9		9	8	.80	—6	— 5	—13	—11	— 6		8	3
Averages									14	11	Averages							7	5

the shoulder joint, would mean a greater movement at the extremes of this total rectilinear excursion than in the middle, and in the attempt to reproduce extent the middle segments would be shorter, those at the extremes longer.

Duration.—In the case of the times the segments never increase throughout the total excursion. On the other hand, in 7 cases the extreme segments are greater than the intermediate. This indicates a tendency for the extreme segments, both initial and terminal, to be greater and the intermediate less than the average of the group. This is especially clear in Series I. and II., where the differences in degree of contraction for the separate movements are greater. Correlating ($r = \cos \pi U$) the extents with the durations of the respective segments, we get the following coefficients:

Observer.		R.	W.	B.	Bt.	V.	C.
Series I.	$r =$	+ .51	— .51	— .51			
II.	$r =$	0	— .71				
III.	$r =$	+ .43	+ .71	+ .61	+ .97	+ .86	+ .86

When the differences between the positions of the successive segments are considerable, as in Series I. and II., the correlation is negative—the longer movements occupy the shorter times, but when the differences in position are less, as in Series III., the correlation is positive—extents and durations tend, on the whole, to vary in the same direction.

Inspection of the average deviations of the individual movements from their averages, as shown in Tables XXIV. and XXV., shows that the extents included in any given segment average are much more constant and uniform than the durations. That is, their average deviations from the average of their respective groups are smaller, the grand average for extent being only 10 per cent. as against 19 per cent. for duration. This difference, however, is not particularly significant, since no special effort was made to keep equal the magnitudes ranging around a given segment. Moreover, there was more chance for variation in the time than in the extent, since the observer's attention was never explicitly called to the duration of his movements and there was no attempt to keep the speed constant. The attempt was always, having made a first movement, to make all succeeding movements of the excursion in question equal in extent to this first, without reference to the magnitude of corresponding segments of other excursions.

Table XXVI. shows the relative accuracy of this performance with respect to the deliberate extents and the incidental times as well. In this table the A.E. represents the average deviation from the standard, while the V.E. represents the per cent. variation from the average of all the segments of the group. The final A.E. and V.E. for extent (14 per cent. and 11 per cent.) are twice as large as the corresponding errors for duration (7 per cent. and 5 per cent.). The durations are more nearly equal than the extents, just as was the case in Chapter IV. As the figures stand, they tend pretty strongly to confirm the conjecture of Kramer and Moskiewicz.

But this closer agreement does not in itself suffice to demonstrate the perception of time to be any more fundamental than the perception of extent. As in most cases of naturally reproduced movements, there is a tendency to repeat the whole original performance (see Chapters II. and IV.), reproducing both duration and extent. In this case factors enter which disturb the spatial judgment but do not affect the temporal. The times are more nearly equal, not because they constitute a common factor which serves as a basis of comparison in reproducing extent, but simply because the change in position of the moving member introduces no factors which affect the judgment of duration. There is indeed no reason for supposing the perception of time to be based on processes in the moving member. It is probably based instead on processes of a more permanent and regular sort, taking place in other parts of the organism. But the judgment of extent is subject to every local change in position, strain, ease of movement, familiarity, inertia, etc.

In producing the illusion of extent all the factors mentioned by the earlier writers are probably effective, allowing for the modification of Loeb's "innervation" theory made by contemporary psychology. These factors seem to fall into two groups: (1) conditions of performance and (2) conditions of perception.

1. *Conditions of Performance.*—It may be supposed that the first movements of the series are more difficult than later ones, since the inertia of the musculature has to be overcome in the one case but is already removed in the other. In the middle portion of the excursion movements are more easily made, since the muscle is already warmed up and in action. While at the terminal end of the excursion it may be supposed that movement is more difficult than in the middle portion because of the greater degree of contraction, entailing greater innervation, or because of the unfamiliarity of the movement. This last factor may affect both ends of the series. Movements here, being less familiar, and the signs which indicate extent being less thoroughly systematized and learned, tend to be made with greater caution. That they are made more slowly is clear from Table XXVI. When the duration of the standard movement has elapsed there is a strong disposition to feel the movement as completed, since its tem-

poral factor has been approximately reproduced. The movement is thus stopped somewhat short of the proper extent, a kind of compromise being effected in which both spatial and temporal accuracy are partially sacrificed, the durations tending on the whole to fall out slightly too long and the extents too short.

2. *Conditions of Perception.*—Delabarre's suggestion that anything which increases the sensory elements of a movement increases its apparent magnitude, though untenable as a generalization, may be applied here with advantage. There is no doubt, introspectively, that at either extreme of the arm's excursion the sensations resulting from a given objective change in position of the limb are relatively intensified. The member is approaching its limit of movement, the tension of muscles and tendons is approaching a maximum as the degree of contraction increases, the skin over the joint is stretched, the subcutaneous tissues are more firmly compressed about the fulcrum of the joint. The sensory elements of any movement made under these conditions will be relatively increased and in so far as extent of movement is judged in terms of intensity of sensation, we should have the Loeb illusion at both extremes of the total swing without considering the difficulty of performance, either as a result of inertia, degree of contraction or unfamiliarity. Moreover, we would expect more complete and prompt adaptation to the sensations aroused by the more familiar movements in the central portion of the excursion and this again would produce a relative decrease in the sensory elements of such movements.

CHAPTER VII

Criteria of the Judgment of Extent

The greater part of the work on movement has been topographical in motive and in method, consisting of observations of motor ability and accuracy under definite experimental or pathological conditions or of attempts to localize anatomically the source of the sensations on which specific judgments are based. Interesting as these results may be to the physiologist or physician, they throw little light on the processes of discrimination, recognition and comparison involved in our judgments concerning movements. In fact there seems to be a kind of "anatomist's fallacy" in such a procedure, at least so far as the psychology of movement is concerned, for it seems to proceed on the tacit assumption that the sensation is, psychologically, what it is anatomically. And, as we might expect, the topographical procedure has led into all kinds of disagreement. Nowhere is this disagreement more apparent than in the matter of the criteria or differentiæ of the judgment of extent. The chief cause of disagreement here seems to have been the desire to simplify the "muscle sense," to trace, if possible, the sensation of movement to a single anatomical source. This term "muscle sense" has been used to designate the whole group of articular, tendinous, muscular, cutaneous and visual elements that go to make up the kinesthetic perception. That the mere sensation of movement may be mediated by any or all of these has been pretty generally agreed, but when specific topics are concerned—the judgments of extent, force, time and direction of movement—opinion is not nearly so unanimous.

After a great number of experiments of the topographical sort, Goldscheider[1] concluded that the joint sensations afford the chief criteria for the judgment of extent and direction of movement. Attempts were made to get a pure muscle sensation isolated from other elements of the kinesthetic sensation. The skin over a muscle was anesthetized and the muscle stimulated electrically. The diffuse sensation produced was said not in the least to resemble the sensation of movement. When the joint alone was anesthetized the consciousness of movement became so blunt that it was evident that the feeling of contraction could not be used for fine discrimination of extents, while anesthesia of the skin produced no disturbance of space perception. On the basis of these and similar experiments

[1] A. Goldscheider, "Untersuchungen über den Muskelsinn," 369 ff.

Goldscheider concludes that "muscle sensations, which were formerly accorded the leading rôle in the cognition of weight and in the estimation of the magnitude and direction of movement, do not appear at all except as a result of intensive stimulation, great fatigue or in the form of muscular pain."

Külpe[2] accepts Goldscheider's conclusion, with certain amplifications of his own. "It may be conjectured *a priori* that muscular and tendinous sensations can not form the ground of our judgment of the position and movement of our limbs in the absence of visual perception. There is no proportionality between the extent and duration of a movement and the possible concomitant excitations in muscle and tendon." "On the other hand, the relation between the positions of the articular surfaces as regards each other and positions or movements of the limbs is just as simple as that between the different parts of the skin or retina and the points from which they are stimulated. We see, therefore, that the articular sensibility furnishes us the real basis of our perception of the position and movements of the limbs where an appeal to vision is excluded." Kramer and Moskiewicz confirm this conclusion by saying: "Sensations arising from the processes of tension of the muscles are unessential to inform us concerning the judgment of the position or movement involved."[3] James, in turn, accepts the theory on the basis, chiefly of Goldscheider's results: "We indubitably localize the finger tip at the successive points of its path by means of the sensations which we receive from our joints."[4]

In striking contrast with this position are the more recent statements of Pillsbury and of Reichardt, leading back to the older position of Brown and Delboeuf. Reichardt,[5] working on the illusions of passive movement, claims that the sense of position is not mediated by the part moved but by processes in the moving muscle. Pillsbury[6] finds that "the sensitivity of joints is decreased by induction currents through the wrist and elbow as well as through the joints in question. This fact, together with the lack of anatomical evidence that the joints have sensory endings, makes it probable that the sensation of movement is derived mainly from the tendon and muscle, rather than, as Goldscheider thought, from the joints."

Under the circumstances, then, we should expect somebody else to abstract some other element and exalt it into the position of chief

[2] "Outlines of Psychology." London, Sonnenschein, 1901, 143.

[3] *Zeit. f. Psychol.*, **25**, 105, 1901.

[4] "Principles of Psychol.," **2**, 193.

[5] *Zeit. f. Psychol.*, **40**, 430, 1906.

[6] *Amer. Jour. of Psychol.*, **12**, 346, 1901.

criterion. And this is what occurs. Bourdon[7] finds that the least perceptible tension of the skin about the dorsal joint of the finger is about .2 mm., and that this is just the tension required to allow the least perceptible movement—1 mm.—to take place. He also insists that to suppose the joint sense to be the source of criteria for judgments of extent of movement presupposes for the articular surfaces a tactual acuity much higher than that of the skin in its most sensitive parts, and that this contradicts the general rule that sensitivity decreases as we go more deeply into the interior of the body. Consequently, he concludes that the criteria of extent of movement are in all probability to be found in the tensions of the skin above the moving joint. Nevertheless we find Pillsbury saying: "That the skin does not serve as source of the sensations which indicate movement may pass without comment."

Still other facts tell against the conception of the joint linings as a "reduced map" of the extent of movements. One is the fact that our movements do not consist of simple joint movements in one direction or of combinations of such movements. Thus in the execution of a compound arm movement of any considerable magnitude the elbow joint tends to double back, beyond a certain point in the movement, retracing its original rotation but in the reverse direction. Particularly is this true if the movement approximates the rectilinear type. As a result of this it follows that the fixed point-for-point correspondence between points on the articular surfaces and points in external space is not so fixed as might at first appear. A point on the membrane lining the shoulder joint may mean almost any point in external space, depending on the complex relation of the positions of elbow, wrist and finger joints. If our most common movements or even our earliest movements consisted of rotations at a single joint, a point for point correspondence might be established. But such is not the case. From the genetic point of view at least our spatial order is built up on a basis of primitive and practical movements which are complex in character and mechanism—such movements as brushing away a fly, pulling or pushing objects to or from the body, striking a blow, raising a lever, etc. The anatomically simple single joint movement comes to be artificial, for greater speed and accuracy are undoubtedly to be gained by the complex movement. But even with these compound movements there might, it is true, be developed a system of local signs on the articular surfaces, the combinations and interrelations of which might come to mean extent of movement. Such a proposition, however, yields the whole argument for the exclusive rôle of the joint sense and affords no reason for

[7] *L'Annee de Psychol.*, **13**, 133–143, 1907.

excluding criteria afforded by sensations from muscles, tendons, skin and subcutaneous tissue.

A striking experiment by Münsterberg[8] shows that the same extent of movement may be represented in one situation (with extended forearm) by a given angular rotation, in another (with forearm flexed) by a rotation three or four times as great. This experiment alone should suffice to demonstrate the empirical basis of the judgment of extent, and to emphasize the importance of factors other than the number of degrees of joint rotation. Still further, whatever importance one may be disposed to attribute to eye movements in the perception of visual space, the fact remains that to a certain extent, even with closed eyes or in the dark room we can know with a certain degree of correctness the position of the eyes and estimate the amount of their movement, although there are no articular membranes involved.

An even clearer illustration is to be found in cases of acquired control over the ear muscles. Diligent practise since boyhood has enabled me to perform either monaural or binaural movements with considerable facility and has developed a rather definite range of recognized extents. In this case there has been neither articular surface nor even cooperation with visual criteria. Movements of the tongue are also made with great accuracy, although we do not ordinarily have occasion to apply objective scales of measurement to them.

Attempts to find a single topographical or anatomical source have thus been futile. Goldscheider's experiment, which for James "completely established" the rôle of the joint sense is contradicted by Pillsbury's results. Adherence to Külpe's suggestion of the accurate correspondence of points on articular surfaces with points in external space requires a tactual acuity which Bourdon can not accept, and nerve endings in the joint linings, which have not yet been satisfactorily demonstrated. Reichardt's attribution of the sensations indicating extent to the processes taking place in the moving muscle is discounted by Duchenne's patients, in whom insensibility of muscles was found along with intact perception of movement. Bourdon's attempt to refer the sensations to skin tension over the moving joint is contradicted by Goldscheider's subjects with anesthetized skin but unimpaired perception of movement. And these topographical attempts fail because, as it appears, sensations do come from many sources and any sensation which can aid in the differentiation of one movement from another serves to identify that movement when it occurs again.

More conciliatory is the statement of Delabarre to the effect that

[8] *Beiträge*, 1892, IV., 178–191.

"movements are judged equal when their sensory elements are equal," although the precise nature of such an equality is not apparent. Aside from the possible tautology of the statement, it is not clear how such heterogeneous elements as duration, speed, force, strain, position, are commensurable. The equality can hardly be of an intensive character, for two excursions may be equal in extent and yet afford sensations of strain that are exceedingly disproportionate to the error in apparent magnitude. A better statement would probably be the one we have already suggested, viz., that movements are judged to be equal which have been *learned to be equal*—that judgment and discrimination are not based on anatomy, nor even on an intensive psychophysical relation between magnitude of stimulus and intensity or extensity of sensation, but are inferential processes, founded in the empirically coordinated consequences of experience. Innumerable secondary and essentially unrelated criteria may be utilized in the recognition and in the judgment, which is a purely qualitative one, not "How much joint movement or skin tension is now felt?" but "What signs can I find to help me recognize this movement among the many other movements with which I am somewhat familiar?" Titchener[9] finds that so irrelevant a thing anatomically as the way in which the arm fell down against the side after completing the movement was in one case the basis for the judgment of extent of arm movement. Even in judgments of resistance, as Bolton[10] has pointed out: "Perceptions of greater do not necessarily rest upon greater perceptions and a sensation of intensity is not an intense sensation." "Judgments of same and heavier are inferences from certain facts, and these facts are the excitations of areas in the one case that remain unaffected in the other."

Woodworth concludes that "there must be a sense of the extent of movement, a sense which is not reducible to a sense either of its force or of its duration or of its initial and terminal positions."[11] There is no contradiction between such a statement and the one just made. To say that we have a direct and immediate sense of the extent of movement may mean just what is here suggested—that a variety of qualitative signs have been learned to mean movements of definite magnitudes, irrespective of the extensity attribute of the particular muscular, tendinous, articular or cutaneous sensations involved. Instead of insisting on the prominence of any one of

[9] "Exper. Psychol.," Vol. 2, pt. 2, 260.

[10] Bolton and Withey, "On the Relation of Muscle Sense to Pressure Sense," *Univ. of Nebraska Studies*, 1907, 7, 21.

[11] "Accuracy of Voluntary Movement," *Psychol. Rev.*, Mon. Supp., 13, 80, 1899.

these sources it seems more satisfactory to say with Sherrington that the muscle sense is based on a "specific set of sensations obtained by specific sense organs in the muscles, joints and all the accessory organs of movement."[12] Any sensitive part that is in any way uniformly stimulated in the process of a given movement contributes its share to the character of the movement as a conscious fact, and any such contribution may be utilized in the recognition of the movement when it occurs again. But this recognition does not seem to be based on the quantitative relations of this "specific set of sensations," nor on any such geometrical correspondence as Külpe suggests. It is throughout a qualitative recognition. Out of the variety of stimulations that accompany excursions differing in direction, extent, resistance and speed, certain combinations have been learned to mean position, others distance, others resistance or strain and still others velocity, however disproportionate the extensities or intensities of the sensations in their own right. A greater intensity of sensation does not mean a greater resistance or pressure. It may mean a lesser objective stimulus under more sensitive conditions.

In studying the accuracy of space perception, therefore, and in analyzing any tendency to error found there, we are investigating just this association of sensation complex with objective meaning. From this fact great uncertainty arises in the application of the psychophysical methods to the study of movements. Observers tend here to refer every stimulus to some absolute scale of magnitudes and to estimate and compare, not by a genuine balancing of impression against impression, but by position claimed or assigned in this absolute or practical objective scale. Thus two movements of different extent are likely to be felt, not so much as "larger" or "smaller" impressions, but rather as impressions that are qualitatively different. Comparisons are seldom made in subjective terms. Since our movements are our means of voluntarily manipulating our environment, they come to be specialized for specific purposes and are thus characterized qualitatively by their function. A movement comes to be recognized as larger than another, not because it produces a more intense sensation, but because it has been *learned to be* a greater movement—a movement that will effect a greater change in an object with which we are dealing. The element of extensity involved in movement is not the primary quality of extensity attributed to all or most of our other sensations. A joint or tendon sensation or a sensation of cutaneous tension may possess an extensity of its own, but it is only empirically and after long experience that this extensity comes to mean definite extent of movement. Or

[12] Stirling, "Outlines of Practical Physiology," 578. London, Griffen, 1902.

an object or movement may come to be felt as greater than another by virtue of the fact that one excites a local sign that the other has not affected. And this local sign, once awakened, constitutes not a quantitative but a qualitative distinction. There is no more reason for supposing that the estimation of movement depends on a highly developed joint sense than there is for believing that it depends solely on any other.

SUMMARY OF EXPERIMENTAL RESULTS

Chapter I. Methods

Description of apparatus designed to record simultaneously and graphically the extent, force, duration and velocity of rectilinear arm movements.

Chapter II. The Illusion of Impact

Movements terminating in impact are affected in perception and reproduction with a large positive constant error, the magnitude of which depends on (*a*) the force of impact and (*b*) the point in the intended movement at which the impact occurs. The greater the force of impact and the less the amount of the intended movement already accomplished, the greater the illusion.

Practise without knowledge has no effect on the constant error. The result of practise with knowledge is not to decrease the illusion so much as to produce a deliberate shift in the scale of extent criteria, leading to a corresponding negative constant error in the judgment of free movements.

The illusion may be explained on the basis of (*a*) the original intention, (*b*) irradiation of the stimulus, (*c*) increase of the sensory elements of the movement complex through fusion of the shock of impact.

Chapter III. The "Indifference Point"

With respect to the experiments on extent of movement reported in this chapter:

1. No magnitude evinces any considerable constant error of reproduction when estimated out of relation to a group or series of which it is a member.

2. The same absolute magnitude may be under one circumstance an indifference point, under another affected with a positive constant error or again with a negative one.

3. A periodic indifference point can be found within the total series (*S*) by working with its special sections (A, B and C).

4. The gradual extension of the series limits is accompanied by a corresponding shift in the region of indifference.

5. The phenomenon of the indifference point, so far as it occurs in our spatial judgments and in our temporal judgments, at least so far as they are a function of extent of movement, is of central

origin, and its position depends on the range or limits of the magnitudes used in a given experiment.

6. The constant errors do not so much represent transformations in a memory image of the stimulus in question as they do the effect on a present judgment of the persistence of the mental set involved in the directions of previous judgments.

7. If the interval between the separate judgments is sufficient the first dispositions are soon dissipated and are no longer adequate to affect the succeeding performance.

8. In the presence of such grouped or serial magnitudes we tend to form our judgments around the mode or central tendency of the series. Toward this mean each judgment tends by virtue of a mental set corresponding to the upper and lower limits of the total range of magnitudes. This is the equivalent, for judgment, of Leuba's "law of sense memory."

Chapter IV. Relation between Extent and Duration

The four methods of separate accuracy test, confusion, correlation and correction fail to justify the assumption that the perception of any one characteristic of a movement is more primitive or fundamental than that of any other.

Extent and duration can be reproduced with about equal accuracy, the difference being slightly in favor of duration. But the incidental durations of movements intended to be of equal extent show a variable error which is greater than that of their corresponding extents as well as that of the same duration magnitudes when deliberately reproduced.

Constant errors seem to be bound up with the process of deliberate reproduction, the constant error for the magnitude attended to (extent or time) being greater than that of the magnitude incidentally reproduced (time or extent). Thus the constant errors for deliberate extent and for deliberate time are both greater than those for incidental extent and incidental time.

The coefficients of correlation show that, disregarding the magnitude of the errors, there is considerable positive correlation between their directions for extent and duration, no matter which factor is being attended to.

Subsequent guesses as to the probable direction of the error of attempts to reproduce either extent or duration correspond more closely to the actual relations of the extents than to those of the durations. The proportion of right guesses in reproduction of extent is greater than in reproduction of duration.

These facts point to separate processes of judgment for the two magnitudes (extent and duration). There is at least no justification for the attribution of more fundamental character to the perception of either. The judgment of extent seems to be based on a system of signs which have been learned to mean extent directly. The same seems to be true of both duration and velocity.

Chapter V. Memory for Extent and Duration

Within the limits of the investigation the accuracy of reproduction of extents, as measured by the variable error, is not influenced by changes in the time interval. With respect to the constant error individual differences are shown.

The curve of memory for duration follows more closely the ordinary statement of the "law of forgetting," in the case of the constant error, although the variable error undergoes little change up to an interval of 30 seconds.

Chapter VI. The Degree of Contraction

The Woodworth modification of the Loeb illusion is present in nearly every case of rectilinear arm movement. The middle segments of a total excursion are underestimated in comparison with the segments at either extreme.

When the differences in position between adjacent segments is considerable, the total swing thus consisting of but few segments (3–4), the durations show just the reverse phenomenon—initial and terminal segments frequently tending to require longer time than intermediate segments. When the differences in position are less (6 and 7 segments) the correlation is positive—extents and durations tend to vary in the same direction.

The average deviation for the durations is only about half as great as that for the extents, but this is not necessarily due to a more fundamental character of the perception of time.

There is a tendency in reproduction to repeat the original performance as a whole. By the conditions of the experiment the spatial judgment is confused while the perception of duration is undisturbed.

Chapter VII. Criteria of the Judgment of Extent

Attempts to find a single anatomical or topographical source for the sensations which serve as criteria of extent of movement are contradictory and futile. Judgment and discrimination are inferential processes, founded in the empirically coordinated consequences of experience. Any sensitive part that is in any way uniformly stimulated in the process of a given movement contributes its share to the character of the movement as a conscious fact, and any such contribution may be utilized in the recognition of the movement when it occurs again. A movement comes to be recognized as larger than others, not because it produces a more intense sensation, nor because of any geometrical correspondence of internal and external points, but because it has been *learned to be* a larger movement—one that will effect a greater change in an object with which we are dealing. Topographical treatment of the criteria of judgments of magnitude involves an "anatomist's fallacy."

A QUANTITATIVE STUDY OF RHYTHM

THE EFFECT OF VARIATIONS IN INTENSITY, RATE AND DURATION

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3

CONTENTS

CHAPTER I

Historical

To produce an impression of rhythm, it is necessary to have a series of stimuli. These stimuli may be sounds, as in the case of poetry and music, muscular contractions, as in dancing and beating time, or lights and electrical shocks, as in some laboratory experiments. The stimuli which give the impression of rhythm, whatever their nature, may vary in intensity, in duration, and in quality, and may be separated by intervals of varying length. A fundamental task of the experimental investigation of rhythm is to investigate the part played by each of these factors. Only after each of them has been studied separately, may we study the effect when two or more of them are simultaneously involved, and when more complicated factors are introduced, as in melody and harmony.

The aim of the present study is to examine quantitatively the dependence of the rhythmical impression on the intensity and duration of the stimuli. Such an investigation is evidently along the same lines as much of the experimental work of Meumann, Bolton, R. McDougall, and others, who have studied the objective conditions of rhythm. It is necessary, therefore, to review the work that has already been done on the perception of rhythm as influenced by variations in the intensity and the duration of the stimuli.

Meumann[1] found that in listening to a series of sounds, some of which were louder than others, there was a strong tendency towards the formation of rhythmical groups. He studied the effect of accented sounds on the intervals preceding and following them. The most general conclusion at which he arrived is that the effect of the more intense sound may be very different according to its position in the rhythmical group.[2] He found that sometimes the interval following the accented sound is overestimated and sometimes underestimated, and also that sometimes the interval preceding the accented sound is overestimated and sometimes underestimated, and in the cases in which he used more than one subject, he gets quite different results under the same objective conditions. Meumann states, also, that, with *most* subjects, the sudden introduction of a loud sound into a series of weaker ones causes an underestimation of the interval preceding, and an overestimation of the interval following, the loud sound;[3] but he does not

[1] *Philos. Stud.*, **9**, 264–306, 1894.
[2] *Ibid.*, **9**, 303 and **10**, 311, 1894.
[3] *Ibid.*, **9**, 276, 1894.

say how many observers gave this introspection, what was the introspection of those who did not give it, how many judgments were made by each observer, or how they were instructed. Meumann made no investigation of the effect of duration in rhythm.

Bolton[1] presented sets of sounds of different intensities and durations, which recurred always in the same order, and asked the subject to point out where the series was grouped. In this way, he sought to determine what was the most natural order in which the different intensities and durations occurred in the group. These experiments led him to state the following general principle: "In a series of auditory impressions, any regularly recurrent impression which is different from the rest, subordinates the other impressions to it, in such a way that they fall together in groups. If the recurrent difference is one of intensity, the strongest impression comes first in the group and the weaker ones after. If the recurrent difference is one of duration, the longest impression comes last."[2]

Bolton calls attention, further, to the long interval which appeared between the groups, the intervals being objectively equal. The pause seemed to be due to the fact that a long interval generally preceded the accented sound. At the same time some subjects, especially 10 and 15, make a short interval after the strongest sound. But in another place, Bolton writes: "The accented long sound frequently appeared more prolonged than the unaccented of the same length: the accent had the effect both to increase the length of sound and of the interval which followed." And consulting his table of results,[3] we find that his subjects often found the interval preceding the accented sound longer than the others, but more often did not. As regards the effect of duration, most of Bolton's subjects remarked upon the long interval or pause which seemed to follow the long sound, and for this reason it was found difficult to make the close of the group come at any other place.

Ettlinger[4] has criticized Bolton for his tendency to generalize his results on duration, which, being limited to the single case in which one sound is twice the duration of the other, do not permit of much generalization. So far as they go, however, his results indicate that the effect of increasing the length of any regularly recurrent sound is to produce an overestimation of the following

[1] *Amer. J. of Psychol.*, **6**, 222, 1894.
[2] *Ibid.*, p. 232.
[3] *Ibid.*, p. 228.
[4] *Ztschr. f. Psychol.*, **22**, 132–133, 1900.

interval, while the effect of increasing the intensity is uncertain. To most of Bolton's subjects, the strongest sound seemed longer than the rest, and the long sound frequently seemed accented.

Schumann[1] asked his subjects to compare the second of two intervals enclosed within a series of three sounds with the first interval. He found, in the case of four subjects, that when the third sound was louder than the preceding, the second interval was underestimated as compared with the first. Three of those same subjects were also tested with regard to the effect of a loud sound which was unexpectedly introduced in a series of weaker sounds. In the case of all three, the interval preceding the louder sound was apparently shorter than the other intervals. Two subjects, on the other hand, obtained the opposite result in both experiments, that is, the interval preceding the accent was overestimated as compared with the other intervals. Schumann explains this apparent contradiction on the ground that the two last mentioned subjects perceived the sounds rhythmically. He made no investigation of the effect of the regular recurrence of a more intense sound every second or every third time in a long series, nor did he study the effect of variation in the durations of sounds on rhythm or on the judgment of intervals.

McDougall[2] found that a loud sound introduced into a uniform series of six beats causes a considerable underestimation of the interval following the loud sound, while it less often and less considerably lengthens the preceding interval. As regards the overestimation of the interval preceding the accent, of the four tables[3] of results which are presented to prove this, one[4] shows an underestimation; another[5] shows practically no constant error, but an underestimation rather than an overestimation, while a third[6] does not show that accent has any effect on the interval immediately preceding, but that a longer interval causes overestimation of the interval preceding that longer interval. As regards underestimation of the interval following the accented sound, we find one table[7] which shows that when the interval following was 20 per cent. shorter than the interval preceding the accent, and 10 per cent. shorter than the remaining intervals, it was judged less than the remaining intervals 26 times,

[1] *Ztschr. f. Psychol.*, 18, 30–36, 1898.

[2] *Harvard Psychol. Stud.*, 1; *Monog. Sup. Psychol. Rev.*, 4, 309–412, 1903.

[3] *Op. cit.*, Tables XXVIII, XXIX, XXXI and XXXII.

[4] Table XXIX.

[5] Table XXXI.

[6] Table XXXII.

[7] Table XXIX.

but as equal to them 31 times, and greater six times. The table shows, therefore, that the interval following the accented sound, which was 10 per cent. less than any of the other intervals, was judged either equal or greater in 59 per cent. of the cases. This is about as strong evidence of overestimation as the other three tables give of underestimation. Still more doubt is thrown upon McDougall's conclusion in this connection from the fact that, though five subjects are said to have participated in the experiments represented in these four tables, no separate record is kept of these individuals, and even then the total number of judgments on any one set of intervals is very often not over six. These experiments were made as already stated with series of six sounds, one of which was louder than the others. Other experiments were made by McDougall in which series which might be called rhythmical were presented to the subject, that is, series in which the accented sound recurred regularly every other time or every third time. In these, it is quite evident that the interval preceding the accented sound is overestimated as compared with the other intervals. McDougall moreover determines the magnitude of this relative overestimation of the interval preceding accent in rhythmical groups of both two and three sounds. To accomplish this, he ascertained at what temporal spacing the grouping disappeared, that is, what relative length of the intervals before and after the accented sound was necessary to produce the impression of temporal uniformity in the series. He refers to the point at which temporal uniformity takes the place of rhythmical grouping as the indifference point. Concerning this point, he writes: "At a certain definite stage in the process the tendencies toward the two forms of apprehension balance each other, so that with the slightest change in direction of attention the rhythmical figure inverts and reverts to the original form indifferently."[1] As regards the effect of duration, McDougall made no investigations corresponding to those on intensity.

Miner[2] investigated the effect of intensity and of duration in visual rhythms, using lights in place of sounds. His subjects mistook a difference in the duration of the lights for one of intensity. The more intense light regularly recurred every second or every third time. The intervals between the lights were always equal. The subjects were asked to group the lights first, in groups in which the brighter came first, and second, in groups in which the brighter came second. While doing this, they had, further, to judge which interval was the longer, and which the shorter. We have no guaran-

[1] *Op. cit.*, p. 382.

[2] *Monog. Sup. Psychol. Rev.*, **5**, No. 4, 1903.

tee that the subjects actually held to this forced rhythm while observing the relative length of the intervals. In fact, Miner, himself, distinguishes two attitudes on the part of these subjects, though all were similarly instructed. These attitudes he calls the rhythmical and non-rhythmical. He reports no introspections, however, on the part of the subjects, on which to base this division of them into rhythmical and non-rhythmical, and it is made merely as an explanation of the disagreement between the results of different individuals. His results do not admit of much generalization. The following statements, however, can be made. The interval which was most often judged the longest, no matter how the sounds were grouped, was that which separated the groups. The interval next most frequently judged the longest was that before the brighter light. Like Meumann, Schumann, and McDougall, Miner also investigated the effect of only one intense stimulus in a longer series of less intense. He found that twelve out of seventeen subjects judged the interval after the bright flash to be the longer.

Miner says that the discrepancy between his own results and those of McDougall may be due to the difference in the quality of the stimulus or the length of the intervals. But this would not explain the discrepancy between Meumann's statements and those of McDougall. Of more importance, probably, is the fact that, in Miner's experiments, the subjects did not know at what point in the series to expect the intense stimulus, whereas, in McDougall's, they did. Miner says that the bright flash was brought in at an unexpected time. Moreover, the subjects were told to notice any difference in the appearance of the interval or the light that followed the bright flash, that is, their attention was directed to the interval and stimulus following the more intense stimulus. With such instructions, we do not know whether the interval following the brighter light appeared longer because of the brighter light or because the subject's attention had been specially directed to the interval and light following the bright flash. McDougall's subjects, on the other hand, knew when the loudest sound was to appear. He writes: "As a single hearing very commonly produced but a confused impression due to what was reported as a condition of unpreparedness . . . . . the method adopted was to repeat each series before asking for a judgment. . . . . . In order to define the direction of attention on the part of the observer, it was made known that the factors to be compared were the durations of the intervals adjacent to the louder sound in relation to the remaining intervals of the series."[1] This difference in the instructions given the subjects may account for the difference in the results. In other words, the disagreement between the results of Miner and McDougall may be interpreted as merely a piece of evidence of the importance of the direction of attention in the estimation of intervals. The results of both investigations agree in that they find an overestimation of the interval preceding that stimulus which, there is reason to suppose, is the object of the greatest amount of expectant attention, namely, the more intense, when the subject knows when to expect it,[2] or the one after the more intense, when they are not informed when to expect the more intense, but have had their attention especially directed, as a result of their instructions, to the stimulus following the more intense. The apparent discrepancy between the results of Miner and McDougall, therefore, goes to confirm Schumann's theory concerning the effect of strain of attention in the estimation of small intervals.

Whether Miner's results are to be considered as contradicting those of Bolton on the effect of an increase in duration is not very clear. Miner found that his subjects mistook an increase in the duration of a light for an increase in intensity, and finds that this apparent increase in intensity has about the same effect on the rhythmical grouping as an actual objective increase in the intensity of the stimulus. But as we have already pointed out, Miner's experiments do not permit us to generalize concerning this effect. And, in fact, we have no way of being sure which of the large number of varieties of effect he mentions were due to apparent intensity and which to the attempt of the subjects to group the sounds in the way that they were instructed to group them.

Notwithstanding many apparent contradictions among the investigations I have reviewed, it is yet possible to indicate the general trend of results so far obtained concerning the effect of variations in the duration and intensity of certain stimuli in a series. There

[1] *Monog. Sup. Psychol. Rev.*, 4, 362–363, 1903.

[2] G. F. Arps und O. Klemm, "Der Verlauf der Aufmerksamkeit bei rythmischen Reizen." *Psychol. Stud.*, 4, 518–528, 1909. These authors, using the sensitivity to temporal displacements of each of the members of a dactylic group (every 3rd sound of the objective series being accented and the intervals all equal) as a measure of the degree of attention bestowed on each of the members, conclude that the greatest degree of the attention occurs at the accented sound and the least at the second unaccented sound. The greatest change in the level of attention occurs therefore during the interval preceding the accented sound. Apart from this work we have the generally recognized fact that intensity is one of the "objective conditions" of attention.

can be no question but that the effect of a more intense stimulus in a series of less intense is different, according as the more intense occurs unexpectedly, or at a time when it is expected, as when the more intense stimulus regularly recurs. The general conclusion indicated, if I may neglect, for the sake of simplicity, the special conditions of the above-mentioned researches, is that a more intense stimulus, if unexpected, causes a relative underestimation of the interval preceding it; if expected (or regularly recurrent), a relative underestimation of the interval following it.

As regards duration, about all that can be said, neglecting special conditions again, is that in the case of regularly recurrent differences, one investigator has found an overestimation of the interval preceding the longer stimulus, another, indications of an overestimation of the interval following the longer stimulus.

CHAPTER II

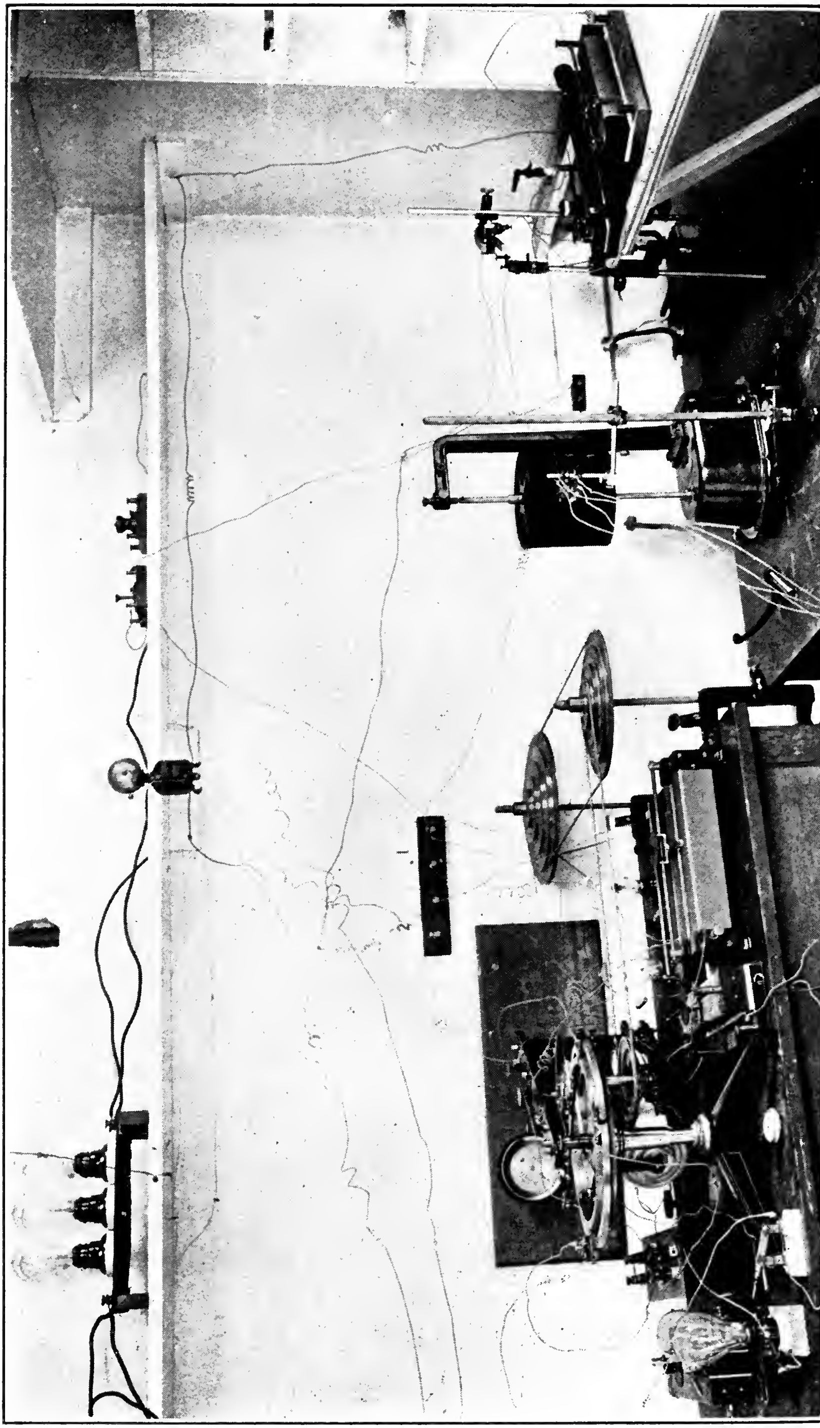
Apparatus and Procedure

The following experiments were performed in the psychological laboratory of the University of Michigan, under the guidance of Professor Pillsbury and Dr. Shepard, whom I am glad to thank for their advice and aid. The work was done mostly during the summer vacations of 1907 and 1908. The subjects used, thirteen in all, were, with one exception, advanced students doing original work in experimental psychology. I take this occasion to express to all those who acted as subjects my appreciation of their patience and conscientiousness.

The first question to be solved, in an investigation of this sort, is how to produce a series of sounds in which it is possible to make accurately measurable variations in the absolute and relative length of the intervals between the sounds, their absolute and relative intensity, their absolute and relative duration, and the relative proportion of sound and silence. I will describe, first, the disposition of apparatus for work in which the effect of intensity was studied and then point out the changes that were necessary for the study of variations in the duration of the sounds and their rate of succession.

Part of the apparatus used in the production of the series of intensively changeable sounds is shown in the cut, opposite page 13, which represents the operator's room. In addition, there was in the subject's room a telephone, head-rest, and a microscope for reading the amplitude of vibration of the telephone plate. This reading was not used in the final estimate of the intensity of the sound, but merely as a preliminary guide.

The sound used for producing the rhythm was that made by a telephone receiver, through which passed an alternating current of 60 alternations per second (a branch from the city lighting circuit), which current, in turn, was interrupted 250 times per second by a tuning fork. The object in having the current interrupted by a tuning fork was to get rid of a click which otherwise occurred at the moment of breaking the current. It was found that, if a current which produced an approximately pure tone was used, there was a very slight click, both at the instant of making the circuit and of breaking it, especially, however, at the break. One could not be sure in judging by the ear alone that this click was not an illusicn due to contrast with the preceding or following silence; but Professor Pillsbury succeeded in demonstrating its



existence objectively through the aid of a string galvanometer set up in circuit with a microphone placed in front of the telephone. I tried many methods of eliminating this click, suggested by officers of the departments of physics and of electrical engineering, but they all failed. Finally, I noticed that in some experiments in which I was using an induction coil in order to get a telephone sound, there was no click at the break and make. As the ordinary inductorium is not absolutely reliable as regards the regularity

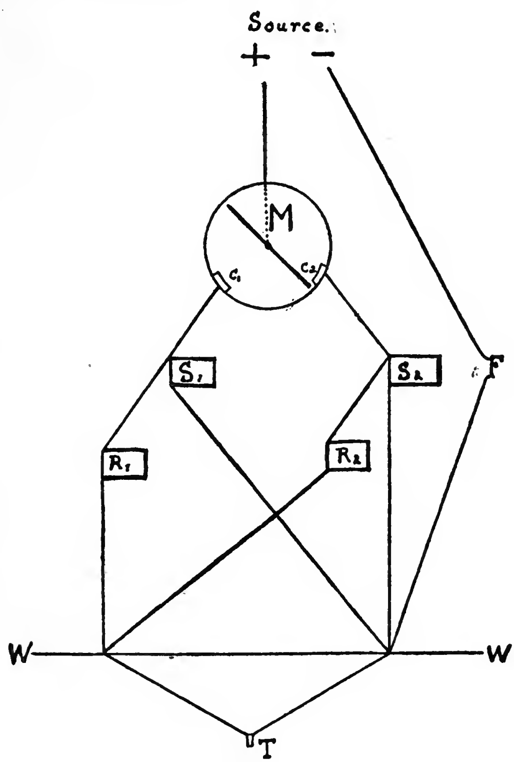


Fig. 1

M = Meumann's machine.
c_1c_2 = contacts.
S_1S_2 = adjustable shunt resistances.
R_1R_2 = adjustable resistances.
WW = wall.
T = telephone.
F = 250 fork.

of vibration and resistance of its interrupter, at the suggestion of Dr. Shepard, a tuning fork with a vibration frequency of 250 per second was substituted. The platinum plate of the fork was provided with a specially devised micrometer screw arrangement, which made it possible accurately to control the closeness of the contact. Needless to say, this contact, as well as all others, were kept bright as new throughout the investigation and frequently renewed. The sound produced in this manner, according to all the subjects who took part in the investigation, was perfectly even and uniform, and was absolutely free from a click at either the beginning or ending.

For the purpose of making and interrupting the sounds, a Meumann's time-sense apparatus[1] was used, on which were arranged ordinarily from 2 to 6 contacts. Each contact, at the time it was closed, formed part of a separate circuit. By changing the amount of resistance in these separate circuits, the intensity of any sound could be varied independently of that of any other. This arrangement is shown in the diagram on the preceding page.

Whenever any measurements of intensity were made, all the contacts and all circuits were arranged as in actual use. This was necessary inasmuch as part of each circuit constituted a shunt for the other circuits.

As a measure of the intensity of the sounds, the distance to which they were just audible was taken. It is not claimed that such a method of measurement is very exact, but on the whole it was considered the most satisfactory. The measurements were all made on the same day, Sunday, during the summer vacation, about two weeks before the opening of college, in Ann Arbor, in an open space on the campus left by the removal of an old building. Only a few series of measurements were taken for each sound, as Sunday was the only day it was quiet enough to work, and it was found impossible in the course of about two months to get more than one good Sunday, that is, one which was free from wind or rain, or the noise of birds, crickets, etc. I obtained as many series of measurements as was possible in one day, working from early in the morning until after dark, with Mr. Dockeray, at the time assistant in psychology in the University of Michigan laboratory. Environmental conditions were constant throughout the day. The method of minimal changes was used, and from 2 to 6 series were obtained with each intensity. At each step, the sound was presented three times in succession, each time for about 1 second. The operator gave the subject a signal by waving a handkerchief a short but variable time before the first sound, and the subject indicated his judgment by raising a handkerchief whenever he thought he heard the sounds

[1] *Phil. Stud.*, 12, 142–152, 1896

and keeping it lowered the rest of the time. The intervals between the three successive presentations of the sounds were made quite irregular. If the subject signaled at such times as to indicate that he heard all three sounds or two out of three, he was considered to have judged "sound audible;" if he signaled correctly for only one of the three sounds, his judgment was called "doubtful;" if he got none right, he was marked as if he had judged "sound inaudible." The method used was therefore really a combination of the method of minimal changes and the method of constant stimuli.

The following measurements were made of the sounds used in this investigation, the distances being given in feet:

Distance audible	No. of series	M. V. (absolute)
24	6	1.2
28	2	1.0
30	2	0.8
32	4	2.6
40	2	2.0
70	3	1.2
136	3	4.3
196	2	6.0
300	4	24.6
420	4	17.3
616	2	35.0
800	4	56.0
1100	2	50.0

In the remainder of this work, whenever the intensity of a sound is indicated, what is meant is the distance to which it was audible.

The rate of rotation of the Meumann's apparatus, and so the rate of succession of the sounds, was controlled by a Helmholtz motor and two ball-bearing speed-reducers. The rate of rotation of the Helmholtz motor was kept as constant as possible through the aid of a speed counter and stop watch, and an adjustable resistance in the circuit passing through the motor. An accurate record of the rate of rotation as well as the duration of each sound and each interval could be obtained by placing in the same circuit with the telephone a time marker writing on a drum alongside another time marker of 100 single vibrations per second. It was, of course, out of the question to measure individually every one of the sounds and intervals used, as the total number was over half a million. What was done was to take several drums of records of each different rhythm used, both at the beginning and end of the hour, and to keep testing the rate of revolution of the time-sense apparatus by a stop-watch and speed counter during the hour. How great a degree of regularity of speed was obtained is indicated by the mean variation of the following measurements of the times of successive revolutions:

Duration of 1 revolution	N.	M. V. per cent.
1.50 secs.	50	0.3
3.00 secs.	56	0.5

A part of this variation is, of course, due to inaccuracies in estimating fractions of a vibration of the fork. I will show later (Chap. IV) that my results would not be changed even by very considerable variations in the rate of revolution. Such extremely small mean variations in the rate of revolution as those just indicated have, therefore, no significance for the results of this investigation, and for that reason, in the tables to follow, no mention is made of the mean variation in the rate of revolution of the time-sense apparatus. Changes in the rate were made by the aid of the speed-reducers. A range of rate was obtained from one revolution in 0.5 seconds to one revolution in 26.0 seconds.

When variations in the duration of the sounds were desired, contacts of different length were used. The accurate measurement of the duration of the sounds was accomplished in the manner described above. Finally, it was possible, within certain limits, to arrange for any desired combination of intensity and duration in the sounds composing the rhythmical series.

Given a series of sounds which may be varied in intensity and duration, the question arises, how are we to investigate the effect of these variations? The question to be solved is, what values have intensity and duration for the impression of rhythm which is obtained in listening to the series of sounds? If the study is to take on a quantitative aspect, there must be some measure of the magnitude of the rhythmical attribute of the total impression; there must be some index to show us whether more or less rhythm is felt, some way of telling which of two rhythms, both of which may perhaps be qualitatively alike, in the sense that they are both trochaic or both iambic, is the stronger or more emphatic. The members of a group may be thought of as being held together more or less securely, by stronger or weaker bonds; and what we desire is a measure of the force with which these members of a group are held together—a measure of what McDougall calls "the rhythmic integration of the stimuli."

A very direct method would be to ask the subject to introspect regarding the relative amount of rhythm produced by two different series of sounds. This task is similar to that required of subjects in experiments on the so-called intensity of sensation. The introspection in the case of rhythm, however, is more difficult, and I doubt if reliable results can be obtained from an investigation carried out in this way. For instance, suppose we compare two trochaic rhythms, one produced by an alternation in intensity and one by an alternation in duration. It may be very apparent that both rhythms are what we call trochaic, but the total impression is very different in the two cases, and it is very hard to isolate the intensive aspect; and consequently it is difficult to say in which case the rhythm is the stronger; and it would be still more difficult to say which is the stronger if one were trochaic and the other iambic. I found that such judgments were not impossible, but I have preferred a more indirect and more objective method, one that demands something far less difficult on the part of the subject.

The following description of the method used in this research may be found rather difficult to follow by those who are unaccustomed to the terminology of rhythm; but I believe that, if the reader will take a pencil and paper and follow out by the aid of symbols the procedure below outlined, he will find no ambiguities. It should be remembered that in all the rhythms here dealt with every second or every third sound is either louder or longer than the others. Also that rhythm is characterized by an apprehension of the sounds in groups and that when there is no grouping there is no rhythm. The method consists in taking as the measure of the amount of rhythm the amount by which, measuring from the point at which all intervals are equal, the internal intervals, or intervals within the group, must be increased or decreased with respect to the external intervals, or the intervals between the groups, in order to cause a disappearance of the rhythm, that is, a disappearance of apparent grouping. Roughly speaking, I have used as the measure of the amount of rhythm the amount of work that had to be done on the intervals to destroy the rhythm. Suppose, for instance, that a certain series of sounds in which every other one is accented, produces an impression of trochaic rhythm, the accented sound seeming to begin the group. Now, such a rhythm can be changed to an iambic one by increasing the interval following the accented sound, with respect to the interval preceding the accented sound. I have found no exception to this possibility. Moreover, as the process of increasing the interval after the accented sound is going on, just before the rhythm becomes iambic, there will be a point reached at which the rhythm can hardly be said to be more iambic than it is trochaic. This point may be called the iambic-trochaic indifference point. It is the point at which the rhythm is destroyed, or at least reduced to a minimum. If, to arrive at this point, it was necessary to produce only a very slight increase in the duration of the interval after the louder sound with respect to the duration of the interval before it, the rhythm may be said to have possessed only a slight degree of temporal segregration to begin with; and that slight degree of temporal segregation to have been in the trochaic direction. This means that, to begin with, the temporal grouping of the sounds was such that the sounds within the same group were only slightly more grouped than two successive sounds belonging to different groups.

Now, if it is desired to determine the effect of intensity or duration on temporal segregation, we must be able to separate the influence toward temporal segregation exerted by intensity and duration from that exerted directly by unequal temporal spacing of the sounds. The temporal segregation we wish to measure is, of course, the apparent, or subjective, temporal segregation—the temporal segregation presented by the rhythm consciousness and not that presented by the objective series. If the indifference point occurs where the intervals are objectively equal, then any differences in duration or intensity which may exist in the sound series are evidently exerting no influence towards temporal segregation. To measure the amount of temporal segregation produced by changes in intensity or duration we must determine the amount of change in the intervals, *from objective equality*, necessary to arrive at the indifference point, or, what amounts to the same thing, take as the measure of the temporal segregation produced by intensity or duration the difference at the rhythm indifference point between the intervals before and after the accented or the longer sound. If, when the intervals are equal and every other sound accented, the rhythm is heard as trochaic, but if this trochaism[1] can be destroyed by increasing the interval after the accented sound, then it is clear that the accent is exerting some influence toward temporal segregation in the trochaic direction. Moreover, if in one case the interval after the accented sound has to be increased 10 per cent., starting with intervals equal, and in a second only 1 per cent., in order to arrive at the point of indifference, it is clear that a greater degree of temporal segregation in the trochaic direction exists in the first case than in the second. On the other hand, if, with equal intervals, a series of sounds should produce the impression of iambic rhythm, but this impression is changed to one

[1] This word was suggested to me by Professor Titchener as at least preferable to "trochaicness."

of indifference by increasing by 10 per cent. the interval *before* the accented sound, then this rhythm could be said to present the same amount of temporal segregation as the series which, with equal intervals, produced the impression of trochaic rhythm, but in which an increase of 10 per cent. in the interval *after* the accented sound brought the listener to the indifference point. Both rhythms could be said to present the same degree of temporal segregation; but one would be in the iambic direction and the other in the trochaic. The influence towards temporal segregation exerted by any such factor as recurrent differences in accent, pitch, duration, etc., may, therefore, be measured by the difference between the external and internal intervals of the group at the indifference point. In the tables of the following chapters this difference has been recorded in the columns headed A–B, which means the difference in duration at the rhythm indifference point between the interval after the accented or the longer second (A) and the interval before the accented or longer sound (B). When A–B is positive, this means that the series is heard as trochaic, when negative, as iambic, providing the intervals are objectively equal. Further, the magnitude of A–B, when positive, is a measure of the degree of temporal segregation in the trochaic sense; when negative, of the degree of temporal segregation in the iambic sense—so far as this segregation or grouping is due to other factors than objective difference of intervals.

But is measuring the amount of temporal segregation the equivalent of measuring the amount of rhythm, the amount of trochaism or iambism? I shall show, in Chapter VI, that rhythmical grouping is a temporal grouping. Rhythmical segregation implies (subjective) temporal segregation. Then "more" or "less" applied to rhythm means more or less temporal segregation; and, in measuring the amount of temporal segregation, we obtain an index of the quantity of rhythm. This same conclusion may be reached by a different line of reasoning. The judgments, iambic, trochaic and doubtful, have a certain range of distribution. If we admit that a rhythm which is judged trochaic 95 per cent. of the judgments is more trochaic than one which is judged trochaic in 60 per cent. of the judgments, then we must also admit that our measure of temporal segregation serves also as a measure of more or less rhythm: because as the degree of temporal segregation in the trochaic direction exerted by an alternation of more and less intense sounds increases (because of increase in the ratio between the intensities) the percentage of judgments "trochaic" also increases, the objective intervals remaining equal. In other words, to destroy a trochaic group which is strongly enough trochaic to be judged trochaic in 95 per cent. of the trials, a larger increase in the interval after the emphasized sound is necessary than that required to destroy a trochaic group which is only strong enough to be judged trochaic in 60 per cent. of the trials. We may conclude, then, that the magnitude of A–B, when positive, may be taken as a measure of the degree of trochaism, when negative, of the degree of iambism, in so far as this trochaism or iambism is due to other factors than objective differences in the intervals between the stimuli.

No use has as yet ever been made of this rhythm indifference point. McDougall, as I have already explained, made use of the indifference point in the estimation of time intervals. The question asked of his subjects "was invariably as to the apparent relative duration of the two intervals,"[1] and the indifference point at which he arrived represents "the quantitative proportion of the two durations necessary to produce the impression of temporal uniformity in the series."[2] The subjects who took part in the present investigation, however, were instructed, except in certain cases mentioned later, first, to judge whether the sounds produced an impression which they would speak of as rhythmical, and second, in case there was rhythm, to indicate as best they could the nature of the rhythm. In their judgment as to the quality of the rhythm, they usually made use of the terms iambic, trochaic, etc.; but as full an introspection as the subject was able to make was taken on every rhythm to which he listened. Nothing was said to the subjects about duration of intervals or temporal uniformity. The indifference point here means a point at which the rhythm is no more one type than another. The iambic-trochaic indifference point is the point at which the impression is no more trochaic than iambic, but at which a slight increase in the interval following the accented sound causes the impression to become one of iambic rhythm, and a slight decrease in the same interval changes the impression to that of trochaic rhythm. That such an indifference point is also the indifference point for the perception of the time intervals is by no means self-evident, and the question can be settled only by experiment. I shall show[3] that the two indifference points correspond very closely indeed, but are not quite identical.

I have defined the indifference point as regards rhythm as that point at which the impression is no more that of one rhythm than of another, *i. e.*, the point at which one rhythm is just as natural and

[1] *Monog. Sup. Psychol. Rev.*, 4, 379, 1903.
[2] *Ibid.*, p. 378.
[3] Chap. VI.

just as easy as another, instead of defining it as the point where no rhythm at all exists, because rhythm may occur even at the indifference point. As Professor Woodworth has pointed out, the same series may be heard in different rhythms.[1] The following introspections from subjects who have had a great deal of experience with rhythms near the indifference point are very definite. Subject Ws. writes: "At the very point where iambic turns trochaic, there seems to be no rhythm at all. The reason I put down 'doubtful' is because I can determine no rhythm. If I could determine rhythm, I could tell whether it was iambic or trochaic. At the turning point, there is just a series of sounds, one louder than the other. It seems to me that at one point there is formed a continuous, even, undivided series, and no rhythm exists. At this point,you can make the rhythm either iambic or trochaic, in your mind." Subject Ww. says: "My general conclusion concerning the indifference point is that I can get any one of three things—no rhythm at all, iambic, or trochaic—any of the three cases may occur depending upon the way I attend to the sounds—the way I listen to them." Subject Br, who always counted when he obtained the rhythmical effect, except in cases where he purposely avoided it, gives the following introspection: "At the time when the notes and intervals were all equal, the idea of number, that is, the impulse to count one, two, dropped out. An idea of mere succession remained." These introspections seem sufficient to establish the fact, that, at the indifference point, rhythm may entirely disappear, but also that either one of two rhythms may be obtained with about equal facility.

Notwithstanding the fact that the impression of rhythm produced by a series of sounds depends to a large extent upon subjective factors, it is none the less true that it depends largely on the nature of the sound series, and it is by no means impossible to study the relation between the series of stimuli and the ensuing impression. When the series of sounds is not near the indifference point, the rhythm perceived by any one subject as the result of any given series of stimuli is practically always the same. And if the subjects are instructed in all cases to indicate which rhythm is the most natural or the easiest, it will be found that the indifference point is really a quite narrow zone, though of course a variable quantity. I instructed the subjects in this manner; and throughout the following work, whenever it is indicated that a certain series of sounds produced a certain impression of rhythm, it is not meant that any other rhythm was absolutely impossible, but that the subject found the rhythm indicated to be the most natural. To indicate the

[1] *J. of Phil. Psych. and Sci. Meth.*, 4, 17, 1907.

manner in which the instructions were followed I cite the following introspection from Ws: "Each time, before determining whether the rhythm is iambic or trochaic, I try it each way. When the loudest and longest note comes first, I put down trochaic; but when it comes at the end of each rhythm, I put down iambic. When I can not distinguish any definite rhythm or tell which comes first, the loud or the soft tone, I put down uncertain. It is not like an impression which comes immediately but which comes after listening a while. I do not notice the length of intervals especially." Subject Wr writes that "Possibly the judgment iambic or trochaic does not mean always that this is obtained the easiest, using easiest in the sense of least effort, but perhaps it has occasionally meant merely most satisfying." All subjects reported that there was no difficulty in following the instructions. Whenever they were not sure of the rhythm, they indicated the fact.

The method used to arrive at the indifference point was the method of minimal changes. The subject was first presented a series of sounds concerning the rhythmical nature of which there was no doubt, a series concerning which he could judge "plainly iambic," or, "plainly trochaic," etc. The time allowed for this judgment in the early part of the investigation was 45 seconds, *i. e.*, the rhythm was allowed to run along unchanged for 45 seconds. In the greater part of the work, however, the rhythm was continued until the subject made up his mind, which event was indicated to the operator by an electric bell signal. In case the subject had not made up his mind, however, at the end of one minute, the rhythm was stopped anyway, and the subject, in these cases, wrote "doubtful." If the judgment "doubtful" occurred on the first rhythm of the series, this judgment was thrown out and the series commenced at some other point where the judgment was that the rhythm was plainly one thing or another. The theory of the method of minimal changes seems to require that we take as our starting-point some point where the judgment is not in doubt. The judgment "doubtful" on the first member of the series occurred very seldom, not over twenty times in the whole investigation; and most of these cases occurred in the presentation of the first series or two of a new rhythm, before the operator had any definite idea as to where the indifference point was located, or, consequently, where best to start the series. The very few remaining judgments of doubtful on the first member of the series occurred in cases where the series was started nearer than usual to the indifference point, either in the hope of hurrying the progress of the investigation or of preventing fatigue on the part of the subject.

In case the first judgment was "trochaic," the interval following the more intense or the longer sound was slightly lengthened, and that preceding shortened by the same amount. This second rhythm was then given for 45 seconds, or until the subject had made up his mind. This procedure was continued until the subject judged "plainly iambic." This whole series was then repeated many times either in the same or reverse direction. In case the first judgment was "iambic," then naturally the succeeding rhythms of that series were produced by shortening the interval following the louder or longer sound and lengthening the interval following the weaker or shorter sound. Similarly, to pass from dactylic rhythm to anapaestic, the interval following the emphasized sound was increased while the others were decreased. The number of rhythms, *i. e.*, the number of steps, in any one series, varied from four to fifteen, but was usually between five and nine. The subjects knew that the change from one rhythm to another was effected by changing the intervals, and that the direction of this change was such that the rhythm would finally go over to something else. They were ignorant of the starting point and the size of the steps. They were informed that the size of the steps might vary considerably or might not vary at all. As a matter of fact, their size varied in different series from .02 to .06 second, in the case of rhythmical measures the total duration of which was 1.5 seconds, and correspondingly for longer or shorter groups. This means that in rhythmical groups the total duration of which was 1.5 seconds the same interval in any two successively presented rhythms of the same series would vary only by from .01 to .03 second, inasmuch as one interval was lengthened as the other was shortened. The most usual variation between any two consecutive steps was .016 second. Very often the subject could notice absolutely no difference in two successive rhythms. I believe that, under the conditions of this investigation, the size of the steps was usually about what would correspond to a just noticeable difference. For any one series of minimal changes, the steps were the same throughout.

It is quite conceivable that under some conditions the judgment concerning rhythm might be strongly influenced by which sound was heard first. There exists, *e. g.*, a tendency, under certain conditions, for persons to hear a series of sounds as trochaic in case the louder note first reaches the ear, though the same series is judged iambic in case the weaker note is the first to reach the ear, or, as I believe may be said with greater truth, the first to receive attention. There can be no doubt of this tendency. I remember that, at the beginning of this investigation, I was rather surprised to hear a subject judge a rhythm to be trochaic which I had judged to be plainly iambic; but, on listening a second time to the same series, I, too, got the impression of trochaic rhythm; and I soon found that which I got depended on which note I heard first. I think it probable that this effect is due to the fact that a certain direction of attention is prescribed by the way the rhythm starts out, inasmuch as this same shifting of the rhythm may occur within one and the same series by shifting the direction of attention, at least according to the introspection of all the subjects I have questioned on this point. The instructions given to my subjects prevented this influence, of the way the rhythm starts out, from affecting my results. Inasmuch as the subjects were to judge which rhythm was the more natural, in those cases in which there could possibly be any doubt concerning the rhythm, and, with some subjects, in every case (as introspections quoted above show), the subjects tried both rhythms to see which was the more natural. Hence it would not matter which way the rhythm started. I have proved that this is the case by keeping throughout quite a period a record of which sound began the rhythmical series. The following table proves that it did not matter for the purpose of this investigation which sound began the series. The table represents the readings in degrees, on Meumann's machine, of the movable contact, at the rhythm indifference point, first, using series in which the longer sound was given first in each case, and second, using series in which the rhythms were always begun on the shorter sound. The temporal value of 1 degree during this work was .00833 second. N refers to the number of series of minimal changes.

Subject	Longer sound given 1st.	N.	Shorter sound given 1st.	N.
Ws	102.5	10	102.5	11
	106.8	9	106.3	11
	110.8	9	112.1	16
	122.9	15	121.3	14
	111.9	4	111.4	7
	84.6	17	85.0	16
	101.5	17	102.0	11
	97.1	6	95.8	5
	97.0	5	96.0	4
	73.3	14	75.0	14
	111.3	14	111.7	13
	107.5	14	107.5	14
Py	105.0	11	114.0	16
	113.8	14	103.4	15
	102.0	6	102.0	6
Wz	102.5	10	102.5	11
	101.0	8	103.4	7
	86.0	16	83.1	6

Notwithstanding that the above results show that, under the conditions of this research, the results were not affected in any very appreciable degree by the question as to which sound came first, nevertheless the precaution was taken throughout to begin about equally often with each note, both in the case of two-membered and three-membered rhythms.

While the method of minimal changes has been used almost exclusively in this research, I have also occasionally used a method of constant stimuli, and cite below results on the same point by the two methods. Both methods, of course, have advantages and disadvantages, but for the purpose of this research no other method than that of minimal changes could even be seriously considered, because of the greater time required, and it would have been absolutely impossible to get any of the subjects to work another hour longer than they did. As it was, three of the subjects, Ws, Dy, and Ww, served for over 300 hours each. From the experiments I performed with the method of constant stimuli, I estimate that 50 per cent. more time would have been required to cover the same ground; and, as far as I can see, the results, even then, would have been less reliable.

It may be well to point out that, while I have spoken of the method used as that of minimal changes, there was no standard stimulus with which a changeable one was compared, and that consequently the procedure is different from that of the method of minimal changes as ordinarily described. The act required of the subject was merely one of introspective description of a single stimulus, the stimulus consisting of a series of sounds. To a certain extent, the judgment was a matter of identification. Further, it may be well to repeat in this connection that the same objective rhythm continued for about 45 seconds, during which time, of course, a large number, say about 30, separate rhythmical groups were presented. The judgment of the subject occasionally underwent several fluctuations before a final decision was reached. Several different and more or less opposed judgments might be made during the continuance of the same rhythmical stimulus; but, unless one of these seemed much less forced than the others, such rhythms were judged "doubtful."

The middle point of this doubtful zone is what is given as the indifference point. In most series, the doubtful zone was quite small. In many series, it was less than the size of the steps used in changing the rhythm. Thus the judgment "iambic" would be given up to a certain point and then a sudden change on the very next step to "trochaic" would occur. In these cases, the indifference point is considered as lying halfway between the last "iambic" and the first "trochaic" judgment. The treatment of series in which the judgments do not follow a regular course has always been a matter for difference of opinion. Some authors advocate considering all judgments as "doubtful" between those judgments which are alike for three successive judgments, while others advocate neglect of all return to the judgment "doubtful" or to the judgment which began the series after one sure judgment has been made which is different from the sure judgment which began the series. Both procedures are plainly unjustifiable theoretically. The question might possibly be settled on an empirical basis, but never has been. The method I have used for treating such series, while one that has never been rigidly advocated, I believe to be the only one that does not lead to absurdities. The method used consists in shifting the doubtful and reverse judgments all to the middle before taking the average. In part, this procedure follows conventional usage. Thus, the rhythm indifference point in the following series would, by most persons, I presume, be considered as lying between numbers 5 and 6, *i. e.*, half way between numbers 3 and 8.

Place in series	Judgment
1	iambic
2	iambic
3	iambic
4	trochaic
5	iambic
6	trochaic
7	iambic
8	trochaic
9	trochaic
10	trochaic

We should get the same result if we shifted judgments 4, 5, 6, and 7 in such a way that all the iambic lay together and all the trochaic together, *i. e.*, changed the places of the trochaic judgments 4 and 6 with the iambic judgments 5 and 7. For the sake of consistency, then, any series of judgments should be treated in the same manner. The following two cases will serve as illustrations.

Place in series	Judgment
1	iambic
2	iambic
3	iambic
4	doubtful
5	iambic
6	trochaic
7	trochaic
8	trochaic

Indifference point in above series is at 5.

Place in series	Judgment
1	iambic
2	iambic
3	iambic
4	trochaic
5	iambic
6	trochaic
7	trochaic
8	doubtful
9	trochaic
10	trochaic
11	trochaic

Indifference point in above series is at 5.

In order to save time and prevent fatigue, I told all subjects to consider the series ended when they had obtained two judgments in succession different from the beginning judgment. As a matter of fact, the results of this research would hardly be noticeably affected by the method used in calculating the indifference point as long as any recognized method was followed. But I consider it advisable to have some one definite rule at the start which gives a reasonable result in any conceivable case.

I have not considered it worth while to give in the tables which follow the range of the zone of doubtful cases. Nor have I tried to work out the correlation between the size of any hypothetically defined doubtful zone and the mean variation of the indifference point in successive series. Inasmuch, however, as neither the method of minimal changes nor that of constant stimuli has ever before been systematically applied to the direct investigation of rhythm, that is, to investigations in which the judgment rendered by the subject is a designation of the nature of the rhythm he gets, it may be worth while, in order to give some general idea of the distribution of judgments, to cite the following records selected as typical:

Method of minimal changes. Subject, Py. Rhythmical stimuli consisting of an alternation of two sounds of unequal, but constant, length, and of equal intensity. Longer sound = .09 second, shorter = .06 second. In the column headed "after," is indicated the duration of the interval after the longer sound; in that headed "before," the duration of the interval before the longer sound.

The table below represents the results for only one series of minimal changes. The total number of series of minimal changes used in this investigation was about 24,000. The indifference point in this series is taken as the point at which the interval after the longer sound is .53 second.

Duration of intervals before and after longer sound, in seconds		Subject's judgments
After	Before	
.61	.39	iambic
.59	.41	iambic
.57	.43	iambic
.55	.45	iambic
.53	.47	staccato
.51	.49	trochaic
.49	.51	trochaic
.47	.53	trochaic

The distribution of judgments, as well as the relation between the methods of minimal changes and constant stimuli, is indicated in the following results from subject Ws. The rhythmical stimulus consisted of an alternation of a loud with a soft sound. Loud sound audible to a distance of 136 feet, soft sound audible to a distance of 24 feet. Both sounds 0.13 second in duration. Total duration of one measure, that is, from the beginning of one loud sound to the beginning of the following loud sound = 1.5 seconds. The method of constant stimuli used consisted merely in giving the same objective rhythms as in the method of minimal changes, but in irregular order.

Method of constant stimuli.

Intervals before and after louder sound, in seconds		Subject's judgments (per cent)			
Before	After	Trochaic	Doubtful	Iambic	N
.66	.57	100.0	0.0	0.0	26
.62	.62	85.7	14.3	0.0	42
.59	.64	71.4	28.6	0.0	56
.57	.66	31.3	59.4	9.3	64
.55	.68	0.0	16.0	84.0	50
.53	.70	0.0	5.5	94.5	54
.49	.74	0.0	0.0	100.0	36

Judging from the above table, the rhythm indifference point is about at the point where the interval before the louder sound is .57 second and that following it .66 second. It will be observed, also, that a change by one step from this point is sufficient to make the judgment in over 70 per cent. of the cases either iambic or trochaic depending upon the direction of the change. Moreover, in this case, as in others with the same and other subjects, the results obtained by this method agree very closely with those obtained by the method of minimal changes. Thus, in the present instance, the indifference point for the same rhythmical series of sounds on the same morning, by the same subject, was found, as the result of ten series of minimal changes, to be at that setting which gives as the interval before the louder sound .57 second (absolute M. V. = .01 second) and that following it .66 second (absolute M. V. = .01 second)—the same result as with the method of constant stimuli.

CHAPTER III

Intensity

The method used for the investigation of the effect of intensity in rhythm consists in finding the indifference point for rhythm with different ratios of intensity between the sounds. The method of determining the indifference point and the method of measuring the intensity of the sounds have been described in Chapter II. Rhythms in which every alternate sound is objectively accented were the ones given most attention. At the rate at which these were usually given, one sound every .75 second, all the subjects invariably got a two-group. Enough work was done on three-membered groups, however, to show that the same general laws hold in both cases.

In the experiments represented in table I., the duration of all the sounds was kept constant and was .13 second, while the total duration of one measure, that is, the time from the beginning of one louder sound to the beginning of the following louder sound, was also kept constant at 1.5 seconds. There were just two different intensities of sounds used in any one rhythmical series, every second sound being louder; and all the louder were of one intensity, and all the weaker of the other intensity. The intensity of the sounds is indicated by the distance, in feet, to which they were just audible. For all the tables presented in this chapter, the weaker sound was just audible at 24 feet. In the column headed A–B, is indicated the duration, at the indifference point for rhythm, of the interval following the louder sound minus the duration of the interval preceding the louder sound. By "before" is meant the interval of silence before the louder sound, *i. e.*, the time from the end of the preceding sound to the beginning of the louder sound; by "after" is meant the interval of silence following the louder sound, *i. e.*, the time from the end of the louder sound to the beginning of the following sound. The quantity A–B, therefore, is obtained by subtracting the corresponding quantity in the column "before" from that in the column "after." N refers to the number of series of minimal changes run through in the determination of any one indifference point. The mean variation (MV) is the mean variation in the duration of the intervals at the indifference point. All the results obtained with any one ratio of intensities are treated as one group in obtaining the mean variation. Inasmuch as one interval was lengthened as the other was shortened, and *vice versa*, the absolute M. V. of one interval is also that of the other; and for that reason, I have given the absolute M. V. rather than the relative. By "louder sound," is meant the distance to which the louder sound was just audible. All durations are given in seconds.

TABLE I

Effect of Relative Intensity on the Iambic-trochaic Indifference Point. Weaker sound just audible at 24 feet.

Subject	Louder sound	Before	After	N	*MV*	A — B
Wr	30	.613	.617	10	*.008*	.004
	32	.603	.627	9	*.005*	.024
	40	.576	.654	9	*.008*	.078
	70	.567	.663	31	*.025*	.096
	136	.555	.675	10	*.010*	.120
	196	.548	.682	15	*.016*	.134
	300	.547	.683	10	*.012*	.136
	420	.536	.694	25	*.024*	.158
	616	.513	.717	11	*.013*	.204
	1100	.509	.721	17	*.016*	.212
Dy	28	.592	.638	6	*.012*	.046
	30	.573	.657	11	*.012*	.084
	32	.564	.666	11	*.014*	.102
	40	.532	.698	18	*.020*	.166
	70	.532	.698	33	*.023*	.166
	136	.506	.724	15	*.016*	.218
	196	.503	.727	23	*.024*	.224
	300	.499	.731	28	*.023*	.232
	420	.503	.727	10	*.014*	.224
	616	.484	.746	22	*.019*	.262
	800	.456	.774	15	*.022*	.318
	1100	.457	.773	34	*.024*	.316
Ww	28	.594	.636	22	*.022*	.042
	30	.581	.648	28	*.016*	.068
	32	.573	.657	20	*.019*	.084
	40	.568	.662	19	*.026*	.094
	70	.558	.672	23	*.024*	.114
	136	.554	.676	25	*.015*	.122
	196	.546	.684	21	*.022*	.138
	300	.546	.684	21	*.020*	.138
	616	.534	.696	21	*.028*	.162
	1100	.534	.696	20	*.016*	.162
Ws	28	.614	.616	20	*.022*	.002
	30	.600	.630	20	*.009*	.030
	32	.590	.640	20	*.012*	.050
	40	.582	.648	38	*.019*	.066
	70	.577	.653	20	*.021*	.076
	136	.568	.662	58	*.033*	.094
	196	.568	.662	56	*.025*	.094
	300	.566	.664	41	*.021*	.098
	420	.559	.671	44	*.028*	.112
	616	.558	.672	46	*.028*	.114
	800	.563	.667	35	*.019*	.104
	1100	.550	.680	38	*.018*	.130

From the preceding table, it is obvious that with equal intervals and every second sound accented the rhythm is trochaic, that is, the effect of accent is to cause the accented sound to appear grouped with the following weaker sound. In chapter II it was shown that when the quantity A–B is positive, its magnitude measures the trochaism of the sound series. It is obvious, therefore, from Table I, that the rhythm becomes more and more trochaic as the ratio between the intensities of the louder and weaker sounds increases. It is further obvious that the trochaism of the series increases at first very rapidly and later very slowly relative to the increase in distance to which the louder sound is just audible, the weaker remaining constant. The same statement can be made concerning the increase in the intensity of a sensation as the energy of the stimulus increases. It is therefore not impossible that the increase in trochaism tends to be proportional to the increase in the ratio of the sensation intensities of the sounds, but not enough is known of the relation between the energy of sound stimuli and the intensity of sensations to make speculation on this point profitable.

That the effect of an objective accent in three-membered groups is similar to that in two-membered groups is shown by Table II, which presents results obtained when every third sound was the more intense. The indifference point for these tables is the dactylic-anapaestic, and was obtained in the same way as in the case of groups of two sounds each, namely, by shortening the interval preceding the more intense sound and at the same time lengthening the interval following it, or *vice versa*. The interval between the two weaker sounds, which were both of the same intensity, (24 feet). was kept constant throughout, and equal to .577 second. As before, the quantities in the column headed A–B represent the duration of the interval following the louder sound minus the duration of the interval preceding it. The duration of all the sounds was .09 second, and the total duration of one measure, that is, the time from the beginning of one louder sound to the beginning of the succeeding louder sound, was 2.0 seconds.

TABLE II

Effect of Relative Intensity on the Anapaestic-dactylic Indifference Point.

Interval between the two weaker sounds (constant) = 0.577 seconds.

Weaker sound just audible at 24 feet.

Subject	Louder sound	Before	After	N	*M V*	A — B
Dy	32	.541	.613	10	*.028*	.072
	40	.525	.629	10	*.022*	.104
	136	.517	.637	9	*.013*	.120
	616	.479	.675	10	*.020*	.196

Subject	Louder sound	Before	After	N	*M V*	A—B
Ww	32	.540	.614	10	*.016*	.074
	40	.523	.631	8	*.013*	.108
	136	.499	.655	12	*.016*	.156
	616	.475	.679	15	*.017*	.204

This same law, namely, that the accented stimulus tends more and more strongly to begin the group with increase in the ratio of intensities of the stimuli, was found to hold good of rhythms in which the stimuli were electrical shocks instead of sounds. The same apparatus was used as in the investigation of sound rhythms except that in place of the telephone, which had produced the sounds, was substituted a pair of sponge electrodes, which were tied to motor points of the arms. Subject Dy gave the introspection that the impression of rhythm was "fully as strong in the case of electrical stimulation as in the case of sound." My own introspection is that there is about as much rhythm in one case as the other. Every third stimulus was the stronger and consequently each group consisted of three movement percepts (each shock producing a twitch of the muscles of the arm): and the indifference point obtained was the dactylic-anapaestic. The results for electrical stimulation are presented in Table III. The total duration of one measure is 2.0 seconds, and the duration of each stimulus equals .09 second. The interval between the two weaker shocks, which were both equal in intensity, was kept constant throughout and was equal to .577 second.

TABLE III

Effect of Relative Intensity on Anapaestic-dactylic Indifference Point when Electric Shocks Are Used as Stimuli.

Interval between the two weakest shocks (constant) = 0.577 second.

Subject	Stronger stimulus	Before	After	N	*M V*	A—B
Dy	weak	.566	.588	6	*.022*	.022
	medium	.553	.601	8	*.017*	.048
	very great	.513	.641	8	*.027*	.128
Ww	weak	.567	.587	6	*.014*	.020
	medium	.561	.593	4	*.016*	.032
	very great	.533	.621	6	*.018*	.088

In the work on sound, all subjects frequently remarked on the greater apparent duration of the louder sound. The difference both in loudness and in duration seemed greater when the rhythm was pronounced than when near the indifference point.

CHAPTER IV

Rate and Intensity

It was desired to study here the effect on the rhythmical impression of changes in the rapidity with which the series was run off. These changes are those produced by varying the rate of rotation of the Meumann's time-sense apparatus. The absolute duration of the measure and of all its parts, intervals and sounds, was varied, while all relative durations were kept constant. One method of changing the rate of a series of sounds is to change only the intervals of silence between the sounds. Such a procedure introduces changes in a factor other than rate, namely, the proportion between the duration of the sounds and the duration of the intervals. In the present experiments, the absolute duration of the sounds was varied in direct proportion to the absolute duration of the intervals between the sounds, so that the proportion of sound to silence and the proportion of either sound or silence to the whole measure was kept constant. The intensities of the sounds were also kept constant.

Tables IV to VI show the effect of variation in the rate of rhythmical series in which every second sound is the louder. The subjects perceived the sounds in groups of two except for the measures whose total duration is indicated as .75 second or .5 second. At these last mentioned rates, the sounds were grouped by four, so that as a matter of fact at these two rates the total duration of the groups was twice that indicated in the tables as the duration of the measure; but the time from the beginning of one of the loud sounds to the beginning of the following is indicated as the total duration of the measure, at these rates as in the other cases, for the sake of readier comparison. In the following tables are presented the duration of the intervals at the indifference point. In the column headed "measure" is indicated the duration of the cycle from the beginning, say, of one loud sound to the beginning of the following loud sound. The expressions, N, M V, A–B, before and after, have the same meaning as in the previous chapter. The quantity A–B, however, which represents the amount by which the interval after the accented sound is longer than the interval before it, at the indifference point for rhythm, is expressed in the following tables as a percentage of the duration, at the indifference point, of the interval before the accented sound. All durations are given in seconds.

TABLE IV

The Effect of Rate on the Iambic-trochaic Indifference Point.

Louder sound just audible at 70 feet; weaker, at 24 feet.
Duration of every sound relative to the total duration of one measure = 13 to 150.

Subject	Measure	Before	After	N	*M V*	A — B
Wr	1.0	.38	.44	13	*.008*	15.8
	1.5	.57	.66	14	*.014*	15.8
	2.0	.77	.87	14	*.007*	13.0
	3.0	1.14	1.32	19	*.054*	15.8
	4.0	1.52	1.76	14	*.027*	15.8
	5.5	2.12	2.39	14	*.055*	12.7
	7.5	3.05	3.11	17	*.100*	2.0
Ws	1.0	.39	.44	11	*.006*	12.8
	1.5	.58	.65	11	*.011*	12.1
	2.0	.78	.87	10	*.016*	11.5
	2.5	.97	1.09	8	*.026*	12.4
Dy	0.5	.18	.23	18	*.007*	27.8
	0.75	.29	.34	19	*.008*	17.2
	1.5	.57	.66	11	*.021*	15.8
	2.0	.77	.87	15	*.015*	13.0
	3.0	1.15	1.31	10	*.032*	13.9
	4.0	1.55	1.73	10	*.040*	11.6
	5.5	2.24	2.27	9	*.051*	1.3
	7.0	3.08	3.08	8	*.096*	0.0

TABLE V

The Effect of Rate on the Iambic-trochaic Indifference Point.

Subject, Ww.
Louder sound just audible at 136 feet; weaker, at 24 feet.
Duration of every sound relative to the total duration of one measure = 13 to 150.

Measure	Before	After	N	*M V*	A — B
0.5	.16	.25	12	*.005*	56.3
0.75	.26	.36	10	*.010*	38.5
1.0	.37	.46	10	*.006*	24.3
1.5	.53	.70	8	*.009*	32.1
2.0	.73	.91	11	*.018*	24.7
2.5	.90	1.16	10	*.022*	28.9
3.0	1.06	1.40	12	*.009*	32.1
3.5	1.29	1.58	10	*.021*	22.5
5.0	1.91	2.19	14	*.041*	14.7
7.5	2.97	3.18	6	*.104*	7.1
10.0	4.03	4.17	7	*.083*	3.5

TABLE VI

The Effect of Rate on the Iambic-trochaic Indifference Point.

Subject, Dy.

Louder sound just audible at 136 feet; weaker, at 24 feet.

Duration of louder sound relative to the total duration of one measure = 37 to 150; of weaker = 13 to 150.

Measure	Before	After	N	*M V*	A—B
1.0	.29	.38	16	*.016*	31.0
1.5	.44	.56	16	*.013*	27.3
2.0	.59	.71	16	*.017*	20.3
2.5	.75	.92	18	*.040*	22.7
3.5	1.05	1.28	16	*.047*	21.9
5.0	1.47	1.87	13	*.072*	27.2
7.5	2.30	2.70	16	*.054*	17.4
10.0	3.20	3.47	16	*.106*	8.4

In considering the above tables, it is necessary to bear in mind the significance of the quantities in the columns headed A–B. As I have previously stated, this quantity is a measure of the amount of temporal segregation presented by the group, and accordingly an index to the amount of rhythm got out of the sound series by the subject. We may therefore describe the effect of variations in rate as follows. Within certain limits the degree of temporal segregation presented by the groups remains very nearly constant. With slower rates, the rhythm entirely or almost entirely disappears. The limits within which the degree of temporal segregation remains relatively constant vary with individuals and with the nature of the objective series. Thus, in the case of subject Dy, in the case presented in Table IV, where there was merely a difference in intensity between the sounds, the rhythm dropped off very suddenly between a rate which gave as the total duration of one measure 4.0 seconds and one which gave as the total duration of one measure 5.5 seconds. With a different sound series, however, one in which the louder sound was also the longer (Table VI), a certain amount of rhythm persisted even at a rate which gave as the total duration of one measure 10.0 seconds; though even in this case there was already a very considerable decrease at the rate of 7.5 seconds for one measure. In general, the tables indicate that, from a rate of 1.0 second for one measure up to a rate of between 4.0 and 7.0 seconds for one measure, the degree of rhythm remains about constant.

Sufficient observations have not been made with very fast rates to generalize concerning the effect of increasing the rate beyond 1.0 second for one measure. There is some indication, however, that with rates faster than 1.0 second for one measure there is a marked increase in the amount of temporal segregation presented by the rhythm. As already stated, grouping by four instead of by two comes in at rates faster than 1.0 second for one measure. Inasmuch as subjective rhythm comes in very markedly at rates of about 1 second for two beats or faster, the increase above noted in the rhythmical effect for the faster rates can hardly be regarded as the effect of an increase in rate *on the rhythmical effect of intensity or duration.* It seems rather that we have in the case of these rates a subjective factor which is more or less independent of any differences which may prevail in the intensity or duration of the sounds composing the series, since at these rates grouping occurs when all the sounds are equal both in intensity and duration.[1]

The rate at which intensity and duration completely fail to exert any influence favoring rhythm is indicated as being somewhat slower than that usually given as the limit of rhythm, being in some cases beyond 10.0 seconds for one measure. While a marked decrease in the amount of rhythm got from a series occurs in the neighborhood of from 4.0 to 7.0 seconds for one measure, the limit at which variations in intensity and duration fail to exert any influence making for temporal segregation is indicated as varying from 7.0 to over 10.0 seconds for one measure. Subject Wr, an experienced introspectionist, told me he got rhythm (when the intervals were not too nearly equal) from a rate which made the total duration of one measure 26.0 seconds. What he meant was that when the intervals were not too nearly equal the sounds seemed grouped by twos, and one sound was louder than the other. Now, grouping is usually held to be one of the chief characteristics of rhythm. Yet Table IV shows for this same subject that, so far as the effect of intensity goes, making one sound stimulus about eight times as energetic as the other failed, practically, to exert any grouping effect when the rate was slow enough to make the total duration of one measure equal to 7.5 seconds. It is evidently impossible to fix the upper (slow) limit of rhythm until we have a definition of rhythm, but by the method I have used it is possible to fix the upper limit at which intensity or duration exert any rhythmical effect in the sense of promoting temporal segregation.

All subjects found the work on the very slow rhythms extremely fatiguing, and it was hardly possible at the slowest rates to run through more than 3 or 4 series of minimal changes at one sitting. Extremely close attention is required at these slow rates to get any rhythm at all.

[1] Meumann, *Phil. Stud.*, **10**, 302, 1894.

CHAPTER V

Duration

In the investigation of the effect on rhythm of variations in the duration of the sounds, I did not find so much agreement between different individuals as in the case of variations in intensity. Individual differences, while quite marked in the case of duration, are yet not so great as to prevent certain generalizations.

The investigation of the effect of duration in rhythm was carried on in the same way as the investigation of the effect of intensity, except that instead of using sounds of different intensity, the intensity of all sounds was kept constant and their duration alone varied. As has already been shown, in the case of two-membered rhythms, if we start out with a trochaic rhythm, in which the louder sound begins the measure, by gradually increasing the interval after the louder sound, we arrive at an iambic rhythm, in which the louder sound ends the measure. I found no exception to the possibility of changing in this manner from trochaic to iambic. Similarly, in the case of rhythm in which we have differences in duration of the sounds but not in intensity, if we start with a trochaic rhythm with the longer sound beginning the measure, by increasing the interval after the longer sound and decreasing the interval before it, we will arrive at an iambic rhythm, with the longer sound second. In this case, the longer sound ordinarily seems to the subject to be accented. I found no exception to the possibility of changing in this manner from trochaic to iambic in the case of any of the subjects that I have worked on, and I have tested thirteen subjects in this way. In the case of one subject however, subject Sh, the *most usual* result was not a change from trochaic to iambic. With a sufficient increase in the interval following the longer sound, the rhythm, instead of changing to iambic, remained trochaic, but a trochaic in which the shorter sound was described as accented. In other words, in the case of this subject the *first* sound of the group was usually accented, irrespective of whether the first sound was the longer or the shorter. While I did not get this result in any other case, three other subjects occasionally stated that they had a tendency to hear a trochaic with the short sound accented instead of an iambic with the long sound accented, when the interval after the longer sound appeared the longer. When the interval *before* the longer sound appeared greater than that following it, the longer sound always seemed accented, that is, in no case was any tendency found to accent the second sound of a two-membered group, when the second sound was the shorter and of equal intensity with the first.

This result seems to indicate that there are two separable objective factors tending to produce subjective accent, at least in some subjects. There is a tendency to accent the longer sound and also a tendency to accent the sound which seems to begin the group. In some subjects there is no evidence of the latter tendency, and only in one subject out of thirteen, subject Sh, was it as strong as the tendency subjectively to accent the longer sound. Only on the supposition of these two separable tendencies does it seem possible to explain the fact, that, when the longer sound appears to begin the group, it invariably receives an accent, whereas when the longer sound ends the group, it occasionally appears less accented than the shorter sound. In the one case the two factors work together; in the other, in opposition.

The apparent intensity of stimuli which, objectively, differ only in duration, is a point of considerable interest. Miner found in his investigation of rhythm that an increase in the duration of a light was mistaken for an increase in intensity. It is, further, a well-known fact, that, in the case of sound, the apparent loudness of a sound increases with the increase in duration, the objective intensity remaining constant. This increase in apparent loudness occurs at first very rapidly and then slower, as the duration is increased; but the maximum of apparent loudness, for sounds of the objective intensity used in this investigation of duration in rhythm, would hardly be reached before the duration had reached at least one second.[1]

The significance of this fact of increase in apparent intensity with increase in duration is very definitely indicated by the following introspection from subject Wr, on a rhythm in which the longer sound was .25 second and the shorter .13 second, and the rhythm given first with both sounds rather loud and then with both sounds rather weak. "There is no doubt but emphasis goes with length: tendency to accentuate short note practically disappeared with weaker intensity." This tendency to accentuate the short note disappears with weak intensities because, when the sounds are weak, a slight difference in duration is of more significance for their apparent relative loudness than when both are quite loud. The results we have just spoken of above, however, show that the greater apparent intensity of one or the other sound is not due

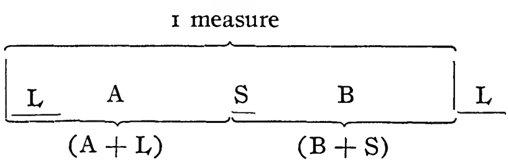
[1] Kafka, "Uber das Ansteigen der Tonerregung," *Psychol. Stud.*, **2**, 256–292, 1907.

merely to the fact that a sound increases in apparent intensity with increase in duration. In the case of subject Sh, and others, in which the first sound of the group was sometimes accented whether the longer or not, we have an indication that some process in the central nervous system is exerting an effect on the apparent loudness of the sounds. I have also many series of introspections which show that the difference in apparent loudness of the two sounds, when the apparently louder is second, is greater as the interval before the second sound is shortened and the interval before the first or apparently weaker is lengthened. The reverse introspection was never obtained. That is to say, in an iambic rhythm, with the second sound apparently louder than the first, the apparent difference in loudness is often reported as decreasing as the intervals become more and more equal. Similarly in a trochaic rhythm, the first sound often seems to decrease in relative loudness as the intervals are made more nearly apparently equal. These introspections were given by subjects Br, Ww, Py, Dy and Ws. When the intervals were nearly equal, such expressions as "very slight accent," "emphasis exceedingly light," "accent doubtful, depending on will" were obtained, while when the intervals were unequal, such introspections as "accent very pronounced," "2nd (or 1st) clearly louder" etc., were given. Concerning a series in which the sounds were unchanged, but the intervals varied in the usual way, subject Ws writes: "The notes are more nearly of the same length as you approach the changing point" (indifference point). Subject Br, concerning a rhythm in which the longer sound was 0.07 second and the shorter 0.06 second: "The longer notes seem more intense and slightly higher than the shorter. When the intervals are about equal, the length and intensity of the notes also seem about equal. According as the attention seizes on one note or the other, is the position of accent and the grouping determined. *At the time the intervals are equal, difference in length and intensity practically disappears.* The difference became more pronounced whenever the rhythm was more pronounced."

These results show that, in some cases, at least, the greater apparent intensity of one or other of the sounds is due to some central process, a conclusion similar to what seems inevitable in regard to the subjective accent in purely subjective rhythm. I think that this process is probably that of attention, but I do not care to insist on this at present, as it would require too much space to give all the grounds for such a belief. It is some process, however, which is quite separate from that involved in the increase in apparent intensity resulting from an increase in duration.

The general conclusion, then, is that the longer sound should naturally appear the louder, as we might say, for sensory reasons, apart from more complicated or more central processes. At the same time, there is some central process, possibly attention, which has some slight effect on apparent intensity. When the longer sound appears the louder there is no necessity for considering this as due to greater attention directed to the longer. On the contrary, the shorter might be receiving the greater amount of attention and yet not appear as loud as the longer, the physiological or sensory effect exerted by duration on intensity being able to swamp the slight effect of the central process on intensity, in case the two processes work in opposition. In those cases, however, where the shorter sound seems the louder, the central process is evidently having a stronger effect than the sensory. It is possible that all the subjects have not used the words intensity, and accent, and emphasis in the same sense, and that the effects referred to in some cases as differences in intensity were differences in some other sort of emphasis; but this makes it all the more desirable to distinguish between the unquestionable effect on intensity of increase in duration and the effect on something referred to as accent or as intensity, exerted equally unquestionably by some other apparently more central process.

The results obtained with regard to the indifference point in two-membered rhythms produced by alternation of a longer and shorter sound of equal objective intensity and of the same quality are presented in Tables VII and VIII. By the total duration of a measure, or in Table VIII, by "Measure," is meant the time, say, from the beginning of one long sound to the beginning of the next long sound. By N, is meant the number of series of minimal changes run through in the determination of any one indifference point. The tables show the length of the intervals at the indifference point for rhythm. By the interval "before," is meant the duration of the interval before the longer sound, *i. e.*, the time elapsing from the end of the short sound to the beginning of the long; and by the interval "after," is meant the interval after the longer sound, *i. e.*, from the end of the long sound to the beginning of the short. The mean variation referred to is the variation in the length of these intervals. All the results obtained from any one ratio are treated as one group in obtaining the mean variation. Inasmuch as one interval was lengthened as the other was shortened, and *vice versa*, the absolute M V of one interval is also that of the other; and for that reason, I have given the absolute M V rather than the relative. In the column A–B, I have indicated the difference in duration between the interval following the longer sound and that preceding it. In the column headed (A + L) — (B + S), is indicated the difference between the duration of the interval of silence after the longer sound + that of the longer sound and that of the interval of silence before the longer sound + that of the shorter sound. In other words, in this column the measure is considered as being composed of two intervals each of which extends from the *beginning* of one sound to the *beginning* of the following sound. The column headed (A + L)—(B + S) presents the duration of the interval extending from the beginning of the longer sound to the beginning of the shorter minus the duration of the interval extending from the beginning of the shorter to the beginning of the longer. The following scheme illustrates this method of treating the measure:



All durations are indicated in seconds. By "longer" or "shorter" is meant the duration of the longer or shorter sound.

TABLE VII

The Effect of Relative Duration on the Iambic-trochaic Indifference Point.
Duration of shorter sound (constant) = 0.13 second.
Total duration of measure (constant) = 1.5 seconds.

Subject	Longer sound	Before	After	N	*M V*	A—B	(A+L)—(B+S)
Ws	.16	.58	.63	31	*.014*	**+ .05**	+.08
(July '08)	.19	.58	.60	58	*.024*	**+ .02**	+.08
	.22	.58	.57	26	*.017*	**—.01**	+.09
	.25	.55	.57	28	*.022*	**+ .02**	+.13
	.28	.55	.54	24	*.020*	**—.01**	+.14
	.31	.55	.51	26	*.010*	**—.04**	+.14
	.37	.55	.45	28	*.014*	**—.10**	+.14
	.43	.55	.39	27	*.020*	**—.16**	+.13
	.48	.54	.35	22	*.010*	**—.19**	+.16
	.60	.54	.23	23	*.018*	**—.31**	+.16
Ws	.18	.50	.56	32	*.020*	**+ .06**	+.11
(July, '07)	.28	.55	.53	18	*.016*	**—.02**	+.13
	.34	.54	.48	41	*.030*	**—.06**	+.13
	.54	.51	.31	22	*.025*	**—.20**	+.21

TABLE VII—(*Continued*)

Subject	Longer sound	Before	After	N	*M V*	A—B	(A+L)—(B+S)
Ww (July, '08)	.16	.57	.64	15	*.016*	+ .07	+.10
	.19	.57	.61	16	*.015*	+ .04	+.10
	.22	.57	.58	16	*.023*	+ .01	+.10
	.25	.54	.58	16	*.015*	+ .04	+.16
	.28	.54	.55	10	*.014*	+ .01	+.16
	.31	.53	.54	10	*.019*	+ .01	+.19
	.37	.51	.49	15	*.016*	— .02	+.22
	.43	.47	.47	10	*.015*	.00	+.30
	.48	.43	.46	10	*.024*	+ .03	+.38
	.60	.40	.37	16	*.013*	— .03	+.44
	.72	.36	.29	10	*.011*	— .07	+.52
	.84	.33	.20	10	*.013*	— .13	+.58
Ww (July, '07)	.18	.56	.62	31	*.025*	+ .06	+.11
	.26	.52	.58	23	*.036*	+ .06	+.19
	.34	.50	.52	28	*.030*	+ .02	+.23
	.42	.48	.46	21	*.034*	— .02	+.27
	.54	.43	.39	25	*.020*	— .04	+.36
Dy	.16	.53	.68	42	*.040*	+ .15	+.18
	.19	.51	.67	35	*.037*	+ .16	+.22
	.22	.53	.62	33	*.020*	+ .09	+.18
	.25	.51	.61	45	*.027*	+ .10	+.22
	.31	.46	.60	33	*.026*	+ .14	+.32
	.37	.43	.57	37	*.031*	+ .14	+.38
	.43	.40	.54	35	*.028*	+ .14	+.44
	.48	.39	.50	28	*.019*	+ .11	+.46
	.60	.36	.41	25	*.038*	+ .05	+.52
	.72	.28	.37	18	*.041*	+ .09	+.68
	.84	.24	.29	15	*.020*	+ .05	+.76
Wr	.16	.61	.60	15	*.027*	— .01	+.02
	.19	.58	.60	11	*.011*	+ .02	+.08
	.22	.58	.57	25	*.020*	— .01	+.08
	.25	.54	.58	16	*.018*	+ .04	+.16
	.28	.55	.54	16	*.023*	— .01	+.14
	.31	.53	.53	17	*.011*	.00	+.18
	.37	.54	.46	15	*.014*	— .08	+.18
	.43	.51	.43	17	*.023*	— .08	+.24
	.48	.49	.40	19	*.014*	— .09	+.26
	.60	.44	.33	19	*.024*	— .11	+.36

TABLE VIII

The Effect of Relative Duration on the Iambic-trochaic Indifference Point.

Subject	Measure	Shorter	Longer	Before	After	N	*M V*	A — B	(A + L)—(B + S)
Ws	.64	.05	.06	.24	.30	10	*.017*	+ .06	+.07
			.07	.22	.30	12	*.018*	+ .08	+.10
			.08	.23	.29	10	*.012*	+ .06	+.09
			.10	.21	.27	10	*.020*	+ .06	+.11
			.15	.22	.22	11	*.019*	.00	+.10
			.20	.23	.17	11	*.011*	— .06	+.09
			.25	.22	.12	11	*.018*	— .10	+.11
Ws	1.11	.10	.13	.39	.49	10	*.017*	+ .10	+.13
			.15	.39	.47	10	*.016*	+ .08	+.13
			.16	.38	.46	10	*.019*	+ .08	+.14
			.20	.38	.44	11	*.016*	+ .06	+.16
			.23	.38	.40	10	*.015*	+ .02	+.15
			.25	.37	.39	10	*.015*	+ .02	+.17
			.29	.38	.34	12	*.017*	— .04	+.15
			.34	.37	.31	10	*.012*	— .06	+.18
Py	1.49	.13	.18	.55	.63	32	*.032*	+ .08	+.11
			.34	.48	.54	26	*.039*	+ .06	+.29
			.54	.40	.43	32	*.031*	+ .03	+.45
Vt	1.11	.10	.13	.39	.49	10	*.016*	+ .10	+.13
			.20	.37	.45	11	*.010*	+ .08	+.18
			.25	.37	.39	10	*.017*	+ .02	+.17
Br	1.50	.08	.10	.60	.72	11	*.030*	+ .12	+.14
			.18	.60	.64	10	*.027*	+ .04	+.14
			.27	.59	.57	10	*.014*	— .02	+.17
			.35	.57	.49	10	*.017*	— .08	+.19
			.43	.58	.40	7	*.017*	— .18	+.17
			.52	.55	.35	8	*.017*	— .20	+.24
			.60	.55	.27	8	*.015*	— .28	+.24

As I have already indicated in Chapter II (pages 17–20), the magnitude of A–B, when positive, may be taken as a measure of the degree of trochaism, when negative of the degree of iambism presented by the group when the intervals between the sounds are objectively equal. A study of the columns headed A–B in the above tables shows that without exception the rhythm becomes less trochaic or more iambic as the ratio between the longer and shorter sounds increases. In some cases, as with subjects Ww, Ws, Wr and Br, the change is one from a trochaic rhythm through an indifference point to a more and more pronounced iambic. In other cases, as with subjects Py, Dy and Vt, the change is one from a very decided trochaic to a less stable trochaic. In the case of Vt, however, there is little doubt but that the rhythm would have become strongly iambic with larger ratios between the durations of the sounds. In the case of Dy, the change is very slight; but it is still evident enough to render it unnecessary to regard this subject as an exception to the general rule. The general law indicated, then, regarding the effect of the relative duration of the sounds in auditory rhythms, is that the rhythm becomes less trochaic or more iambic as the ratio between the longer and shorter sounds increases. As I have found this law to hold with several different absolute durations of the shorter sound, it seems more or less general, though I am unable to present data concerning its limits.

Subject Sh might prove an exception to this generalization. I was unable to study the subject thoroughly enough to be sure whether he would or not. In his case, I found that, with a ratio of two to one and equal intervals, the rhythm was trochaic with the longer sound accented. When the interval before the longer sound was made considerably shorter than that after it, the rhythm usually changed to a trochaic with the shorter sound accented. I believe, consequently, that in generalizing the effects of duration on rhythm, it is safer to omit the words iambic and trochaic. I would therefore state the law of the effect on auditory rhythms, composed of groups of two sounds each, exerted by changes in the ratio of the durations of the sounds, as follows: With an increase in the ratio between the duration of the longer and shorter sounds, if the intervals between the sounds are objectively equal, there is an increase in the tendency to obtain a rhythm with the longer sound at the end of the group or a decrease in the tendency to obtain a rhythm with the longer sound at the beginning of the group.

It is interesting to note that when the intervals are objectively equal, the series is heard as trochaic rhythm when the difference in the duration of the sounds is sufficiently small. Out of a total of thirteen subjects, only two, Ws and Wr, ever showed any exception to this statement, and even in these cases the prevailing tendency with the smaller ratios is towards trochaic. In every case, with a ratio of durations of approximately 2 to 1 (25 to 13), with equal objective intervals, trochaic rhythm was the result.

The most strongly trochaic rhythms produced by duration changes are those produced by the smallest ratio of durations. It looks as though, were the series continued up to a ratio of one to one, the series would be more trochaic than ever. This is, however, not the case. When there is no difference at all in duration (nor in accent) the indifference point was found to be almost anywhere, that is, all judgments become doubtful. The strongest trochaic which it is possible to produce by duration changes alone, therefore, is produced when the longer sound is longer, but only very slightly so, than the shorter sound. It may be that the trochaic rhythm of the smaller ratios was due chiefly to the difference in apparent loudness between the longer and shorter sounds. A comparison of the effect of slight differences in duration with slight differences in intensity lends support to this idea. If we rank the four subjects used in the investigation of the effect of intensity with respect to the magnitude of the quantity A–B for the two small intensity ratios of 30 to 24 and 32 to 24 (feet at which audible), and then with respect to the same quantity for small ratios of durations, we find that the correlation is perfect. In other words, if a slight difference in intensity produces a strong trochaic rhythm for any given subject, a slight difference in the duration of the sounds does also; whereas if a slight difference in intensity produces only a very weak trochaic, the same will be true of a slight difference in duration. I believe, therefore, that the trochaic rhythm produced by small ratios in the durations is due to the different effect as regards intensity of a long and short sound. Now this difference in apparent intensity should increase as the difference in duration increases. But after a certain comparatively short duration is reached, the increase in apparent intensity is very slow. It seems probable, therefore, that, in rhythms produced by short sounds, we have to deal with two separate factors, intensity and duration, even though the sounds differ physically only in duration. The effect of intensity is to make the longer sound begin the measure, an effect which may be spoken of as trochaic, whereas the effect of duration, as such, is to make the longer sound end the measure, which usually means an iambic effect. With a small difference in the absolute durations of the longer and shorter sounds, the effect of intensity is comparatively great; but as the longer sound is increased more and more, the intensity difference between the two sounds becomes less and less relative to the difference in the duration between the sounds. When the difference in duration is sufficiently increased, therefore, we find that the trochaic effect of intensity tends to disappear because of the iambic effect of duration, that is, we get trochaic rhythm with small ratios but tend more in the iambic direction with large ratios (the absolute duration of the short sound remaining constant).

This conclusion is further confirmed by the results on absolute duration. The difference in intensity between the longer and shorter sounds for any given ratio of their durations will be smaller, the longer the short sound. And my results on the effect of absolute duration, presented in Table X, show that, if the ratio is kept constant, the rhythm becomes less trochaic or more iambic, as the absolute duration of the sounds increases.

The effect of the difference in intensity produced by a difference in duration may also explain in part at least the case of subject Dy, in which there was only a slight decrease in the trochaicness of the rhythm with an increase in the ratio between the durations. For, with Dy, differences in intensity had much more effect than with any of the other subjects investigated for the effect of variations in intensity. The greatest magnitude obtained for the quantity A–B, in the investigation of intensive differences with Dy, was 50% greater than the greatest magnitude of the same quantity for any of the other three subjects. Inasmuch as the effect of intensity is trochaic, we should expect to find the tendency towards iambic resulting from large ratios between the durations to be less in the case of Dy than in the case of the other subjects.

I have confined myself chiefly to the investigation of two-membered groups; but that the above mentioned general law concerning the effect of variations in the relative durations of the sounds holds for three-membered groups, as well as two-membered groups, is indicated by Table IX, where the quantity A–B changes from +.02 to —.06 with increase in the ratio of duration between the longer and the two shorter sounds. The method which was used here to determine the indifference point was to vary only the intervals immediately preceding and following the long sound. The two shorter sounds, both of equal duration, were kept the same distance apart throughout, being separated by an interval such that this interval plus the duration of one of the shorter sounds equals one-third the total duration of the measure. By the interval "before," is meant the interval before the longer sound; by the interval "after," the interval after the longer sound, as in the tables on rhythms made up of groups of two sounds.

TABLE IX

The Effect of Relative Duration on the Anapaestic-dactylic Indifference Point. Subject, Ww.

Duration of the two shorter sounds (constant) = 0.08 second. Total duration of measure = 2.0 seconds. Interval between the two shorter sounds (constant) = 0.58 second.

Longer sound	Before	After	N	*M V*	A—B	(A + L)—(B + S)
.16	.54	.56	10	*.013*	+ .02	+ .10
.30	.50	.44	11	*.024*	— .06	+ .16

Before proceeding to the discussion of the effect of variations in the absolute duration of the sounds, it is desirable to bring up the question of the effect of change in the duration of the intervals while the absolute and relative duration of the sounds remains unchanged. This is necessary inasmuch as, in the above tables, which show that there is a change in the iambic direction as the ratio of the durations increases, there occurs simultaneously with the increase in the ratio between the sounds a decrease in the total amount of silence within the group. The reason for this is that the total duration of the group was kept the same, and evidently, then, as one of the sounds was increased in duration in order to get bigger ratios, the total amount of silence remaining had to be decreased. The question might therefore arise as to whether the change noted above in the iambic direction was due to the increase in the ratio between the durations of the long and short sounds, or whether it was due to the decrease in the proportion of the total duration of the group occupied by silent intervals. I have not sufficient data to state in a definite way what is the effect on rhythm of variation in the duration of the intervals, the absolute and relative duration of the sounds remaining constant; but I have data to show that it is utterly impossible that the changes noted above, from trochaic to iambic rhythm with increase in the ratio of the durations of the sounds, could be due to the coincident decrease in the proportion of the total duration of the measure constituted by the intervals of silence. In the case of subject Wr, with a ratio of durations of 19 to 13, and equal intervals of .59 second, the quantity A–B = + .02 second; with the ratio 43 to 13, and equal intervals of .47 second, the quantity A-B = —.08 second, and with a ratio of 60 to 13, and equal intervals of .39 second, A–B = —.11 second. That this change of A–B from + to — (*i. e.*, change from trochaic to iambic rhythm) is not due to the shortening in the intervals is shown conclusively by the following table, where it is

TABLE X

Effect of Proportion of Sound to Silence on Iambic-trochaic Indifference Point

Subject, Wr.
Duration of shorter sound = .13 second.

Longer sound	Before	After	N	*M V*	A — B
.19	.58	.60	11	*.011*	+ .02
.43	.51	.43	17	*.023*	— .08
.60	.44	.33	19	*.024*	— .11
.43	1.19	1.25	14	*.047*	— .06
.60	1.07	1.20	18	*.025*	— .13
.72	.98	1.17	16	*.025*	— .19

seen that the intervals are in every case about twice as great as in the case of the ratio 19 to 13, where the intervals were .59 second and the rhythm trochaic, and yet the rhythm in these cases, too, becomes strongly iambic as the ratio between the sounds increases. Evidently the change in A-B from + .02 second to — .19 second, as the ratio between the durations of the sounds increases, can not be due to a decrease in the intervals of silence, for these have not decreased, but nearly doubled.

As regards the result of variation in the absolute duration of the sounds, the following table seems sufficient to establish beyond a doubt that in rhythms in which the sounds of a two-membered group are of different durations, with a given ratio of these durations the rhythm becomes more trochaic or less iambic as the absolute duration of the sounds decreases, and less trochaic or more iambic as their absolute duration increases. The effect of change in the absolute duration of the sounds is exceedingly great, so that it is impossible to say of any given ratio of durations that it will produce a certain rhythm, even in the same subject: the absolute, as well as the relative, duration of the sounds must be specified.

TABLE XI

Effect of Absolute Duration on the Iambic-trochaic Indifference Point

Subject, Ww.

Ratio between durations of long and short sound = 2 to 1.

Longer sound	Before	After	N	*M V*	A — B
.09	.63	.81	20	*.009*	+ .18
.25	.54	.58	16	*.011*	+ .04
.45	.41	.40	20	*.015*	— .01
.90	.87	.77	20	*.016*	— .10

In the above table, it is true that the intervals have not been kept constant, but enough has been stated already concerning the effect of intervals to show that the changes here shown from a marked trochaic to a marked iambic are due to changes in the absolute duration of the sounds.

A question of fundamental importance in the investigation of the effect of duration in rhythm concerns the proper method of presenting the results. Should the intervals between the sounds be treated separately, or should only the temporal distances from the beginning of one sound to the beginning of the next be considered? At first consideration, this question may seem to have little significance, at least in the present investigation, inasmuch as the duration of the sounds and intervals have been measured separately, and consequently it is possible to present the results in both ways, and I have so presented them. And it is undoubtedly true that, if all we want is a description of what sort of rhythm is heard from any series of sounds, both the length of the sounds and the intervals between them, in case intervals exist, should be stated. But if we wish to understand or to explain our data, it is of great importance to know from what points to measure in presenting the results. For instance, should a rhythmical group be considered as being made up of "members" which extend, say, from the beginning of one sound to the beginning of the following? Dr. Brown, in his investigation of spoken verses writes: "On the whole I have decided to consider only the beginning of the syllables."[1] In the case of spoken rhythms or of any other rhythms in which the sounds are not of uniform intensity throughout, the question of measurement is perhaps more difficult than in the case of sound rhythms such as those used in this investigation. While in the present investigation the sounds were of uniform intensity throughout their length, it is yet possible that what really *counted* to the listener was neither the actual interval between the sounds nor the time elapsing from the beginning of one sound to the beginning of the next, but the interval between certain points of greatest subjective stress, the subjective stress being due, perhaps, to the occurrence, at certain points, of greater attention, of motor performances, or to unknown factors; and these theoretically possible points of greatest subjective stress might conceivably occur almost anywhere. The significance which is to be attached to any data concerning the objective conditions of rhythms in which variations in duration occur depends upon the mode of tabulating the results.

I have obtained some data which seem to me to solve this question concerning the proper treatment of results. As I shall show in Chapter VI, the temporal spacing of the sounds at the indifference point for rhythm very nearly coincides with that spacing at which all the intervals appear equal, a fact which justifies the conclusion that perception of rhythm and perception of time are closely allied processes. This being the case, it is possible to tell whether in rhythm the interval between the sounds or the interval from the beginning of one to the beginning of the following is the more significant by asking the subject, after having determined his indifference point for rhythm, to compare intervals, first asking him to compare the intervals between the beginnings of the sounds and then asking him to compare the silent intervals between the sounds. Of the two kinds of interval comparison, that one which gives results corresponding to the judgments on rhythm will be indicated as the

[1] *Archiv. of Psychol.*, No. 10, 28, 1908.

process more nearly allied to the process involved in judging of the rhythm; and consequently the more significant mode of tabulating the results will also be indicated. In other words, if the results concerning the indifference point for rhythm correspond with the results in determining the point where the intervals between the the sounds appear equal, and do not correspond with those where the intervals between the beginnings of the sounds appear equal, then the data concerning rhythm should be presented in a way which will make it clear what were the actual silent intervals between the sounds. Four subjects were tested in the above manner. It is unnecessary to insert at this point the detailed results of these tests, as the data are given in Chapter VI. In the case of three subjects, Dy, Ww, and Wr, it was found that the indifference point for rhythm corresponded with that for the comparison of the intervals from the end of one sound to the beginning of the following; while in the case of the fourth subject, Ws, the results on rhythm corresponded with the results on the comparison of intervals between the beginnings of the sounds. There is some ground, then, for the conclusion that usually the intervals which are most important in deciding what will be the nature of the rhythmical impression are the intervals between the end of one sound and the beginning of the following. But, in the case of Ws, there is no doubt that the rhythm judgment was more closely allied to the process of comparing the intervals between the beginnings of the sounds than to that of the comparison of the actual silent intervals. This conclusion is borne out by the following introspection: "I do not mean by intervals the time between the notes. The interval is the difference between the times when the two notes *strike*." Again, "Tones are more nearly of the same length as you approach the changing point, the point where iambic changes to trochaic. The *tones* differ but the *beats* don't." Again, after an hour spent in attempting to compare the actual silent intervals between the sounds, "It is funny, because it is hard to catch the end of a sound and measure intervals in such a manner." The subject plays the piano and organ, and is the only piano player, in fact, the only practical musician of any sort who took part in this investigation. In piano playing, of course, the beat producing any sound comes at the beginning of the sound, at least the greatest amount of tactual and kinaesthetic sensation would be received at the moment of pressing down the keys; moreover, in music, especially organ music, there are practically no silent intervals between the sounds. These considerations suggest that possibly the reason why the judgments on rhythm corresponded in this case with judgments on the intervals between the beginnings of the sounds was that there was in this case a motor performance coinciding with the beginning of the sounds, a performance which may have been absent, insignificant, or of a different character in the case of the other subjects. The importance of motor phenomena in the origin of the impression of rhythm has been sufficiently emphasized by previous authors.

The results of this investigation show very plainly the significance of this question concerning the treatment of results. If we consider the two-membered group made up of two periods, each extending from the beginning of one sound to the beginning of the next, it is true without exception that, when these periods are equal, the rhythm is heard as trochaic, and, providing these periods are kept equal, the rhythm becomes more and more trochaic as the ratio between the durations of the sounds increases. Thus, while in the case of the ratio of durations of 2 to 1, with a rather long absolute duration of the sounds, say a duration of the shorter sound equal to .40 second, we might expect to find the rhythm strongly iambic if the intervals between the sounds were equal, it would without question be heard as trochaic if the intervals between the *beginnings* of the sounds were equal. No subject was found, in this investigation of rhythms produced by variations in duration only, who in any case obtained iambic rhythm from any series of sounds, the intervals between the beginnings of which were equal. As long as the intervals between the beginnings of the sounds are equal, the rhythm becomes more and more trochaic with an increase in the ratio of the duration of the longer sound to that of the shorter. In some subjects, this increase in trochaism is very nearly proportional to the increase in the ratio of the durations, in others, it is less rapid. It seems, therefore, impossible to state the exact relation between the increase in the ratio of durations and the increase in the temporal segregation of the group (in this case, the increase in trochaism) until the causes of individual differences can be quantitatively estimated.

CHAPTER VI

The Meaning of Rhythmical Grouping

Every one is agreed today that the essential thing in the perception of rhythm is the experiencing of groups. It is this experience of groups which distinguishes rhythm in the psychological sense of the word from rhythm in the sense of a regularly recurring event, such as the revolution of the earth about its axis. Thus, Meumann writes that the large number of well-trained observers, which he used in his investigations of rhythm, gave without exception the introspection that a subjective binding together of the impressions into a whole is inseparable from the simplest case of the perception of rhythm.[1] Bolton found in the case of every one of thirty subjects that grouping was the irremissible sign of rhythm. And Miner states at the outset of his treatise on rhythm that he uses the word rhythm only in the sense of rhythmic grouping.

But, while all recent writers on rhythm recognize that the experience of groups is an essential factor in the experience known as the perception, or the feeling, of rhythm, there is wide diversity of opinion as to what we mean when we speak of the members of a series of impressions as being experienced in groups. I am unable to judge just to what extent this diversity of opinion goes, because of the impossibility of ascertaining, in the case of many writers, whether they are referring to the experience of groups, or to the causes of this experience—whether they are describing a state of consciousness, or presenting a theory as to the origin of this state. To give an example, Bolton writes as follows: "The conscious state accompanying each wave of attention grasps together or unifies all the impressions that fall within the temporal period of the wave."[2] If he had written merely that the attention wave grasps the impressions together, one would believe that he was talking of the origin of the group experience; but when he says that it is the *conscious state accompanying* the attention wave, it seems as though he was giving an introspective description of the group experience. Some of the interesting introspections reported by Bolton show the same ambiguity. He says of himself, "The puffs of a locomotive may now be grouped by two or three, but the association of the drive-wheel making one revolution to four sounds renders any other form of grouping than by four difficult."[3] In

[1] *Phil. Stud.*, 10, 271, 1894.

[2] *Amer. Journ. of Psychol.*, 6, 220, 1894.

[3] *Ibid.*, p. 205.

this case, did the idea of the drive-wheel, like the conscious state accompanying an attention wave, grasp or tend to group four impressions together, and so produce a grouping or did the group experience consist merely in the simultaneous experience of the four impressions and the idea of the drive-wheel making one revolution? The introspections by his subjects are also often ambiguous, for instance, the introspection of subject No. 12, of whom Bolton writes: "As to the nature of the group, the subject described his feelings as a tendency to go back when he had heard three or four clicks, as the case might be. He says he has a 'mouthful'—a unity—and when he has one, he seeks to get another."[1] Another subject "noticed rhythms in the sound of mill wheels. When he gave his attention to these sounds he visualized a series of points on a line which he counted by four or two. When he was asked to count a series of dots he said they were divided off into twos by a bracket above them."[2] In this case, the meaning of rhythmical grouping seems to be a grouping by brackets, the idea of bracketed points existing, apparently, more or less simultaneously with the percepts of the mill sounds. Subject No. 7 described some of his groupings as though grouping merely meant counting to eight. At times, Bolton seems to consider that the grouping is a temporal grouping, as when he writes as follows: "The weaker or less accented sounds seem to run together with the stronger, and to form organic groups which are separated from one another by intervals which are apparently longer than the interval which separates the individual clicks."[3] But Bolton nowhere tries to show how "the conscious state accompanying each wave of attention" causes the impressions which it unifies to appear separated by shorter intervals than the intervals separating two successive impressions which fall in different waves, *i. e.*, in different groups.

Meumann writes that for many observers the grouping was always temporal, a temporal holding together, in which the members of the groups appear to follow quicker upon each other, while between every two groups lies a pause.[4] He regards grouping as an intellectual act, which shows itself in the subordination of certain impressions to others, for instance, the subordination of the more intense to the less intense. In verse, however, Meumann

[1] *Amer. Journ. of Psychol.*, **6**, 196, 1894.

[2] *Ibid.*, p. 198.

[3] *Ibid.*, p. 204.

[4] *Phil. Stud.*, **10**, 283 and 304, 1894.

says that grouping may be conditioned entirely by meaning.[1] In this case, then, rhythmical grouping means logical unity.

The position of McDougall seems to be that a rhythmical group is an awareness of a temporal segregation of the impressions. "The whole group of elements constituting the rhythmic unit is present to consciousness as a single experience; the first of its elements has never fallen out of consciousness before the final member appears, and the awareness of intensive differences and temporal segregation is as immediate a fact of sensory apprehension as is the perception of the musical qualities of the sounds themselves."[2] At other times, he seems to regard the grouping as the experiencing of impressions along with the experience of an ideal form, or "Gestaltsqualität," as when he speaks of the experience of rhythm as being supported by the conception of an ideal form which the series of stimuli fulfils.[3] He says further that the synthesis of elements may be mediated by changes in the ideal significance and relation of the various members; also by movements of the head, jaw, throat, eyes, or by muscular strain;[4] but he does not show how these changes in ideal significance or these movements of various parts of the body produce temporal segregation.

The view has been put forth by several recent authors that the rhythmical group consists in the experience of muscular strains along with the experience of the other impressions, such as a series of sounds. According to this view, a rhythmical group may be thought of as a series of impressions strung together by one longer state of strain, much like fish strung together on a string; as when Miner speaks of the separate sensations from the external world as being strung in groups. Stetson represents this view when he says that "the continuity of the rhythmic series, whereby all the beats of a period seem to belong to a single whole, is due to the continuity of the muscle sensations involved and the continuous feeling of slight tension between the positive and negative muscle sets;[5] nowhere within the period does the feeling of strain die out." Similarly, Miner says that "feeling the groups to be units is an illusion due to the presence of movement or strain sensations along with the sensations that are grouped." "These kinaesthetic sensations provide the factor by which unit sensations ap-

[1] *Phil. Stud.*, **10**, 396, 1894.

[2] *Harvard Psychol. Stud.*, **1**, 1903 and *Monog. Sup. Psychol. Rev.*, **4**, 322, 1903.

[3] *Ibid.*, p. 468.

[4] *Ibid.*, p. 343.

[5] *Harvard Psychol. Stud.*, **1**, 1903 and *Monog. Sup. Psychol. Rev.*, **4**, 455, 1903.

pear bound into groups."[1] In speaking of visual rhythms, however, he says: "The units in the group seem crowded closer together and a longer interval appears before the next group starts."[2] This indicates that the grouping is temporal. We might expect, therefore, some attempt on the part of the author to show that strain sensations, which are said to hold the impressions together, cause an underestimation in time; he makes no such attempt. And while it might possibly be held that strain sensations and the perception of time are identical, such a theory would rather lead us to expect the strain sensations to force the separate impressions apart from each other rather than hold them together, because according to any theory which makes strain sensations the basis of the perception of time, the greater the strain sensations during any interval, the greater the apparent duration of that interval.

As I have already stated, in the present research in all the work done on the effect of duration and intensity on rhythm, the subjects were instructed to judge concerning the rhythm, and nothing was said to them concerning intervals. In order to obtain some idea of the nature of the rhythmical group, however, after the work on rhythm was all over, I carried on some investigations on the effect of the same variations in intensity and duration that I had used on rhythm on the apparent length of the intervals. The indifference points of which I have spoken in the chapters on intensity and duration were indifference points for rhythm. This rhythm indifference point should not be confused with what is spoken of as the indifference point in the comparison of intervals. The instructions given the subject and the experimental procedure used in obtaining the various rhythm indifference points have already been described. The procedure for obtaining the point at which all the intervals appear equal, that is, for obtaining the indifference points in the comparison of intervals, was in every respect the same as that used in the study of the indifference point for rhythm; but the instructions given the subject were different. In obtaining the indifference point for intervals, the subject was asked to ignore the rhythm, and (every second sound being more intense or longer) to judge merely whether the interval preceding the louder or the longer sound was greater, equal, or less than that following it.

When there is much of a difference in the duration of the sounds, the subject's idea of what is meant by intervals will evidently have a great influence on the results obtained. If the word interval

[1] *Monog. Sup. Psychol. Rev.*, 5, No. 4, pp. 2, 20, 1903.

[2] *Ibid.*, p. 5.5.

means to the subject the time between the *beginnings* of the sounds, the indifference point for the comparison of intervals will be quite different from what is obtained when the subject is asked to consider the intervals which exist between the sounds. No instructions were at first given to any of the subjects concerning what they should consider to be the interval, as it was desired to see what they did naturally. Rather curiously none of the subjects seemed in any doubt as to what intervals to compare. The possibility that interval might mean anything but one thing never occurred to them, unless it was pointed out to them; and yet one of the subjects, Ws, started in to compare the intervals between the beginnings of the sounds, while three others compared the intervals of silence between the sounds. In all four subjects the results obtained by the comparison of intervals, leaving the subject to follow his own ideas concerning the meaning of intervals, gave as the indifference point for the comparison of intervals in any given series of sounds practically the same point which had been previously obtained as the rhythm indifference point for the same series. As long as the subjects compared intervals in the way which seemed most natural to them, their results for the interval indifference point and the rhythm indifference point corresponded very closely; but when subject Ws was asked to compare actual intervals, results were obtained very different from the results obtained on the rhythm indifference point; and when subjects Dy, Ww, and Wr compared the intervals between the beginnings of the sounds, the interval indifference points thus obtained diverged widely from the corresponding rhythm indifference points. By "before" and "after" in the following table is meant, as usual, the actual intervals of silence before and after the longer sound.

TABLE XII

Duration of shorter sound (constant) = 0.13 second. Total duration of measure = 1.5 seconds.

Subject	Longer sound	Intervals at indifference point: Before	After	Nature of indifference point
Wr	.60	.44	.33	rhythmic
Wr	.60	.46	.31	silence
Wr	.60	.55	.22	beginnings
Ws	.60	.54	.23	rhythmic
Ws	.28	.55	.54	rhythmic
Ws	.28	.54	.55	beginnings
Ws	.28	.45	.64	silence
Ww	.37	.51	.49	rhythmic
Ww	.37	.51	.49	silence
Ww	.37	.58	.42	beginnings

Results are presented for three different sorts of indifference point, the iambic-trochaic indifference point, the indifference point for the comparison of the actual intervals of silence between the end of one sound and the beginning of the following sound, and the indifference point for the comparison of the intervals between the beginnings of the sounds. These three sorts of indifference point are referred to in the column headed "nature of indifference point," as "rhythmic," "silence," and "beginnings."

The comparison of intervals thus affords a method of determining what intervals are the most significant in rhythm, for, evidently, if comparing intervals in one way gives results the same as obtained in judging rhythm, while comparison in a second way gives widely different results, the process of judging of the rhythm must be more closely allied to the first way of comparing intervals than to the second.

TABLE XIII

Intensity of weaker sound (constant) = 24 feet.

Subject	Intensity of louder sound	Duration of every sound	Indifference point for interval comparison				Indifference point for rhythm (iambic-trochaic)			
			N	*M V*	Before	After	Before	After	N	*M V*
Ww	136	.13	19	*.037*	.57	.66	.55	.68	21	*.015*
Ww	136	.22	15	*.026*	.94	1.11	.90	1.16	10	*.022*
Ww	136	.31	10	*.033*	1.31	1.56	1.29	1.58	10	*.021*
Ww	136	.45	16	*.028*	1.92	2.17	1.91	2.19	14	*.041*
Ww	136	.68	16	*.055*	2.93	3.22	2.97	3.18	6	*.104*
Ww	136	.07	14	*.008*	.30	.32	.26	.36	10	*.010*
Ww	136	.09	14	*.011*	.36	.46	.37	.46	12	*.006*
Ws	28	.13	11	*.018*	.62	.62	.61	.62	20	*.022*
Ws	32	.13	10	*.018*	.60	.63	.59	.64	20	*.012*
Ws	70	.13	10	*.018*	.60	.63	.58	.65	20	*.021*
Ws	196	.13	15	*.010*	.60	.63	.57	.66	56	*.025*
Ws	300	.13	10	*.012*	.58	.65	.57	.66	41	*.021*
Ws	1100	.13	10	*.015*	.56	.67	.55	.68	38	*.018*
Wr	136	.13	18	*.010*	.58	.65	.56	.68	10	*.010*
Wr	1100	.13	11	*.016*	.54	.69	.51	.72	17	*.016*
Dy	136	.13	19	*.019*	.58	.65	.51	.72	15	*.016*
Dy	420	.13	14	*.024*	.48	.75	.50	.73	10	*.014*

The results obtained on the indifference point for the comparison of intervals, are shown in Tables XIII and XIV, in which are also given the results on the rhythm indifference point for the same ratios of intensities and durations of the sounds. In Table XIII, results are presented for sound series in which all the sounds of a series were of the same length but every other sound the louder.

In Table XIV results are presented for series in which the sounds were of equal loudness but every other sound the longer. In the columns headed "before" and "after" are given the duration, at the indifference point, of the intervals before and after the longer or the more intense sound.

TABLE XIV

Intensity of all sounds = 24 feet.

Subject	Duration of longer sound	Duration of shorter sound	Indifference point for interval comparison				Indifference point for rhythm (iambic-trochaic)			
			N	*M V*	Before	After	Before	After	N	*M V*
Ws	.28	.13	10	*.009*	.53	.56	.55	.54	24	*.020*
Ww	.37	.13	15	*.015*	.51	.49	.51	.49	15	*.016*
Ww	.61	.22	12	*.022*	.76	.90	.75	.91	12	*.018*
Ww	.86	.30	8	*.028*	.98	1.34	1.00	1.32	14	*.028*
Ww	1.23	.43	6	*.033*	1.53	1.80	1.47	1.86	6	*.028*
Ww	1.65	.65	6	*.030*	2.35	2.65	2.21	2.79	6	*.037*
*Dy	.37	.13	12	*.019*	.43	.57	.40	.60	10	*.045*
Wr	.19	.13	10	*.010*	.58	.60	.58	.60	11	*.011*
Wr	.25	.13	9	*.010*	.54	.58	.54	.58	16	*.018*
Wr	.31	.13	15	*.017*	.52	.54	.53	.53	17	*.011*
Wr	.43	.13	15	*.018*	.47	.45	.51	.43	17	*.023*
Wr	.60	.13	13	*.027*	.46	.31	.44	.33	19	*.024*

*In this case the longer sound had an intensity of 70 feet.

The results show that the absolute and relative durations of the intervals at the iambic-trochaic indifference point are almost the same as when the two intervals (the intervals before and after the louder or the longer sound) appear to be equal in length. The rhythm indifference points and the interval indifference points are almost, but not entirely, identical. If we term the differences in the durations of the intervals before and after the louder or the more intense sound constant errors, then it may be said that considering both Tables XIII and XIV together, most frequently the constant errors are less when the subject judges intervals than when he judges rhythm. In the case of series in which the sounds present only duration differences, however, there seems to be no reliable difference, as in Table XIV the constant errors for rhythm are in five cases greater, in three cases the same, and in four cases less than the constant errors for comparison of intervals. In Table XIII, however, it is fairly evident, perhaps, that the constant errors due to differences in the intensity of the sounds are usually slightly greater for the rhythm indifference points than for the interval indifference points, as the constant errors for the rhythm judgments are greater than those for the interval judgments in fourteen cases and less in three cases. The two sets of results are not exactly identical, but they are so close to it that the conclusion seems fairly safe that they are the result of mental operations which have about the same basis. In other words, the statement by the subject, that certain sounds form an iambic group, is equivalent to the statement that he has perceived a shorter interval before the louder or the longer sound than after it, and the statement that the sounds form a trochaic group means that the subject has perceived a shorter interval after the louder or longer sound than before it. This is the equivalent of saying that the experiencing of a rhythmical group is an experiencing of temporal relations, that the meaning of rhythmical grouping is temporal segregation. This conclusion in no way implies that the subject makes judgments concerning intervals whenever he hears rhythm, but merely that he perceives these intervals. Nor does it imply that in comparing intervals one necessarily judges of rhythm; nor possibly, even, that he has experienced rhythm, since it has never been proved that the immediate experience of a temporal grouping is of itself all that is necessary for the experience of rhythm.

In the hope of making clearer the relation between the perception of a temporal grouping and the perception of rhythm, I cite a few introspections. They are quoted literally, and are to be treated as data requiring interpretation, and not accepted at their face value. Subject Br: "The rhythmical grouping seems a temporal grouping without attention being paid to the temporal relations at the time the rhythm is heard." Subject Wr was asked, after his first hour of work in comparing intervals, which intervals he had been comparing, the intervals between the beginnings of the sounds or the intervals of silence between the sounds. He replied that he did not know which he had been doing. Subject Dy: "Rhythm is not always present in comparing intervals, but it sometimes comes. At such times it seems difficult to judge intervals without the rhythm being the predominating factor. The rhythm I notice in comparing intervals is not always the same as when I put my attention on the rhythm itself, *e. g.*, if I get a trochaic rhythm while judging intervals, and then turn my attention to the rhythm I sometimes get iambic instead. An iambic never changed to a trochaic in this way." Ws: "Rhythm becomes apparent to the ear by unequal lengths of sound. The short intervals come between the sounds which make the rhythm and the long intervals between the groups of sounds." At another time, "When the loudest and longest note comes first, I put down trochaic; but when it comes at the end of each rhythm, I put down iambic. I do not notice the length of intervals especially." Same subject, summer, '07: "When there is absolutely no difference in the length of intervals, the rhythm is trochaic." Summer, '08: "In comparing the intervals, the intervals are equal in iambic rhythm." After an experiment in which this same subject was asked to judge in the case of each sound series, first, whether the rhythm was iambic, doubtful, or trochaic, and second, whether the intervals were equal or unequal, and if unequal whether the interval before or after the louder sound was the longer: "In keeping a record of both rhythms and intervals I went through two different processes of judgment; the two judgments did not seem alike. I had never before realized that the doubtful rhythms were those where the intervals are equal."

The very slight difference already noted as existing between the interval indifference point and the indifference point for rhythm does not seem large enough nor reliable enough to invalidate the conclusion that the grouping referred to when we speak of rhythmical grouping is a temporal grouping. What slight tendency there is for constant errors (in the sense used above) to be smaller when the subject judges of intervals than when he judges of rhythm seems to me to indicate merely the effect of the different attitude on the part of the subject in the two cases, the difference being, to judge from my own introspection, that, in comparing intervals, both sounds are about evenly attended to, there is a comparatively continuous strain, while in judging rhythm, some of the sounds, *e. g.*, the more intense, are attended to more than others. Such a view enables us to understand the statement of Meumann that his subjects only judged the interval between the groups as longer than the intervals within the group, when they did not set out especially to judge intervals ("Wenn sie nicht darauf ausgehen, die Intervalle bewusst zu vergleichen."[1])

We are now in a position to understand in what consists the grouping effect of duration and intensity, and consequently their rhythmical effect in so far as rhythm consists in grouping. A regularly recurring more intense sound has the effect of increasing the temporal value of the interval preceding it as compared with that following it. This means that if the intervals are equal the sounds appear temporally grouped in such a way that the interval preceding the more intense sound separates the group, that is, that the sounds, if heard as rhythm, produce the impression of trochaic rhythm. This effect of the more intense sound may therefore be

[1] *Phil. Stud.*, 10, 304, 1894.

spoken of as trochaic. The regularly recurring more intense sound continues to exert this effect even though, because of an objective shortening of the interval preceding the more intense sound, the rhythm is heard as iambic. The more intense sound is exerting an influence towards trochaism even when it occurs in an iambic rhythm. Similarly, if the distinction between the duration effect *per se* and the intensity effect of an increase in duration be admitted, the results obtained on duration show that a regularly recurring shorter sound also exerts a trochaic influence, and this, too, whether the rhythm is heard as iambic or trochaic. In referring to the trochaic effects of the shorter and the londer sounds, I am speaking relatively. I mean that the effect of the regularly recurring shorter or louder sound is trochaic compared to the effect of the weaker or the longer sound. It is equally true, of course, if we are speaking relatively, to say that an increase in duration or a decrease in intensity exerts an iambic effect, or causes an underestimation of the interval preceding the longer or the weaker sound.

SUMMARY

It is possible to pass from one rhythmical grouping to another by changing the relative duration of the intervals between the sounds. Thus, a trochaic rhythm, that is, one that is composed of groups of two sounds each, the louder sound beginning the group, may be changed to an iambic rhythm, one in which the louder sound ends the group, by increasing the interval immediately following the louder sound or by decreasing the interval immediately preceding it. Similarly, a rhythmical group which begins with a sound longer than the other sounds of the group may be changed to one in which the longer sound ends the group by increasing the interval immediately following the longer sound, or by decreasing all the other intervals. As we pass in this way from rhythmical groups beginning with the louder or longer sound to rhythmical groups ending with that sound, we pass through a zone where the tendency towards the two forms of grouping is equally strong. The middle point of this zone may be termed the rhythm indifference point. If the rhythm indifference point occurs where all the intervals are objectively equal, then any differences which may exist in the objective duration or loudness of the sounds are obviously not exerting any effect towards grouping. But if, when the intervals between all the sounds are objectively equal, grouping is still perceived, the grouping must be regarded as brought about by other factors than objective differences in the intervals. The amount of this grouping effect can be determined by finding out what change from objective equality of intervals is necessary in order to cause the grouping to disappear, that is, by ascertaining the amount by which one of the intervals has to be lengthened or shortened with respect to the others in order to arrive at the rhythm indifference point. We may say, therefore, that the influence towards rhythmical grouping exerted by factors other than objective differences in the intervals, that is, by such factors as recurrent differences in accent, duration, pitch, etc., is measured by the difference, at the rhythm indifference point, between the external and internal intervals of the group.

When the intervals are equal, and every second stimulus the stronger, the rhythm is trochaic, and when every third is the stronger, dactylic. That is, a regularly recurring difference in intensity exerts a tendency towards rhythmical groups with the more intense sound at the beginning. In other words, accenting certain sounds of a series has the same effect on the position of those sounds within

the rhythmical group as objectively increasing the interval preceding them. For instance, if the sounds corresponding to the odd numbers of an equally spaced series be accented, those sounds appear to begin the groups; and similarly, without accenting them, but by sufficiently increasing the interval immediately preceding them, they may be made to begin the groups.

This trochaic or dactylic effect caused by an increase in the relative intensity of every second or every third sound, that is, the tendency of the accented sound to begin the rhythmical group, may be measured by the amount by which the interval immediately following the stronger stimulus has to be increased in order to arrive at the rhythm indifference point: and in this way the effect on rhythm of variations in the relative intensity of the sounds may be studied. *With an increase in the ratio of the intensity of the louder sound to that of the weaker, there is an increase, first rapid and then slow, in the tendency of the more intense sound to begin the group.* In other words, with equal intervals, as the difference in intensity between the louder and weaker sounds increases, the intensity of the weaker sound remaining constant, the rhythm becomes more and more trochaic, if composed of two-membered groups, or more and more dactylic, if composed of three-membered groups.

I have stated that the rhythmical effect exerted by regularly recurrent accents is measured by the difference between the internal and external intervals of the group at the indifference point. This is an absolute measure. By dividing this by the total duration of one measure we get a relative measure, that is, a measure of the rhythmical effect relative to the total duration of one measure. In comparing the rhythmical effect of intensity, duration, etc., in sound series which are run off at different rates, the relative measure is what should be taken into consideration. The rhythmical effect of any given ratio of intensities between the louder and weaker sounds, relative to the rate at which the series is run off, remains constant, in two-group rhythms, for rates varying approximately from one to four seconds for one measure, usually shows a marked decrease by the time a rate of seven seconds for one measure is reached, but in some cases does not entirely disappear at a rate of ten seconds for one measure.

Measurements of the rhythmical effect of changes in the relative and absolute duration of sounds, made by the same method as that used in the case of intensive differences, lead to the following generalizations. *With an increase in the ratio of the duration of the longer sound to that of the shorter, there is an increase in the tendency of the longer sound to end the group or a decrease in its tendency to* *begin the group.* When the ratio of the duration of the longer sound to the duration of the shorter is small, that is, when there is not much difference in the duration of the sounds, and when, further, the absolute duration of the sounds is also small, the longer sound tends to begin the group. A small regularly recurrent increase in duration, then, may have the same effect, providing the absolute duration of the sounds is small, as a regularly recurrent difference of accent. But whereas an increase in relative intensity has the effect of *increasing* the tendency of the accented sound to begin the group, an increase in relative duration has the effect of *decreasing* the tendency of the longer sound to begin the group, and often results in an exceedingly strong tendency on the part of the longer sound to end the group.

If all intervals are kept equal, and every second sound is somewhat longer, we may have a trochaic rhythm. This seems at first sight to indicate that the effect of a slight increase in duration is a trochaic tendency whereas the effect of a considerable increase is an iambic tendency. It is necessary, however, to make a distinction between duration *per se* and the increase in apparent intensity of a stimulus due to an increase in duration. The apparent intensity increases as the duration, at first fast but later very slowly. The difference in the apparent intensity of a sound one second in duration and of a sound two seconds in duration is small, very small compared to the difference in duration. The difference in apparent intensity of a sound one-fiftieth of a second in duration and a sound two-fiftieths of a second in duration is very great, even in comparison with the increase in apparent duration. As we increase the duration of every second sound, therefore, we have two separate and antagonistic effects to keep in mind. First, there is an increase in the tendency of the longer sound to *begin* the group, due to the effect of duration on apparent intensity: second, there is a much more rapid increase in the tendency of the longer sound to *end* the group, due to the effect of an increase in duration *per se.* The increase in the second tendency is so much faster than that in the first, that the second may overcome the first, when the increase in duration is great, even though the first tendency may have been the stronger when the increase in duration was small.

The difference in the apparent intensity between two sounds due to a difference in their duration decreases, compared to the difference in apparent duration, as the absolute duration of the sounds increases. As the absolute duration of the sounds is increased, then, the trochaic effect of duration due to the effect of duration on intensity should decrease relatively to the iambic effect of duration as such. And measurements show that, in fact, *with a constant ratio between the durations of the sounds, as their absolute duration increases, there is a decrease in the tendency of the longer sound to begin the group or an increase in its tendency to end the group.*

The effect of both intensity and duration in rhythm may be generalized as follows. If every second or third sound is made more intense or is made shorter, the effect on grouping is the same as if the interval immediately preceding that sound were increased relative to the other intervals. The effect of the more intense sound, when all the sounds are of equal duration, or of the shorter sound, when all the sounds are of equal intensity, is a relative overestimation of the interval preceding the more intense or the shorter sound. There is an objection to speaking of an overestimation of any interval, however, in that the subjects in the experiments so far considered were not estimating intervals, but were judging rhythm. But when the subjects were instructed to estimate intervals, it was found that, in fact, *the interval preceding the regularly recurrent more intense sound or the regularly recurrent shorter sound is relatively overestimated.* The rhythm indifference point and the indifference point for the estimation of intervals are almost, though not exactly, identical. This close correspondence between the rhythmical grouping and the temporal grouping, or rather this correspondence in the points where both disappear, indicates that rhythmical grouping is a temporal grouping; that is, that rhythmical grouping is determined by the duration of the subjective intervals, not by the objectively measurable intervals, but by the subject's consciousness of these intervals, that is, by the intervals considered as mental magnitudes.

CONTENTS

CHAPTER I

Introduction

CHAPTER II

General Statement of Results

CHAPTER III

The Solution of Problems

CHAPTER IV

Puzzle Material and Tests of Intelligence

CHAPTER V

The Place of Analysis in the Practise Curve

CHAPTER VI

Transfer

THE PSYCHOLOGY OF EFFICIENCY

CHAPTER I

Introduction

1. Statement of Problem

The present study is an attempt, under simplified conditions and with special emphasis upon the motor type of process, to analyze human methods of meeting relatively novel situations and of reducing their control to acts of skill. It thus involves the taking of practise curves, and is similar to the studies previously made by other investigators in learning processes such as the acquisition of telegraphy[1] or of shorthand,[2] of a foreign language,[3] of skill in typewriting[4] and in tossing balls,[5] etc. It differs from these studies, however, in that the original situation is distinctly of the problem type, and in that the acquisition of skill in the succeeding manipulations also involves the problem type of consciousness to a very considerable degree.

On account of this emphasis on the factor of thought, the study is also related to the recent investigations on the thought processes carried on chiefly in Germany.[6] The interest in the present study,

[1] Bryan and Harter, *Psychological Review*, Vol. IV., p. 27; Vol. VI., pp. 346–375.

[2] E. F. Swift, *American Journal of Psychology*, Vol. XIV., pp. 224–230.

[3] E. F. Swift, "Garman Memorial Volume of Studies in Philosophy and Psychology," pp. 297–313.

[4] E. F. Swift, *Psychological Bulletin*, Vol. I., p. 295. W. F. Book, "The Psychology of Skill," *University of Montana Publications in Psychology*, Psychological Series No. 1.

[5] E. F. Swift, *American Journal of Psychology*, Vol. XIV., pp. 201–224.

[6] K. Marbe, "Experimentell-psychologische Untersuchungen über das Urteil, eine Einleitung in die Logik," 1901. A. Binet, "L'étude expérimentale de l'intelligence," 1903. H. J. Watt, "Experimentelle Beiträge zu einer Theorie des Denkens," *Arch. f. d. ges. Psych.*, iv., 1905. N. Ach, "Ueber die Willensthätigkeit und das Denken," 1905. A. Messer, "Experimentell-psychologische Untersuchungen über das Denken," *Arch. f. d. ges. Psych.*, viii., 1906. K. Bühler, "Tatsachen und Probleme zu einer Psychologie der Denkvorgange," *Arch. f. d. ges. Psych.*, ix., 1907. G. Störring, "Experimentelle Untersuchungen über

however, is dynamic rather than structural. It deals with the part which different sorts of thought processes actually play in the meeting of novel situations, and, as far as possible, with the conditions favoring the development of variations. From the latter point of view there are points of contact with Royce's study of invention.[7]

The problem as thus treated may have lost somewhat in precision on account of the breadth of processes entailed, but it is hoped that there has been a corresponding gain in continuity and in the exhibition of organic relationships. The writer plans to follow up this rather general study with detailed investigations on some of the special problems raised.

The study is of human methods, but it was undertaken with the hope that some light might be thrown on animal methods of learning as well. The studies were not planned to have exact similarity in material to that used with animals, but there are, nevertheless, certain fundamental points in common, such as the situations being novel and demanding some form of manipulation.

2. Material Employed

a. General Description.—Mechanical puzzles were chosen as the material to be employed. The term mechanical is used to indicate that all the puzzles involved actual manipulation of materials. No trick puzzles were used, *i. e.*, all the puzzles were possible of solution and all the physical materials required were supplied to the subject. The puzzles might be roughly classified into analytical and synthetical, and, again, into tridimensional and bidimensional. Most of the puzzles were analytical and tridimensional. These were for the most part made of wire, and involved the removing of some part of the apparatus, such as a ring, star or heart, from the rest. Some of the puzzles were of the synthetic or construction type, such as the familiar jig-saw puzzles or rarer forms involving three dimensions.

The movements required for solution were, in general, rather complex. In certain cases the degree of complexity could be indefinitely increased, and yet a single rule be developed for solution in the various resulting forms.

b. Advantages.—The puzzles presented the following advantages in relation to the purpose of the experiment:

(1) They constituted genuine problems. None of the subjects solved an unfamiliar one at sight.

einfache Schlussprozesse," *Arch. f. d. ges. Psych.*, xi., 1908. R. S. Woodworth, *Journal of Philosophy*, etc., 1907, 4, 170. E. B. Titchener, "Lectures on the Experimental Psychology of the Thought Processes," 1909.

[7] J. Royce, *Psychological Review*, Vol. V., pp. 113–144.

(2) They involved transformations in three dimensions, and the ability to construct transformations in the third dimension seems to be a decidedly undeveloped function. For this reason the problems were in a special sense "novel."

(3) The form of the puzzles invited immediate motor response, and yet highly indirect types of solution were in some cases necessary. The result was a wide range of methods varying from "accident" to anticipatory analysis of a most complex sort.

(4) The puzzles fell into several distinct groups according to the principle of solution. Each group was composed of several puzzles differing from each other more or less widely in detail. Opportunity was thus furnished for the study of the place of "perception of similarity" in "transfer."

(5) The physical manipulation of the puzzles was easy, and a long series could be taken at a single sitting without perceptible fatigue.

(6) The movements of the subject could be recorded by the experimenter and this objective account of his behavior could then be compared with his own account given during or at the close of the trial.

(7) Puzzle material can readily be employed with children, with primitive people, and possibly with the mentally abnormal, and it has many points of similarity with the "puzzle-boxes" used with animals. It thus offers rather unusual advantages for comparative psychology.

3. Conduct of the Experiments

a. General Procedure.—The method of conducting the experiments was very simple. The subject was seated comfortably at a table, on which the puzzle was placed. The puzzle was covered by a screen. After the warning signal a starting signal was given, and the screen removed. When the manipulation for the given trial had been completed, the puzzle was immediately removed by the operator and prepared for the following trial. The subject was given no opportunity to examine the puzzle except during the actual trial.

The number of trials for a given subject with a given puzzle varied from 1 to 1,440. The standard number was 50. Fifty-one series were taken in which the number of trials composing the series equaled or exceeded 50.

The number of trials at a given sitting varied with the subject and the puzzle. The sittings were usually of an hour and a half in length. In some cases an entire series of 50 trials was completed in this interval. In others several periods were consumed in gaining the first solution. When more than a single sitting was required for a given series, that fact is noted in the tables or the accompanying discussion.

The times given, unless otherwise specified, are for one-way solutions, *i. e.,* the subjects took the puzzles apart, but the operator put them together again out of view of the subjects. The exceptions referred to were all the puzzles solved by *Rr* and some of those solved by *Wh.* In these cases, the subject took the puzzle apart and also put it together again. The time was taken separately for each operation, two practise curves being thus kept for each puzzle.

Only one subject was used at a time.

b. Instructions to Subjects.—Only the most general indications as to the nature of the solutions required were given the subjects. Thus, in the case of the "analytical" puzzles, the subjects were simply told, "Some part of the puzzle is to be removed." What that was they were to determine on examining the puzzle. The indefiniteness of the instructions made the problem more difficult, but it was the means of testing such qualities as the ability to size up a situation, to eliminate the irrelevant, and to use independent judgment in that selection. It also brought out into prominence and made available for study the great rôle played by more or less explicitly conscious assumptions as to the nature of the problem. All of these things it was the purpose of the study to investigate. Since all knew the nature of the problem in detail by the second trial, the later course of the practise curve was not affected.

The subjects were instructed to "solve" the puzzle each time as rapidly as they could consistently with the stage of progress attained. They worked with knowledge of results as far as the time taken by the separate trials was concerned. No information was given them concerning errors made. The guiding principle was to keep the subjects stimulated to the most efficient activity. Their interest during the first few trials of a given series was naturally mainly on working out the essential elements of the problem. After that had been accomplished, their attention was more and more given to the development of speed. With the exception of a very few cases, carefully noted, the subjects did not stop to repeat a part process during a given trial. The analyses performed, whether of the main or the minor elements of manipulation, were thus of a snap-shot order. Had the plan of allowing the part processes to be repeated been followed, the drops in the curves would undoubtedly have been steeper, but it would have been more difficult to evaluate the results, especially in comparison with other practise curves.

c. Records.—The subject's account was written or dictated by him at the close of each trial. In the earlier solutions in a given series, *i. e.,* in the first two or three trials with a new puzzle, in the case of a number of the subjects, a running account was kept during the process of solution. This comprised exclamations showing the emotional attitude, and more or less extensive descriptions by the subjects of what they were doing and why they were doing it. These remarks were recorded by the experimenter.

The experimenter's account consisted of a description of the movements made by the subject, and, as just stated, of whatever he may have said during the trial. Abbreviations for the important movements were employed, and the movements were recorded at the time of their occurrence. The fact that the movements were in three dimensions, and were variable and complex, made it seem unwise to attempt a mechanical form of registration.

The discussion in the later sections is, in general, based on this triple account: the experimenter's description of what was done, the subject's account written at the close of each trial, and the remarks made by the subject during the trial.

d. Chronometer.—The times of the separate trials were taken by means of a split-second cumulative stop-watch reading to tenths of a second. The split hand proved of value in taking times for the part processes as well as of the whole. The times taken during the first trial varied from a maximum of 33,060 seconds, nearly ten hours, to 7 seconds. The times of the shortest trials varied from 0.4 second (highly specialized conditions for one subject in only one puzzle) to 824.8 seconds. Only in the one special case just mentioned did the time fall below one second. The time fell below two seconds only in 5 puzzles out of 36, 80 per cent. of these cases being in two puzzles. The percentages of the total number of cases in which the times fell below one and two seconds respectively are as follows: below one second, one fifth of 1 per cent.; below two seconds, 2 per cent. The percentages are based on a total of 7,000 cases. It is believed, then, that the chronometer was sufficiently refined for the purpose in hand, especially since the low times are restricted to special cases and the conclusions do not rest on a high degree of accuracy in those cases.

4. Individuals Serving as Subjects

All except five of the subjects had done work in psychology and had some special interest in it. The five mentioned as exceptions were four boys from sixteen to twenty years of age employed about the campus, and the laboratory mechanician. Of the other subjects seven were engaged in instruction in psychology or in research.

Two were instructors in related fields and had had considerable psychological training. The remainder were graduate or special students in psychology. Five of the subjects were women.

The designations for the different subjects is as follows:

The four boys—with grammar school or high school training —*Co, St, Mc, Ry.*

The mechanician, *We.*

The seven with professional training in psychology, *Wh, Bg, Rr, Kk, Re, Hn, Bs.*

The two instructors in related fields, *Br, Ft.*

The graduate and special students, *Rd, Ta, Tz, Fe, Mt, Hy, Rg, Pz, Be, Rs, Ds, Pn, Tr.*

There were, altogether, twenty-seven subjects. Nine of them completed long series on at least six puzzles each. The remainder were given fewer puzzles or shorter series. The nine subjects who took the long series with six or more puzzles were *Co, St, Mc, Wh, Bg, Rr, Rd, Ta, Tz;* the first three in each of groups 1, 3, and 5 given above. The five women subjects were *Rd, Fe, Hy, Rg,* and *Ds.*

5. Explanation of Terms

The terms "analysis," "variation," and "transfer" are used very freely in the discussion which follows.

The term "analysis" is used very broadly for the whole process of mental emphasis, the setting up of an hypothesis on the basis of this emphasis, and the various ways of testing the hypothesis. It would include, at one extreme, the case where the entire process is in terms of ideas, where the thinking is highly symbolic and complex, and where the testing is also done by further thinking, and, at the other extreme, the case where there is a simple noticing of a variation taking place unpremeditatedly and its purposive completion or later adoption.

The term "variation" was used in the statement just given concerning the term "analysis." It is used for the whole set of conscious or "unconscious" changes in methods of attack which might in any way be considered novel. A process of "analysis" would be a "variation," but there might be variations which would not be analyzed.

The term "transfer" is used in the sense involved in the recent discussions of general and special training.

6. Acknowledgments

The writer first became interested in the problems of behavior through an experimental study on the behavior of animals made

under the direction of Professor Edward L. Thorndike. The present investigation of human methods of behavior under conditions analogous to those employed with animals is a direct outgrowth from the study mentioned.

The use of puzzles as material was suggested by Professor E. H. Lindley's "Study of Puzzles."[8]

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[8] E. H. Lindley, "A Study of Puzzles," *American Journal of Psychology*, Vol. VIII., pp. 431 ff.

CHAPTER II

General Statement of Results

1. Methods of Learning

a. "Human" and "Animal."—A great deal has been made of the contrast between "human" and "animal" methods of learning. The latter has been represented as a process of mechanical stamping in of those random or instinctively determined movements which have brought success, and the stamping out of those which have failed.[1] The "human" method has been described as one of understanding of principles and the consequent learning by a single successful experience.[2] These designations, human and animal, have been used as equivalents of "trial and success" and "reasoning" respectively, and so have stood primarily for opposed types of learning wherever found, and secondarily to mark characteristic differences between the learning of men and of animals. The method of "trial and success" has been said to take place under conditions of attention in the case of animals and to be found in men in the acquisition of skill where the attention was on the result rather than on the process or where the variations occurred and built themselves up into habits unconsciously.[3] The results of the present study go to show:

(1) That even where the task is not mainly that of gaining a new form of motor control but is essentially that of "learning by understanding" there are important differences in human methods of learning;

(2) That some of these methods show objective points of similarity to animal methods;

(3) That the "human" and "animal" methods should not be considered as exhaustive of the forms of learning, but as two out of a larger and, at present, undetermined number;

(4) That the "human" and "animal" methods should be considered as limiting members of a series of methods in which different types of analysis play an important if not the determining rôle;

[1] E. L. Thorndike, "Animal Intelligence," p. 45.
[2] M. F. Washburn, "The Animal Mind," p. 237.
[3] E. L. Thorndike, "Animal Intelligence," pp. 101–2.

(5) That these distinctions as to analysis do not correspond merely to that between percept and image but are also found within the perceptual field itself.

The second point will be considered first and then the remaining points will be considered in close connection.

The behavior of human subjects in the puzzle tests to be described in this monograph, at one time or another, showed many of the features usually accredited to the behavior of animals in contrast with that of human beings. The times for repeated successes in a number of cases remained high and fluctuating, the time for later trials in a given series being often greater than that for the first success. Acts which made no change in the situation whatever were at times repeated indefinitely and without modification. In successive trials of a series, after an essential step toward a solution had been performed correctly, it was reversed and done over several times with irrelevant movements interspersed before the subject passed on to the next step. This was clearly not a case of failure to get motor control but of lack of analysis of the mechanical principle involved. This occurred in one case so late in the series as the twenty-first trial. Roundabout methods of solution involving a number of irrelevant movements were persisted in late in the series. In practically all of the cases random manipulation played some part and, in many cases, a very considerable part in the gaining of success.

The reason for this similarity to "animal" methods seems to be that the functions tested were relatively poorly developed. The subjects could not mentally construct in any completeness the spatial transformations required. Discriminations were difficult, and there was no complete system of terms to stand for the discriminations when once made. The ideas with which the subjects sought to reason out the problem were not closely enough related to the case in hand to be of much value. Sometimes they were of negative value in eliminating useless lines of attack; at other times they seemed to be a positive hindrance to the solution. Thus, in some of the puzzles the problem was to separate two apparently closed and interlocked figures. The attempt to use the conception of intersecting circles or of links in a chain in some cases led to the conclusion that the parts of the puzzles or one of them was not really a closed figure after all. This negative result seems to have been of value in that it led to a search for the opening. In other cases the subject persisted in the identification of the puzzle with an actually closed figure, and this identification resulted in a paralysis of mental and physical effort.

This lack of ideas closely related to the puzzles and of development of the fundamental capacity of constructing transformations in three dimensions was reflected in the attitude of the subjects. A number spoke, especially in dealing with their first puzzle, of feeling helpless or hopeless and of being convinced that the thing to be done was impossible.

Highly complex reasoning processes, sudden drops in the curves to a permanently low level, quick adoption of shortcuts, and elimination of erroneous or uneconomical forms of manipulation were, of course, also found, but the opposites of these, as stated above, were surprisingly in evidence.

b. Types of Analysis.—As stated in the introduction, the term "analysis" is used very broadly for the whole process of mental emphasis, whether of the analytic or synthetic type in the narrower sense of the terms. The getting of a single organized view of a mass of details would thus be classed as "analysis," not as synthesis.

It was suggested above that instead of simply contrasting two methods of learning, the "human" and the "animal," it might be more profitable to consider these as limiting cases in a series of grades or kinds of analysis, and to attempt, as far as possible, to chart out these various types. It might very well follow, were such a complete examination of human methods and their conscious accompaniments made, that the various and, probably, fairly numerous forms of learning employed by animals might be more clearly distinguished and evaluated. The results of this study can go only a little way in this direction, and the distinctions given below are not presented as in any way exhaustive or exclusive, but as indicating the sorts of differences which appeared most prominently in the course of the experiments.

The analyses met with in the experiments are classified from the following points of view: (1) Explicitness and results, (2) extent, (3) time-relations to motor variations, (4) material.

(1) *Explicitness and Results.*—These are classified together because the latter may usually be taken as a measure of the former. There is a wide range of variation in felt clearness from the extremely vague to the perfectly clear. This range of felt clearness is matched by difference in results. Some of these differences stated in terms of clearness or results or both are as follows: (*a*) Vague feeling of familiarity when the variation chances to occur again, (*b*) explicit recognition of the variation when it recurs, accompanied by anticipation of it on experience of its immediate antecedent, (*c*) ability to image it factually in part, (*d*) ability to image it completely, (*e*) ability to describe it verbally, (*f*) ability to use it in

novel combinations and to state a general formula for its use under varying conditions.

As exceptions to this parallelism of felt clearness and results may be mentioned the feeling of perfect clearness which sometimes occurs but is not followed by ability to recall, and the many instances of illusions, where solutions that look perfectly clear for the moment are later seen to be impossible.

In the list given just above of grades of explicitness or clearness there was no intention of dealing with the different degrees of reality-feeling or belief which might also to a certain extent parallel that series.

(2) *Extent.*—The basis of division here is the extent to which the entire manipulation of a given puzzle is analyzed. The subdivisions, rather arbitrarily chosen, are (*a*) partial, (*b*) schematic, (*c*) total. In discussing these separate subdivisions other criteria will, to a certain extent, be introduced.

(*a*) Partial. One of the most significant forms of partial analysis met with was the picking out of the portion of the puzzle to be attacked. In many cases this was a mere spatial analysis, "locus" analysis, without involving any perception of mechanical necessities. Possibly the solution came once by accident and the subject noticed the part of the puzzle concerned. Such "locus" analysis is followed by an abrupt drop in the curve owing to the immediate elimination of random movements connected with other parts of the figure. There is no gradual wearing away or "stamping out" but a sudden and complete elimination, and this with a very low grade of analysis. The sudden drops in many of the animal curves may well be due to this sort of analysis. Random movements still occur, but they are limited to the "locus."

Another important form of partial analysis noticed was that of a single step in the process while the other steps were attained only by random movement. This single step was often the final one. The solution would come accidentally, but the subject would notice the last step. In the subsequent trials he would know what to do if he chanced to get to that step but not how to get there.

(*b*) Schematic. In some cases the subject would glimpse the main line of attack, the general plan of solution, but without having analyzed the steps in detail.

(*c*) Total. This covers the cases where the analysis reaches all the steps or elementary movements. The cases included may be subdivided according to the degree of unity obtained in the organization of these elements.

In some cases the whole process remains merely a series of different steps arbitrarily following each other.

In other cases a low grade unity is obtained by noticing that the steps follow each other in a sort of rhythm with perhaps an approximate reversal of movement.

In still other cases the entire series of transformations is combined into a single construction, perhaps of the factual image type, and in yet higher forms a general formula may be substituted for this working image and still greater independence of special conditions be obtained.

In connection with the discussion of the partial forms of analysis it may be worth while to mention the use of the split-second watch, or some more precise instrument, for the purpose of substituting the variability of separate steps in the solution for the variability in total times of solution. This is important since the separate steps are, at times, of very different significance.

(3) *Time Relations to Motor Variations.*—As stated in the introduction, a process of analysis may be looked upon as a variation, but the variations chiefly discussed in this study are of the motor type. The relations of these two types of variation, acts of analysis and motor responses, may be quite varied, especially as to time relations. At one extreme is the motor variation which, perhaps, brings success but which runs its course unnoticed. At the other extreme the analysis may come first and only after a considerable interval be followed by the motor response. Again, the analysis and the motor variation may be simultaneous and yet clearly distinguishable, a flash of insight and a motor impulse. In other cases the analysis occurs at some point in the course of the motor variation. A movement may be started either unconsciously or without any realization of its significance, then the perception of its significance may come, and, if not too late, the course of the movement may be continued purposely. This simple process of noticing, even vaguely, the significance of a movement begun, and its purposive continuance, is essentially similar to the complex forms of hypothesis making and testing. The technique of acquisition of skill in manipulation is, in these humble cases, best described not as contrasting with the complex thinking processes, but rather as in striking accord with them. In each case we have a variation set up as an hypothesis, tested and accepted for control purposes or rejected as the case may be.

(4) *Material.*—The analyses may be classified from the standpoint of the mental material in which they occur as (*a*) sensory or perceptual analyses, and (*b*) image or ideational analyses. The subdivisions refer to the degree of mental organization, the relative amount of meaning in the material at the time of analysis, the terms

sensory and image referring to the lower, and perceptual and ideational to the upper limits of mental organization in the respective fields. Both divisions might be cross-classified again into factual and verbal.

It has been customary to contrast perceptual with ideational analyses to the discredit of the former, and to make the presence or absence of images the important question in the estimation of animal intelligence. To the writer it seems probable that there are wide differences in value in the sorts of analysis to be found within the perceptual field itself, and that these differences may rank in importance with the difference between the fields as perceptual or ideational, peripheral or central. These differences have, to a certain extent, been included in the preceding classifications. Thus, a "locus" analysis and an analysis of mechanical relations might both be within the perceptual field and yet differ widely in value. The process of perceptual analysis does not seem to consist in plastering an image on to a percept, but it seems to be a direct transformation within the perceptual field itself. The experience seems to correspond to what some writers have spoken of as a "movement of attention." The experience of the analysis is distinct from that of ordinary perception, on the one hand, and from that of a motor impulse on the other. It is oftentimes a striking experience and seems to come with a rush or as a flash. These suggestions are based directly on the writer's introspections, but are supported by occasional remarks of subjects to the effect that they seemed to see the relations involved in solution directly, and without the use of imagery.

The advantage of the ideational field over the perceptual, in part at least, was noted as consisting in the lopping off of irrelevant details, the reduction of distracting motor tendencies, which are quite pronounced with the perceptual field, and the resulting ability in the case of the ideational image field of keeping the entire system of transformations within the span of consciousness. Part of the advantage of the verbal image field over the factual is to be found in the still further foreshortening made possible and also in the greater control in the way of recall. The verbal image does not always come after the factual but is used in building up the factual, the latter often being dragged in bit by bit and held by means of words until a total factual image has been built up. The complete factual image may then be superseded by a compact verbal statement. The generalized formula was essential in the case of some of the puzzles in which complex changes could be introduced, the changes, however, being in accordance with a single principle.

There was no magic in the verbal statement, however, as was shown by failure in a case where the rule was memorized without the performance of the analyses of which it was the expression. The generalized formula was most readily developed by those who had been trained in the logic of scientific method.

The relative independence of analysis and imagery is shown by the double fact that, on the one hand, the mere presence of images directly related to the solution of the problem in hand was found to be of no avail unless the act of analysis were performed, and, on the other hand, that previous experience seemed to be effective in determining a perceptual analysis in which no trace of imagery could be found.

The general point which it has been the aim of this section to maintain is that in place of merely contrasting extreme types of learning, such as the "human" and the "animal," or of making a hierarchy of percepts, images and concepts, what is needed is a working out of the sorts of analysis which may occur within the perceptual field, and within the image field, whether factual or verbal, and, in connection with this, to determine more precisely the significance of the mental stuff, or fields, themselves. On this double basis it might be possible to exhibit the rich complexity of human forms of learning, and by means of this to catalogue the narrower but still surprisingly extensive range of animal methods. The gross characterizations would still have their value, but the finer distinctions would undoubtedly prove significant in precise evaluations.

2. Conditions of Efficiency

a. The Fact of Variations.—It seems to be pretty generally agreed that abrupt drops in time, in a given practise series, or sharp increases in score when the times are kept constant, are due to variations in method. In accordance with this plateaus have been interpreted as conditions of stagnation in which variations fail to appear.[4] In the puzzle experiments this general result was confirmed. The efficiency was found to be directly dependent upon success in getting the most appropriate methods or technique.

b. The Consciousness of Variations.—It has been maintained by some that variations in method are most effective when they are not attended to, when they come and also build themselves into habits "unconsciously" or "marginally" rather than "consciously" or "focally."[5] The results of the puzzle experiments are in accord with

[4] Book, "The Psychology of Skill," p. 158.

[5] Swift, "Mind in the Making," p. 213. Book, "The Psychology of Skill," p. 171.

this view so far as the coming of variations is concerned, but not as to the subsequent relations, the employment of the variations. A large percentage of the fortunate variations came altogether unpremeditatedly. The various relations in which a motor variation and its analysis may stand have been indicated in section 1, and the case of the variation appearing first and its analysis or the realization of its significance coming at a later point in its course bulks large numerically in the results. There seems to be a natural tendency to vary which may be favored or hindered in ways to be discussed presently.

The value of the variations, in the puzzle experiments, seems to have varied rather directly with the precision with which they were analyzed and with the extent to which they were treated as hypotheses to be systematically tested with subsequent adoption or rejection. In other words, the drops in the curves, the time being the variable, were coincident with consciously adopted variations rather than with "unconscious" ones.

The reason for this difference of results, as between the present study and some earlier work on training, is not perfectly clear. It may be that the task of learning the typewriter or of using the punching bag is more complex than that of manipulating the puzzles. In complex forms of manipulation attention to a single point of method is likely to mean the loss of anticipatory adjustment to the next step, and consequently a loss in time in so far as that later part of the manipulation is concerned. This seems to have been the case in the negative instances cited by Book.[6] For the control of the variation itself, however, and especially for its reliability under changed conditions, the explicit analysis and testing seem to be of very great value. The loss due to failure of anticipatory adjustment to other parts of the process is likely to be more than compensated by the increased control of the variation itself.

c. Conditions Favoring Variability and Control.—(1) Physical Condition. Book and others have pointed out the significance of good physical condition for furthering the occurrence of variations in method.[7] In his experiments the upward change of level from the plateaus came on "good days" and as a result of the variations which then occurred. Variations in method in dealing with puzzle problems were likewise found to be dependent on high level attention and the latter to be decidedly affected by the physical tone.

(2) Attitude. Of especial significance in the determination of the occurrence and character of variations in the puzzle experiments

[6] "The Psychology of Skill," 1908, pp. 96–97.
[7] Ibid., p. 133.

was what might be called the personal attitude of the subject. Two forms of personal attitude inimical to the occurrence or utilization of variations appeared with especial prominence. They might be called the submissive, or suggestible, and the self-attentive.

The former of these appears in the presence of a person supposed to "know the answer." The object of attention is here the person with the "prestige-suggestion" rather than the problem itself. Not only is the tendency for variations to appear around the problem inhibited by the shift of attention to the "prestige-person," but the special forms of variation which constitute criticism and evaluation are similarly affected. This attitude must be carefully reckoned with in all tests of intelligence. In the puzzle experiments this tendency was so marked with two of the subjects that it was necessary for the operator to entirely screen himself from view in order that slight movements of his should not be taken, correctly or erroneously, as indications of the correctness or incorrectness of certain manipulations. This negative effect of the "prestige-suggestion" was even more marked in certain cases of reactions to novel situations of a more distinctly social type than the puzzle experiments.

In the "self-attentive" attitude the attention, as is suggested by the name, is not on the problem but on the self. The self is felt to be on trial. "What sort of a self shall I and others consider myself to be?" is the question which occupies attention, and this is usually accompanied by a state of worry, of emotional tension, which still further distracts from the problem in hand.

These two attitudes are closely related in that both arise from a lack of confidence in the self, a lack of a self-confidence which would permit the attention to go to the problem in hand. They are especially likely to arise in face of a novel situation, one concerning which the subject thinks himself to be ignorant or inept. The puzzles presented a novel situation in this sense, and the occurrence of these attitudes was especially prominent in the first attempts of a subject with his first puzzle. In some cases the first success brought a complete reversal of attitude in dealing with later puzzles even in cases where there were few elements in common between the puzzles concerned.

(3) Assumptions. The variability of the subjects was affected not merely by their physical condition and emotional attitude, but also by the general assumptions which they made and more or less explicitly held in mind concerning the nature of the special problem in hand. These assumptions often, apparently, were set up accidentally and became thoroughly entrenched without being subjected to criticism. The first glance at a puzzle seemed in many cases to

suggest a particular way of stating the problem or of defining the place or type of solution without there being any active search for other ways of looking at the matter or any criticism of the way accepted. The assumption thus uncritically set up in some cases limited the movements made by the subject to a certain portion of the puzzle, and consequently, in some instances, rendered the solution impossible.

This negating effect of a given assumption was qualitative as well as spatial. The essential parts of the apparatus might be selected but with the wrong assumption. This assumption, then, either limited the motor variations or made the subject insensitive to variations not in line with the assumption in question. In some cases the appropriate variation would take place many times but would pass unnoticed. To the observer it would seem that the subject must have a negative hallucination in regard to the variation in question.

The assumptions often had an inertia, an apparently volitional persistency about them, which seemed inversely proportional to the amount of critical consideration accorded to them.

These fixed assumptions were broken up in several ways. In some cases the puzzle was solved almost immediately on coming back to it later in the day. The particular set of consciousness had been broken up by this change and new points of view were possible. There were a number of rather striking cases of a similar sort on first awaking in the morning. There seemed introspectively to be here not merely an increase in general variability due to physical freshness, but also a shifting of values and probabilities, which led to new lines of attack. At other times the change of point of view was more purposive. Random manipulation was occasionally adopted by the subjects with the hope that either the solution might be reached accidentally or that some novel position of the puzzle might suggest an opening. More sophisticated attempts were at times made, especially by those who had had training in logic and scientific methods. In these cases the assumption would be analyzed out, criticized, and modified or set aside while an attempt was made to get other assumptions, and even to exhaust the possibilities in the way of assumptions and then to take up the problem of their relative evaluation. The bare logical contradictory proved its value in stimulating this search for a concrete other.

The purposive employment of this technique of explicit consciousness and definition of assumptions, of search for new points of view, of rapid but tentative evaluation of assumptions and the thorough testing of the one chosen as most probable, with accompanying avoidance of mere repetition, seems to have been a very valuable means of furthering efficiency in the solution of the problem. It was, of course, limited in effectiveness by differences in variability native to the individual or due to age, physical condition, presence of related habits and ideas, attitude, and the thousand and one factors whose happy balance is essential to maximum efficiency.

The shifting of assumptions whether occurring by accident or purpose often resulted directly in the solution. There were several instances of this sudden success after the subject had spent three or four hours in working under a given assumption and had become hopeless of the outcome and thoroughly disgusted with himself. In some of these cases the shift of assumptions was due to instructions given by the operator to the subject to critically define the assumption under which he was working, to seek out other assumptions, and to test them either in turn or in accordance with their probability.

In connection with the attempt to change assumptions there appeared at times an interesting shift in the accompanying feelings of probability. In one case of this sort the subject reported three distinct stages: (1) Hypothesis "A" looked impossible and "B" perfectly convincing, (2) "A" and "B" appeared of the same degree of probability, (3) "A" appeared convincing and "B" impossible.

3. Transfer

The term, transfer, is used rather broadly to include both the specific and general effects of a given experience on succeeding experiences.

a. Specific Motor Habits.—(1) A given subject was tested with a puzzle thrown in chance positions. He was then trained to approximately the physiological limit in handling four special but important positions. He developed no general rule to include his treatment of these special positions. He was then retested with the puzzle in chance positions. Another subject was trained entirely with chance positions, in a series approximately half the length of the first subject's series. The second tests of the first subject showed no improvement over the initial results and were inferior to those of the second subject. This failure to profit by the highly specialized training seems to have been due to the lack of a generalized rule of procedure. As it was, each chance position was first reduced to one of the four special positions and then the solution was proceeded with instead of being performed directly.

(2) A certain puzzle was so arranged that it could be presented in various foims. The manipulations for these various forms could all be comprised under a single formula. This general formula

could be deduced from any one of these special forms. A number of subjects were tried with this puzzle. As soon as skill was acquired in dealing with one form of the puzzle it was changed to another form. The subjects who developed the general formula during the solution of the first form were able to use the specialized habits built up in the first form in the second. Those who formed merely the special habits without developing the principle attempted to carry over the habits without modification and were greatly embarrassed by the change.

(3) A subject was tested with a puzzle in a given form. Then all the motor habits necessary for the rapid solution of this form were built up by practise on the separate acts of manipulation involved. The elements were organically related in the successive forms of the practise series, so that the practise was not on the separate elements merely but on their connections. At the close of the practise series the subject was given the complete form, which was identical with that of the initial test. This form was not recognized as being related to the practise series, and the habits built up there were not brought into use.

In general, the value of specific habits under a change of conditions depended directly on the presence of a general idea which would serve for their control.

b. Concrete Imagery.—The mere presence of imagery, although vivid and of closely related puzzles, was no guarantee of its efficiency. Very often attention rested on some superficial point of similarity and progress toward solution seemed to be delayed instead of hastened. The value of the image as well as of the motor habit depended on the precision of the analysis.

c. Attitudes and Attention.—As has been previously stated, the first success often brought a complete change of attitude toward the puzzles. This transfer or extension of mood seemed at times to be almost reflexly accomplished, so direct did it appear. A change in the subject's idea of himself, from that of one incapable of solving such a problem to one capable of doing so, probably played a part in the change of mood. A similar but less decided change of mood was at times accomplished, in the absence of success, by the suggestion that the subject was doing as well as others. An attitude of self-confidence was at times self-induced through an idea of its value, and subjects were able by this means to avoid a state of confusion when in difficulty, to which state they had previously fallen victims.

No evidence was secured in favor of an automatic change in level of attention, but there were indications of its indirect control by means of ideals of what constituted an efficient state of attention.

d. Ideals of Method.—The great significance of ideals of method has perhaps been sufficiently emphasized. This significance was especially striking in proportion as the situation in question was distinctly novel. The idea of efficiency as a goal to be reached, the ideals of scientific method, and the ideal of an optimum personal attitude were among the most important of these.

4. Memory

The most striking point in regard to memory was its relation to continued analysis. Memory cues were promptly substituted for continued perception of relations. This meant, where successfully carried out, a great saving in time, but very often the attempt was premature. There were illusions of memory with resulting errors and perhaps failure after initial success, and there was memorizing of irrelevant features along with the significant ones. In some cases the conviction as to the correctness of an erroneous memory was so great as to prevent further analysis and to keep the subject indefinitely at a fruitless line of attack. It was easier to rely upon memory than to analyze the relations out afresh, and this may have contributed something to its volitional character. In general, the subjects succeeded best who held their memories flexibly as hypotheses subject to rejection or revision as the case might be.

5. Plateaus and Hierarchies

The term plateau is here used in a broad sense to include periods of little improvement whether of long or short duration. Plateaus in which the times were fluctuating but remained at a high level occurred where there was a shifting back and forth between rival methods, more or less consciously employed, or where some feature remained intractable to control. Plateaus of the uniform type occurred where a single method had become well established which was not the most efficient one for the situation.

As has been stated earlier, the drops in the curve depended very largely on variations in method and their conscious use as hypotheses. Where the puzzle involved repetitive elements combined in complex ways, a most valuable variation was the getting of some mental grasp of the processes as a unity. Various forms of unity have been discussed under section 1, Extent of Analysis. In general, ability to anticipate the succeeding steps was most important for the process of short-circuiting, and where this process was total in character, in the way of a unitary view, the development of higher units of action was most rapidly attained.

CHAPTER III

The Solution of Problems

1. General Nature of the Processes of Solution

a. Accident and Analysis.—The solutions of the major puzzle-problem, on the one hand, and of the minor problems of manipulation concerned with the acquisition of skill, on the other, were of the most varied types as far as the relation of analysis to success is concerned. In general, the solutions were not the result of mere straightaway thinking and the consequent formulation of a thoroughgoing plan of action, but were the outcome of an extremely complex interrelation of more or less random impulses and of ideas.

In a number of cases the solution came entirely by accident as far as the essential movement was concerned, but, even then, there was usually some awareness of the general position just before or just after the successful movement. Thus, in one case of a purely accidental success a vivid visual impression of the final position was obtained, and the solution later flashed upon the subject by means of an involuntary analysis of the resulting image. In most cases of accidental success, however, instead of a precise analysis there resulted at first merely a limitation of the problem center to the general region or kind of movement connected with the solution.

In a contrasted type of cases anticipatory analysis played the leading rôle of determining the main line of solution and accidental impulses were effective merely in the detailed processes.

(1) A number of illustrations of the different ways in which the successes were actually achieved will next be given. These are grouped into four grades or types distinguished according to the relative dominance and temporal priority of accident or of analysis.

(*a*) In the following cases there was no element of anticipation of the result connected with the successful movement:

"The final solution was a sheer accident. I was just starting to put the parts back in the original position in order to begin a new solution, when I thought I saw my way through."—*Bg* (Semicircle and Ring Puzzle).

"I have no idea in the world how I did it. I remember moving the loop of the heart around the end of the bar, and the two pieces suddenly came apart."—*Mt* (Heart and Bow Puzzle).

LIST OF PUZZLES: KEY TO PLATE I

1, Fan Wire; 2, Bicycle; 3, Semicircle and Ring; 4, Heart and Bow; 5, Hook and Eye; 6, Two Face.

7, Maze Wire; 8, Sliding Triangles; 9, Hinged Loop; 10, Hinged Dart; 11, Star and Crescent; 12, Hinged Rectangle; 13, Cross and Ring.

14, Double Hinged Dart; 15, Triple Horseshoe; 16, Triple Ring; 17, Hinged Dumbbell.

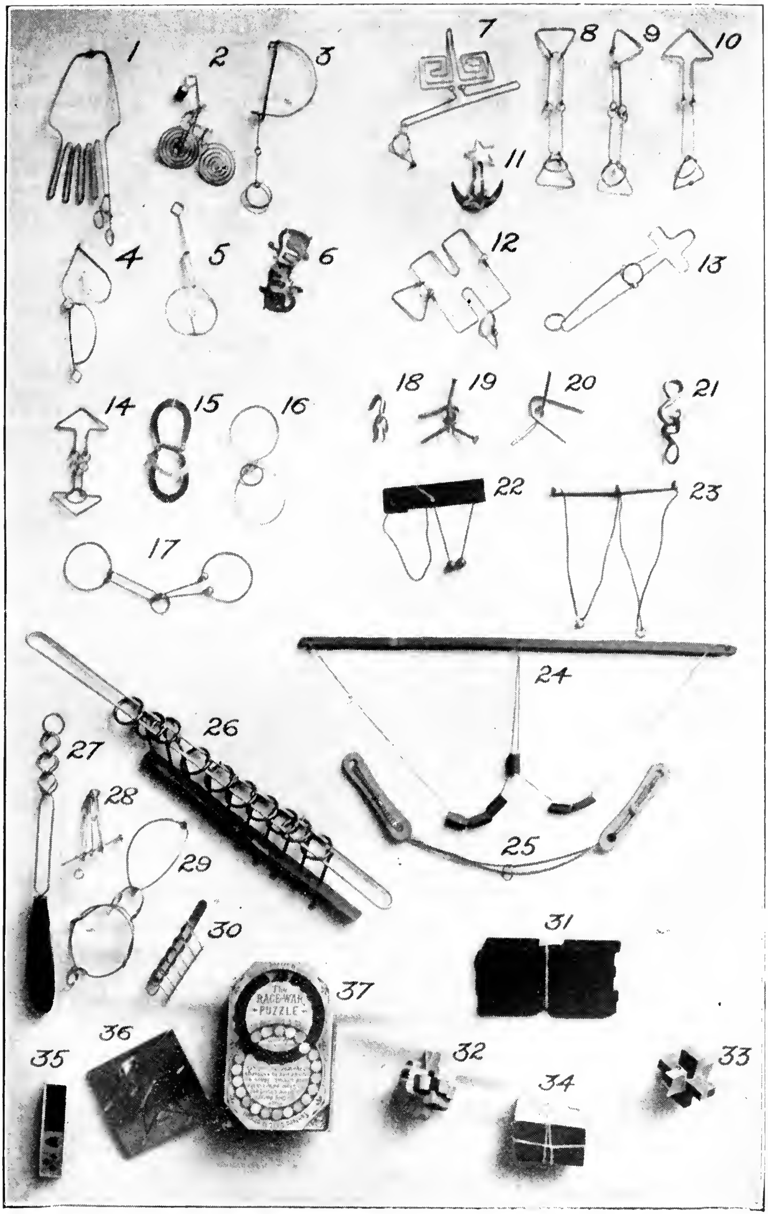
18, Jiujitsu; 19, Twisted Nails; 20, Twisted Wire; 21, Twisted Anchors.

22, Fighting Pig; 23, Fighting Pig; 24, Fighting Pig; 25, Chain and Ring.

26, Chinese Ring 10; 27, Mounted Wire Loop; 28, Chinese Ring; 29, Double Circle and Semicircle; 30, Chinese Ring 6.

31, Six-piece Cross Long; 32, Twelve-piece Cross; 33, Wizard Cross; 34, See Dar.

35, Katzenjammer; 36, Lone Star War; 37, Race War.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The
RACE-WAR
PUZZLE

PLATE I

"I tried random fumbling for several minutes purposely to see if anything would turn up. . . . I was only inattentively aware of what I was doing and did not plan it out. Was shocked with surprise when the rider came off."—*Rr* (Fan Wire Puzzle).

(*b*) In the cases to be given next the successful movement was entered upon accidentally, but was completed purposely and with more or less definite anticipation of the result.

" . . . So I shook the thing into the original position, and worked with it again aimlessly. The first thing I knew, the pointed ends were through the rings, and by working at this position in a manner hard to describe, I got the two apart."—*Wh* (Twisted Nails).

"I got it off in a way I had decided I couldn't. I saw a little way ahead that it would come off."—*Ta* (Wire Maze).

"The impulse to swing up the central loop came with a rush. It was followed by a partial perceptual cognition of its correctness." —*Rr* (Wire Maze).

"I tried this, at first idly, but then, as I progressed, I had a dim idea that I was doing something, and gave careful attention. As I did this, I saw that I had not merely made a difference, but had entirely freed the end of the chain which I had used as a loop, and that therefore I could entirely free the chain from the stick. I saw this a little before I came to it, but not when I started the movement of the loop through the hole, nor even when I passed the rest of the thing through the loop."—*Wh* (Chain and Ring).

"It seemed convenient to *keep* hold of the ring after I had once started it."—*Be* (Hinged Rectangle).

(*c*) In the following group the general locus of the essential movement or some detail of it was determined upon in advance of the successful manipulation but the solution was not worked out in detail and "accident" entered into the final process:

"Then a conspicuous part of one section caught the eye and from that time it seemed certain that it must have something to do with the solution. Work was kept on about that point. The final solution had some element of chance, I do not know just how I did it."—*Rd* (Heart and Bow).

"I tried bending the hinged circle and pushing it through the loops. This came as a perceptual impulse, a sudden glimpse of relations not fully worked out, but fairly clear for the first step."—*Rr* (Double Circle and Semicircle).

In the following transitional case the "accidental" impulse following on the preliminary analysis was caught in passage and was terminated purposely:

"The result followed a deliberate attempt to attack the loop end after recognizing the ring end to be impossible. The last step in the solution was a perceptual impulse with a flash of recognition of its similarity to a movement in a similar puzzle."—*Rr* (Semicircle and Chain).

(*d*) In the following cases the anticipatory analysis was much more complete:

"I knew how to do it as soon as I saw it. A visual image of the corresponding part of the Heart and Bow came on first sight."—*Ta* (Hook and Eye).

"Considerable uncontrolled manipulation was indulged in, but the ring was not gotten off in such manipulation. The random movement seems, however, in some way to have suggested a new way of looking at the puzzle, and the latter ended in its solution. The manipulation resulting in success was here the testing of a definite hypothesis."—*Wh* (Wire Maze).

"I began with rather aimless placing of pegs in order to get the hang of the moves . . . made several trials with a little analysis preceding each, but with no attempt at a complete analysis. Each time I would find myself brought to a stop without finishing and would have to analyze. Finally I analyzed the whole thing out beginning at the end, as follows: Since the hole marked 'Havana' is to be left open till the last (according to directions), the two holes distant two places from this can be left till the next to the last; the two holes distant two places from these come next in order. I followed back in this way till I saw which were the first that must be filled. My analysis worked promptly."—*Wh* (Lone Star War).

The cases given above were selected from a large number in the most of which the relations are much more complex. In the cases given as illustrating accidental solutions, various unsuccessful attempts to think the thing out or to follow some apparently promising suggestion had been made previous to the successful impulse. As has been previously stated, in most of the cases of accidental success the subject noticed the final position of the parts just after the solution, and so was in a position to eliminate a lot of movements concerned with other portions of the puzzle.

(2) Difficulty of Anticipatory Analysis. In the cases given as illustrating the anticipatory type of solution, it will be noticed that in the second and third the development of a plan of procedure followed upon a period of more or less random manipulation. The first illustration is that of an immediate and total solution. But this subject had already solved a puzzle very similar to the one in question. No cases were found in which a really novel puzzle was

"seen through" at once. One of the subjects was given the task of solving a fairly difficult puzzle without touching it. Three hours were spent in consideration of the puzzle before the subject felt sure that he had solved it. Despite the improbability of the references to the parts of the puzzle being intelligible to the reader a quotation or two from the subject's notes may throw some light on the difficulties involved. "I guess this last was wrong, for the folding ring would not drop from the staple to the split ring, as I had thought; and as it does in the case of the small staple, where it falls across without trouble upon the larger staple. But does it do this? It is hard for me to be sure. Simple as the question seems, I can not easily, without trial, make out the answer. But if I imagine a solid ring in the position into which I have supposed the folding ring to 'fall,' I see that it could not be got back into original position of the folding ring, and thence I conclude that I was mistaken in supposing that the folding ring would simply fall over on the large staple. . . . If I could see how to put the folding ring—supposed to be entirely off—on to the larger staple, I should have it. . . . I am anxious to put my plan to the test. I believe that I can see through it all right."

The subject then began the actual manipulation to test the theories worked out. "Immediately I met a difficulty, and see that I shall have to analyze further. The difficulty consists in the small ring getting in the way. . . . Got through the previous difficulty all right, but ran across another."—*Wh* (Double Circle and Semicircle).

The difficulties in picturing transformations in three dimensions are so great that few of the subjects tried with any great persistence to think the thing clear through in advance. Then too the impulse to manipulate seemed to be very strong, and this served to inhibit prolonged anticipatory analysis. "I started with the purpose of working by plan only, to solve it with anticipatory analysis, but got too impatient to see the parts in other positions to really test it."—*Ta* (Hinged Rectangle).

Even after a long practise series in which the puzzle had been successfully manipulated and the act reduced to one of skill, the subject was in some cases without any definite geometrical conceptions of the actual transformations. The necessary cues for movement had been learned in their proper order, but a unified conception had not been obtained. In one case of this sort—another subject working on the "folding ring" puzzle mentioned two paragraphs above—a low grade unity was obtained by conceiving the process as one of rhythmic reversal. This conception of the process came as a distinct experience and proved itself of value in the acquisition of control of the part movements involved, but it was felt to be a rather superficial conception.

Although a number of the puzzles had familiar geometrical forms, such as circles, pentagons, triangles, etc., geometrical concepts of the usual school type did not seem to play a very large part in the actual solutions. The problem in most of the cases required more or less complicated transformations in three dimensions, and the mental construction of these transformations constituted a difficult task and one for which the more statical type of training received in the study of geometry seemed to furnish but little assistance. The experiences of everyday life with extensible objects came in a number of times. "If it were a rubber band and one end were held on the split ring just inside the loop of the large staple, the other end could be so moved as to compass at once the end of the staple and the end of the split ring . . . ." A few puzzles in two dimensions were tried where the problem was the construction of a figure. Part of the problem was to determine the sort of figure to be constructed. "I hit on the idea of a square from the result of one of the tentative combinations. This seemed a likely enough goal for the puzzle and I could approach the problem methodically, namely by making a table of the relative sides and areas, and seeing if the total area was such that it would be a perfect square of any side that could be got from combining the sides of the pieces. . . . The total area would thus be that of the side of a square with its side the longest side of the large triangle. I considered the other combinations of sides which would add up to 4, and experimented a little quickly getting the square." The two-dimensional problem was more susceptible of attack by geometrical methods than were the tridimensional.

There was no question that the puzzles demanding transformations in three dimensions constituted a novel problem and called forth untrained functions. That was suspected and was the reason for the selection of the material, but the extent to which it proved true was, nevertheless, surprising.

Is there a single function for "transformations in three dimensions" or are there numerous special functions? No experiments were definitely planned to test this point. It was noticed, however, that the ability to see the solution of a puzzle already known was affected by changing its position relative to the observer. The ability to manipulate it was, likewise, affected unfavorably, and the ability to take a puzzle apart could be highly perfected without that being true of the ability to reverse the process.

b. Significance of Scientific Methods.—Ideas of general methods of procedure seemed to be more important in the attacking of new problems of the tridimensional sort than were the ideas of geometry. There was considerable difference among the various subjects, as to the degree to which variations were set up as explicit hypotheses to be definitely tested, as to flexibility of fundamental assumptions concerning the problem and as to the judgment used in their formulation, as to the employment of system and classification, and the developing of generalizations.

(1) Generalization. The value of explicit analysis and generalization based upon it is shown in the following case. In the Chinese-Ring puzzle there are numerous rings to be dropped through a loop in a rather intricate way. One of the subjects found that he could get certain rings off and not the others. Then it flashed across his mind that there was no apparent fundamental difference in the relation of the rings, and that if he analyzed a single case, a general rule could probably be developed. This was done and the puzzle solved. The subjects who did not develop an explicit rule in this case got into great difficulty when the number of rings was changed. Another subject not only analyzed out the general rule of procedure mentioned, in the first trial, but also developed a corollary from it as to the turning points, which saved him an immense amount of time and error as compared with the subjects who did not develop this at all or developed it so late in the series that erroneous methods of procedure had become well established.

(2) Grasping the Problem. There were great differences as to the readiness and precision with which the concrete problem was formulated. With the wire puzzles the subjects were simply given the general instruction: "Some part of the puzzle is to be removed." They were to "size up" the situation and form their own judgment as to the special problem in each case. In the Star and Crescent puzzle there is an interlinked star and an encircling star. The interlinked star is interlocked with an ellipse hinged in the center. The relation is that of two links in a chain, one link being jointed. The fact of one link being jointed does not change or affect the relation of interlinkage. The encircling star is the only thing that can be removed. One subject worked at this puzzle for 33,060 seconds—nearly ten hours altogether on five different days, and failed. At the close of the attempt he was asked to state what he was trying to do. He said that he was "trying to get the interlinked star off first and then the encircling star would come off of itself." Even here there was some elimination, since the subject said that it was impossible to separate the two parts of the hinged link, and that the solution was to be obtained by passing the star by some means over the hinge. In

the case of another subject in the course of 300 seconds all the essential points in the solution of the same puzzle had been determined. " Star ' 2,' the encircling star, is the one to come off. Neither star will go through the other, therefore the encircling star *must* go over the crescent. The movable star *must* go down in the middle of that thing (the hinged link)."—*Br* (Double Star and Crescent).

(3) Classification and Elimination. In some cases the classification and methods of elimination used constituted the essential element in the solution, and were rather elaborate in character. With the construction puzzles where there were a good many pieces to be fitted into each other to produce a given figure—as a tridimensional cross—the number of possible combinations was exceedingly large. Of two subjects, both began with the classification of the pieces, as to symmetry of the notches, etc., but one developed a much superior method of elimination of possible combinations to that of the other. An extract from the record of the more successful subject will be given, and, although the reader may not be able to picture the transformations described, yet the method may become obvious. "Six pieces! No doubt it is a triaxial construction like the other one. [Subject here classified the pieces.] None have grooves on opposite faces. Does this mean that the pairs must face each other, *i. e.,* have grooves toward each other, as in the other puzzle? . . . Which are the pairs? . . . The pieces can be arranged in order according to the amount cut out, and perhaps this gives the clue to the order in which they have to be put in. For if, as is certain, the piece with the least cut out has to be the last to go in, may it not be that the piece with the most cut out has to be the first, because it will allow the most to be put in after it itself is in position? I suppose that it is not, however, the amount cut out of a single piece but of the pair that counts. . . . Any pair that you choose must be either mates or at right angles. This ought to be of some help for if this is settled for only two bars, the field of experimentation would be narrowed. The two that are alike can not form a pair, for. . . . Therefore the two that are alike must cross at right angles. Found a third which can not be a pair of either of the like ones, and thus had one of each pair. Now sought among the remaining two (excluding the plain bar) for one which would be the mate of one of the like ones. This was not such plain sailing, as the 'judgment' was involved. Judged largely by the space left. One of the selections promised well, and, following it up, I reached the conclusion." *Wh* (Six-piece Cross).

Elimination by Means of the Dilemma. In the case of another puzzle where the number of different possible combinations is 1,296, and only one is the correct one, the method of elimination becomes crucial. For unless a person adopted a rigid method of recording what he actually did each time and did something different from what had been done at any previous trial, he would be likely to repeat the same fruitless combinations indefinitely. Of two subjects who tried this puzzle, both classified the elements and each tried at first an apparently promising but really fruitless method of combination. Then in the case of one subject the idea of exhausting the possibilities by means of a dilemma, with elimination at each division, occurred together with a happy insight as to starting point. The other subject failed to solve. In the case of this particular problem, the getting of a particular combination of figures on the faces of four cubes, an algebraical or "logical" type of analysis was much more applicable than in the case of the wire puzzles. Some transformations in three dimensions had to be imagined, but, in the main, substitutions could be made for this in terms of symbols which could be treated in a highly analytical fashion.

(4) Flexibility and Explicitness of Assumptions. One important difference in methods of solution was to be found in the flexibility of the assumptions employed as well as in the explicitness with which these assumptions were conceived. It had apparently never occurred to the subject who worked ten hours on the Star and Crescent puzzle to question his assumption that the interlocked star was the one to come off. That assumption suggested itself to him in some more or less accidental way, and was not itself made an object of criticism. The proper method of manipulation in its initial stages occurred accidentally a great many times in his work, but he seemed blind to all such possibilities.

(*a*) Uncritical Acceptance of Similarity Suggestion. Similarity to another puzzle in some cases acted in this negative way, the similarity being in regard to some non-essential feature, and being accepted uncritically. A number of puzzles were similar in having a ring which could be removed from a triangle, around whose point it hung, by folding the figure, leaving the triangle at the hinge, and moving the ring around the figure. The subjects solved the later specimens of this class very readily. Then a puzzle which folded and had a ring on a movable triangle, as in the other cases, but which differed from the others in that the triangle was to be left at the starting point and not brought to the hinge, was given to the subjects. They brought the triangle and ring to the hinge and attempted to leave the latter there. This made the solution impossible, but the attempt was persisted in and chance opportunities for the correct solution went unnoticed. One of the subjects said after success came: "I thought of the other hinge puzzles as soon as I saw the hinge and thought that this would have to work in the same way." He spoke of a feeling of compulsion, that "the triangle just had to go to the hinge."—*Ta* (Wire Maze).

Another subject, after many attempts to solve in the accustomed way of the other similar puzzles, brought himself to question his method of attack. "Maybe I am working on the wrong line altogether. . . . Suppose there were no triangle, how would it come off? The rider is the only thing that holds it on. Hence, if I get rid of the rider in the right way, it will come off. . . . I wonder how it would do to leave the triangle at the starting point." The suggestion as to leaving the rider at the starting point was the correct one. It followed this conscious attempt to change his assumptions.—*Tz* (Wire Maze).

The Triple Horseshoe puzzle was given to another subject after he had solved the Star and Crescent. The Triple Horseshoe is similar in many respects to the Crescent puzzle, but there is an essential point of difference. This subject not merely noticed the similarity at once, but questioned it instead of blindly accepting it: "This reminds me of the Star and Crescent as far as the folding, but perhaps you *don't do it* that way. The problem is to get the small horseshoe off. You naturally can't pull it over the large one. There must be some other way of getting it, some feature here is quite different from the Star and Crescent. Let's take an entirely different start." The correct variation followed almost at once on this conscious attempt to get a change of attitude.—*Br.*

(*b*) Volitional Character of Assumptions. In some of the subjects the sticking to an assumption once started upon had a decidedly volitional character. The assumption had almost the force of a "fixed idea." Aside from the specific determination to stick it out on the line started, there is the strong influence of mental inertia, the dread of changing. The latter tendency comes out with especial strength in the case of the introduction of possible improvements in manipulation after a certain degree of success has been obtained. One dislikes to change even though he may realize that his present method may not be theoretically the best. He dreads to give up something that he feels confident of for something in which there is a possibility of not panning out. It is rather strange that a similar tendency should be found in the case of attempts that are not successful, in the first solution of a problem. The mental attitude becomes fixed in a more or less chance way, and there is hesitancy about changing even when the present method does not bring progress.

(*c*) Forced Change of Assumptions. The subjects were thrown entirely upon their own resources as far as the solution of the problems were concerned, except in a few cases where the experimenter wished to study the matter of assumptions. In the case of the Hinged Rectangle puzzle one subject worked 3,600 seconds chiefly at one given point in the figure. He was then asked to state his assumptions. He did so, stating that he was trying to pass the ring over the end of the central loop and so off. (The ring came off, not there, however, but at a hinge on the side of the figure.) He was asked to try a change of assumptions. After 480 seconds more he was asked what he was doing. He replied, rather sheepishly, that he was still working along the same line. He was then asked to state what other possible ways of solution there might be. He mentioned the correct one, "Well, it is just possible that the ring could come off at the hinges." He could not seem to bring himself, however, to test the second hypothesis in any thoroughgoing way. —*We.*

(*d*) Dilemma and Discovery: the dilemma as a device for securing flexibility, explicitness and increased variability. The subject mentioned just above was tried with another puzzle—a fairly difficult one. After he had worked for awhile along an impossible line and with no signs of changing he was asked to state his assumption as to the general plan of attack. He did so. He was then asked to state another. He did that. He was then required to work alternately for 120 seconds on each assumption, and within this period assigned for each of the main assumptions he was to determine explicitly what minor assumptions were possible. With this rather mechanical method of procedure, the solution was soon obtained. The operator did not tell the subject what the other possibility was, but forced him to look for another. In this case, the divisions were made exhaustively, *i. e.,* working at one side of a given point on the figure or on the other, and within a given part of that side of the figure to attack this promising point or some other. The some other was a logical contradictory, a mere non-A, in some cases, but it stood for the fact that there might be some other concrete possibility besides the one chosen. Another subject, who had previously failed with the same puzzle, was tried with it again, and forced to dichotomize from the most general assumption down. The dichotomy consisted of a given assumption and its contradictory, but where a fruitful concrete rival hypothesis was developed *that* was included specifically within the contradictory, the mere something different. In this case the subject was not asked to alternate, but was asked to carry his divisions down till they included the final movement. He did this, and then was asked to choose which major assumption and which minor under it he wished to test. He was able to eliminate some possibilities at once and choose the correct combination. He was then given the puzzle and solved it at once. In his previous attempt he had worked on the unfruitful hypothesis. Of course there is no mechanical way for the production of insights, but the conscious attempt to get into a different attitude, to realize that there may be other possibilities and to search for them, may be effective as a stimulus. In this case the happy insight followed at once on the demand for a rival assumption. The subject exclaimed suddenly: "Think I've an idea."—*Wh* (Bicycle puzzle).

The method of constant dichotomizing if carried to the bitter end might very well sidetrack a person from reaching the solution in the most direct manner, it might prevent his following a fruitful suggestion without delay and with confidence that manipulation or new suggestion would in some way bring the total solution. A fairly good chess player was asked to solve the Heart and Bow puzzle without touching it and with the method of exhaustion applied not merely to the major element in the solution, but to the details of manipulation at each step. As stated before, the thinking out of transformations in three dimensions with any precision is extremely difficult—at least it was for all persons tested—and in this case the subject found it impossible to carry some of the minor transformations through so as to decide definitely between rival hypotheses in advance of testing them. He outlined the correct line of attack, locating the critical point in the solution, but could not carry the processes through so as to be able to say precisely how he would get to that point. Consequently in the 5,000 seconds spent in this way a very important point in the manipulation at the "critical" part was not determined. The solution to that came the morning after as he was thinking the experience over.—*Ft* (Heart and Bow puzzle).

The subject stated that he thought the necessity for working out all the details in the exhaustive fashion required had impeded him in working out the essential point. The purpose of the experimenter in this case was that of testing not only how far the general solution could be attained in this way, but also the extent to which the entire technique of manipulation could be worked out in this completely anticipatory manner. The case serves probably, however, as an illustration of the difficulties in carrying over the algebraical type of solution to cases where the perceptual and image functions are essential but undeveloped. The total time for solution in this case was 10 times that of the average of the subjects who began manipulating at once, and 100 times that of the subject who analyzed the central element and began attack there. There were two cases, out of the twenty-five solutions of this puzzle by different people, where the time with manipulation exceeded the time of the subject who did not manipulate at all. In one of these the time required was 13,532 seconds! This was a case, on the other hand, of fixed assumptions and variability limited by them.

(5) Summary. General scientific method in the sense of rendering hypotheses explicit and at the same time keeping them flexible, of the active search for new points of view, of employing some form of registration so as to avoid mere repetition, of the rapid evaluation of different possibilities and of testing the ones selected, is not a substitute for the development of the novel type of perceptions and insights required in the experiments under consideration, but it seems to have shown itself to be an important means of stimulation and control.

2. The Process of Analysis as Experience

The process of analysis, of "seeing through a thing," is a very distinct experience. In many cases it came as an extremely sudden transformation, a "flash" experience. The "rush" of a new impulse and the "flash" of cognition of its meaning are both classed in this discussion, rather naïvely perhaps, as variations. The degree of novelty in the variations of course varied widely. The variation in some cases was recognized as a revival of an earlier impulse or "idea," but now in an explicit instead of in an earlier indefinite form or in a form in some way especially adapted to the change of conditions. The element of novelty is just as striking as that of its revived character in the cases where the latter can be detected.

The process of analysis in the percept and in the image seem introspectively to the writer to be of the same sort. The process in the case of perception seems to be a change taking place immediately in the perceptual stuff, not the addition of imagery to it.[1]

3. Discrimination and Difficulty

A number of cases were noted where discrimination took place only as a result of actual difficulty. One subject was trained in the solution of a puzzle and then the parts were so changed that the appearance was very different, but the manipulation required was exactly the same. The change in appearance was not noticed by the subject.—*Wh* (Gem Fastener).

A similar experiment was tried, with the same subject, with two

[1] This result is exactly parallel to that obtained by Woodworth in the staircase figure. (*Journal of Philosophy*, etc., 1907, **4**, 170.)

variants of another puzzle which differed in the direction of certain wires, but were alike in manipulation; although it was suggested to the subject that there might be differences, none were noticed, until the puzzles were examined simultaneously and with the purpose of looking for differences. In the Hook and Eye puzzle a loop is to be moved around and into a reentrant ring. It can be moved in from one side but not from the other. Of nine subjects who solved the puzzle only two made the discrimination in advance of the actual difficulty. Of those two, one started to move in the wrong direction but retraced before getting into difficulty, and the other made the error as a result of inattention on the following trial.

An illustration from the records of one of the subjects concerning the discrimination in question will show the part that difficulty plays. The puzzle consists of a ring of wire with a reentrant end and a loop encircling the wire. The loop is removed by following the wire to the point where it becomes reentrant and then passing it over the reentrant end.

Trial 1: "I noticed the possibility of springing the ring apart inside of the square, and so thought of pushing the loop in there. I suspected rather than saw that this was the path to solution. Felt sure enough to start in that direction, and, as I proceeded, I saw into the last part of the process before I got to it."

Trial 2: "I tried first putting it around the wrong way, in the first trial I had not felt perfectly clear why I had put it one way rather than the other. Some mechanical resistance, not experienced in the preceding trial, made me suspect that I was wrong, and I looked more closely, but tried the other way and succeeded before clearly seeing why one way was necessary rather than the other, and saw that as the ring ended in the central prolongation it was necessary to push the rod off the end of the prolongation to get it off the ring."—*Wh* (Hook and Eye).

4. Discrimination and Imagery

This tridimensional transformation type of discrimination did not seem to have any direct relation to the extent of visual imagery as reported by the different subjects. The following table of subjects for whom the conditions were directly comparable will show the lack of correlation. The times for the first successes in the case of the Heart and Bow puzzle, first line, and the Star and Crescent, second line, should decrease from left to right if there were a direct relation between visual imagery and success, since the subjects are arranged in the order of their visual imagery from left to right, the subject at the left claiming to have no visual imagery whatever.

	Br	*Bg*	*Tz*	*Rd*	*Ta*	*Fe*
Heart and Bow	43	275	572	851	325	13,532
Star and Crescent	476	968	1,476	1,775	1,280	fail (1,660)

The relation indicated here would be that of a negative rather than of a positive correlation. It is not claimed that a negative correlation would hold if a larger number of cases were examined, but that visual imagery is not essential to the process. The subject who made the best record states that he is entirely without visual imagery. He is an instructor in logic and scientific methods and is without training in manual arts. Of the two subjects who reported the most vivid visual imagery one was very successful in dealing with the puzzles, especially in the detection of similarity of a given puzzle to earlier puzzles in the series, but the other subject required 400 times as much time to do the first puzzle and 200 times as long to solve a puzzle which was similar to it as did the first "visile." *Wh* seems to be practically destitute of visual imagery, but he solved fifty puzzles, some of them being done under difficult conditions, as with eyes closed, with his hands behind his back so that both lack of vision and unaccustomed tactual-motor conditions would prevail, and with vision but without the privilege of manipulating at all. His records were not included in the table just given as he performed the solutions in a different order from the other subjects. His records for these two puzzles were low, 54 seconds and 238 seconds, but this may have been due in part to transfer from earlier and similar puzzles.

As stated above, the writer finds the process of analysis, as far as his introspections have gone, to be of the same sort whether occurring in the field of perception or of imagery. He found great difficulty in the attempt to image the tridimensional transformations in advance of any movement, but, on the other hand, he found at certain stages a decided help in withdrawing the puzzle from sight or in closing his eyes and then attempting to work out the relations involved. The advantage in favor of the image seems to consist in the shearing off of irrelevant detail, and in the bringing of the total process within the time span. Since the tendencies to movement are more pronounced with perception than with ideation, the irrelevant detail mentioned is more likely, in the case of perception, to result in distracting movements. The writer's visual imagery is rather scanty in detail of objects, it comes part by part, and is poor in coloring; his "imagery" is shot through with motor tendencies. Since no motor trains were actually set going by the latter, they were not distracting, and they seemed to be of positive benefit in binding the different parts together. The building up of a unified image is, however, no easy matter for the writer. In the case of a complex puzzle with balls strung on loops of string the final unified image was obtained after a long process of painful effort. The different stages in the transformation were constructed bit by bit and with the help of verbal imagery, both to stimulate the process and to fixate what had been obtained. Finally all the different steps were put together and the total process was given verbally. After it had once been obtained the process could be reviewed much more rapidly and surely than with actual manipulation.

The subject who had complete and ready visual imagery and who also succeeded well was rather reluctant to state the process of solution verbally; the verbalization followed upon the development of the visual imagery. In the eighth trial of the Chinese Ring puzzle he said: "I think I can do it again, but I can not tell how. I believe I have a visual image of the different stages of the process, but I can't describe the separate steps." In the eleventh trial he gave a verbal account of the main process. The subject with "no visual imagery" described whatever he was doing while he was doing it.

5. Attitudes

a. The Self-attentive Attitude.—That the puzzle situation was decidedly of the novel type is shown not merely by the slight capacity of the subjects to construct mentally the required transformations but also by their emotional attitudes. The first impression was frequently that the puzzle was impossible of solution, and the subjects spoke of feeling "hopeless" or "helpless." There was a fairly constant undertone of questioning as to whether the subject was not really very much inferior to ordinary people, especially when things were slow in opening up. Some of the subjects expressed the fear that the experimenter must be getting bored or that he would think them extremely stupid. An extract from one of the subject's accounts will illustrate the attitude.

"It seemed to me that if anybody had given it to me without saying that it was a puzzle (a bona fide one) I would have said it was impossible up to the last minute. I have a feeling now of loss of esteem. I had this all along because I couldn't do something which was made for people with ordinary brains to do. One conclusion that kept running through my mind all the time was that I had a subordinary brain. I couldn't help having a gleeful, self-satisfied feeling when it actually seemed to be coming off, although it was a surprise."—*Tz* (Chinese Rings).

An illustration from the sixteenth trial in the first practise series for the same subject will show much the same mental state of affairs springing up after the puzzle had been pretty well learned, when, all of a sudden, a new position had to be coped with, the novel situation breaking in on a fairly well established adjustment. "The heart was not slipped over the end of the bar (it would then have been off) but was pushed back on the same side and instead of the heart falling free it was in an entirely new position. This had to be studied out before the heart could be freed. I felt chagrined and became nervous, feeling that the time was being wasted. I felt a dislike to waste more by studying anew the manner of releasing the heart from this new position. But I knew this was necessary, so I gave up almost wholly the idea of time and the *how* became uppermost. I became then less nervous."

This nervous self-consciousness seriously lowers efficiency both in the first solutions and in the process of skill acquisition. When the break occurs even late in the practise curve all the old errors, apparently outlived, crop out again with an almost fatal regularity and by their reappearance gain new strength.

The latter part of the last quotation illustrates the more or less complete change from this self-conscious attitude to one of absorption in the thing to be done. The same subject after solving his eleventh puzzle said: "I did not hurry, made up my mind not to get flustered—a different attitude toward the puzzles now than at first. I look at them now as I would a problem in mathematics, *i. e.,* I feel that I have some fundamental principles with which to attack them. I had made up my mind before not to get rattled but didn't hold to it. There seems to be a change."—*Tz* (Wire Maze).

This reversal of attitude was stimulated and brought about in various ways:

1. A chance success with the first puzzle in the case of one subject resulted in a complete change of attitude toward the remaining puzzles. The chief factor here seems to have been the removal of the conviction that the puzzle was impossible of solution. This conviction could not be overcome by the assurances of others; actual personal success, although a chance one, was necessary in this case.

2. The voluntary holding before the mind of the value of the objective attitude and the conscious attempt to get it seemed efficacious in many instances. This was true not merely in getting the first success with a given puzzle, but also, and strikingly so, in the acquisition of skill in manipulation.

3. After several puzzles had been successfully dealt with confidence was developed in a number of the subjects that the new puzzles

would not be wholly unlike the old. They felt that they had a body of rather closely related experience which could be used in attacking the new problems. At the beginning of the series the opposite conviction was noted, that the subject had nothing with which he could directly attack the problem.

4. Confidence was developed also in connection with the belief that the subject had some general methods with which to deal with novel situations. Under this head might be mentioned belief in the value of general logical and scientific methods, and the belief in the self as essentially variable provided favorable conditions were supplied. This last factor was noted only in the case of a few subjects.

Knowledge that another subject had succeeded where a given subject had failed in some cases seemed to stimulate the subject, to bring increased confidence, and in others to result in depression and an apparently lessened capacity to vary. In the latter cases the subject's attention seemed controlled by the idea that he was much inferior to others and in the former cases by the idea that it couldn't be so very difficult after all.

In concluding this section on the Self-conscious Attitude it may be stated that one of the most striking phenomena of the whole investigation was the large place in the consciousness of the subjects occupied by this idea of the self being on trial, usually with an accompanying apprehension that it was proving sadly deficient, and a correspondingly insistent demand for social appraisal. The experiments were not planned to encourage this socialized attention but rather to minimize it. There were almost constant indications of its presence, however, and occasionally it cropped out with a vigor which betrayed its latent strength. Thus, one sedate superintendent of schools, who was acting as a subject, suddenly broke out with, "You simply must tell me how I am getting along in comparison with the others."

b. The Suggestible Attitude.—In two of the subjects there seemed to be a special sensitiveness towards any movements of the operator which might give an indication as to the course to be pursued. In such cases as this there is a lack of confidence in the self but the attention is directed not to the self but to some other person. The center of gravity, if one may so describe it, of the responsibility is located elsewhere and the suggestions, intentional or unintentional, of the other person or persons concerned are accepted uncritically. This tendency was noted by the writer in his own case in novel situations of a more distinctly social type, such as business transactions of an unaccustomed sort, or other similar cases where persons instead of things were to be dealt with and where the other person was felt to have superior information as to the matter in hand and the self to be deficient.

c. The Problem Attitude.—In contradistinction from these two attitudes, which are certainly not favorable to efficiency, a third attitude, conducive to efficiency in dealing with novel situations, might be called the problem attitude. A tentative outline sketch of such an attitude will now be given. It would be an attitude of self-confidence as opposed to the self-distrust of the two preceding ones. The self-confidence would not be one of sluggish complacency however, but would be expressed in a high level of intellectual activity, of attention.

Attention would be directed to the thing to be done rather than to appraisal of the self. This does not mean that attention might not be directed to the analysis of some successful movement made by the self, whether in terms of resident or remote cues of action, or some general condition of effective response by the self, for in these cases the attention is on the problem of working out the mechanism of control.

There would be a freedom towards variations and a confidence that the organism would furnish them.

There would be a corresponding and compensating tendency toward critical evaluation of variations, a flexible holding of assumptions and suggestions and a rigid testing of them, ideational or manipulative, whether these suggestions emanated from the self or from a "prestige-person." The various factors entering into this attitude, self-confidence, high-level attention to the thing to be done, openness toward novel lines of attack, and critical evaluation of suggestions from whatever source, were noted in the puzzle series to be connected with efficient forms of response.

CHAPTER IV

Puzzle Material and Tests of Intelligence

As possible tests of comparative intelligence Cole[1] has used, on the basis of Kinnaman's[2] results and his own with animals working with puzzle boxes, the ratios of trials 1 to 2. The ratio for Kinnaman's monkeys was found to be 2:1 and for Cole's raccoons to be 3:2. The ratio in the case of human subjects working with puzzles of the physically analytical as opposed to the physically constructive type was found by the writer to be 7:1. This result was obtained by adding together the first times of all comparable solutions for thirteen different puzzles, there being seventy-three such first solutions in all, and finding the ratio of this sum to the sum of the times for the second trials concerned. The question as to the significance of this ratio will now be discussed, the value of certain other possible measures will be considered in the light of the results obtained with the puzzles, and certain general considerations bearing on the problem will be adduced. The use of ratios will be considered from the standpoint of measurement of individuals within a single group and from that of the comparison of different groups.

1. Intra-Group Measurement

a. Ratio of Trials 1 and 2.—If the individual ratios of trials 1 and 2 for the seventy-three cases mentioned be calculated, the time of each first trial being used as the numerator and its corresponding record for trial 2 being used as denominator of the ratio in question, the range of variability exhibited is striking. The maximum ratio thus found is 134:1 and the minimum ratio is inverse, 1:4.5. If the first and third trials instead of the first and second are used, the extreme instances are further apart, the upper ratio is now 200:1 and the lower is 1:13.4. The group, or inter-group, ratio would thus be decidedly misleading if taken without some measure of variability within the group. Incidentally the inverse ratios show that the process of learning manipulations by human beings is not always a case of getting a clear idea as a result of a single success with a consequent sudden drop in the curve, but is at times similar

[1] L. W. Cole, *Jour. Comp. Neur. and Psych.*, Vol. XVII., p. 211.

[2] A. J. Kinnaman, *Am. Jour. Psych.*, Vol. XIII., pp. 126–127.

to the lower types of animal curves in that the curve rises from the initial point, the later times being longer than the earlier.

Not merely, however, does the single group ratio fail to represent the variability of ratios within the group, but the individual ratios themselves, if taken without reference to the absolute times involved, give an inadequate and at times false impression of the sort of mental processes involved. Two subjects, *Fe* and *Br*, had not solved any puzzles previous to the puzzle series and the puzzle in question, the Heart and Bow, was the first one of the puzzle series. Their results would, therefore, be considered comparable. In the case of *Fe* the ratio of first and second trials, in time, is 38:1, while in the case of *Br* it is only 6:1. Apparently then, as far as this particular test is concerned, *Fe* showed the higher form of mental process. If we look, however, at the absolute times and the relation of these to the minimum handling time reached toward the end of the practise series, a very different conclusion is required.

The absolute times for trial 1 were,	*Fe*	13,532;	*Br*	43
The absolute times for trial 2 were,	*Fe*	351;	*Br*	7

The time of *Fe* for trial 1 is 315 times that of *Br*, and for trial 2 it is 50 times. The time of *Fe* for trial 2 is next to the longest record out of fifteen subjects and the time for *Br* is next to the shortest. The minimum time reached by any subject in the series of 50 trials was 1.7 seconds. Quotations from the subjects' accounts will show the difference in the kinds of analysis involved. In trial 1, after examining the end of the bar running across one side of the bow, *Br* said: "This wiggly thing must be for something or other." He then pushed the loop of the heart (the "wiggly thing") over the end of the bar, and so separated the parts, thereby solving the puzzle. He then said, "I saw it just before I did it; it was semi-planned." He had seized on the essential points involved and then solved with perceptual guidance without having worked out the details in anticipation. *Fe* exclaimed as the puzzle came apart, "How did I do it?"

In trial 2 *Br* said, "I just took the loop-business and put it through. The puzzle is just as clear as writing my name." In trial 3 *Fe* said, after it had come apart accidentally, "I did not get it with a purpose. I have a general notion of where to work." In the case of the latter subject there was accidental success followed by a mere "locus" analysis, while in the former case there was the analysis of the essential element. The "locus" analysis, however, effected the cutting off of a whole range of fruitless movements connected with other parts of the puzzle and so resulted in a tremendous drop in time.

b. Absolute Measures.—It would seem from the illustration just given, that ranking by absolute times would obviate the difficulty just raised as to possible misrepresentation of the kind of analysis involved. That this is not the case as applied to the absolute times for trial 1 will be seen from the following illustrations. In the case of the puzzle referred to above, the Heart and Bow, the time of one accidental first success was forty times that of another. While the times for accidental success might in a way measure the relative variability of the subjects and so be of value, it would be a very unprecise form of measurement, as it would be determined by the chance order as well as by the number of the variations.

The time for later trials is often greater than for trial 1. In one case of accidental first success with the Heart and Bow puzzle the time for trial 2 was three times that for trial 1, the times being respectively 575 seconds and 1,885 seconds. In another case, Star and Crescent puzzle, the time for trial 3 was thirteen times that of trial 1, the times being (1) 132 seconds, (2) 590 seconds, (3) 1,775 seconds.

A third illustration is given in the fact that an accidental solution is often shorter than one that is planned. The time for the accidental success mentioned in the previous sentence, 132 seconds, is less than one third the time for one success reached by analysis, 476 seconds, and less than one ninth the time of another, all for the same puzzle.

While the time for trial 1 might be thus largely affected by accident, the times for trials later than this would reveal the kind of analysis performed in trial 1 whether the success were accidental or planned. A successful variation may be unpremeditated and yet be caught on the wing by attentive analysis, and consequently result in a drop in time in the succeeding trial. Of course accidents may be repeated without analysis having taken place, but this seems to have been a rare occurrence in the puzzle series. As measures of the early part of the curve the following times have been used: for trial 2, for the median of trials 2–6, and for the lowest of trials 2–6. The following table presents these measures for nine subjects, whose results are comparable, for the Heart and Bow puzzle. The table includes also a grading as to the kind of analysis made during the first trial and as to the degree to which it was accidental, and, further, the times for trial 1, and the individual ratios of trials 1 and 2. The gradings as to analysis are based upon the subjects' own accounts and also upon the operator's records of their behavior. The letters under the caption "Anticipation" in the table refer to the degree to which the solution was a result of anticipatory analysis. The grade "A" means that the solution was accidental. The grade "C" means that a certain portion of the puzzle was selected as the central element, and that the solution followed on this focalization. Four grades were used in grading solutions as to anticipatory analysis, but only two grades were found with this puzzle. The numbers under the term "Analysis" stand for the degree to which the analysis was carried as a result of trial 1 and preparatory to trial 2. A very high percentage of the solutions is seen by the letters to have been accidental, viz., 6 out of 9. The subjects are arranged in the order of the times for trial 2. A second table presents the results of the preceding table in terms of relative rank only and includes in addition a ranking as to errors and a summation rank. The grade as to errors was determined by taking into account both the number of errors and the point in the curve where they disappeared. The lower numbers indicate that the errors were few and disappeared early. The summation rank is given in two forms, a total summation of all the columns, and a summation of all except for trial 1 and the ratios of 1 and 2.

TABLE I

	Ratio 1 and 2	Trial 1	Trial 2	Ratio of Trial 2 *Br* Standard	Median 2–6	Average 2–6	Smallest of 1–6	Anticipation	Analysis
Br	6.0:1	43	7.0	1.0	8.0	8.2	6.2	C	1
Ta	27.0:1	325	12.2	1.7	9.4	8.9	5.9	A	2
Rd	49.0:1	851	17.3	2.4	19.2	26.7	16.0	C	4
Bg	5.9:1	275	46.6	6.6	16.0	18.2	5.5	C	3
Tz	2.7:1	572	212.4	30.0	18.4	54.8	8.2	A	3.5
Mt	1.4:1	351	256.4	36.6	50.0	104.0	27.0	A	4
Ry	20.0:1	5,203	260.0	37.0	214.0	228.0	143.0	A	4
Fe	38.6:1	13,532	351.0	50.0	261.0	233.0	40.0	A	4
Po	0.3:1	575	1,885.0	269.0	45.0	404.0	18.0	A	4

Consideration of the tables will show that the grading by the medians of trials 2–6 agrees most closely with the grading as to kinds of analysis, as to errors made, and to the total rank. It will also show that grading as to times for trial 1 and as to ratios of trials 1 and 2 depart widely from the standards mentioned. Since the median agreed more closely with the above than did the times for trial 2 or for the averages of trials 2–6 or for the lowest of trials 1–6, and since it is more easily found than the average, it was adopted as a measure of the early part of the curve.

c. Ratios with a Common Numerator.—If ratios of trial 1 to some other measure of the early part of the curve be desired, the use of a

TABLE II

	Ratio 1 and 2	Trial 1	Trial 2	Median 2-6	Average 2-6	Smallest of 1-6	Analysis	Errors	Rank Columns 3-8	Total Rank
Br	5	1	1	1	1	3	1	1	1	1
Ta	3	3	2	2	2	1	2	2	2	2
Rd	1	7	3	5	4	5	5	5	5	4
Bg	6	2	4	3	3	2	3	4	3	3
Tz	7	5	5	4	5	4	4	3	4	5
Mt	8	4	6	7	6	7	5	7	6	6
Ry	4	8	7	8	7	9	5	9	8	8
Fe	2	9	8	9	8	8	5	8	9	8
Po	9	6	9	6	9	6	5	6	7	7

single numerical standard as numerator for all the fractions would have the advantage of giving the same rank as the absolute measures themselves and of furnishing further indications as to the sort of mental processes involved. The following table gives the ratios of the highest individual time for trial 1 to the medians of trials 2–6 for the same puzzle and subjects as the tables just preceding. The highest time was 13,532 seconds, *Fe*.

TABLE III

	Br	*Ta*	*Rd*	*Bg*	*Tz*	*Mt*	*Ry*	*Fe*	*Po*
Ratios of 13,532 seconds to times for medians ..	1,691	1,440	705	846	735	271	63	52	301
Individual ratios	5	35	45	17	31	7	24	52	13

It will be seen that, while *Fe*'s ratio is highest when the individual times for trial 1 are used as numerators, it is lowest if a constant numerator is employed.

d. Comparison of the Practise Curves as Wholes.—The measures so far presented concern chiefly the first few trials of the curve, and measure the readiness and completeness with which the main problem is solved. Success in handling the puzzles in minimum time depends, however, on the solution of minor problems of manipulation. Some convenient test is needed for the comparison of the curves as a whole, since it is at times not convenient to present the entire curves, and since it is often difficult to make the comparison even then. The following table is prepared by sampling the curves at various points by means of the medians of a few trials at those points. The columns present respectively the medians for trials 2–6, 6–10, 21–25 and 39–50, and a summation ranking for the whole curve. As compared with the previous tables this one is incomplete as it was not possible to secure fifty trials with all the subjects. The most marked case of change of rank as between this table and the previous ones is that of *Tz*, who here ranks number 1. The rise in rank in his case apparently resulted from an explicit analysis of the minor problems of manipulation.

TABLE IV

Medians:	2–6	6–10	21–25	39–50	Summation Rank
Br	1				
Ta	2	2	2	2	2
Rd	5	5	2	3	4
Bg	3	3	4	4	3
Tz	4	1	1	1	1
Mt	7	7	3		
Ry	8	8	6	5	5
Fe	9	6	5		
Po	6	4			

e. Perception of Similarity as a Test.—A test of the mastery of a given puzzle problem may be found not merely in the ways already indicated but also in the readiness with which a related problem is solved, and in the nature of the mental processes involved. Six of the nine subjects listed above were tried after several days' interval with a puzzle closely similar to the Heart and Bow. No suggestion was given by the operator as to there being any relation to a previous puzzle. The subjects were graded as to their standing in trials 1 and 2 in the new puzzle, the Hook and Eye. Their rank is as follows:

Br	*Ta*	*Rd*	*Bg*	*Tz*	*Fe*
3	1	5	4	2	6

The ranking agrees fairly well with the ranking gained by use of the practise curves of the Heart and Bow puzzle.

2. Inter-group Measurements

The comparison of groups by means of the ratios of the averages for the first and second trials would seem to be open to the same objections as the comparison of individual ratios within a given group when both numerator and denominator vary. Valid intra-group comparison seems to necessitate the use of a constant numerator and the same principles would apparently apply to inter-group comparisons. As compared with an animal group the different puzzles and subjects might be taken as a homogeneous group, and it was so considered in determining the group ratio of 7:1. But within this one group there was a wide variation between the ratios of the averages of trials 1 and 2 for the different puzzles, the ratios ranging from 1.5:1 to 40:1. The times for trials 1 and 2 respectively were averaged for four selected subjects for eight different puzzles. The ratios are given in the following table.

Heart and Bow	7 :1	Cross and Ring	10 :1
2 Star and Crescent	4 :1	Hinged Rectangle	11 :1
Chinese Rings (4)	2.5:1	Hook and Eye	2 :1
Chinese Rings (5)	3 :1	Semicircle and Ring	1 :1

The subjects were *Bg, Rd, Ta, Tz.* The puzzles are arranged in the time order of their performance. The higher ratios belong here to the puzzles in which analysis of the "locus" type played the largest part. The lowest ratios belong to the puzzles in which the transformations were especially difficult to construct, as the Chinese Ring and the Semicircle and Ring. The ratio is low for the Hook and Eye puzzle because of its similarity to the Heart and Bow. This similarity resulted in low initial times.

The range of variability in ratios here with a narrowly selected group of subjects and with puzzles all of the same general class is twice that of ratios used in the comparison of men and animals. It is not maintained that a high ratio always indicates a "locus" type of analysis, as that is not the case, but that the two may be found together, and that for that reason inferences from ratio to mental processes are insecure unless the nature of the problem is specified.

A test for inter-group comparison making use of modifications of a given puzzle form is touched upon in the following section.

3. General Conditions of Comparative Tests

Even if the comparison were confined to members of a group working with a single puzzle, certain general factors would need to be either equated or evaluated before the measure secured could be taken as a measure of intelligence. These factors have been for the most part treated in other chapters and will be but briefly touched upon in this connection.

a. Physical Condition.—Thus *Br*'s efficiency in getting first solutions was apparently seriously impaired by a necessary change from a favorable to an unfavorable time of day in case of one of the meetings. He complained that he could get "no insights."

b. Degree of Development of the Fundamental Function.—As stated in a previous chapter the capacity to construct transformations in three dimensions seems to be relatively undeveloped. The two-dimensional problems seemed much easier of solution. The writer had intended to examine into the effects of manual training and of the study of descriptive geometry by taking series with subjects both with and without the training in question but otherwise as homogeneous as possible, but he has not been able to do that as yet. A single manual training teacher was tried with a few of the puzzles and she did very well but that may have been due to other causes. Whether there is a single function of the sort referred to or whether there are many was not determined, but it was noted that presenting the puzzle in a new position interfered with readiness of solution, that learning to do the puzzle one way did not necessarily bring the ability to reverse the process, that pronounced success with one type of puzzle was not always accompanied by success with puzzles of a different type.

c. Concrete Related Knowledge.—*Wh* was given puzzles involving all the fundamental principles of a new and complex one, the Hinged Rectangle. He solved this the first time in 45 seconds. The best time made by a person who had not solved the puzzles involving the principles was 1,346 seconds; the puzzle was a difficult one and two subjects failed to get it. The subjects who had been given some but not all of the elements of the complex puzzle ranged in times from 157 seconds to 5,132 seconds. Their records are given in the following table.

Bg	*Ta*	*Tz*	*Mc*	*St*	*Co*	*Rd*
156.8	362.4	375	964.6	2,189	3,262	5,132

d. General Methods.—Ideals of scientific method seemed to be effective in proportion to the novelty of the experience. The boys, without scientific training, failed to develop a general rule or formula in the case of one puzzle where complications were introduced necessitating changes in manipulation which could be predicted if a general formula of solution had been developed for the solution of the puzzle in its first form. This generalization was made by those with scientific training. The boys did not show themselves much if any inferior in the use of concrete related knowledge as supplied in earlier and similar puzzles.

e. Attitudes.—This topic was discussed at the close of Chapter III.

If these and possibly other important factors were under control, we might hope to get some test for intelligence which would measure the variability of the subjects in both qualitative and quantitative terms. The term variation is used to include not merely the coming of a suggestion or impulse but also its evaluation. One of the best ways of doing this seems to be that of training the subjects with a puzzle of a given type and then studying their capacity to use this knowledge in dealing with more or less thoroughgoing transformations of the principle involved. The table just given shows the range of facility in utilizing related knowledge in dealing with a new case. The following table for the Hook and Eye puzzle gives further detail. This table has been presented already but in the form of relative rank only. All the subjects had solved a closely similar puzzle previous to solving this.

TABLE V

HOOK AND EYE

Ta	*Tz*	*Ry*	*Br*	*Bg*	*St*	*Co*	*Fe*
7.4	12.0	29.0	44	117.4	164.2	448.2	1,455.4
4.4	3.9	62.4	7	5.6	4.4	4.0	1,869.0
5.0	2.8	13.7		4.6	2.8	3.4	617.4
2.8	1.8	11.7		3.0	3.0	3.2	25.8
2.6	1.8	6.5		5.0	2.4	2.6	26.8
2.5	4.2	3.8		4.2	2.3	2.6	23.8
2.3	1.8	3.0		3.6	2.6	2.2	16.0
2.3	3.3	2.9		3.6	2.2	2.4	8.4
1.8	2.9	2.8		3.8	3.0	2.0	6.8
1.9	1.6	4.9		5.4	2.2	2.4	5.4

The two subjects making the best records, *Ta* and *Tz*, stated that they thought at once of the Heart and Bow puzzle. *Fe* had a vivid mental picture of the latter puzzle, but her attention became fixed on an irrelevant feature and she made no use of the experience. This result illustrates rather powerfully the difference between having the related experience, or even recalling it, and the using of it in dealing with a problem.

By varying the number and complexity of modifications of the original form of problem, the subjects could be distributed according to their capacity to use related knowledge, and a comparison between groups would become possible in terms of modifications mastered, even when it would be difficult to compare the difficulties of the original problem. This seems to promise especially well where the groups are widely separated as with animals and men.

CHAPTER V

The Place of Analysis in the Practise Curve

It was suggested in the preceding section that not merely the first few trials, but also the whole course of improvement in practise, might be used as a "test of intelligence" provided the many and seriously complicating factors involved were borne in mind. According to the results of Book and of Swift the course of practise would hardly seem suitable for such a purpose. They found, to be sure, that drops in the curve were very largely dependent on variations in method, but they also found that these variations were most effective if allowed to grow into habits of action without being specifically attended to. This is quite possibly an overstatement of their position, but, with that understanding, it may serve to define the question more sharply. To quote:

Swift, "Mind in the Making," p. 213: "It is suggestive that in all these experiments the method by which the reaction was improved was hit upon unconsciously. The learner simply tried to do the thing upon which he was working, and, in the process, he found himself using an improved method, and the new acquisition was always well along before it was discovered. . . . In order to test the matter further, the writer has since tried the experiment of learning to handle a punching bag skilfully, and here also it is quite clear that all of the delicate movements by which the bag is made continually to rebound with a rapidity that the eye can not follow, were happened upon quite unconsciously. There is a subconscious utilization of experience."

Book, "The Psychology of Skill," p. 171: "The new adaptations or forward steps were made quite unintentionally so far as the subjects were concerned. They were simply fallen into when the conditions were favorable for making a forward step and were executed marginally for some time before the learners became aware of their presence and value for the work. When the advantage of the new method had been noticed it was generally thereafter made use of purposefully though, even then, consciousness seemed to be more a hindrance than a direct help."

The results of the practise series with the puzzles seem to show, however, that with such material, at least, the method of conscious control is the efficient one. A large percentage of the motor variations come, to be sure, without definite anticipation; they are started and perhaps well along in their course before their significance is realized; but their value seems to depend rather directly on their being noticed, purposely tested, and adopted or rejected as the case may be. The value of the explicit analysis of the variation is, of course, most pronounced when the conditions are changed, but the conditions are rarely precisely the same, however careful one may be in the attempt to make them so.

1. Percentage of Drops due to Conscious Variations

One hundred and twenty-eight curves were examined and the drops in time marked. Reference was then made to the accounts of the trials concerned as kept by operator and the subject. If it were found that at the point in the practise curve where a given drop occurred a variation in method had been introduced by the subject and reported by him, and if the variation were known to have some significance as a means of control, the drop was recorded as due to the conscious adoption of a variation. Variations often appeared in a single trial and were reported by the subject but without becoming established. In some cases variations were introduced which apparently resulted in a rise in time. The determination of the cause of a single drop in the curve is not a simple matter, and there is, very likely, a considerable range of error in the determinations, and the percentages given should not be taken as exact, but as standing for an area rather than a point.

Number of practise curves	128
Total number of drops	308
Number of conscious variations coincident with drops	222
Percentage of drops due to conscious variations	70

In certain cases only the objective record was kept. Variations in method were noted, and, if the subject brightened up at a certain point, moved directly and precisely, and maintained the variation after that time, the variation was marked, "objectively purposeful." Such cases are not included in the percentage given above. If they are added, and the operator feels confident of the interpretation of the objective signs, as borne out by the cases where the subjects' records were also available, the percentage would be increased to 78.

2.6 per cent. of the drops were credited to variations in method for which there was neither objective nor subjective record indicating that the variation was the object of attention on the part of the subject. 5.5 per cent. were put down as due to increased attention during the last ten trials of the series of fifty, the final spurt. 3.6 per cent. were attributed to a uniformity of method, the absence of disturbing variations or oscillations between several different methods which sometimes occurred. The rest of the drops were put down as unaccounted for. These last amounted to 12.5 per cent. of the total number. If they were all counted as cases of drops due to accidental variations, the per cent. for unconscious variations would be 12.5 plus 2.6, or 15.1. There was no evidence that they were due to this cause, and the supposition is made simply to try to get some statement in terms of limits. The extremes in comparability of per cents. would thus be:

A	Per Cent.	B	Per Cent.
Conscious variations	78	Unconscious variations	15.1
Unconscious variations	2.6	Conscious variations	70

The analyses of the variations differed in the ways described in Chapter II., in time of occurrence in relation to course of the variation, in explicitness, completeness, causal, local or merely mnemonic character, etc. These various forms of analysis are of very different value, but they all agree in that the general nature of the process involved is that of setting up an hypothesis and more or less critically testing it.

2. "Curves" Illustrating Significance of Conscious Variations

(1) The first illustration consists of the records of the first fifteen trials of the subjects *Rd* and *Tz* with the Star and Crescent puzzle.

TABLE VI

	Rd	*Tz*		*Rd*	*Tz*		*Rd*	*Tz*
1	132.0	1,476.0	6	63.6	25.0	11	42.0	31.4
2	590.6	241.0	7	109.6	50.0	12	14.8	22.4
3	1,775.5	44.2	8	43.2	28.6	13	28.0	66.6
4	278.4	33.8	9	70.0	37.6	14	9.0	35.4
5	750.0	32.7	10	28.6	24.6	15	14.6	29.0

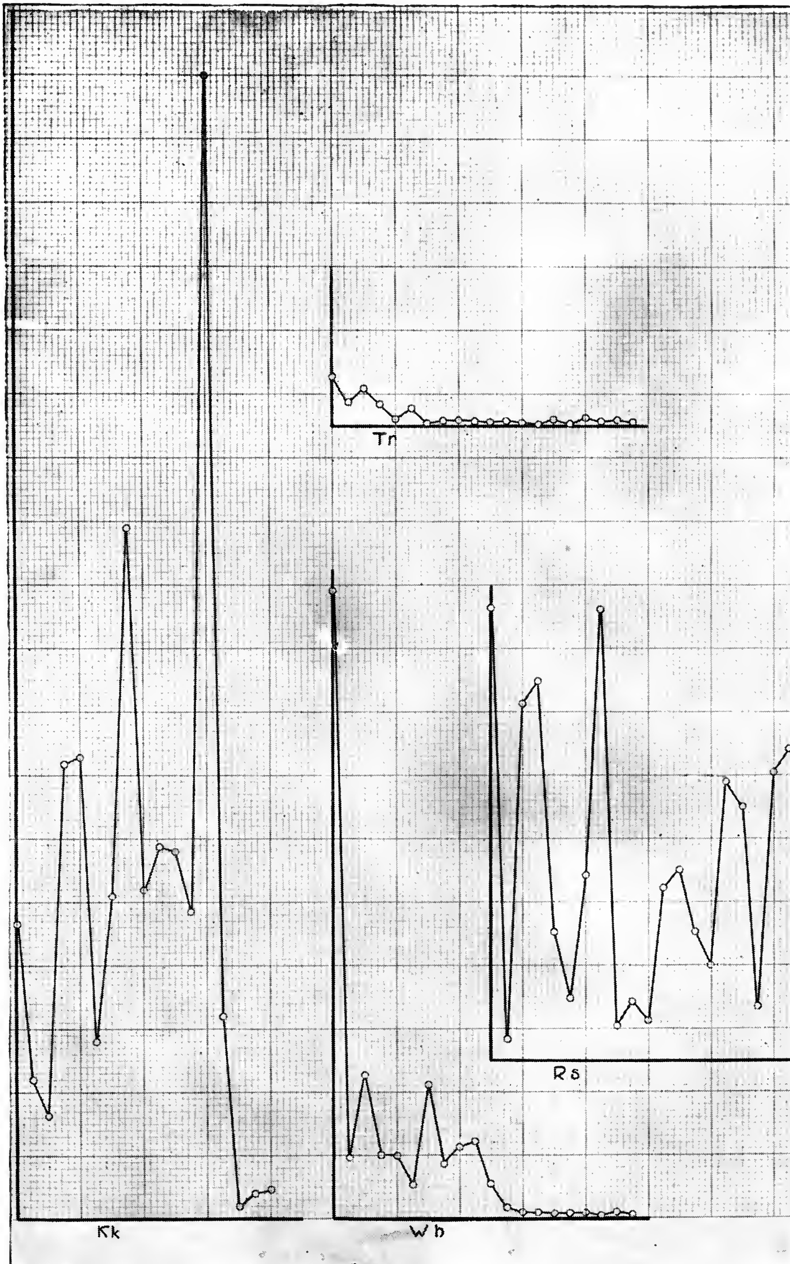
In the case of *Rd* it will be noticed that instead of a drop after trial 1, there is a rise, and that the times remain higher until trial 6. The first success came in such a way as to render discrimination difficult. There are two stars, but only one can come off. They were closely associated by chance in the first success. In trial 3 subject realized that the stars must be kept apart, but did not work out a technique for it till the latter part of trial 5. The very fact that she had difficulty after succeeding easily in trial 1, made her nervous, and she lost confidence in things which she felt she clearly understood. "Began to doubt the necessity of slipping the star over the crescent. Then I doubted the things I had discovered in the first trial, *e. g.,* the folding back of the loops, and I decided that I did not know what to do." (Long time of 7 due to another cause.)

In the case of *Tz,* the distinction between the two stars was sharply made in trial 1, and the technique for the control of the one which could not come off was worked out in trial 2. *Tz's* case is given as a contrast to *Rd's* as to quickness with which the fundamental points were consciously brought under control, but, even then, accident played a large part in *Tz's* analysis. Trial 1, *Tz:* "The problem now was to manipulate so as to keep the end star from getting in the way. I did so, but do not know exactly how, *i. e.,* one or more of the steps were chance ones." In trial 2, the subject got the correct position for control first, and then realized that he had what he was looking for. "As I was looking in a haphazard way for a position, I saw that I had what was almost certainly the correct position." The experimenter's account shows that the "accident" followed a vague idea, and that there then came suddenly a full realization of significance. "Think it goes here—some way—." "The position was all right but subject keeps looking for a place." "Now I have it." In trial 3 he said, "I think I know all the fundamentals now."

The illustration just given shows the importance of hitting on the right method, and also the large part played by accident in the development of conscious control. The fact that two things were associated in the successful movement, *Rd* trial 1, seemed to result in an apparently necessary connection. *Rd's* difficulty came in breaking up the association resulting from the peculiar circumstances attending the first chance success. The same thing occurred with *Rd* in the Heart and Bow puzzle. A circuitous process was maintained until trial 13 when it was seen that a large part of the movements were unnecessary and a short cut was consciously adopted. There was, in that case, a sharp drop in the curve.

(2) The second illustration is from the results with the Twisted Nails puzzle. The conditions were not normal, in that neither subject saw the puzzle while working with it. *Wh* worked the puzzle with eyes closed, and *Kk* with eyes open but not on the puzzle.

In the case of *Wh* the first success came accidentally after a futile attempt to solve it ideationally. The position which led to success was noted, and the movements were finished purposely, although the details were not worked out. In trials 9, 10 and 11, an



TWISTED NAILS—PUZZLE NO. 19

Scale: smallest division = 4″. *Kk* and *Wh* as described in text; *Tr*, normal curve; *Rs*, distraction.

TABLE VII

	Wh	*Kk*		*Wh*	*Kk*		*Wh*	*Kk*
1	397.0	186.0	7	82.4	213.4	13	3.6	720.0
2	38.4	87.6	8	34.2	436.6[1]	14	3.2	127.0
3	91.0	65.0	9	45.0	207.4	15	2.6	7.0
4	40.0	287.0	10	48.6	234.6	16	2.6	16.0
5	39.4	291.6	11	21.6	232.0	17	2.8	16.0
6	20.4	112.0	12	6.6	194.0	18	1.2	17.0

[1] One hour later.

important rule concerning details of manipulation was worked out. There is a second sharp break in the curve at this point.

Kk failed on the thirteenth trial after spending a longer time than on any previous one. He was then asked to look at the puzzle. The remaining trials are with vision. The lack of vision proved a more serious hindrance for *Kk* than for *Wh*. The latter was able to analyze in terms of tactual motor experience. The former analyzed the movement only after he was permitted to use his eyes. The latter case shows that mere repetition of the success was not sufficient for formation of habit.

(3) The following six records from the Heart and Bow puzzle show the results of conscious variations at different points on the curves and the marked contrast between different series as to the quickness and thoroughness of analysis.

The "curves" in the accompanying table (Heart and Bow) will be treated individually in the order right to left.

Fe. The drop from trial 1 to trial 2 was due to a locus analysis. There is little improvement till trial 5. To quote from the subject's record for trial 3: "I did not get it with a purpose. Have a general notion of where to work; was trying to make a mental picture of how the rod should be when you took it away." At the end of trial 4 the subject noticed the position carefully as the puzzle came apart again by accident. "I got a good mental picture that time. Now I know it." There is a sharp break in the curve at this point and it does not rise again to the previous level. In the seventh trial the essential process was verbally described. The later variability was connected with the process of orienting the puzzle, the subject shifting from one method to another.

Mt. The first successes were due to chance and the slow drop in time in trials 1–3 was "locus" analysis. Trial 1: "I have no idea in the world how I did it. I remember moving the loop of the heart about the end of the bar, and the two pieces suddenly came apart. I think that I can do it sooner next time, not because I know just how to do it, but I remember the parts of the puzzle which I brought together in the first success." Trial 2: " . . . do not yet know what movements to make." Trial 3: "Success was still largely chance; did not anticipate except that I knew there was a certain part of the

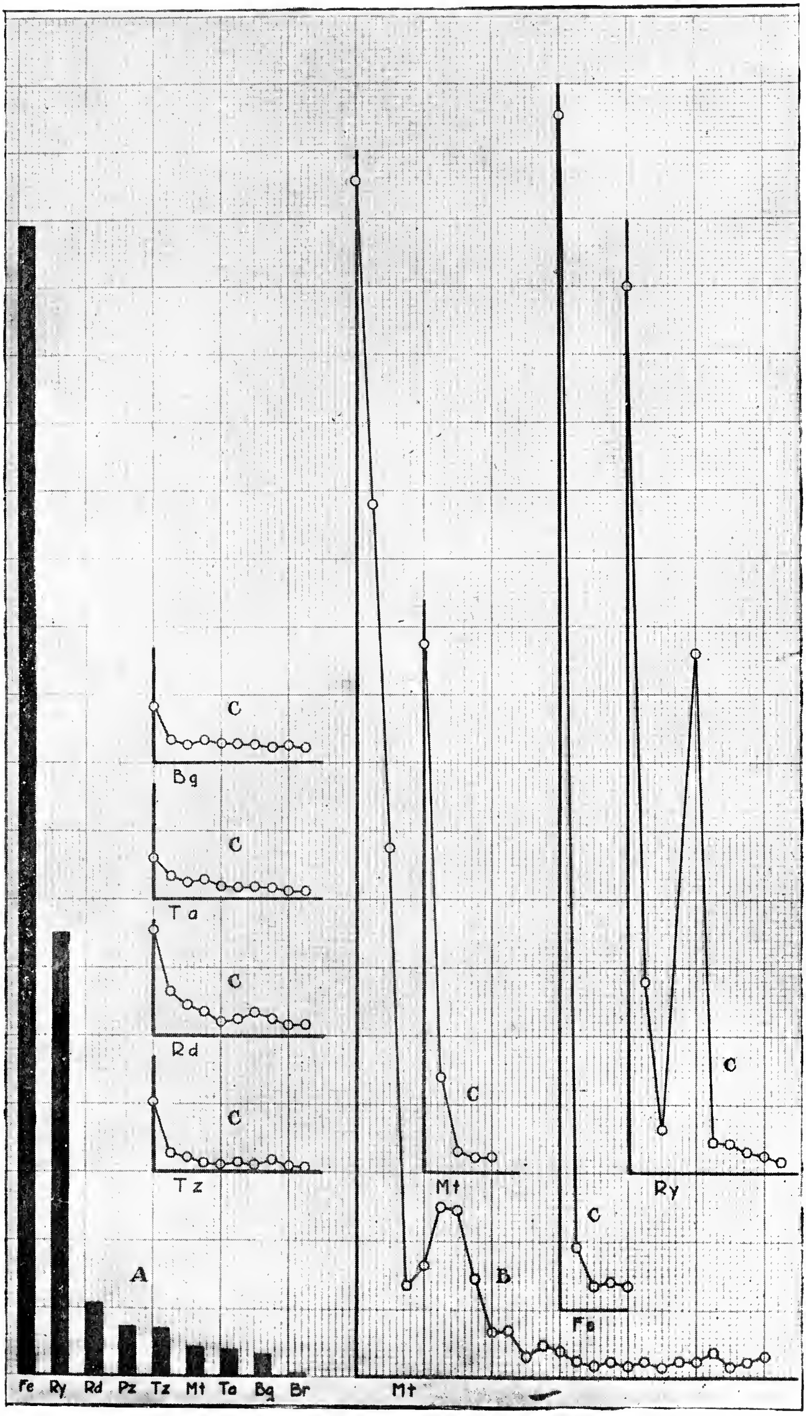
TABLE VIII

HEART AND BOW PUZZLE

	Tz	*Ta*	*Ry*	*Rd*	*Mt*	*Fe*
1	572.0	325.0	5,203.0	851.0	351.0	13,532.0[3]
2	212.4	12.2	260.0	17.3	256.4	351.0
3	18.4	8.4	196.0	50.0	155.0	437.4
4	20.8	5.0	214.0	16.0	27.0	261.8
5	14.4	14.4	331.0	31.1	33.0	40.0
6	8.2	9.8	143.0	19.2	50.0	65.0
7	5.6	7.0	54.0	11.2	49.6	15.0
8	6.6	6.2	24.0	20.2	28.0	30.6
9	4.0	4.8	56.0	13.0	13.6	18.8
10	3.6	10.8	58.0	10.4	13.7	10.2
11	6.0	5.0	21.0	13.4	6.0	6.5
12	4.2	5.6	40.0	12.0	9.5	12.8
13	3.6	4.3	12.0	7.0	8.0	6.7
14	4.0	8.0	13.0	9.0	5.0	10.6
15	3.4	4.6	13.0	7.0	3.6	6.0
16	28.0	6.0	101.0	7.8	4.8	7.0
17	3.6	5.9	118.0	7.6	3.6	10.0
18	2.8	6.8	19.0	10.0	4.4	7.8
19	2.2	4.7	32.0	7.0	3.3	8.7
20	2.4	4.1	62.0	5.4	4.5	6.5
21	2.3	3.9	189.0	3.8	4.8	7.0
22	2.0	3.5	244.0[2]	3.6	7.7	6.0
23	2.0	4.1	152.6	6.1	3.2	7.0
24	1.8	6.8	19.4	5.1	4.7	7.0
25	3.0	4.3	128.0	4.1	6.2	7.4
26	2.4	3.6	172.0	4.3		
27	2.9	4.2	9.0	4.8		
28	5.5	5.2	18.4	7.0		
29	4.6	3.7	6.0	7.0		
30	2.4	3.8	7.6	5.0		
31	2.6	4.3	9.4	7.0		
32	3.0	3.6	13.0	13.0		
33	2.1	2.7	8.6	4.0		
34	2.8	6.1	6.0	4.0		
35	2.2	3.9	8.2	10.0		
36	8.6	4.6	10.4	5.1		
37	2.4	2.5	6.2	5.0		
38	3.5	2.7	4.4	9.8		
39	5.0	3.8	10.0	4.2		
40	2.6	3.6	5.4	4.0		

[2] Two days later.

[3] Three days (trial 1 completed on third day and also the rest of the series).



HEART AND BOW—PUZZLE NO. 4

Scale for *A*: smallest division = 80″; scale for *B* and *C*: smallest division = 2″. *A*, times for trial 1 for 9 subjects; *B*, entire curve for *Mt*—25 trials; *C*, medians for successive groups of 5.

TABLE VIII—*Continued*

	Tz	*Ta*	*Ry*	*Rd*
41	2.3	2.8	4.2	5.3
42	2.0	2.6	5.2	2.8
43	1.8	3.8	5.2	3.2
44	3.1	2.5	4.6	3.2
45	2.0	3.6	10.0	4.6
46	2.8	3.1	5.6	3.2
47	3.0	2.3	5.0	3.2
48	1.7	4.6	3.8	5.2
49	1.9	2.8	3.4	5.0
50	1.8	2.3	3.8	2.6

puzzle to work at.'' Subject noticed, however, the way in which the puzzle came apart and was able to describe it. ''Hold the heart in the right hand and the bow in the left. Move the loop of the heart through the end of the bow. Can't describe the other movements; the rest is chance. Think I will get it next time.'' The essential movement was caught here, and the curve breaks abruptly to a lower level at this point. Details of technique were worked out in trials 11 and 13. Trial 11: ''It is easier to run the loop of the heart under the end of the bar. Had done this before but just realized its importance.'' Trial 13: ''Noticed that when the bow is in a vertical position the bar on the upper side should be in a horizontal position. Pass the loop underneath, and with a sort of twist pass the end of the bar through the loop of the heart.'' Trial 14: ''Went through as anticipated. Feel that I understand solving the puzzle.''

Rd. Trial 1: The subject tried various things in trial 1 which did not reappear in the later trials. There was finally a locus analysis which in this case was anticipatory and resulted in success. ''Then a conspicuous part of one section caught the eye, and from that time it seemed certain that it must have something to do with the solution. . . . The final solution had some element of chance, I do not know just how I did it.'' Trial 4: ''The way out seemed more familiar. I could foresee the result of the movement.'' There is a rather decided drop in the curve at trial 12. *Rd* had been using a very roundabout method of orientation, but in this trial she struck upon a shortcut which she consciously adopted. ''It is not necessary to slip the bow over the indentation. Hold the puzzle so that the square ends of the bow and bar are above the indentation. Pull the loop of the bar down. Then push the end of the bow up.'' Trial 39: ''Took the heart with the left hand and seized the bow with the right at once.'' This variation in method was a short cut on an earlier method of orientation. The drops in the curve are closely associated with the explicit analyses mentioned.

Ry. After trial 1 the movements were limited to the neighborhood of the ends of the bow and bar, *i. e.,* the general locus of attack was defined although there were persistent errors within that locus. In trial 1 the movements at other parts of the puzzle were connected with the idea of (*a*) getting loop end and part of heart inside the bow, and (*b*) pushing the bow inside the loop of the heart. The success of trial 1 served, although accidental, to eliminate the movements connected with those views, but the errors about the new locus persisted long. In trials 2 and 3 the puzzle came apart without the subject's noticing it. In trial 4 the heart was all ready to withdraw for ten seconds before he withdrew it. In trial 5 the correct position was not utilized until its fifth appearance. In trial 7 the subject said, "I think I have it now." There was apparently here a rough analysis of the main features of solution and there is a decided drop in the curve at this point. This analysis, however, did not include the necessary discrimination of the correct method of attack from a very similar but incorrect one. This incorrect position was chanced upon in trial 16 as the subject was trying to hurry. He became "rattled" and all his old errors connected with the general locus of attack reappeared. These persisted in the immediately subsequent trials and high and irregularly fluctuating records result. The interval of two days between trials 21 and 22 comes 6 trials later than the beginning of this period and so can not be held responsible for the loss of speed, though it may have contributed to that result.

The period of mental confusion as to the main line of solution cleared up in trial 26. The subject showed, toward the end of this trial, signs of increasing alertness. He made the taking-off movement with decision; numerous similar opportunities in the same trial had been passed by. One error which appeared six times in this trial did not appear again in the series. He mentioned, however, at this point that he did not yet understand the error which precipitated the catastrophe in trial 16. This error was avoided during the later part of the series, but the reason for the error did not become clear to the subject until after the close of the series, when he was given the puzzle to examine for that purpose.

The errors, in the case of this subject, dropped out in the inverse order of their difficulty of discrimination from the essential movement, the false movements most like the correct movement dropping out last. Pushing the body of the heart instead of the reentrant loop dropped out first, trial 4. The further errors are all concerned with the pushing of the loop. Pushing the loop through the bar instead of the bow dropped out in trial 7. This is the easiest discrimination as to the use of the loop. Pushing the loop through the bow end from the wrong side disappeared in trial 9. It was explicitly recognized as an error. Getting the loop crosswise of the bar after inserting it in the bow disappeared in trial 24; while putting the loop through the bar end after having put it properly through the bow end remained until trial 27. The last-mentioned error hung on, after it was recognized to be an error, as a result of the disaster in trial 16. The correct method seems to have been worked out to a sufficient degree to serve as a guide in the later trials although the error in question was not understood. This case illustrates how slow and gradual the development of discrimination may be.

Ta. The first success for *Ta* was purely accidental. He gained, however, a vivid picture of the final position as the puzzle came apart, and on the basis of this image the analysis of the essential movement took place at once. The subject attacked the puzzle in trial 3 with complete certainty as to the essential movement, and in trial 7 he was able to anticipate the details of manipulation. No important variations were introduced later in the curve. There is evidence of a final spurt. The curve of *Ta* may be contrasted with that of *Mt* as to the initial drop since the first success was in each case accidental and the times for the first success were approximately the same. The time for trial 2 for *Ta* is, however, only one twentieth that of *Mt*. As contrasted with the gradual dropping out of errors in the case of *Ry* there is complete disappearance of errors in the case of *Ta* after trial 1.

Tz. Trial 1: " . . . The puzzle was finally studied and as many possible combinations as occurred were followed either mentally or by actual doing, but the final solution was largely by accident as I did not expect that that trial would be successful." The long time of trial 2 is due to the fact that the subject made an explicit memory error as to the direction of the movement. This error was definitely recognized in trial 2 and did not reappear. "My recollection that I inserted the loop in the reverse direction in trial 1 was probably wrong as in trial 2 I was unsuccessful until I inserted it in a contrary direction, which makes me think that I must have inserted it in that direction for the first time, also the situation seemed familiar when success came."

In trials 3, 4 and 5, the attention of the subject was directed to the question whether any other mode of solution was possible. He convinced himself that there was none, and, beginning with trial 6, his attention shifted to the gaining of facility in manipulation. Important variations in method were consciously worked out in trials 8, 9, and 13, and there is a rapid and uniform decrease in times to a very low limit in trial 24. Trial 8: "Tried a new way. . . . Thought that manipulation might be easier to take hold of the straight rod, as that is the one to be turned to release the ring. However, I experienced trouble with the bow." Trial 9: "Taking the bow in the left hand and manipulating the bar with the fore and middle fingers while removing the heart with the right hand-hold before the fore and middle fingers gives best results so far." Trial 13: "I think that a firmer hold of the bow would help the manipulation of the bar." The long time of trial 16 was the result of an accident. The subject made a quick move and barely missed getting the puzzle apart. He was greatly surprised to find it still together and became embarrassed and perplexed. He pulled himself together, however, and then solved as before. It is interesting to note that the disasters of *Ry* and *Tz* came on the same trial, number 16; that in the case of the latter recovery was immediate and complete, while in the former there was a prolonged relapse, and that there had been explicit analysis of method of manipulation in the case of *Tz*, but not in that of *Ry*.

In trial 25, *Tz* deliberately abandoned one of the methods of control worked out in trials 8 and 9, and adopted a less secure method which seemed to promise greater speed. The curve rises after this and the results are more irregular until the very end. This unfortunate change of method was suggested by a chance variation in trial 25.

It may be of interest in this connection that *Tz* was the oldest of the subjects concerned, and that he had had no special training in motor lines, that he considered himself to be clumsy, and yet that he made the lowest and best record with this puzzle, and that this greater success in manipulation was correlated with many effective variations of method and explicit consciousness in their employment.

(4) The accompanying table for the Triple Horseshoe puzzle includes six curves from subjects under normal conditions, marked *Bg, Co, Mc, St, Tz* and *Br;* one curve from a subject who worked with his hands behind his back and without seeing the puzzle, *Wh;* and two curves from subjects working under distraction, *Tr* and *Rs.* The distraction consisted in counting to two hundred by adding digits which constantly varied within the limits 2 to 9, of then reversing the process until, as a result of continued subtraction, negative 200 was reached, then of reversing again and passing through zero to plus 200, etc. The subjects counted aloud and were required to do all the work mentally. Since the digits ran up from 2 to 9 and down again, changing each time, and since there were changes at the limits also to be borne in mind, the task proved to be an ex-

acting one. An additional subject, *Ds,* working with distraction, failed to solve the puzzle although 2,700 seconds were spent upon it.

TABLE IX

THE TRIPLE HORSESHOE

	Bg	*Co*	*Mc*	*St*	*To*	*Br*	*Wh*[4]	*Tr*[5]	*Rs*[5]
1	325.0	194.0	604.4	967.6	623.4	168.6	428.6	237.0	1,285.0
2	63.0	50.0	28.8	102.0	34.6	85.0	1,177.0	1,047.0	285.0
3	26.4	8.0	9.4	10.6	73.0	60.4	222.4	160.0	453.0
4	64.6	12.0	6.4	5.4	59.0	69.4	14.4	325.0	435.0
5	18.0	24.4	18.4	59.4	39.0	95.0	96.2	251.0	
6	12.0	10.0	10.6	9.0	26.0		64.0	448.0	
7	11.0	4.0	22.0	13.0	35.0		34.2	59.0[6]	
8	5.0	22.0	10.6	75.4	108.0		25.6	22.0	
9	4.4	5.0	14.4	93.0	61.0		31.0	23.0	
10	35.0	24.0	5.0	14.6	18.0		24.6	42.0	
11	12.0	6.0	4.2	56.6	10.4			83.0[7]	
12	8.6	2.0	5.6	9.2	10.0			7.0	
13	7.4	43.0	12.8	8.0	10.6			5.0	
14	3.4	34.0	5.0	12.0	91.7			4.0	
15	7.4	9.0	14.0	4.4	23.6			17.0	
16	6.6	9.0	5.4	12.4	60.8			17.0	
17	11.4	6.4	6.4	37.8	40.0				
18	7.4	8.0	8.8	16.2	33.4				
19	5.0	15.0	13.6	3.8	9.8				
20	6.8	13.8	5.6	12.4	21.0				
21	4.6	51.0	5.4	4.2	26.0				
22	4.6	2.0	16.6	7.0	19.6				
23	3.6	16.0	4.2	3.6	6.8				
24	2.8	8.6	4.4	22.2	7.6				
25	14.4	2.0	5.0	6.8	21.0				
26	16.0	13.0	3.8	6.6					
27	9.8	7.0	19.0	10.8					
28	3.4	3.6	9.0	6.0					
29	13.0	4.0	3.4	4.0					
30	15.0	3.0	13.6	6.4					
31	2.6	6.0	11.0	6.0					
32	13.0	14.0	4.4	2.6					
33	5.2	7.0	3.8	3.2					
34	7.0	7.0	10.8	21.4					
35	6.0	21.0	4.0	4.0					
36	17.0	21.0	4.4	4.8					
37	6.4	15.0	4.6	4.8					
38	5.0	5.0	3.2	3.0					
39	5.0	4.4	4.2	10.4					
40	3.4	3.8	8.6	28.2					

[4] Hands behind back, vision excluded, but no distraction.
[5] Distraction by counting.
[6] Following day.
[7] No distraction.

TABLE IX—*Continued*

	Bg	*Co*	*Mc*	*St*
41	2.4	2.0	7.8	3.0
42	5.0	3.6	4.2	2.8
43	4.6	5.0	4.0	3.0
44	2.2	7.4	3.4	4.2
45	7.0	8.0	4.6	3.0
46	3.4	3.8	8.2	4.2
47	12.0	11.0	7.6	3.0
48	2.0	7.6	4.8	5.6
49	8.0	3.4	19.0	29.0
50	4.2	2.2	4.6	10.0

The puzzle is a very difficult one to control, and none of the subjects hit upon the precise twist required to give both speed and stability. This accounts for the unusual variability in the last part of the curves of the normal subjects.

Despite his handicap *Wh* managed to analyze out an important step in the process in trial 2, and to add to it in trial 4. He did not succeed, however, in mentally constructing the transformation so that he felt that he understood the puzzle geometrically.

The distraction in the case of *Rs* was very complete, and there was no further drop after trial 2. The result was similar in his case with the Twisted Nail puzzle where the series was much longer, 21 trials.

Tr's attention slipped in trial 6, as he felt success coming, from the counting to the puzzle and he analyzed the essential movement during that slip. There is a sharp break here to a lower level. Distraction was discontinued in the eleventh trial and there is another drop in level at this point. The experiments with distraction were performed to determine if repeated success plus the emotional accompaniment would stamp in the habit if attention were left out. So far as it goes the evidence is negative.

(5) See Table X and Plate IV.

The curves *A* and *B* are similar in that the solutions were in each case the result of a thoroughgoing anticipatory analysis resulting in a sudden sharp drop to a very low level. The curves are also similar in the uniformity attained in manipulation times. The physical conditions of the puzzles were more favorable for accurate manipulation than in the case of most of the puzzles.

Puzzle *A* was solved by systematic exhaustion of possibilities by means of successive dilemmas. 1,296 different combinations were possible and solution by any other method appears highly improbable. The drop in trials 3–5 was connected with the conscious adoption of shortcuts and the substitution of memory cues for fresh analyses. The drop at trial 12 was connected with the conscious

TABLE X

	Rr A	*Wh* B	*Rr* C	*Rr* D			*Wh* D	*Wh* E	*Rr* E
1	13,500.0	2,090.0	18,000.0	1,037	75*	35	5,320	—	547
2	1,295.0	350.0	178.0	505	49	26	240	62	71
3	280.0	39.0	152.0	349	80	29	185	27	90
4	80.0	26.0	134.0	185	47	22	215	25	40
5	30.0	20.0	118.0	333	91	24	209	21	106
6	33.0	21.0	73.0	162	75	28	120	22	48
7	25.0	20.0	72.0	155	58	30	95	22	108
8	20.0	18.0	67.0	98	104	28	78	18	255
9	18.0	18.0	66.0	103	69*	23		22	37
10	60.0	17.0	41.0	80	57	30		21	22
11	21.5	19.0	66.0	100*	97	24		18	22
12	22.0	17.0	77.0*	81	69	23		24	40
13	19.0	19.0	145.0	64	56	25		20	22
14	18.0	16.0	80.0	70	89	24		16	18
15	31.5	12.0	114.0	75	67	20		19	15
16	20.0	13.0	97.0	59	56	21		16	13
17	15.0	15.0	77.0	127	49	22		16	15
18	19.5	18.0	94.0	99	52	29		16	15
19	17.5	13.0	112.0	58	47	23		17	15
20	16.5	13.0	63.0	64	49	20		15	14
21	21.0	21.0	80.0	60	61	26		15*	14
22	18.5	17.0	85.0	55	45	24		18	13
23	16.5	25.0	65.0	55	78	20		15	13
24	17.5	17.0	50.0	74	41	24		16	13
25	17.0	18.0	87.0	65	40	22		17	11
26	13.5	13.0	105.0	65	47	20			
27	13.0	36.0	76.0	62	36	18			
28	12.0	19.0	56.0	46	32	24			
29	12.0	19.0	62.0	57	29	28			
30	12.5	15.0	54.0	48	58	30			
31	16.5	14.0	39.0	58	36	25			
32	16.5	17.0	53.0	68	27	19			
33	12.0	14.0	146.0	43	28	17			
34	31.0	13.0	95.0	46	34	17			
35	14.0	15.0	57.0	48	25	18			
36	9.5		45.0	58	27	35			
37	14.5		102.0	51	35	21			
38	15.0		50.0	50	37	21			
39	12.0		65.0	99*	30	21			
40	12.5		69.0	67	22	20			

A, Katzenjammer Puzzle.
B, Jig-saw, Ivory, Square.
C, Wizard Cross.
D, 12-piece Cross.
E, Lone Star War.

C, trial 12*, = 3 days later.
D, trial 11 , = 10 hours later.
D, trial 39 , = 1 hour later.
D, trial 51 , = 5 hours later.
D, trial 59 , = 3 hours later.
D, trial 97 , = 12 hours later.

TABLE X—*Continued*

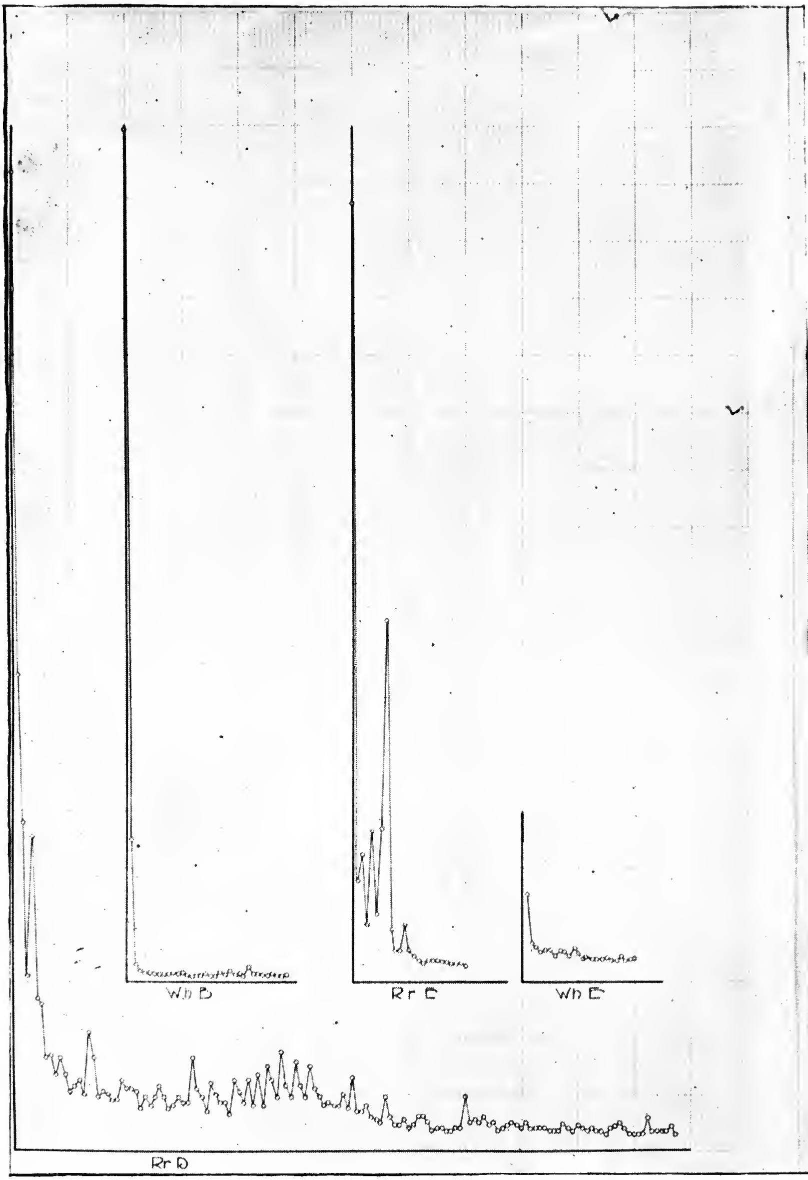
	Rr A	*Rr* C	*Rr* D		
41	12.0	65.0	56	23	28
42	12.5	53.0	43	23	19
43	12.0	51.0	71	22	
44	11.5	50.0	59	22	
45	11.5	47.0	50	24	
46	12.0	55.0	51	24	
47	11.5	53.0	40	56*	
48	12.0	41.0	75	30	
49	10.5	41.0	62	32	
50	10.5	40.0	51	30	
51		36.0			
52		48.0			
53		36.0			
54		33.0			
55		159.0			
56		53.0			

substitution of a new order, an order of convenience in place of the order of discovery. The substitution was the result of a perceptual impulse which was immediately approved in judgment. The inertia of the mentally established made itself felt however in a dread of change as such. But the change was made and it proved to be of value.

The drop at trial 25 was connected with a consciously adopted variation. "Used the heart-diamond-club as check with the green." The drop in trial 38ff. was connected with the adoption of the verbal cue "spade-two diamonds" before trying the purple, and with "a negative recognition of the orange."

No instructions were given to *Wh* as to the sort of figure to be constructed from the ivory pieces of puzzle *B*. The cross seemed probable and in testing that hypothesis the square was suggested. This was then chosen as *the* hypothesis because it could be exhaustively tested. The square on the hypothenuse side of a large rectangle was selected after a table of the squares on each of the sides had been compared with the approximate total area of all the pieces. The square was then constructed. Quotation from *Wh*'s account for trial 2 will show the ready supplanting of analysis by memory, and the volitional character, the stubbornness, of the memory assumption. "I put the two large triangles together at once and remembered which pieces went together to form the third side, but got these two in the wrong order, and fussed around with all sorts of combinations without thinking of changing the order of these two pieces. I think the reason I got the wrong order was that in the first round when I

PLATE IV



Scale of *Rr* D: smallest division = 6″; scale of *Wh* B: smallest division = 14″; scale of *Rr* E: smallest division = 4″; scale of *Wh* E: smallest division = 4″.

noticed that these two pieces would make up a side, I began with the triangle and looked for something to make up the proper length, and found the parallelogram suitable. So, I had the order: triangle—parallelogram in mind; but this was the order of discovery, not the order of arrangement on the square. What finally deflected me from this false order was partly the inability to get the other pieces together, and partly, perhaps, a recollection of the fact that had impressed me when I looked at the completed square after my first success, namely, that there was a triangle in one corner. This came back to me once or twice, I think, while I was working over the pieces, and I believe it came up just before I altered the order of the pieces.''

Trial 3: ''Got the pieces which troubled me last time correct without hesitation. The rest of the time was principally occupied in getting the positions exact enough to give a good square.''

Trial 4: ''Knowledge of the exact positions of the last three pieces to be inserted saved some little time.''

Trial 8: ''Time saved by getting the pieces near together and squeezing them at the close.''

Trial 10: ''Carried a little further the device mentioned in 8.''

(In trial 20 the subject changed the position of the first pieces and this was followed by more irregular and longer times.)

C. The Wizard Cross is composed of six square-cut wooden bars notched in the centers so as to interlock and form a tridimensional cross. There are a great many notches in each bar and the number of possible combinations is very large. The actual solution by *Rr* seemed to have involved three principal factors: (1) Practise in the discrimination of the parts, (2) selection of combinations in accordance with the requirements of the complete cross (as mentally pictured), (3) an attempt to exhaust the possibilities thus limited by beginning with each different type of piece—there were two pairs and two odd pieces—and working the limited combinations systematically. The solution came during this systematic search. The chief points in the practise with this puzzle were concerned with the perfecting of the discrimination of the different bars with anticipatory adjustment, and the simultaneous use of all the fingers. The notches were different on the different faces of the same bar, and it took considerable practise to hold the distinctions in mind so that a given bar could be recognized from any face. This was not perfectly accomplished during the series. There was considerable growth in anticipatory adjustment, but there was no such final unity as with the Chinese Ring. This difficulty was both simultaneous and successive. The pieces did not lock well until the last was in, and the task of holding three or four or five pieces from slipping while inserting another and preparing for the next step was not an easy one. There were numerous slips even to the end. The puzzle, then, is much more difficult of manipulation than *A* or *B,* and, although many control variations were followed up, a thoroughly satisfactory one had not been hit upon by the close of the series.

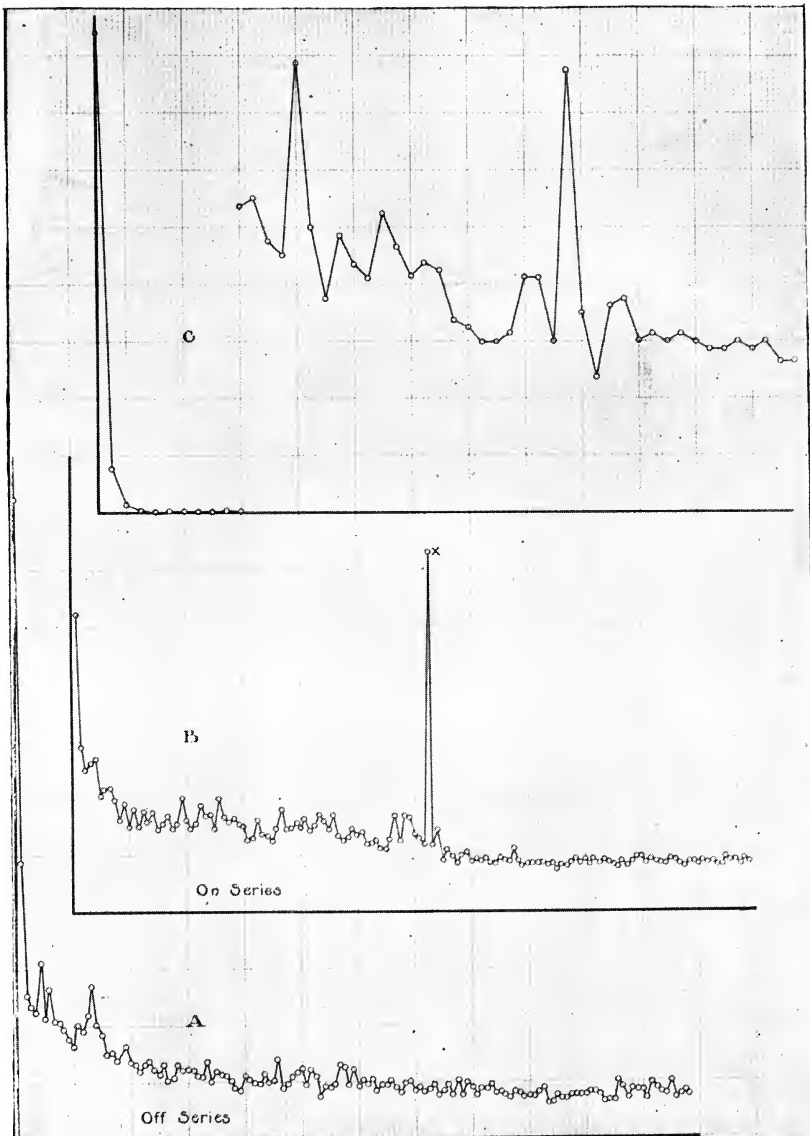
D. The Twelve Piece Cross was similar to the Wizard but the notches were simpler. There were, however, twice as many parts. *Rr* solved the puzzle 142 times and the table shows that the time of manipulation was very much reduced and that the puzzle was gotten under good control. This was in spite of the fact that the puzzle was so complex in manipulation that no sooner would one point of manipulation seem fairly well settled than a new difficulty would break out requiring readjustment all around or switching the attention off the method previously concerned. Contrary to the usual custom the analysis was not restricted here to the snap-shot type, but ten minutes was taken at trial 81 to work out a new technique. There is a decided drop in the curve in this locality and a great decrease in variability.

The significance of consciously employed variations is illustrated again by the drop in *Wh*'s curve in trial 7. To quote—Trial 5: "There is some difficulty in getting one of the pieces." Trial 6: "Trouble at the same point as before. . . . The difficulty can be avoided by raising one of the pieces and so easing the whole joint." Trial 7: "Time mostly saved by the device mentioned in the last entry."

E. Wh tried to get the puzzle, the Lone Star War, at first by random manipulation, but failing there he resorted to analysis. He employed a method which he transferred from mathematics and often attempted to use with the puzzles, namely, to consider the puzzle solved and then to retrace the steps. The method was successful here. The method was made more explicit and unified by an additional bit of analysis in trial 2. *Wh* then turned his attention to the problem of telescoping. "The overlapping is helped, perhaps, by using the hands not absolutely simultaneously on symmetrical wholes, but alternately, thus getting ready for the next pair while inserting the second of the preceding pair."

Wh tried on his own initiative the experiment of putting the flags in the holes in any order instead of the fixed order required to solve the puzzle. The last five records are of this attempt, and their average is 16.2 as against 16.0 of the five in the fixed order just preceding. The prescribed order is as easy or easier than the random.

PLATE V



A and *B*, Star and Crescent Puzzle, No. 11

By averages of tens. Scale: smallest division = 0.8″. *B*, analysis at *x*.

C, Katzenjammer Puzzle, No. 35

Rr subject. Scale: trials 1–11 smallest division = 160″; scale: trials 11–50 smallest division = 0.4″.

Rr solved the puzzle by chance manipulation the first time. He then tried to rely on memory, but in trial 8 he gave up this unsatisfactory method for one of analysis. The analysis was not however of so deductive a type as *Wh*'s. Trial 8: "Took time to work out a scheme: Place one of the flags opposite Havana at Malanzar, four next to Havana, then the two points, and then the two angles. This scheme was recognized geometrically—names not noticed, order of the four not yet memorized." Trial 13: "The order is: four corners, then center." The four had been giving trouble in trials 9–12. Trial 14: "Correct: memory and perception; conscious telescoping."

(6) The accompanying diagram gives the curve for *Rr* for 1,440 solutions of the Star and Crescent puzzle.

CHAPTER VI

Transfer

1. Results in Detail

(1) *Wh* and *Re* were subjects in a series of experiments with the Jiu Jitsu puzzle, all but one of the experiments being tried with *Wh* only. This is a small puzzle consisting of two symmetrical parts which are to be separated. Each of the parts resembles a small staple, the arms of which have been bent back at their middles 180 degrees, the points of the arms being brought so close together that the points of one of the staples if held at an angle of 90 degrees would barely pass through those of the other. If this has been done and the staples pushed clear in so that they touch at the necks, then the puzzle is in the usual position for solution, which consists in the reverse process of removing the parts. If one of the staples be held in a constant position, the other may be pushed on it from either the right or the left. The initial movements in taking off are different for these two cases, but the discrimination is not an easy one to make. For each of the right and left methods of insertion two positions of the puzzle were chosen. These positions were such as would result if the puzzle were held vertically and freely. They differed merely in that one was obtained from the other by rotating the puzzle 180 degrees. There were thus four principal positions. These were: A, right insertion, zero degrees; B, right insertion, 180 degrees; C, left insertion, zero degrees; D, left insertion, 180 degrees. The positions are referred to in the following discussion by the letters A, B and C, D, just given. The positions of a given pair, A and B, or C and D are more closely related than positions from separate pairs.

(*a*) There was decided transfer between the processes of taking the puzzle apart and putting it together. The subject practised 400 times taking it apart without being allowed to put it together or to see it put together. He was then given five trials at putting it together. The average time in seconds for the first five trials in taking the puzzle apart was 46, while in putting it together it was only 4.6, one tenth of the former; the longest record of the latter series was 7 seconds and the average deviation was 0.9. The transfer here was evidently one of method or idea, as the motor habits built up in the former, analytic, series would be the reverse of the ones in the latter, the synthetic.

(*b*) There was transfer from the C position to the D. This does not appear strikingly if only the first trial of each series be compared, but it comes out clearly if the immediately following ones are examined. The first trial for C took 18.4 seconds, of D 9.6 seconds, but the average of the immediately succeeding five trials is 17 seconds for C and only 1.9 for D. Since the times for the second ten trials with C average 1.8 and with D 1.6, the drop in times of D can not be attributed to greater ease of manipulation. The subject's record for trial 143, No. 1 of D: "Found that by turning the thing a quarter toward me, it would be in the position of the last few trials. Turned it and used motor habit number 2." The transfer here, then, depended on the analysis which enabled the subject to employ his motor habits.

(*c*) The subject was given a special practise series, with the A position, of 40 trials, the average of the first five being 7.3 and of the last five, 1.4. He also practised with eyes closed and hands placed in position, the average for the last five trials being 0.36 second. The subject was next given 90 trials with the three other positions, and was then told that he would be tried again with the A position. The average of these five trials was 15.8 seconds, twice as long as that of the first five of the special A series and eleven times as long as that of the last five of that series. The interference here seems to have been that of the C and D positions just practised, and to have been due to a failure to control the habits by a carefully discriminated idea of the relations of the different positions. This is borne out by the subject's record. During the fifth trial of this A test series, trial 167 in the table, he made the following analysis: "I was wrong. The right hand does not take the easiest hold, but the thumb is under . . . in positions A and B grasping the side more towards me or more anticlockwise, if the left hand is imagined to be farther away. In positions C and D the reverse must be the case . . . (trial 168). My scheme worked all right." Five more A's were then given. The following times are for the fives before and after the analysis; the second five begins with trial 169: trials 163–7, 25, 23, 13, 7, and 11 seconds; trials 168–172, 1.6, 1.4, 2.0, 2.6, and 1.4 seconds. This sudden change is evidently not in motor habits but in their control by an idea. The motor habit for A must have been carried nearly to perfection during the last part of the original series, for with eyes closed the average time for the last five trials was 0.36 second, practically one third of a second.

(*d*) In the following part of the experiment both subjects were employed. *Re* practised during the whole series of 250 trials with the puzzle in a chance position. The operator varied the position of the two parts in putting the puzzle together in the two principal ways, right and left, mentioned above, and the sequence was irregular although the total number for each type was approximately the same. The puzzle was each time tossed in the air by the operator and so came down on the table in a chance position.

Wh practised 400 times with the puzzle in the four main positions, A, B, C, D, described above. He practised each of these four positions until he had developed a special technique for handling it in minimum time. He also practised solving the puzzle in minimum time with the positions taken in pairs, the members within the pairs coming irregularly, and with the four positions in chance order. The tactual motor coordinations were especially developed by a long series with the eyes closed. In the early part of these series, trials 29–32, the puzzle was tossed up in the air, the conditions being the same for these four trials as they were for all the trials in the case of *Re*. At the close of the series of 400 trials of *Wh* the puzzle was again tossed in the air, giving the chance positions as in the first test of four trials. The average time of *Wh* in the final test series, 28 trials, was 11.6 seconds, the average for the last ten was 9.1 seconds. The average of the four trials in the first test series was 12.2 seconds. The average time of the last four of the A, B, C, D positions, chance sequence, just preceding the second test series was 1.8 seconds. There was evidently, then, but little gain, in the handling of the chance positions in which the puzzle was thrown in the air, from all the special practise which had intervened. This conclusion is confirmed by comparison with the series of *Re*. The average of the first ten of his series of 250, all being tossed up, was 335 seconds, of the four trials, 29–32, corresponding to *Wh's* first test series was 41 seconds, and of the last ten, trials 240–249, was 7.2 seconds, the average of the last 28 trials, 222–249, being slightly lower, 7.0 seconds, and the average for his last hundred trials being 7.6 seconds. *Re*'s results at the close of the series are considerably lower than *Wh*'s, although the latter manipulated the puzzle 179 trials more than the former.

The practise on the chance positions directly seems, then, to have been much more effective for the handling of the chance positions than the reduction to fixed rules and mechanization of four principal positions. If *Wh*'s attention had been directed toward the developing of a general rule instead of four special ones, it is probable that the transfer would have been greater.

TABLE XI

Jiu Jitsu Puzzle. *Wh* Subject

The four main positions are designated A, B, C, D. A means right insertion and zero degrees; B, right and 180 degree; C, left and zero; D, left and 180.

1	53.4		44	4.4	A	86	2.0	B	128	0.8	C	173	3.0	B
2	66.4		45	4.0	A	87	1.0	B	129	2.4	C	174	2.6	B
3	91.8		46	3.4	A	88	1.8	B	130	0.8	C	175	2.0	B
4	30.0		47	2.2	A	89	1.8	B	131	6.0	C	176	3.0	B
5	16.8		48	2.2	A	90	1.4	B	132	0.6	C	177	1.8	B
6	26.0		49	1.6	A	91	1.6	B	133	0.4	C	178	2.4	B
7	75.8		50	1.6	A	92	1.6	A	134	0.4	C	179	1.8	B
8	14.4		51	1.4	A	93	1.2	A	135	0.5	C	180	1.8	B
9	17.0		52	2.2	A	94	1.2	A	136	4.8	C	181	12.0	B[1]
10	8.0		53	1.2	A	95	0.6	A	137	1.6	C	182	5.0	A
11	8.0		54	1.0	A	96	1.8	A	138	1.0	C	183	2.4	A
12	7.4		55	1.0	A	97	1.2	A	139	0.8	C	184	3.4	B
13	39.2		56	1.4	A	98	1.4	B	140	2.0	C	185	8.4	C
14	15.0		57	1.2	A	99	2.2	B	141	1.6	C	186	12.6	D
15	8.0		58	1.6	A	100	2.6	A	142	9.6	D	187	10.4	B
16	5.0		59	1.4	A	101	1.6	A	143	2.2	D	188	30.0	D
17	5.0		60	1.4	A	102	2.8	B	144	1.4	D	189	14.0	C
18	8.0		Eyes closed,			103	1.8	A	145	2.8	D	190	60.0	A
19	10.6		hands in			104	2.0	B	146	1.8	D	191	19.2	D
20	5.0		position.			105	1.0	A	147	1.4	D	192	8.6	B
21	13.0		61	1.6	A	106	2.4	B	148	1.6	D	193	11.2	C
22	3.2		62	2.0	A	107	18.4	C	149	2.2	D	194	11.0	A
23	3.0		63	1.0	A	108	5.6	C	150	2.0	D	195	5.6	D
24	3.0		64	0.6	A	109	41.6	C	151	2.0	D	196	3.4	B
25	5.0		65	0.4	A	110	8.4	C	152	1.0	D	197	5.0	C
26	14.4		66	0.4	A	111	20.1	C	153	1.4	D	198	3.4	A
27	9.0		67	0.4	A	112	8.0	C	154	1.2	C	199	62.4	D
28	10.0		68	0.2	A	113	20.6	C	155	1.0	C	200	7.0	C
First test,			69	0.4	A	114	9.8	C	156	1.0	D	201	6.2	D
tossed.			70	1.2	A	115	3.6	C	157	2.6	C	202	4.6	C
29	20.0		71	4.0	B	116	4.0	C	158	1.6	D	203	21.4	B
30	7.4		72	3.4	B	117	2.6	C	159	1.6	D	204	10.6	D
31	7.4		73	2.8	B	118	1.6	C	160	2.6	C	205	7.2	A
32	15.0		74	2.4	B	119	1.6	C	161	1.6	D	206	5.6	C
33	3.2	A	75	1.6	B	120	1.2	C	162	1.6	C	207	4.4	A
34	7.0	A	76	2.8	B	121	1.4	C	163	24.8	A	208	7.0	D
35	4.6	A	77	1.2	B	122	2.8	C	164	23.4	A	209	4.4	B
36	15.8	A	78	1.0	B	123	1.4	C	165	13.4	A	210	5.8	C
37	5.4	A	79	1.0	B	124	1.4	C	166	6.6	A	211	7.2	A
38	8.8	A	80	2.6	B	125	2.0	C	167	11.2	A	212	3.6	D
39	6.0	A	81	1.8	B	126	1.8	C	168	1.6	A	213	5.6	B
40	4.4	A	82	2.8	B	Eyes closed,			169	1.4	A	214	5.4	C
41	23.4	A	83	3.0	B	hands in			170	2.9	A	215	6.4	A
42	3.6	A	84	120.0	B	position.			171	2.6	A	216	6.4	B
43	5.4	A	85	3.2	B	127	0.5	C	172	1.4	A	217	6.0	D

[1] Change of instructions: "Either A, B, C or D."

TABLE XI—*Continued*

JIU JITSU PUZZLE. *Wh* SUBJECT

The four main positions are designated A, B, C, D. A means right insertion and zero degrees; B, right and 180 degree; C, left and zero; D, left and 180.

218	5.0	A	262	2.2	C	303	7.4	B	345	3.0	B	389	2.4	B
219	6.4	C	263	2.1	B	304	4.4	A	346	3.0	C	390	2.4	D
220	12.0	B	264	2.5	D	305	4.4	D	347	2.4	A	391	2.5	C
221	8.6	D	265	2.8	D	306	5.0	C	348	2.2	D	392	2.4	B
222	4.6	A	266	2.8	A	307	4.0	D	349	1.8	B	393	1.8	A
223	4.2	A	267	3.4	C	308	2.4	A	350	2.5	C	394	2.2	D
224	3.4	B	268	2.8	A	309	5.0	B	351	4.4	A	395	1.5	A
225	7.6	D	269	3.4	B	310	3.4	C	352	2.2	D	396	1.6	B
226	5.6	C	270	2.2	C	311	4.0	B	353	1.8	D	397	2.0	C
227	5.4	A	271	2.4	D	312	3.0	C	354	2.8	B	398	3.4	B
228	7.6	B	272	2.0	A	313	3.8	A	355	4.6	A	399	1.4	A
229	5.4	C	273	2.2	A	314	4.2	D	356	2.6	C	Trials 400–		
230	3.8	A	274	2.4	C	315	3.8	D	357	4.8	B	404, puzzle		
231	4.0	B	275	3.8	B	316	3.0	C	358	2.4	A	put together.		
232	2.6	B	276	3.0	D	317	2.8	B	359	2.6	D	Second test,		
233	7.0	C	277	3.6	B	318	4.0	A	360	2.6	C	tossed.		
234	3.2	D	278	2.4	B	319	5.0	A	361	2.6	A	405	13.2	
235	2.6	A	279	2.6	C	320	5.8	C	362	2.4	B	406	3.8	
236	3.8	B	280	2.6	A	321	3.8	B	363	20.0	D	407	6.0	
237	2.8	D	281	3.6	D	322	2.8	D	364	2.2	C	408	12.0	
238	3.2	C	282	2.6	A	323	4.0	C	365	3.4	B	409	7.0	
239	3.8	A	283	2.8	C	324	7.2	B	366	2.8	A	410	12.0	
240	3.8	B	284	4.8	D	325	3.2	A	367	2.2	D	411	4.2	
241	2.6	D	285	2.2	D	326	3.6	D	368	3.8	C	412	11.4	
242	2.2	A	286	2.2	A	327	5.0	C	369	2.2	A	413	9.0	
243	2.4	C	287	1.8	C	328	3.0	B	370	2.6	B	414	19.0	
244	2.4	B	288	2.0	B	329	2.6	A	371	4.6	D	415	19.4	
245	3.0	A	289	3.8	C	330	5.0	C	372	2.6	C	416	7.2	
246	2.4	D	290	3.0	B	331	3.2	A	373	4.9	D	417	35.6	
247	3.4	C	291	2.2	A	332	5.8	B	374	2.4	B	418	8.2	
248	2.6	B	292	3.4	D	333	3.0	D	375	2.2	A	419	6.0	
...........[2]			293	2.2	B	334	3.4	C	376	2.8	C	420	27.6	
249	4.6	C	294	2.6	C	335	2.4	A	377	3.0	B	421	21.6	
250	2.8	B	295	2.0	B	336	3.8	B	378	2.4	D	422	7.4	
251	2.8	A	296	1.8	A	...........[3]			379	2.6	B	423	3.0	
252	12.2	D	297	2.0	C	Eyes closed,			380	2.4	C	424	3.8	
253	2.6	B	298	2.4	B	fingers near.			381	2.4	A	425	19.2	
254	2.5	A	299	2.4	D	337	5.8	C	382	1.8	D	426	8.4	
255	2.4	C	300	2.0	A	338	5.8	D	383	1.8	B	427	5.0	
256	3.0	A	301	2.4	B	339	6.6	B	384	2.0	D	428	11.4	
257	2.5	D	302	2.4	C	340	4.8	A	385	2.4	A	429	15.6	
258	3.2	B	Eyes closed,			341	2.4	D	386	2.0	C	430	14.4	
259	3.6	C	fingers near,			342	2.4	A	387	2.0	B	431	4.2	
260	2.6	D	not in			343	4.6	C	388	3.4	C	432	6.0	
261	2.4	B	position.			344	2.8	B						

[2] One day later. [3] Eight-hour interval.

The subject worked with eyes open except in the cases specially designated, 61–69, 127–141, 337–399.

The times for putting the puzzle together are as follows: 400, 7.0; 401, 3.8; 402, 3.8; 403, 3.8; 404, 5.0.

TABLE XII

Jiu Jitsu Puzzle. *Re* Subject

Puzzle put together right (R) or left (L) and then tossed up in the air to fall by chance.

1	415.0		51	35.6	L	101	10.8	R	151	6.4	L	201	6.8	R
2	98.4		52	23.6	L	102	53.0	L	152	10.6	L	202	4.0	L
3	749.0	L	53	5.4	L	103	9.0	R	153	8.8	R	203	6.0	R
4	600.0	L	54	10.4	R	104	17.4	L	154	17.4	L	204	16.6	L
5	110.0	L	55	3.4	R	105	43.0	L	155	7.6	R	205	7.4	R
6	203.0	R	56	12.0	L	106	7.6	R	156	12.0	L	206	10.0	L
7	108.0	R	57	27.0	R	107	18.4	L	157	4.4	R	207	8.8	R
8	642.0	R	58	9.4	R	108	3.6	R	158	2.6	R	208	3.4	R
9	263.0	L	59	7.4	L	109	6.4	L	159	9.4	L	209	32.0	L
10	162.0	R	60	5.6		110	4.6	R	160	12.0	R	210	5.2	L
11	63.0	R	61	5.0		111	15.0	L	161	7.0	L	211	2.0	R
12	118.0	L	62	2.8		112	9.6	L	162	4.4	R	212	6.4	L
13	23.0	R	63	64.0	L	113	3.6	L	163	4.0	R	213	3.2	R
14	37.0	R	64	14.6	L	114	14.0	R	164	13.4	L	214	9.0	R
15	568.0	L	65	7.4	R	115	24.0	L	165	3.2	L	215	5.4	L
16	62.0	L	66	8.6	L	116	25.0	L	166	8.8	R	216	9.2	L
17	46.0	L	67	12.4		117	20.0	R	167	5.2	R	217	8.6	L
18	91.0	R	68	7.6		118	13.0	R	168	10.6	L	218	14.6	L
19	75.0	R	69	4.4		119	8.6	L	169	8.0	L	219	3.0	R
20	505.0	L	70	9.0	L	120	12.0	L	170	4.4	R	220	11.8	L
21	49.0	L	71	26.6	L	121	6.0	R	171	5.6	L	221	4.8	R
22	133.0	L	72	4.4	L	122	8.4	R	172	2.6	R	222	5.2	R
23	16.0	R	73	41.4	R	123	7.6	L	173	10.8	L	223	10.4	L
24	154.0	L	74	5.0	R	124	30.4	L	174	15.4	L	224	1.4	R
25	96.0	R	75	10.2	L	125	2.0	R	175	6.0	R	225	6.4	L
26	16.0	R	76	10.6	L	126	11.0	R	176	6.4	L	226	6.4	R
27	15.0	R	77	7.4	L	127	16.8	L	177	6.8	R	227	8.2	L
28	130.0	L	78	12.6	R	128	3.0	R	178	5.8	L	228	12.2	L
29	53.0	R	79	7.2	R	129	8.6	R	179	5.4	R	229	6.6	R
30	50.0	L	80	8.8	L	130	14.0	R	180	7.4	R	230	7.0	L
31	11.0	L	81	7.2	R	131	43.0	L	181	15.0	L	231	2.4	R
32	50.0	L	82	4.4	L	132	8.8	L	182	8.2	R	232	6.4	L
33	51.0	L	83	16.4	L	133	6.2	R	183	5.8	L	233	9.0	R
34	8.4	R	84	56.0	L	134	8.8	L	184	8.8	R	234	13.4	L
35	77.0	L	85	10.0	L	135	6.0	R	185	3.0	R	235	8.2	R
36	12.6	R	86	8.6	L	136	6.6	R	186	6.6	L	236	6.2	L
37	31.0	L	87	41.6	L	137	17.0	L	187	12.4	L	237	4.0	R
38	29.0	L	88	7.0	L	138	13.6		188	3.2	R	238	7.0	L
39	24.0	R	89	15.0	R	139	2.3		189	7.4	L	239	3.6	R
40	68.0	L	90	11.0	R	140	14.0	R	190	4.0	L	240	11.0	R
41	34.0	R	91	7.0	R	141	9.6	R	191	4.8	R	241	5.4	L
42	13.0	L	92	11.4	L	142	19.0	R	192	7.4	R	242	3.0	R
43	16.0	R	93	5.6	R	143	6.8	L	193	2.4	L	243	13.8	R
44	68.0	L	94	45.4	R	144	1.6	R	194	13.0	R	244	7.8	L
45	16.0	L	95	16.4	L	145	7.4	L	195	8.4	L	245	4.6	L
46	32.0	R	96	6.4	L	146	3.0	R	196	5.8	L	246	5.4	R
47	34.0	R	97	7.0	R	147	30.0	L	197	5.4	R	247	5.2	L
48	10.4	L	98	8.4	L	148	6.6	L	198	11.4	L	248	11.6	R
49	6.6	R	99	8.2	R	149	8.2	L	199	4.0	R	249	4.2	L
50	4.0	R	100	9.4	R*	150	8.4	R	200	10.6	L			

* 24 hour interval.

(2) *Rr*, acting as both subject and operator, solved the Double-hinged Dart and Ring puzzle seventeen times before noting explicitly that there was a right and left form solution. This analysis was made explicit during the eighteenth trial; the difference had been vaguely felt previously, and the explicit analysis seems to have been aided by the similar analysis previously made for the Jiu Jitsu puzzle. In the next three trials the subject tested his ability with the right-hand method, the A position. He then practised 139 times with the left or B position, and then took a test and practise series with the A position, trials 161 to 200. As *Rr* was both subject and operator this series is different from the others in that it includes the times for putting together, given in separate columns, as well as that of taking apart. The times of the three A trials of the first test were 22, 21 and 20 seconds, average 21 seconds; if the time for the trial just preceding, in which the difference between the A and B positions was worked out, be included, the average of the four A's amounts to 27 seconds. The average of the first twenty of the second A series was 71.7 seconds, the average of the first three being 93 seconds. The average of the first twenty of the B series was 53 seconds and of the last twenty was 10.1 seconds. The B process was thus highly perfected, but the times for the second A test are from three to four times as high as those of the first test. The transformations required in the case of this puzzle are very complex, although the general plan of solution is simple. The detailed moves in the B solution were analyzed out very well, but the whole series of movements involved were not worked up into a single unified mental construction. Consequently the subject was not able to construct a solution for the A position on the basis of the one for B. This might have been done, since the processes are symmetrically related, if the construction had been made for B. In consequence of this failure there was no idea to control the motor habits, and there was decided conflict, or "negative" transfer, as the result of the training with the B series.

(3) *Wh* was tested with three trials with a puzzle constructed by interlacing two "Gem" paper fasteners. He was then given a practise series of 109 trials and then again tested with the initial position. The practise series was so arranged that by means of part movements arranged from simple to complex there was direct training for the manipulation required in the second test. All that was needed for rapid manipulation in the second was that the second test position be seen to involve but a single slight initial movement in addition to those used with the member of the practise series just preceding. When the second test series was given, the position was not, however,

TABLE XIII

Double-hinged Dart and Ring. *Rr* Subject

	Off	On	Off	On	Off	On	Off	On
1	89.0	72.0	23.0	8.5	10.5	7.5	12.5	6.5
2	70.0	72.0	10.0	4.0	13.5	5.0	11.0	5.0
3	35.0	40.0	40.0	4.0	26.0	12.5	10.0	5.0
4	47.0	21.0	13.0	6.0	10.0	8.5	9.0	26.0
5	27.0	18.0	37.0	5.5	14.0	7.5	8.0	6.0
6	72.0	18.0	40.0	21.0	10.0	10.0	8.5	6.5
7	180.0	26.0	50.0	7.5	20.0	8.5	8.0	5.0
8	21.0	26.0	41.0	4.5	12.5	4.5	11.5	6.5
9	65.0	17.0	16.0	5.5	30.0	5.0	8.0	7.5
10	60.0	25.0	13.0	7.5	10.0	4.5	8.0	4.5
11	70.0	13.0	13.0	11.0	120.0	5.5	47.0	5.0[5]
12	32.0	33.0	28.0	7.0	15.5	6.0	165.0	3.5
13	33.0	10.0	11.0	7.5	13.0	8.5	67.0	10.0
14	31.0	13.0	15.0	6.0	22.0	4.5	30.0	7.5
15	120.0	16.0	16.0	6.0	18.0	5.5	27.0	7.5
16	20.0	20.0	19.0	5.5	21.0	3.5	205.0	6.0
17	50.0	13.0	19.5	4.0	17.5	5.5	210.0	8.5
18	45.0	13.0[4]	19.5	5.0	12.5	5.0	207.0	6.0
19	22.0	6.0[4]	22.0	4.0	12.0	3.5	95.0	7.0
20	21.0	12.0[4]	15.0	4.5	13.5	4.0	22.0	8.0
21	20.0	11.0[4]	11.5	6.0	21.5	13.5	18.0	8.0
22	110.0	11.0	12.0	4.5	11.5	6.5	50.0	12.0
23	26.0	6.5	19.0	7.0	8.5	12.5	60.0	6.5
24	35.0	8.5	17.5	5.5	30.0	5.0	60.0	12.5
25	36.0	8.5	21.0	7.0	12.5	10.0	80.0	6.0
26	53.0	9.0	11.5	10.0	8.5	7.0	35.0	8.0
27	43.0	6.0	11.5	10.0	11.0	4.5	21.0	6.0
28	120.0	11.0	22.0	4.5	13.5	8.5	55.0	6.0
29	105.0	12.0	28.0	6.5	11.5	3.5	19.0	6.0
30	260.0	12.0	19.0	5.5	11.5	6.5	60.0	8.0
31	21.0	10.0	35.0	7.0	18.5	5.5	17.5	7.5
32	40.0	6.5	17.0	6.0	10.0	6.0	18.0	8.0
33	38.0	7.0	18.0	6.0	9.0	3.5	18.0	8.5
34	21.0	9.0	13.0	5.0	22.0	8.0	22.0	7.5
35	35.0	8.0	13.0	5.5	12.0	14.0	12.5	10.0
36	37.0	5.0	22.0	8.5	9.0	4.0	18.0	6.0
37	20.0	6.5	20.0	7.0	11.5	7.5	12.5	5.0
38	21.0	7.5	15.0	11.5	10.0	10.0	12.0	4.5
39	18.5	6.0	21.5	6.5	8.0	10.0	17.5	12.0
40	18.0	11.0	42.5	12.5	10.0	5.0	23.0	6.5
41	22.0	10.0	12.0	5.5	9.0	10.0	22.0	8.5
42	20.0	8.5	10.0	7.0	8.5	10.0	45.0	5.0
43	12.0	8.5	13.0	3.5	8.5	3.5	14.0	7.0
44	17.0	6.0	13.5	5.5	11.5	5.0	13.5	5.0
45	7.0	6.5	12.5	4.0	8.0	6.5	11.5	6.5
46	13.0	6.0	22.0	8.0	11.0	12.0	8.5	5.0
47	25.0	10.0	16.5	6.0	9.0	7.0	17.5	7.5
48	11.0	5.5	17.5	4.5	17.0	6.5	17.5	5.5
49	16.0	5.5	13.0	4.0	7.5	10.0	15.0	7.5
50	18.0	7.5	8.5	4.0	8.5	9.5		

First test, trials 18–21, practise series 22–170, second test 171–199.

[4] First test.

[5] Second test.

recognized to be the same as the initial position, but was thought to be something altogether new. The subject, consequently, failed to make use of the motor habits developed in the practise series. The times for the three trials of the first test were: 17.4 seconds, 10.6 seconds, 20 seconds; the times for the first three trials of the second test were: 32.4, 15, 10.4 seconds, the averages being respectively 16 and 19. The average for the entire last ten constituting the second test series was 15.2 seconds.

(4) The Chinese Ring puzzle is admirably adapted for use in transfer experiments in that the number of rings can be changed at will, and because the manipulation varies with each change, but in such a way that a single formula might be developed from the solution of any given case which would hold for all possible cases. Five arrangements of the puzzle were employed—(1) four rings, (2) five rings, (3) six rings, (4) seven rings, (5) ten rings. Each arrangement involved all the movements of all the forms preceding its own and some in addition. The number of movements necessary increased very greatly with each additional ring.

The tables show the records for eight subjects for the four-ring arrangement. Fifty trials each were taken with the puzzle in this form. Six subjects completed series with the five-ring form. These were carried far enough to get the method of manipulation well established for this form. Of the two subjects who do not appear in the table for the five-ring form one left the university and the other failed to solve. The subjects for the six-ring type are the same as for the five-ring type with the addition of *Wh*. He began with the six-ring form. Records for the seven-ring form were taken with three subjects and for the ten-ring form with four subjects.

The record of *Rd* is unique for the completeness of the transfer from one form to the others. This may have been due in part to previous experience with another form of the Chinese puzzle. She had seen this other form fifteen years before. She had seen others work it and vaguely remembered the rule. She had not been interested in it, and was quite positive that she had never worked it herself. Nevertheless, the manipulation seemed familiar. If she had not worked it previously her present performance was certainly remarkable, for she not only did not make mistakes as to principle, but she reacted in large units directly whereas it was necessary for the other subjects to think out the detailed steps separately. In trial 1, four-ring form, *Rd* stated the rule vaguely; in trial 2 she described the process of solution explicitly, and in trials 5 and 7 worked out and described some short cuts. These short cuts were adopted at once and were maintained. *Rd* thus initiated the right habits of

TABLE XIV

CHINESE RINGS. FOUR-RING FORM

	Bg	Co	Mc	St	Rd	Ta	Tz	Bs
1	2,525.0	14,553.0	561.0	3,035.0	115.0	977.0	7,576.0	420.0
2	2,608.0	252.0	508.0	512.0	34.0	1,322.0	315.0	471.4
3	766.0	193.6	89.2	163.4	59.6	1,638.0	344.0	455.0
4	311.0	341.0	282.0	73.6	21.4	79.6	525.0	1,519.0
5	2,382.0	334.0	146.6	217.0	15.2	275.0	236.4	154.0
6	232.0	805.0	6,302.0	25.8	30.7	95.4	154.0	371.0
7	45.0	532.0	127.6	37.6	15.8	187.4	2,388.0	320.0
8	61.0	96.0	181.0	31.2	12.8	39.0	244.0	67.4
9	74.0	58.0	50.0	25.4	11.2	73.4	2,738.0	97.4
10	62.0	205.0	34.0	36.4	10.0	41.6	365.4	87.0
11	23.4	54.4	24.0	42.0	20.4	37.4	408.8	115.0
12	29.0	211.0	28.0	39.2	11.6	88.6	710.0	344.6
13	26.0	125.0	40.6	36.6	11.6	34.5	162.0	81.6
14	26.4	88.0	30.4	26.8	13.0	35.0	89.0	86.0
15	20.0	44.6	20.8	27.0	10.0	25.0	64.0	45.6
16	82.6[6]	108.0	48.8	31.6	12.3	22.0	41.6	55.0
17	50.0	245.0	28.4	19.8	11.9	29.6	96.4	60.8
18	29.0	89.0	24.0	30.0	12.6	86.4	56.6	169.4
19	25.2	111.0	34.4	25.4	15.3	19.6	33.0	34.0
20	26.6	129.0	24.0	24.0	10.3	18.0	25.0	135.6
21	20.0	65.0	22.6	22.0	11.3	17.0	39.7	142.6
22	27.0	131.0	23.0	19.6	13.4	28.2	27.5	32.6
23	20.8	138.0	21.0	89.0	11.8	20.4	29.8	25.0
24	19.0	72.6	22.0	20.8	15.3	32.0	19.6	23.0
25	17.0	32.8	16.6	29.4	12.4	22.2	19.2	34.6
26	28.0	35.0	14.6	62.6[7]	11.7	17.2	37.0	23.8
27	16.4	36.0	16.4	61.4	9.5	15.6	17.4	28.6
28	18.2	26.4	14.2	28.6	11.2	11.2	17.6	22.0
29	20.4	15.0	15.0	32.0	9.0	15.0	20.1	19.6
30	32.6	18.8	14.0	23.4	13.0	10.4	19.5	23.4
31	15.5	23.8	24.0	19.4	7.6	12.2	34.8	22.0
32	22.4	72.0	17.2	27.8	8.3	14.2	16.0	20.4
33	21.8	18.4	43.2	27.8	10.2	16.8	31.0	21.4
34	25.4	21.4	17.6	19.6	13.7	25.0	13.2	23.4
35	18.8	12.4	14.0	17.8	10.0	12.6	16.6	29.4
36	17.4	23.4	24.4	14.0	13.8	13.7	16.8	70.4
37	15.6	16.6	25.8	19.2	11.2	11.0	16.6	38.0
38	19.4	15.0	14.0	23.0	10.1	14.9	36.0	55.6
39	18.0	16.4	18.6	18.2	7.9	12.0	14.8	41.2
40	18.4	22.4	16.8	18.0	11.9	10.5	17.9	39.0
41	27.8	23.0	22.2	18.2	17.0	13.2	18.4	55.4
42	17.4	17.4	13.4	21.2	12.9	11.2	16.8	39.0
43	21.4	23.4	14.6	39.4	10.6	11.2	16.8	39.0
44	21.2	19.4	20.6	25.0	9.3	14.5	19.9	32.6
45	20.6	21.6	12.6	19.4	8.6	11.2	19.6	33.0
46	17.6	31.2	11.2	16.0	9.0	11.2	18.3	100.0
47	18.2	18.6	17.6	13.6	9.4	14.6	16.9	18.8
48	14.2	31.0	13.0	13.8	11.7	11.2	13.3	24.6
49	20.0	16.0	19.8	12.2	9.2	9.7	20.0	23.6
50	16.2	4,500.0[8]	24.8	13.4	8.3	10.3	13.1	22.0

[6] Two days later. [7] One day later. [8] Failure.

TABLE XV

CHINESE RINGS. FIVE-RING FORM

	Bg	*St*	*Rd*	*Ta*	*Tz*	*Bs*
1	1,020.0	1,678.0	42.0	985.0	84.3	704.0
2	275.0	2,670.0	30.0	140.0	173.8	903.0
3	331.0	635.0	32.3	431.3	332.0	269.0
4	294.0	1,115.0	30.8	449.0	86.8	166.6
5	186.0	424.0	25.3	130.0	57.2	175.6
6	185.0	1,299.0	40.0	127.0	84.0	117.0
7	91.0	1,374.0	31.0	68.0	54.0	234.0
8	98.0	580.0	23.0	64.0	51.6	294.0
9	186.0	211.0	23.5	57.0	55.0	97.8
10	107.0	1,196.0	29.4	41.0	67.9	164.0
11	109.0	493.0		42.6	61.8	57.0
12	76.6	65.0		50.6	68.0	73.0
13	95.0	245.0		38.0	48.3	65.0
14	73.0	133.0		80.0	42.2	71.0
15	49.4	495.0		43.3	45.7	49.6
16	60.0	167.0		90.0*		50.4
17	49.4	231.0		51.8		54.0
18	38.2	355.0		69.5		43.2
19	54.2	77.4		58.7		82.0
20	43.0	644.0		33.3		41.6
21	56.0	207.0		52.0		50.0
22		99.0		33.0		79.6
23		211.0		41.3		46.4
24		97.0		37.3		56.0
25		92.6		31.0		52.6
26		39.8		38.8		
27		186.0		34.2		
28		535.0		32.0		
29		310.0		29.0		
30		175.0		29.8		
31		94.0		37.0		
32		113.6		39.2		
33		65.0		32.2		
34		56.0		35.0		
35		46.0		37.0		
36		65.0				
37		50.0				
38		116.0				
39		53.0				
40		43.7				
41		142.0				
42		154.0				
43		50.0				
44		60.0				
45		60.0				
46		65.0				
47		62.6				
48		84.0				
49		61.8				
50		73.7				

* 24 hours later.

TABLE XVI

CHINESE RINGS. SIX-RING FORM

	Bg	*St*	*Rd*	*Ta*	*Tz*	*Bs*	*Wh*
1	249.0	279.0	86.3	470.0	182.0	1,595.0	10,830.0
2	676.0	58.3	71.5	168.0	210.0		1,080.0
3	980.0	63.2	54.7	71.3	141.6		390.0
4	296.0	72.0		99.5	103.2		237.0
5	393.0	49.0		76.4	95.6		175.0
6	380.0	53.8		72.2	83.8		137.0
7	150.0	53.2		66.9	118.6		110.0
8	114.0	305.0		60.5	98.0		85.0
9	130.0	55.0		57.6	69.0		81.0
10		55.4		56.6	56.8		134.0
11		52.0					100.0
12		53.0					
13		47.7					
14		47.3					
15		377.6					
16		48.0					
17		47.8					
18		50.0					
19		63.0					
20		57.0					

SEVEN-RING FORM

	Rd	*Ta*	*Tz*
1	112.9	1,290.4	332.4
2		235.0	
3		206.6	
4		174.4	
5		156.7	
6		170.0	

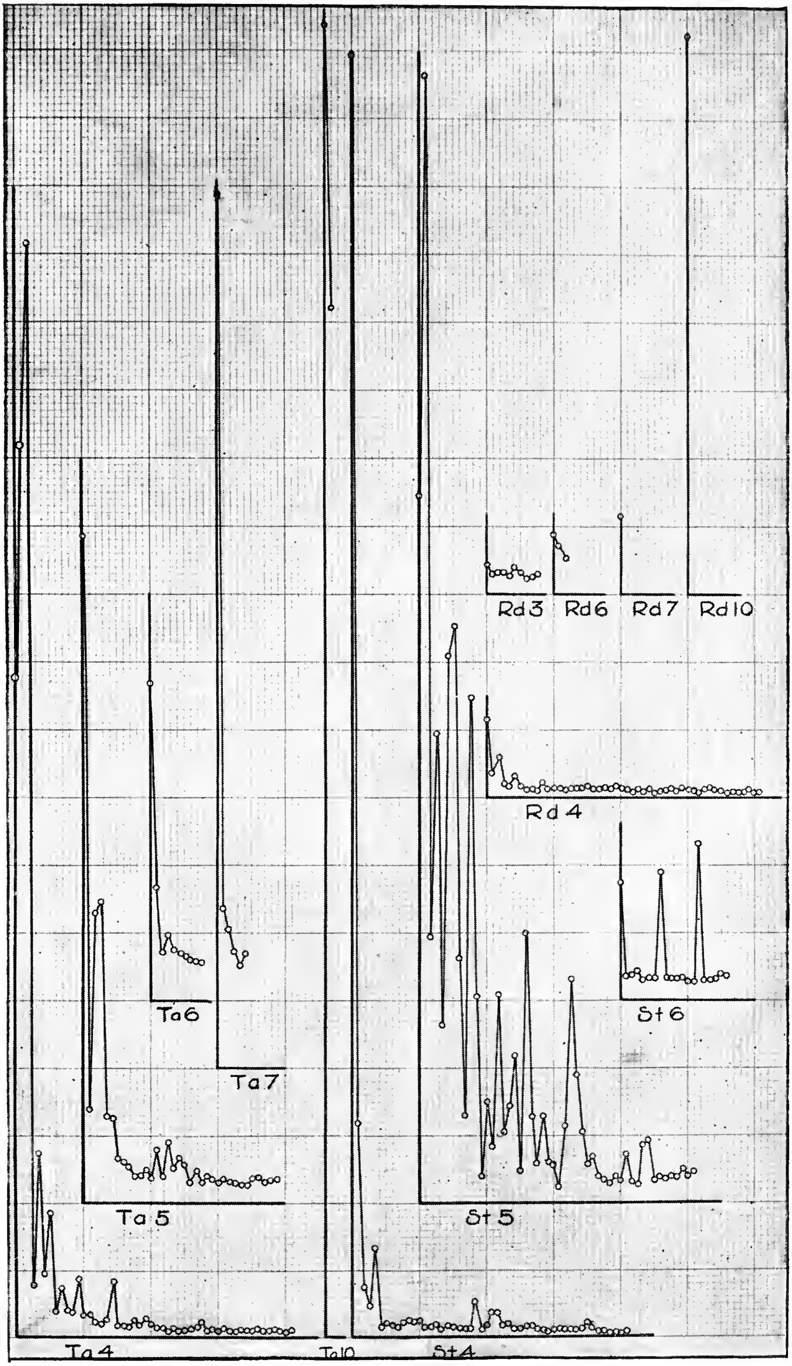
TEN-RING FORM

	Rd	*Ta*	*Tz*	*Wh*
1	824.8	1,935.0	2,142.0	1,260.0
2		1,521.0		

manipulation from the start and did not have to contend against the reappearance of erroneous methods as did the other subjects.

The records for *Co* and *St* form a striking contrast to those of *Rd*. Both of these boys moved rapidly and they were quite variable in their movements. This was especially true of *St*. Where other boys would stick, he would find a way out simply by means of his greater variability, rapidity and pertinacity. *Co* finally learned the particular combination required for the four-ring form, but he did not develop a formula for it or even see the necessity of the order of movements which he finally settled upon. At the twenty-fifth trial he began the control of one of the part processes which had previously been uncontrolled, and his curve drops abruptly here to a lower level. After having solved the puzzle twenty-four times more he, however, failed to solve it in the fiftieth trial. A chance move put the puzzle into a novel position, and he was unable to get it back, although he spent 4,500 seconds in the attempt. The solution of this novel position was an essential step in the solution of the five-ring form of the same puzzle, and he consequently failed in that.

St made an analysis in trial 6 which enabled him to avoid *Co*'s difficulty, and his curve drops abruptly to a permanently low level. His records at the close of the fifty trials with the four-ring form are the third from the best. With the change to the five-ring form, however, his records are by far the worst. The curve remains high and fluctuating until the forty-third trial, and even then his times are much longer than those of any other subject, although *St* had had the benefit of from fifteen to forty more trials with this form than the other subjects. That this result was not due to lack of variability or concrete analysis, nor to lack of motor facility, is shown by his records in the four-ring form. The reason for this failure of transfer is that *St* did not develop a general formula on the basis of the special case given in the four-ring form. The initial movements in solving the five-ring arrangement are precisely similar to those in solving the three-ring form but they are different from the first movements with the four-ring form. Now the three-ring form comes in as a part process in solving the four-ring form, and so *St* had had the opportunity to discriminate and to generalize on the basis of the discrimination. He learned to react differently to the four-ring and the three-ring combinations, and this fact enters into the explanation of what success he did obtain with the five-ring form. He reacted to the five-ring form, however, precisely as if it were the four-ring form. This only made the solution more difficult because all the steps thus initiated had to be done over again, and this method of attack led into situations from which it was very difficult to extricate himself. *St*'s method finally became stereotyped, but it included the following serious error. He would take off the first four rings as if it were the four-ring form, and then he would put them all back on again. The puzzle would now be exactly as it was to start with. He would then go ahead and solve correctly. There was transfer here, but with a vengeance. The motor habits were transferred entire, uncontrolled by any analysis of relations. There was, however, positive as well as negative transfer, since he succeeded in solving after having retraced the false steps, and the three-ring habit evidently came in on the second attempt.



CHINESE RINGS PUZZLES, NOS. 26 AND 30

4- and 5-ring forms obtained by modification of No. 30; 7-ring form obtained by modification of No. 26. *Ta* 4, 5, 6, 7, 10—scale: smallest division =10″; *Rd* 4, 5, 6, 7, 10—scale: smallest division=10″; *St* 4, 5, 6—scale: smallest division=16″.

This result with *St* illustrates the point made in an earlier chapter, that a single practise curve is not sufficient as a mental test but that it should be supplemented with other curves on variants of the original test, and that by decreasing the points of similarity between the original problem and its variant the difference in capacity between subjects can be more rigidly tested.

It might seem surprising, at first, that *St*'s records with the six-ring form should be lower than with the five-ring, despite the fact that the addition of one ring approximately doubles the total time of manipulation. It is also noteworthy that in the six-ring form his low times are lower than those of any other subject, including *Rd*. *Rd,* to be sure, made only three trials, but *St*'s time for trial 5 is considerably lower than *Rd*'s for trial 3. This again shows that *St* was not lacking in speed or accuracy of movement, but excelled in them. The reason for his success with the six-ring form is that it is solved by using the same initial movements as the four-ring form. *St* carried over his four-ring habits again, but this time they happened to be appropriate. The high times in trials 8 and 15 show the lack of the fundamental principles. In each of these trials he made the same serious error four times.

The high times for trials 1–11, *Bg,* with the five-ring form were due also to carrying over the method of solution from the four-ring form unchanged. The high times of the six-ring form were due to the carrying over of the method of starting finally adopted for the five-ring form. *Bg* had become explicitly conscious of the different ways of starting necessary for the four-ring and five-ring forms, but he did not generalize so as to include the next change.

Ta did not work out the generalization as to method of beginning until trial 5 of the seven-ring form, although he had suspected it before. In each change, previous to that from the seven-ring to the ten-ring form, there had been negative transfer in beginning, *i. e.,* he had used the method appropriate to the form just preceding.

These three cases form an ascending scale in that *St* did not discriminate the beginning of the five-ring form from the four-, that *Bg* made this distinction but did not generalize it so as to include the six-ring and other forms, and that *Ta* did explicitly generalize so as to include all possible forms, although he did this so late that there was but one application.

Tz seems to have gotten the generalization earlier, in the four-ring form, but not quite explicitly and the effects of his idea were masked by the revival of inappropriate habits which had become established early in the four-ring series.

Wh began with the six-ring form instead of the four-ring form of the other subjects. He worked out the general formula of solution in trial 1 and developed the rule for the initial movement as dependent on the number of rings and as applicable to any possible number of them in his second trial. As a result of working out this formula *Wh*'s times for this form after only two trials are lower than those of another subject who had solved the puzzle in the four- and five-ring forms over seventy times but who had not developed a general formula for starting.

Some details will now be given from *Wh*'s notes. *Wh*'s analysis in trial 1 has been given above.[9]

Trial 3: "Worked strictly according to plan outlined."

Trial 4: "Same as above. . . . When first ring is on, it is only necessary to get it above the bar, not around it, before slipping the first two off." (This is a valuable short cut.)

Trial 5: "I can see that time will be saved by remembering by rote whether to start by dropping 1 or 2. So far I have always figured it out. The fourth has to come off to allow the sixth to come off, therefore the second has to come off." Already there has been some short-circuiting here, because the getting off of the fourth ring is really conceived as a unit.

Trial 6: "Same as before. Quicker start by rote. Hesitation *after* the sixth was off and figuring out of return."

Trial 7: "Same method: Rote in getting off sixth and in remembering always to start, in putting on, with first and second, then third, etc. Less hesitation about method of putting rings on—up through and over instead of over and down through as in dropping them."

Trial 8: "More effort at speed. Time saved in the return by beginning promptly at the left side and passing the dropped ring up through and over end. Still hesitations at transition points."

Trial 9: "Transitions more automatic. More feeling of knowing where I am."

After ten more trials on a subsequent day with taking the puzzle apart and twenty-two trials in putting it together *Wh* was given a five-ring form which also differed considerably in appearance from the six-ring type he had been using. He solved this without error the first time. To quote—trial 1: "A good deal like another I've tried, why not the same thing? . . . I tried the movements without much detailed examination and found that the same things could be done, though the friction was greater. There were but five rings instead of six, and therefore the beginning had to be made by dropping one instead of two as in the previous puzzle."

[9] See p. 27.

The tendency of habits to transfer inappropriately despite the understanding of the general principle is illustrated by a quotation from trial 3. "The old habit of dropping two as the first move (correct for six rings) tends to persist. Both the last time and this I started in this way, though I recovered myself in a few seconds."

Wh was later given the ten-ring form of the puzzle to solve. The solution of the ten-ring form includes the actual solution of all the lesser forms, nine, eight, seven, six, five, four, three, two as component parts of itself. *Wh* solved this the first time without error and in very quick time. *Rd* was the only one to solve it in less time and she had developed or partly revived and developed the same formulæ during her first trials with the puzzle and she had also carried through a long practise series. In solving the ten-ring form both of these subjects reacted to large units, and avoided confusions into which the other subjects, whose attention was more on details, often fell. The other subjects would forget in which direction they were going at times and so retrace their steps. One subject thus retraced a long subprocess five times in a single trial. Since the taking-off process for a given number of rings, say eight, involved putting on the first six rings, and since the putting on and taking off alternated, it was easy to become confused as to the general direction if the subject's attention were on details.

The experiments with the Chinese puzzle have some points of similarity with Judd's experiment[10] of throwing the darts at an object under water. In this case, however, the fact that there was a general formula that could be applied to all the cases was not even suggested to the subjects.

(5) The Mounted Wire Loop and Chain puzzle is a variation on the Chinese ring. The method of solution is precisely the same, but the superficial appearance is very different. The table for this puzzle shows that there were decided transfer effects from the Chinese puzzle. *Be* is the only subject who solved this puzzle who had not previously solved the Chinese ring. He had not attempted the latter, and had made very good records in the puzzles he tried. His records might, then, be taken as a standard from which to reckon the transfer gained by other subjects. Of these, the series of *Rr* shows the least transfer. He had done the Chinese puzzle but a few times and that was seven months previous to the time concerned. He had worked out the general formula of the Chinese ring in the first trial but he had not developed the important corollary as to the initial movement at that previous time. *Rr* used parts of the Chinese ring

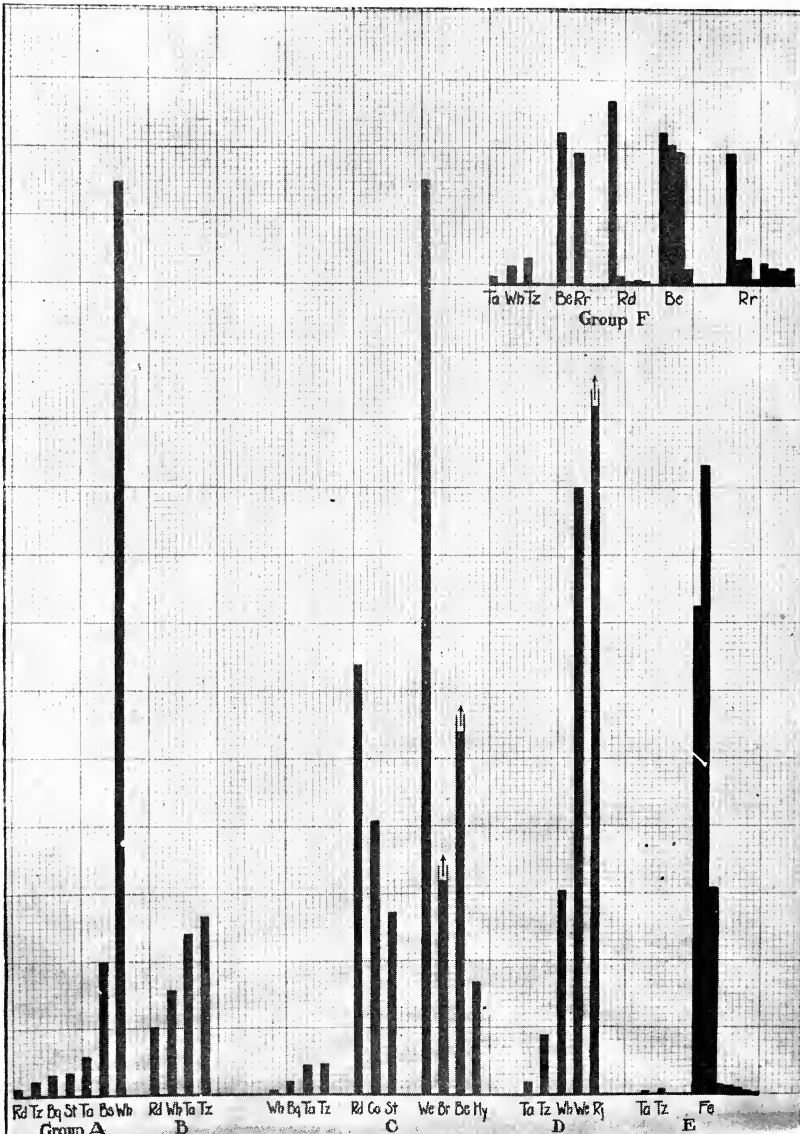
[10] *Educational Review,* Vol. XXXVI., pp. 38–42.

TABLE XVII

MOUNTED WIRE LOOP AND CHAIN

	Off *Rr*	On	Off *Wh*	On	*Rd*	*Ta*	*Tz*	*Be*
1	1,560.0	1,660.0	220.0	93.0	2,162.0	94.3	321.8	1,800.0
2	290.0	294.0	62.0	58.0	98.0	52.5	51.6	1,653.0
3	300.0	283.0	58.0	54.0	37.6	47.6	39.3	1,566.0
4	70.0	106.0	60.0	38.0	51.4	36.0	41.9	186.0
5	264.0	188.0	37.0	47.0	38.0	36.0	32.0	
6	202.9	97.0	34.0	48.0				
7	175.0	65.0	54.0	40.0				
8	195.0	89.0	40.0	32.0				
9		93.0	32.0	98.0				
10	110.0	52.0	42.0	37.0				
11	114.0	55.0						
12	121.0	61.0						
13	56.0	51.0						
14	72.0	45.0						
15	205.0	51.0						
16	53.0	62.0						
17	165.0	55.0						
18	63.0	47.0						
19	114.0	54.0						
20	58.0	44.0						
21	121.0	46.0						
22	57.0	41.0						
23	120.0	28.0						
24	54.0	41.0						
25	31.0	29.0						
26	140.0	28.0						
27	43.0	76.0						
28	38.0	27.0						
29	65.0	30.0						
30	34.0	56.0						
31	57.0	95.0						
32	78.0	29.0						
33	43.0	35.0						
34	33.0	24.0						
35	37.0	29.0						
36	32.0	120.0						
37	31.0	38.0						
38	32.0	27.0						
39	30.0	28.0						
40	35.0	26.0						
41	36.0	26.0						
42	26.0	26.0						
43	24.0	20.0						
44	30.0	30.0						
45	27.0	21.0						
46	26.0	22.0						
47	33.0	22.0						
48	26.0	21.0						
49	25.0	22.0						
50	24.0	21.0						

PLATE VII


Ta Wh Tz Be Rr Rd Be Rr
Group F
Rd Tz Bq St Ta Bs Wh
Group A.
Rd Wh Ta Tz
B
Wh Bq Ta Tz Rd Co St We Br Be Hy
C
Ta Tz Wh We Rj
D
Ta Tz Fe
E

GROUP A. CHINESE RING (6-RING)—PUZZLE NO. 30

Previous practise series on closely related 4- and 5-ring forms by all subjects except *Wh.* Transfer from 4- and 5-ring form to 6-ring shown by comparison of *Rd* or *Tz* with *Wh.*

GROUP B. CHINESE RING (10-RING)—PUZZLE NO. 26

Time of optimum manipulation of 10-ring form is ten times that of 6-ring; *Wh*'s times for the 10-ring is one ninth that of his time for the 6-ring. The transfer here is connected with a thorough analysis and a verbal statement of the rule of procedure.

GROUP C. HINGED RECTANGLE—PUZZLE NO. 12

This is a complex of several elemental devices, each being found isolated in simpler puzzles. All of these simpler puzzles were known by *Wh,* who at once perceived and utilized the similarities involved. *Bg, Ta* and *Tz* show transfer of a part of these elements; one was not known to them. *We, Br, Be, Hy,* and *Rd, Co, St* either lacked the original experiences or failed to utilize them.

GROUP D. BICYCLE—PUZZLE NO. 2

Ta and *Tz* show immediate and almost total transfer from puzzle 4; *Rj,* no transfer-failure; *Wh* and *We,* utilization of scientific method, without much transfer from related puzzles.

GROUP E. HOOK AND EYE—PUZZLE NO. 5

Ta and *Tz* showed marked transfer from puzzle No. 4. Contrast results with *Fe*'s: *Fe* recalled puzzle 4, but failed in transfer because of lack of analysis.

GROUP F. MOUNTED WIRE LOOP—PUZZLE NO. 35

See text, Transfer (5).

All records on Plate VII. are for trial 1 only except *E–Fe,* and *F–Rd–Be–Rr.*

Scale for groups A, B, C and F is 80 seconds to smallest division, and for groups D and E is 20 seconds to the smallest division.

solution as they occurred to him, but he did not revive the whole process nor make the complete identification. There was evidently some little transfer, however, if the comparison with *Be*'s record may be taken at face value.

Wh had solved the ten-ring form of the Chinese puzzle on the day previous to his solution of the puzzle under consideration. He made a complete identification of the two puzzles and carried over his methods and habits apparently without loss.

Rd had carried the manipulation of the Chinese ring puzzle to the highest perfection reached by any of the subjects, yet her initial time for the Mounted Wire Loop is longer than for any other subject. This was due to the fact that she had failed to make an explicit discrimination as to one point of detail in the earlier series. This was probably due to the fact that no difficulty chanced to arise at that point and that correct habits of manipulation were built up although they had not been based upon a careful analysis of correct and incorrect forms. *Tz* and *Ta* had both encountered the difficulty in the Chinese Ring series and they avoided it with the Loop.

After *Ta* had spent 30 seconds of trial 1 in examination he moved the puzzle to see if a novel position might not result which would suggest something. (This was a general method of his.) The similarity to the Chinese rings immediately flashed across his mind, and he called up an image of that puzzle and the rule for solving it. This rule he applied directly to the Mounted Wire Loop. *Tz* also made the identification in trial 1. The low times for the second trials of *Wh, Rd, Ta* and *Tz,* show, if compared with that of *Be,* the transfer effect from the Chinese ring.

In the case of the curves for *Wh* and *Rr* it will be noticed that times are given for taking the puzzle apart and also for putting it together, the subjects acting also as operators. Comparison of the off and on series for *Rr* will show that there was no complete transfer from one series to the other. The initial time for the on series is longer than that for the off, and the break in the on curve comes much more quickly and sharply than in the off. This drop was due to a detail analysis suggested by the Chinese rings and also from the off series. The idea transferred from the off series was that of the successive movements forming a reversal. It is interesting that this idea should seem to have been more effective in the on series than in the off. The transfer between the off and on series was here clearly due to an idea.

2. General Discussion and Summary

The following summary includes a résumé of results presented in other chapters as well as this one.

It has seemed advisable to the writer to use the term transfer in a very broad sense to include the effect of any given experience on any subsequent one whether the effect results directly or by means of an idea, whether the transfer is one of method, or of material, or of motor processes, and whether it is positive or negative. Angell[11] does not include under this topic "the transfer from one region to another of specific information useful in two or more fields (mathematics to physics)." Since, however, he includes motor habits and these appear to be pretty much on a par, considered as common elements, with information-components, the exclusion hardly seems necessary. It is not enough, manifestly, that the information elements be objectively common, they must be recognized to be so, *i. e.*, mental functioning is involved as in the case of habit; the information components and the motor habits are also similar as constituting common elements in the two experiences concerned. The conception of identical elements (Thorndike and Woodworth) does not seem to have much value as a criterion for the existence of transfer since it covers so wide a range of phenomena as: general principles of method, special information processes, and eye movements. The definition of identical elements in terms of common cell action in the brain is similarly of value only as a demand for explanation. Fracker has made the distinction between the case of identical elements, "transfer," and the case where there are no identical elements but where the elements are all different but are still related in the mind of the person concerned, "spread of training." If the relation is seen and employed, you seem, however, to have a "common element" after all, and it would appear to be better to conconsider them both as cases of transfer but to make the distinction between (*a*) transfer of the direct, necessary or reflex type and (*b*) transfer of the conditional sort, dependent on an act of analysis.

How far are ideals of methods of thinking developed in connection with one kind of subject-matter applicable to other kinds? Judd and Angell each emphasize the interdependence of mental functions, but both seem to question the extension of methods in any considerable degree to fields not closely related materially. "Unhappily the type of analysis and inference which is valid in mathematics is very different (practically) from that which is valid in linguistics and history" (Angell). "There can be no single factor in all of the scientific man's methods of thought unless, indeed, the man himself be the identical element" (Judd). The latter statement may have particular application, however, to the conception of "identical elements" rather than to the matter of transfer itself.

[11] *Educ. Rev.*, June, 1908.

To the writer, the problem consciousness considered in itself and as to conditions of efficiency seems to have many characteristics in common irrespective of the degree of relatedness of the material concerned. Since in the writer's view the course of efficiency in the practise curve is largely a matter of securing the appropriate variations and of their conscious control, the problem consciousness would, from the standpoint of efficiency, be almost universal in scope and its general features or factors would serve as means for transfer of an extensive sort. The distinction was made above between direct or necessary transfer and conditional transfer, the latter involving an act of analysis. The statements above in reference to the wide possibility of transfer presuppose in the main such acts of analysis.

The following classification of transfer factors is based directly on the puzzle experiments. Some of the special factors are naturally specifically related to the puzzle series while the general factors seem to be of wide applicability. The conditions of transfer of the special factors are believed to be general although the material itself may have been specific.

A. *General Factors*

(*a*) *The Ideal of Efficiency.*—This involves the active search for methods of control, and would properly embrace all the succeeding factors.

(*b*) *Level of Attention.*—High level attention was a precondition of success. Transfer of this factor seemed to be both direct, a result of change of attitude, and indirect, a result of the idea of its value and conscious attempt to realize it, as by effort of will, control of physical condition, search for stimulus.

(*c*) *Attitudes.*—The change from the self-conscious to the problem-attitude occurred sometimes automatically, and sometimes deliberately by means of an ideal. The most powerful stimulus to change of attitude and so of its transfer was personal success. It did not matter much whether it was accidental or planned.

(*d*) *Methods of Attack.*—(1) Conscious control of assumptions. The value of explicit consciousness of the assumptions made concerning a problem and of openness of mind and active search for other assumptions than the chance first one was recognized and generalized as a point of method common to the different situations encountered.

(2) The dilemma seemed to prove itself of considerable value as a stimulus to discovery of novel points of view both as to the nature of the problem and as to minor features. The dilemma was consciously generalized as a method of attack.

(3) Active search for distinctions and for their appropriate classification took place independently of the use of the dilemma, and constituted a highly general form of method.

(4) The search for new points of view at times took the form of random manipulation, now in the hope of gaining success by an accidental variation, and, again, in anticipation of a happy suggestion from some chance position. These methods of attack were consciously generalized and applied.

(5) The careful testing of hypotheses as opposed to mere repetition was a consciously adopted general point of method.

(6) The ideal of the value of generalization, and of statement in a formula, was noted as a case of conscious transfer.

B. *Special Factors*

(*a*) *Related Ideas.*—(1) Geometrical concepts played an almost negligible part in the work of solution. This was especially true of tridimensional puzzles. What was needed was ability to construct transformations in three dimensions and the static training of geometry seemed at times even to interfere with the dynamic problem. The concept of symmetry was of some value, but in the main the transfer value of mathematics in so far as it appeared seemed to be largely in the form of general methods, as that of considering the problem solved and working the solution in reverse order. The failure of mathematical training to develop the capacity or capacities for dynamic construction was rather striking.

(2) Ideas of common objects in connection with which movement was a familiar feature, as rubber bands, were employed with some success.

(3) The greatest transfer in the way of related ideas was that from similar puzzles. Transfer of this sort also gave the most immediate solutions. The mere presence of a vivid image of a closely related puzzle was not sufficient, however, of itself. A distinct act of analysis was necessary in addition. The analysis was at times apparently due to previous experience and yet took place as an immediately perceptual act without the revival of distinct imagery.

(*b*) *Motor Habits.*—(1) The mere presence, in the case of change of conditions, of motor habits appropriate to the new conditions did not necessitate positive transfer. It could coexist with negative transfer.

(2) The degree of positive transfer varied directly with the precision of analysis of the similarity of the new case to the old. The similarity suggestion needed, as was the case with memory sug-

gestions, to be treated as an hypothesis to be held tentatively and tested rather than to be accepted at once at face value and then persisted in unquestioningly.

(3) In some cases a generalized formula developed in connection with the first case was essential to effective transfer of motor habits to later modifications of the first case.

(4) Transfer was more effective in those cases where the formula or general rule was developed in the first few trials, and where the formation of perceptual-motor habits had been controlled and interpenetrated by it from the start, than when the generalization had been arrived at after those habits had been set up.

References to Summary on Transfer

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89

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90

ON CERTAIN ELECTRICAL PROCESSES IN THE HUMAN BODY AND THEIR RELATION TO EMOTIONAL REACTIONS

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CONTENTS

ON CERTAIN ELECTRICAL PROCESSES IN THE HUMAN BODY AND THEIR RELATION TO EMOTIONAL REACTIONS

I. PHYSICAL AND PHYSIOLOGICAL

THIS report[1] deals with experiments conducted to develop the best available method of applying the so-called "psycho-galvanic reflex" to the study of the emotional reactions both in normal individuals and various forms of psychoses. The work is divided into three parts, as follows: (1) the investigation of the physiological causes of the galvanometric deflections; (2) the study of sources of error in their use arising from physical causes, and of the methods of eliminating them; (3) the testing of the value of the deflections as indicators of emotion by statistical comparison with the introspective estimates; and supplementarily, the application of the method of inquiry to the study of nervous and mental disorders.

Most of the time at our disposal was devoted to the first three portions of the problem, and the few experiments with patients were carried on rather with a view to testing the methods of application than with the hope of securing any notable psychological information.

The apparatus at the beginning of the experiments consisted of (*a*) a Leeds and Northrup, Type H, D'Arsonval galvanometer, sensitivity 38×10^{-10} ampères, (*b*) the Ayrton shunt, (*c*) non-polarizable electrodes of the calomel type, (*d*) the Gordon cell, where an outside current is used. The deflections are read direct from a millimeter scale, 2 m. distant from the galvanometer mirror.[2] The elec-

[1] The material here presented was collected almost entirely during the time between October 1 and December 1, 1909. For this reason the observations consist largely of single experiments, which seem to the writers, however, to reflect with comparative accuracy the properties of the method under consideration. In view of present conditions in the problem, their publication seems justified; cf. Dunlap, *Psych. Bull.*, VII., 174–177, also Sidis' reply, *Psych. Bull.*, VII., 321–322. The second named writer is mainly responsible for the work up to p. 26 and from this point the first named.

[2] Values are uniformly given in terms of mm. deflections. With the galvanometer at full sensibility, the current is 19×10^{-10} ampère per mm. of deflection; when cell-current is used, the current is 1900×10^{-10} ampère per mm. of deflection unless otherwise specified.

trodes were constructed of glass tubes shaped like ordinary test tubes, 3 cm. inside diameter, 15 cm. long, with the conducting wires sealed in the bottom point of each. These were fitted into holes in a pair of horizontal boards on each side of a massage table. The subject reclined on this table with the hands strapped to the boards, palms down, with the middle finger protruding downward in the holes, each being immersed in the fluid to a depth of between 4 and 5 cm. The straps were drawn tight enough to limit the motions of the hands to a small range. Moreover, the position was comfortable and there was no tendency to move the hands during the experiments. The depth of immersion, therefore, remained practically constant and with it such changes of resistance as would be caused by a change in the area of the surface immersed or by a change in the length or shape of the conducting column of fluid. In some of the first experiments, funnels with saturated cotton were inserted in the tubes; the hands were strapped down with the palms in contact with the cotton. Evaporation caused such changes in resistance that this method was soon abandoned for that of immersing the fingers. With the subject and apparatus thus arranged, a large number of experiments were carried on in which the subject responded in the usual manner to a series of association words and after a short interval graded the feeling aroused as (A) strongly emotional, (B) rather emotional, (C) rather unemotional, (F) practically devoid of emotional reaction. With this form of experiment as the basis of our method, a variety of studies were carried on to learn the sources of error psychical and physical in its use, and to modify the procedure and apparatus accordingly.

Before describing the experiments, it may be well to mention some of the disadvantages in other forms of apparatus which led to the adoption of that which is described above. Dry metal plates, which have been used in many experiments, have the serious objection that changes in pressure or in area of the surface in contact produce marked changes in the deflections, thereby confusing the picture. The use of copper plates in a strong electrolyte has the disadvantage of introducing an electromotive force greater than that produced by the body. The result of this is that in studying the causation of deflections, one is handicapped in the effort to discriminate between changes in body resistance and changes in electromotive difference of potential developed by the body. Calomel electrodes are relatively non-polarizable and with them it is often possible to conduct an experiment without the development of a difference of potential between the two electrodes greater than .0001 volt, while the difference of potential between the two hands varies from

0 to .006 volt. These values are easily calculated by measuring the resistance of the body with a current of known voltage and by proper interpretation of other observations noted below.

There has been much discussion about the nature of the physiological processes involved in the psycho-galvanic reflex. Physically, the chief point under discussion is whether the galvanometric deflections are caused by changes in electrical difference of potential or in body resistance. Physiologically, the question is of what organs are the seat of the phenomena. Peterson and Jung discuss these questions as follows:[3]

"So far as has yet been determined, it would seem that the sweat glandular system is the chief factor in the production of this electric phenomenon, inducing on the one hand under the influence of nervous irritation a measurable current or, on the other hand, altering the conductivity of the body. Since water contact excludes changes induced by pressure on metal electrodes, and blanching of the fingers by the Esmarch bandage excludes changes in connection with the blood supply, both of these factors play but a small part in the deviations of the galvanometer. Change in resistance is brought about either by saturation of the epidermis with sweat, or by simple filling of the sweat-gland canals or perhaps also by intracellular stimulation; or all of these factors may be associated. The path for the centrifugal stimulation in the sweat-gland system would seem to lie in the sympathetic nervous system. These conclusions are based upon facts at present at hand and are by no means felt to be conclusive."

Sidis and Kalmus claim to have proved that the deflections are caused by changes in potential developed within the body and to have excluded resistance changes and sweat-gland activity from playing any part.

The various possible physiological explanations may be enumerated as follows: Changes of electrical potential may be caused by "action currents" emanating from voluntary muscles, from the smooth muscles of the blood vessels, from nerve trunks, or from sweat-glands. They might also be caused by electro-chemical action between the sweat and the electrolyte in the electrodes. They might be caused by thermo-electric phenomena at surfaces of contact between electrically different substances within the tissues or at the point of contact between the tissues and the electrolyte. Resistance changes might be caused by vaso-dilation or vaso-constriction or by sweat-gland activity. In the latter case, they might result from change in the glandular tissue or from filling and distention of the sweat tubules.

[3] *Brain*, XXX., p. 158.

The assertion by Sidis and Kalmus that resistance changes have been excluded is based on the following experiments and reasoning. "Hypodermic needles were inserted well under the skin until blood flowed freely. The hands, with the needles in position, were placed within the liquid electrodes." After establishing this connection between the body fluids and the electrolyte, and thus eliminating skin resistance, deflections were obtained similar to those without needles. This is said to prove that changes in skin resistance do not cause the deflections. Body (subcutaneous) resistance within the skin is said to be ruled out as follows: "Heating and cooling the arms put in an Esmarch bandage so as to exclude circulatory variations brought about galvanometric deflections. The experiments with hot and cold applications gave but slight variations, insufficient to account for the galvanometric phenomena observed under the influence of emotional states. The variations due to raising the temperature did not differ from those due to lowering the temperature. Furthermore, after a minute or two of continuous cooling or heating the arms, the reading was the same as that before the temperature changed. The hot and cold applications acted, therefore, in the nature of mere temperature stimulations." From this it is argued that the deflections are not traceable to resistance changes resulting from heating of the body. Esmarch bandages were used to exclude circulatory changes and deflections still followed the use of stimuli. The conclusions are summed up as follows: "Our experiments go to prove that the causation of the galvanometric phenomena can not be referred to skin resistance, nor can it be referred to variations in temperature, nor to circulatory changes with possible changes in the concentration of the body-fluids. Since the electrical resistance of a given body depends on two factors—temperature and concentration—the elimination of both factors in the present case excludes body resistance as the cause of the deflections. Our experiments, therefore, prove unmistakably that the galvanic phenomena due to mental and physiological processes can not be referred to variations in resistance, whether of skin or body. Resistance being excluded, the galvanometric deflections can only be due to variations in electro-motive force of the body."

The evidence does not seem to be conclusive. The fact that when skin resistance is eliminated by the use of needles deflections still occur, shows that under those conditions change in skin resistance is not the sole cause of deflections, but it does not exclude it as a contributing cause, nor under other conditions, as the main cause. The conclusion that heating and cooling the arms merely acted as thermal stimuli is doubtless correct, but it does not follow that the deflections are independent of thermal changes in the body; for owing to the great insulating power of the subcutaneous fat and the rapid circulation of the blood, it is doubtful if the body fluids were perceptibly heated or cooled in this experiment.

In the present experiments, the relative influence of potential and resistance changes in causing deflections was studied by use of two separate methods. One was to connect the galvanometer with electrodes as nearly isopotential as possible and insert the fingers in these. The other was to connect the body and galvanometer in series with a Gordon cell of known voltage. For the first method, the various electrode tubes which had been prepared were connected with the galvanometer in pairs to show their relative potentials. This was done as follows: the wires leading from the two electrodes to be tested were connected with the two poles of the galvanometer. The ray of light from the galvanometer mirror was brought to the zero point in the middle of the scale, before the circuit was closed. Two limbs of a Y-shaped tube were then introduced into the two tubes and the electrolyte sucked up far enough to establish a fluid connection between the electrodes. With all but one of the pairs thus tried, the galvanometric deflection was over 400 mm. With the remaining pair (nos. 2 and 3) the deflection was + 50 mm. at the beginning of the experiment, and after testing each with each of the other tubes, it was — 92 mm., the change being probably the result of polarization. This pair of electrodes was used exclusively in all our first series of experiments dealing with body currents. The difference of potential between these two electrodes, which is indicated by a deflection of 50 mm., was estimated as follows: the current is 95×10^{-9} amp., and the resistance of a similar pair of electrode-tubes and the column of fluid connecting them was found with the aid of the Gordon cell to be about 1,060 ohms, to which must be added the resistance of the galvanometer, 520 ohms. By Ohm's law the electromotive difference of potential is approximately .00015 volt. This is then the difference of potential which existed between electrodes 2 and 3 at the time of this test. The current was not driven through tubes 2 and 3 for fear of polarization, but all the tubes were the same size and shape and the fluid in them of the same concentration. Their resistance was therefore approximately equal. Immediately after this difference of potential was tested the Y-tube was removed and the subject inserted the middle finger of his right hand in electrode 2 and that of his left in 3. Thus the circuit consisted of the body connected in series with the galvanometer through the two electrodes. After a series of experiments in muscular contractions, the galvanometer reading was

— 300 mm. The fingers were then reversed with respect to the tubes and the reading was then + 300. At other times similar results were obtained, as, for instance,—reading with fluid connection between electrodes — 240, reading with fingers in first position + 55, in reversed position — 98. In these experiments the fact that reversal of the fingers gives a reversal of the deflection shows that there is a difference of potential between the fingers generated in the body which is greater than that which exists between the two electrodes; and where the deflections in the two directions are approximately equal, the body potential is much greater.

The ratio between the two may be calculated as follows: The point on the scale midway between the positive and negative deflections may be assumed to be the value which the deflection would have if the electrode potential alone were acting through the resistance of the body. The ratio between the electrode potential and the body potential is equal to that between this hypothetical value and its difference from the actual deflection caused by the combined effects of electrode and body potentials.

The resistance of the body may be calculated from the experiments in which the Gordon cell was employed. With this method the sensitivity of the galvanometer was reduced to .01 of its full value. Under these conditions the deflections usually began at about 400 mm. and rose to 600 mm. in a few minutes, seldom going much beyond that value. The e.m.f. of the cell used is .67 volts. From this it is calculated that a deflection of 500 mm. indicates a body resistance of slightly over 6,000 ohms, a deflection of 600 mm. slightly over 5,000 ohms. From this, in turn, it can be estimated that under average conditions of body resistance, a deflection of 500 mm. with the galvanometer at its full sensitivity connected with the hands and without the Gordon cell, indicates a body potential of about .006 volt. This is about the largest deflection we have observed in the present experiments. The variations in body resistance under varying conditions are such that these values are only rough approximations. In some cases the resistance was found to be as great as 35,000 ohms.

Thus it is clear that a difference of potential exists between the fingers which produces a considerable deflection in a sensitive galvanometer. But it does not follow from this that changes in the initial deflection following emotional stimuli are caused by changes in potential, for, with a constant potential difference, marked changes in resistance would produce changes in deflection. However, examination of deflections produced in the word tests shows that resistance changes can not alone account for the phenomena, as the deflections sometimes increase and sometimes decrease in response to stimuli, and, furthermore, they often cross the zero point, indicating an actual reversal of the current; and in general the size of the reaction deflection seemed to be independent of the amount of the initial deflection. Clearly then, the deflections following stimuli are due in part, if not wholly, to changes in the electro-motive difference of potential between the immersed fingers.

To study the influence of resistance changes, the second method was used, namely, the introduction into the circuit of the Gordon cell in series with the body and the galvanometer. Owing to the magnitude of the current, the sensitivity of the galvanometer was reduced one hundred-fold by means of the shunt. As stated above, the deflections thus caused ranged generally between 500 and 600 mm. If a series of word stimuli was then tried, the deflections which resulted invariably consisted in an increase in the initial deflection. Their average magnitude was between 10 and 20 mm. and they rarely exceeded 60 mm.; thus the maximum deflections indicated an increase of about 10 per cent. in the current passing through the body. At first sight it might appear that here also the deflections are due to electrical potential in the hands augmenting the current of the cell, but the fact that the deflections here are all in one direction, while with isopotential electrodes they may be in either, is much against it. Furthermore, a little quantitative study shows that a difference of potential of the magnitude detected with isopotential electrodes could not possibly produce such large deflections as are seen with the cell current. The galvanometer being at .01 of its full sensitivity which is employed with isopotential electrodes, it would require 100 times as much e.m.f. to produce a given deflection as it does to produce the same deflection at full sensitivity, and yet the deflections in the two cases are found to be of about the same magnitude. With isopotential electrodes and the galvanometer at full sensitivity, the deflections following stimuli rarely exceed 100 mm. It would require 30 times the e.m.f. which this indicates to produce a change of 30 mm. with the sensitivity reduced, and yet 30 mm. is by no means an infrequent deflection with the Gordon cell. It follows from this that electro-motive changes can not account for the deflections observed when a strong outside current is used. In short, *emotional reactions are accompanied both by changes in the difference of potential between the immersed fingers and by changes in the resistance of the body.*

The physiological basis of the electro-motive phenomena should be considered under the headings already enumerated. "Action currents" in voluntary muscles are a very improbable cause, for contractions of the voluntary muscles are not felt by the subject to occur during the experiment and if they existed they would not be likely to escape detection. In a special experiment, the effect of contracting voluntary muscles was studied as follows: The subject, lying motionless with the hands strapped in place and the fingers in the isopotential electrodes, vigorously exerted antagonistic muscles in the arm without moving it, and gave simultaneously a verbal signal. The observer noted and recorded the deflections. Deflections regularly followed the exertions, differing from those of emotional stimuli chiefly in having a shorter latent period. They followed the exertions by an interval of a second or less, whereas in the case of emotional reactions the interval is about three seconds. In this series, six contractions were made in the right arm and seven in the left. In every case, the deflection following was in the direction which indicated a relative fall of potential in the finger of the arm in which the muscles were exerted. The experiment was repeated with two subjects, one new to the experiment, and the results were almost uniformly consistent with the foregoing. This harmonizes with the view that the deflections in this case were the result of the "action currents" of the muscles, for muscular contraction is characterized by the release of negative electrical charges. The deflections in these experiments were rarely greater than 40 mm. and yet the muscles were exerted vigorously. It is, therefore, extremely improbable that contractions of voluntary muscles so slight as to escape the subject's attention would produce "action currents" great enough to give the deflections of 50 mm., sometimes over 100 mm., which follow emotional stimuli. Voluntary muscle, therefore, may be excluded with a reasonable degree of certainty.

Smooth muscle fibers in the blood vessel walls might give off "action currents" that would produce the deflections and the curve of their contraction is much more like the curve of the deflections than is that of striped muscle. The experiment of Sidis and Kalmus with Esmarch bandages in preventing effective vaso-dilatation probably had little effect on the vaso-motor nerve impulses or on the resulting release of muscular energy; so that "action currents" may still have emanated from the vascular walls. However, other evidence led us to believe that this plays at most a minor part in producing the deflections, as will be explained later. "Action currents" in nerve trunks were not available for separate study. Like those in the vascular walls, we could neither prove nor disprove their influence, but believe that they also play at most a minor part.

The remaining tissues whose "action currents" might cause the deflections are the glands in the skin, of which the sweat-glands are the most numerous and active. The literature concerning glandular "action currents" is not altogether satisfactory. Lillie,[4] in presenting a hypothesis to explain "action currents" on the grounds of relative permeability to ions in cell membranes, implies that cell activity in general is attended by a release of negative charges. Loeb[5] implies the same thing. It is not clear whether the generalization is based on muscles and nerve alone or whether glands have also been shown to release negative charges. Waller,[6] in "The Signs of Life," shows that in a frog's skin the "action current" is outgoing, that is, the outer surface has a higher potential than the inner. He confuses his electrical terminology and his explanation is unsatisfactory. Bayliss and Bradford[7] showed peculiar variations in the "action currents" of the skin of frogs related to the season of the year. It seems that these phenomena are too obscure to be of value as a basis of comparison with the phenomena in human skin. Mendelsohn[8] in the "Dictionnaire de Physiologie" reports similar findings in the frog's skin, but states that in the paw of the cat, when the sciatic nerve is stimulated, there is an in-going "action current" accompanying the secretion of sweat. Waller also quotes the previous observation of Tarchanoff, that under stimulation of a variety of types, "parts of the skin in which sweat-glands are most abundant become negative to parts containing few glands" (Lecture VII., p. 124). Thus experimental evidence concerning mammalian skin containing sweat-glands shows that in action the surface potential is relatively lowered. This agrees with Lillie's implied generalization concerning the release of negative charges in active tissues, and it is probable that human sweat-glands would show similar "action currents."

If the negative charges released from the sweat-glands produce electro-motive phenomena, we should expect the difference of potential between the hands to be slight, since the sweat-glands of the two are, as far as we know, alike and should produce approximately equal electrical charges. The difference of potential should occur only when the glandular activity in one hand is for some reason greater than that in the other, and should in most cases cause de-

[4] R. S. Lillie, "The General Biological Significance of Changes in the Permeability of the Surface Layer or Plasma-membrane of Living Cells," *Biological Bulletin*, Vol. XVII., pp. 188–208.

[5] J. Loeb, "Dynamics of Living Matter," pp. 68–69.

[6] Richet's Dictionnaire de Physiologie, Mendelsohn's article on "Electricité," Vol. V., p. 350.

[7] Bayliss & Bradford, *Journal of Physiology*, Vol. VII., p. 217.

[8] Waller, "The Signs of Life," Lecture IV.

flections in one direction as often as in the other. This is precisely what occurs in a series of reactions; the stimulus may be followed by a positive or negative deflection. Neither seems to predominate; the galvanometer may cross the zero point several times during an experiment. It is, however, characteristic of the deflections that they tend to be in the same direction as their immediate predecessors, that is, there is apt to be a fairly long series of consecutive positive deflections, and when after some irregularity a negative deflection appears, it is apt to be followed by a series of negative deflections. While this picture agrees with what we should expect if sweat-gland activity were the cause, we should expect it just as much if the "action currents" came from the muscles of blood vessels. Some distinguishing test should be found. Two methods of differentiating were tried; one was to compare the electrical potential of portions of the integument known to differ in the abundance of sweat-glands; the other was to ascertain the influence of drugs on the phenomena.

First we attempted to eliminate the sweat-glands from the electrodes altogether by the method described by Sidis and Kalmus, coating the immersed skin with shellac and paraffin, leaving only the finger nails uncovered, and comparing the result with that obtained with the bare skin. The middle fingers were covered with two coats of shellac and one of paraffin. As a control experiment, the fingers thus coated were inserted into the electrodes before the paraffin was scraped from the finger nails. A deflection of + 5 mm. resulted, and on reversing the fingers with respect to the electrodes the deflection was reversed to — 6 mm. This showed that even this insulation was not complete, since the difference of potential between the fingers caused a measurable deflection. With the fingers in position, the effect of a strong stimulus was tried. An operatic graphophone record was used which had previously given rise to large deflections under the usual experimental conditions. At the climax of the piece, where the subject was aware of the most intense affect, a deflection of 2 mm. was noted. This observation suggests that the conclusion of Sidis and Kalmus that "the skin has little or nothing to do with the phenomena" (because when the skin is covered with shellac and paraffin, leaving only the finger nails exposed, deflections are still obtained), is not necessarily correct. Even if the insulation had been perfect, their experiment would have proved only that something else besides sweat-glands could produce deflections; it could not prove that sweat-glands play no part. After the control experiment, we scraped the finger nails bare, leaving the skin coated and repeated the record, then repeated it a third time with the fourth fingers (not coated) in the electrodes. The deflections in each case amounted to only 1 mm., which indicated either that the skin resistance was too high or the electrical reactions too sluggish at that time to furnish any satisfactory data. This experiment seemed to us important and would have been well worth repeating had time permitted. It was immediately followed by a similar experiment with the Gordon cell current, which threw considerable light on the resistance phenomena and will be described under that heading.

At other times significant results were obtained by immersing skin surfaces with different degrees of abundance in sweat-glands. If active sweat-glands produce negative charges which cause deflections, increase of activity must cause a fall of potential in the skin, and the more active the glands, the greater will be the fall of potential. If, however, in two skin surfaces unequally supplied with sweat-glands, the difference lay only in the number of glands, the individual glands all being equally active, the two surfaces would be electrically equivalent, for the glands are arranged, so to speak, in multiple arc, and if all developed the same potential, the resultant potential of the whole surface would be no greater than that of a single gland. A surface more richly supplied with glands than another can only undergo a greater fall of potential if the glands are individually more active as well as more numerous. It is well known that the palms of the hands are richer in sweat-glands than the dorsal surfaces, whereas there is little difference in vascularity between them. This fact might enable us to secure evidence to differentiate between vascular and sweat-gland activity. The relative potentials of these surfaces were tested by the use of soaked cotton pressed into the electrodes (not in funnels). With the dorsal surfaces of the two hands in contact with the fluid of the two electrodes, the reading was — 5 mm.; with both palms in contact + 6 mm.; with the right palm and left back — 36 mm.; with the right back and left palm + 21 mm. In all these, the right hand electrode was connected with the positive pole of the galvanometer. The experiment was repeated with similar results. This indicates that in each case the palmar surface has a lower potential than the dorsal surface of the other hand and the difference of potential is greater than that which exists between homologous surfaces. This evidence accords well with the above-mentioned observations of Tarchanoff cited by Waller, in which similar differences of potential were seen to be developed under the influence of various stimuli. It distinctly favors the view that sweat-glands reduce the potential of the skin, but it does not demonstrate that the deflections which accompany emotional reactions are due to glandular activity. To test this, a certain graphophone record, which at a certain specific point always produced an appreciable emotional reaction in the subject, was repeated three times; first, while the backs of the hands were in contact with the solutions, then with both palms, and finally, with the right palm and left back. If the emotional reaction in the three cases were the same, we should expect a progressive increase of deflections, for in the more active palms there would probably be developed a greater difference of potential than in the less active dorsal surfaces, and in the third case, the difference in the fall of potential between the active palm and the less active dorsal surface should be the greatest of the three. In this case the subject reported as might be expected, that the emotional reactions were not the same, but progressively less each time. The three deflections in their order were 3 mm., 7 mm., and 3 mm. Another record was employed, first with both palms in contact, then with the right palm and left dorsal surface. Two strong affects were felt the first time, producing deflections of 5 mm. and 7 mm. The second time the corresponding points in the music caused much weaker affects, which gave deflections of 7 mm. and 4 mm. In the last of each series, where one palm and one dorsal surface were employed, the affects were marked by augmentation of the existing deflections, which fact harmonizes with the assumption that the deflections are caused by increase of sweat-gland activity. Time prevented further repetitions which should have been made with long series of association words. These experiments, though wholly inadequate for conclusions, suggest that during emotional reactions there is probably a greater fall of potential in the palm than in the dorsal surface. They should be repeated many times before substantial inferences could be drawn. The best test would be to use two galvanometers measuring simultaneously the same reactions by the two methods.

It may be well here to mention a peculiar phenomenon noted in studying the possibility of error arising from varying the depth of the immersion of the fingers. It was found that when the subject moved his fingers in and out of the tubes, varying the depth of immersion from about 3 cm. to about 6 cm., a change in the deflection occurred as follows: an increase in the depth of immersion of one finger produced a change in the same direction as a decrease in the depth of the other. This was verified by two long series of experiments on two subjects and in both the changes followed this principle in almost every case. The few exceptions, 11 out of a total of 127, may easily have resulted from affects which are not subject to control; they do not disprove the regularity of the tendency. This phenomenon can not be explained by the fact that greater immersion decreases the resistance. If that were the case, increase in the immersion of either finger would increase the deflection. The explanation suggested by Professor T. W. Richards is that near the base of the finger, the skin has a lower potential than near the top, and this appears to be the only reasonable explanation. If the electrical potential is caused by sweat-glands, it would indicate that the glands near the base of the finger are the most active.

Another experiment should be mentioned here as bearing on this phase of the question. A series of word tests was given when the subject had two fingers of the same hand in the two electrodes. These fingers were then removed and corresponding fingers of opposite hands were introduced and another series of word tests given. Again two fingers of the same hand were introduced and a third series given. The reaction deflections were of about the same magnitude in all three series, varying from 1 mm. to 20 mm. In the first series, the initial deflections were much larger than in the other two, and the reaction deflections, although of about the same magnitude, differed from those in the other two series in that they were all positive, thus being augmentations of the initial deflection which was positive, whereas in the two latter series, some were positive and some negative. This would indicate that in the first series one finger was inserted relatively deeper than the other, so that skin of lower potential was immersed. The uniformly positive reaction deflections here are significant, as tending to show that they were caused by a fall of potential in the more active skin surface and were probably produced by activity of those glands giving rise to the initial deflection. In the other two series, where the initial deflection was lower, the fingers were probably immersed to more nearly equivalent depths and the reaction deflections were sometimes positive and sometimes negative, as the activity of one or the other finger predominated. The fact that the deflections in all three series were of approximately the same magnitude is also in favor of the view that the essential activity is in the skin or superficial layers rather than in the muscles or large nerve trunks, for these would be apt to influence opposite hands far more differently than fingers of the same hand.

Further evidence on the relative influence of glandular and vascular activity was sought by the use of pilocarpine and atropine. Of these atropine should furnish the better evidence as it paralyzes the nerve endings in the sweat-glands, suppressing the secretion, while it somewhat increases vaso-dilatation, and if the essential activity is vascular, it undoubtedly consists in dilatation, not constriction, for experiments with the current from the Gordon cell show that affects are accompanied by decrease in body resistance, which could hardly result from vaso-constriction. If the deflections are chiefly caused by vascular action, the effect of atropine upon them would be difficult to predict without more detailed knowledge of its action than we possess. If it increased the susceptibility of the vessels to vaso-dilator stimuli, it should increase the deflections. If it caused them to dilate nearly to their full capacity, it should diminish deflections by limiting further dilatation. In any case, the action of atropine on the vascular system is slight and it should not diminish deflections due to vascular activity to any marked extent, whereas its paralyzing action on the sweat-glands is such that it should greatly diminish deflections caused by their activity. Pilocarpine stimulates the nerve endings in the sweat-glands, increasing the secretion; it also causes some degree of vasodilatation, which Cushny considers merely incidental to the increased activity of cutaneous glands. The prediction of its influence on vascular response to stimuli is as indefinite as it is with atropine and it is uncertain how we should expect it to influence the deflections resulting from vascular activity. Moreover, equal uncertainty attaches to the prediction of its influence on deflections assumed to result from sweat-glands; it hinges on the question whether the stimulation of the drug is of a nature which renders the glands more susceptible to other stimuli or less so. If pilocarpine merely increases the secretion of sweat without rendering the glands more responsive to other stimuli, there would be merely an approximately symmetrical fall of potential in both fingers which probably would not influence the deflections. In short, if the sweat-glands are the chief cause of deflections, atropine should greatly diminish them, while the effect of pilocarpine is not predictable. If vascular changes are mainly responsible, neither atropine nor pilocarpine should produce a very marked effect, and what effect they did produce might be either increase or decrease. The method of testing the effects of these drugs was as follows: The subject reclined as usual with the middle fingers in the electrodes and responded to a series of test words, the deflections being recorded by the observer. The drug was then administered subcutaneously without disturbing the position of the subject. After the action of the drug had begun to manifest itself, a second series of words was given. The body resistance was measured before and after the experiment by means of the Gordon cell. The experiment with pilocarpine was made with two subjects. In one subject it appeared to increase the difference in potential between the hands, in the other to decrease it; in both subjects the average magnitude of the reaction deflections was decreased. Further details are omitted as the uncertainty regarding the exact action of this drug renders the results of little significance in the question at hand. The only evidence of value in the drug experiments is that furnished by atropine.

The effect of atropine was studied by the same method as that employed with pilocarpine. In the first subject a hundredth of a grain was given after the first series of words, and after a pause of twenty minutes a second series of words was given. Then as the drug had produced no symptoms, a fiftieth of a grain was administered, and after fifteen minutes, dryness of the mouth being appreciable, a third series of words was tried. The results were as follows. The initial or resting deflection was less after the first dose of atropine than before, but after the second dose, it increased. The ratios of the body potential to the electrode potential as estimated by reversing the fingers were 40 to 26, 40 to 55, and 85 to 35, at the times of the three series respectively. But this method of inference can not be altogether relied upon, especially in this series of experiments in which electrodes were used of a design which was found to show changes in potential when subjected to disturbances such as might occur in reversing the hands. This point will be discussed more fully later. The average of the reaction deflections in the first series of 19 was 1.5 mm. In the second series of 20, after the first dose, it was .8 mm. In the third series of 24, after the second dose, it was .67 mm. In this same subject, the average deflection was reduced from 1.7 to 1.5 by pilocarpine. In the second subject, the same method was employed, but since some dryness was detected after the first dose of .01 of a grain, the second dose consisted of only .01 instead of .02. In this subject, the initial deflection decreased after the first dose, remaining fairly constant throughout the second and third series. Reversing the fingers gave such irregular results as to be worthless from the point of view of estimating body potential. This was probably the result of joggling the electrodes in shifting. The average of 24 reaction deflections in the first word series was 2.4 mm.; in the second, after one dose of atropine, the average of 21 was 1 mm.; after the second dose, the reflex was practically obliterated; only 4 out of 12 stimuli being followed by any deflections, and none exceeding 1 mm. In this subject, then, as well as in the other, the reaction deflections were reduced more by atropine than by pilocarpine. The results suggest that atropine probably reduced the difference of potential between the hands, but this is uncertain. It clearly reduced to a marked extent the deflections following stimuli.

On the whole, the findings concerning the influence of these drugs on initial or resting difference of potential between the hands are of little value. Moreover, the diminution by pilocarpine of the deflections following stimuli is of doubtful value since its interpretation for the reasons set forth above is not clear. The one significant result is the marked diminution of the reaction deflections by atropine. Since this drug has a marked paralyzing effect on the sweat-glands, but has comparatively little effect on the vaso-motor system, the evidence has some weight in the question at issue, and tends strongly to support the view that the sweat-glands are the chief source of these deflections.

The evidence has been given bearing on "action currents" of the various tissues whose activity might be involved. There remain for consideration electro-chemical activity between the sweat and the electrolyte, and thermo-electrical phenomena at surfaces of contact between electrically different substances within the tissues or at their points of contact with the electrolyte. The principal electrolyte in sweat is sodium chloride. Electro-chemical action between this and potassium chloride which surrounds the fingers in these experiments is so slight that it is highly improbable that it constitutes an important factor in the deflections under consideration. The same is true of thermo-electrical phenomena, for the substances within the tissues are such that even with large temperature changes at their points of contact, the e.m.f. developed in this way would be very slight and it is probable that the temperature change in active tissues is extremely small. That thermo-electrical phenomena at the point of contact between the tissues and the electrolyte are not of much consequence is indicated by the fact that deflections produced when the electrodes were maintained at body temperature by a thermostat were not found to differ in any way from those produced when the electrodes were at room temperature, several degrees cooler than the body.

The foregoing statement of evidence and inferences concerning the causation of electro-motive changes in the hands practically eliminates all factors except "action currents" in the muscles of the vascular system and in sweat-glands from playing any considerable part. The various facts noted all tend to support the view that sweat-gland activity is the most important factor, although none can be said definitely to prove it. None of the facts seem to oppose this view in any way. Obliteration of the deflections by atropine supports the sweat-gland hypothesis fairly strongly and tends to indicate that if vaso-motor activity plays any part, it is at most a small one.

The resistance changes must depend chiefly on vaso-dilatation or on sweat-gland activity. This assumption is due not only to the absence of any other probable cause, but is supported by the following experiment. The resistance of the body was measured by the use of the Gordon cell, with the middle fingers of the two hands immersed in the electrodes, and was found to be about 18,000 ohms. The fingers were then soaked in warm water and again inserted in the electrodes. The resistance was then found to be about 14,000 ohms. Next the two index fingers, which had not been previously soaked, were inserted and the resistance was found to be 35,000 ohms. Then the index finger and the fourth finger of the same hand were introduced into the electrodes and the resistance was found to be 31,000 ohms. It was then noted that when the index finger and the middle finger of the same hand were introduced into the electrodes, the middle finger only having been previously soaked, the resistance was higher than when the middle fingers of the opposite hands, both of which had been previously soaked, were employed. This shows that by far the greater part of the body resistance is in the skin or superficial layers. The resistance of the structures within the skin forms so small a part of the total resistance that it would have to be enormously reduced during an emotional reaction to produce the changes often noted amounting to 10 per cent. of the total resistance. It is impossible that there should be sufficient change in internal resistance to produce the observed deflections. Therefore, the cause of the resistance changes noted must lie chiefly in the surface layers.

We have then to consider the relative importance of vaso-dilatation and sweat-gland activity in the causation of the observed resistance changes. It has been noted that the deflections following emotional stimuli with the cell current always marked a lowering of body resistance. Immediately after the experiment already described (p. 10), in which the skin was coated with shellac and paraffin in the study of potential changes in the hands, the Gordon cell was introduced and a similar experiment was performed to throw light on the corresponding resistance changes. The galvanometer was shunted as usual to .01 of its full sensitivity. The fourth fingers (not coated) were inserted in the electrodes and a deflection of 195 mm. was observed. The middle fingers, coated as already described with shellac and paraffin, except on the surface of the nails, were then inserted. A deflection of 58 mm. resulted. A graphophone record was then played which at a definite point produced a fairly marked emotional reaction in the subject. At the time the affect was felt, the galvanometer reading rose from 64 mm., where it had gone at the beginning of the record, to 65 mm. The index fingers (not coated) were then inserted, and a deflection of 100 mm. resulted, rising during the first part of the music to 170 mm. and at the point where the marked affect was felt, to 210 mm. The nails of the middle fingers were then coated with paraffin, and the fingers inserted as before. The deflection resulting was 33 mm. The same record was then played, and at the point where the marked affect was felt, the galvanometer reading which had remained at 33 mm., rose to 35 mm. Another graphophone record was then played in which a still more definite emotional reaction occurred at a certain point. The galvanometer reading during the early part of the music was 26 mm. When the affect occurred, it rose to 29 mm. This was repeated without removal of the fingers. The affect was marked by a rise from 25 mm. to 28 mm. The paraffin was then scraped from the finger nails, leaving the remainder of the skin still coated and the deflection following their insertion was 136 mm. The record was repeated and when the affect occurred, the reading rose from 140 mm. to 144 mm. The index fingers (not coated) were then inserted and the galvanometer read 200 mm. The record was repeated and when the affect occurred, the deflection rose from 210 mm. to 265 mm. In these experiments, the deflections are inversely proportional to the body resistance; thus a deflection of 100 mm. indicated a body resistance of 35,000 ohms; a deflection of 25 mm., a resistance of 140,000 ohms. It is clear that very considerable conduction occurs through the finger nails, since covering them with paraffin reduced the deflection from 60 mm. to 30 mm., and scraping them bare again raised it from 26 mm. to 136 mm. The highly vascular tissues beneath the finger nails being brought into fairly close relation with the electrolyte, we should expect that if vaso-dilatation were the chief factor in the reactions, the deflections marking an affect would be about the same percentage of the total deflection as when the skin is exposed. In the first experiments with the finger nails bare and the remainder of the fingers coated, we note an increase of the initial deflection of 1 mm., or about 2 per cent. With the bare fingers, the conduction was only increased three fold, but the same passage in the music caused a deflection of 40 mm., over 20 per cent. of the total, and yet the affect was probably less in the second case than in the first. With the second graphophone record, it was shown that even when the fingers were completely coated, a deflection of 3 mm. occurred in each of two successive tests; with the finger nails scraped bare, although the total deflection was increased from 25 mm. to 130 mm. due to conduction through the finger nails, the deflection marking the affect was only 4 mm. With the bare fingers, although the total conduction was increased only in a ratio of

200 to 130, the affect (by this time distinctly weakened by repetitions of the stimulus) caused a deflection of 55 mm. Thus it is seen that increasing the conduction through a vascular region without sweat-glands did not cause an increase in the reaction deflections, whereas the exposure of skin containing sweat-glands to the electrolyte greatly increased these deflections. It is not even necessary to assume that vaso-dilatation caused the small deflections observed when the finger nails were bare, since approximately equal deflections were observed when they were covered, and may well have been caused by the sweat-glands in the skin, since the insulation has been shown to be incomplete. Thus, this experiment furnishes strong evidence tending to prove that the resistance changes are chiefly, if not wholly, due to sweat-gland activity.

The evidence furnished by drugs concerning the resistance changes was slight. In neither subject were word tests employed with the cell current in the pilocarpine experiments. In each, however, the resistance was measured before and after the administration of the drug. In one, there was a slight increase; in the other, a considerable decrease. With atropine, in only one subject was the resistance measured before and after, and in this case, it remained practically the same. In the other subject, the resistance was not measured before the administration of the drug; but at the end of the experiment, after the obliteration of the deflections caused by body currents had been shown, the cell was introduced, and the deflections following the test words were almost as completely obliterated. If the reduction of resistance is due to sweat-gland activity, we should expect a similar reduction to follow the administration of pilocarpine. That this was not so in one of the subjects, would seem at first sight to militate against the sweat-gland hypothesis. However, it is perfectly possible that the way in which the sweat-glands lower resistance is by the temporary distention of sweat tubules with secretion following a fairly sudden access of activity. It is quite conceivable that the production of sweat under the influence of pilocarpine is so gradual that the tubules carry off the sweat as fast as it is produced, and that the distention is, therefore, not appreciable. The failure of atropine to increase the resistance may likewise be due to the fact that the tubules are normally empty and the paralysis of the sweat-glands can not, therefore, make them any emptier. The fact that the sudden fall of resistance following stimuli was obliterated by atropine is strong evidence that it is caused by sweat-gland activity. Furthermore, a study of the structure of the skin suggests that on physical grounds it is probable that a marked fall of resistance could be more easily accounted for by the filling of tubules with sweat and the resulting establishment of many columns of conducting fluid than by the greater abundance of blood beneath the skin, which results from vaso-dilatation. It seems highly probably, then, that the resistance changes are caused chiefly, if not wholly, by the activity of the sweat-glands.

It has been demonstrated that emotional states are marked by electro-motive changes in the skin and by lowering of body resistance. It has been shown that the electro-motive changes probably result chiefly from sweat-gland activity and that there is somewhat greater probability that the same activity is the cause of the resistance change. The probability in both cases is reinforced by the harmony of these findings. It is further significant that the deflections produced in the two ways are similar in character. In each case, the deflection usually begins after a latent period of about 3 seconds, and rises rapidly to a maximum from which it soon starts to fall gradually toward the starting-point. There is this difference; the resistance deflections are much more regular and almost always adhere closely to this type; the deflections caused by electro-motive changes are irregular, the latent period may be prolonged and the main deflection may be preceded by a short preliminary deflection in the opposite direction; and instead of returning regularly almost to the starting-point after the maximum is reached, it may remain there, or after a pause go even higher. These facts are in harmony with the supposed difference in the causation of the two types of deflection. The tubules, suddenly distended with fluid, would be expected to empty themselves gradually and at a uniform rate, as their elastic walls contracted, and the original resistance would then be reëstablished. The difference of potential set up between the fingers by variable differences in the activity of the two skin surfaces could not be expected to subside with the same regularity.

It can not be said that anything final is established by these few experiments, where so many intricate and inseparable factors are involved; but it seems eminently probable in view of the harmony of the evidence that the psycho-physical galvanic reflex is principally the result of a single physiological activity, the secretion of sweat, which manifests itself physically in two ways, by changing the electrical potential of the surfaces of the body and by lowering the resistance of the skin.

II. SOURCES OF ERROR IN PRACTICAL APPLICATIONS

Experiments were conducted to study the various sources of error which should be met in applying the deflections to the analysis of emotional reactions. An effort was made to determine the magnitude of error which might be expected from unconscious motions of the fingers which would change the depth of immersion. The subject stood before the tubes and moved his fingers in and out of the solution, varying the depth of immersion from about 3 cm. to about 6 cm. The character of the changes resulting has been described and discussed in connection with the electrical potential of the skin. What concerns us now is the magnitude of the deflections. The maximum deflection produced by this change in the depth of immersion of one finger, after the initial changes due to soaking had ceased, was 40 mm. with one subject, and 28 mm. with the other. From this, it may be inferred that motions of the fingers which were too small to be perceived by the subject would not cause any considerable error, and even motions as great as the apparatus would allow would not produce deflections large enough to simulate or obscure the reaction deflection of a moderate affect, owing also to the differences in latent period.[1]

An attempt was made to ascertain whether a convenient means of insulation could be found which could be applied to the skin at the level of immersion, so that the surface in actual contact with the fluid should be absolutely constant. Shellac was found to be wholly inadequate, as even when the fingers to be immersed were completely covered with two coats, the first drying before the second was applied, and the second drying before testing, deflections resulted almost as great as those obtained without insulation. It has already been stated that even when a coat of paraffin was added to shellac, insulation was not complete. When the fingers completely covered by rubber cots were placed in the electrodes, no deflection was produced. The insulation was here complete. Rubber cots were then applied with their tips cut off so that a constant surface was exposed beneath the fluid. Changes in the depth of immersion here caused changes in the deflections similar to those thus caused without insulation, but much smaller. The explanation of this is difficult. If any fluid worked up under the edge of the rubber, it must have been an exceedingly thin film and its resistance very

[1] Cf. Peterson and Jung, *Brain*, XXX., p. 159.

high, yet it might conceivably account for the changes. The decrease in resistance in the fluid resulting from bringing the finger nearer to the mercury could not amount to more than 4 or 5 ohms, and this compared to the 5,000 or 6,000 ohms in the body is too small to account for the changes. It is possible that the increase in pressure from deeper immersion causes a better saturation of the skin or in some such way improves conduction. It was concluded from these experiments that the available methods of insulation serve rather to give a false sense of constancy of contact than to give any real constancy, and it seemed, therefore, better to trust to the stationary position of the hands, which is sufficiently reliable for practical purposes.

The question of thermo-electrical phenomena brings up important considerations which were dealt with as follows. The temperature coefficient of the calomel electrode is .0006 volt. That is, if one electrode becomes heated 1° C. more than the other, the potential difference between them becomes modified to the extent of .0006 volt, which is about the average difference of potential produced by the body, and .1 of the approximate maximum in our experiments. When the fingers are inserted in the tubes whose temperature is approximately that of the surrounding air, the fluid at the surface is warmed. Under these conditions, convection is not favored, and the heating of the lower portions of the tubes containing the calomel and mercury is very slow. However, some change of temperature occurs throughout the tubes, and though this tends to be nearly the same in the two electrodes, it is probable that in a fairly long experiment one electrode will be heated somewhat more than the other, perhaps to the extent of one or two degrees centigrade. Assuming all conditions within the body to remain constant during the experiment and the body resistance to be 6,000 ohms, a change of 1° C. would cause a change of 50 mm. in the deflection of the galvanometer. These figures were verified by the following experiment. The temperature of one electrode was raised to 35° C., the other being maintained at 20° C. The galvanometric deflection, which varied between 20 and 40 mm., when the electrodes were at the same temperature, was raised to over 700 mm. by this change. This indicates a rise in the deflections of 47 mm. per degree, which is in close agreement with the calculated value. If any such change resulted from warming of the electrodes by the fingers, it would occur gradually during the course of the experiment, and hence would not impair the value of a given electric bodily reaction whose duration is only a few seconds; but it would change the starting-point of the individual deflections and possibly falsify the relation between those at the beginning and those at the end of the series. It, moreover, confuses the problem of the causation of the deflections by the addition of a new factor. To measure the actual effect of heat from the fingers upon the deflection, two of the electrodes (Nos. I. and V.) were connected with the galvanometer and the circuit closed with the electrolyte in the Y-tube. The subject then inserted his fingers into the tubes, completely insulated from the fluid by thin rubber cots, the Y-tube being still in place to complete the circuit. Subsequent changes in the deflection were thus due solely to heat acting in two ways, through the change in electrode potential and through lowering the resistance of the electrolyte. In ten minutes the deflection rose to 105 mm. The finger was then withdrawn from one of the tubes (No. V.); in five minutes more the reading was 110. The remaining finger was then transferred from tube I. to tube V., and after eight minutes, the reading was 125. The further changes were as follows: finger transferred to I., after six minutes, 148 mm.; transferred to V., after ten minutes, 144; transferred to I., after five minutes, 160. At this point, the fingers were removed and the bottom of tube V. was grasped with the hand. This caused a comparatively rapid rise to 190 mm. Tube I. was then grasped with a resulting fall in the deflection. This experiment shows that the effect of warming with the inserted finger was to materially lower the resistance of the electrolyte, while owing to the lack of convection, no appreciable change in potential occurred. The potential change which depends upon the temperature of the mercury, appeared only when the bottom of the tube was grasped. When the Y-tube is replaced in the circuit by the human body, the change in the resistance of the electrolyte becomes negligible. It seems, therefore, that for most practical purposes, experiments conducted at room temperature are satisfactory. But for the sake of eliminating as far as possible the thermal factor in studying the cause and extent of the phenomena, it seemed worth while to introduce a thermostat to keep the electrodes as nearly as possible at body temperature. Another advantage of this procedure is that it prevents the increase of skin resistance, which is apt to occur if the circulation is sluggish when the skin is in contact with cool fluid.

A thermostat was, therefore, arranged consisting of a long iron tank full of water placed transversely on the floor under the massage table, heated by an electric coil and regulated by an acetone reservoir with platinum and mercury contact and a rheostat. An electric stirrer was introduced and it was found that with this apparatus the variation in temperature did not exceed one or two tenths of a degree centigrade. It was necessary with the thermostat to employ electrode tubes of a new design, for, if the original tubes were immersed in the water bath, the platinum wires sealed into them at the bottom point could not be effectively insulated from the water and a short circuit would result. For this reason, tubes were made having a diameter of 5 cm. for most of their length, but narrowing at the bottom to a diameter of 2 cm. A short platinum wire was welded to the end of a copper wire and passed through a slender glass tube until only the platinum protruded from the end. This end of the glass tube was sealed off, and bent upwards at a sharp angle. This glass tube was placed in the larger electrode tube with the protruding platinum point at the bottom and mercury was added till the platinum was wholly covered; next above this was a layer of calomel and the tube was filled with potassium chloride. As with the other tubes, both the calomel and potassium chloride had been prepared in the usual way for non-polarizable electrodes. In this way, the current was led off above the level of the water bath, and a short circuit was avoided. In the course of the experiments with these electrodes, a new and serious source of error appeared, to which allusion has already been made. Although the inner tubes containing the conducting wires were tied in such a way as to hold them as securely as possible in one position, it was found impossible to wholly immobilize them. It might be possible to seal them so as to do so. When any joggling occurred, moving the platinum contacts within the mercury, changes in electrode potential resulted, showing themselves by considerable deflections of the galvanometer. As long as care was taken to avoid joggling, these changes did not occur. These tubes were used in most of the later experiments and it is believed that error was not permitted to occur in this way except in the instance already mentioned. However, the danger of error arising in this way during the conduct of experiments is considerable, especially if coöperation is at all questionable. Since the potential changes arising from warming the electrodes are of no practical consequence, it seems advisable in the great majority of experiments to dispense with the thermostat and use electrode tubes such as were first described with platinum wires sealed into the bottom points, thus eliminating the more serious source of error.

In relation to such reactions as the emotional, it is reasonable to expect greater uniformity in the use of a cell current, if our conclusions regarding the physiology of the phenomena are correct, for, as has been pointed out, deflections with isopotential electrodes depend upon the skin surfaces of the two fingers being differently affected. It is obvious that an intense affect stimulating the sweat-glands of two fingers to great activity might stimulate them almost equally; whereas, a slight affect only weakly stimulating the sweat-glands might happen to stimulate those on one side much more than those on the other. Thus the intense affect would cause a smaller deflection than the slight affect. In a long series of tests, however, the average of the deflections following strong stimuli would tend to be greater than the average of those following weak stimuli, for with large potential changes in the skin, the difference between the two sides would in a majority of cases be greater than with small potential changes. With the cell current, we should expect more regularity, for the deflections are caused by lowering of the resistance which results from the secretion of sweat and this should occur invariably with strong affects. Just such a difference was noted between the body current and the cell current. With the former, marked affects occasionally occurred, showing only small deflections, whereas with the cell current, this was almost never the case. It would seem to follow from this that the use of the cell current is the more reliable method of measuring affects.

III. ON THE REACTION DEFLECTION AS RELATED TO THE INTENSITY OF EMOTIONAL RESPONSE WITH SPECIAL REFERENCE TO THE ASSOCIATION EXPERIMENT

The purpose of the accompanying remarks is to further describe a number of experiments that were made with a view to testing the criteria of the emotional reactions in the association experiment. No one who has followed the recent tendencies in the literature of the association test can fail to appreciate how closely these newer viewpoints are bound up with questions of affective reaction; but it is in every way desirable to study these reactions as quantitatively as may be, though the means at present to our hands are far from perfect.

Under present conditions the only scientific approach to this problem is the correlation of objective criteria of emotional reaction with those of the introspection. There is at present no criterion of mental reaction so trustworthy as the subject's own honest and careful account of it. Sources of error in the introspection of emotion there are, indeed, and experiments such as those to be described throw light on their nature, but the writers are much out of sympathy with the practise of going over the head, or more accurately, routing under the heels of introspection for a psychogenic explanation of any phenomena without the assurance that the phenomena in question are eliminated from the physical (or logical) sources of error in the method.

The basis of the mode of inquiry is, then, to present to the subject situations of greater or lesser emotional appeal, to note the character of objective reaction thereto, and to compare it with the subject's own account of the emotional reaction. The free association test is, as it happens, very much the best means of presenting such situations.

The objective criteria of emotional reaction in the association test may be considered as of three sorts: the character of the response word, the reaction time of the response, and the involuntary somatic reactions.

Probably one of the first things learned by an experimenter with the association test is the wide variation in the way in which different subjects "take" the experiment. At bottom, this is probably what produces the difference between *Sachlicher Typus* and *Kon-*

stellationstypus. There are differences in temperament under which some subjects react with much more egocentric responses than others; that is, the responses are chosen much more with reference to the subject's special experience. Where this is done, a *Konstellationstypus* is the general result. This varying egocentricity of the responses is, however, not wholly a matter of individual difference, for it changes not a little with the mood of the subject at different times. In so far as the responses give insight into the nature of these temperamental differences, they are of undeniable value, but they have the disadvantage of not being very coercive, because there is no certainty of how far the subject has observed or attempted to observe the conditions of the experiment in uttering the response. When response words of an intimately personal nature present themselves, pretty much every one can "dodge" and pretty much everyone does do so, to a greater or less extent. But as dodging takes time, those associations which involve suppression will tend to have longer reaction time than those which do not. The assertion of the correspondence of long reaction time with heightened emotional response has been very generally made, and well supported on theoretical grounds; but there is need of more systematic correlation of this factor with the introspective findings, before the degree of its reliability can be accurately estimated.

Whether in the nature of cause, effect, or identity, the emotional reaction is usually considered to be very intimately associated with the organic processes. The emotional reaction is as the introspection detects it; the organic reaction we may estimate with such degree of accuracy as our objective methods permit. Various aspects of the organic reaction may be considered, as the breathing, heart-rate, blood pressure and the like. Judging from the history of the problem, it would seem that the electrical reactions are those in which further study is the most immediately desirable.

Given the technique above described, the problem becomes essentially that of observing the closeness of relationship between the galvanometric reaction-deflection, and the introspectively given intensity of emotional response. Since, in addition, the association times can be recorded (by a stop-watch) without difficulty, an immediate comparison is afforded of the reliability as "*Komplexindikatoren*" of the reaction-deflection and the association time.

A satisfactory method of dealing with the introspective data is of course required. Since the essential thing to determine is the intensity of emotional reaction, the object of the experiment is best served by making the individual's task in recording this datum as simple and definite as possible. The ideal plan, of course, would be to have the subject arrange the different emotional reactions in the order of their intensity, but this is obviously impossible. The original procedure, and that followed for the most part, was to assign the reaction to one of four groups: (A) strongly emotional, (B) rather emotional, (C) rather unemotional, (F) practically devoid of emotional reaction. This grade was determined by the subject as soon as possible after the response was given, and announced to the operator when called for. In some cases the response-words were dispensed with, and the subject remained silent except when asked for the grade; but this diminishes somewhat the efficiency of the procedure. It stands to reason that the grade was assigned without any knowledge of the deflection to which it attached.

A short representative series (no responses spoken) is as follows:

Stimulus-word	Emotional Grade		Reaction-Deflection in mm.
peevish	C		11
wrong	F		9
go	C +	the reaction was noted to "persist"	11, then to 14
where	B		20
ring	B —		12
now	F_2		8
telltale	F +		6
quiet	F_1		9
widow	C		7
amorous	B		20

In matters of this sort, the less the subject is hampered with technical definitions of the qualities to be graded, the more reliable his gradings are likely to be. It is much better to let the subjects find out for themselves what they judge by than to tell them in the beginning to judge by criteria that they can not be expected to construe in the same way as they are presented. With continued practise in the experiment, it was but natural that certain criteria should separate themselves out to the subjects' observation. These tended to reduce themselves more and more to a basis of somatic sensation, though the results are not very different, whatever criteria are uppermost in consciousness. If it be permissible to introspect introspection, the sources of error would, as a matter of experience, operate mainly in the direction of making the grades too low. With some subjects there may be in the first experiments a noticeable tendency to be chary about assigning the highest grades at all, owing to their frequent relation to intimate personal affairs. Where there is suppression, the grade is apt to be underrated. And if the emotion aroused is one that the subject regards as of a degraded origin, it may receive a low grade independently of the fact that the emotional reaction has been quite pronounced. Conversely, ideas that might ordinarily be associated with elevated emotions may be graded high, though the ideas do not now arouse such a reaction, but this error seems to reach serious proportion only in subjects quite unpractised in introspection. There seems to be a real introspective awareness of these sources of error, and they can greatly distort a genuine correlation between the functions observed.

A more refined method of dealing with the introspective data was evolved not only to guard somewhat against these errors, but also to obviate the external difficulty that the reaction deflections could not be relied upon to maintain the same order of magnitude throughout a prolonged experiment. This procedure was to segregate the associations into small groups, regularly of five, which the subject would then endeavor to arrange in order of the intensity of their emotional reaction; or the subject would grade the words as previously, and in case of the same grade being assigned to two of the five words, would decide which of the two reactions had been stronger. As an illustration, the following words may be quoted, which were given incidentally, and not as part of a regular experiment.

Stimulus-word	Emotional Grade	Reaction-Deflection
mountain	C +	6
marry	A	25
trouble	B —	2
hope	B	5
bicycle	F	1

(The correspondence in order is here rather better than the average; it is in fact perfect except for the displacement of *mountain* which may, however, owe much of its deflection to having come first in the test.)

This is much the more satisfactory way of making the experiment, when the subject's introspective ability is sufficient to permit it.

The observed relationships between the objective and the introspective criteria can hardly be stated by any of the more evolved correlation methods, since the quantitative relations of the emotional grades are not sufficiently definite. In the original method of recording, one is practically limited to stating the central tendency of the deflections that are assigned to each group of emotional grades.

The greatest number of experiments is with F.L.W. as subject, and it is perhaps fair to add that the practise in introspection that comes with special psychological training was probably greatest in this subject.

Tabulated as above, the experiments with this subject resulted as follows:

RELATION OF EMOTIONAL GRADE TO GALVANOMETER DEFLECTION AND TO ASSOCIATION TIME

N. B. The figures express central tendencies only; no claim is here made for the reliability of the method in single observations.

The median association times are given in fifths of a second.

Experiments with Subject F. L. W.

Expt. No.	No. of Stimulus-words		Emotional Grades A	B	C	F	Remarks
I.	26	Av. Defl.	55	70	43	41	Body currents. Palms resting upon cotton in funnels in electrodes. No specific criterion of emotional intensity. The median association times of this experiment are included in those of the next, of which it is chronologically a part.
		Med. Time	See next experiment.				
		No. Cases	*1*	*5*	*12*	*8*	
II.	72	Av. Defl.	47	31	26	15	Body currents, middle fingers immersed in electrode solution.
		Med. Time	17	13	12	12	
		No. Cases	*5*	*17*	*25*	*25*	
III.	25	Av. Defl.	32	26	20	19	Body currents. Grading by "localized" somatic sensation.
		Med. Time	17	13.5	12+	12	
		No. Cases	*1*	*6*	*8*	*10*	
IV.	25	Av. Defl.	64	57	46	47	Body currents. Grading by "intellectual" estimate.
		Med. Time	12	11	11—	10	
		No. Cases	*4*	*6*	*8*	*7*	
V.	25	Av. Defl.	43	29	17	14	Body currents. Grading by "diffuse" somatic sensation.
		Med. Time	9	11	9.5	9	
		No. Cases	*3*	*7*	*9*	*6*	
VI.	25	Av. Defl.	68	35	28	27	Body resistance, shunt .01. Grading by "diffuse" somatic sensation. Future experiments are graded by this criterion.
		Med. Time	10+	10.5	9	9.5	
		No. Cases	*1*	*6*	*10*	*8*	

Expt. No.	No. of Stimulus-words		Emotional Grades A	B	C	F	Remarks
VII.	15	Av. Defl.	145	38	12	11	Body resistance, thermo-electric current. Difference in temperature of electrodes, approximately 14° C.
		Med. Time	Not recorded.				
		No. Cases	*1*	*3*	*6*	*5*	
VIII.	25	Av. Defl.		9	10	5	Body currents, index and third fingers of right hand.
		Med. Time		8	9	8	
		No. Cases		*5*	*10*	*10*	
IX.	12	Av. Defl.	10	3	2.5	2	Body currents, middle fingers of either hand in usual manner.
		Med. Time	13	12	10	10	
		No. Cases	*1*	*1*	*4*	*6*	
X.	17	Av. Defl.	26	10	13	10	Body currents, index and third fingers of right hand.
		Med. Time	14	12	13	11	
		No. Cases	*1*	*6*	*6*	*4*	
XI.	25	Av. Defl.	5	4.3	2.4	1.3	Body currents, middle fingers of either hand.
		Med. Time	12	13	10	11	
		No. Cases	*2*	*3*	*9*	*11*	
XII.	24	Av. Defl.	9	4.3	3.3	1.3	Body resistance, shunt .01. Electrodes in thermostat.
		Med. Time	9	10	11	9	
		No. Cases	*2*	*6*	*9*	*7*	
XIII.	23	Av. Defl.	13	6.5	5.3	6.2	Body currents, thermostat. Excluding the first two reactions (C's), which are anomalous.
		Med. Time	10	10	10	10	
		No. Cases	*1*	*8*	*9*	*5*	

Expt. No.	No. of Stimulus-words		Emotional Grades A	B	C	F	Remarks
XIV.	25	Av. Defl.	44	15	8	4	Body resistance, shunt .01, thermostat.
		Med. Time	12	11	10	9	
		No. Cases	*2*	*11*	*6*	*6*	
XV.	21	Av. Defl.	13	3	2	3+	Body currents, thermostat. Omitting the first four reactions, which though preserving the correlations, are anomalously large.
		Med. Time	16	9	10	10	
		No. Cases	*3*	*6*	*6*	*6*	
XVI.	25	Av. Defl.	7	6.6	3	2	Body resistance, thermostat. The largest deflection, 16, attaches to the stimulus-word *bitch*, assigned the grade of B +. Cf. p. 28.
		Med. Time	No responses given.				
		No. Cases	*2*	*5*	*6*	*12*	
XVII.	25	Av. Defl.	4	2.3	1.9	1.7	Body currents, thermostat.
		Med. Time	Not recorded.				
		No. Cases	*4*	*7*	*8*	*6*	
XVIII.	21	Av. Defl.	2	1	0.6	1	Under 1/100 gr. atropin, immediately after the above, and under the same external conditions.
		Med. Time	Not recorded.				
		No. Cases	*2*	*6*	*5*	*8*	

In the subjoined experiments A.F. was subject:

Expt. No.	No. of Stimulus-words		Emotional Grades A	B	C	F	Remarks
I.	25	Av. Defl.	5.5	2.4	1.4	1.6	Body resistance.
		Med. Time	8	12	12	11	
		No. Cases	*2*	*5*	*10*	*8*	
II.	25	Av. Defl.	13	6.7	3.5	2	Body currents, electrodes in thermostat.
		Med. Time	11	12	11	10	
		No. Cases	*2*	*9*	*12*	*2*	
III.	10	Av. Defl.		21	5.2	8	Body resistance, shunt 0.1.
		Med. Time		11	11	10	
		No. Cases		*3*	*6*	*1*	
IV.	25	Av. Defl.	9	4	4	2	Body currents, electrodes in thermostat.
		Med. Time	Not recorded.				
		No. Cases	*2*	*9*	*8*	*6*	
V.	20	Av. Defl.	4	1.7	0.7	1.5	Body resistance, shunt .01.
		Med. Time	Not recorded.				
		No. Cases	*1*	*5*	*8*	*6*	

In the following experiment, the responses were given without the grades, which the subject assigned afterwards from memory.

VI.	91	Av. Defl.	23	15	7	4
		Av. Time	11	12	13	12
		No. Cases	*4*	*12*	*40*	*35*

Taking into account the inaccuracies of introspection, as well as the sources of error remaining in the experimental method, these results seem to show that in central tendencies a fairly close relationship exists between the intensity of the objective reaction and the electrical disturbances in the tissues involved. In point of comparison with the association time, the relative superiority of the deflections is evident.

What the figures do not indicate, is the reliability of the method for individual cases. This is the most important practical feature of the problem, it being of some forensic interest to know with just what certainty the specially affective moments in an individual's mental economy may be objectively determined and measured. For the above form of presentation this is sufficiently well indicated in the mean variation of the association times and reaction deflections attaching to the different emotional grades. The following experiments, made some time previous to the present ones, especially well illustrate this relation, because both in the naïveté of the grading and in the intensity of the emotional reactions involved, they approximate more nearly than the present tests to the actual conditions of *Tatbestandsdiagnostik.*

Number of Stimulus-words		Emotional Grade A	B	C	F	Remarks
99	Av. Defl.	50	21	16	15	Body currents. Palms strapped to cotton in electrode funnels. Quoted from Wells and Cady, *American Journal of Insanity,* LXV., 165–166.
	M. V.	10	9	6	7	
	No. Cases	*2*	*23*	*36*	*38*	
100	Av. Time	16.5	14.0	11.8	10.9	Observe that the average and median are practically the same; the median, indeed, tending to be slightly longer.
	M. V.	1.5	1.9	2.0	1.8	
	Med. Time	16.5	15.0	12.0	10.9	
	No. Cases	*2*	*20*	*37*	*41*	

It is easily seen that the individual reactions, outside those of the A grades, are subject to so large a probable error that neither time nor deflection has much significance for placing them. The deflections which attach to the A grades are separated from the remainder by a greater margin of probability than the times are, indeed the margin is here quite considerable, and it is precisely these stronger reactions that it is psychodiagnostically most important to detect. Substantially this relation exists also in the more recent experiments.

Before finally condemning the method for the individual cases, except in the strongest emotional reactions, an examination may be made of its behavior in those cases where the associations are segregated in groups of five, and ordered in relative position. This gives a limited opportunity for correlation by the Woodworth per cent. of displacement. Thus the example quoted on p. 29 would show two displacements out of a possible 10, 20 per cent.

Arrangements of this nature were available in about a third of the experiments above quoted, totalling 36 groups of five reactions, 23 for F.L.W. and 13 for A.F. The correlation by the Woodworth per cent. of displacements is as follows for the different factors under consideration:

Summary of Correlations

The lower the figure, the closer the correlation; 50 per cent. = 0 correlation

	F.L.W.	A.F.
Defl. Emot.	27 per cent.	30 per cent.
Time Emot.	39 per cent.	44 per cent.
Defl. Time	38 per cent.	42 per cent.

The validity of these averages is limited somewhat by the fact that the differences in emotional reaction are greater in some groups of five than in others. Thus some will contain only B and C grades, while others may cover the complete range, A, B, C, F. Chance errors are much more likely to break down a real correlation in the former case than in the latter. As a matter of fact the correlations with the deflections are more positive, the greater the range becomes.

Of special interest is the condition with the A-grades. In the arrays considered this grade is assigned fourteen times, and in thirteen cases it attaches to the greatest deflection in the array; in the fourteenth it is tied with a B+. The deflections here put in a class by themselves the reactions attaining this grade, which, for that matter, the introspection does also. Only two of these fourteen A's have the longest association time in the array; a third is tied with a C. Twice the time is actually shortest, and twice tied for shortest. The greater reliability of the deflection is here also evident.

Unfortunately it is not so evident that considerably increased deflections necessarily attach to an A emotional grade. There are uncontrolled factors which may occasion a considerable deflection in one of the lower grades. Only when the greatest deflection in the array is half again as much as the next greatest, is it possible to say with comparative assurance that one is dealing here with an emotional grade of A or B. Among the sixteen cases in which such a difference exists there are two exceptions, both in F.L.W.

Special attention should be called to the fact that the correlation of the two objective criteria—the deflection and the time—is but slightly more positive than that of the introspection and the time, and much less positive than that of the introspection and deflection. This militates considerably against any supposition that the objective criteria are significantly influenced by any mental factors independent of the introspection. In so far as these measures are measures of emotional response, they should be influenced together by the factors of the emotional response; and since they are not so affected together, but their correlations with the emotional grades are relatively independent, their relation to the emotional reaction does not seem to be influenced by extra-conscious mental factors to any important degree.[1]

Previous mention has been made of the phonograph as a source of emotional stimuli. The advantage lies in the greater constancy

[1] When, as occasionally happens, a low emotional grade attaches to both an increased deflection and a lengthened association time, this is best interpreted as the result of calling up a considerable body of rather vivid associative imagery, which lengthens the association time by increasing the difficulty of choice, and at the same time obscures the introspection of the emotional reaction. We can merely offer it as a matter of experience that less clear affects are apt to be considered less intense. One must remember also the possibility of suppression causing an underestimation of the emotional grade. In this way the association time, while of little value in itself as a measure of the affect, may often be useful in modifying the interpretation of the deflections.

of stimulus. It is effective enough in the individuals of musical perceptions, provided the instrument is of a good grade and carefully handled, and the records properly selected. The writers found the final trio of *Faust* the most effective of the records employed. Some subjects, unaware of the nature of the phenomenon, have observed the deflections while listening to the record, and discovered for themselves the relation of the movements of the light to the more stirring portions of the record.

The above recounted experiments indicate the most that can be expected of the method in its present evolution. The examination of the quantitative relationships of the deflections in the different experiments is sufficient to indicate how great are the variations in the susceptibility of the same individual at different times. This seems indeed, to be but slightly less than that between a number of different individuals. There are recorded occasional experiments in which the subject is absolutely refractory, *i. e.,* the electrical reactions are unmeasurably minute, or fail altogether. The greatest galvanometric activity observed by the writers in any individual is about ten times that prevailing in the experiments just described. No account has been taken of any galvanometric phenomenon but the principal reaction deflection; indeed, the very important question of the electrical reaction time has been practically disregarded because the apparatus is not of a type to lend itself to precise determinations on this point. A considerable opportunity for advance in the problem rests in the improved instruments and methods that are becoming available. For the present, it would be unwise to make absolute claims, but it may be reasonably asserted that as an objective criterion of emotional reaction, the electrical reflex appears distinctly superior to any analogous procedure as yet developed.

APPENDIX

Examination of Patients

Experiments were made with four cases of mental disease, two of whom were cases of catatonic stupor, one a stupor of undetermined nature, and one of senile dementia. The senile case, although able to answer simple questions, showed practically no deflections with isopotential electrodes, when tested with a variety of stimuli. Graphophone records were played, questions were asked, substances with strong odors were held close to the nose, and a threat of a prick with a pin was made. Throughout all this series of stimuli, the deflections remained nearly constant, the ray of light moving slowly to and fro, but at no point showing abrupt changes such as are noted with ordinary subjects.

With a second case, one apparently of deep confusion, with total inaccessibility, slight deflections were noted, but most of these seemed associated with bodily activity, which it was difficult to prevent. Only a few stimuli were given, and little significant evidence was obtained.

The two cases of catatonic stupor showed some rather striking phenomena. In the first case,[1] the consciousness was fairly marked, and, although the patient could not be made to speak, he, nevertheless, seemed aware of his surroundings and inclined to resist the efforts to place his fingers in the electrodes. He was finally induced to comply long enough to enable the putting of a few questions and the repetition of two graphophone records. Although the patient showed no outward sign of hearing or understanding the questions, a definite deflection followed each one of them and the magnitude of the deflections appeared significant. Most of them varied between 2 and 13 mm.; one, however, which concerned a personal friend, was followed by a deflection of 49 mm. Considerable deflections occurred during the playing of the graphophone records, the readings varying from 85 mm. at the start to 108 mm. at the end. The second case of catatonic stupor was an admirable one for study. The patient showed no evidence whatever of consciousness, lying motionless in whatever position placed. The electrode tubes were placed at opposite sides of the bed and the middle fingers of the two hands

[1] This patient had been in the apparatus several times previously, with similar results, in experiments from the previous work of Wells and Cady.

allowed to lie motionless in the fluid. A current from a dry cell was used. No stimulus of any sort produced any visible outward response. With the galvanometer reduced to .01 of its sensitivity, only slight changes followed verbal stimuli, although a deflection of 12 mm. was produced by the touch of a cold metal key upon the forehead. The galvanometer was then shunted to .1 of its full sensitivity and questions were addressed relating to events in the patient's history, concerning which she had talked during an earlier stage of the disease. These were interspersed with sentences in the Gothic language, which were, of course, meaningless to the patient. Slight reactions followed nearly all the sentences, whether English or Gothic. In some cases, the reactions following questions of significance to the patient were no greater than those following Gothic sentences, but in certain instances, deflections of 18 mm. and in one case, 30 mm. followed questions of special significance. The touch of a cold key on the forehead caused a deflection of 31 mm.; the entrance of the nurse into the room caused a deflection of 12 mm.; the shutting of a door outside marking the approach of the examining physician caused a deflection of 15 mm.

The evidence furnished by these experiments tended to show that the failure of ordinary response in these cases of catatonic stupor resulted rather from inhibition of reaction than from failure to apprehend. They mark a contrast between these conditions and that of the senile patient, who although able to converse with a slight degree of intelligence, showed no evidence of affective reaction, and possibly also with that of the second named case, who showed no reaction of comprehension to questions that if understood could scarcely have failed to be of marked emotional import.

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THE RELATIVE MERIT OF ADVERTISEMENTS

A PSYCHOLOGICAL AND STATISTICAL STUDY

BY

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ARCHIVES OF PSYCHOLOGY

EDITED BY

R. S. WOODWORTH

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CONTENTS

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THE RELATIVE MERIT OF ADVERTISEMENTS

INTRODUCTION

Advertising is an integral part of business to-day and is rapidly becoming more and more important. There seems to be no limit either to the extent of its usefulness or to the ingenuity displayed in presenting its message in new and attractive forms. Its tremendous significance financially is evident from the fact that, according to different estimates, from $600,000,000 to $1,000,000,000 are spent in this country every year in various phases of advertising. Surely any aspect of our national life involving such sums warrants careful and detailed study.

One of the few things that all advertisers are agreed on with regard to their business is that a large percentage of the money and energy expended is wasted. The International Correspondence School, for instance, estimates that 18 per cent. of newspaper advertisements are "entirely worthless" and that only 15 per cent. can be called "good advertisements." A very conservative estimate of the yearly loss in this country would be $200,000,000. Whatever the loss it is not due simply to inexperience and carelessness. Even among the best laid plans prepared by "experts" losses occur.

One of the reasons for this condition is the extreme difficulty of estimating even approximately the actual results from advertising campaigns. To determine the value of any one of a set of advertisements used in a series is almost impossible. "Keyed" results of various types are used, but there is the greatest diversity of opinion as to their value. Undoubtedly experience is the best teacher. But where there are so many factors to be evaluated and where they are combined in such complex ways it is difficult to estimate their worth, especially when the experience of others can not be readily compared with one's own. And besides this difficulty, there is the great objection to relying entirely on experience as a guide in the fact that it leads us to wisdom only after we have spent our money. Knowledge so acquired is of value only as applied to "next time." This situation has prompted the recent discussion of establishing an Institute for Advertising Research. It certainly seems to the author that such an institute would be of incalculable value, even if it did no more than to present, in as complete a form as possible, the lessons that have been learned in the past.

The main object of this report is to determine whether psychology can be of any value in this situation, *i. e.*, whether psychological tests can be employed to estimate the value of advertisements before they have been actually used. If so, to what extent?

The writer believes that both of these questions have been most favorably answered by the results of the experiments herein reported. It has been found that the methods he has used give results in close agreement with known advertising returns. Certain principles as to the structure and the nature of successful advertisements have also been deduced. The report then, while it is of interest to psychology in that it is a discussion and development of a method of research, and, to a slighter degree perhaps, in that it throws some light upon principles of action in man, is of great interest to advertising in that it establishes the relative value of different appeals in the sale of commodities.

The material presented advances from simple experiments to those which are more complex and refined. The writer considers the discussion of vacuum cleaner and piano advertisements in Chapters III. and IV. as preliminary in nature. It is of value here in that it demonstrates the validity of the whole method of procedure through the consistency of its results with those of the more refined experiments. Chapters V. and VI. deal with the *order* of preference of different advertising appeals for breakfast food and toilet soap, but not with the actual *amount* of their value. Chapter VII. presents a study of fifty advertisements in which the *difference in amount* of value of one advertisement from another is given. As this is the first experiment of the kind to attempt such results, its precise validity remains still to be established. However, the writer does feel justified in claiming that the results closely approximate to the actual conditions.

CHAPTER I

Summary of Previous Experimental Work in the Psychology of Advertising

There are three distinct lines of investigation in advertising from the psychological standpoint. First, we have the general set of factors and their specific values which enter into the production of the stimulus (whether it be reading matter, electric signs, or samples); second, the general factors operating in fixating the impression—the factors of association and memory; and third, the factors operating to influence the response to the original stimulus. The advertising man states the several problems as follows: "Attract attention; arouse interest; create desire; and effect a sale." None of these divisions, of course, pertains to disparate processes but only to convenient and useful phases of a single reaction. This report is concerned with the factors which operate to influence response to stimuli or, in other words, the factors which create desire. Factors of attention, memory, etc., are not specifically considered.

Practically all the work so far carried on by psychologists in their investigation of advertising relates to the first line of research —the endeavor to determine the "attention-value" of colors, of different type, of preferred positions on the page, etc.

Gale[1] considered the value of relevant and irrelevant words and cuts and found for both sexes "a constant and decided increase in the value of relevant words with each consecutive flash of a certain advertisement." Relevant words and cuts were preferred to irrelevant. "The men were proportionately more caught by words. Similarly on the question of relevancy and irrelevancy, the female attention was more susceptible to irrelevancy, as it was also to cuts, than was the masculine attention." He also considered the relative attention-value of different parts of the page, of different sizes of print, and of different colors with reference to black and white background.

Scott[2] in a number of experiments considered the attention-value of large and small advertisements. He also compiled various data in regard to the average time spent by various groups in reading newspapers, the per cent. of persons who read advertisements in magazines, etc.

[1] H. Gale, "Psychological Studies," 1900, Chap. II.

[2] W. D. Scott, "The Psychology of Advertising," 1908.

Starch[3] also considered the relative attention-value of the various pages and quarter-pages of a magazine.

There are to my knowledge but three attempts to investigate the factors operating to influence the response from the advertisement. Gale[4] selected four articles (soap, pianos, house-furnishings, and clothes) and wrote six different advertisements for each article, "each advertisement emphasizing one or two special reasons for dealing with that firm. Thus the special points of cheapness, forced sale, age, reliability, credit, prizes, testimonials, qualities, prices, home manufacture, etc., were made. As all the advertisements were fictitious as to names and firms the readers were asked to imagine themselves in a new city where they needed to go out and buy each of the articles, and from these advertisements they must select the place where they would go first, then their second choice, third choice, etc., to the sixth place, which would attract them least of all. Then especially they were to give their reasons for each choice. Replies were obtained from 72 males and 33 females. A minority of the subjects were college students, the remainder were friends and relatives of the students."

The advertisements for the soap were as follows:

No. 1. Our velvet soap is attractive to touch, sight, and odor.
No. 2. Colonial soap. Richard Endicott and Co. Founded in 1831.
No. 3. Soap Special Sale at "Swanson's Mammoth."
"Lilly"—6 cakes for 34 cts.
"Queen's Own"—6 cakes for 37 cts.
Omaha Packing Co.'s new "Expansion"—6 cakes for 29 cts.
Texas Cattle Co.'s "Get there"—6 cakes for 23 cts.
Swanson's "Pride"—6 cakes for 21 cts.
"Gold Standard"—6 cakes for 19 cts.
Limit of one dozen to each purchaser.
No. 4. America Soap. Government Standard. Official tests by U. S. chemist shows less than a half of one per cent. of impurities. Eagle Soap Co.
No. 5. Free. A durable toy balloon given away for three days with each purchase of four cakes of Universal Soap.
No. 6. Patronize Home manufactures. Flour City soap at their home store.

I have taken the data given for the soap advertisements in Gale's Table XVI. and have calculated them on the basis of one hundred subjects so as to equalize the results from the unequal number of men and women subjects. These data were then handled in the same manner as my own, *i. e.*, give the position of the average judgment for each advertisement. They are as follows:

[3] D. Starch, "The Psychology of Preferred Positions," *Judicious Advertising*, April 5, 1910.

[4] *Loc. cit.*

Adv.	Men	Women	Men and Women
No. 4	2.46	2.29	2.38
No. 2	2.69	2.11	2.40
No. 6	3.22	3.49	3.36
No. 1	3.50	3.49	3.50
No. 3	3.68	4.06	3.87
No. 5	4.62	5.47	5.05

The "old firm" and "pure soap" appeals are then ranked first, "home industry" and "attractive" (according to Gale) or "quality" appeals stand mid-way, and the "special sale" and "premium" appeals are rated last.

Gale had each subject state the reason he assigned each advertisement its respective place and based his results on these "reasons." His summary of the above data is as follows: "It is seen that the age of the firm is the reason most given for choosing the first place; then follows government test and cheapness with the men, while purity and generally 'attractive ad.' with the women. The reason most unanimously given for the last choice is the prize offered in No. 5; it being characterized so largely by the women as a fake is due to this also. Cheapness is the second largest reason given for last choice, and the first reason for next to the last choice. Between these reasons determining the extremes of first and last choice is that of patronizing home manufactures, which appears strong for a third and fourth choice."

Gale does not give all his data for the other three articles studied, consequently an accurate comparison can not be made between his results and the writer's. His general summary of the appeals in the four articles is as follows in a descending order of merit: (1) Age, (2) reliability, (3) attractive, good advertisement, (4) cheap bargains, (5) qualities stated, (6) prices stated, (7) style, (8) fake, brag. "Men were a little more influenced by age and women by reliability and by what they called an attractive or good advertisement. Contrary to expectations and to the information of experienced advertisers, it appears that cheapness influenced our male answers more in favor of an advertisement and the females more against an advertisement. About 60 per cent. of the answers for first choice were made from the age or reliability of the firm. This seems to show that they tried to substitute for their own want of experience with the firms the experience of the public who had dealt with them."

Scott, in his "Psychology of Advertising," in the section on The Laws of Progressive Thinking, gives the following data secured from letters in which the writers state their reasons for preferring a certain advertisement:

1. 607 for reliability of the firm or the medium or the goods, in the opinion of the writer.
2. 508 for money considerations.
3. 418 for the construction of the advertisement.
4. 408 because of the present need.

As I understand it, the above figures show the number of letters received during one month in which "the writers told which advertisements (of a certain magazine) they were most *interested* in and what it was in each particular advertisement which interested them." My own observations would indicate that factors of *interest* may be considerably different from the factors which lead one to *buy,* although, on the whole, they will correlate fairly high. They would further indicate that introspections as to why one is actually interested or led to buy are very difficult to obtain and take into account only a few of the many complex reasons.

Hollingworth[5] prepared "fifty abstract appeals, each designed to reach a different interest, instinct, or line of argument and corresponding for the most part to the salient points of various widely differing sorts of advertising copy." Thirty women, mainly undergraduates, and twenty men of corresponding age and class arranged the 50 appeals in an order of merit with respect to persuasiveness. Following are samples of the appeals which were used. All were typewritten on separate slips of the same size.

> 1K6. *Scientific.*—Our 1K6 article is manufactured by approved scientific methods and scientifically tested processes by technically trained men working under the constant supervision of experts.
>
> 2B7. *Family Affection.*—A final day must come to every man and no one wants to see his children left dependent on mere accident. You owe a duty of provision and foresight to your family. A 2B7 will guarantee their comfort and security when you are gone.

"Certain sources of 'error' in such an experiment are at once obvious. (1) It is difficult to keep out of even these abstract appeals some suggestion of special reference. Thus the appeal to appetite will inevitably suggest food, some health appeals are strikingly medicinal in tone, and doubtless in most cases there is a more or less pronounced tendency to think of one article rather than another. (2) There is a certain feeling of self-consciousness and reserve in submitting honestly to such an experiment, a tendency to place too

[5] H. L. Hollingworth, "Judgments of Persuasiveness," forthcoming paper in *Psych. Rev.* Besides this report Hollingworth has carried on a number of experiments with advertisements, one of which is reported in the following chapter in support of the method used throughout this report. A full account of these experiments can be found in his forthcoming book on "The Principles of Appeal and Response."

low certain appeals which bulk large outside of the laboratory, or a tendency towards ideal arrangement strongly suggestive of the inclination to give learned responses in association tests.

"The group of appeals, as a whole, falls into three rather sharply defined sections, the series breaking at values 15.2 and at 29.0 at which points there are wider gaps. These sections, moreover, correspond to qualitatively different groups of appeals.

"In the first group with value ranging from 4.0 to 15.2 fall the appeals to health, cleanliness, scientific construction, economy of time, appetite, increase of efficiency, safety, durability, quality, modernity, and family affection. The general characteristic of these appeals is that they are strictly relevant in tone, describe the article precisely or point out some specific value, or 'selling point' which it possesses.

"In the second group, with values ranging from 21.0 to 29.0, fall the appeals based on the general reputation, guarantee or assertion of the manufacturer, and on a set of specific and more or less social feelings and interests, such as sympathy for others (not family), courtesy, invitation, elegance, hospitality, sport, cheapness, etc. The characteristic of these appeals is that they do not relevantly describe the article but try to connect the article with some specific instinct or effective conception. And these appeals are distinctly less personal, more social, than those of the first group.

"In the third section, with values ranging (with one exception) from 41.0 to 45.8, fall the rather vague appeals to avoid substitutes, to civic pride and clan feeling, social superiority, recommendation the ideals of fashion, foreign origin, and finally the appeal of beauty. The chief characteristic of this group seems to be that while, as in the second group the statement is semi-relevant or incidental, the feeling appealed to is indeterminate and general.

"The only considerable sex differences, cases in which the difference in position is say 5 places or over, are on the appeals entitled appetite, safety, nobby, family affection, sympathy, elegance, and recommendation, which are placed higher by the men, and on time saved, guarantee, medicinal, substitutes, efficiency, durability, quality, and hospitality, which are placed higher by the women."

The second source of error pointed out by Hollingworth must largely account for the very low position of such appeals as social superiority, ideals of fashion, and beauty. The very fact that they are placed last would support the view that they should be ranked extremely high, if they are out of place at all, as all experiments of this order clearly show that the two extremes of an order of merit arrangement are definitely determined whereas the remainder of the series is composed of the "indifferent" appeals.

CHAPTER II

On the Validity of the "Order of Merit" Method as Applied to Advertising

Section 1. *General Survey of the Method*

The experimental work of this report has all been done with the use of the so-called "Order of Merit Method." The peculiar point of this method is that a series of stimuli is arranged according to some designated order by each subject. The great advantage of this method over that of the more generally used one of "Paired Comparison" is the comparative ease and quickness with which a large number of stimuli may be graded on the basis of the given criterion. This method facilitates the obtaining of results from a large number of subjects, thus avoiding the small "select" groups so commonly used in psychological experiments. A second feature of the method, not yet appreciated by many psychologists, is the ability to secure judgments upon very complex stimuli. Not only in these cases may the stimuli be too complex to be analyzed into their component parts but the resulting judgments may also be based on so many details that they too can not be analyzed through introspections. Yet with all these complications a series of judgments may be secured that will not vary greatly for the same individual if repeated after considerable lapses of time. In fact, one of the striking points of the method is this reliability of the judgments.

Cattell was the first to make use of the "order of merit method" in his study of two hundred shades of gray.[1] Since then he has employed the method in the study of eminent and scientific men.[2] Sumner made a study of beliefs;[3] Wells, a study of literary merit[4] and of the variability of individual judgments;[5] Norsworthy, a study

[1] J. McK. Cattell, "The Time of Perception as a Measure of Difference in Intensity," *Philos. Studien*, XIX., 1902.

[2] J. McK. Cattell, "Statistical Study of Eminent Men," *Pop. Sci.*, LIII., 357; "Statistics of American Psychologists," *Amer. Jour. of Psych.*, XIV., 310; "Statistical Study of American Men of Science," *Science*, N. S., XXIV., 621, 622 and 623; and "A Further Study of Men of Science," *Science*, N. S., XXXII., 827 and 828.

[3] F. B. Sumner, "A Statistical Study of Belief," *Psych. Rev.*, V., 616.

[4] F. L. Wells, "The Order, Position, and Probable Error of Ten Leading American Authors," Columbia Univ., "Cont.," XVI., 3.

[5] F. L. Wells, "On the Variability of Individual Judgments," "Essays Philosophical and Psychological in Honor of William James," 1908.

of the validity of judgments of character;[6] Thorndike, a study of handwriting with special reference to the construction of a scale for quality of handwritings;[7] Downey, a study of family resemblance in handwriting;[8] and Hollingworth has made frequent use of the method in the study of advertising,[9] and of judgment.[10] In all these studies an order of preference was established. The question is, how nearly does such an order of superiority in "pulling-power" of advertisements approximate the known results? The following pages give some data on this point, *i. e.*, on the reliability of the method when applied to advertising appeals.

Section 2. *Application of the Method to Advertising Problems*

One experiment of Hollingworth's with advertisements is recorded here. It is with a set of Bullard Machine Tool Co.'s advertisements. A short review of the results of this experiment as compared with the returns recorded by the advertiser is given. Similarly, a short review is then given of (1) a set of fifty advertisements and then (2) a set of eight advertisements from the Packer Manufacturing Co.

Section 3. *Results from a Set of Lathe Advertisements*

There were five advertisements in the set of Lathe advertisements from the Bullard Machine Tool Co. They were tested by the order of merit method with respect to: (1) attention-value, (2) persuasiveness or "pulling-power," and (3) memory-value. Advertisement No. D consisted of a large cut, No. A contained the same cut but the advertisement was arranged so as also to present some "reason-why copy," advertisement No. E contained a smaller cut with radiating phrases descriptive of its special advantages, advertisement No. C contained a quarter-page cut and three-quarters of a page of "reason-why copy," and advertisement No. B contained a very small cut and very good "reason-why copy" surrounding it. Ten mechanics and engineering students were the subjects. The results are shown in Table I.

[6] N. Norsworthy, "Validity of Judgments of Character," "Essays Philosophical and Psychological in Honor of William James," 1908.

[7] E. L. Thorndike, "Handwriting," *Teachers College Record*, XI., March, 1910.

[8] J. E. Downey, "Preliminary Study of Family Resemblance in Handwriting," *Psych. Bulletins*, Univ. of Wyoming, No. 1, 1910.

[9] One of his minor experiments is reported in the following pages. See also, H. L. Hollingworth, "Judgments of Persuasiveness," forthcoming article in *Psych. Rev.*

[10] H. L. Hollingworth, "Experimental Studies in Judgment; Judgments of the Comic," *Psych. Rev.*, XVIII., 2.

TABLE I

RESULTS FROM EXPERIMENT WITH BULLARD LATHE ADVERTISEMENTS[11]

Advertisement	Order According to: Attention-Value	Memory-Value	Pulling-Power	Summary of Judgments as to Pulling-Power: Average	Av. Deviation	Actual Order of Inquiries Rec'd
A	2.0	4	4	3.0	*0.8*	4
B	3.0	2	2	2.6	*1.7*	2
C	4.5	1	1	2.3	*0.9*	1
D	1.0	5	5	4.4	*0.8*	5
E	4.5	5	3	2.7	*0.7*	3

Because of the very small cut in advertisement No. B, 4 subjects ranked it last or next to last with respect to "pulling-power," the other six ranked it first or second. As will be pointed out later, an advertisement of all reading matter or all picture will be ranked very high or very low by approximately one half of a group of subjects and the reverse by the other half. Advertisement No. C consists of about half picture and half reading matter and consequently appeals to both groups. The advertisements with large cuts, as No. A and No. D, have high "attention-value" but slight "pulling-power" or "memory-value." "The best advertisement, psychologically, is neither No. B nor No. C, but a combination of the two styles—as large a cut as possible for attention value and *definite, concise, pointed, underscored* 'copy' as in advertisement No. B. Such an advertisement would appeal to both types of mechanics, the visual and the audile," *i. e.,* those preferring picture-ads and those preferring "copy-ads."

This order of merit method does not, of course, give the actual *amount* of superiority of one advertisement over another, as found in business, but does give the *order* of superiority. When this order was compared with the actual number of replies for catalogues received by the Bullard Co. from each advertisement, it was found that the two orders agreed exactly. Advertisement No. C "pulled" 40 times as many replies as advertisement No. D, which was the poorest of the five, while it cost but one sixth as much as No. D. We have here then complete agreement between the results secured from the "order of merit method" and actual results in business.

SECTION 4. *Results from a Set of Packer's Tar Soap Advertisements*

As recorded in Chapter VII., a series of fifty Packer's Tar Soap advertisements were arranged according to the "order of merit"

[11] In every case throughout these experiments except with the Packer's Tar Soap Ads "1" represents the highest grade; the lowest grade will be equivalent to the number of advertisements used, *e. g.,* here it is "5."

method in the order in which they would lead the subject to buy the soap. Twenty-five subjects were employed. When the order was compared with the order submitted by Mr. Edward A. Olds, Jr., of the Packer Manufacturing Co., and with the one from the Blackman-Ross Advertising Agency, we found a high degree of similarity between the three orders. The resemblance between the experimental order and either of the other two is equal to a coefficient of correlation[12] of + .52. The resemblance between the order of the Packer Manufacturing Co. and the Blackman-Ross Agency is equal to + .64. There is then nearly as great agreement between the experimental order and that of the Packer Manufacturing Co. as between the latter and the agency, which is now handling their advertising business.

Eight advertisements were then chosen from the fifty for a more detailed study of the reliability of the "order of merit" method. The advertisement that *averaged* highest among the twenty-five subjects was first selected. It has a rank of + 64 on the experimental scale (see Table XXI.). Then seven other advertisements were so selected that there was approximately an interval of 10 points between each advertisement. The eight advertisements so chosen and their position on the scale given in parenthesis are as follows: No. 29 (64), No. 48 (54), No. 39 (44), No. 40 (34), No. 4 (24), No. 35 (14), No. 8 (2), and No. 19 (— 6). These advertisements were arranged in an order of merit by 100 subjects, 21 of whom were graduate men, 39 undergraduate men, and 40 undergraduate women. The ratio of 60 men to 40 women was preserved because the twenty-five subjects who sorted the entire fifty Packer's Tar Soap advertisements were composed of 15 graduate men and 10 undergraduate women. The maintenance of this ratio, however, introduced another source of error—the employment of undergraduate men, which from the results appears to be a more serious error than a deviation from this ratio would have been.

The directions given the hundred subjects for the sorting of the eight advertisements are as follows:

[12] To those unfamiliar with this term I might explain that a coefficient of correlation is a mathematical term expressing the relationship between two groups of data taking into account the specific variabilities of each datum from its central tendency. A coefficient of + 1.00 represents complete agreement between the two groups of data, a coefficient of — 1.00 represents complete reversal of this relationship, and a coefficient of 0 represents no relationship between the two groups other than that due to mere chance. For example, a coefficient of + .40 represents the relationship between the physical or mental traits in brothers and + .80 represents similarly the relationship of these traits in twins. (Cf. Thorndike, "Mental and Social Measurements," Chap. IX., or Whipple, "Manual of Mental and Physical Tests," pp. 27–46.)

DIRECTIONS

Look over these eight advertisements.
Then arrange them in the order in which *you* would *buy* the soap.
Take for granted that each advertisement represents a different make of soap.

Table II. gives the results of the 100 sets of judgments. The first section of the table gives the position of the *median*[13] judgment with its average deviation (A.D.) for each of the eight advertisements as determined by the 21 graduate men. The second section gives the results for the 39 undergraduate men and section three gives the total judgment for the 60 men. Section four similarly gives the results for the 40 women, and section five for the

TABLE II

GRADES AND AVERAGE DEVIATIONS OF EIGHT PACKER'S TAR SOAP ADVERTISEMENTS

(In the first five sections a grade of "1" is the highest possible and "8" the lowest, in the last three sections a grade of "100" is the highest and "—100" the lowest.)

Advertisements	21 Graduate Men		39 Undergraduate Men		60 Men (Total)		40 Women		100 Subjects (Total)		25 Subjects	Packer Mfg. Co.	B. R. Agency
No.	Med.	A.D.	Med.	A.D.	Med.	A.D.	Med.	A.D.	Med.	A.D.	Med.		
29	2.5	*1.2*	4.3	*1.6*	3.5	*1.7*	2.5	*1.6*	**3.2**	*1.7*	**67**	**71**	**36**
48	2.7	*1.2*	2.3	*1.5*	2.5	*1.4*	2.3	*1.3*	**2.4**	*1.4*	**54**	**100**	**33**
39	2.8	*1.9*	4.8	*2.2*	3.5	*2.2*	3.5	*2.0*	**3.6**	*2.1*	**43**	**88**	**52**
40	3.8	*1.7*	3.7	*1.2*	3.7	*1.4*	4.9	*1.5*	**4.1**	*1.6*	**35**	**73**	**6**
4	5.5	*1.7*	5.3	*1.7*	5.4	*1.7*	5.4	*1.6*	**5.3**	*1.7*	**25**	**67**	**0**
35	5.2	*1.1*	4.1	*1.9*	4.8	*1.7*	4.8	*1.7*	**4.8**	*1.7*	**11**	**58**	**— 8**
8	6.6	*1.0*	5.5	*1.7*	6.1	*1.5*	6.0	*1.6*	**6.1**	*1.6*	**10**	**0**	**—17**
19	7.0	*1.2*	7.4	*1.1*	7.3	*1.2*	7.0	*1.4*	**7.1**	*1.3*	**—10**	**— 6**	**—17**

[13] With a few exceptions the median has been used throughout this report instead of the average as a measure of the central tendency of the group. It may be defined as the measure (or datum) above which and below which are equal numbers of the separate measures (or data). It is a better expression of the central tendency of the group in these experiments for it is very much less influenced by extreme cases (or data). What we want here is the position to which an advertisement is assigned by the majority of persons, not the average of the good, bad, and indifferent judges. This the median gives whereas the average takes into account the quality of the judgment. In other words, erratic judgments influence the median less than they do the average. Another point in its favor is the ease with which it may be calculated. Cf. Thorndike, "Empirical Studies in the Theory of Measurements," pp. 1–4.

In this particular case the average as well as the median judgments support the above facts.

entire 100 subjects. Section six gives the average position and the median position as assigned by the twenty-five subjects (see Chapter VIII.). The figures in section six are not comparable with those of the other sections but do indicate the order of preference. This section also gives the position assigned these advertisements by the Packer Manufacturing Co. and the position assigned them by the Blackman-Ross Advertising Agency.

The 25 subjects grade them in the following order: No. 29, 48, 39, 40, 4, 35, 8, and 19. The Packer Manufacturing Co. place No. 48 first, No. 39 second, and No. 29 third and the remainder in the above order. The Blackman-Ross Agency place No. 39 first, No. 29 second, No. 48 third, and the remainder as above.

The 100 subjects rank No. 48 first (thus agreeing with the Packer Manufacturing Co.), No. 29 second, and the remainder as do the 25 subjects except that they rank No. 35 above No. 4. This interchange of positions of these two advertisements is found in all of the subgroups and also among individuals who served as subjects both among the 25 and 100. I believe, it is to be fully explained by the fact that advertisement No. 4 was badly torn at the start of the experiment with the 100 subjects and when mended became badly wrinkled. This injury to its appearance caused it to be ranked lower than it would have been if not torn. This particular discrepancy between the results of the 100 and the 25 subjects should not then be counted as a weakness in the reliability of the method. The 21 men in the first subgroup rank No. 29 slightly higher than No. 48, while the women reverse the order with a similarly slight difference. The median for these 61 subjects results in ranking both at 2.5 but if the men are weighted to give a ratio of 15 to 10, No. 29 is given a position of 2.44 and No. 48 a position of 2.50. As has been pointed out before, the introduction of the 39 undergraduate men to secure a ratio of 60 men to 40 women introduced the error of comparing one group of two "select" classes with a second group of three "select" classes. The superiority of No. 48 to No. 29 as shown in the results of the 100 is hence due to the use of undergraduate men as subjects—a glance at their median judgments makes this even more evident. It is very evident then, that, if a similar selection of subjects had been used and No. 4 had not been mutilated, there would have been complete agreement between the two experimental orders of preference.

However, considering the order of the 100 as obtained in the experiment, we have the following correlations between the four orders of preference:

Order of the 100 subjects	and the order of the	25 subjects	+ .947
"	"	"	Packer Mfg. Co.	+ .893
"	"	"	B-R. Agency	+ .866
"	25 subjects	"	Packer Mfg. Co.	+ .840
"	"	"	B-R. Agency	+ .920
"	Packer Mfg. Co.	"	"	+ .866

It had been hoped that it would be possible to obtain some data which would throw light on the perplexing question whether with equal differences in preference between the advertisements one would obtain approximately equal difference on the scale. Due to the two constant errors, as pointed out above, this point can not be determined here.

SECTION 5. *Conclusion*

The results from the one short set of advertisements show perfect agreement with results as reported by the business house as to their relative "pulling-power." Similarly the results from a set of fifty advertisements show nearly as close agreement with either of two reports from advertising experts as do the two advertising experts agree with each other. When eight of these advertisements are used, we have an extremely high agreement between the two experimental results and a slightly less high agreement between either of the two experimental results and the two reports of the advertising experts. Since we have no data to the contrary, we must conclude that the method does give results closely in accord with actual advertising returns.

CHAPTER III

Preliminary Experiments

Section 1. *Experiment I*

Four sets of advertisements taken from current monthly magazines were used in this experiment. Each set consisted of ten advertisements of the same commodity, namely, vacuum cleaners, pianos, breakfast foods, and toilet soap. (On Plates I. to IV. are shown photographs of these forty advertisements.) Ten subjects, including myself,[1] judged the advertisements in each set and arranged them in the order as to their merit in creating a desire for the article advertised. The advertisement that displayed the article most to be desired was ranked "1," while the advertisement that displayed the article least to be desired was ranked "10." We then have ten judgments upon each of the four sets of ten advertisements. The ten subjects consisted of 3 graduate students in psychology, 1 graduate student in economics, 2 college graduates engaged in engineering work, 1 clerk in an engineering office, 1 negro elevator-man, 1 senior at Barnard College, and 1 middle-aged woman. (The data of this experiment will be considered in connection with the following experiment.)

Section 2. *Experiment II*

This experiment was presented to the students in Psychology 7–8 at Barnard College during April, 1910, as one of the regular experiments of the course. Below is the wording of the instructions as given.

The Psychology of Advertising

Apparatus.—There are in this experiment three groups of advertisements. Each group contains ten different advertisements dealing with a particular commodity.

Procedure.—Consider each group entirely by itself and in the following order: (1) pianos, (2) vacuum cleaners, and (3) breakfast foods.

Read through the ten advertisements of the first group and then arrange them in a descending series of merit as to *persuasiveness,* so that the advertisement on the top of the pile (when you are through) will represent the article you most prefer (as judged from the advertisements themselves and not from previous experience); so that the second advertisement in your pile will represent

[1] At that time I had no idea how the results would turn out and so could serve as well as any one else as a subject.


—Anyone who can afford brooms can now afford the best Electric Suction Cleaner made
1
One Dollar
Puts the
"RICHMOND" Suction Cleaner
In Your Home
The McCrum Howell Co
2
Get the dust out of your home—it's dangerous
The Santo more than saves its cost every year but best of all it saves the woman
3
—Anyone who can afford brooms can now afford the best Electric Suction Cleaner made
You see here an Electric Suction Cleaner which weighs but TEN pounds—instead of sixty
$1 PUTS THE RICHMOND Suction Cleaner In Your Home $1
4
HEALTH-ECONOMY-EFFICIENCY REALIZED in The Invincible JUNIOR
Realized at last in a moderate priced air cleaning machine for the home
"Invincible Junior" Electric Renovator
5
To the Public: AN EXPLANATION AND A PROMISE
The Ideal Vacuum Cleaner
"It Eats Up the Dirt"
6
A Pound of Flour
Scott Suction Cleaner
Price, $59.75
Sears, Roebuck and Co.
7
The New York Vacuum Cleaner
NEW YORK VACUUM CLEANER CO.
8
What the Bath Tub means for Personal Cleanliness
The Ideal Vacuum Cleaner means for Household Cleanliness
The Ideal Vacuum Cleaner
It Eats Up the Dirt
9
Up from Slavery
New York Vacuum Cleaner
10

PLATE I.

the article which you next prefer, etc.; until you have the advertisement at the bottom of the pile representing the article which you least desire. Then consider the second group in the same manner and then the third.

"*Persuasiveness,*" as used here, may be explained thus: The most persuasive advertisement would show the article you would choose out of the ten if you had to base your choice solely upon the information supplied you in the ten advertisements.

When you have finished sorting the three groups report to the instructor and receive further instructions.

The above comprised Sheet No. 1. This was given to the student together with the three sets of advertisements. Only after they had completed their judgments was Sheet No. 2 given them.

THE PSYCHOLOGY OF ADVERTISING. SHEET NO. 2

Results.—Record the order of your preference of each group by noting the number of the advertisement as given in red pencil on the back of each advertisement.

State definitely, if possible, why you prefer the advertisements in the above order, noticing especially the first three and the last three.

Conclusions.—Does the reading-matter or the picture interest you the more in the article? Which convinces you the more as to the value of the article?

Do you feel any confidence in your arrangement as to preference?

What would constitute your ideal of an advertisement for (1) a piano and (2) a breakfast food?

The three sets of advertisements used here were the same as are shown in Plates I. to III. and as were used in Experiment No. 1. The set of toilet soap advertisements of Experiment No. 1 was, however, not used again. The data used below were from the first twenty women in the class to complete the work.

Table III. gives the average judgment of the 10 subjects of Experiment No. 1 and the 20 subjects of Experiment No. 2 together with their average deviations.

SECTION 3. *Experiment III*

This experiment served mainly as a preliminary study to ascertain if definite results might be obtained from "made-up" advertisements. It was made necessary due to the recognition shown in Section 4 of this chapter that there was a different reaction to "picture" from "non-picture" advertisements. And as it was deemed well-nigh impossible to classify motives as portrayed in pictures, it was judged best to confine the remainder of the study to "non-picture" advertisements.

The experiment consisted in the judging of three sets of ten advertisements each pertaining respectively to vacuum cleaners,

TABLE III

DATA FROM EXPERIMENTS I. AND II. GIVING GRADES AND THE AVERAGE DERIVATIONS FOR THE FOUR SETS OF TEN ADVERTISEMENTS

Ten Vacuum Cleaner Ads.

Ad. No.	10 Subjects Av.	10 Subjects A.D.	20 Subjects Av.	20 Subjects A.D.
1	3.3	1.4	4.7	2.3
7	3.6	2.2	5.0	2.3
4	5.3	2.6	5.4	2.5
3	5.5	2.0	5.5	2.4
10	5.5	2.6	5.0	2.2
5	5.6	1.5	6.7	2.0
8	5.8	1.8	5.6	2.6
6	6.6	2.5	7.1	2.2
9	6.9	2.3	5.3	2.4
2	6.9	3.1	4.5	3.1

Ten Piano Ads.

Ad. No.	10 Subjects Av.	10 Subjects A.D.	20 Subjects Av.	20 Subjects A.D.
4	4.4	1.8	6.5	2.6
8	4.5	2.3	5.9	2.2
1	4.6	2.2	4.7	2.5
5	5.2	2.4	5.3	2.9
7	5.4	2.1	5.7	2.2
2	5.7	3.4	5.2	2.0
10	5.8	1.9	5.2	2.6
6	6.1	2.3	4.0	2.1
9	6.1	3.7	6.6	2.6
3	6.8	2.0	6.7	2.5

Ten Breakfast Food Ads.

Ad. No.	10 Subjects Av.	10 Subjects A.D.	20 Subjects Av.	20 Subjects A.D.
2	3.7	3.0	3.9	3.0
3	3.9	1.9	4.2	1.8
7	4.8	1.8	6.5	1.4
10	5.4	2.7	4.7	2.5
6	5.5	2.3	5.2	1.6
4	5.9	2.3	6.0	2.3
9	6.1	2.3	6.5	2.9
1	6.2	2.4	4.7	3.4
8	6.3	1.8	6.4	2.6
5	7.2	2.4	7.2	1.4

Ten Toilet Soap Ads.

Ad. No.	10 Subjects Av.	10 Subjects A.D.
10	2.7	1.2
3	3.6	2.4
9	4.8	2.0
1	5.0	1.0
2	5.0	2.2
5	5.3	2.0
6	5.6	2.0
8	6.1	1.9
7	8.2	1.6
4	8.7	1.5

pianos, and breakfast foods. The aim was to so word each advertisement as to suggest but one motive and that the characteristic statement of the corresponding advertisement in Experiment No. 1. In a few cases the writer's own words were substituted for some phrase or they were given in an endeavor to state in words the idea, he conceived the picture in the advertisement was meant to convey. The numbering of the advertisements corresponds respectively with the numbering in Experiment No. 1. The ten subjects and the directions for their judgments were the same as in that experiment.

Following are the 30 "made-up" advertisements as used in this experiment.

1. THE TEN VACUUM CLEANER ADVERTISEMENTS

1. Any one, who can afford a broom, can now afford the best Electric Suction Cleaner made.

Vacuum Cleaner No. 1 costs less per month for electricity than the average family spends for brooms.

Its total cost is less than the cost of one single annual house-cleaning, to say nothing of saving the wear and tear which house-cleaning brings to the furniture.

2. ONE DOLLAR puts the *Vacuum Cleaner No. 2* in *your house.*

Our guarantee is absolute. If, within one year, the machine breaks or shows defect, it will be replaced with a new one. This is the guarantee of a $3,000,000 company.

You are paying the price of a Suction Cleaner now, anyway, whether you have one or not, paying it in needless house-cleaning, paying it in hard sweeping and dusting, paying it in the damage which dust does to your furniture.

A *single dollar* will save this waste.

3. Get the *dust out of your home*—it's dangerous.

No member of your family is wholly safe from contagious diseases until every particle of *dirt* and *dust* is removed. To be safe, your home should be *dustless.*

Please remember this one fact—you can not have a *dustless home* without *Vacuum Cleaner No. 3.* Thorough house-cleaning is impossible without it.

4. Here you have an Electric Suction Cleaner that weighs but *ten* pounds—instead of *sixty.*

With it you may clean by electricity without lugging a 60- to 80-pound machine from room to room—upstairs and down—the first really portable machine to be placed on the market.

Vacuum Cleaner No. 4 does all that any cleaner or suction cleaner can do.

5. See other machines at work, if you like. Compare them with *Vacuum Cleaner No. 5.* Note its superiority to them in its *light weight,* hence easily carried about; the *ease with which* the attachments are made; and the *ease with which it is operated.*

A boy or girl can clean house with it far better than several strong men and women can clean house without it.

6. Thousands of persons who have ordered *Vacuum Cleaner No. 6* have been unable to get their machines except by waiting two or three weeks or longer.

Our factory, which, when we began advertising in April, 1908, had a capacity of 30 machines a day, has literally been swamped. So helpless were we under the flood of orders that we were forced to suspend advertising for two months.

Now, however, we are able to assure the public of our ability to fill all orders on the day received.

We now have over 100,000 sq. ft. of floor space, with an output of 500 machines a day and the ability to increase it to 1,000 machines, or about 30,000 per month.

7. *A Pound of Flour.*

We scattered a pound of flour over a clean rug and worked it right down into the fabric.

Then we swept the rug with a broom for ten minutes and recovered just 2 ounces of flour mixed with nap.

But five minutes' work on the same rug with the *Vacuum Cleaner No. 7* took up 12 ounces more, six times as much flour in half the time, and not a shred of nap.

8. No home can really be *healthfully clean* without *Vacuum Cleaner No. 8.*

Think of the countless number of *disease germs* in the dust of the ordinary room. Would you free yourself of this ever-present danger?

No other invention has ever done so much for absolute *safety* in the home by doing away with *dirt* and *disease.*

9. What the Bath Tub means for *Personal* Cleanliness, *Number Nine Vacuum Cleaner* means for *Household* Cleanliness.

It was not so many years ago that such a thing as a household bath tub was unknown among any class.

The standard of *household* cleanliness now has been raised just as high as the standard of *personal* cleanliness. The grand semi-annual housecleaning is just as much out of date as the grand weekly bath. The most thorough possible cleaning every day—that is the new standard for home as well as person.

10. *Up from Slavery.*

Up from *servitude*—up from the three D's, Dirt, Disease, and Drudgery. *Vacuum Cleaner No. 10* saves the *woman.* It is the new servant in the house.

2. The Ten Piano Advertisements

1. *Piano No. 1* is associated in the minds of all, who know it, with artistically furnished rooms, with beautiful surroundings of sculpture and painting, and all that goes to make a cultured home.

2. *Piano No. 2* may justly be termed one of the *Institutional Products* of America.

Throughout almost four decades of the last century and of the opening decade of the present, all the thought, effort, and experience of the original founders and their direct family successors have been steadfastly consecrated to the intense purpose of making the *Best Piano* that human hands could fashion.

3. The *Tone-Poetry* of Chopin—
his inmost soul's dream-images are at last perfectly revealed through the exquisite *tone shadings* of *Piano No. 3.*

4. *Start the Children Right.*

To accustom the delicate and maturing hand of the child to the great possibilities of a properly built piano action, such as *Piano No. 4* possesses, not only facilitates correct technique, but avoids the necessity of subsequently unlearning faults that retard correct advancement.

5. Why pay $700 to $800 for an Upright Piano when *Upright Style M* of *Piano No. 5* is offered for the moderate price of $550.

We are able to make this extraordinary price because of the great number of pianos we are handling and because we sell direct from factory to consumer.

6. The desire of so many to have a grand piano has induced most makers to produce tiny "grands"—so little that they serve only to accent the value of the upright. The size of these pianos is a concession to convenience and fashion—a fad—that does not consider musical effect. When a piano is less than 5 feet 10 inches in length it ceases to be a grand piano. It is an arrested development. It is only a piece of furniture.

Piano No. 6 has all the characteristics of our great concert grands, in modified volume. It is a Real Grand Piano: it occupies a distinctive position between the uprights and the larger grands: there is a *scientific reason for its size,* 5 feet 10 inches.

7. *The Many Moods in Music* insure to more people *happy, self-forgetting recreation* than any other form of entertainment.

The pleasure of a great and soothing art is trebled by *Piano No. 7.*


Chickering
THE OLDEST IN AMERICA THE BEST IN THE WORLD
Made Solely by CHICKERING & SONS
KRANICH & BACH PIANOS
WRITE FOR
EVERETT Tone is the Heart of Harmony
The EVERETT PIANO
THE JOHN CHURCH COMPANY
Kranich & Bach PIANO
START THE CHILDREN RIGHT
Kranich & Bach, 233 45 E. 23rd Street New York
Chickering
Chickering & Sons.
FACTS About Grand Pianos
THE STEINWAY MINIATURE GRAND
STEINWAY & SONS
The Baldwin Piano
The Baldwin Company
The Knabe
The EVERETT PIANO
Everett Tone is the Heart of Harmony
THE JOHN CHURCH COMPANY
The Baldwin Piano
The Baldwin Company
1
2
3
4
5
6
7
8
9
10

PLATE II.

8. Where others have failed to build a small yet perfect Grand Piano, meeting present-day requirements, the *House of Piano No. 8,* after years of careful research and experiment, has succeeded in producing

The World's Best Grand Piano
in the small size of
5 feet, 2 inches.

9. The *World's Greatest Women Musicians, Chaminade, Carreno,* and *Nordica* use and unqualifiedly endorse *Piano No. 9.*

10. It is not through the glittering treble nor by way of the sonorous bass that a musician gets into the tone:

In the *Middle Register* lies the soul of the instrument, from which composers draw their inspiration. Extreme octaves serve for brilliancy, to color a harmony, or to glorify a climax.

Consequently the *Middle Register* has been especially considered in *Piano No. 10.*

3. The Ten Breakfast Food Advertisements

1. When you want to eat a mighty appetizing breakfast food remember *Breakfast Food No. 1.*

Children can hardly wait for breakfast; it's the same with adults.

2. In an article published in the *Saturday Evening Post* of March 10, 1909, Dr. Smith, the great medical authority on foods, says, about brain and muscle building:

"There is one kind of food that seems to me of marked value as a food to the brain and to the whole body throughout childhood and adolescence (youth).

"This food is the most nutritious of all the cereals, being richer in fats, organic phosphorus and lecithins."

It is from this cereal, exclusively, that *Breakfast Food No. 2* is made.

3. *Breakfast Food No. 3,* when served alone, seems as good as anything can be.

But try serving it in a dish of *Sliced Bananas.*

Then judge if any breakfast dish was ever more inviting.

4. Sometimes the *men* know best.

All *men* like *Breakfast Food No. 4.*

Why not try some?

5. Lack of success in life is due to Mental Dulness.

Mental Dulness usually comes from imperfectly nourished brains.

Breakfast Food No. 5 is promptly digested and contains the ingredients necessary for a well-nourished brain.

6. *Brain-power* is what wins to-day. Brute force can not compete with well nourished "gray matter."

Breakfast Food No. 6 is the ideal brain and nerve food. It is quickly digested, and the phosphate of potash combines with albumen in the system to form new brain and nerve cells.

7. Did you ever examine *Breakfast Food No. 7* through a magnifying glass?

Upon every granule will be seen small, shining crystals of sugar.

Our patented process alone brings about this formation of sugar from the starchy cereal, thus providing the elements nature uses for rebuilding the brain and nerve centers.

8. Thanksgiving Breakfast would be as cheerless without *Breakfast Food No. 8* as the dinner would be without the *Turkey.*

9. Cereals are the food of the ages. But never before were these cereals put into such inviting form.

It is done by Professor White's process—by putting the whole kernels into sealed steel tubes. Then the tubes are revolved for sixty minutes in a heat of 540 degrees.

The heat turns the moisture in the grains to steam and creates an enormous pressure. Then the tubes are opened and the steam explodes. Instantly every starch granule is blasted into a myriad of particles.

Imagine such a food. Do you wonder at the wide demand for *Breakfast Food No. 9?*

10. You can improve your *health* in 30 days by increasing the amount of *Breakfast Food No. 10* you eat.

Millions have found that frequent and regular eating of this food resulted in clearer skins, rosier cheeks, firmer muscles, and clearer and more active minds.

Table IV. gives the average judgment and the average deviation (A.D.) of the ten subjects with respect to the three sets of advertisements.

Table V. states the order of preference of the actual advertisements used in Experiment I. and the "made-up" advertisements based on them, which were used in Experiment III. The correlations between the average judgments of the ten subjects in Experiment I. and Experiment III. indicate that the "made-up" advertisements are essentially different from the actual advertisements. Consequently no valid comparison of the two sets can be made.

A discussion of the data concerning the Vacuum Cleaner advertisements and the Piano advertisements will be found in Chapter IV.; concerning Breakfast Food advertisements in Chapter V.; and concerning Toilet Soap advertisements in Chapter VI.

Section 4. *Picture vs. "Copy" Advertisements*

Before passing on to a detailed consideration of the results from these experiments let us note a peculiarity in the data of Experiment II., which is not shown in the results of Table III. An analysis of the data from the twenty Barnard students showed that the group could be divided into two parts, each of whose members judged in the main with the subgroup she was in. Table VI. shows this division on the basis of their reaction to advertisement No. 1 and the figures opposite each subject show the *deviation* from the individual's ranking of each advertisement and the average ranking of the twenty. The algebraic signs (+ and —) indicate whether the individual judgment ranks the advertisement higher or lower than that of the average. A study of this table reveals the striking fact that as regards advertisements Nos. 1, 2, 7, 8, and 10 these two sub-groups react as units and where the first group[2] ranks the particular

TABLE IV

DATA FROM EXPERIMENT III. GIVING GRADES AND AVERAGE DEVIATIONS FOR THE THREE SETS OF TEN "MADE-UP" ADVERTISEMENTS

Vacuum Cleaner Ads.			Piano Ads.			Breakfast Food Ads.		
Ad. No.	Av.	A.D.	Ad. No.	Av.	A.D.	Ad. No.	Av.	A.D.
5	4.0	*1.6*	6	3.6	*1.9*	2	2.7	*2.0*
4	4.2	*2.2*	2	3.8	*1.4*	6	4.2	*1.6*
7	4.2	*2.4*	4	4.7	*2.0*	3	5.1	*2.3*
2	4.3	*2.3*	10	4.7	*2.5*	1	5.2	*3.2*
1	5.2	*1.8*	9	5.0	*2.8*	9	5.4	*2.5*
3	5.2	*2.4*	3	5.7	*2.8*	10	5.9	*1.9*
10	6.1	*3.3*	5	5.8	*1.6*	7	5.9	*1.9*
9	6.3	*2.0*	8	6.5	*2.0*	5	6.1	*2.1*
8	7.0	*1.6*	1	7.5	*2.3*	4	7.0	*2.4*
6	8.5	*1.4*	7	7.7	*1.5*	8	7.5	*2.1*

TABLE V

ORDER OF PREFERENCE OF ADVERTISEMENTS USED IN EXPERIMENTS I. AND III.

	Vacuum Cleaner Ads.		Piano Ads.		Breakfast Food Ads.	
Position	Exp. I No.	Exp. III No.	Exp. I No.	Exp. III No.	Exp. I No.	Exp. III No.
1	2	5	8	6	1	2
2	4	4	7	2	2	6
3	6	7	2	4	8	3
4	7	2	5	10	3	1
5	3	1	6	9	5	9
6	9	3	3	3	6	10
7	8	10	4	5	9	7
8	10	9	1	8	4	5
9	5	8	9	1	7	4
10	1	6	10	7	10	8
Correlation	+ .05		—.26		+ .33	

advertisement considerably above the average judgment of the 20, the other group ranks it considerably below, or vice versa. Not only is this true if the averages of the groups are considered but also if we consider the individuals separately in the groups. The differences are great enough to warrant one in supposing that the two groups are each composed of subjects who constantly judge in an opposite manner. As regards the group judgments of the other five advertisements we find no appreciable difference in their ratings. A study of advertisements Nos. 1, 2, 7, 8, and 10 shows that No. 2

[2] The division into two groups has apparently no correlation with imagery nor scholarship, if the final mark in Psychology 7–8 can be interpreted as a judge of the latter.

TABLE VI

Showing the Deviation between each of the Subjects' Judgments and the Average Judgments of the 20 Subjects for each of the Ten Breakfast Food Advertisements

Sub-	Advertisements									
jects	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7	No. 8	No. 9	No. 10
A	— 5.3	+ 2.9	— 2.9	— 2.1	+ 4.2	+ .2	+ .5	— 2.6	+ 2.5	+ 2.7
B	— 5.3	+ 2.9	+ 1.2	+ 2.0	— .9	— 1.9	+ .5	+ 1.4	— 2.5	+ 3.7
C	— 4.3	+ 1.9	+ 1.2	— .1	+ .2	— 2.9	+ 1.5	— 3.6	+ 2.5	+ 2.7
D	— 4.3	+ 2.9	+ 1.2	— 4.1	+ 3.2	+ .2	+ .5	— 1.6	+ 4.5	— 2.4
E	— 4.3	+ 1.9	— .9	+ 3.0	— 2.9	— 1.9	+ .5	+ 5.4	— 1.5	+ .7
F	— 3.3	+ 2.9	+ 1.2	+ 2.0	+ .2	+ .2	+ 4.5	— 3.6	— 2.5	— 1.4
G	— 3.3	+ 2.9	+ .2	+ 4.0	— 2.9	+ .2	+ .5	— 2.6	— .5	+ 1.7
H	— 3.3	+ 2.9	— 1.9	— 3.1	+ 4.2	+ 3.2	+ 1.5	— .6	— 3.5	+ .7
Av.	— 4.2	+ 2.6	— .1	+ .2	+ .7	— .4	+ 1.3	— 1.0	— .1	+ 1.0
I	— .3	+ 2.9	+ 2.2	+ 3.0	— 1.9	— 2.9	— .5	+ 2.4	— .5	— 5.4
J	+ 3.7	— 1.2	— 2.9	— 2.1	— 2.9	— .9	— 2.5	+ 4.4	+ 3.5	+ .7
K	+ 3.7	— 6.2	+ 1.2	— .1	— .9	+ 3.2	— .5	— .6	+ 1.5	— 4.4
L	+ 3.7	+ 1.9	— 1.9	+ 1.0	— 2.9	— 3.9	— .5	+ 3.4	— 1.5	+ .7
M	+ 3.7	+ .9	— .9	+ 4.0	+ 1.2	— 1.9	— 1.5	+ 2.4	— 2.5	— 5.4
N	+ 3.7	— 3.2	+ 1.2	— 4.1	+ 2.2	+ 1.2	+ .5	+ 4.4	— 2.5	— 3.4
O	+ 3.7	— 6.2	+ .2	+ 1.0	+ .2	— .9	— 1.5	+ 2.4	+ 3.5	+ 2.7
P	+ 2.7	— 6.2	+ 1.2	— 1.1	+ 1.2	+ 1.2	— 2.5	+ 1.4	— 1.5	+ 3.7
Q	+ 2.7	— 4.2	+ 3.2	+ 2.0	+ .2	+ .2	+ .5	— 3.6	— 2.5	+ 1.7
R	+ 2.7	— 4.2	+ 3.2	— 2.1	+ 2.2	+ 1.2	— 2.5	— 3.6	+ 3.5	— 1.4
S	+ 1.7	+ 1.9	+ .2	— 3.1	— 2.9	+ 4.2	— 1.5	— .6	+ 1.5	— 1.4
T	+ 1.7	— 5.2	— 5.9	+ 2.0	— .9	— 1.9	— .5	+ .4	+ 2.5	+ 3.7
Av.	+ 3.1	— 2.9	— .1	— .2	— .3	+ 1.0	— 1.1	+ .9	+ .5	— .3

and No. 10 have no picture in the advertisement, No. 1 is all picture, No. 8 is three fourths picture, and No. 7, although only one half picture, appears to me as predominantly a picture-advertisement, for it is the picture that holds the attention and not the reading matter. We see now that, with the exception of No. 7, the first group in Table VI. ranks the advertisements *with* pictures high and the advertisements *without* pictures low, while the second group ranks them in just the reverse manner. Just why this exception arises in the case of advertisement No. 7 is difficult to say. It may be that it should not be classed as a predominantly picture-advertisement. Or that it was reacted to by these two groups on other grounds. Or, as I believe, the exception may be explained on the ground that preference is always positive in nature; either we like a thing or we dislike it. As this advertisement was ranked low by the whole twenty, it indicates a positive dislike which would result in the first group consciously ranking it lower than the second group because the picture did not appeal to them. The reading-matter of the advertise-

1
CREAM OF WHEAT
2
The highest medical authority on foods,
Sir James Crichton Browne, LL.D.—F.R.S.
gives the best reasons for eating more
Quaker Oats
The Quaker Oats Company
3
Puffed Wheat or Rice with Bananas
Whole Grains Made Wholly Digestible
Puffed Wheat, 10c
Puffed Rice, 15c
Made Only by the Quaker Oats Company
4
Foods Shot from Guns
Puffed Wheat
Puffed Rice
How to Get Your Wife
To Buy Puffed Wheat
She Will Do It
Puffed Wheat, 10c
Puffed Rice, 15c
Hearty—Digestible
Made only by The Quaker Oats Company
5
Mental Dullness
Grape-Nuts
FOOD—
"There's a Reason"
6
Brain Power
Grape-Nuts
"There's a Reason"
7
Have a Look!
Grape-Nuts
"There's a Reason"
8
CREAM of WHEAT
Christmas
Breakfast
Cream of Wheat
FOR SALE BY ALL GROCERS
CREAM OF WHEAT CO.
9
The Foods Shot from Guns
To the Million We Kept Waiting
The Foods of the Ages in Enticing Form
Puffed Wheat, 10c
Puffed Rice, 15c
Made only by
THE QUAKER OATS CO.
10
YOU can improve your health in thirty days by increasing the amount of Quaker Oats you eat
The Quaker Oats Company

PLATE III.

ment on the other hand did not so strongly not-appeal to the second group and was therefore not consciously judged poor but found a place between the two extremes.

Table VII. states in another way the validity of the division of the twenty subjects into two groups. The table gives the average of the correlations of six of the subjects of each of the two subgroups with each of the other nineteen subjects. (These subgroups of six happened to be the first six in each group as originally arranged.) The averages show but one exception to the fact that each subject of the twelve so considered correlates higher with the various members of her group than with the members of the other group. I might add that exceptions in the individual correlations are rare.

As will be noted frequently in the following chapters, this is a very common phenomenon and, I understand, advertising men are familiar, at least to some extent, with this fact. As a practical suggestion it would seem then that the "ideal" advertisement should consist of half picture and half "copy," so as to catch the attention and hold the interest of both groups of readers.

TABLE VII

SHOWING THE AVERAGE OF THE CORRELATIONS BETWEEN EACH OF SIX MEMBERS OF THE TWO SUBGROUPS WITH ALL THE OTHER MEMBERS OF THESE GROUPS

Subjects	Average Correlation With Her Own Group	Average Correlation With the Other Group	Subjects	Average Correlation With Her Own Group	Average Correlation With the Other Group
C	+ .54	— .13	J	+ .30	— .19
D	+ .31	— .17	K	+ .24	— .40
E	+ .25	— .09	O	+ .23	— .22
F	+ .49	— .09	P	+ .31	— .28
G	+ .46	+ .02	Q	+ .26	— .04
H	+ .38	— .20	T	+ .11	+ .20
Av.	+ .40	— .12	Av.	+ .24	— .16

CHAPTER IV

Vacuum Cleaner and Piano Advertisements

Section 1. *Consideration of the Data Concerning Vacuum Cleaner Advertisements*

The results (see Table III. and also Plate I. for illustrations of the advertisements) do not show any very decided preferences. The group of ten subjects ranks advertisements No. 1 and No. 7 with positions of 3.3 and 3.6; Nos. 4, 3, 10, 5, and 8 with positions from 5.3 to 5.8; while Nos. 6, 9, and 2 are rated considerably lower, *i. e.,* 6.6, 6.9, and 6.9 respectively. The group of twenty Barnard students rate Nos. 2, 1, 10, 7, 9, 4, 3, and 8 between 4.5 and 5.6 and place Nos. 5 and 6 considerably lower, *i. e.,* 6.7 and 7.1 respectively.

Advertisements Nos. 1, 7, and 8 consist of a good large "relevant cut" with "reason-why" reading matter. The first two are ranked first and second by the men and second and fourth by the women. We may say, therefore, that they are preferred to the others by the thirty judges. Advertisement No. 8, on the other hand, is ranked seventh by the men and eighth by the women. Its superlative language ("the most wonderful Cleaning Device the World has Ever Known," etc.) probably accounts for its position.

Advertisement No. 2, which is actually the second page of a two-page advertisement in which No. 1 is the other page, was ranked on an average last by the men and first by the women. But when we consider the judgments of the women in detail we find that 12 of them gave it a rank between first and fourth, while the other 8 gave it a position between sixth and tenth. This is another example of the curious phenomenon discussed in Chapter III., namely that approximately half of these 20 women judge a picture advertisement high and a non-picture advertisement low, while the other half judge them in just the reverse manner. Advertisement No. 2 stands, consequently, first in the judgment of 12 women of the group of 20 and of 3 of the group of 10 men and last in the judgment of the other 15 of the two groups. Advertisements Nos. 4, 3, 10, 5, and 9 have smaller and less clear-cut pictures. They are grouped approximately together and about the mid-point (5.5). The data present no clear preference between them.

Advertisement No. 6 depicts the factory and a statement of the "tremendous success" that has accompanied their business. The advertisement is ranked eighth by the men and last by the women. It is clear that its appeal is the least effective of all the ten.

A casual inspection of the ten advertisements shows that they are very complex in their make-up and suggest a great number of motives, and a study of the introspections shows that the different individuals were *consciously* influenced some by one, some by another of these motives. Consider, for example, these introspections of subjects who ranked No. 2 as first: "Cheap, price only one dollar, therefore, if not good, not much would be lost." "Guaranteed—responsible firm, claims to do a lot of work." "Appealed to me on account of the detailed description of the uses of the cleaner and the guarantee given." "I think the pictures influenced me considerably here because I could see probably how the thing worked." "Gives good plausible reasons why you need a cleaner, and why one of this particular kind." "Makes it worth while to buy one—gives good guarantee." On the other hand, the introspections are well-nigh unanimous in their condemnation of No. 6. "Poor picture; factory argument tiresome." "Unattractive picture and much uninteresting reading-matter." "Never like advertisements to make apologies for not being able to meet demand—does not ring true." "Too monotonous and uninteresting," etc.

Turning now to the data from Experiment III. (given in Table IV.) we note that advertisement No. 6 is again most decisively ranked last. Evidently an appeal based on the size of the factory or the number of sales does not interest or arouse the imagination of these subjects as compared with the other appeals. The predominating motives in the advertisements, which were given first, second, and third places, were "light weight," "easily adjusted," "easily operated," "more effective than a broom," and "saves carpet." Then come the motives of price: "guarantee," "installment plan," and "as cheap as present methods." And finally, in the advertisements ranked sixth to ninth appear motives of "dust and dirt," "cleanliness," "health," and "the saving of effort or work." We have here a progression from the specific reasons why a *certain* cleaner should be purchased to the general reasons why *any* cleaner should be purchased. Whether such a progression was in the minds of the judges is hard to say. Such a ranking of motives is, however, of great value if we have in mind the selling of a particular vacuum cleaner. The advertisement that sells its competitors' goods as well as its own is hardly a "howling success."

SECTION 2. *Consideration of the Data Concerning Piano Advertisements*

Very little reliance can be put upon the conclusions drawn from the data on Piano advertisements. (See Table III. for the data and Plate II. for illustrations of the advertisements.) The average judgments show that the preferences are slight, while the average deviations are in a number of cases very large, showing that there was no unanimity of judgment among the subjects. Advertisement No. 8 was the only advertisement that did not receive one judgment for first position and all of the ten received at least one judgment for last place. The correlation between the judgments of the two groups of subjects is but + .114.[2] That such should be the case seems to the writer one of the strong proofs of the validi[illegible] the whole method of procedure. The advertisements are com[illegible]d very much alike with the exception of No. 9 and No. 3. This the results indicate. The article advertised is one which is not bought at the instance of any one advertisement. Moreover, in all probability, none of the subjects used in the experiment had ever bought a piano nor was in a position to do so at that time. Under such conditions one should not expect decisive results and we do not get them.

However, there are a number of interesting points which are worth while to note. The two Everett Piano advertisements, No. 9 and No. 3, were ranked last by both groups. Introspections from subjects who ranked them high are as follows: "I liked those advertisements best with artistic pictures and those containing the opinions of great artists who use the pianos." "I ranked it second because it was endorsed by artists whose opinion one would be very likely to respect." "The words 'Everett Tone is the Heart of Harmony' with the simple pictures of Chaminade, Carreno, and Nordica—'the World's Greatest Woman Composer, Pianist, and Singer, respectively' made me give this advertisement first place. It was straightforward, plain, and convincing with none of the 'This is the best, the one, and the only genuine,' etc." But, on the other hand, a larger number of subjects are not attracted by such an argument. For example, "Simply the fact that Chaminade, Carreno, and Nordica used the Everett Piano is no reason why I should want to possess one." "Publishing the picture of a few artists does not satisfy me as to the value of the piano." "Nothing in an advertisement makes it appeal less to me than the mention of those people who use the article." "Stupid, sentimental (Everett tone, heart of harmony), inartistic, pretentious, basing its appeal on the judgment

[2] By the formula $1 - (6\Sigma d^2/n(n^2 - 1))$ as given in Myer's "Text Book of Experimental Psychology," p. 131.

1
Your Complexion
2
With Packer's Tar Soap
"YOU CANNOT BEGIN TOO EARLY."
The routine use of Packer's Tar Soap not only assures scalp cleanliness, but creates a healthful condition of the underlying glands, with all that this means in the control of dandruff and the growth and beauty of the hair.
3
IVORY
IVORY SOAP—IT STANDS ALONE.
Ivory Soap . . . . 99 44/100 Per Cent. Pure.
4
HEALTH, BEAUTY, ECONOMY.
To ensure the health of your skin and the full natural beauty of your complexion nothing will serve you so efficiently and so economically as
PEARS
which is Matchless for the Complexion
5
That Baby of Yours
6
For Finest Fabrics or Coarsest Carpets
PEARLINE
7
SAPOLIO
Mary, Mary, quite contrary, How does your housework go?
With greatest ease, sir, if you please, use Sapolio!
8
What Bobby Wants—and Should Have.
Ivory Soap . . . . It Floats.
9
"Brightening Things Up" for the Holidays.
Ivory Soap . . 99 44/100 Per Cent. Pure.
10
IVORY
Ivory is not the only good soap. There are others.
Ivory Soap . . 99 44/100 Per Cent. Pure.

PLATE IV.

of those, who have probably been paid to give it favorably. It gives the impression of fraud." "No. 3 a stupid advertisement, neither the reading nor the picture appealed to me. Picture didn't mean anything."

The response to three of these advertisements shows a sex difference. Advertisement No. 6, the only advertisement composed of all reading matter (except a very small cut), was ranked first by the women and eighth by the men. (Nine women ranked it very high and two women ranked it very low.) Whether it was the name of the piano—Steinway—or whether it was some other factor which caused it to appeal to the women is difficult to state. Advertisement No. 4, the Knabe Baby Grand advertisement, and No. 8, the Kranich and Bach advertisement, were ranked first and second, respectively, by the men and eight and seventh, respectively, by the women.

Advertisement No. 1 is ranked second by the women and third by the men. It is a Chickering advertisement displaying a grand piano in a beautiful parlor with a portentous staircase ascending to the left. Scott[3] concludes that this type is the ideal Piano advertisement associating, as it does, the Chickering Piano with an "atmosphere of cultured refinement and elegance." A Steinway Miniature Grand advertisement, without a picture is preferred to it by the women, while a Kranich and Bach "Start the Children Right" advertisement and a Knabe half-page cut advertisement are preferred to it by the men. On the whole, however, it would average first in position for the entire thirty subjects.

From Experiment III. (see Table IV.) we see that advertisement No. 7 was ranked last in that experiment while in Experiments I. and II., where the actual advertisements were shown, it was ranked much higher. As it appears in Experiment III., it consisted simply of a direct quotation of the first paragraph and the last half of the second paragraph of the actual advertisement. The advertisement is evidently robbed of its strength by the elimination of the picture. The aim of the advertisement is to appeal to the pleasure obtained from possessing such an instrument and thus stimulate desire for it. Such appeals, the writer believes, are the strongest for the creation of prospective buyers. Other appeals are necessary, however, to insure the sale of the specific brand.

[3] W. D. Scott, "The Psychology of Advertising," pp. 194–195.

CHAPTER V

Consideration of Breakfast Food Appeals

The following experiment on Breakfast Food advertisements was so planned as to avoid the difficulties encountered in the experiments of Chapters III. and IV. The scheme of "made-up" advertisements in those chapters was amplified so that they should more nearly resemble actual advertisements. In doing so it was necessary to broaden the appeals in order to make them more comparable with business conditions, but in doing this it becomes more difficult to accurately ascertain the value of any one appeal specifically. In this regard there are two extremes to be avoided—first, the statement of pure appeals and nothing else, and second, the use of actual advertisements with their many appeals. In the former case we no longer have advertisements to deal with and so frustrate our aim, while in the second case, we are forced to rely on our own or collective opinion as to just which one of the many appeals in the advertisement is the dominating one and also what is the force of the other appeals. Such a situation is as difficult to determine as the original question.

A set of twenty advertisements was prepared based to a very large extent on actual Breakfast Food advertisements. In many cases phrases or whole sentences were quoted, while in other cases the writer arranged the sentences endeavoring throughout to follow the phrasing characteristic of such advertisements. Each "made-up" advertisement was typewritten on a separate sheet of paper. The twenty sheets were given to the subject together with a sheet of "directions." These typewritten instructions were used to insure as uniform an approach to the sorting of the advertisements as was possible. Following are the "Directions" that were used:

Directions

Read through the twenty advertisements and then sort them into five piles according to their "persuasiveness." Pile No. 1 will represent the most persuasive, No. 2 the next, etc., with pile No. 5 as the least persuasive.

When the above is completed sort each pile as to their persuasiveness with respect to themselves. When you are through with the five piles you will have the twenty arranged in a descending series of merit as to persuasiveness..

The most *persuasive* advertisement will show the article you would choose out of the twenty if you had to base your choice solely upon the information supplied you in the twenty advertisements.

The subjects consisted of 29 women and 21 men. Of these there were among the women 22 undergraduate and 3 graduate students and 4 who were not in college, and among the men 11 undergraduate and 8 graduate students and 2 who were not in college. All the undergraduates were juniors or seniors at Barnard or Columbia college. The study of the relative merits of these twenty appeals is consequently based on the likes and dislikes of college men and women. The relation of the judgments of such a class of persons as compared with other groups of individuals is discussed at length in Chapter VI.

Following are the twenty advertisements used in the experiment.

BREAKFAST FOOD NO. 1

When you want to eat a mighty *appetizing* breakfast food remember Breakfast Food No. 1.

Children can hardly wait for breakfast; it's the same with adults—it's so good.

BREAKFAST FOOD NO. 2

In an article published in the *Saturday Evening Post* of March 10, 1909, Dr. F. E. Smith, the great medical authority on foods, said,—

"There is one kind of cereal that seems to me of marked value as a food. It is from this cereal, exclusively, that Breakfast Food No. 2 is made.

"I most heartily recommend its use for all."

BREAKFAST FOOD NO. 3

Do you like *Bananas?*

Try serving some Breakfast Food No. 3 in a dish of sliced bananas.

Then judge if they do not increase your enjoyment of the bananas and furnish you with a delightful breakfast dish.

BREAKFAST FOOD NO. 4

Sometimes the *men* know best.

Most men like Breakfast Food No. 4 while traveling.

Why not try some at home and let the rest of the family judge?

BREAKFAST FOOD NO. 5

Lack of success in life is due to Mental Dulness.

Mental Dulness usually comes from imperfectly nourished brains.

Breakfast Food No. 5 is promptly digested and contains the ingredients necessary for a well-nourished brain.

BREAKFAST FOOD NO. 6

Breakfast Food No. 6 is the ideal *brain* and *nerve* food. It is quickly digested, and the phosphate of potash combines with albumen in the system to form new brain and nerve cells.

BREAKFAST FOOD NO. 7

Did you ever examine Breakfast Food No. 7 through a magnifying glass. Upon every granule will be seen small shining crystals of sugar.

Our patented process alone brings about the formation of sugar from the starchy cereal, thus providing the elements Nature uses for our nourishment.

BREAKFAST FOOD NO. 8

Thanksgiving Breakfast would be as cheerless without Breakfast Food No. 8 as the dinner would be without the *Turkey.*

BREAKFAST FOOD NO. 9

Never before were cereals put into such form.

It is done by Professor White's process—by putting the whole kernels into sealed steel tubes. Then the tubes are revolved for sixty minutes in a heat of 540 degrees.

The heat turns the moisture in the grains to steam and creates an enormous pressure. Then the tubes are opened and the steam explodes. Instantly every starch granule is blasted into a myriad particles.

Such is Breakfast Food No. 9.

BREAKFAST FOOD NO. 10

You can improve your *health* in 30 days by increasing the amount of Breakfast Food No. 10 you eat.

Those that have done this have found that frequent and regular eating of this food resulted in clearer skins, rosier cheeks, firmer muscles, and clearer and more active minds.

BREAKFAST FOOD NO. 11

Brain-power is what *wins* to-day. Brute force can not compete with well-nourished gray matter.

Do you want to be a *success* in life? Do you want to reach the top of your profession? Then note—

Breakfast Food No. 11 has been especially designed to meet the enormous demands our civilization makes upon our mental activities. Its regular use for breakfast will go far toward that *success in life.*

BREAKFAST FOOD NO. 12

Breakfast Food No. 12 is sold from 60 degrees North Latitude—Norway—to 45 degrees South Latitude—New Zealand.

In every village and city in all that great region you will find fellow-beings eating it for breakfast.

1847. BREAKFAST FOOD NO. 13. *1910*

For many years we have been engaged in manufacturing and selling our breakfast food. Our growth in business has been slow and steady, based on the complete satisfaction of our customers.

We are now in a position to greatly increase our business and so resort to advertising. But our ideal still remains of rendering complete satisfaction to each customer and thereby keeping his trade for life.

A $2.00 SOUVENIR SILVER SPOON *Free*

Send us 10 of our trade-marks cut from the paper package of Breakfast Food No. 14 and ten cents (for packing and carriage). We will send you a beautiful silver souvenir spoon of the old Mission Dolores of California. You can get this pattern only from us.

The spoon is genuine Roger's extra plate. Bought in any store it would cost you $2.00.

We make this offer in order to get you into the habit of using our Breakfast Food. Once you commence we know you will never stop.

BREAKFAST FOOD NO. 15

Royal Patrons
King Edward VII.
Emperor Wilhelm.
King of Spain.
Tsar of Russia.
Queen of Norway.
King of Portugal.

Royalty is in a position to know and secure the best article of its kind in every department of manufacture.

Can any intending purchaser afford to overlook this most remarkable indorsement and convincing testimonial to the superior merits of Breakfast Food No. 15?

BREAKFAST FOOD NO. 16

Prepared in *clean* kitchens, by *clean* people, with *clean* equipment.

Guaranteed under the Pure Food Law of June 30, 1906.

Thousands of visitors annually witness its preparation in our *model* kitchens.

BREAKFAST FOOD NO. 17

Patronize Home Industries.

Breakfast Food No. 17 is manufactured by us in this your home city. We employ hundreds of employees who live here and help make this, your city, prosperous.

Prefer our breakfast food to similar brands made elsewhere and help us in our business and in so doing help every one else living here.

BREAKFAST FOOD NO. 18

"I used Breakfast Food No. 18 throughout my African hunting expedition; no better ever made."

Extract from *Theodore Roosevelt's* own account of his African Expedition. (October *Scribner's,* page 403.)

BREAKFAST FOOD NO. 19

While other Combinations have *increased prices* to the consumer, our remarkable purchasing power, modern labor-saving equipment, and skilled workmen have combined to *lower prices* on Breakfast Food No. 19.

No need for you to pay 12½ to 25 cents a package for breakfast foods, when we can give you equally good for 10 cents.

BREAKFAST FOOD NO. 20

The World's Finest Manufactory and the Largest Building in the world devoted exclusively to the making of Breakfast Foods is our Manufacturing Home.

This great building faces the Lake Shore Drive in Chicago, America's finest boulevard, is six stories in height, and extends the length of a block from Ohio to Ontario Streets.

This is where Breakfast Food No. 20 is made.

Table VIII. gives the results from the 50 subjects. The advertisements are arranged in the order of preference figured from the median judgments. The medians are given with their quartiles (Q)[1] and probable errors (P.E.). The P. E. is figured from the average deviation (A.D.) using the formula, P.E. = .845 A.D. The average judgment is also given and its P.E. figured from the A.D.

It is apparent that there is considerable variability in this group of subjects with respect to the relative superiority of the different advertisements. This is, of course, only to be expected as all the appeals used in this series are in use for foodstuffs and practically all for breakfast foods and consequently all should have considerable "pulling-power." This fact would seem sufficient proof for the assertion that all the twenty lie some distance above the zero point of persuasiveness. If this point could be determined in some manner we would then be able to state in actual quantities the merits of the several advertisements. But such a result will only be attained after many experiments coupled with extensive records obtained in actual business. That there are advertisements which would fall below the zero point seems certain to the writer. A tooth-paste advertisement painted on bill-boards about Peekskill during 1910 displayed a certain negro minstrel in most outlandish costume. Overhead was the statement that he always used —— —— Tooth-Paste. Tooth-paste in a tube is now associated in the writer's mind with the repulsive figure and he will probably never use any such brand because of the accompanying emotion of disgust.

With the exception of a few minor changes in the order Table VIII. is corroborated in Tables III. and IV. In one or two cases, *e. g.*, advertisements No. 11 and No. 5 of Table VIII. and the corresponding advertisements—No. 6 and No. 5—of the latter tables, we find a more decided difference in the order of merit in the preliminary experiments than here. In this particular case the pictures of Experiment I. gave a far better representation of the "Success" and "Mental Dulness" appeals than did the "write-up" of this experi-

[1] "In so far as the measurements are distributed symmetrically about the median, the P.E. calculated directly will be the same as the distance from the central tendency reached by counting off in either direction 25 per cent. of the cases." This latter measure is the quartile. As the distribution of judgments for many of these advertisements is not symmetrical the quartile is given as well as the P.E. The quartile averages .44 of a position greater than the P.E. with these data. (See Thorndike, "Mental and Social Measurements," pp. 78–79.)

TABLE VIII

GRADES AND DEVIATIONS IN TERMS OF PROBABLE ERROR AND QUARTILE OF 20 BREAKFAST FOOD ADVERTISING APPEALS

Ad. No.	Median	Q.	P.E.	Average	P.E.
16	4.5	*3.2*	*3.0*	5.6	*3.1*
2	6.5	*5.2*	*4.0*	7.5	*4.1*
11	7.9	*5.1*	*4.0*	9.5	*4.1*
3	8.0	*4.5*	*3.7*	9.2	*3.7*
1	8.2	*4.4*	*4.0*	8.7	*4.0*
6	8.2	*4.7*	*4.3*	9.1	*4.3*
10	8.5	*4.0*	*3.5*	9.3	*3.5*
5	9.3	*3.8*	*3.8*	10.3	*3.8*
7	9.5	*3.6*	*3.2*	9.9	*3.1*
13	9.8	*4.3*	*3.7*	9.8	*3.6*
18	9.8	*6.3*	*4.8*	10.3	*4.8*
19	9.8	*3.4*	*3.4*	10.4	*3.5*
9	11.0	*4.3*	*3.9*	10.3	*3.9*
12	11.5	*3.5*	*3.2*	11.0	*2.9*
8	12.5	*4.2*	*4.1*	12.2	*4.1*
4	13.0	*5.0*	*4.1*	11.4	*4.2*
17	13.1	*3.5*	*3.6*	12.4	*3.6*
15	14.0	*4.8*	*4.5*	12.9	*4.5*
20	14.9	*2.8*	*3.0*	14.6	*3.1*
14	16.8	*3.2*	*3.4*	15.9	*3.1*

ment. The wide divergence in Experiment III. is probably due to the fact that the ten subjects while sorting the advertisements in that experiment recalled to some extent the pictures used in Experiment I. which they had sorted some time before.

From the data of Table VIII. it is apparent that advertisement No. 16 is rated first. Advertisement No. 2 has about 9 chances in 1,000 of being rated as high as No. 16; the remaining advertisements have less than 1 chance of so being rated. Advertisements Nos. 11, 3, 1, and 6 are rated practically equal, having less than 50 chances in 1,000 of being rated as high as No. 2. Advertisement No. 10 is rated nearly as high as these four. The five advertisements Nos. 5, 7, 13, 18, and 19 stand half-way between No. 10 and No. 9 with, on the whole, less than 80 chances in 1,000 of being rated as high as the former or as low as the latter. The remaining advertisements have the following approximate chances in 1,000 of being rated as high as the position of the advertisement just preceding it in the scale: No. 12, 229 chances; No. 8, 126 chances; No. 4 and No. 17, 156 chances; No. 20, 78 chances; and No. 14, 4 chances. The appeals rank then in the following order: "Cleanliness" is clearly first; "Doctor's recommendation," clearly second; "An Aid to Success in Life," "Taste," and "Health" tie for third place; "Sold by reliable firm," "Recommendation of Roosevelt," and "Cheap" tie for fourth place; then follow "Process of Manufacture," "Sold Everywhere," "Patronize Home Industry," "Royalty," "Magnificent Factory," and finally "Souvenir Spoon," which is clearly last of all.

Let us now consider the advertisements in detail.

Advertisement No. 16, based on Heinze's general appeal of cleanliness, was rated first of the twenty appeals. There is no difference here between men and women; 65 per cent. of each rated it fifth or better and only 7 per cent. rated it below twelfth position. It is certainly an exceedingly strong appeal and, as will be shown later, stands first for soap advertisements. An excellent example of this appeal appeared in the September number of the *Outlook:* "Soda Crackers Safe from Contamination. Uneeda Biscuit, in their dust-tight, moisture-proof package, are protected against all those harmful elements to which bulk soda crackers are open—dust, germs, dampness, odors, handling, and even insects. Maybe you've seldom thought about the matter. Maybe you've never realized as you've tried to eat a tasteless, tough, ordinary cracker, what uncleanliness and deterioration it has gone through."

Watson[2] would identify this appeal under the more general one of hygiene. The results of this experiment all go to show that the mere appeal of "cleanliness" is much stronger than that of "health." It is not denied that they are related but rather it is claimed that the former is connected from earliest childhood with innumerable habits of thought and action whereas the later is less definitely associated with daily life and hence weaker in its effects.

The last paragraph of this advertisement was meant to merely emphasize the central theme of cleanliness but it undoubtedly aided materially in also convincing the reader of the sincerity of the copy. This is a very important factor—to the writer's mind, the most important factor in all advertisements. Such a policy, as that of Heinze, where 35,000 persons visit his plant yearly, must "produce a thorough confidence in the cleanliness and general worth of his products upon the part of the buying public."

The relation between the appeal of cleanliness and that of taste is brought out very well by Scott.[3] "The manner in which the environment fuses with an article and determines its value is well illustrated by food in a restaurant. The food may be of the very best quality and the preparation may have been faultless, yet if the service is poor—if the waiter's linen is dirty and his manner slovenly,—

[2] C. L. Watson, "The Hygienic Idea in Advertising Copy," *Printer's Ink,* July 14, 1910.

[3] W. D. Scott, "Theory of Advertising," p. 101.

the food does not taste good and is not appetizing. You may reason out that the waiter has nothing to do with the preparation of the food and that his linen has not come into contact with it, but all your reasoning will do you but little good. The idea of dirty linen and this particular food are in your mind indissolubly united, and now, instead of thinking of food in the abstract, you are compelled to think of food in this particular relationship, and the result is anything but appetizing."

Advertisements Nos. 3, 1, and 8[4] (ranked fourth, fifth, and fifteenth) may be considered together for they are the only advertisements of the twenty that appeal directly to the taste of the food. A careful survey not only of the Breakfast Food advertisements but also of all food advertisements will show that this "taste" appeal is little used. "Grape-Nuts is of the opinion that we eat in order to enlarge the lobes of the brain and thereby make more money. Heinze thinks we choose our foods chiefly to avoid benzoate. The breakfast food folks and the coffee substituters spend most of their money on considerations of health or the high cost of living. Kellogg dwells on the sincere flattery of his imitators. Armour spends a good deal of space on his spoons. Walter Baker, until very recently, has advertised his laurels rather than his excellent goods,"[5] etc.

Our data, as shown in Table IX., suggests that there are three general types of persons, those who respond to "taste" appeals, those who are indifferent to such appeals, and those who consider them with suspicion. The table shows 22 of the 50 subjects as belonging to the first group and 9 belonging to the third. The remainder of the 50 appear under the second group. The first and last group, as shown in this table, are quite constant in their attitude towards all three of the advertisements. Introspections also apparently support such a division. The first group "prefers taste and well-known authority," "eats bananas with breakfast food," considers No. 3 "an honest advertisement" and says that it "appeals most." In the third group we find these advertisements referred to as "bombast" or having "no appeal" and "slight association," etc. There is a much closer relationship between the judg-

[4] The original of advertisement No. 8 (see No. 8 in Experiment I. shown in Plate III) was of course only run in November numbers of the monthly magazines and hence might not be expected to be a particularly strong appeal except at Thanksgiving time. It was used because of the difficulty of finding other direct appeals to the taste.

[5] S. H. Page, "Writing Appetite into Copy," *Printer's Ink*, July 14, 1910. A most interesting article and one which considers many of the points brought out in this section.

TABLE IX

Judgments of "Taste" Appeals, *i. e.*, Advertisements No. 1, No. 3, and No. 8

Group ranking No. 1 high

Subjects	No. 1	No. 3	No. 8
MA	1	6	7
FA	1	8	19
MB	1	4	16
FB	1	20[6]	3
FC	1	16	3
FD	2	1	3
FE	2	1	18
FF	2	7	16
MC	2	4	5
FG	3	5	1
MD	3	11	10
FH	4	5	15
ME	4	14	12
MF	5	17	9
MG	5	10	14
MH	5	19[6]	7
FI	5	10	20
MI	5	1	14
FJ	6	7	5
FK	6	4	5
MJ	6	4	12
FL	6	4	1
Av.	3.5	8.1	5.2

Group ranking No. 1 low

Subjects	No. 1	No. 3	No. 8
MK	19	8	20
ML	19	4	20
FM	19	2	18
MM	18	19	10
FN	18	16	20
FO	18	16	20
FP	17	18	2
MN	17	16	6
FQ	16	10	9
Av.	17.9	12.1	13.9

ments of No. 1 and of either No. 3 or No. 8 than there is between those of No. 3 and No. 8. With Spearman's Method of Correlation calculated from the per cent. of unlike signs ($r = \cos \pi U$) we obtain the following correlations:

Between advertisements No. 1 and No. 3 + .588
Between advertisements No. 1 and No. 8 + .368
Between advertisements No. 3 and No. 8 + .063

The division, then, into the three groups is apparently not as clear as would seem from Table IX.

The value of the "taste" or "non-taste" appeals depends ultimately on the considerations which lead us to eat. Page[7] asks this question but dismisses it with the assumption "That our extrava-

[6] In these two cases the subjects dislike bananas and consequently were not only not attracted but were positively repelled by the advertisement. It was therefore just as much a response to taste in their cases as if they had ranked No. 3 first or second.

[7] S. H. Page, *ibid.*

gant, self-indulgent nation eats at least partly for pleasure.'' To the extent that we eat for pleasure the ''taste'' appeal must surely be the strongest. Who upon turning the pages of a monthly magazine has not wanted some griddle-cakes and Karo Syrup, or Whitman's chocolates, or some Nabiscos, or even a Shredded Wheat Biscuit, when shown with berries, sugar, and cream? The California Fruit Canner's Association have a number of ideal advertisements in the subway and street cars of this city. One of them, showing the rectangular can opened at one end and beside it a dainty dish of asparagus already to be eaten, has the following copy: ''Raised under perfect conditions.'' ''Cut and packed at its perfection. Unmistakably the finest quality you can buy.'' The whole is gotten up very tastefully and, I dare say, every one looking at it feels like eating some. Compare such appeals with this one: ''Brain and Nerves require special Food for their nourishment because they are the most highly 'specialized' organs of the body. The Food required by Nature for rebuilding Brain and Nerve Cells is found in Grape-Nuts.''[8]

James[9] contends that ''not one man in a billion, when taking his dinner, ever thinks of utility. He eats because the food *tastes* good and makes him want more. . . . The connection between the savory sensation and the act it awakens is for him absolute. . . . It is not for the sake of their utility that they [instincts] are followed, but because at the moment of following them we feel that that is the only appropriate and natural thing to do.'' It seems inconceivable that any one would not grant the above—that men and women should be stimulated to *feel* like eating food ''shot from a gun,'' or prescribed as ''healthy'' by a physician, or ''guaranteed to form new brain cells,'' unless sick at the time. But such the data and introspections would seem to indicate. There are several among these fifty subjects who consciously consider the constituents of the various foods and endeavor to have ''well-balanced menus.''

Advertisements No. 2, No. 15, and No. 18 form another group of appeals which arouse interest in the prospective purchaser on the basis of the opinion of authorities. The breakfast food in these cases was recommended by a physician, or was used by royalty, or by Roosevelt on his African hunting expedition. They were ranked second, eighteenth, and eleventh, respectively. With Spearman's method of correlation calculated from the per cent. of unlike signs we obtain a correlation of —.279 between the judgments of No. 2 and No. 15 and — .125 between No. 2 and No. 18, while between No. 15 and No. 18 there is a correlation of + .809. Tables X. and XI.

[8] Cf. *Hampton's Magazine,* November, 1910.

[9] W. James, ''Psychology,'' II., 386.

present these relationships in a different manner. It is especially noticeable in the last group in Table XI. where only two individuals, who rank No. 18 between sixteenth and twentieth positions, rank No. 15 above sixteenth place, one at thirteenth and the other at fourth. Evidently then, persons judge differently a doctor's recommendation from one of royalty or of a man, as Roosevelt, but do rank recommendations from the two latter in a similar manner. The statement of a responsible physician is of considerable value as far as Breakfast Foods are concerned in the eyes of half of our subjects. A smaller number are influenced by the statement that Roosevelt

TABLE X

Comparison of Judgments of Advertisement No. 2 with No. 15 and No. 18

	Ad. No. 2. Av.	Ad. No. 2. A.D.	Ad. No. 15. Av.	Ad. No. 15. A.D.	Ad. No. 18. Av.	Ad. No. 18. A.D.
Group of 22 ranking No. 2 between first and fifth	2.3	*1.0*	12.3	*5.6*	9.6	*5.4*
Group of 20 ranking No. 2 between sixth and fifteenth	9.6	*2.4*	12.3	*5.3*	11.2	*6.2*
Group of 8 ranking No. 2 between sixteenth and twentieth	16.8	*0.9*	16.4	*3.1*	10.6	*5.9*

TABLE XI

Comparison of Judgments of Advertisement No. 18 with No. 15

	Ad. No. 15. Av.	Ad. No. 15. A.D.	Ad. No. 18. Av.	Ad. No. 18. A.D.
Group of 15 ranking No. 18 between first and fifth	8.9	*5.3*	2.4	*0.9*
Group of 18 ranking No. 18 between sixth and fifteenth	12.0	*4.2*	10.1	*2.4*
Group of 17 ranking No. 18 between sixteenth and twentieth	17.4	*2.5*	18.0	*0.9*

TABLE XII

Sex-Differences in the Average or Median Judgments of Ads. No. 2, No. 15, and No. 18

Ad.	Men. Av.	Men. A.D.	Men. Med.	Men. A.D.	Women. Av.	Women. A.D.	Women. Med.	Women. A.D.
2	6.2	*4.0*	5.0	*4.0*	8.5	*5.4*	7.0	*5.3*
15	10.3	*6.1*	11.0	*6.1*	14.8	*4.8*	17.7	*4.0*
18	7.8	*5.6*	5.6	*5.6*	11.7	*5.1*	14.0	*4.6*

TABLE XIII

Sex-Differences in the Judgments of Advertisements No. 2, No. 15, and No. 18

Ad.	Sex.	Per Cent. of Judgments according to Position. 1–5 Per cent.	6–15 Per cent.	16–20 Per cent.
2	Male	52	43	5
	Female	38	38	24
15	Male	38	33	29
	Female	7	31	62
18	Male	47	31	22
	Female	19	42	39

uses the food and a still smaller number by the Royalty appeal. On the other hand 8 consider No. 2 of little value as a reason to buy Breakfast Food, 17 so consider No. 18, and 24, No. 15.

This difference of response is due to a very considerable degree to the factor of reliability in the testimonial. "There can be little doubt that the heyday of the trustworthy testimonial has passed. There have been those who have taken advantage of the testimonial. But it is a fact that there are still hundreds of people of brains who are willing to write testimonials which are sincere."[10] The Sanatogen Campaign started a short time ago is an illustration of very extensive use of this appeal. They recognize the popular feeling that testimonials are generally bought and consequently take great care to use only those from men and women who would never be thought of as selling their name.

These three "authority" advertisements appeal more to men than to women. Table XII. gives both the average rank and the median rank for the men and the women for these three advertisements. (The distribution of judgments in No. 15 and No. 18 is markedly bi-modal and hence it is extremely difficult to present any one term as an indication of the tendency of the men as distinguished from the women.) An analysis of the data, as given in Table XIII., shows a greater per cent. of men than women ranking each of the three advertisements high and a correspondingly lower per cent. of men than women who rank them low. The men were somewhat older than the women and this tendency to accept some one else's word for the superiority of an article when in doubt themselves may be due to that fact or it may be a characteristic of men as compared with women.

Advertisements Nos. 5, 6, 7, 9, 10, and 11 are general appeals to "health." They are all ranked between 7.9 and 11.0. Advertisements Nos. 11, 6, and 10 are judged superior to the other three. They make a *direct* appeal to health by giving the constituents of the food, or by describing the results of eating it in terms of "clearer skins, rosier cheeks," etc., or in terms of the "success" that accompanies "well-nourished gray matter." The other three make an *indirect* appeal by discussing the process of making the food or by stating that "mental dulness usually comes from imperfectly nourished brains," thus implying the need of "our" Breakfast Food.

This difference in "pulling-power" between *direct* and *indirect* statements of the same appeal is shown in the preliminary experiments with Breakfast Foods advertisements (see Experiments I., II.,

[10] "The Sanatogen Campaign and the Use of Testimonials," *Printer's Ink*, June 23, 1910.

and III.). The numbers of the six advertisements used here, except No. 11, correspond to the numbers in those experiments. Both No. 6 and No. 11 were based on No. 6 of the preliminary experiments as the latter combined two distinct appeals. If then we compare the average position of Nos. 6, 10, and 11 in the four experiments with the average position of Nos. 7, 5, and 9 we have the following:

	Exp. I.	Exp. II.	Exp. III.	Exp. V.
Av. Position of No. 6, 10, 11	5.5	5.0	4.8	9.3
Av. Position of No. 7, 5, 9	6.0	7.2	5.8	10.2

As in each of the four experiments the remaining appeals were different it is unfair to compare the amount of the averages of one experiment with that of the others. What the data prove here is that *direct* appeals are ranked higher than *indirect* ones.

Advertisement No. 5 is not only *indirect* according to the above classification, it is also *negative* in its appeal, *i. e.,* it states what happens when you don't use its breakfast food. A comparison of its position with that of No. 11 in this experiment or No. 6 of the three preliminary experiments, where the *positive* side of this appeal is used, gives the following:

	Exp. I.	Exp. II.	Exp. III.	Exp. V.
Position of Ad. No. 11 or No. 6	5.5	5.2	4.2	9.5
Position of Ad. No. 5	7.2	7.2	6.1	10.3

This superiority of the "affirmative" appeal to the "negative" appeal is well-nigh universally accepted as a doctrine, yet it is violated constantly by advertisers. Nye[11] reviews the question very ably in a recent issue of *Printer's Ink.* Among other things he says: "It takes a judicial mind to weigh the evidence pro and con and hand down a decision favorable to the article or goods advertised. Unfortunately, the average consumer, even the average space buyer, has not such a mind." Hence it would "behoove us to stick to the arguments strictly for it, and to present them in such a way that the reader can not possibly carry away with him an unfavorable impression fused with the other."

However, it seems to the writer, it is not so much a question of "affirmative" and "negative" appeals as appeals which *do* or *do not* fit the reader's general frame of mind. The point is that we are all capable of exercising our judgment when necessary, but ordinarily we act without thought of the reasons for our action. If afterwards a reason is required we are able to give one or more. It is a question, however, whether the action was due to these "given reasons" or to

[11] F. W. Nye, "The Affirmative Argument *vs.* the Negative in Advertising Copy," *Printer's Ink,* September 15, 1910.

a more complex fusion of feelings and ideas, which we are unable to analyze by introspection. The reasons that were given in the introspections as to the arrangements of the various appeals show this point strikingly. A study of them would convince anyone that they were practically worthless as far as explaining the individual's judgments. Yet the judgments, themselves, are valid enough. Our actions are much more the outcome of unanalyzable feelings and ideas than of definite ones. To secure *deliberation* both sides of the question should be given. To secure *action* only those affirmative statements should be presented as will fuse with the general state of mind of the buyer and will produce a pleasurable attitude toward the article. Cuticura Soap, for example, is introduced to the better classes of society by a pleasant picture with headings such as "Best for Baby's Skin," etc., while to the poorer classes of society, the advertisement is headed "Wonderful Cure of Sore Hands," etc. In both cases the situation described fuses with the general state of mind of the reader and the suggestion of Cuticura Soap is pleasantly aroused in his mind because it suggests an outlet to a desire—in the one case, a clean healthy baby and in the other case, healed hands.

Docknell[12] in pointing out how action was "forced from the greatest number of human beings" through the writings of Confucius, Buddha, Mahomet, and the Bible, laid down five principals, which are well-worth repeating in this connection. Taking the Bible as the standard of the four, we see that:

"1. It offered people something they wanted, or which, after its perusal, they realized they needed.

"2. It appealed primarily to the interest of its readers, that is to say, it talked more about its reader's wants and needs than of the remedy it offered.

"3. It always used simple language.

"4. It constantly repeated its message and said the same thing over and over again in the same and different ways. It did not consider that once is often enough to tell a message and expect it to be remembered.

"5. It is always dominant. It is always superior. It always affirms. It never appeals to its readers for confirmation of its standards. Every line breathes dominance, superiority, and confidence in its power to dictate to its readers as to what their action must be in order to acquire what it suggests."

A frequent appeal common to nearly every line of merchandise is represented by advertisement No. 12. "See, Everyone buys this

[12] T. E. Docknell, "The Law of Mental Domination," *Advertising and Selling,* September, 1910.

Breakfast Food. What's the matter? Why don't you?," is the inference from it. "Fifteen Million Human Hands wind Ingersoll Watches every night," or "Fifteen Million Dishes every Month of Puffed Rice," or "Ice worth over $100,000 at the lowest wholesale prices used in one month to keep Borden's milk ice-cold" are examples of which hundreds of others might be found. It is scarcely ever used alone, however, as in this experiment. This appeal is rated very much higher than another common one, No. 20, which emphasizes the size or output of the factory and suggests, though more indirectly, the same line of thought. Both of them must be considered weak in their suggestion to prospective purchasers. We, of course, buy some things because every one else has them, many other things we buy, on the other hand, because no one else has them. On the whole, we buy because we want to and the successful advertisement must make us desire the article. "The advertisement used in recent magazines showing a case of Toasted Corn Flakes loaded on a pack mule along with an Indian in front of an adobe hut is a distinct but unsavory attempt to broaden the appeal for that well-known article of diet. It does not really add to our pleasure in a dish of corn flakes to know that they are also eaten by aborigines of the Great American Desert, nor should we expect it to increase the sales."[13]

Advertisement No. 13 was meant to convey the idea of an old reliable house which had built up its business by conservative methods. The author has never been satisfied with its wording and its position at eighth place can not be taken to indicate that a properly-worded appeal along this line would not be ranked much higher. (This point is considered in more detail on page 53.)

The Souvenir Silver Spoon appeal was ranked last by 17 of the 50 subjects and averaged last of all the twenty. Yet we find five subjects ranking it second, fifth, sixth, seventh, and eighth, respectively, and four others ranking it eleventh. One college girl, who ranked it last, said that she believed many college girls did buy for the souvenirs and her laboratory partner was one who rated it high. Evidently, however, judging from our data, college students have little faith in an article so advertised. Yet many articles for use by such a group are advertised in this manner, *e. g.,* Armour's Beef Extract, H–O Breakfast Food, etc. The wide use of this appeal coupled with the use of "trading stamps" indicate that the souvenir-appeal is a strong one for certain classes.

Advertisement No. 17 may be a strong appeal in some places or

[13] W. W. Hudson, "Advertising Appeal," *Judicious Advertising,* October, 1910.

attached to other articles, but one would scarcely expect it to be so in New York City and applied to breakfast foods. It is very interesting to note that the women rate it considerably higher than the men (11.1 as compared with 14.6) and that of the nine who ranked it above eighth place seven were women.

We must realize that all these appeals produce results, a blank page with only the name of a breakfast food upon it will also produce results. The question in this discussion is not which are good and bad, but which produce the greatest returns for the least effort. Undoubtedly the advertisement that generates the maximum of pleasure, providing that pleasure is strictly relevant to the article itself, has the strongest appeal. A good example of irrelevantly aroused pleasure is the recent Cream of Wheat advertisement showing a group of men playing horse-shoe in the middle of the village street. In the background is the typical Cream of Wheat bill-board advertisement. The pleasure aroused in the advertisement is all centered in the characters gathered about the game, not in the breakfast food. It might very properly be asked, How many women ever played this game or are interested in the old bums of the town? Yet we find a whole-page colored advertisement in the *Delineator* of October, 1910! How many packages will such an advertisement sell? Such an advertisement must approximate pretty closely to the zero point of efficiency.

There are at least two primary considerations for breakfast foods —they must be fit to eat, clean, and not unhealthy and they must taste good. Knowing nothing of the food the first consideration is most important but once that is granted the question of taste ranks above every other consideration. Jane Addams[14] sums up the whole point when she states that "perhaps the neighborhood estimate (of their New England kitchen) was summed up by the woman who frankly confessed that the food was certainly nutritious, but that she didn't like to eat what was nutritious, that she liked to eat 'what she'd ruther.' "[15]

[14] J. Addams, "Autobiographical Notes," *American Magazine*, June, 1910.

[15] Compare also W. D. Scott, "Theory of Advertising," pp. 101, 216–218; "Psychology of Advertising," Chapter on The Psychology of Food Advertising; and T. A. DeWeese, "The Principles of Practical Publicity," pp. 33–34 and Chapter XVII.

CHAPTER VI

Consideration of Soap Advertising Appeals

This chapter deals with two problems: first, what is the order of superiority of soap advertising appeals for college students, and second, what is the relation between such an order for college students and similar orders of preference for other groups of individuals. The first problem is identical with that discussed in the previous chapter, except here the commodity is soap instead of breakfast food. The second problem has not been discussed so far in this report. It had been hoped that it could be considered in some detail, taking into account a large number of different groups of individuals. But the extreme difficulty of securing a large number of individuals of any one group coupled with the fact that many uneducated persons were unable to follow the directions, has made it impossible to carry out the work as originally planned. However, a sufficient amount of data is given to indicate the general relationship between judgments obtained from college students and those obtained from other groups of society.

Section 1. *Order of Preference of Soap Advertising Appeals for College Students*

This experiment is similar in nature to the one of Chapter V. The same number of "made-up" advertisements are used; in many cases the appeals are identical in nature. The "directions" used in the Breakfast Food Experiment are made use of here too. Thirty-seven of the fifty subjects are the same. (The fifty subjects used here consisted of 27 women and 23 men.) It is consequently valid to compare the strength of the appeals in the one group with that of the other. Following are the twenty "made-up" advertisements which were used in this experiment.

1. Beauty

A good natural complexion and a fair, soft skin are necessary essentials of *beauty.*

Toilet Soap No. 1 is the most perfect beautifying agent known, possessing those special and unique qualities which render the skin pure, clear, and of exquisite softness.

More than all the cosmetics in the world, it is the special beautifier of the complexion.

2. In an article published in the *Ladies' Home Journal* of April, 1909, Dr. G. B. Spencer, the noted physician and skin specialist of London, said:

"Toilet Soap No. 2 seems to me of marked value as a soap. I most heartily recommend its use for all."

3. *That Baby of Yours*

will have a tender, burning, irritated skin unless you exercise care in choosing the soap to be used for his daily bath.

Toilet Soap No. 3 is Baby's Friend.

Its use can be recommended to all who would be careful of the baby's welfare.

4. *With Toilet Soap No. 4 and Lukewarm Water*

A few months ago, one of our managers visited the Carnegie Institute in Pittsburg.

Accompanied by the custodian, he entered one of the assembly halls, the walls and ceiling of which were covered with mural paintings.

These paintings were as clean and bright as if the painter had just finished them.

Mr. X. looked at them for several minutes, admired them greatly, and then asked: "How do you keep those murals so clean?" The reply was, "With Toilet Soap No. 4 and lukewarm water."

"But," you say, "I have no mural paintings that need to be cleaned. What I want to know is, how to clean laces, shirt-waists, blankets, curtains, and the like."

We answer, as did the custodian of the institute, "With Toilet Soap No. 4 and lukewarm water."

5. *Try it at our Expense*

We feel that words can not adequately represent the superiority of Toilet Soap No. 5.

Consequently, we make you this offer.

Send us a two-cent stamp, to cover postage, and we will send you a half-size trial cake of our toilet soap.

Once you have used it, you will never be without it.

6. *Does not Irritate the Skin*

Soaps containing strong alkali, coloring matter, and adulterants, will dry and irritate the skin and destroy its softness.

Toilet Soap No. 6 has nothing to hide—no dyes to deceive, no high perfumes to delude the sense of smell.

Its use leaves the hand with none of those "drawn feelings," but soft, and moist, and clean, as after the use of a cold cream.

7. *The Soap for Particular People*

Toilet Soap No. 7 is designed to meet the precise desires of those who appreciate good form and taste.

It comes in several different tints of cream, and pink, and green, so as to match the color scheme of your room.

It is most delightfully scented, thus giving an added pleasure to its use.

Its presence in your home is only another way in which your character may be interpreted and your individuality reflected.

8. *Dear But Worth It*

Our Toilet Soap No. 8 costs almost twice as much as ordinary toilet soap.

We spare no expense in its manufacture.

The best is never cheap.

9. *Guaranteed*

All merchants handling our Toilet Soap will refund you its cost if after careful trial you are not pleased. Simply return the remainder of the box and get your money back.

Can we do more to interest you in Toilet Soap No. 9?

10. *Much Depends on Your Health*

Doctors tell us that our health depends in large degree upon the condition of our skin. The pores must be kept open for proper perspiration. In this way do we get rid of much of the poisonous waste products of the body.

Toilet Soap No. 10 is guaranteed to dissolve the fatty substances that clog our pores and thus insure to us healthy skins and healthy bodies.

11. Toilet Soap No. 11 is of special value for shampoos and the bath.

It lathers freely and quickly. No rubbing for two or three minutes to get a little lather which disappears as soon as more water is added.

With its use the morning shower can be made a true bath and in the same length of time.

12. *Sold Everywhere*

Wherever you go on this great globe you will find Toilet Soap No. 12 for sale.

If you ascend the Yang-tse-Kiang to Chang-Sha in China or visit Wellington in New Zealand or enter a drug store on Broadway in New York City you will find our soap awaiting your purchase.

13. *1836. 74 years of Service 1910.*

For many years we have been engaged in manufacturing and selling our soap. Our growth in business has been slow and steady, based on the complete satisfaction of our customers. We have been satisfied with this in the past.

But we are now in a position to greatly increase our business and so resort to advertising. However, our ideal still remains of rendering complete satisfaction to each customer and thereby keep his trade for life.

14. *Souvenir Tea Spoon Free*

There is a coupon with each cake of our Toilet Soap No. 14. These coupons are valuable.

We will mail a silver souvenir teaspoon to every person who sends us before December 31, 1910, thirty of these coupons and ten cents (for packing and carriage).

These souvenir spoons are genuine Roger's extra AA plate and are the same as sell for $2.00 in any store.

15. *Royalty* is in a position to know and secure the best article of its kind in every department of manufacture.

Such members of royalty as the Tsar of Russia, the Queen of Norway, the King of Portugal, the King of Spain, along with King Edward VII. of England, and Emperor Wilhelm of Germany are constant users of our soap.

Can any intending purchaser afford to overlook this most remarkable indorsement and convincing testimonial to the superior merits of Toilet Soap No. 15.

16. *99 Per cent. Pure*

Toilet Soap No. 16 is just a white, *pure,* oval cake of soap, made from edible products.

It contains no ''free'' (uncombined) alkali.

It is *pure* and *clean.*

17. *Exhilaration in the Bath*

They who can take an ice-cold Bath successfully know the fine, cheering, after-glow which follows it.

But that splendid influence on the skin is impossible to many people whose heart-action will not permit it.

There is, however, a safe substitute for the coldness in the water. That substitute is X, the chief ingredient of Toilet Soap No. 17.

X possesses some wonderful characteristics.

Its first action is Anodyne—bringing ''Therapeutic Rest'' to the skin.

Its next action is control of Hyperæmia—or control of blood in the small vessels of the skin.

Its third action is Antiseptic, destroying all Bacteria in contact with the skin.

Its fourth action is that of a powerful Healer and Nutrient, replacing affected tissue with sound Flesh and Fiber, while feeding the skin through its pores.

Can you conceive a finer Toilet Soap?

18. ''I used Toilet Soap No. 18 throughout my African Hunting expedition; no better ever made.''

Extract from *Theodore Roosevelt's* own account of his African Expedition. (October *Scribner's,* page 403.)

19. While other Combinations have *increased prices* to the consumer, our remarkable purchasing power, modern labor-saving equipment, and skilled workmen have combined to *lower prices* on Toilet Soap No. 19.

There is no need for you to pay 5 to 10 cents a cake for soap, when we are able to give you two cakes for 5 cents.

20. The *World's Finest Manufactory* and the *Largest Building* in the world devoted exclusively to the making of Toilet Soap is our Manufacturing Home.

This great building faces the Lake Shore Drive in Chicago, America's finest boulevard, is six stories in height, and extends the length of a block from Ohio to Ontario Streets.

This is where Toilet Soap No. 20 is made.

The first section of Table XIV. gives the results from the 50 subjects. From these figures it is evident that the 20 advertisements can be divided into eight groups in which the probable errors of the medians in one group do not overlap with those of another, *i. e.,* that the chances are more than even that the positions of the advertisements in one group would not be interchanged with those of

TABLE XIV

GRADES AND QUARTILES (Q) OF SOAP ADVERTISING APPEALS FOR DIFFERENT GROUPS OF INDIVIDUALS

	50 Students.		25 Business Men.		25 Teachers.		20 Grad. Students.		21 Students.		10 Students.		101 Men Summary.	
Ad. No.	Ave.	Q.	Ave.	Q.	Ave.	Q.	Ave.	Q.	Ave.	Q	Ave.	Q.	Ave.	Q.
16	3.8	*2.5*	4.8	*2.2*	4.0	*2.6*	4.0	*2.4*	3.0	*2.8*	2.5	*1.9*	3.9	*2.5*
6	5.0	*2.5*	4.3	*3.0*	7.1	*2.3*	7.0	*2.6*	5.0	*2.8*	6.0	*2.0*	6.0	*2.9*
10	6.3	*3.6*	6.0	*3.3*	8.0	*4.5*	7.5	*5.0*	5.8	*2.7*	6.0	*3.1*	6.5	*4.1*
8	8.3	*4.0*	13.7	*4.1*	7.1	*3.8*	7.0	*4.8*	10.6	*4.1*	4.3	*4.2*	8.6	*5.0*
11	8.5	*3.6*	5.0	*3.5*	10.3	*3.7*	7.0	*4.0*	6.1	*4.1*	5.2	*3.2*	6.4	*3.9*
4	8.5	*4.1*	13.7	*3.6*	9.3	*5.3*	10.5	*5.7*	10.0	*3.9*	12.5	*2.2*	12.1	*5.0*
2	9.0	*4.3*	12.0	*4.1*	7.3	*4.1*	7.5	*3.8*	10.3	*3.6*	9.8	*2.9*	9.5	*4.0*
9	9.0	*4.5*	9.7	*4.1*	9.3	*5.4*	9.0	*5.4*	9.0	*5.0*	13.0	*3.8*	9.6	*5.4*
3	9.5	*4.9*	8.8	*2.7*	11.0	*3.6*	12.5	*4.0*	9.0	*3.3*	9.5	*3.5*	9.6	*3.7*
17	10.5	*5.3*	6.3	*5.6*	9.9	*4.1*	10.0	*5.7*	9.0	*4.3*	7.5	*5.4*	8.6	*5.0*
5	10.7	*4.1*	6.2	*3.9*	10.3	*3.7*	10.5	*6.5*	11.7	*4.8*	12.5	*4.8*	10.1	*5.5*
1	10.8	*3.6*	9.0	*3.6*	11.3	*3.6*	13.0	*2.3*	9.3	*3.6*	11.0	*3.0*	10.7	*3.7*
13	10.8	*3.5*	8.3	*4.0*	10.0	*4.6*	11.5	*3.5*	9.0	*3.8*	7.0	*3.8*	9.4	*4.3*
19	12.7	*3.3*	15.6	*2.3*	15.8	*3.5*	11.5	*2.4*	14.8	*3.3*	15.8	*1.7*	14.8	*3.2*
12	13.0	*3.6*	13.7	*2.0*	11.3	*3.7*	13.5	*3.0*	15.0	*3.0*	12.5	*4.0*	13.1	*3.2*
18	13.5	*5.8*	13.7	*4.0*	8.0	*4.9*	9.5	*6.0*	12.8	*5.8*	13.0	*2.9*	12.1	*5.4*
15	13.5	*5.5*	13.0	*3.6*	10.8	*6.0*	8.5	*5.0*	10.3	*4.4*	9.5	*3.2*	11.1	*4.7*
7	15.8	*4.3*	11.0	*4.8*	13.0	*2.9*	11.5	*4.5*	13.8	*4.9*	16.5	*4.4*	12.3	*4.6*
20	16.1	*2.6*	16.7	*2.7*	17.3	*2.3*	16.2	*2.5*	15.8	*3.1*	16.8	*1.8*	16.5	*2.5*
14	18.3	*2.2*	17.8	*3.1*	19.3	*1.8*	17.8	*3.0*	18.3	*1.6*	19.9	*0.3*	18.8	*1.8*

another group, if a greater number of subjects were employed. Advertisement No. 16 is thus clearly first; No. 6 second; No. 10 third; Nos. 8, 11, 4, 2, 9, and 3 fourth; Nos. 17, 5, 1, and 13 fifth; Nos. 19, 12, 18, and 15 sixth; Nos. 7 and 20 seventh; and No. 14 eighth. (The P.E. of No. 3 does slightly overlap with No. 17 and to that extent the fourth and fifth groups are not distinct.) The value of appeals for soap are then as follows: "Cleanliness" first; "Does not irritate the Skin" second; "Health" third; "Expensive," "Shampoo and Bath," "General use," "Doctor's recommendation," "Guaranteed," and "Baby" fourth; "Bath," "Try it at our expense," "Beauty," and "Reliable Firm" fifth; "Cheap," "Sold Everywhere," "Roosevelt recommendation," and "Royalty" sixth; "For particular people" and "Large factory," seventh, and "Souvenir Spoon" last.

A comparison of these soap appeals with the twenty breakfast food appeals of Chapter V. reveals the fact that ten of them are identical in thought and approximately so in phrasing. These ten are Nos. 2, 10, 12, 13, 14, 15, 16, 18, 19, and 20. Seven are given the same relative position among the twenty in both sets, while three, Nos. 2, 10, and 18, are ranked differently. Advertisement No. 2, the "doctor's recommendation," is rated second for breakfast food and

TABLE XIV. (*Continued*)

52 Students. Ave.	Q.	43 Women. Ave.	Q.	95 Women Summary. Ave.	Q.	59 Farmers. Ave.	Q.	14 Business Men. Ave.	Q.	6 Doctors. Ave.	Q.	22 Misc. Men. Ave.	Q.	97 Men Summary. Ave.	Q.	Ad No.
2.2	*1.5*	3.4	*5.0*	**2.7**	*2.4*	11.6	*3.9*	13.0	*3.3*	17.5	*4.0*	10.0	*4.6*	**11.7**	*4.5*	**16**
3.8	*2.3*	4.4	*2.0*	**4.1**	*2.0*	9.3	*4.6*	15.0	*5.9*	9.5	*8.0*	11.2	*6.1*	**10.4**	*5.7*	**6**
5.1	*2.0*	5.4	*3.1*	**5.3**	*2.7*	10.7	*5.5*	13.5	*3.5*	12.5	*4.0*	8.0	*5.5*	**10.7**	*5.1*	**10**
7.2	*4.7*	4.8	*4.2*	**6.4**	*4.4*	10.9	*4.0*	11.0	*4.5*	7.5	*2.1*	11.7	*3.2*	**11.3**	*4.6*	**8**
9.3	*2.9*	6.9	*3.2*	**8.3**	*3.3*	11.7	*4.4*	10.8	*3.7*	11.0	*5.5*	14.8	*5.5*	**11.4**	*4.8*	**11**
11.0	*4.2*	12.3	*5.4*	**11.6**	*4.3*	11.0	*5.3*	7.5	*2.5*	5.0	*2.9*	13.5	*4.7*	**10.2**	*5.3*	**4**
9.5	*3.5*	9.9	*2.9*	**9.7**	*3.0*	10.6	*3.2*	13.5	*4.4*	13.5	*1.6*	8.0	*4.7*	**10.7**	*3.8*	**2**
9.2	*4.5*	9.6	*3.8*	**9.4**	*3.9*	12.7	*4.4*	9.0	*5.8*	13.5	*2.5*	9.2	*4.8*	**11.6**	*5.1*	**9**
9.3	*3.5*	11.0	*3.5*	**9.8**	*3.8*	10.2	*5.5*	9.0	*4.2*	6.0	*3.2*	10.5	*4.0*	**9.7**	*5.1*	**3**
8.2	*4.1*	8.8	*5.4*	**8.4**	*4.5*	13.4	*4.9*	6.0	*5.3*	14.8	*0.7*	9.2	*4.1*	**11.4**	*5.1*	**17**
9.8	*4.9*	10.3	*4.8*	**10.1**	*4.7*	10.8	*5.2*	15.0	*7.3*	5.5	*2.5*	11.8	*3.3*	**11.6**	*5.7*	**5**
8.6	*4.2*	6.9	*4.5*	**8.1**	*4.7*	10.6	*4.2*	11.5	*7.9*	15.0	*1.9*	10.0	*5.9*	**10.8**	*4.9*	**1**
9.8	*4.2*	10.4	*3.8*	**10.1**	*4.0*	10.9	*5.3*	11.0	*5.6*	5.0	*2.0*	9.5	*3.4*	**9.4**	*4.9*	**13**
14.7	*3.7*	16.0	*4.4*	**15.1**	*4.0*	7.0	*5.8*	11.0	*5.0*	5.0	*0.9*	8.0	*4.9*	**7.1**	*5.4*	**19**
12.3	*2.7*	13.3	*4.0*	**12.6**	*3.1*	9.9	*5.4*	8.2	*2.9*	7.5	*5.5*	8.5	*3.9*	**8.7**	*4.9*	**12**
14.3	*3.4*	12.9	*3.4*	**13.7**	*3.4*	12.8	*5.4*	11.0	*5.0*	14.5	*3.2*	13.5	*4.1*	**13.1**	*5.2*	**18**
14.3	*2.2*	15.1	*3.2*	**14.5**	*2.6*	9.3	*4.8*	12.0	*4.3*	12.5	*3.8*	13.5	*5.3*	**10.6**	*5.6*	**15**
17.5	*3.1*	12.8	*4.4*	**15.4**	*3.7*	9.1	*4.2*	8.5	*2.9*	18.5	*3.9*	12.0	*3.9*	**9.6**	*4.6*	**7**
16.8	*2.4*	15.0	*3.0*	**16.6**	*2.6*	11.7	*4.7*	9.5	*5.1*	7.0	*2.5*	10.0	*5.7*	**9.7**	*5.2*	**20**
19.3	*1.3*	19.0	*2.7*	**19.1**	*1.8*	7.9	*4.8*	12.1	*3.2*	5.0	*1.0*	8.5	*3.8*	**8.2**	*4.7*	**14**

seventh for soap; No. 10, a "health" appeal, is rated seventh for breakfast food and third for soap, and No. 18, the "Roosevelt" appeal, is rated eleventh for breakfast food and sixteenth for soap. It seems natural that No. 2 and No. 18 should be stronger breakfast food appeals than soap appeals, but, on the other hand, it does not seem fitting that a "health" appeal should be weaker. The discrepancy may, perhaps, be explained as due to the different phrasing of these two health appeals, although the main theme is the same.

Let us now consider the advertisements in detail.

Advertisement No. 16 is based on the "purity" appeal so commonly used by both Ivory and Fairy Soap. Whether it is rated first in the minds of these subjects because of the prevalence of its use and hence the unconscious recognition of it as *the* standard appeal for soap advertisements or whether it is naturally the strongest appeal for such a commodity and has been discovered to be so by soap advertisers is an interesting question in the history of advertising. Suffice it to say, it is now the strongest appeal for this class of subjects.

Advertisement No. 6 emphasizes the effect of the use of the soap on the skin. "It does not irritate the skin" but "leaves the hands soft and clean and moist." In other words, it claims to give us those pleasurable feelings we desire when using soap. Such an appeal is comparable to that of "taste" for breakfast food advertisements. Advertisement No. 11 presents the same general appeal but limits it to the bath and shampoo. Either it is not written so well or the special appeal of the bath is not so strong as the general one of toilet. Advertisement No. 17 also appeals to the pleasure of the use of soap—but here restricted to that of the cold-bath. Its exaggerated style and ridiculous claims cause it to be ranked much lower. The distribution of the judgments for this appeal is bi-modal—one group of 21 rank it between first and eighth with the mode at one and the other group of 29 rank it between eighth and twentieth with the mode at fourteen. Although its final position is low, yet, because of the large number that rank it very high, we must conclude that it is a very successful appeal. It will not appeal to every one, but it does appeal strongly to some.

This advertisement illustrates very beautifully the strength of "affirmative" appeals. I doubt whether any one of the fifty subjects, if asked in an examination, would agree to the statement that any soap could "replace affected tissue with sound flesh and fiber, while feeding the skin through its pores." The majority while reading over the advertisements did not notice this point at all, but afterwards, when their attention was called to it, they felt as though they had been "stung" and wished to put the advertisement lower in the scale. Anything that would have caused deliberation would have ruined this appeal.

Advertisement No. 10 not only appeals to "health" but states that the soap does just what we want it to do—it cleanses the skin. Like Nos. 16, 6, and 11 it describes what the soap is like and mentions specific qualities. Such appeals are undoubtedly the strongest.

Advertisement No. 8 is an unusual appeal for soap but one which has considerable promise as a very effective advertisement. There is a large class of persons who enjoy buying the best of any particular article. The feeling of satisfaction in knowing that one has something that the majority haven't makes the extra cost insignificant. It is evident from Chapter VI. and the data here that if the "cost" is the main appeal to be presented, an expensive soap will appeal more than a cheap one for this class of buyers.

Advertisements Nos. 2, 15, and 18 may again be considered together. With breakfast food advertisements we found no correlation between the judgments of No. 2 with either No. 15 or No. 18 but a high correlation between No. 15 and No. 18. With that commodity No. 2 was ranked second and No. 15 and No. 18 eighteenth and eleventh, respectively. Here they are ranked seventh, sixteenth, and seventeenth. And here also we find a correlation of + .13 between No. 2 and No. 15, + .25 between No. 2 and No. 18, and + .64 between No. 15 and No. 18. The peculiarities, then, of these three appeals as shown in the preceding chapter are present here but not so prominently. There is, however, very close agreement among the twelve that ranked No. 2 fifteenth or lower in also ranking No. 15 or No. 18 low. The average positions for these three advertisements by this subgroup are: No. 2, 16.6; No. 15, 13.8, and No. 18, 15.3.

As pointed out in Chapter V., the writer has felt that No. 13 does not properly convey the idea of an old established firm. In order to determine whether a better worded appeal might have greater weight, 21 subjects, all of the fifty of last year who could be found, were asked to again sort the twenty advertisements, but this time using the following appeal instead of the old No. 13.

1836 Seventy Four Years of Soap-Making 1910

For seventy-four years we have been supplying you with *good* soaps. True, the soaps we sold your grandparents—and even your great-grandparents—were *not* as good soaps as we are selling *you* TODAY.

In those days we had neither our present superb facilities nor our wonderful experience. We have been growing—improving all the time. Our ideal has been to make a soap that will give you complete satisfaction in every way.

For seventy-four years we have held this ideal—*and have lived up to it.* TO-DAY—*13MS* Toilet Soap stands as a monument to our sincerity of purpose and to our ability to attain our aims.

The 13MS Toilet Soap Co.

This appeal was written for this purpose by Mr. S. D. Hofheimer, of Frank Seaman, Inc., Advertising Agents. The comparative results for the two "wordings" are as follows:

	Position	Upper Quartile	Lower Quartile
With old No. 13 appeal........	9.3	4.6	4.1
With new No. 13 appeal........	9.0	4.3	3.5

Evidently then the new appeal has a trifle greater value. Those who judge both appeals as poor do not rank the new appeal quite so low as they do the old appeal. It is possible that still other wordings of this appeal might be ranked higher, but, on the whole, it would appear that its present position approximates its true value.

Summary.—The results here confirm those of previous chapters. The strongest appeals are those which are strictly relevant and describe the article itself or some one of its specific advantages. Then come more general appeals which do not describe the article or its use but endeavor to create a desire for it by associating it with strong instincts or habits of life.

SECTION 2. *The Relation of the Order of Preference of Soap Appeals for College Students with Similar Orders from Other Groups of Individuals.*

The data used below were all secured between the months of March, 1910, and February, 1911. The same twenty appeals were used as given above except for a few minor changes which are here listed. Instead of the "Directions" used above the following was substituted as being simpler and more easily understood.

DIRECTIONS

Read through the 20 advertisements.

Sort them in the order in which *you* would *buy* the toilet soap.

Place (1) the advertisement, which describes the soap you would buy on top; (2) your second choice (the one you would take, if you couldn't get the first) under the first, etc.; until you have the 20 arranged with the soap which you would least care to spend your money for at the bottom.

(It is suggested, that sorting first the advertisements into 3, 4, or 5 piles and then sorting each pile by itself may be an easy method.)

Each of the "made-up" advertisements was signed by the name of the Soap Co., *e. g.*, "The 1AG Toilet Soap Co.," etc. The first sentence of advertisement No. 1 was changed to read—"A good complexion and a fair, soft skin are necessary essentials of beauty." The second sentence of No. 7 was changed to read—"It comes in several different tints of cream and pink and green." The last sentence of No. 9 was omitted. The last sentence of No. 11 was changed to read—"Its use will make your morning shower a true bath and in less than the usual length of time." The last sentence of No. 15 was changed to read—"Do not overlook this most remarkable indorsement and convincing testimonial to the superior merits of Toilet Soap 15VO." The last sentence of No. 17 was changed to read—"There is no finer Toilet Soap." The last sentence of No. 20 was omitted. Following is a sample of these advertisements just as they were used:

Does Not Irritate the Skin

Soaps containing strong alkali, coloring matter, and adulterants, will dry and irritate the skin and destroy its softness.

Toilet Soap 6FL has nothing to hide—no dyes to deceive, no high perfume to delude the sense of smell.

Its use leaves the hand with none of those "drawn feelings," but soft, and clean, and moist, as after the use of a cold cream.

The 6FL Toilet Soap Co.

Table XIV. presents the results obtained from various groups of individuals. In each case the median position of the judgments is given and its quartile. The P.E. of the median is, of course, the quartile divided by the square root of the number of cases.[1] The first section gives the median and quartile of the group of 50 students which has been discussed above. The second section presents the data from 25 young business men in the class of advertising at the 23d St. Y. M. C. A., which I was able to secure through the courtesy of Mr. Geo. E. Beck, the Educational Secretary of the Association, and Mr. F. L. Blanchard, the leader of the class. The third section presents the data from 25 graduate students in Teachers College, nearly all of whom are teaching in this city or vicinity. The fourth section presents similarly the data from 20 graduate students in Columbia University. In the next two sections we have data from 21 undergraduates from Columbia College and 10 from Dartmouth College. The latter were obtained through the aid of Professor W. V. Bingham. Section seven gives a summary of these five groups of men. Section eight gives the data from 52 Barnard students, 22 of whom performed the experiment last year and 30 this year. Then follows a group of 43 women comprising married and unmarried women and ranging in age from 20 to 60. Eighteen of these were secured through the assistance of my sister, Miss A. L. Strong, of San Francisco. The tenth section comprises both of these groups of women. Then come five sections giving four groups of subjects and a summary of the four. The data were secured for me through the great courtesy of Mr. Wm. McGee, of Garrison, N. Y. All of the subjects in these sections live in the vicinity of that place and consequently represent a distinct class of subjects so far as it is possible to differentiate people according to environment. The first of these five sections, or the eleventh in all, deals with the data from 59 farmers, the twelfth section deals with 14 business men, the thirteenth with 6 doctors, the fourteenth section with 22 others comprising—4 blacksmiths, 3 saloon-keepers, 3 store-keepers, 3 policemen, 2 bakers, 2 lawyers, 2 postmasters, 1 plumber, 1 undertaker, and 1 painter. The last section gives a summary of these last four groups. (The data for the 3 policemen and 1 painter were received after the summary was calculated and are consequently not included in it.) In Table XV. will be found the detailed distribution of the judgments of the 101 men, the 95 women, and the 97 men from the vicinity of Garrison.

[1] This relationship is only true when the distribution of judgments follows the normal curve of distribution. In those cases where we do not have this symmetrical distribution the P.E. of the median position can be only approximately indicated. By calculating it from the quartile rather than the A.D. we obtain a larger figure. Whatever error arises then will tend to underestimate the accuracy of the median position rather than to overestimate it.

TABLE XV

DISTRIBUTION OF JUDGMENTS OF 101 MEN.

Position	Number of Advertisements																			
	16	6	10	8	11	4	2	9	3	17	5	1	13	19	12	18	15	7	20	14
1	15	7	6	10	10	6	5	3	1	12	5	1	7	0	1	7	5	0	0	0
2	20	14	7	6	1	4	3	8	1	7	3	3	6	0	5	5	6	2	0	0
3	11	7	8	4	15	6	6	2	4	6	9	4	3	1	2	6	4	3	0	0
4	11	11	8	8	5	5	5	6	4	7	9	6	3	0	2	4	3	2	3	0
5	5	8	11	5	11	4	7	8	7	6	2	5	10	0	1	3	3	4	0	0
6	9	7	11	1	10	2	8	3	7	4	5	3	6	3	3	5	6	7	0	0
7	10	9	6	12	3	2	5	10	9	3	2	6	3	2	3	3	7	3	1	1
8	3	8	6	4	8	5	7	6	10	5	8	5	7	5	4	1	2	3	1	2
9	2	12	7	3	3	5	4	4	5	6	4	8	6	7	5	7	3	5	4	1
10	4	3	3	8	5	4	9	6	6	7	6	6	7	2	2	3	8	7	2	2
11	3	0	2	6	8	3	5	3	5	3	4	14	3	7	10	4	6	7	4	4
12	3	0	5	3	5	8	5	5	9	3	7	3	7	8	9	4	6	9	2	0
13	1	3	3	2	3	7	9	2	6	7	6	11	6	6	6	5	5	4	4	4
14	0	0	4	7	4	10	3	3	5	8	4	5	5	7	7	9	3	7	9	1
15	0	3	4	8	2	5	3	3	6	3	3	5	4	7	14	8	8	2	9	4
16	2	2	4	1	2	6	3	8	2	5	3	6	4	14	12	3	5	4	11	7
17	1	1	4	6	4	4	3	7	5	0	9	7	6	6	3	8	1	6	13	8
18	0	4	1	3	1	5	7	5	5	5	3	2	5	13	7	3	6	4	9	12
19	1	1	1	3	0	3	2	8	3	2	6	2	1	9	5	6	11	10	13	14
20	0	1	0	1	1	7	2	1	1	2	3	2	1	3	0	7	2	12	16	40

TABLE XV. (*Continued*)

DISTRIBUTION OF JUDGMENTS OF 95 WOMEN

Position	Number of Advertisements																			
	16	6	10	8	11	4	2	9	3	17	5	1	13	19	12	18	15	7	20	14
1	25	16	4	10	2	0	6	4	3	12	1	6	2	0	0	0	2	2	0	0
2	20	7	11	12	6	3	2	3	2	7	5	6	3	1	3	2	1	1	0	0
3	10	18	10	6	5	3	3	8	6	5	2	9	5	1	2	0	2	1	0	0
4	8	10	10	10	10	5	6	5	4	4	5	5	6	2	2	3	0	1	0	0
5	5	12	16	4	6	4	2	2	9	6	9	2	7	3	1	3	2	0	2	0
6	4	6	11	6	4	4	5	10	3	4	5	11	6	1	3	2	2	4	2	0
7	5	11	6	9	7	4	5	7	6	6	5	5	3	4	5	4	0	2	1	2
8	4	3	1	5	9	8	8	4	5	4	4	6	4	7	2	4	4	6	4	1
9	2	1	6	2	9	7	9	5	7	4	8	11	7	1	7	5	2	1	0	1
10	0	2	4	2	11	6	8	6	9	7	6	0	7	10	8	3	1	4	0	1
11	2	1	5	4	3	3	12	7	6	5	4	2	6	3	6	6	7	3	8	3
12	0	1	2	4	5	4	5	5	7	8	2	5	8	2	8	8	4	7	6	2
13	2	2	2	3	4	6	8	5	8	1	2	7	6	6	7	5	8	6	5	2
14	2	1	4	5	1	2	2	7	6	7	5	5	5	2	6	10	12	1	6	6
15	1	0	0	1	3	6	3	1	0	5	8	6	4	7	11	8	11	9	7	3
16	3	1	0	5	1	7	2	7	5	0	7	1	5	7	5	8	10	8	10	4
17	0	0	0	3	4	9	4	4	2	1	8	5	4	11	5	8	8	3	11	5
18	0	1	1	1	2	6	2	3	2	5	4	1	4	7	9	5	6	11	21	5
19	1	1	0	3	2	5	2	0	2	2	3	4	2	12	3	5	9	12	8	21
20	1	0	2	0	1	3	1	2	3	2	2	1	1	8	2	6	4	12	4	40

TABLE XV. (*Continued*)

DISTRIBUTION OF JUDGMENTS OF 97 MEN FROM VICINITY OF GARRISON, N. Y.

Position	Number of Advertisements																			
	16	6	10	8	11	4	2	9	3	17	5	1	13	19	12	18	15	7	20	14
1	2	6	7	4	2	7	4	7	6	8	5	4	8	6	5	1	2	5	6	2
2	5	4	6	2	4	4	3	6	4	5	7	5	3	6	9	4	7	3	5	5
3	2	6	2	3	3	9	3	8	6	3	5	3	5	5	4	4	5	7	7	7
4	3	2	5	5	7	2	5	1	4	3	6	7	6	9	6	4	4	8	2	8
5	5	6	2	6	4	5	1	2	4	4	4	5	5	12	4	5	6	7	1	9
6	8	2	8	4	6	4	5	1	5	0	6	6	4	8	4	4	3	1	9	9
7	5	6	6	3	6	5	4	3	6	7	3	2	7	4	4	3	8	5	7	3
8	1	6	3	6	3	5	10	3	6	4	4	7	2	3	11	2	2	5	6	8
9	6	4	6	3	5	4	7	7	6	6	2	3	9	0	7	3	5	7	5	3
10	5	7	2	7	3	5	5	5	8	4	4	4	2	6	5	8	6	6	3	2
11	5	4	6	7	6	5	7	5	5	5	2	7	5	2	4	4	5	8	2	3
12	6	5	3	6	4	6	8	4	2	1	7	5	6	5	4	4	6	6	3	6
13	10	3	6	6	10	4	6	7	4	5	3	5	4	2	2	4	6	4	3	3
14	6	4	2	5	3	4	5	9	5	1	6	6	6	3	4	10	1	3	7	7
15	5	3	6	9	5	4	3	4	3	7	7	6	5	4	4	4	5	4	5	3
16	8	1	6	3	10	6	8	7	1	6	5	4	1	6	5	4	3	5	3	5
17	3	7	4	3	6	4	5	6	5	10	6	6	4	4	6	5	6	2	2	3
18	1	4	7	3	3	7	3	5	6	2	7	4	7	3	1	12	3	4	10	4
19	8	9	5	8	3	4	3	4	4	7	6	3	6	2	1	5	7	4	7	0
20	3	7	5	4	4	3	3	3	7	8	3	5	2	7	6	7	7	3	4	6

If now we compare the results obtained from the group of 50 college students with (1) the 101 men and (2) the 95 women, we find a very high degree of resemblance. Using the Pearson "product-moment" method of calculating correlation we obtain coefficients, respectively, of +.90 and +.95 and with the "rank-differences" method coefficients of +.88 and +.91.[2] Now, if we desire to note the differences that occur in the several orders of superiority, we have two methods of doing this: first, by noting the *difference in the amount* of the median position of the respective advertisements and second, by noting the *difference in the order* of superiority of the advertisements. The first method, considered together with the variability of the respective positions, *i. e.,* the quartiles, gives a measure of the *uniformity* with which the first group ranked the advertisement as compared with the second group. Comparison of the data from the group of 50 students with the 101 men shows little difference between them as to uniformity of judgment. The average of the quartiles for the former is 3.89 as compared with 4.02 for the latter. The former group is thus slightly less variable in its judgments than the latter. The average of the quartiles of the 95 women is 3.45, which indicates that they as a group are less variable in their judgments

[2] If these coefficients could be corrected for attenuation they would show a still higher figure indicating a very close relationship.

than either of the other two groups. Not only is the variability greater for the men if we thus consider the average quartiles of the two groups but if we consider the quartiles of each advertisement in the one order with that of the corresponding advertisement in the other. For in this case the quartiles for every advertisement except No. 19 are greater for the men than for the women. This same point is also shown clearly when we compare the range of judgments. For the 50 students there is a range from 3.8 to 18.3; for the 101 men a range from 3.9 to 18.8; while for the 95 women it is from 2.7 to 19.1. That this group of 95 women should show less variability in their judgments than the 101 men is rather surprising as 43 of the 95 women range in age from 20 to 60, come from various walks in life, have had varying degrees of education, and are living—some in New York city, some in San Francisco, and some in Garrison, N. Y. The men are all college men, with the exception of most of the members of the subgroup of 25 business men and are all within twenty years of each other in age.

The second method, as outlined above, gives the *relative rank* of one advertisement with another on the basis of the entire twenty. By this method, then, and considering only differences of more than one position, we find that advertisements Nos. 4, 12, and 19 are ranked higher by the 50 students than by the 101 men and advertisements Nos. 7, 11, 13, 15, and 17 are ranked lower. The only differences of more than five positions are with advertisement No. 4, the Ivory Soap "Carnegie Institute" appeal and No. 13, the "reliable firm" appeal. Comparing the group of 50 students with the 95 women we find that advertisements Nos. 2, 4, and 19 are ranked higher by the former than by the latter and advertisements Nos. 1, 3, and 17 are ranked lower. Here again No. 4 is ranked higher by more than five positions by the 50 students than by the 95 women and No. 1 is so ranked lower. No. 1 is the "beauty" appeal and is ranked fifth by the women as against twelfth by the 50 students. We may conclude then that the group of 50 students represents very closely groups of educated men or young business men and groups of college women or women of the middle class and of varying ages. Advertisement No. 4, however, is ranked too high by the students while No. 13 is ranked too low to represent other groups of men and No. 1 is ranked too low to represent other groups of women.

Handling the data in the same manner we find that advertisements Nos. 2, 7, 11, 13, 15, and 17 are ranked higher by the 101 men than by the 95 women and advertisements Nos. 1, 3, and 12 are similarly ranked lower. Men then rank the following appeals slightly higher than women: "Doctor's recommendation," "For

Particular People," "Shampoo and Bath," "Reliable Firm," "Royalty," and "Exhilaration in the Bath," while they rank these lower: "Beauty," "Baby" and "Sold Everywhere." Of these nine appeals the "Reliable Firm" appeal is, however, ranked decidedly higher by the men than by the women and the "Beauty" appeal decidedly lower. But the interesting thing in the two orders of superiority is not these differences but that the two orders agree so closely. This is only another instance in which experimental work has failed to show any marked differences between men and women as to mental traits.

Now let us turn and compare our order determined from the 50 students with the order from the 97 men living about Garrison. Here the one striking point is the dissimilarity between the two. With the Pearson "product-moment" method of calculating correlation we obtain a coefficient of —.700 between the two groups and with the "rank-differences" method a coefficient of —.532. If we consider the range of positions for the advertisements we see that that of the 97 men is from 7.1 to 13.1 or only 6 places while that of the 50 students is from 3.8 to 18.3 or 14.5 places. This greater variability of the former group is likewise revealed in the amount of the quartiles—all but three are greater than with the group of 50 students and the average quartile is 5.01 as compared with 3.89 of the latter group. As a quartile of 5.00 would result from a chance sorting and the range of positions is but three tenths of the possible range it is evident that the data from the 97 subjects do not differ much from a chance distribution. That being the case very little can be postulated regarding the relative superiority of one appeal over the next. It may be that this is a characteristic of such a group of uneducated persons—that they are unable to differentiate complex appeals. That is to say, that on the whole any one of these appeals is as strong as any other one in selling soap. Further experiments should be carried out, however, before such a radical position can be affirmed.

Turning now to the order of superiority of appeals as determined by the 97 men we have the following: "Cheap," "Souvenir Spoon," "Sold Everywhere," "Reliable Firm," "For Particular People," "Baby," "Large Manufactory," Ivory "Carnegie Institute" Ad., "Does not irritate the skin," "Royalty," "Doctor's Recommendation," "Health," "Beauty," "Expensive," "Shampoo and Bath," "Bath," "Try it at our Expense," "Guaranteed," "Cleanliness," and "Roosevelt." We may conclude that an order of superiority as determined by college students will not represent at all such a group as the men living in and around Garrison, N. Y.

Having compared the three "Summary" groups of subjects with the group of 50 students let us now note to what extent the various subgroups resemble the summary groups. Table XVI. is presented in order to give the detailed facts in as concise a manner as possible and so that comparisons can be made in those ways which are desired by the reader. The coefficient of correlation between each subgroup and the summary is given in order to indicate how closely the latter does really represent the former. The range of judgments and the average quartiles are shown in order to give an idea of the variability of the subgroup. And then the various advertisements are given which are ranked more than one position either higher or lower than in the order of the summary. By glancing over such a list one can determine which advertisements are judged differently by the various subgroups while the absence of an advertisement from these lists indicates that the subgroups are agreed as to its merit.

The majority of the variations listed in the above table are deviations of two or three positions from the order of the summary. Any deviations of five or more positions must be interpreted to be an indication of some real difference between the subgroup and the summary. The 25 business men rank No. 8, the "Expensive Appeal," ten places below the summary order and they rank No. 5, the "Try it at our expense" appeal, six places above the summary order. The 25 teachers rank No. 4, the Ivory "Carnegie Institute" appeal, and No. 18, the "Roosevelt" appeal, seven and nine positions, respectively, above the summary order, whereas they rank No. 11, the "Shampoo and Bath" appeal nine positions lower. The 20 graduate students rank No. 15 and No. 18, the "Royalty" and "Roosevelt" appeals six places higher than the summary order, while they rank No. 1, the "Beauty" appeal, No. 3, the "Baby" appeal, and No. 13, the "Reliable Firm" appeal, five, six, and seven positions, respectively, below that order. The 21 undergraduates rank No. 3, the "Baby" appeal, five positions higher and No. 8, the "Expensive" appeal, eight positions lower than the summary order and the 10 Dartmouth undergraduates rank No. 9 six places lower than this order. In general graduate students are more influenced by such appeals as "Royalty" and especially "Roosevelt" than are undergraduates and young business men and they are less influenced by such appeals as "Beauty," "Baby," and "Reliable Firm." The group of young business men rank the "Expensive" appeal low as compared with the other subgroups but are impressed by such an appeal as "Try it at our Expense."

There is no characteristic difference between the various subgroups of women. If we compare the results from the data of 22 Barnard students secured in April, 1910, and the data of 30 Barnard

TABLE XVI

SOME DETAILED FACTS CONCERNING THE SUBGROUPS

Subgroups	Correlation with Summary	Range	Q.	Ads. ranked higher than Summary Order	Ads. ranked lower than Summary Order
25 Business Men	+ .85	4.3 to 17.8	3.5	1, 3, 5, 7	2, 8, 18
25 Teachers	+ .79	4.0 19.3	3.8	2, 4, 8, 18	1, 3, 11, 13, 17
20 Graduate Students	+ .82	4.0 17.8	4.1	2, 4, 7, 8, 15, 18, 19	1, 3, 10, 13, 17
21 Undergraduates	+ .89	3.0 18.3	3.8	1, 3, 4, 9	2, 5, 8, 17
10 Undergraduates	+ .93	2.5 19.9	3.1	3, 4, 8, 12, 15	2, 5, 6, 7, 9
101 Men (Summary)		3.9 18.8	4.0		
52 Barnard Students	+ .98	2.2 19.3	3.3	3, 9, 17	
43 Women	+ .96	3.4 19.0	3.8	7, 20	3, 12, 15, 19
95 Women (Summary)		2.7 19.1	3.5		
59 Farmers	+ .72	7.0 13.4	4.8	1, 2, 5, 6, 7, 8, 10, 15, 16	4, 12, 13, 17, 20
14 Business Men	+ .14	6.0 15.0	4.6	4, 8, 9, 11, 16, 17, 18	2, 5, 6, 10, 13, 14, 15, 19
10 Doctors	+ .56	5.0 18.5	3.1	4, 5, 8, 9, 11, 18	1, 2, 7, 12, 15
22 Miscellaneous Group	+ .31	8.0 14.8	4.6	1, 2, 5, 9, 10, 16, 17	3, 4, 6, 7, 11, 13, 14, 15, 20
97 Men (Summary)		7.1 13.1	5.0		

students secured in November, 1910, we find a remarkably close relationship between the two orders of preference for the different appeals and also for the actual amounts assigned each advertisement. The Pearson "product-moment" method of correlation, which takes both of these factors into account, gives a coefficient of $+.93$. If this was corrected for attenuation we would have practically $+1.00$. That a group of individuals should give results almost identical with that of a similar group of different individuals separated by an interval of half a year in time and pertaining to such supposedly complicated mental processes is truly surprising. Admitted that much remains to be done in establishing the relationship between results obtained by this method and actual business conditions, it is certain that the processes studied and measured here are as definite and constant as those met with in the other sciences.

The facts pertaining to the group of 97 men from Garrison as presented in Table XVI. show clearly that the summary order of preference does not closely represent the various subgroups. The correlation figures are low and the number of appeals which are ranked differently by the subgroups is large. However, these subgroups are better represented by their summary than by the order of preference of the 50 students. The corresponding coefficients of correlation between the subgroups and this order are respectively, $-.26$, $-.24$, $-.12$, and $+.01$.

Summary.—A group of 50 college students will represent very closely the judgment of groups of educated men and women, of young business men, such as attend evening schools, etc., and of women of the middle class regardless of age. They will not represent at all the judgment of groups from small towns and farming sections such as the regions around Garrison, N. Y., from which the data were obtained.

It is fair to extend the results as set forth in previous chapters regarding the judgment of college students to groups of educated men and women in general. But as the data of this report are mainly concerned with cheap articles of common use, very little can be postulated concerning the relation of various groups of individuals with regard to more expensive commodities.

CHAPTER VII

A Study of 50 Packer's Tar Soap Advertisements

Section 1. *The Effect of Repetition on "Pulling-Power"*

Should the same advertisement be repeated again and again in an advertising campaign or should an entirely new advertisement be presented very frequently? Should the same cut be repeated and the copy varied or the reverse? Should the same general arrangement of copy and cut be maintained, as in the Fairy Soap Advertisements of to-day, or should an entirely new arrangement be presented with each new advertisement? Should the same advertisement be displayed in all the magazines of one month or should a different advertisement appear in each magazine?[1] Such are only a few of the many questions as to the desirability of *repetition* that constantly confront the advertising man of to-day.

Back of all such questions lies the general one—does an advertisement have an intrinsic value which is constant to the same observer at all times or does repetition affect its value? In other words, does an advertisement wax or wane in interest as do so many jokes?

Hollingworth[2] found that naive jokes and calamity jokes in which the predicament of the victim is self-induced wax in interest, while the sharp retort, the pun or play on words, wit, caricature, and the occupation-joke wane. Jokes combining the two elements remain static.

With such work in mind and using the same method an attempt was made to determine the effect of repetition upon fifty different Packer's Tar Soap Advertisements.[3] These advertisements are shown in Plates V. to IX. In these five Plates two numbers are shown under each advertisement. The first is the number by which the advertisement is referred to in the tables and text. The second number is the position among the fifty as assigned by the experimental data and as shown in Table XX. For example, the first advertisement in Plate V. is marked "1–29"; the "1" is the number by which

[1] Two interesting articles on this point of recent date are J. DeWolff, "Repetition of Advertising Copy," *Printer's Ink,* October 20, 1910, and R. Barstow, "Repetition—Desirable and Otherwise," *Printer's Ink,* October 6, 1910.

[2] H. L. Hollingworth, "Experimental Studies in Judgment; Judgments of the Comic," *Psych. Rev.,* XVIII., 2.

[3] These advertisements were obtained through the courtesy of Mr. Edward A. Olds, Jr., of the Packer Manufacturing Co.

the advertisement is designated, the "29" indicates that it is ranked twenty-ninth in the fifty. It should be noted that many of them have long been discarded from use. Ten subjects were used, five men and five women, all students with some experience in experimental psychology. None knew the object of the experiment nor were they told at any time that they would be asked to sort the advertisements again. The fifty advertisements were given them with the following type-written directions.

DIRECTIONS

Sort these 50 advertisements into 10 piles according to the order in which *you would buy* the soap.

Take for granted that each advertisement represents a different make of soap.

Endeavor to make equal the difference in merit betwen each pile and the one above it.

Pile No. 1 will contain those advertisements whose soap you feel very much attracted to buy, while on the other hand, pile No. 10 will contain those advertisements whose soap you feel that you would not buy.

In every case they were informed that they might actually sort the advertisements into less than ten piles if they chose but the gaps should be indicated. In this way, *e. g.*, they did not need to put any in pile No. 10, if they felt that there was no advertisement which made them feel that they would not buy the soap. Each of the ten subjects sorted the fifty advertisements once a week for five successive weeks. There were eleven irregularities: Two arrangements being made within five days of each other, three after eight days, three after ten days, and three after fourteen days. In every case, however, the same hour of the day was used.

Wells[4] and Downey,[5] basing their statements on data secured under practically the same conditions as my own, state that a week is sufficient interval to overcome any serious defect in judgment from memory. My own introspections coincide with those of Downey, that "in general, there was a memory of the order of groups rather than of individual cards." Subject Dod said, "I have no memory of the position of the several advertisements, but I do remember the reasons which actuated me in the sorting of the advertisements." Personally, I felt very strongly on sorting the advertisements a sixth time that when I placed No. 45 first, I had radically departed from my previous arrangements and was dumbfounded when I discovered that it averaged second in the five sortings.

[4] F. L. Wells, "On the Variability of Individual Judgments," "Essays Philosophical and Psychological," 1908.

[5] J. E. Downey, "Preliminary Study of Family Resemblance in Handwriting," *Psych. Bulletin*, Univ. of Wyoming, No. 1, 1910.


Packer's Tar Soap
From the Pine Tree to the People
Packer's Tar Soap
For the Hair and Skin
Boy or Girl
Packer's Tar Soap
Packer's Tar Soap For Baby's Bath
Packer's Tar Soap
For Irritated Skin
Packer's Tar Soap
Pure as the Pines
A Luxury for Bath and Shampoo
Packer's Tar Soap
In Earliest Infancy
Packer's Tar Soap
The Tired Tourist
knows the refreshing invigorating, soothing and cleansing effects of
Packer's Tar Soap
in all exhausted overheated and aching conditions
Indispensable to Travelers.
No Woman Is Beautiful
Packer's Tar Soap
Packer's Tar Soap
Packer's Tar Soap
The Breath of the Pines
Packer's Tar Soap
1—29
2—10
3—7
4—38
5—15
6—22
7—47
8—48
9—11
10—12

PLATE V.

Tables XVII. and XVIII.[6] give the average position of each advertisement for each of the five successive arrangements for the five men and five women respectively. From these a study of waxing and waning for the two groups of subjects can be made. Table XIX.[6] shows graphically whether a certain advertisement waxed or waned with any one of the ten subjects. A full arrow (↑ or ↓) indicates a final displacement of more than one full position as compared with the first arrangement; a check (^ or ˅) indicates similarly a displacement of only one position; and a dash (—) indicates that the advertisement was placed in the same position or pile all five times. From this table then a study of the waxing and waning for the individual subjects may be made. Conclusions from Tables XVII. and XVIII. must be checked in Table XIX., else the large variation of one individual may offset the slight opposite tendency of the other four and make it appear that the group-preference waxes or wanes with respect to that advertisement.

From these data I would conclude that advertisements Nos. 2, 3, 16, 22, 26, and 49 wax and Nos. 1, 6, 8, 10, 11, 12, 19, 27, 28, 31, 32, 33, 36, and 38 wane with the women. The former group consists of pleasing "artistic" advertisements while the latter are not specially "artistic." Two women, to whom I showed these groups, *felt* that there was a decided difference between the advertisements in the two groups and that it was only natural that the former should wax and the latter should wane. They were utterly unable to give any reasons for their feeling, however. But with the group of waxing advertisements (Nos. 2, 7, 24, 26, 34, 35, and 36) and the group of waning advertisements (Nos. 1, 3, 6, 8, 11, 16, 18, 20, and 43) of the men there does not seem to be any reason or feeling for the two groups. Neither does a study of the six advertisements, which clearly waxed or waned for both men and women (Nos. 2, 26, 1, 6, 8, and 11) show any reasons for their rise or fall in preference. A careful study of the two groups of waxing and waning advertisements for each subject failed to show any general principle, except with the one subject C, a married man. Here all the advertisements depicting women waxed.

The failure to obtain definite results may be due to any one of three reasons: First, advertisements do possess an intrinsic value which is constant to the same observer at all times under the same conditions. Repetition then does not affect its "pulling-power." Second, the method of experimentation is not adequate, the waxing and waning effect which was noticed being due merely to fluctuations

[6] These tables are on file in the Psychological Laboratory of Columbia University.

in the judgments. Third, the advertisements combine various elements, some of which tend to wax and some to wane. Under such conditions there would be extreme difficulty to determine what these elements are and their tendencies from such complex advertisements. Further work should be carried on but using very simple "lay-outs."

Section 2. *The Relative Merit of the Fifty Advertisements*

After obtaining the above results regarding the effect of repetition on the value of advertisements, it was deemed worth while to study the relative merits of these advertisements as to "pulling-power." In all the previous experiments, as has been shown, the great defect in the value of the results has been the inability to ascertain the actual merit of any advertisement. The results in every case were relative. We could determine which advertisement in the set was the most effective, we could even state how many chances there were in a thousand that the first advertisement was superior to the second. But in no case could we judge the actual superiority of the first advertisement over the second; in other words, how much more business the first would bring than the second. The following experiment is an attempt to solve this question.

The ten subjects used in the repetition-experiment were retained and fifteen others, ten men and five women, were employed. Of the fifteen men five were married and had children. All of the twenty-five had had some experience in experimental psychology—such were selected in the hope that valuable aid might be obtained through introspections. The subjects represent fairly well the class of people who are appealed to by the advertisements, except that they lack a group of married women.

The following typewritten directions were given to the subject together with the fifty advertisements.

Directions

Sort these 50 advertisements according to the order in which *you would buy* the soap.

Take for granted that each advertisement represents a different make of soap.

Arrange them into as many piles as you desire; but so arrange them that the difference in superiority of one pile over the next is "just noticeable."

If the superiority of one advertisement over the next is more than "just noticeable," leave as many gaps (empty piles) as you feel are needed to indicate this superiority.

After the sorting was completed a second set of directions was given them, as follows:

DIRECTIONS No. 2

Designate the pile, if there is such, which has no appeal to you at all. The piles above it should then all have an increasing appeal for you to buy their soap and the piles below it should have an increasing negative effect upon you (*i. e.*, prejudice, distaste, or disgust with the soap).

Note down, as best you can, the reasons why you have thus arranged the advertisements. That is to say, what were the guiding principles in the sorting of the advertisements.

The subjects sorted the advertisements into piles ranging in number from six to thirty-seven. The highest pile was arbitrarily assigned the value of 100 and the pile, which the subject designated as having no appeal, was assigned the value of zero. The piles between these two were assigned values proportionately. The piles below the "no-appeal" pile were assigned correspondingly negative values. The values assigned to the advertisements in the following tables are thus figured from (1) the advertisements which the subject considered the best in the set and (2) from the advertisements which the subject considered of no appeal. Considerable care was taken in every case that each subject understood the meaning of "no appeal," so that as far as possible it had the same content for all twenty-five subjects. It is believed that this zero point does actually approximate the zero point of appeal in advertisements. The 100 mark, of course, simply marks the best advertisement in the fifty. Whether that point is below perfection and, if so, how far must remain unanswered in this report. But from a business point of view the difference between the *ideal* advertisement and the *best* advertisement is of far less moment than the questions—"which is the best advertisement"; "how much better is it than the others in use"; and "why is it better."

The results from the twenty-five subjects are set forth in Table XX. The median judgment with its quartile[7] is given. The first column lists the advertisements by number in the order of the medians of the twenty-five subjects. Then follow the results from the fifteen men, the ten women, and finally the combined results of the two groups. The fifth section gives the position of each advertisement as determined by Mr. Edward A. Olds, Jr., of the Packer Manufacturing Co., as based on the firm's experience. Here, as above, the best advertisement is graded 100, the advertisement of

[7] The quartile was calculated by averaging the distance from the median in both directions to the point which included 25 per cent. of the cases. As shown later, the distribution of judgments in this experiment is approximately symmetrical: hence, the quartile gives a very close approximation to the P.E. See Thorndike, "Mental and Social Measurements," pp. 78–79.

TABLE XX

GRADES AND DEVIATIONS IN THE JUDGMENT OF PACKER'S TAR SOAP ADVERTISEMENTS

Ad. No.	15 Men Med.	15 Men Q.	10 Women Med.	10 Women Q.	25 Subjects Med.	25 Subjects Q.	Packer Mfg. Co.	B.-R. Agency Av.	B.-R. Agency P.E.
11	84	*18*	75	*40*	77	*20*	85	62	*17*
12	84	*20*	45	*44*	75	*31*	31	31	*17*
29	67	*26*	70	*28*	67	*23*	71	36	*29*
21	71	*21*	48	*22*	67	*26*	86	4	*5*
13	60	*19*	54	*42*	60	*24*	81	2	*3*
48	60	*20*	44	*21*	54	*21*	100	33	*37*
31	53	*19*	50	*49*	53	*18*	91	36	*10*
43	60	*25*	40	*21*	53	*23*	94	56	*31*
3	60	*31*	41	*33*	53	*30*	70	0	*0*
2	60	*21*	42	*30*	50	*23*	42	23	*12*
10	60	*18*	40	*44*	50	*26*	76	6	*6*
9	50	*14*	53	*52*	50	*28*	77	18	*10*
45	70	*21*	37	*39*	50	*31*	65	52	*28*
42	78	*38*	20	*39*	50	*40*	87	65	*38*
49	50	*21*	39	*29*	47	*26*	23	0	*0*
5	17	*29*	61	*26*	47	*31*	75	4	*5*
15	47	*38*	31	*21*	45	*27*	21	21	*23*
47	45	*17*	29	*38*	40	*20*	63	0	*0*
50	47	*17*	38	*47*	40	*23*	44	0	*0*
30	45	*16*	22	*62*	40	*29*	68	— 7	*10*
46	40	*32*	20	*39*	40	*34*	66	18	*21*
6	41	*22*	23	*42*	36	*25*	79	0	*0*
20	53	*21*	9	*38*	36	*27*	69	0	*0*
40	35	*27*	32	*30*	35	*28*	73	6	*21*
18	50	*22*	—11	*35*	35	*35*	19	—17	*9*
23	32	*19*	37	*24*	34	*20*	84	46	*30*
16	32	*19*	42	*26*	34	*24*	39	0	*0*
22	32	*19*	50	*23*	34	*28*	80	— 8	*9*
1	45	*24*	20	*31*	33	*27*	48	0	*0*
17	33	*22*	39	*30*	33	*27*	13	—17	*9*
34	45	*22*	—28	*47*	33	*42*	64	19	*13*
14	31	*20*	6	*47*	30	*34*	60	8	*7*
32	59	*27*	13	*33*	30	*34*	28	—25	*0*
24	60	*34*	5	*38*	29	*41*	16	0	*0*
26	40	*32*	— 6	*42*	28	*47*	40	0	*0*
38	58	*20*	0	*6*	27	*30*	56	8	*9*
44	50	*33*	4	*20*	27	*32*	82	26	*9*
39	77	*34*	4	*35*	27	*48*	88	52	*29*
4	25	*30*	14	*29*	25	*32*	67	0	*0*
28	52	*35*	— 9	*45*	23	*47*	78	81	*3*
25	20	*37*	10	*11*	20	*31*	34	— 8	*9*
41	24	*15*	—26	*44*	18	*29*	50	0	*0*
36	30	*22*	—17	*25*	17	*29*	38	— 8	*9*
37	18	*19*	—34	*47*	11	*28*	46	38	*21*
35	18	*23*	—28	*47*	11	*29*	58	— 8	*9*
33	16	*20*	—11	*42*	11	*29*	25	0	*0*
7	10	*20*	— 2	*46*	10	*24*	6	—17	*9*
8	20	*19*	—40	*47*	10	*29*	0	—17	*9*
27	0	*7*	— 6	*36*	0	*7*	—12	—17	*9*
19	0	*25*	—43	*30*	—10	*27*	— 6	—17	*9*


11—1
PACKER'S TAR SOAP
PURE AS THE PINES
12—2
EMBLEMS OF PURITY
Purity, sweetness, refreshing and delightful cleanliness are the sensations produced by the use of
PACKER'S TAR SOAP
Pure as the Pines
13—5
Pure as the Pines
Packer's Tar Soap, the standard of purity and quality, is known, recommended and used by medical practitioners the world over.
Packer's Tar Soap
is exactly adapted to the skin and hair, at all times and in all climates. It brings the health-giving properties of the balsamic pines into the home. Refreshing, cleansing, soothing—it is
UNEXCELLED FOR THE
Nursery, Toilet and Shampoo
THE PACKER MANUF'G CO. NEW YORK
14—32
PACKER'S TAR SOAP
Pure as the Pines
15—17
For Prickly Heat and Irritated Skin use Packer's Tar Soap and
DON'T SCRATCH
PRICKLY HEAT
Directions for Local Treatment: To promptly relieve itching and smarting, cleanse thoroughly with Packer's Tar Soap, then apply a lather and pat dry with a towel, repeating as often as desired. This not only removes acrid secretions but soothes the sensitive nerve endings in the skin and gives immediate relief.
16—26
PACKER'S TAR SOAP WINS
on every point in all competitions. Its purity, mildness, excellent cleansing and refreshing qualities, together with its well known antiseptic and healing qualities, have made it the standard for all toilet, bathing and shampooing purposes.
Packer's Tar Soap soothes while it cleanses, and quickly relieves irritated or inflamed skin.
17—30
FOR SHAMPOOING
TOILET, BATH AND NURSERY
PACKER'S TAR SOAP
THE STANDARD
FOR MORE THAN A QUARTER CENTURY
ACCEPT NO SUBSTITUTE
PACKER'S TAR SOAP
EXQUISITE CLEANSER SOOTHING ANTISEPTIC FOR THE
HAIR AND SKIN
19—50
Packer's Tar Soap
IMPARTS to the SKIN the SOFTNESS SMOOTHNESS and the HEALTHY GLOW of INFANCY
SOOTHES WHILE CLEANSING
20—23

PLATE VI.

"no-appeal" as zero, while the intervening advertisements were assigned proportionate values. It is scarcely necessary to repeat that the results of the Packer Manufacturing Co. are not based upon carefully compiled data but only upon the judgment of the firm based on their business experience. Any one familiar with advertising knows that such data have not been compiled for any extensive set of advertisements, let alone a series of fifty extending over twenty years of service. If such data did exist, they could not be used to their full face value, as an advertisement of twenty years ago might have been very effective then and be out of date to-day. The change of style of dress in the cut would alone seriously impair its value. The sixth section of this table gives the average position of each advertisement with the P.E. of that position from the arrangements of three advertising men in the Blackman-Ross Advertising Agency, which is now handling the Packer Manufacturing Co.'s business. Their arrangement was based on an "attention-value" basis instead of "appeal" or "pulling-power" value. Besides, each of the three men assigned values only to about one half of the fifty advertisements. The negative values which they assigned are much more reliable than the positive due to the fact that their attention was especially directed to the poor advertisements. The above five *orders* of preference are also shown in Table XXI.

All references to the order of preference of these fifty advertisements will be based on the median judgment, unless specially mentioned as based on the average, except with the order of preference of the three advertising men of the Blackman-Ross Agency where the average was used instead of the median. In this one case it was manifestly a fairer measure. Besides the reasons already mentioned in Chapter II. as to the superiority of the median over the average with data secured by the "order of merit method" we have here the difficulty of combining a set of data (from the 15 men) of very much less range or variability of judgment with one of great range (from the 10 women), especially in the negative direction. To average such data together is to give in a large number of cases very great value to one or two results far below the zero point, which, if anything, should be discounted. To reject such extreme variates as is done in many physical measurements, as for example under Chauvenet's criterion, would only defeat the object of this study. What is wanted is the position the majority of people would assign and not the average position of good, bad, and indifferent judges. This the median clearly gives. That there is no great difference in the relative order of the advertisements when determined by the aver-

TABLE XXI

"PULLING-POWER" VALUE OF 50 PACKER'S TAR SOAP ADVERTISEMENTS

(Numbers throughout table refer to Number of Advertisement in the Fifty. See photos, Plates V to IX.)

Grades

	—50	—40	—30	—20	—10	0	+10	+20	
15 men subjects						19 27	7	5 35 8 33 37 25	41
10 women subjects		19 8	37 34 35 41	36	18 26 33 28 27	7 38	39 14 44 24 20 25 4 32	1 42 46	6
25 subjects					19	27	33 7 35 8 37	36 41 25	28
Packer Mfg. Co.					27 19	8	7 17	24 18 15	49
Blackman-Ross Advertising Agency			32	7 8 17 18 19 27	22 25 30 35 36	1 26 3 33 4 41 6 47 16 49 20 50 24	5 10 14 13 21 40 38	9 15 46 34	2

age judgment or by the median judgment is shown by the following three correlations:

Correlation between average and median judgment of the 15 men	+ .93
Correlation between average and median judgment of the 10 women	+ .89
Correlation between average and median judgment of the 25 subjects	+ .87

The higher ratings in Tables XX. and XXI. of the advertisements by the Packer Mfg. Co. are due simply to the fact that but one man rated them, while the lower ratings in the other columns are due to the fact that they represent the average of a number of ratings in which all did not agree as to the best advertisements.

The following advertisements were chosen for a study of the distribution of the judgments: No. 29 (64.1), No. 48 (54.4), No. 39 (44.4), No. 40 (34.2), No. 4 (24.4), No. 35 (13.9), No. 8 (2.2), and No. 19 (—5.9). The positions given here are based on the average judgment. The actual distribution of the judgments of these advertisements is shown in Table XXII. and parallel to it is shown the

TABLE XXI

"PULLING-POWER" VALUE OF 50 PACKER'S TAR SOAP ADVERTISEMENTS

(*Continued*)

	+30	+40	+50	+60	+70	+80	+90	+100
				2				
				3				
				10				
	14	1	9	13				
	16	30	18	24				
	22	26 34 15	44 20 32	43	39	11		
4	36 23 17 40	46 6 47 50	49 28 31 38	48 29	45 21 42	12		
		10						
	23	17 2						
	15 45	43 3	22 9					
30	47 40 50	49 16 48 12 21	31 13	5	29 11			
	1							
	16		2					
26	17 6	30	9 3					
38	14 22 18	46	10 31					
39	24 23 20	47 5	42 43	21				
4 44	32 34 40	50 15 49	45 48	13 29	12 11			
		16		34 4	3 5 9	6 13 11 21		
33 32	12 25 36	26 2 50 37 1	41 38 35	14 47 45 46 30	20 29 40 10 28	22 44 23 42 39	31 43	48
	29		39					
44	12 48 31 37	23	45 43	11 42		28		

theoretical distribution as determined with the use of a table of values of the normal probability integral corresponding to values of x/σ. σ in this case was obtained from the A.D. of the average of the twenty-five subjects ($\sigma = 1.25$ A.D.). From the table it is clear that the distribution of judgments follows the probability curve distribution. With a larger number of cases the two would, no doubt, coincide. We may, therefore, assume that the various relationships of the normal curve of distribution hold throughout this series of judgments. (See Chapter II., Section 4, for a further study of these eight advertisements.)

Table XXIII. gives a number of correlations which indicate the resemblance between the five orders of preference of the fifty Packer's Tar Soap advertisements. None of these coefficients of correlation have been corrected for attenuation; hence the true coefficients are in every case somewhat higher than those given here.

SECTION 3. *A Discussion of the Several Advertisements*

There are nine so-called "copy-ads." among the fifty. They may be divided into three classes: (1) Five that are set off with pine trees

TABLE XXII

DISTRIBUTION OF 200 JUDGMENTS OF SOAP ADVERTISEMENTS

Distance from Median	Actual Frequency	Theoretical Frequency
+ 120	100	100
+ 110	100	99
+ 100	100	98
+ 90	99	97
+ 80	96	95
+ 70	94	92
+ 60	88	87
+ 50	84	80
+ 40	74	70
+ 30	50	56
+ 20	29	40
+ 10	20	21
Median	0	0
— 10	19	21
— 20	38	40
— 30	54	56
— 40	68	70
— 50	79	80
— 60	86	87
— 70	92	92
— 80	95	95
— 90	97	97
—100	98	98
—110	99	99
—120	100	100

or pine cones as borders to the copy or similar effects (Nos. 11, 12, 29, 2, and 10); (2) two that display a small cut of a man shampooing at the top of the page (Nos. 42 and 39); and (3) two that are only set off with a border (Nos. 14 and 26). (No. 26 does have a few

TABLE XXIII

RELATIONSHIP EXPRESSED IN COEFFICIENTS OF CORRELATION BETWEEN THE FIVE ORDERS OF PREFERENCE AS TO THE VALUE OF APPEAL AMONG FIFTY PACKER'S TAR SOAP ADVERTISEMENTS

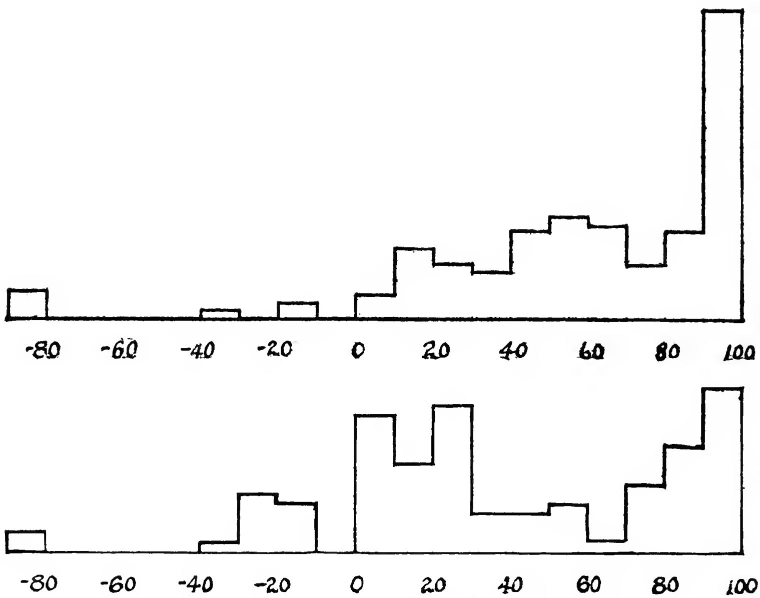
	r.	P.E.
Between 15 men and 10 women	+ .53	.07
Between 15 men and 25 subjects	+ .74	.04
Between 15 men and Packer Mfg. Co.	+ .52	.06
Between 15 men and Blackman-Ross Agency	+ .59	.06
Between 10 women and 25 subjects	+ .87	.02
Between 10 women and Packer Mfg. Co.	+ .53	.07
Between 10 women and Blackman-Ross Agency	+ .31	.09
Between 25 subjects and Packer Mfg. Co.	+ .52	.07
Between 25 subjects and Blackman-Ross Agency	+ .51	07
Between Packer Mfg. Co. and Blackman-Ross Agency	+ .64	.04


Packer's Tar Soap
FOR
WINTER CHAPS
PACKER'S TAR SOAP
21—3
For
Prickly Heat
and
Irritated Skin
use
Packer's
Tar Soap
and
—DONT SCRATCH!
22—27
Prickly Heat—Chafing—Sunburn
and other skin irritations common
to the summer respond at once to
the soothing and emollient action of
PACKER'S TAR SOAP
23—28
Packer's? Packer's!
PACKER'S TAR SOAP
FOR THE HAIR AND SKIN
24—34
Packer's Tar Soap
A
CHAIN
OF
EVIDENCE
25—41
THE HAIR
PACKER'S
TAR SOAP
THE SKIN
26—39
SANTA CLAUS
TO "PACKER'S"
Santa Claus
27—49
If You Want
Hair
When Old
Shampoo
when
Young
with
PACKER'S TAR SOAP
PURE AS THE PINES
28—40
Conservation
Packer's Tar Soap
29—4
PACKER'S TAR SOAP
PURE AS THE PINES
30—18

PLATE VII.

needles in the background but, I believe, they are too hazy to have any appreciable effect.) The first five are assigned positions one, two, three, ten, and eleven, respectively, the second group are assigned positions thirteen and thirty-six, and the third group thirty-two and thirty-nine. Advertisement No. 39, which is judged thirty-sixth in the fifty, should here properly be ranked about eighteenth. The judgments of this advertisement are very markedly bimodal: twelve judgments lie between 100 and 70 and the remaining thirteen between 27 and —22. The median here is subject to a great error in being assigned to the thirteenth judgment which is 27. This is shown in the very large P.E. of the median, the largest of the fifty advertisements, *i. e.,* 10 places, or in other words, the chances are even that the true median will lie between the twenty-second and forty-third position. If the average of the five central judgments is taken it receives a rank of 43 which gives it eighteenth place. If averages instead of medians were used in this study No. 42 would receive tenth position and No. 39 eleventh. In that case the five "pine-copy-ads." receive first, second, third, sixth, and eighth. In any case the three classes of copy-ads., as stated above, are ranked differently by the twenty-five subjects. If the judgments of the five "pine-copy-ads." are shown in a diagram according to their distribution and the judgments of the other four are similarly shown as in Table XXIV. we see that with the former we have a distribution with an undistributed upper end while with the latter group we have a bi-modal distribution. We have here only another instance of the fact already pointed out in Chapter III. that a copy-ad. or a picture-ad. will be ranked high by about half of the people and low by the other half. The four advertisements of the second group consist practically entirely of "copy"—hence the bimodal distribution of the judgments. Of these No. 42 and No. 39 are set off by a small cut and we find that they rank much higher than the other two. The five "pine-copy-ads." have enough of "picture-effect" so that there is no decided group judgment against them. To put this fact in other words, the five produce a pleasing effect through the pine tree or pine cone borders, the reader's attention is attracted to them and he unconsciously glances through their copy. Once having done so they are judged on the basis of their copy. The other four have no such pleasing bait to attract the attention and consequently are read by only that class of people who prefer reading-matter to pictures. The opposite class glance at them and turn to the next one. Subject Dod was one of the subjects that ranked the five "pine-copy-ads." high and No. 14 and No. 26 low. He gave as his reason "the lack of a picture." I am unable to see any particular difference

TABLE XXIV



DISTRIBUTION OF JUDGMENTS OF "COPY-ADS."

Upper diagram represents distribution of "pine-copy-ads" (Nos. 2, 10, 11, 12, 29). Lower diagram represents distribution of "copy-ads" (Nos. 14, 26, 39, 42).

in appeal in the "copy" of the nine advertisements and would conclude that their difference in appeal is to be explained as given above. The "pine-copy-ads." should be approximately twice as effective in selling soap as the "copy-ads." without these decorations. (Eighty per cent. of the judgments of the former are higher than the median position of the latter.)

Advertisements No. 11 and No. 12 are undoubtedly first and second, No. 29 and No. 21 third and fourth, and No. 13 fifth according to these figures. Advertisements Nos. 48,[8] 31, 43, 3, 2, 10, 9, 45, 42, 49, and 5 certainly rank between sixth and seventeenth place. Advertisement No. 15 is given seventeenth position, but its P.E. of position extends from tenth to twenty-first. There is also a slight chance that No. 46 (twenty-first) might displace No. 49 and No. 5 and be rated fifteenth. The above group, eliminating the "copy-ads." already considered, consist of three fourth page cuts and a few sentences of copy with the exception of No. 31 and No. 45 where

[8] Note, however, the discussion of this advertisement on page 75.

the cut occupies but one half of the page. The majority of them depict children in various positions. No. 31 and No. 9 depict the head of a pretty woman, No. 45 a man shampooing his hair, and No. 15 three men in a canoe. The last three of this group, including No. 15, are certainly irrelevant cuts; the remainder suggest the soap either directly or indirectly. Reference to Table XXI. makes their relative rank clear.

Though the pictures of No. 21 (Winter Chaps) and No. 13 (Emblems of Purity) are actually irrelevant, yet the two subscriptions associate the pleasing effect of the picture with the soap. Mr. Edward A. Olds, Jr., of the Packer Manufacturing Co., is quoted in *Printer's Ink*[9] as follows: "I think our two little 'winter chaps' is one of our best pieces of copy. It has at least aroused as much human interest as any that I know of. I have been stopped on the street by acquaintances and asked who those boys are, where they live, and all sorts of questions as to their financial circumstances." Scott[10] styles it, "a full-page advertisement possessing great attention value" and states that in one experiment of his it was mentioned more than any other. Advertisement No. 13 is a publicity advertisement for Easter time, yet it is evident that it is one of the best of the fifty. The suggestion of "purity, sweetness, and delightful cleanliness" has already been shown to be the strongest appeal for soap among college students (see Chapter VI.).

Advertisements No. 48 and No. 43 are ranked first and second by the Packer Manufacturing Co. But the Blackman-Ross Advertising Agency rate them eleventh and fourth respectively. The data here restrict them to sixth position or lower. There is no doubt that such children-appeals stand very high among women. As there were no older women among the twenty-five subjects it may be that the position assigned them here is too low when all classes of consumers are considered. The data from one hundred college students as to the relative merits of eight of the fifty Packer's Tar Soap advertisements, which are given on page 13 would imply that No. 48 should be given at least third place.

Commencing with No. 47 we have twenty advertisements within the grades eighteen and twenty-seven. Advertisement No. 47 ranges[11] between eighteenth and twenty-third position; No. 50 between seventeenth and twenty-fifth; No. 30 between seventeenth and twenty-

[9] G. H. Whitney, "The Personalities of Advertising Models," *Printer's Ink*, December 15, 1910.

[10] W. D. Scott, "The Psychology of Advertising," pp. 164–65.

[11] I have employed this term to designate the deviation equal to the probable error of the median, *i. e.*, the chances are more than even that the true median judgment does not lie outside these limits.

eighth. The next ten range between eighteenth and thirty-fifth and the next seven between twenty-second and fortieth. Evidently there is little difference between the twenty in appeal. The advertisements are so complex that it is well-nigh impossible to state the principles underlying their respective merits. Only such as are particularly striking to the writer will consequently be mentioned. As the data from which these deductions are taken, together with the advertisements, are given here it is possible for the reader to review the deductions himself and also to make any others as he may see fit.

Advertisement No. 31 shows a half-page cut of a pretty girl with flowing hair while No. 30 present a much larger cut of the same girl. The former has several sentences of copy while the latter has only a few words in the form of headings. Subject Woo prefers No. 31 as "better because of the copy." As the former is ranked between sixth and fourteenth and the latter between seventeenth and twenty-eighth there is probably a difference of ten places between them. From the scale of Table XXI. the former is 32 per cent. stronger in appeal. Advertisement No. 9 is very much on the same line as No. 31 and ranks about the same in value.

Advertisements No. 45, ranked thirteenth, and No. 40, ranked twenty-fourth, depict a man shampooing his hair. The consensus of opinion in the introspections was that the head of the former was superior to the latter. These subjects expressed themselves very emphatically that they did not like the rhyme

"His Greatest Delight
Every Saturday Night"

and ranked the advertisement low because of this dislike, although they all liked the face very much. It had a "cheapening effect" and evidently aroused distrust or suspicion—the opposite of confidence. It is interesting to note that Mr. E. A. Olds, Jr., stated that they had discontinued the use of the phrase because of expressed dislike to it. These two advertisements and No. 42 and No. 39 are the only ones with a direct appeal to men to shampoo with Packer's Tar Soap. I believe a combination of the cut of No. 45 or No. 40 and the copy of No. 42 or No. 39 would form a much stronger advertisement than any one of the four as they stand now. The attempt at expressing pleasure—"delight" or "satisfaction"—should be better developed and incorporated in the advertisement. The portrayal of pleasant emotions is probably the most difficult of all appeals to make, but it stands to reason that when well done it must bring results.

The three kitten-ads., Nos. 23, 16, and 22, are ranked about twenty-seventh. As will be pointed out later, they were ranked high


Packer's Tar Soap
31—8
Packer's Tar Soap
LUXURIANT HAIR –
grown white—dignifies an attractive personality when its lustre and vigor are preserved by systematic shampooing with
PACKER'S TAR SOAP
32—33
Packer's Tar Soap.
33—44
Packers Tar Soap
FIVE YEARS' GROWTH
"I send my picture, which represents a growth of hair of but five years, that you may see what Packer's Tar Soap has done for me."
34—31
Packer's Tar Soap
PURE AS THE PINES
CLEANSING REFRESHING ANTISEPTIC
THE STANDARD
FOR THE
HAIR AND SKIN
35—45
Packer's Tar Soap
-Pure as the Pines
THE ANTISEPTIC AND HEALING VIRTUES OF PINE-TAR
PACKER'S TAR SOAP
Emollient
Antiseptic
Healing
36—43
Packers Tar Soap
EVIDENCE
accumulates of the superiority of Packer's Tar Soap for cleansing the hair thoroughly and keeping it soft, lustrous and healthy.
37—46
Packer's Tar Soap
Pure as the Pines
38—35
A Refreshing Shampoo with
PACKER'S TAR SOAP
not only cleanses the hair, but imparts vigor to the whole scalp. Its generous, creamy lather rubbed into the scalp with gentle and continued manipulation softens and removes all dandruff and seborrheal scales. Its antiseptic and tonic action imparts vigor to the glandular structures which nourish and enliven the hair, thus stimulating its growth and maintaining its vitality.
THE PACKER MFG. CO., Suite 87 C, 81 Fulton St., New York
39—36
Packer's Tar Soap
PACKER'S TAR SOAP
40—25

PLATE VIII.

by the women and low by the men. Evidently they rank among the first fifteen for women—having high attention-value. Two of the women, however, maintained that they suggested Bon Ami. To the extent that this suggestion holds true they are not very good advertisements for Tar Soap. The copy emphasizes that Packer's Tar Soap is good for prickly heat, etc. If the advertisements had been sorted in summer instead of in November these appeals might have been ranked somewhat higher.

The last twelve advertisements of the fifty appeal on the whole to women to use the soap. No. 35 gives a testimonial of Mary Tayloe as to the great use of Packer's Tar Soap. A number of the subjects would give no credence to the testimonial: "It was easy enough to fake," or "I have no respect for testimonials, I think they are all fakes." The majority felt that these advertisements were overdrawn, exaggerated. Such comments as these were common: "Too much hair, a fake; seems so after using several hair-dopes," referring to No. 30. "No. 34 must be a lie," or "'Five years' growth'—she probably always had it." "Wouldn't read it," referring to No. 25. "Over-done, doubt the hair-growing property," referring to Nos. 8, 33, 34, 37, and 41.

No. 27 is ranked without doubt forty-ninth. The women, however, place it above several advertisements which especially appeal to women through cuts of beautiful hair, etc. In other words they are more prejudiced by the latter than by it. No. 19 is rated last by all the three groups.

Advertisement No. 28 is rated first by the Blackman-Ross Agency and thirteenth by the Packer Manufacturing Co. It is rated fortieth here. It is a direct appeal to men and, as pointed out later, is rated much higher by them than by the women. This is one of the noticeable differences between the arrangement of the twenty-five subjects and the advertising men.

Advertisement No. 1, ranked twenty-ninth, stands approximately in the middle of the group of fifty, whether considered according to relative position or actual position on the scale. Its appeal that even in India is Packer's Tar Soap sold implies that, therefore, it must be a good soap. This appeal of "Sold Everywhere" has been shown in Chapters V. and VI. to rank fourteenth out of a possible twenty for breakfast foods and seventeenth out of a possible twenty for soaps among college students. Its position in this set is another confirmation of those chapters.

From these results it would appear that "copy-ads." decorated by a border of pine trees or pine-cones appeal most strongly to college people. Advertisements with good pictures of the soap in actual use and with simple language with no suggestion of exaggeration would stand equally high. Relevant pictures of children rank high. How they would compare with relevant pictures of pretty women can not be determined from this study.

The appeal to use the soap to aid the growth of hair would apparently be very weak. However, the general style of the advertisements suggesting this thought and the cuts used are fifteen and twenty years old. What would be the actual value of the appeal in a modern advertisement must be left to future experiments, as the Packer Mfg. Co. have not used "copy" of this nature in recent years.

Section 4. *Sex Differences in the Judgment of the 50 Advertisements*[12]

An inspection of the diagram of Table XXI. shows that the range of judgments for the men is much less than for the women, *i. e.*, from + 84 to 0 for the men and from + 75 to — 43 for the women. Both have 55 per cent. of the entire range below the median judgment. But the average A.D. of the medians of the individual judgments for each advertisement for women is 69 per cent. greater than for the men. This is the more striking as the women would apparently be a much more homogeneous group than the men as they were all juniors or seniors in Barnard College and within a very few years of each other in age, while the men include graduate students and professors and vary at least twenty years in age. This difference in variability is shown not only in the average A.D., but also in a comparison of the respective average deviations of each advertisement. Table XX. shows that with only seven advertisements out of the fifty is the quartile with the women less than with the men. This is not a result of the less number of female as compared with male subjects for we have only eleven advertisements that show a lower P.E. of the average among the women when we compare the average position that 5 men assign with the similar position of 5 women. A comparison of the average probable errors of the two groups shows us that the P.E. of the women averages 69.7 per cent. greater than that of the men. We have here then three interesting facts in regard to sex-difference in the judgment of advertisements: (1) The absolute range of difference of judgment for the women is 71 per cent. greater than for the men. (2) The women rank 13

[12] It should be borne in mind that the women were all undergraduates and the men were all college graduates and average at least seven years older. It is thus actually a sex difference plus an age difference. However, the results of Chapter VI. show that older women rate soap advertising appeals almost exactly as do college women. Consequently, I do not believe that the question of age has much effect here.

advertisements as negative in appeal while the men do not rank any —the 13 occupying 36 per cent. of the entire range of the women. (3) The variability of the individual judgments of the women is 70 per cent. greater than that of the men.

Why we should obtain apparently conclusive evidence in Chapter VI. that women are less variable in their preference for soap advertising appeals and here, on the other hand, apparently just as conclusive evidence to the contrary is difficult to understand. It is true that the methods employed in the two chapters are different. But if different methods can give exactly opposite results as to variability, they can be of little value as to its determination. Personally, I believe, that the situation is this. The results of Chapter VI. show less variability among women than men. The results of Chapter VII. show that when women are given an equal opportunity with men to rate appeals (advertisements) they are able to classify their dislikes as readily as their preferences, which the men do not do. Such a condition naturally results in a greater total range (where methods of experimentation similar to those in this chapter are used) and consequently in a seemingly greater variability. A careful analysis of the data will not really show greater variability of judgment among the women. What it does show is that women have more and greater dislikes than men and are surer of them.

There are seven advertisements that the women *actually* rank higher than the twenty-five men and women together. They are advertisements Nos. 29, 9, 23, 5, 16, 22, and 17. The first three, however, do not differ by as much as the P.E. of the median of the twenty-five or of the fifteen men. With only the last four can it be said that the women actually rank them higher on the scale than the twenty-five. Only four of these seven do the men actually rank lower than the median of the twenty-five, *i. e.,* Nos. 5, 23, 16, and 22. And only with the first of these four is the difference greater than the P.E. of the median of the twenty-five. The *order* of the twenty-five then does not vary with the exception of No. 5 for the men and Nos. 5, 16, 22, and 17 for the women by more than the P.E. of position from the order of either men or women.

From the data in Table XX. the P.E. of the obtained median from the true median may be obtained. (P.E. median obt. — true = quartile/$\sqrt{n}$, n equaling the number of subjects.) Turning now to Table XXI. we can draw up a schedule stating not in terms of position on the scale but in terms of relative position among the fifty advertisements the probable range of each advertisement. For example, the median position of No. 5 for the fifteen men is 17 and for the ten women is 61. The quartiles, respectively, are 29 and 26. Then the P.E. of the above medians are $29/\sqrt{15}$ and $26/\sqrt{10}$ or 7.5 and 8.2. It is evident then that the chances are even that the true median position of No. 5 for the men lies between 24.5 and 9.5 and for the women between 69.2 and 52.8. Turning now to Table XXI. we find that the forty-first to forty-eighth advertisements in the series for the men lie between these limits, while the second to the fifth advertisements lie for the women similarly between the second set of limits. Consequently we are certain that the difference between the assigned positions of 17 for the men and 61 for the women is greater than the P.E. of their positions.[13] Only such sex-differences are considered here.

By the above method we find that the following advertisements are ranked higher by the men than by the women: Nos. 12, 42, 20, 18, 34, 32, 24, 38, 44, 28, 41, and 36. The following are ranked higher by the women than by the men: Nos. 5, 40, 33, 16, 22, 17, 25, 7, and 27. Four of the above advertisements (Nos. 12, 29, 42, and 5) are ranked above the sixteenth position by the twenty-five subjects, the remainder are ranked below the twenty-second position in the fifty. It is evident that the two sexes nearly agree about the best advertisements but disagree about the poorer ones.

Among the advertisements preferred by the women over the men we have the three "kitten-ads." (Nos. 16, 22, 23); the "baby in the satchel-ad." (No. 5); the "little boy in the cart-ad." (No. 17); the "tired tourist-ad." (No. 7); and the "letter to Santa-Claus-ad." (No. 27). The main point of all these advertisements is their irrelevancy of cut. Among the twelve advertisements preferred by the men over the women only two can be grouped under the heading of irrelevancy—No. 20, a mother and naked child, and No. 36, two children. This preference for the irrelevant among women confirms the early work of Gale upon attention-value. He states that "the female attention was more susceptible to irrelevancy, as it was also to cuts, than was the masculine attention."[14]

[13] Just how great a difference does lie between such respective positions can be determined from the following formula, P.E. of difference between Advertisement A and B =

$$\sqrt{\left(\frac{\text{P. E. t. med.-obt. med. A}}{\sqrt{n}}\right)^2 + \left(\frac{\text{P.E. t. med.-obt. med. B}}{\sqrt{n}}\right)^2}.$$

However, for all practical purposes in this study a difference which is so great that the P.E. of the true-obtained median of one position does not overlap that of the other is sufficient as an indication of an actual difference in merit.

[14] Cf. Chapter I., p. 4.


"Use Packer's Tar Soap for Washing the Hair"
41—42
Packer's Tar Soap
42—13
With Packer's Tar Soap
"YOU CANNOT BEGIN TOO EARLY."
PACKER'S TAR SOAP
43—9
Packer's Tar Soap
Pure as the Pines
"THE MAIN HOPE
IN THE PREVENTION OF PREMATURE BALDNESS
44—37
HIS GREATEST DELIGHT
EVERY SATURDAY NIGHT
PACKER'S TAR SOAP
45—14
PACKER'S
TAR SOAP
PURE AS THE PINES
46—19
In Early Childhood use
Packer's Tar Soap
Pure as the Pines
A LATHER-LUXURY
Children enjoy using Packer's Tar Soap. It quickly removes scurf or dandruff from the head and relieves itching caused by rashes, chafing etc.
47—20
48—6
49—16
Careful Mothers—
the world over—
use
Packer's Tar Soap
50—21

PLATE IX.

Of the advertisements preferred by the men we have the following "copy-ads.": Nos. 12, 42, and 39; and the following half "copy" and half "cut-ads.": Nos. 38, 44, 36, and perhaps 28. Only No. 25 among the advertisements preferred by the women could be considered as approximating a "copy-ad." and there the main interest, apparently small, I should judge, would lie in the three small cuts. Advertisements Nos. 24, 32, and 28 appeal directly to men, for they consist of cuts of men and appeal to the preservation of their hair. But No. 40 preferred by the women is also a direct appeal to men. Advertisements Nos. 34 and 41 are direct appeals to women to use the soap and depict women shampooing or combing the hair. All the other advertisements of this type (*e. g.,* Nos. 33, 35, 37, 8, etc.) were ranked higher by the men than by the women although the differences are not great enough to exceed the P.E. The explanation of this phenomenon lies in the fact that in sorting any material in the "order of merit" method the tendency on the part of nearly all subjects is to arrange the first half of the material upon some criterion and then to arrange the remainder upon the basis of a criterion logically opposite to the former. That is to say, this series of advertisements was arranged with the criterion of "suggestion-to-buy" and also with the criterion of "distaste" or "prejudice-against-buying." The men didn't know whether women's hair could be benefited by the use of Packer's Tar Soap and didn't care. They merely threw all such advertisements down near the bottom of the list but not clear to the bottom. The very bottom was reserved for those advertisements which they were sure were no good. But the women were sure of the merits of such appeals and consequently placed them clear at the bottom. I say "sure" because the majority expressed themselves very forcibly in their introspections that they didn't believe any soap would grow hair. It is interesting to note in this connection that the copy of all these advertisements emphasizes the "efficacy of Packer's Tar Soap as a pure, antiseptic cleanser for shampooing" and "keeping the hair soft, lustrous, and healthy," etc. No where do they make the claim that its use makes the hair grow. But because of the frequent display of women's hair by patent medicine advertisers to-day, the subjects, especially the women, jumped at the conclusion that the same claim was being made here—that the soap would grow hair—and they did not bother to investigate further. Hence their introspections along that line. It is also of interest to note that these particular advertisements have not been used for several years because of this situation—the patent medicine men having ruined this appeal by their gross exaggerations.

8

ATTENTION AND MOVEMENT IN REACTION TIME

BY

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CONTENTS

INTRODUCTION

Any complete sensory reaction may be divided into five parts: (1) The latent period in the sense organ (before the sensory impulse is aroused), (2) the time consumed in the conduction of the impulse from the sense organ to the appropriate sensory (projection) center, (3) the time consumed in cortical elaboration (association), (4) the time consumed in the conduction of the impulse from the motor area in the brain down to the cord and out over the lower neurones to the striate muscle, (5) the latent period in the striate muscle itself.

There are definite, external, objective factors which influence the time of a simple reaction. Some of these external factors are—difference in the quality and intensity of the stimulus, different resistances offered by the keys with which the subject reacts; the position of the body during the reaction, mode and extent of the movement called for by the reaction. Further there are subjective factors, such as the state of attention, fatigue, temperament, habit.

In spite of numerous investigations already devoted to the subject of reaction times, discrepancies of result and differences of opinion still appear, and there is still need for work before the influence of these various factors can be understood. The present study is concerned for the most part with the influences which have been above classed as subjective, and particularly with those classed under the head of attention; but some experiments have been directed towards the objective factors.

Under the head of attention, the problems to be considered are: (1) the different effect on the reaction time of directing attention upon the stimulus and of directing it upon the movement ("sensory and motor reactions"), (2) the effect of attempting artificially to induce the sensory and motor forms of attention, and (3) the change of the reaction time brought about by changing the interval between the "ready" signal and the stimulus, and the possible connection of this change with the so-called wave of attention.

Under the head of objective factors the following points are considered: (1) The resistance of the reagent's key, (2) the advantages of movements of pressure and of release, and (3) the excess of movement involved in the reaction. In connection with the latter study, a new key was devised for measuring excess pressures.

The experiments were performed in the Psychological Laboratory of Columbia University during the years 1908–10, under the direction of Professors Cattell and Woodworth. The writer is also indebted to the late Professor Bergström, formerly of Indiana University, under whom he received his first training in experimental psychology and with whom he became interested in chronometric work. Professor J. B. Watson, of Johns Hopkins University, has read the manuscript and given many valuable suggestions.

CHAPTER I

Direction of Attention

Simple reactions, when they have been made many times, become automatic or reflex in their nature. This view was advanced by Cattell[1] when he gave his analysis in neural terms. James[2] expresses the view that simple reactions are measures of a reflex, the arc for which is a transient result of previous cerebral conditions. It seems that Wundt[3] in his earlier writings was of the opinion that the central processes played an important part, but later considers reactions of the muscular or motor type as brain reflexes due to practise. Finally, Lipps[4] in an elaborate manner shows that the transformation of the sensory into a motor current involves neither conscious perception nor conscious will.

Since this is the case, it follows that reaction time experiments in their present state of development add little to our knowledge of the states of consciousness intervening between the stimulus and the reaction. They are however valuable in helping to analyze the attentive attitude *before the stimulus is given.* This point has been definitely stated in the recent works of Ach[5] and Deuchler.[6] Pyle[7] makes an analysis of the introspections of the expectant attention in reactions. In his discussion he agrees, in the main, with Ach as to the nature of the "foreperiod" of the reaction.

These investigators agree in general that there are two chief directions which attention may take in the foreperiod: the one is toward the stimulus, the other toward the movement. The reactions resulting from the former have been called sensory, while the latter are known as motor reactions. For many subjects there is also a neutral type in which it is hard to tell whether the attention is directed more toward the stimulus or more toward the movement. This has been known as the natural reaction; although for some sub-

[1] *Mind,* 1886, **11**, 232–233.

[2] "Principles of Psychology," 1890, I., 90–97.

[3] Physiol. Psych., 1887, II., 266.

[4] "Grundtatsachen," 179–188.

[5] "Uber die Willenstätigkeit und das Denken," 1906.

[6] "Beiträge zur Erforschung der Reaktionsformen," *Psychol. Stud.,* 1908, **4**, 353.

[7] *Am. Journ. of Psychol.,* 1909, **20**, 545.

jects the natural reaction would be of the sensory type while in others it may be of the motor form.

These distinctions were first brought into prominence by Lange[8] in 1888. Since then many others have found the same kind of differences, but they vary much as to the extent of the differences. Some of their results are shown in the following table:

TABLE I

Observer	Nature of Stimulus	Sensory Time	Motor Time	Difference	No. of Subjects
Lange[9]	Auditory	227	123	104	3
Lange	Visual	290	113	177	2
Lange	Tactile	213	108	105	1
Dwelshauvers[10]	Auditory	279	137	142	3
Titchener[11]	Visual	268	181	87	3
Martius[12]	Auditory	161	141	40	Several
Martius	Auditory	242	172	70	1 Unpractised
Martius	Auditory	179	130	49	1 Unpractised
Münsterberg[13]	Auditory	162	120	42	1
Della Valle[14]	Auditory	160	125	35	Several
Bergemann[15]	Auditory	120	100	20	2
Angell[16]	Auditory	133	127	6	1
Angell	Auditory	132	134	— 2	1
Angell	Auditory	173	159	14	1
Cattell[17]	Auditory	105.9	105.4	0.5	1
Cattell	Electric	142.7	142.8	— 0.1	1
Cattell	Auditory	105.6	108.8	— 3.2	1
Cattell	Electric	201.6	281.4	— 79.8	1

It will be observed that there is much variation in the difference between the motor and sensory reactions.

It was the purpose of the following experiment: (1) to collect again some carefully made reactions, both sensory and motor, under such conditions as to throw light on their proper relations; (2) to

[8] *Phil. Stud.*, 1888, **4**, 479.

[9] Lange, *Phil. Stud.*, 1888, **4**, 479.

[10] Dwelshauvers, *ibid.*, 1890, **6**, 217.

[11] Titchener, *ibid.*, 1893, **8**, 138.

[12] Martius, *ibid.*, 1890, **6**, 167.

[13] Münsterberg, *Beiträge zur experimentellen Psychologie*, 1889, **1**, 74.

[14] Della Valle, *Psychol. Stud.*, 1907, **3**, 294. The data here given were obtained in a study of the attention wave, and with various intervals between the preliminary signal and the stimulus; and I have selected the reaction times with intervals of from three to four seconds.

[15] Bergemann, *ibid.*, 1906, **1**, 179. The reaction times are reported in the form of curves of frequency of occurrence.

[16] Angell and Moore, *Psychol. Rev.*, 1896, **3**, 245. Reports are also given here of reactions to visual stimuli.

[17] Cattell, *Phil. Stud.*, 1893, **8**, 403.

take one or two subjects well trained in psychological experimentation through a series of introspective studies in order to ascertain, if possible, more of the cause and the nature of the motor and sensory reactions.

The apparatus for the first part of the experiment consisted in a pendulum chronoscope of the Bergström type with its accessories, as described by its inventor.[19] Though the readings of this instrument were checked up by means of a control hammer, as well as against the Hipp chronoscope, yet it appeared that shorter reaction times were obtained by use of the pendulum chronoscope than by use of the Hipp; and Professor Cattell desires it to be stated that he suspects the existence of some constant error in the readings of my chronoscope and therefore in all the records that are to follow—a constant error which, because constant, would not invalidate the comparisons on which the conclusions of the present study are based.

The auditory stimulus was given by means of a spring sound hammer to which was fastened a scale so that the hammer could be drawn to a uniform tension for each stroke.

The subjects all had some preliminary experience in reactions. In eight of them this experience consisted only of about 100 practise reactions. After this practise the difference between the motor and sensory type of reactions was explained, and the subjects were asked to assume voluntarily the respective attitudes. The instructions were made as nearly uniform as possible. With one exception each subject gave 100 sensory and 100 motor reactions. These were taken in groups of 25 motor and 25 sensory, alternating, until the total 200 were recorded. All reactions throughout this paper are recorded in thousandths of a second (σ). Since the chronoscope scale read only

TABLE II

	Number of Reactions	Sensory	M. V. Sensory	Motor	M. V. Motor	Sensory minus Motor
Hw.	100	114.6	10.3	101	8	13.6
Re.	100	135.4	13.7	111.7	10.9	23.7
Bn.	100	161.1	16	138.3	9.2	22.8
Et.	100	147.7	17.1	103.7	9.2	44
Pf.	100	102.8	7.6	92	7.2	10.8
Kl.	100	125.3	11	116.4	9.3	8.9
Hy.	100	114.5	10.2	103	9.1	11.5
Ca.	100	133.5	14	117.4	11.4	16
Ca.	100	135.4	13	121.4	12.6	14
Py.	50	145	7.9	125	10.1	20
Ds.	100	114.3	9.2	102	6.2	12.3

Average excess of the sensory over the motor reaction time, 18σ.

[19] "Pendulum Chronoscope and Accessories for Psychological Experimentation," *Psychol. Rev.*, 1910, **17**, 1.

to five-hundredths, it was necessary at times to estimate the third place digit. This could be done with a fair degree of accuracy. The chronoscope was checked and corrected by means of a falling screen in connection with the same wires and attachments used in the experiment.

The results of this experiment are given in Table II. Each subject shows, on the average, a longer reaction time for the sensory type of preparation than for the motor. The differences are, however, not large, and much overlapping occurs between the times for the two types, as is evident from the mean variations.

Two subjects were next chosen to undertake a new series of reactions for the purpose of introspective study. These two subjects, having had considerable experience in the psychological laboratory, could be depended on to understand the questions asked and to furnish reliable data. So far as the experimenter's observation can be trusted, the two men differed greatly in temperament.

The procedure in this case was as follows: A preparatory signal was given, consisting of the word "ready"; at a varying interval after this signal, the stimulus was given with the sound hammer. The subject was instructed to react as quickly as possible, but not to assume any predetermined attitude. It was hoped that both sensory and motor types of reaction would appear spontaneously, and be distinguished by the subject's introspection. One hundred reactions were thus made at a single sitting; it was thought best to concentrate the experiment within one sitting in order to avoid the variations in reaction time that occur from day to day.

The introspective observations were obtained in reply to the following set of questions, which were put to the subject after each reaction:

1. Was your reaction sensory (*s*) or motor (*m*)?
2. In your judgment was it longer (+) or shorter (—) than the previous one?
3. Do you think you can react faster (+) or not (—)?
4. Did your attention to the signal and stimulus seem to be steady (*a*), of steadily increasing force (*b*), pulse-like (*c*), wave-like (*d*), or gradually diminishing (*e*)?
5. Was the stimulus given at the most opportune time (+) or not (—)?
6. Do you want a longer (+), shorter (—), or same (0), interval between the ready signal and the stimulus?
7. Any other introspection of interest, and what would you suggest that would increase your efficiency?
8. Four degrees of confidence in your judgments, *a, b, c, d*.

The questions were answered by means of certain previously agreed upon marks or signs shown on the list. This enabled us to record the replies rapidly. Some of the questions served merely as checks upon others. Thus number 6 merely confirms or denies the reliability of the answer to number 5, and numbers 5 and 6 have a very direct bearing on number 3.

Some of the introspections thus obtained are given here and others will be referred to in the following discussion of the results.

Both subjects noted a strong tendency for their attention to shift back and forth in the period between the signal and the stimulus. This fact was also observed by many of the subjects who took part in the later experiments on fluctuation of attention.

One of the two subjects classified nearly all of this group of reactions as either motor or sensory; while the other left 49 unclassified as over against 44 classified. Their classification was as follows:

First subject: Motor 88, Sensory 86, Unclassified 5.
Second subject: Motor 20, Sensory 24, Unclassified 49.

The quickest fifty reactions were selected; of these thirty-three had been classified by the subjects as motor reactions. Seventeen of these fastest reactions were of the sensory and mixed types.

The judgments (in answer to question 2) of the length of the reaction in comparison to the one just preceding came out as follows:

First subject:	Those judged shorter 71 per cent. correct.
	Those judged longer 72.5 per cent. correct.
Second subject:	Those judged shorter 80 per cent. correct.
	Those judged longer 76 per cent. correct.

In the general introspections, nearly all the subjects reported more of a strained feeling during the motor reactions than during the sensory.

Discussion of Results

From the summary in Table I. of previous results on this problem, it is evident that great differences exist both in the absolute time reported for the simple reaction, and as regards the direction and the extent of the difference between the sensory and motor forms. The results vary from a moderate difference in favor of the sensory form to a very decided advantage for the motor.

As will be seen in Table II., my own results showed, in every individual, a faster reaction when attention was directed toward the movement.[20]

[20] Only once, in my experience in laboratory work, have I found a student that reacted more quickly in the sensory than in the motor form. This occurred before I was keeping data on this particular problem and I regret that I can not report the actual reaction times. For all my subjects, the difference between motor and sensory reaction times ranges from 10 to 30σ, averaging 18σ.

From the further introspections of the two experienced subjects, it would appear that various states of attention are possible. The subjects distinguished two or three different motor attitudes and perhaps as many or more of the sensory. It is probable that if certain subjects were highly trained in giving introspections, they could divide their reactions into many minute sub-classes. The distinction between sensory and motor was, however, more marked than any other division that could be made.

Where the two types are not apparent to the subject, they can soon be made so by briefly defining them. As has been said, most of the subjects recognized the two types before they were told of the distinction and felt that they could consistently react in one or the other way if they voluntarily undertook to do so. A further discussion and analysis of this difference in the motor and sensory form will be given in Chapter II.

We may briefly summarize the results so far obtained as follows:

1. The motor and sensory attitudes in reactions are easily distinguished by most individuals.

2. It is possible to assume voluntarily either attitude and hold it throughout the reaction experiment.

3. For all individuals tested for this research, the sensory reactions were longer than the motor by from 10 to 30 averaging 18σ.

4. If the subjects were allowed to react without voluntarily assuming either attitude it was found that they could classify their responses under the headings of normal, sensory and motor; and that the times of reaction under these conditions differed in the same way as when the three attitudes were voluntarily assumed.

5. Subjects are able introspectively to give good estimates of the relative lengths of their reaction times, and this fact affords some reassurance as to the validity of their other introspective reports in this experiment.

6. A stronger feeling of effort and strain attends the motor than the sensory reaction.

CHAPTER II

Attempts at Artificial Direction of Attention

The customary method of obtaining sensory or motor reactions is to define the kind of attention desired in an experiment and depend on the subject's report as to how nearly this has been attained. There is no good objective indication of the mind's attitude. Partial indications are indeed given by the way the head is turned, the fixation of the eyes, and certain other movements, showing to what the subject is attempting to attend. Thus, in all my experiments when the subjects were told to attend to the stimulus, they would look in the direction whence the stimulus came, although they could not see the apparatus. When told to attend to the movement, their gaze would most likely be turned to the hand and the reacting key, though several of my subjects closed or nearly closed their eyes. These external indications, however, can not be depended on to the extent desired in reliable experimental work.

A further complication lies in the tendency, reported by my subjects, to shift the attention back and forth between the movement and the stimulus. This was especially true when the interval between the ready signal and the stimulus was prolonged beyond what the subject expected. As was reported in the previous chapter, the motor attitude, for nearly all the subjects, was accompanied with considerable feeling of strain; at least, this was true when they were able to report that the motor attitude was definitely taken. If effort is required to make the motor type of reaction, the subject is at times likely to relax and give a response approaching the neutral type. After some practise, no doubt, the motor form would become easier and it is also reasonable to suppose that if a subject be strictly of the motor type, the sensory attitude will require the more effort. One subject, Th., reported no difference in the amount of effort required to react in the two forms.[1]

Since attitudes may be so easily changed, the introduction of any external factor may make much difference in the responses. Experiments in binocular rivalry[2] show that a very slight variation in the

[1] He is one of the subjects, however, who noticed the distinction between motor and sensory reactions of themselves and before it had been pointed out to them. Not much difference appears between his motor and sensory times.

[2] Helmholtz, "Physiologische Optik," 2d ed., 1896, pp. 915 ff.; Breese, *Psychol. Rev.* Monogr. Suppl., No. 11, 1899.

field of vision for one eye causes that field to predominate. Reversible perspective figures show the same effect when different parts of the drawing are attended to. Here it is a case of conflicting visual impressions. In the cases described in this paper, the shifting takes place between the motor and sensory kinds of reaction. If, therefore, the conditions of the experiment can be made to favor the sensory or the motor type of attention, we should expect the corresponding form of reaction to take place. The following experiment was undertaken with the object of directing the attention by external means.

The control was exerted as follows: To induce the motor type of attention, the resistance of the key was varied from reaction to reaction, and the subject was required after each reaction to judge whether the resistance encountered in that reaction was greater or less than that encountered in the preceding. Since the time of reaction is affected by the resistance of the key, as is shown in another chapter, I used a difference in resistance of only 25 grams, which as was shown by the experiment is too small to influence the time of reaction appreciably.[3] The subject was instructed to react as quickly as possible and to make his judgment of the resistance after the reaction, from his memory impression. To induce the sensory type of attention, the auditory stimulus was varied in clang character by the use of two sound hammers, one of which struck on a heavy metal base and the other on a small piece of metal mounted on wood. The intensity of these two stimuli was, as nearly as could be estimated, about the same. It was necessary to avoid any large difference in the intensity, because of the fact that differences in the intensity affect the reaction time. Upon this point, Dolley and Cattell[4] say that: "The reaction time becomes shorter as the intensity of the stimulus is increased, but the difference in the time is small so long as the stimuli are moderately strong." The stimuli given in this test were all moderately strong, therefore any slight variation in their intensity would have but a slight effect on the reaction times. The subject was required to report which of the two sound stimuli he had

[3] It was necessary, in this experiment, to use the downward pressure, or the positive movement, in the reactions, since with the release movement the subject would have been able to judge the resistance of the key beforehand, from the pressure exerted in closing it, and his mind would, therefore, be free at the moment of reaction to attend in any direction, the thing which it was hoped to avoid.

[4] "On Reaction Time and the Velocity of the Nervous Impulse," Nat. Acad. of Sci., 1893; see also, Sven Froeberg, "The Relation between the Magnitude of the Stimulus and the Time of Reaction," *Archives of Psych.*, 1907, No. 8.

received in each reaction, and thus his attention was directed to the stimulus.

Besides these "induced sensory and motor" reactions, the experiment included "voluntary sensory and motor" reactions of the usual sort, and "normal" reactions, in which the subject was simply instructed to react as quickly as possible without considering the direction of attention.

The order of our program as worked out was: (1) 50 normal reactions, (2) 25 voluntary sensory reactions, (3) 25 voluntary motor reactions, (4) 25 induced sensory reactions, (5) 50 induced motor reactions, (6) 25 induced sensory reactions, (7) 25 voluntary motor reactions, (8) 25 voluntary sensory reactions, (9) 50 normal reactions. The repetition in the order mentioned was intended to compensate for the practise effects. The entire program was carried out at two sittings with the exception of one subject.

The results are shown in Table III.

TABLE III

Subjects	Normal	Voluntary Sensory	Voluntary Motor	Induced-Sensory	Induced-Motor
Da.	108.1	112.2	93	110.7	112.8
	10	*9*	*11*	*11*	*9*
Hw.	91.2	109	94.4	103	128.7
	10	*12*	*11*	*12*	*12*
Pf.	95.4	104.6	80.3	93.1	117
	10	*10*	*8*	*7*	*12*
El.	124	147	102.2	147.6	157.1
	12	*15*	*10*	*13*	*14*
No. of reactions	(100)	(50)	(50)	(50)	(50)

This experiment was followed by another in which the subject was asked to pass judgment on both the stimulus and the movement of the same reaction. Voluntary motor and sensory reactions were also included. The order of the program was the same as in Table III., except that the normal reactions were omitted.

TABLE IV

Subjects	Voluntary sensory reactions	Voluntary motor reactions	Reactions with both sensory and motor discriminations
Da.	112.2	93	161
	11	*10*	*14*
Br.	129	107	133
	13	*9*	*19*
Rc.	135.4	111.7	185
	12	*10*	*15*
Th.	149	143	185.6
	13	*10*	*23*
No. of reactions	(50)	(50)	(50)

From these tables it appears, by comparing the induced sensory with the voluntary sensory reaction, that the two times are nearly the same. We may conclude that the artificially induced sensory attitude was practically the same as the voluntary sensory attitude. On comparing the induced motor with the voluntary motor columns, however, the divergence is striking. Evidently, what was induced by requiring the subject to attend to the resistance of the key was something quite different from the voluntary "motor" attitude. It was, in fact, as far as concerns its effect on reaction time, nearly equivalent to the sensory attitude.

The following inferences seem to be justified from this experiment,[5] in connection with those reported in the preceding chapter.

1. As to the motor reaction. The so-called motor attitude is not perfectly and unambiguously described as an attitude of attention to the movement. It is not an attitude of preparation to observe the movement. When the movement has to be observed, as in our "induced motor" reactions, the time is much longer than in the usual motor type. Our subjects reported that the "induced motor" attitude was introspectively different from the voluntary motor, and that it was more complicated, though in what the complication consisted their introspection did not reveal. It is a fair inference from the conditions of the experiment that the complication was equivalent to a preliminary adjustment to observe the movement, as well as to make it. The usual motor attitude consists solely in a preparation to make the movement, not to observe it as it occurs. Therefore the distinction between sensory and motor reactions is not unambiguously indicated by these names, which seem to imply that the attention is in the latter directed to the movement in the same way as it is directed to the stimulus in the former. Compared with the "induced motor" attitude, the voluntary motor attitude is less complicated, more single-minded, and consists simply in a *preparation to react.* As compared with the "normal" or "natural" attitude, the voluntary motor attitude is characterized by a greater feeling of effort and by quicker reaction times, and may probably be regarded as the natural attitude keyed up to a higher pitch.

2. As to the sensory reaction. Nothing is more probable than that the sensory attitude has not always been the same thing. It has differed in different subjects, and in the hands of different experimenters. Some experimenters have regularly obtained large differences in the time of the sensory and motor reactions, where others

[5] See Angell and Moore, "Reaction Time," *Psychol. Rev.*, 1896, **3**, 245–258. Ach, "Ueber die Willenstätigkeit und das Denken," 1906.

have obtained only small differences. The divergences between investigators can scarcely depend on the individualities of the subjects on whom they worked; for it would be a strange coincidence if only one class of subjects happened to come to the hands of one experimenter, and another class to the hands of another. It is practically certain that the attitudes induced by different experimenters, under the name of the sensory reaction, have differed. This difference need not have been due to the formal instructions, but the experimenter's own attitude and expectations may have been subtly imparted to his subjects. An exact definition of the sensory attitude can not therefore be expected, but only an indication of some scale along which the attitude can vary. Our results with the "induced" sensory and motor reactions, and with reactions when judgments of both stimulus and movement were required, point to the existence of one such scale—namely the scale of preparation to observe. Observation is something additional to reaction, and the amount of observation required, and so of the preparation to observe, can vary from zero up to an unlimited degree. In the induced motor reactions, no less than in the induced sensory, some preparation to observe is demanded, and the speed of the reaction is slackened; while in the reactions which called for observation of both stimulus and movement the speed is still further reduced. Now the voluntary sensory reaction does not differ in time from the induced sensory, and seems to be essentially the same thing. Our results, therefore, suggest that, whereas the motor attitude is a single-minded preparation to react, the sensory attitude is a preparation to observe as well as to react, and that this preparation to observe may interfere in different degrees with the preparation to react, according to the amount of energy which is diverted from the preparation to react into the preparation to observe. In other words, the distribution of attention, or of preparatory tension or innervation, varies in an uncontrolled way in the sensory type of reaction.[6]

These results and considerations suggest a different explanation of the long time of the sensory reaction from that given by Wundt. It is not necessary to suppose, with Wundt, that the perception of the stimulus, in the sensory reaction, precedes the initiation of the motor impulse. There is no guarantee that the movement is held back till the stimulus is perceived, or that the movement is really a response to the percept. In the case of our "induced motor" reaction, the movement must precede the percept, since it is the movement which

[6] These conclusions are very similar to those drawn by Ach (*op. cit.*) from quite other evidence.

is to be perceived, and yet the slackening of reaction is practically the same as in the sensory type. The probability is that *the preparation to observe interferes with the preparation to react,* by complicating the total preliminary adjustment, and diffusing the available energy over a greater area. The motor type of reaction accordingly provides an index of the maximum efficiency of the single adjustment to react; and the difference between the motor and the sensory reaction times indicates the degree to which additional adjustments have interfered with this main adjustment.

CHAPTER III

The Attention Wave

Fluctuations of attention have usually been investigated by Lange's method of minimal stimuli.[1] The consideration underlying this method is evidently that a stimulus which lies just above the threshold should disappear with the least wavering of attention; and accordingly records of the appearance and disappearance of minimal stimuli have been accepted as records of the wave of attention. Titchener,[2] in discussing this point, reports that Ferree and Geissler failed to find fluctuations in liminal pressure stimuli or with electro-cutaneous stimulations. This conclusion is opposed to that of Lehmann[3] and Wiersma.[4] In the same way, we have contradictory reports concerning auditory stimuli in the work of Heinrich and of Dunlap. Many others[5] have performed experiments on this problem with varying results.

[1] N. Lange, *Wundt's Philos. Stud.*, 1887, **4**, 395.

[2] "The Elementary Psychology of Feeling and Attention," p. 267.

[3] "Ueber die Beziehung zwischen Athmung und Aufmerksamkeit," *Phil. Stud.*, 1894, **9**, 66.

[4] "Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen, *Zeitschr. f. Psychol.*, 1901, **26**, 168.

[5] Stevens, H. C., "The Relation of the Fluctuations of Judgments in the Estimation of Time Intervals to Vaso-motor Waves," *Amer. Jour. of Psychol.*, 1902, **13**, 1.

Pillsbury, W. B., "Attention Waves as a Means of Measuring Fatigue," *Amer. Jour. of Psychol.*, 1903, **14**, 541.

Galloway, "The Effect of Stimuli upon the Length of the Traube-Hering Waves," *Amer. Jour. of Psychol.*, 1904, **15**, 495 ff.

Killen, B., "The Effects of Closing the Eyes upon the Fluctuations of Attention," *Amer. Jour. of Psychol.*, 1904, **15**, 512.

Jackson, G. H., "The Telephone and Attention Waves," *Jour. Phil., Psychol. and Sci. Meth.*, 1906, **3**, 602.

Bonser, F. G., "A Study of the Relation between Mental Activity and Vaso-motor Changes," *Psychol. Rev.*, 1906, **10**, 57.

Ferree, C. E., "An Experimental Examination of the Phenomenon usually Attributed to Fluctuation of Attention," *Amer. Jour. of Psychol.*, 1906, **17**, 84.

Slaughter, J. W., "The Fluctuations of the Attention in some of their Psychological Relations," *Amer. Jour. of Psychol.*, 1901, **12**, 313.

Taylor, R. W., "The Effect of Certain Stimuli upon the Attention Wave," *Amer. Jour. of Psychol.*, 1895, **12**, 335.

Seashore, C. E., "Die Aufmerksamkeitsschwankungen," *Zeit. f. Psych.*, 1902, **30**, 448.

The principal facts obtained from all these experiments are:

1. Attention does not remain the same for any considerable length of time.

2. The fluctuation in time is related roughly, at least, to certain respiratory and vaso-motor changes.

3. The waves are easily lengthened and shortened.

4. There is more evidence of the existence of these regular fluctuations for visual than for auditory and tactile stimuli.

5. The length of the waves varies with the general bodily conditions and is effected by fatigue, general vitality, and illness.

A brief report may here be made of a preliminary experiment,[6] undertaken for the purpose of gaining first-hand acquaintance with the phenomena of fluctuation, as they appear with the use of minimal stimuli. The problem also included a study of the relation of fluctuations of attention to such bodily activities as breathing and circulation, and especially to the Traube-Hering waves in plethysmographic tracings.

The plan of the experiment was as follows: A Masson disc was constructed by taking a white disc, a little over eight inches in diameter, and mounting along one radius small black squares, with spaces of one millimeter between them. The disc, placed on an electric color wheel, was rotated at a speed of approximately two thousand revolutions per minute. Each black square then produced a faint ring of gray on the surface of the disc. The rings became fainter towards the circumference of the disc, and the outer ones were never distinguishable.

The observer was seated about two meters from the rotating disc. His hand was placed on a bulb connected with a recording needle that marked on the slowly revolving drum of a kymograph. He pressed the bulb when he saw the greatest number of rings, and released it as the rings disappeared. The high points on the curve thus

Eckener, Hugo, ''Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize,'' *Phil. Stud.*, 1893, **8**, 343.

Hammer, Bertil, ''Zur experimentellen Kritik der Theorie der Aufmerksamkeitsschwankungen,'' *Zeit. f. Psychol.*, 1904, **37**, 363.

Marbe, Karl, ''Die Schwankungen der Gesichtsempfindungen,'' *Phil. Stud.*, 1893, **8**, 615.

Pace, Edward, ''Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit der Massons'schen Scheibe,'' *Phil. Stud.*, 1893, **8**, 388.

Important general references are: Pillsbury, W. B., ''Attention''; Titchener, E. B., ''The Elementary Psychology of Feeling and Attention''; and Arnold, Felix, ''Attention and Interest.''

[6] This experiment was made in the psychological laboratory of the University of Indiana.

TABLE V

TABLE SHOWING RELATIVE PROPORTION OF BLACK IN EACH SUCCESSIVE RING GOING FROM THE CENTER TO THE CIRCUMFERENCE

First ring	.0398
Second ring	.0265
Third ring	.0198
Fourth ring	.0159
Fifth ring	.0132
Sixth ring	.0113
Seventh ring	.0099
Eighth ring	.0088
Ninth ring	.0079
Tenth ring	.0072

indicated when the most lines were visible, and the low points when the fewest could be seen. Pneumographic and plethysmographic tracings were simultaneously recorded. A Verdin time marker traced a time line in fifths of a second.

To make the results as free as possible from complications, the whole apparatus was set up in a dark room which was also comparatively sound proof. Care was taken to have the light approximately uniform, although no actual photometric measurements were made. When distracting influences were present, such as the noise of an electric buzzer, the tinkling of a bell, or talking among those present, four or five rings could be seen. When the observer concentrated his attention while he was feeling fresh, eight and nine rings were plainly visible. Two observers observed these fluctuations plainly, while a third was not so certain that he got them. The latter did not try for very long periods.

The waves thus obtained slightly exceeded in length those reported in most experiments. They were rather uniform and about ten seconds in length. They corresponded in time, at least, to the Traube-Hering wave found in the corresponding circulatory tracing. Our knowledge of the conditions of the sense organs is not adequate to clear up the cause of the fluctuations, *i. e.*, we can not exclude the possibility that they are of purely peripheral origin.

In all cases where minimal stimulations have been used, we have had to be dependent largely on the mere report of the observer. It seems probable that, in experiments with minimal stimuli, the factors of preconceived ideas and suggestion might have much influence on the result. As a rule those working with faint stimulations and similar test material of subtle character, and depending wholly upon the introspective reports of their subjects, have obtained a great variation in their results. A more objective method of studying periodic variations in the efficiency of attention would certainly be desirable, and the reaction time method seems well adapted for such a purpose.

Before coming to that, however, mention should be made of the method adopted by O. Klemm[7] whose experiment bears some resemblance to those conducted by the method of minimal stimuli, but differs from them in this important respect, that there was an objective check on the reliability of the subject's introspections. Klemm used a method of least noticeable differences. A visual, auditory, or tactile stimulus was continued for fifteen or thirty seconds, and at some time during its continuance a small variation was introduced, which the subject was to try to detect. The question was, whether the accuracy of observation would vary according to the interval between the commencement of the stimulus and the change which was to be detected. The results show a variation, which is interpreted as meaning that one is prone to break a task up into a rhythmic form, which differs according to the length and character of the work or task in hand.

That the reaction time method may be available for testing fluctuations of attention is indicated by the following facts: Ebbinghaus[8] reports that reaction times of one fourth to one sixth seconds are shortened by one twentieth of a second by the power of attention. Cattell[9] reports that he was able to distinguish introspectively three degrees of attention, and that he found a slight lengthening and shortening of reactions corresponding to them. Obersteiner[10] used reaction time as a measure of attention. Finally, Sharp[11] comes to the conclusion that the reaction time of a person might well be taken as an index of his attention.

In a reaction time experiment, the attention is strongly excited by the ready signal: if then a considerable interval follows before the stimulus, there is a chance that the attention may fluctuate; if so, the readiness for the stimulus would vary and the reaction time would vary according to the interval elapsing between the ready signal and the stimulus. This was the basis of the method adopted in the following experiment.

Since this work was begun, the paper of Della Valle[12] has ap-

[7] "Untersuchung über den Verlauf der Aufmerksamkeit bei einfachen und mehrfachen Reizen," *Psychol. Stud.*, 1908, **4**, 283.

[8] "Grundzüge der Psychologie," 1902, p. 589.

[9] *Mind*, 1886, **11**, 242.

[10] *Brain*, 1879, **1**, 441.

[11] *Amer. Jour. of Psychol.*, 1899, **10**, 16.

[12] *Psychol. Stud.*, 1907, **3**, 294–298.

peared, in which essentially the same method is used for the same purpose. In his experiment, he used intervals varying from one to ten seconds, and with twelve individuals found the sensory and motor reaction times for each interval. His results show fluctuations of remarkable regularity and uniformity in both motor and sensory types of reaction.

What I hoped to obtain from the reaction time method in studying fluctuations of attention was: (1) To secure an objective record, free from some of the uncertainties of unchecked introspection, and (2) to exclude the possibility of variations in the condition of the sense organ and to observe whether changes occur in the central processes. The stimuli used were well above the threshold, consequently slight fluctuations in their intensity, or in their effects on the sense organ, could hardly make much difference in the reaction time, because, as has been shown, even considerable changes in the intensity of a sound stimulus make only a slight change in the reaction time.[13]

The problem, then, specifically becomes *a study of the state of attention in the interval between the ready signal and the stimulus for reaction;* the stimulus coming at various intervals of from one to ten seconds after the ready signal.

The apparatus was the same as has been described in Chapter I., *i. e.,* a Bergström-type pendulum chronoscope. The chronoscope was checked by a Cattell drop screen. A mechanical sound hammer was at first used to give the stimulus; later an electric sound hammer was substituted. The length of the intervals was gauged by means of a watch.

The procedure was as follows: The subject seated himself in a comfortable position and waited for an auditory stimulus, the nature of which had been made known to him beforehand. He was instructed to concentrate his attention on the work in hand upon the hearing of the ready signal. After a measured interval, the stimulus was given and the reaction time noted. This measured interval was varied from one to ten seconds. The subject never knew, with the exception of the records in Tables XIV. and XV., which interval to expect. Ten readings were taken for each interval, but the intervals were given in a promiscuous order. The tables show averages for ten reactions after the respective intervals. Occasionally, some distracting influence or some other factor would cause one reaction to differ greatly from the others. When one of these abnormal reactions occurred, it was not recorded. There were, however, very few dis-

[13] Dolley and Cattell, "On Reaction Time and the Velocity of the Nervous Impulse," *Nat. Acad. of Sciences,* 1893, 7, 393.

carded records. Reports from the subjects were continually kept as to the feeling of attention, the direction of attention, etc.

The following table shows the averages of ten "normal" reactions for each interval for eighteen subjects. After careful trial the "normal" attitude was found to be the most reliable. The mean variation for each ten reactions is given in italics just beneath the average.

The discussion of these results will be facilitated by restating

TABLE VI

Individuals examined	Intervals in Seconds 1	2	3	4	5	6	7	8	9	10	Average for each Individual
A	124	144.4	135	124	142.2	148	122.3	139.5	122	142	134.3
	31	*43*	*30*	*21*	*20*	*22*	*30*	*30*	*12*	*11*	
B	136	133	131	159	169	190	169	165	180	187	162
	13	*12*	*5*	*7*	*21*	*6*	*19*	*15*	*14*	*15*	
C	115.3	118.8	144.3	163	151	113	113.4	121.8	140	142	132.2
	41	*15*	*41*	*5*	*31*	*20*	*14*	*39*	*57*	*2*	
D	144.5	130.5	140	125	130.5	129.5	115.5	112	116.5	110	125.4
	30	*8*	*18*	*14*	*16*	*22*	*15*	*10*	*7*	*12*	
E	162.5	146.5	148.3	153	165	156	178.2	185	181.5	177.5	165.3
	20	*16*	*17*	*22*	*27*	*27*	*25*	*18*	*27*	*34*	
F	166	161.5	145	133	129.8	139.3	152.5	143	134.5	133	143.7
	16	*13*	*20*	*11*	*16*	*20*	*22*	*16*	*16*	*10*	
G	163	172	148	157	165	157	167	165	171	170	163.5
	11	*6*	*21*	*12*	*8*	*23*	*13*	*14*	*25*	*18*	
H	102.7	110.1	125.3	125.5	149.7	143.7	128.8	125.2	126.5	140.9	127.8
	12	*18*	*13*	*21*	*30*	*20*	*22*	*11*	*21*	*28*	
I	114	125	144.4	122.2	117	134	135	134	141.7	121	128.8
	17	*17*	*14*	*18*	*9*	*17*	*13*	*8*	*26*	*11*	
J	171	156.5	133.5	156.5	141	135.5	127.5	160	164.5	143	148.9
	27	*18*	*7*	*19*	*18*	*7*	*23*	*9*	*14*	*18*	
K	142	133.5	126.3	129.3	140.5	137	139.5	141.1	154	142.8	138.5
	7	*6*	*12*	*15*	*9*	*10*	*19*	*16*	*24*	*21*	
L	159.2	155.5	148.3	161.1	140.4	153.4	144.5	138.3	143.1	127.6	147.1
	22	*14*	*11*	*17*	*16*	*20*	*10*	*14*	*12*	*11*	
M	147.5	153.7	148.2	147.5	169	162.5	162.5	163.1	165.6	159	157.8
	19	*19*	*12*	*15*	*10*	*12*	*14*	*12*	*9*	*13*	
N	137.3	108	118.9	104.1	109.7	114.3	105.1	108.2	103.1	104.1	111.2
	30	*17*	*11*	*9*	*12*	*11*	*8*	*12*	*14*	*12*	
O	87	85.4	88.4	89.2	82.4	85	89.5	87	84	95	87.2
	9	*9*	*8*	*7*	*10*	*9*	*12*	*11*	*7*	*6*	
P	93.2	81.8	89	93.7	85.2	101.6	89.9	85.8	95.5	94	91
	9	*12*	*15*	*16*	*14*	*13*	*18*	*13*	*18*	*18*	
Q	128.4	128.2	122.8	136.7	128.4	133	123	134.3	127.6	127.6	129
	31	*10*	*10*	*11*	*14*	*20*	*11*	*12*	*15*	*10*	
R	112	109	110.6	111.3	107.1	106.5	106.3	110	108	108.9	108.9
	12	*10*	*16*	*12*	*14*	*11*	*13*	*13*	*11*	*12*	
Averages for each interval	133	130	132	132	134	134	131	134	136	134	**133.5**

TABLE VII

Individuals examined	Interval 1	2	3	4	5	6	7	8	9	10
A	92	108	100	92	106	110	91	104	91	106
B	84	83	81	99	104	117	104	102	111	115
C	87	90	109	123	114	86	86	92	106	107
D	115	104	112	100	104	103	92	89	93	88
E	99	89	89	92	100	94	108	112	110	107
F	115	111	101	93	90	97	106	100	94	93
G	100	105	90	97	101	97	102	101	104	103
H	80	86	98	98	117	113	101	98	99	110
I	87	97	112	95	92	104	105	104	110	94
J	114	105	89	105	94	91	87	108	111	96
K	103	96	91	93	101	99	101	102	111	103
L	108	105	101	109	104	104	91	94	97	87
M	93	97	94	93	107	103	103	103	105	102
N	123	97	107	94	99	102	94	97	93	94
O	100	98	101	102	95	96	103	100	96	109
P	102	90	98	103	94	112	99	94	105	103
Q	100	99	95	106	100	103	95	104	99	99
R	103	100	101	102	99	97	97	101	100	100
Av	100.3	97.8	98.3	99.8	101.2	101.6	98.1	100.3	101.9	100.9
M. V.	*9*	*6*	*6*	*6*	*5*	*6*	*6*	*4.5*	*6*	*6*

them in the form shown in Table VII. Here the individual's average time for each interval is shown in per cent. of his average for all intervals. The absolute times are, then, discarded, in favor of the relative times for the different intervals. The advantage of this derived table is, first, that it permits of a more ready examination for fluctuations, since each individual's general average is now reduced to 100, so that any entry less than 100 indicates a relatively quick reaction, and any entry greater than 100 indicates a relatively slow reaction. In the second place, the derived table permits of a fairer averaging of the results from all the individuals, since it eliminates the absolute speed of each individual, and retains only the changes from interval to interval. In the original table, the general averages for the successive intervals are disproportionately determined by the slower individuals and their large absolute variations; whereas, in the derived table, the quick individual has the same weight as the slow.

In either the original or the derived table, the influence of the interval is seen to be slight, when the average of all the subjects is considered. The reaction time after one interval differs but little from the reaction time after another interval. The mean variation of the eighteen individuals for each interval is so large as to make the differences between the intervals rather unreliable. Yet the chances are that some of the differences indicated are genuine.

There can be little doubt, from the data, that an interval of two or three seconds, between the ready signal and the stimulus, is more favorable for a quick reaction than an interval of one second, or than an interval of over four seconds. The favorable appearance created by the low average for the 7-second interval is less reliable, because of its isolation.

When we turn from the general average to the individual records, we find a great lack of uniformity. To judge by these figures, the "fluctuations" of one individual, during ten seconds of strained attention, follow an entirely different course from those of another individual. Subjects B, H and I appear at their best at the beginning, and grow worse, though with some possible recovery towards the close of the ten seconds. Subjects D and N begin at their worst, and gradually improve. L is much like the second of these groups, and M much like the first. A, C, E, F, and J give an oscillatory impression, the number of waves in the ten seconds varying from 1½ to 3½ for these five individuals. K completes one wave in 5 seconds, and then remains steady except for one lapse. G and O are pretty steady, except for one specially good interval in G's case, and one bad one in O's case. P's curve can hardly be called anything but erratic, while Q and R are very steady throughout. The total amplitude of the "wave" in Q's case measures only 11 per cent. of his average reaction time, and in R's case only 6 per cent.; whereas in the cases of B, C and H it amounts to 36–37 per cent. of their respective average reaction times.

These individual differences are not to be taken very seriously, on account of the small number of reactions of each individual. Perhaps, however, the evidence is sufficient to indicate that individuals do differ in the fluctuations of their preparedness to react, and that there is no one typical curve of preparedness, such as is asserted by Della Valle.

Some notion of the reliability of the above individual differences[14] can be got in the following manner: Compare the variability of each individual within each interval with his variation from one interval to another. The variabilities within each interval are provided by Table VI. in the form of mean variations; and the variation from

[14] Besides the statistical consideration advanced in the text, it seems also important to remark that every subject whose records appear in these tables had had previous practise, none having had less than one preliminary series of 100 reactions, and most of them having had over three such series. As this amount of practise should have made the reactions of each individual tend towards uniformity, the considerable irregularities observed may be attributed to the disturbing effect of varying the interval.

interval to interval can be computed from the same table by determining the mean variation of each individual's average times for the separate intervals. Evidently a genuine fluctuation from interval to interval would be indicated by a large mean variation of the interval-averages in comparison with the mean variation of the single reaction times within the separate intervals. If the division of all the individual's reaction times into ten groups according to the interval were equivalent to a random division—or if, in other words, the interval made no real difference in the time of reaction—then the averages of the ten groups should vary less than the single reaction times; and, in fact, the mean variation of these averages should be about equal to the mean variation of the single reaction times divided by the square root of the number of cases in a group. The number of cases in a group being 10, the mean variation of the group averages should be about one third of the mean variation within the separate groups. If the mean variation of the group-averages is decidedly greater than this, the inference is that there is a genuine variation in reaction time according to the interval.

These considerations have led to the construction of the following table, in which the upper line gives the designations of the 18 subjects, the second line the average of each individual's mean variations within the ten separate groups, the third line the mean variation of his ten group-averages about his general average, and the bottom line the ratio of the numbers in the third line to those in the second.

Indiv.	R	Q	O	P	A	G	K		E
M.V. a	12.4	14.4	9.8	14.6	25.0	15.1	13.9	13.5	23.3
M.V. Av	1.7	3.4	2.5	4.6	9.0	5.8	5.7	6.9	12.1
Ratio	.14	.24	.26	.32	.36	.38	.41	.51	.52

Indiv.	H	N	L	C	I	F	D	J	B
M.V. a	19.8	13.6	14.7	26.5	15.0	16.0	15.2	16.0	12.7
M.V. Av	10.4	7.3	8.4	15.8	9.0	10.1	9.7	12.8	17.7
Ratio	.53	.54	.57	.60	.60	.63	.64	.80	1.39

Did the interval make no real difference in the reaction time, the ratios in the bottom line of this table should center about .32; as a matter of fact, 14 out of the 18 individuals give a ratio greater than this, and the average of all is .52. This is satisfactory evidence that the length of the interval is a genuine factor in determining the reaction time. But the table also shows great individual differences in this respect; those individuals who stand at the beginning of this table vary no more from interval to interval than would be expected to occur by chance. Reference back to Table VI. shows that three of the four individuals who are least affected by the interval are also those who have the shortest reaction times, whereas three of the four who are most affected by the interval are among the slowest reagents. Taking all the individuals into consideration, we find a positive correlation of moderate degree (Pearson $r = + .51$) between the ratio above considered and the average reaction time; the quicker reagents are on the whole less affected by changes in the duration of the interval than are the slower reagents. Correlating the ratio with the *best* average reaction time made by each subject for any single interval, we still obtain $r = + .30$. Correlating the ratio with the mean variation of the single reaction times within their own groups (*i. e.*, correlating the first with the last row of figures in our last table), we obtain $r = 0$; but this figure also would be positive except for the single case of individual B, who is peculiar in giving very uniform times within each interval, but very different times for the different intervals. On the whole, it appears that the "better" reagents are the less affected by variation of the interval between ready signal and stimulus; and certainly the indications are that the condition of preparedness which gives rise to the very quickest reactions is little affected by varying the interval. This condition is not so unstable but that it can be maintained for a period of ten seconds.

If the individuals most affected by the interval agreed among themselves in their curves of fluctuation, we might conclude that some physiological rhythm came to the surface in their records, though concealed in those other individuals who are little affected by variation of the interval. In fact, however, the individuals who are most affected by the interval are seen in Table VII. to differ altogether in the character of their fluctuations.

To sum up, then, the results of this experiment, we may conclude that no necessary and universal fluctuation occurs in the period of waiting for a stimulus to reaction (up to 10 seconds), and that some individuals maintain a uniform and very perfect preparation to react during this period. Individuals differ, however, and in two respects: first, in the degree of their susceptibility to variations of the interval; and second, in the curve of their fluctuations. An interval of two or three seconds appears to be in general the most favorable; but 5 of the 18 subjects are better suited by an interval of one second, and it is in respect to this shortest interval that the individual differences (as measured by the mean variation in the bottom line of Table VII.) are greatest.

A similar experiment was tried with visual stimuli. The preparatory signal again was the spoken word "ready." The visual stimulations were produced by means of a revolving mirror throwing the light of a sixteen candle power incandescent electric bulb through a lens focused on a translucent screen. Whenever the mirror was turned at such an angle as to produce an image on the screen, it simultaneously made connection through the chronoscope. The results of this experiment are shown in Tables VIII. and IX., the latter being derived from the former as Table VII. was derived from Table VI.

TABLE VIII

TABLE SHOWING REACTIONS FOR EACH INTERVAL TO VISUAL STIMULI

		1	2	3	4	5	6	7	8	9	10	Av.
Br.	(1)	238.6	191.9	183.2	188.2	175.4	199.2	168.4	180.1	214.4	207.4	194.7
		48	*23*	*10*	*17*	*12*	*26*	*9*	*21*	*37*	*33*	
Br.	(2)	190.5	157.5	158.9	155.0	181.0	173.0	165.5	181.0	184.7	157.5	170.5
		24	*17*	*10*	*16*	*34*	*19*	*25*	*22*	*24*	*21*	
Br.	(3)	217.8	194.2	205.5	168.2	206.5	180.5	225.2	221.5	210.0	210.5	204.0
		36	*27*	*32*	*12*	*32*	*22*	*31*	*11*	*30*	*21*	
Ba.		191.5	195.9	183.7	191.2	182.9	183.1	195.8	188.0	184.2	187.1	188.3
		18	*17*	*17*	*18*	*17*	*16*	*19*	*17*	*12*	*19*	
Ri.		208.0	178.2	158.5	170.8	173.8	167.0	189.0	167.8	168.5	184.6	176.6
		23	*25*	*29*	*23*	*26*	*33*	*27*	*23*	*27*	*33*	
Th.		241.2	196.9	205.0	187.0	211.5	233.4	236.8	217.7	212.6	219.6	216.2
		27	*30*	*14*	*29*	*18*	*30*	*25*	*31*	*21*	*15*	
Av.		214.6	185.8	182.5	176.7	188.5	189.4	196.8	192.7	195.7	194.5	191.7

TABLE IX

		1	2	3	4	5	6	7		9	10
Br.	(1)	122	98	94	97	90	103	86	92	110	107
Br.	(2)	112	92	93	91	106	102	97	106	108	92
Br.	(3)	107	95	101	83	101	88	110	109	103	103
Ba.		102	104	97	102	97	97	104	100	98	99
Ri.		118	101	89	96	98	94	107	95	95	105
Th.		112	91	95	87	98	108	109	101	99	102
Av.		112	97	95	93	98	99	102	101	102	101
M.V.		*5*	*4*	*3*	*6*	*4*	*7*	*7*	*5*	*5*	*4*

The agreement between individuals is greater here than in the preceding experiment. There is indeed little to suggest an "attention wave" of the usual sort; but the individuals agree in finding the interval of one second too short, and those of 2–6 seconds, on the whole, more favorable than those still longer. The most favorable interval seems here to be 3–4 seconds. Fig. 1 shows in graphic form the same results. In this figure as well as all those that follow in this chapter the dotted line represents a value of 100 per cent., or the general average of each individual.

When it became evident that groups of individuals did not show regular fluctuations in the reaction time, the next problem was to see whether the records of a single individual would show them. Perhaps there might be a curve or type of fluctuation typical and constant for him from day to day. Hence, a series of reactions on different days were taken for two individuals. The reactions were made to the same auditory stimulus used in Table VII.

Of the following tables, X. and XII. give the results of this experiment in their original form, and XI. and XIII. are derived in the

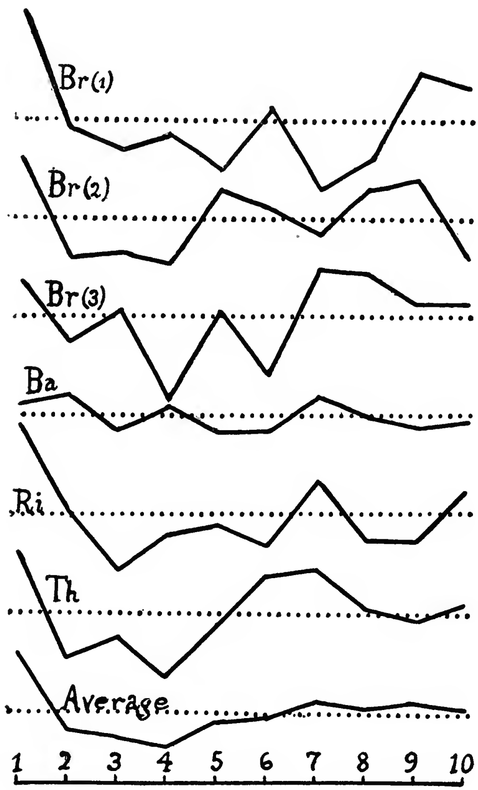

Fig. 1. Graphic Form of Table IX.

manner previously explained. The bottom lines require some explanation: in Tables X. and XII. they give the average of the mean variations shown in italicized figures above, each entry in the bottom line therefore indicating the variability of the single reactions at each interval. In Tables XI. and XIII., on the other hand, the bottom line gives the mean variation of the averages above, and thus indicates how much the different series, on different days, varied from each other.

Since the main object of the present experiment is to examine whether each individual presents a uniform curve of efficiency during the interval following the ready signal, the results can be best seen in Figs. 2 and 3, which are simply graphic forms of Tables XI. and XIII. respectively.

The figure for either subject shows clearly enough a regularity in the curve of preparedness during the variable interval following the ready signal. Moreover the curves of these two subjects agree in general form: starting from a high point, they all descend rather quickly to a low point from which then they rise again, though the amount of this later rise is variable and on the average slight. Under the conditions of this experiment, accordingly, these two subjects agree in not being fully prepared for reaction till more than a second after the preliminary signal.

The individuals differ rather steadily in the speed with which the low point of the curve is reached; the first individual is usually at his best within 2–3 seconds, but the second not till 3–4 seconds.

After the first low point is reached, the further oscillations are inconstant for each individual and give no evidence of anything corresponding to the classic wave of attention.

The unfavorable character of the intervals of 1–2 seconds seems by no means necessary even for these two subjects, for the large mean variations indicate that the degree of preparedness varied greatly immediately after the ready signal was given, and was sometimes excellent. There is some tendency for the subject to "take chances" on the short intervals. Since all intervals up to ten seconds came equally often and in irregular order, the chances of receiving the stimulus one or two seconds after the ready signal were small, and a leisurely making ready to react was usually as good as a hasty preparation, and somewhat less strenuous. In view of the fact that some of the subjects in the previous tables found the intervals of one and two seconds favorable, it may be doubted whether the unfavorable appearance of these short intervals with the two subjects Th. and Br. means anything beyond this subjective reliance on the law of probability.

Some light on this question may be expected from the following experiment, performed with the subject Br., one of the two who participated in the experiment just preceding. In this new experiment the subject knew in advance what length of interval was to occur between the ready signal and the stimulus. The ten reactions with each interval were, on each day, taken in sequence before passing to another interval. The stimulus was again auditory.

The results show that foreknowledge of the interval makes the

TABLE X

Table showing Reactions for Each Interval to Auditory Stimuli for the Same Subject, Th.

Interval	1	2	3	4	5	6	7	8	9	10	Av.
1.	242	181.7	184.5	199	182	181.2	212	223.3	208.4	210	202.4
	30	*17*	*13*	*17*	*19*	*20*	*15*	*19*	*15*	*16*	
2.	224.8	189	171	188.1	183.3	191.7	188.5	188.7	188.6	192.1	190.6
	32	*26*	*13*	*17*	*12*	*11*	*10*	*9*	*7*	*13*	
3.	241.4	212.1	161.2	170.2	164.9	180.3	175.1	180.1	173	179	183.7
	37	*39*	*12*	*16*	*17*	*9*	*17*	*16*	*15*	*21*	
4.	188.9	175.6	165.8	166	159	165.4	165.4	164.4	161	165.9	167.7
	20	*16*	*14*	*14*	*12*	*16*	*17*	*15*	*13*	*14*	
5.	206	166.3	167.4	172.8	169.2	169.2	180.7	170.1	181.1	164.5	174.7
	35	*18*	*15*	*14*	*14*	*17*	*15*	*9*	*18*	*20*	
Av.	220.6	184.9	170.0	179.2	171.7	177.6	184.3	187.1	182.4	182.3	184.0
Av. M. V.	*30*	*23*	*13*	*16*	*15*	*15*	*15*	*14*	*14*	*17*	

TABLE XI

	1	2	3	4	5	6	7	8	9	10
1.	119	90	92	99	90	89	105	109	103	104
2.	118	99	91	99	96	101	99	99	99	101
3.	131	115	88	93	90	98	95	98	94	97
4.	112	104	99	99	95	98	98	98	96	99
5.	118	95	96	99	97	97	103	98	104	94
Av.	120	101	93	98	94	97	100	100	99	99
M. V.	*5*	*7*	*3*	*3*	*3*	*3*	*3*	*3*	*3*	*3*

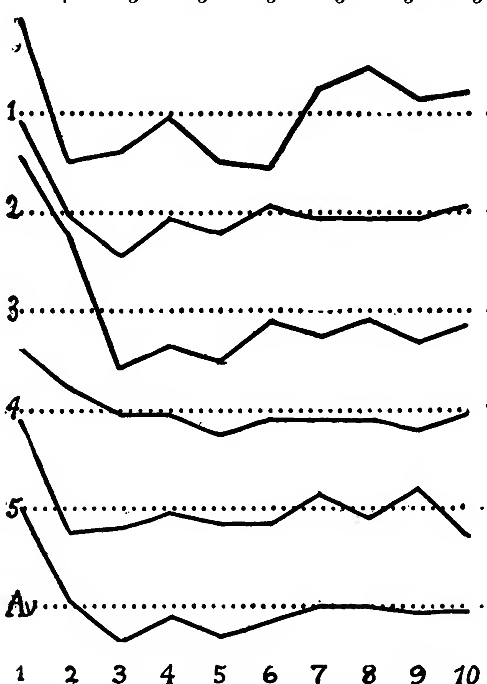

Fig. 2. Graphic Form of Table XI.

TABLE XII

TABLE SHOWING REACTIONS FOR EACH INTERVAL TO AUDITORY STIMULI FOR THE SAME SUBJECT, BR., ON SEVERAL SUCCESSIVE DAYS

Interval in Sec.	1	2	3	4	5	6	7	8	9	10	Av.
1.	189.5	177	160	149.5	176.7	168.7	186.2	171.8	185	176.3	174.1
	19	*23*	*11*	*17*	*15*	*14*	*17*	*32*	*12*	*27*	
2.	178.1	145	134.5	138.4	144.9	144.3	170.7	165	156.9	151.1	152.9
	22	*19*	*15*	*11*	*12*	*19*	*21*	*16*	*19*	*7*	
3.	155.5	146	138.5	133.5	126	133.5	136.5	140.5	137	140.5	138.8
	20	*13*	*11*	*9*	*12*	*15*	*6*	*17*	*12*	*6*	
4.	181.8	131.8	124.1	133.2	133.5	125.7	132	132.3	134	138.6	136.7
	15	*9*	*10*	*6*	*17*	*11*	*10*	*12*	*15*	*9*	
5.	199.9	142.1	133	116	128.6	125.5	129.2	125.7	127.5	139.7	137.7
	20	*12*	*11*	*8*	*10*	*13*	*14*	*13*	*8*	*14*	
Av.	181.0	148.4	138.0	134.1	141.9	139.5	150.9	149.1	148.1	149.2	148.0
Av. M. V.	*19*	*15*	*12*	*10*	*11*	*14*	*14*	*17*	*13*	*9*	

TABLE XIII

	1	2	3	4	5	6	7	8	9	10
1.	109	102	92	86	102	97	107	99	106	102
2.	116	95	88	91	95	94	112	108	103	99
3.	112	105	100	96	91	96	99	101	99	101
4.	132	97	91	98	98	92	97	97	98	101
5.	144	103	97	84	93	91	94	98	92	102
Av.	123	100	94	91	96	94	102	101	100	101
M. V.	*12*	*4*	*4*	*5*	*3*	*2*	*6*	*3*	*4*	*1*

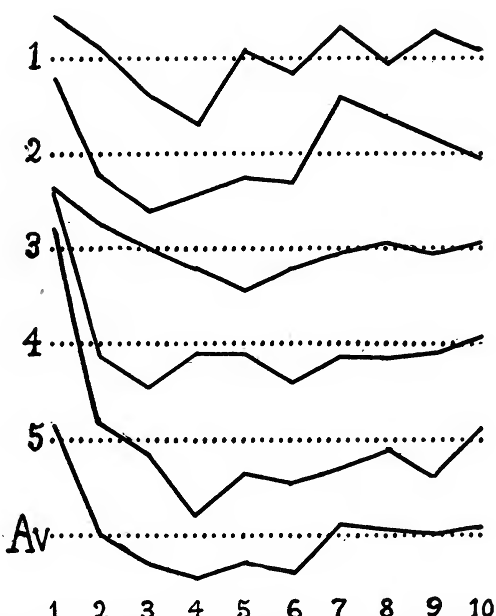

FIG. 3. Graphic Form of Table XIII.

reaction quicker, and much more uniform throughout the range of intervals. Still, the interval of one second is less favorable than those of two and three seconds; and the longer intervals again give somewhat slower reactions. The series of reactions taken on different days are in fair agreement as to these two points. It appears thus that this subject can not readily adjust himself for the reaction within the first second after the ready signal; and also that he is not likely to be at his best at the end of an interval of over 4 seconds.

Since fluctuations of the classic type did not appear in the preceding experiments, the most promising lead for further work seemed to lie in the direction of examining more completely the matter of the most favorable interval. The experiment was accordingly modified by requiring the subject to busy himself with a form of mental work, and then, at the preliminary signal, to drop this work and prepare himself promptly for reacting. The thought underlying this experiment was that the adjustment to react involves an inhibition of the previously existing adjustment, but that, in the usual conditions of reaction time experiments, the previously existing adjustment is of no great intensity, and therefore does not require much time to inhibit. If the previously existing adjustment were intense—*i. e.*, if

TABLE XIV

TABLE SHOWING REACTIONS FOR EACH INTERVAL TO AUDITORY STIMULI WITH KNOWN INTERVAL. SUBJECT BR.

	Intervals in Seconds										
Days	1	2	3	4	5	6	7	8	9	10	Av.
1.	115.2	109.6	111.1	121.9	111.4	111.5	131.6	130.5	120.8	122.2	118.5
	9.7	*9.2*	*9.4*	*10.3*	*9.4*	*9.4*	*11.1*	*11.0*	*10.2*	*10.3*	
2.	110.5	112.1	107.4	109.8	107.3	105.4	115.1	111.5	117.5	120.2	111.7
	9	*10*	*8*	*9*	*8*	*7*	*8*	*10*	*6*	*8*	
3.	123.3	110.4	110.4	115.4	119.3	127.8	126.6	116.2	122.4	116.6	118.8
	10	*9*	*9*	*8*	*9*	*10*	*10*	*8*	*9*	*10*	
4.	141.4	129.2	132.1	134.2	140	145.8	134	139.7	156	150.4	140.3
	11	*8*	*9*	*7*	*10*	*8*	*6*	*9*	*8*	*10*	
5.	134	127.8	137.2	137.4	139.9	145	147.6	149.7	146.2	138.7	140.4
	9	*7*	*7*	*6*	*9*	*8*	*6*	*7*	*10*	*8*	
Av.	124.9	117.8	119.6	123.7	123.6	127.1	130.9	129.5	132.6	129.6	125.6

TABLE XV

	1	2	3	4	5	6	7	8	9	10
1.	97	92	94	102	94	94	111	110	102	103
2.	99	100	96	98	96	94	103	100	105	108
3.	104	93	93	97	100	107	106	98	103	98
4.	100	92	94	96	100	104	95	99	111	108
5.	96	91	98	98	100	103	105	107	104	99
Av.	99	94	95	98	98	100	104	103	105	103
M. V.	*2*	*2*	*1*	*2*	*2*	*5*	*4*	*5*	*2*	*4*

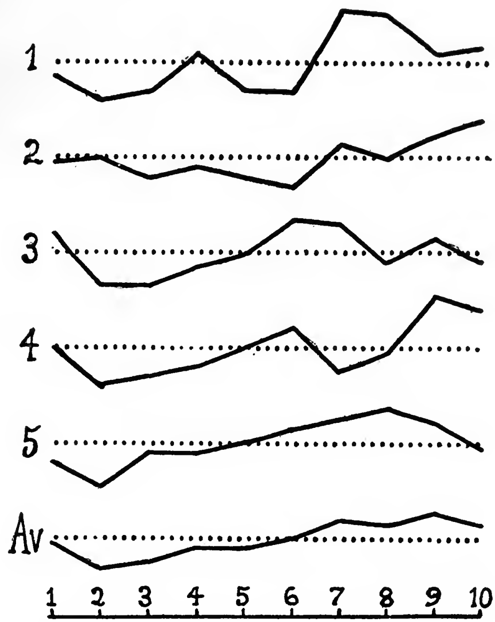

FIG. 4. Graphic Form of Table XV.

attention were strongly occupied with something quite apart from the reaction experiment—the transition from this existing adjustment to the adjustment to react might be a matter of some difficulty, and require more time than in the usual experiment.

The procedure was as follows—the stimulus being auditory, and the general conditions the same as before. A sheet containing printed columns of figures was placed before the subject, and he was instructed to add as rapidly as possible (without writing the sums). On hearing the signal "Ready!" he was to cease adding and be ready to react to the auditory stimulus. After the reaction he was to return to his adding and continue at it till the next ready signal.

Of the three subjects who took part in this experiment, the first made a single series of 100 reactions, the second made two such series on different days, and the third made three such series. The results are shown in Tables XVI. and XVII. (the latter being derived from the former in the same way as hitherto), and in Fig. 5.

Comparison of the reaction times for Th. and Br. in Table XVI. with those in Tables X. and XII. shows that the novel conditions of the present experiment at first caused a general slowing of reaction after all intervals, but that practise under the new conditions quickly increased the speed of reaction.

TABLE XVI

TABLE SHOWING REACTIONS FOR EACH INTERVAL, THE SUBJECT STOPPING THE ADDING OF SIMPLE NUMBERS AT THE READY SIGNAL

terval in Sec.				3	4	5	6	7	8	9	10	Av.
Pf.		133.9	83.9	90.2	88.9	95.5	84.2	88.8	94.7	91.9	85.7	93.8
		40	*11*	*12*	*13*	*16*	*17*	*27*	*14*	*14*	*15*	
Th.	(1)	302.7	211.3	231.6	201.2	243	229.8	213.8	244	225.8	228.8	233.2
		50	*39*	*22*	*33*	*22*	*22*	*21*	*16*	*29*	*25*	
Th.	(2)	252.8	205	187	199.4	204.1	192.6	205	190.8	196.8	220.1	205.4
		29	*28*	*20*	*14*	*25*	*12*	*7*	*21*	*5*	*27*	
Br.	(1)	253.3	160.2	143.4	149.2	163	145.9	136.5	150.9	146.5	154.7	160.4
		78	*24*	*13*	*16*	*23*	*86*	*63*	*17*	*13*	*16*	
Br.	(2)	166.2	151.1	153	152.9	142.9	139.9	157.6	149.2	150.3	168.5	153.2
		13	*17*	*12*	*23*	*20*	*12*	*17*	*18*	*21*	*24*	
Br.	(3)	155.3	133	120.3	116	120	123.3	117.3	124.9	124.7	128.8	126.4
		17	*14*	*10*	*11*	*10*	*12*	*13*	*5*	*12*	*13*	
Av.		210.7	157.4	154.2	151.3	161.4	152.6	153.2	159.1	156	164.4	

TABLE XVII

		1	2	3	4	5	6	7	8	9	10
Pf.		143	89	96	95	102	90	95	101	98	91
Th.	(1)	130	91	99	86	105	99	92	105	97	98
Th.	(2)	123	100	91	98	100	94	100	93	96	107
Br.	(1)	158	100	89	93	102	91	85	94	91	97
Br.	(2)	109	99	100	100	93	92	103	98	98	110
Br.	(3)	123	106	96	92	95	98	93	99	99	102
Av.		131	98	95	94	99	94	95	98	97	101
M.V.		*15*	*5*	*3*	*4*	*3*	*2*	*5*	*3*	*3*	*7*

As to the most favorable interval, contrary to what might be expected, it was not definitely lengthened by the need of transition from active mental work to the reaction experiment. The balance seems to be shifted slightly in favor of the longer intervals; and the interval of one second is more unfavorable than before; but on the whole the most striking outcome of the experiment is the quickness with which the transition from adding to the reaction experiment can be accomplished.

A modification of the experiment with mental work, introduced for purposes of control, consisted in requiring the subject to continue the adding after the ready signal. The adding was not to be interrupted in preparation for the reaction, but was to be continuous. The results appear in Tables XVIII. and XIX., and in Fig. 6.

The absolute time of reaction, which was increased in the preceding experiment, is still further increased in this, as is seen by comparing the records of Th. and Br. in Tables XVI. and XVIII. This fact is of some importance for purposes of control, since it shows that the reaction was not, in these individuals, so automatic as not to re-

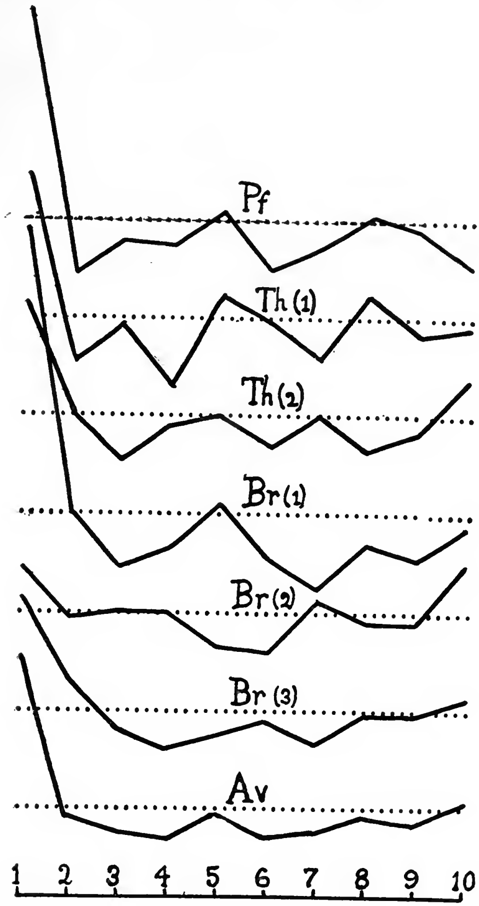

FIG. 5. Graphic Form of Table XVII.

quire undivided attention. The curve of readiness to react is seen to differ considerably in continuous adding from the curves which have resulted from previous experiments. The fluctuations are comparatively slight within most of the series, and the unfavorableness of the one-second interval has almost disappeared. As a control experiment, therefore, this goes to show that the curves obtained in the previous experiments were truly expressive of the process of forming and holding an attentive adjustment to react.

The experiment with continuous adding was more comfortable for the subject than the experiment which demanded the cessation of adding at the ready signal. The sudden interruption of adding—

TABLE XVIII

TABLE SHOWING REACTIONS FOR EACH INTERVAL WHILE THE SUBJECT WAS ADDING CONTINUOUSLY

Interval in Sec.	1	2	3	4	5	6	7	8	9	10	Av.
Ri.	202	193.2	195.2	201	217.3	222.1	204	155.2	208	202	200
	30	*20*	*34*	*38*	*42*	*43*	*46*	*17*	*27*	*21*	
Th.	301.4	318.1	321.9	337.3	315.2	325.5	324.3	331.7	359.6	370.3	330.5
	44	*36*	*37*	*38*	*17*	*31*	*36*	*37*	*28*	*23*	
Br.	202.2	205.2	213.8	207.6	212.3	212.5	194.4	243.2	229.9	207.2	212.8
	35	*38*	*35*	*42*	*42*	*40*	*26*	*33*	*51*	*41*	
Br.	197	158.7	153.2	162.5	166.3	174.2	183.9	178.1	182.2	181.4	173.8
	25	*23*	*19*	*19*	*27*	*27*	*40*	*20*	*13*	*11*	
Br.	167.7	167	153.5	156.5	178.2	175.1	154	182	173.7	175	168.3
	14	*26*	*21*	*13*	*21*	*29*	*20*	*19*	*23*	*21*	
Av.	214.1	208.4	207.5	213.0	217.9	221.9	212.1	218	230.7	227.2	

Each entry gives the average of ten reactions.

TABLE XIX

	1	2	3	4	5	6	7	8	9	10
Ri.	101	96	98	100	108	111	102	78	104	101
Th.	91	97	98	102	96	98	98	100	109	113
Br. (1)	95	96	100	98	100	100	91	114	108	97
Br. (2)	113	91	88	93	95	100	106	103	105	105
Br. (3)	100	99	91	93	106	104	92	108	103	104
Av.	100	96	95	97	101	103	98	101	106	104
M. V.	*6*	*2*	*4*	*3*	*5*	*4*	*5*	*9*	*2*	*4*

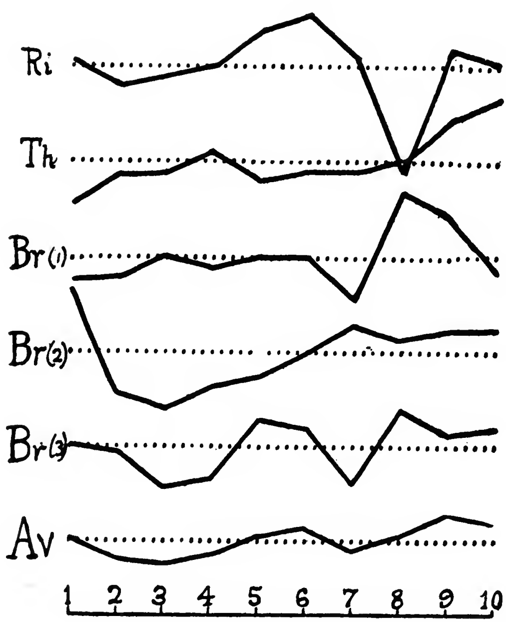

FIG. 6. Graphic Form of Table XIX.

in the midst, often, of seeking for a sum—was felt to be disagreeable, and it was more satisfactory to be allowed to proceed with the adding, taking care of the reaction "on the side." There was some degree of active adjustment for the reaction, but as the adding proceeded during the "interval," thought of the reaction was likely to lapse from consciousness, and the stimulus sometimes brought a shock of surprise. The relatively slow reactions after the longer intervals, in this experiment, were sometimes due to this forgetting about the reaction. It should be mentioned that the adding done under these circumstances was rather inaccurate; many errors were committed and at times considerable confusion was felt. The reaction itself offered no great difficulty to any of the subjects; it usually seemed slow to them, and to come as a kind of reflex response to the stimulus. If the stimulus followed shortly upon hearing the signal "Ready!" the reaction seemed introspectively easy and quick, but when the stimulus came after a long interval, it caused surprise and the reaction seemed to the subject slow and difficult.

Two of the subjects, who had had the most practise, were asked in one series each to record the interval that seemed most favorable for reaction. They were not told the length of the interval that had elapsed after the ready signal till the (auditory) stimulus came. They reported only as to the favorableness or the unfavorableness of the interval after the reaction had been made. There were the usual ten intervals with ten reactions for each interval. The following table shows the intervals and the number of times out of ten trials it was called favorable. The two-second in-

TABLE XX

Intervals in Sec.	1	2	3	4	5	6	7	8	9	10
Hw.	6	10	4	2	2	3	1	2	1	0
Wo.	4	9	5	6	6	5	4	3	3	1

terval was accordingly most preferred subjectively. The result of the introspective judgments as shown in Table XX. is also shown by the curves in Fig. 7.

The following introspective observations should also be recorded. Table VI. shows that in any individual's record there are fluctuations of attention which are shown by the reactions. The introspective reports were that the fluctuations were due partly to a shifting of attention that could be plainly felt by the subject. The subjects often said they found it hard to hold the attention steadily for the longer intervals. Though the reactions were required to be made with a "normal" attitude, there was often found a tendency to employ either a purely motor or a purely sensory reaction,[15] or to shift from one to the other. In regard to feelings of strain, these were reported as being most in evidence in the first few seconds following the ready signal; they then disappeared and the mind often seemed a blank.

The general outcome of the preceding experiments is thus unfavorable to fluctuations, in the usual form, but favorable to the reality of a most favorable interval with the duration of about 2–3 seconds.

In regard to fluctuations, a single day's reactions often present a striking appearance of a recurrent wave of attention, as shown in Table VI.; but individuals differ altogether in the character of these fluctuations. The crests and troughs are as likely to come at one time

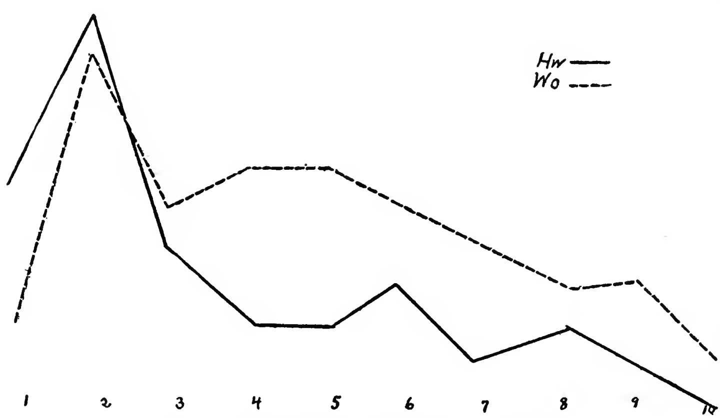

FIG. 7. Introspectively Preferred Intervals.

as at another. Moreover, some individuals, whose reactions are so quick as to indicate a very perfect adjustment, show no fluctuations except such as would be expected from chance variation. As to whether the fluctuations shown by any individual would remain constant for him from day to day, our experiments have discovered one type of wave which does remain constant; but this is a very simple type, consisting simply in the occurrence of a most favorable interval. Other oscillations were inconstant from day to day, in the individuals tested. It is possible, to be sure, that others of the forms of oscillations shown in Table VI. might remain constant for some individuals, but we did not find individuals maintaining any constant curve, aside from the simple curve due to the existence of a single opti-

[15] A few of the subjects thought that after reacting for some time they might become more uniform and efficient if they were allowed to adopt a motor form altogether. It is doubtful whether this result would have been obtained, since the motor form causes fatigue especially for a long series of reactions.

mal interval. Now it would seem that the reaction time ought to be a fairly delicate indicator of fluctuations of attention, or of readiness to react; if it indicates so clearly the existence of a most favorable interval, it ought also to indicate the existence of any physiological rhythm of a regular and universal character, exercising an influence upon cerebral efficiency. Our results seem to justify a conclusion adverse to such a rhythm, so far as it could be expected to appear within a space of ten seconds. The manifold fluctuations which make their appearance in short series of reaction experiments must be due to something less "physiological," less fundamental and universal. Some of these less universal factors have been suggested by the introspections of our subjects: such as expectations based on the probability that the stimulus will or will not come immediately; shiftings from the sensory to the motor attitude; intrusion of extraneous thoughts and recall to the work in hand.

My conclusion in regard to fluctuations is therefore in close agreement with that cited above from Klemm, and in entire disagreement with that of Della Valle.

As Titchener well says,[16] "Consciousness is always in a flux. . . . Sensible quality, for instance, can not maintain itself in consciousness for any length of time; wherever there is sensory adaptation—in pressure, in temperature, in sight, in smell, to some extent in taste—there is also gradual change or disappearance of quality." There are always certain sensory stimulations present beside the one utilized in an experiment. The absolute and relative strength of such stimulations constantly changes. Changes of this kind materially affect the exciting influence of any task to which one may hold oneself by voluntary attention. Likewise there are many paths for possible motor discharges. At times it may be easier for a movement to be made at one place, at other times at another. Partial or weaker discharges always take place over other paths besides the one specifically demanded by the conditions of the task. The relative strengths of these accompanying motor discharges also vary from time to time, contributing to or detracting from the strength of the original reaction. Since these various paths are so numerous and their inter-relations so complex it is not strange that fluctuations of the attentive attitude occur. For the same reasons we would not expect the fluctuations to be uniform or rhythmic in character. The changes that do occur might be in part peculiar to the individual. To discover the determining factor of these individual variations would require an analysis of his whole experience and his present situation. We have then no evidence that this set or balance of mind

[16] "Psychology of Feeling and Attention."

changes in any rhythmic or periodic way; it varies rather, by the chance combination of circumstances. Fluctuations are caused by the complex discharges over motor paths and stimulations over sensory paths, rather than by bodily rhythms such as are found in the blood circulation and in breathing.

Now as regards the most favorable interval, the subjects who have been repeatedly examined in my experiments, under varying conditions, have shown pretty uniformly an optimal interval of from 2 to 4 seconds. Previous investigations have indicated optimal intervals as follows: Cattell,[17] $\frac{3}{4}$ to 2 seconds; Lange,[18] 2 seconds; Bertels,[19] $2\frac{3}{8}$ seconds. An "adequate interval" of about the same durations has been indicated by experiments on the time sense: Estel,[20] $2\frac{1}{4}$ seconds; Mehner,[21] $2\frac{1}{2}$ seconds.

The duration of the optimal interval, in my experiments, is not sharply limited, but extends at least from 2–4 seconds. It differs also in different individuals; in some, it includes intervals as short as 1 second and as long as 10 seconds.

The concept of an optimal interval requires that intervals shorter than a certain range shall be relatively unfavorable, and that intervals longer than a certain range shall again be relatively unfavorable. The most favorable interval must lie between intervals that are less favorable. *A priori,* the concept would seem to be justified beyond question; for, if a subject is not already fully prepared to react on receiving the ready signal, some time must be required for him to reach his maximal degree of preparedness; and, again, this maximal degree of preparedness can scarcely be maintained indefinitely. The time of best preparation must therefore be preceded and followed by times of inferior adjustment. Some individuals, however, reach the condition of maximal preparation in less than 1 second; and some individuals also maintain this condition till the end of 10 seconds, so that no optimal interval appears for them under the conditions of our experiment. It is a fact of some importance that the same individual sometimes reaches his optimum within the first second, and maintains it till the end of the ten seconds, and that such individuals are often those whose preparation to react is most perfect, as judged by the quickness of their reaction times. The conclusion seems justified that the adjustment to react can be maintained by some individuals in its most perfect condition for a time of at least ten seconds. But this feat does not seem to be accomplished by most individuals.

[17] *Mind,* **11**, 239.
[18] Wundt's *Philos. Stud.,* 1887, **3**, 492.
[19] Inaug. Dissert., Dorpat, 1886.
[20] Wundt's *Philos. Stud.,* 1885, **2**, 37.
[21] *Ibid.,* 1886, **2**, 560.

CHAPTER IV[1]

Resistance of Keys as a Factor in Reaction Times

The results tabulated in this chapter are from records made in the psychological laboratories of Indiana and Columbia universities during the years 1907–10. A few records with varying resistances were taken in the summer of 1906. These showed an increase in the reaction time as the resistance of the reacting key was increased. With a view to the further investigation of this problem, special keys were designed and a program for more experimental work was arranged.

As finally formulated, the purpose of the experiment was: (1) To find what difference in the reaction time would be caused by increasing the resistance of the reacting key from 50 to 500, 1,000 and 1,500 grams; (2) to find the variation in the number of taps that could be made in five seconds with the same series of resistances; (3) to ascertain the changes in the reaction time for an isolated movement, namely, that of the last joint of the index finger with a series of varying resistances beginning at 1,000 grams and increasing 500 grams in each series until the increase had gone beyond the lifting ability of the muscles involved; (4) the effect on the reaction time of using a release type of key held down against various resistances and released at a given signal.

As will be shown more fully below, the measurements for most of the records were made with the keys which required a movement like that of the ordinary telegraph key, while for those in Table XXIV. an ergograph was used.

The apparatus used for measuring the reaction time was an improved type of pendulum chronoscope (the same as was used for the time measurements in other parts of this report) with accessories as designed by Professor Bergström.[2] The reacting key, which was of the break circuit type, was so arranged that the tension of a spring could be brought to bear upon the lever of the key in such a way that various pressures could be required for the breaking of the circuit.

[1] Part of the results given in this chapter was published in the *Psychological Review,* September, 1909, under the same title as the above. The contribution was from the Psychological Laboratory of Indiana University. Additional work has been done on this subject which is included in the present chapter. This additional work has been done almost wholly at Columbia University.

[2] *Psychological Review,* 1910, **17**, 1.

For the release type of key varying pressures were required to keep the lever from springing back and breaking the circuit.

The signals were given by a spring sound hammer with a scale indicating the height of the hammer stroke, thus making it possible to make the force of the stroke uniform. A stimulus of a uniform intensity was thus assured.

To count the number of taps a recorder like that described by W. L. Bryan in *The American Journal of Psychology,* November, 1892, was employed. The rate of tapping was obtained by means of a metronome which was regulated with a stop watch.

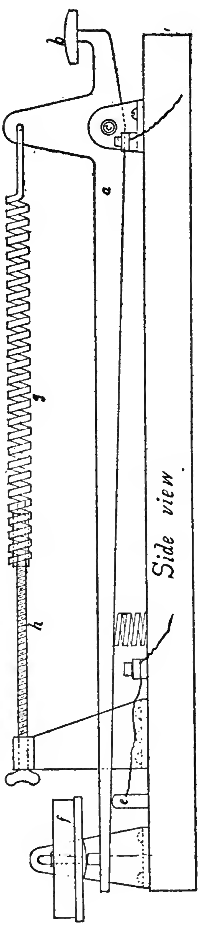

Fig. 8.

The key used for most of the reaction measurements in general and for all of the excess measurements is shown in Fig. 8. This key consisted of a long steel blade *a* (see Figs. 8 and 9) so mounted that the button *b* was on the short arm of the lever, which in turn was mounted on an axle at *c*. Under the long arm of the lever was fastened a short spring which allowed only a small movement even for a considerable increase in pressure. Since it was found that the same effect could be obtained by strengthening the spring *g*, this short spring was dispensed with in a second construction of the key. A brass post *e* stood under the long end of the lever and here the current was made or broken at platinum contacts. Just above *e* stood another post which could be turned so that it projected over the lever *a*. This also carried a platinum contact tip that could come in contact with a similar kind of mounting on the top side of the lever. This arrangement allowed the current to be broken by the release of the key since the connections were thereby made through *x* (Fig. 9) instead of through *a*. It was the breaking of these currents which stopped the chronoscope in the reaction experiments.

In the first part of the experiments, a receiving tambour *f* was used in connection with the end of the lever which in turn was connected by means of a rubber tube with a recording tambour. This latter tambour traced its record upon a revolving kymograph drum. The records so obtained showed the amount of movement of the lever after the connection at *e* was broken. In other words the height to which the tambour lever rose indicated the excess pressure exerted by the finger on the key. An empirical scale for measuring the tambour records was obtained by placing standard weights on the button *b* of the key and noting the height to which the tambour lever was raised by each standard weight. With this scale the tambour strokes could be measured and their value ascertained in grams of pressure upon the key.

In the later experiments the tambours were replaced by an upright brass stake and sliding rider. This slide had just enough friction to remain at the point to which it had been raised by the lever. Since the tension of the spring remained in a constant direct proportion to the distance to which it was drawn within the narrow limits of the key it was possible to calibrate the heights of the slide on the stake in such a way that excess pressures could be read in grams directly from the scale.

The varying resistances were introduced by the spring *g*, the tension of which was changed by the screw *h*. A pointer was fastened to the head of the spring which projected over a scale fastened alongside the spring. This scale indicated the initial pressure which was being exerted by the constant strain of the spring. This pressure had to be overcome by the subject before a reaction of the positive type could be made. This also gave the resistance against which the subjects had to hold in the negative movement, the release of which caused the reaction to be recorded.

The records given in the tables were taken throughout a period of seventy-two weeks. They were made in part at Indiana University and were finished at Columbia University. With a few exceptions a full set of reactions was taken at a sitting with rests between trials to avoid fatigue which seemed to have much influence especially with the heavier resistances. A few records were discarded where it was known with a fair degree of certainty that the subject had been disturbed by outside influences. Subjects were kept as free as possible from distracting influences and were asked to give as nearly as possible a uniform concentration of effort on the work at hand. The experimenter endeavored to have all the reactions made with the same kind of movement so as to have as nearly as possible the same muscles called into play.

Tables of the results show in each instance the amount of resistanc of the key in grams and the corresponding reaction times, also the mean variation.

In the first series of experiments the resistance of the reaction key was set at 50, 500, 1,000, and 1,500 grams. A series of ten reactions was taken with each of the resistances in ascending order, and then in reversed order. For the release movements the same order was kept up, only that in this case the pressures released were 100, 500, 1,000, 1,500, and 2,000 grams. Some of the subjects did not go to the end of the series, for they found it too fatiguing and could not maintain the same kind of movement. One subject felt no definite resistance at less than 500 grams, therefore no reactions are recorded from him from 100 grams.

In the second set of experiments, an ergograph[3] was employed as a reacting key in connection with the pendulum chronoscope; and so arranged that whenever a certain weight was lifted on the ergograph it recorded the reaction time on the chronoscope. This reacting movement had the advantage of isolation and uniformity.

When the ergograph and the chronoscope were used together the following order was followed: Ten reactions were taken for every resistance beginning with 1,000 grams as the lightest and increasing the amount by 500 grams each time. This increase was continued up to a point where the subject was unable to lift the weight,[4] then the order was reversed. The ergograph resistance is counted as if it were applied 31.8 mm. from the center of rotation of the joint.

The subjects who served in the experiments were chosen from the students working in the laboratories of Indiana and Columbia universities. For the first experiments they were Mr. Smith and Mr. Durgee. In the ergograph reactions Messrs. Miller and Harris acted as subjects. In the release type of reactions, I had the services of Dr. Hollingworth, Professor Brandon, and Mr. Rice. They will be referred to by their respective initials. The writer also took part in

[3] J. A. Bergström, "A New Type of Ergograph with a Discussion of Ergographic Experimentation," *Amer. Jour. of Psychol.*, 1903, **14**, 510.

[4] The "weight" used in this experiment was always produced by the tension of a spring; the inertia of weights suspended over pulleys was thus avoided.

In the reactions where the ergograph was used as the reacting key the movement was clearly a lifting movement, the hand lying palm up and the weight lifted by means of contracting the index finger, the last joint of which was incased in a thimble, but no weight used in the experiment was produced by any other means than a spring. The weight registered in the tables is the pressure required just to break the circuit. The pressures beyond this point would be quickly stopped, in the positive type by the rapidly increasing tension of the spring and in the negative type by the post's catching the lever after it had fallen only a very short distance (about 8 mm.). This fall allowed a rise of the button of about 1.5 mm. No excess of pressure for the release type of reactions was measured. It is the intention of the writer to so modify the key in the near future that this can also be measured.

nearly all experiments; he will be designated by the letter B. All the subjects had done over twelve weeks experimental work in psychology. H. and M. were especially robust men and had shown themselves very reliable in experimental work. None of the subjects had any preconceived notions as to what the results of the experiment would be and their effort in all the reactions was to make them as quickly as possible.

From the experiments thus preformed we have the following results:

The averages for 125 reactions each for the 50, 500, 1,000, and 1,500 grams resistance are shown in Table XXI. These reactions records were made by S., D., and B. on the first type of key used, which did not record the excess pressure. We also have the averages of thirty-five records of the number of taps made in five seconds on the same key. The resistance for the tapping records was the same as in the series from which the reaction records were made. The records of this table are regarded as preliminary and are, therefore, given merely as simple averages.

TABLE XXI

Resistance of Key	50 Grams	500 Grams	1,000 Grams	1,500 Grams
Av. reaction time for all records of S., D., and B.	112.8	127.5	141.8	149.7
Av. No. of taps in 5 sec. S.	57.8	56.5	54.8	52.3
Av. No. of taps in 5 sec. B.	52.6	48.8	44.2	40.8

In Table XXII., in which the results are given more in detail, will be found averages for 180 reaction records for each resistance made by H., M., and B. on the second key, described on page 40, which recorded the excess of pressure in the reaction. A few tapping records are also reported. The same resistances were used as in the preceding table.

TABLE XXII

Resistance of Key	50 Grams		500 Grams		1,000 Grams		1,500 Grams	
		M. V.		M. V.		M V.		M. V.
Av. reaction time for H. ..	71.3	12.6	92.2	12.6	102.3	12.	108.6	13.
Av. reaction time for M. ..	98.1	11.8	116.5	8.4	130.5	11.5	139.3	10.2
Av. reaction time for B. ..	75.	12.	89.6	10.4	102.9	10.4	119.8	9.2
Av. reaction time for all ..	81.7		99.3		111.9		123.2	
Mean variation for all	14.23		10.49		11.22		10.86	
Av. No. of taps in 5 seconds for all	49.6		49.2		44.8		41.8	

Table XXIV. gives the averages of reactions when the ergograph was used in the place of a reacting key. In the ergograph the joint of the index finger of the right hand was used to produce the move-

TABLE XXIII

Resistance of Key	50 Grams	500 Grams	1,000 Grams	1,500 Grams
Average excess for H.	711.2	864.1	1,081.2	1,171.
Average excess for M.	854.0	970.5	802.0	891.6
Average excess for B.	814.1	1,327.9	1,202.9	935.3
Average of all readings	793.2	1,051.1	1,028.7	998.8

TABLE XXIV

Resistance in Grams, 31.8 mm. from Center of Rotation of Joint	Reaction Time M.	Reaction Time H.
1,000	85.0	88.3
1,500	85.5	109.6
2,000	94.1	117.3
2,500	101.8	121.6
3,000	103.3	136.5
3,500	112.3	147.3
4,000	112.8	157.6
4,500	124.9	164.2
5,000	154.0	183.3
5,500		209.3
6,000		216.8

ment. This table shows the averages only. This series was the last taken, the subjects thus had had the practise of all previous experiments.

Near the limit of the muscles' ability to lift the weight, the reaction time is evidently nearly double what it is for the lighter weights.

It may be interesting in connection with this series of reactions to notice the practise effects. As has been found in many other measurements, the greatest improvement is in those processes which start at the lowest levels, that is, the greatest reduction of reaction time is in those persons whose time was longest at first. The practise effect also appears more influential in the slightly more complicated reactions. Most of the subjects accomplished the greatest part of their practise in the first twenty-five reactions, although for these experiments usually more than one hundred practise reactions were used. Three samples of these practise sets were selected at random showing the result of the practise.

	Average for First Twenty-five Reactions	Average for Last Seventy-five Reactions
A.	125	105
B.	119	114
C.	173	133

The reason so few cases have been given is that the other subjects, at some time or other, had had some experience in experiments of this kind. This practise consisted only in getting an acquaintance with the apparatus and in finding what the movement really is. Further

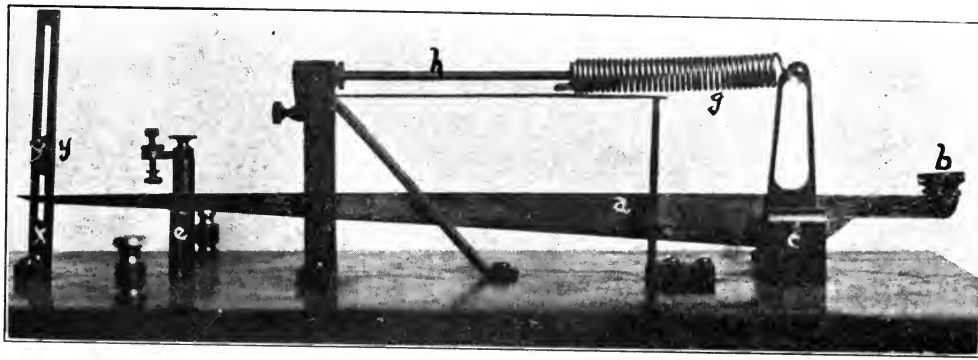

Fig. 9.

slight practise effects also appear in continued repetition of the experiment. The progress in the later practise periods becomes much slower. Often it fails to show decreases in the reaction time at all.

The following table shows the effect of practise on the excess pressures. To measure the effect of practise, the average excess for each successive day was found, the subject going through the programs outlined at the first of this chapter.

TABLE XXV

Successive Days	B.	M.	H.
1	1,187.5	1,493.0	1,869.37
2	880.0	1,321.25	1,333.67
3	1,120.0	655.62	921.25
4	911.2	605.0	661.25
5	758.7	480.62	827.5
6	653.7	694.37	
7	884.3		

It will be observed that economy in the use of energy is obtained. From the table it is evident that there is a reduction of excess movement in the reactions from day to day. The movements were as nearly alike as they could voluntarily be made. The stimulus to which the agent reacted remained constant. The task was so simple that it could not very well be misunderstood, so practically the only remaining factor in which improvement could be shown was the control of the amount of energy necessary to move the key against the given resistance.

The work on the influence of the resistance of the key was continued by using the release type of reaction. The subject, here, would hold the key down against the pressures in Table XXVI., and when the auditory stimulus was given would release the key as quickly as he could.

TABLE XXVI

Resistances	A	B	C
100	107.4	148.9	
	9	*12*	
500	118.3	129.1	140
	11	*9*	*12*
1,000	115	132.8	149
	17	*8*	*11*
1,500		130.4	140
		17	*22*
2,000			143
			14

It will be seen from a study of this table that the variation of resistance against which the key is held does not make a marked difference in reaction time when the release movement is used. The results are uniform even though the resistance increased. It will be remembered that there is a regular increase in the time of reaction with an increase of resistance in using the positive type of movement.

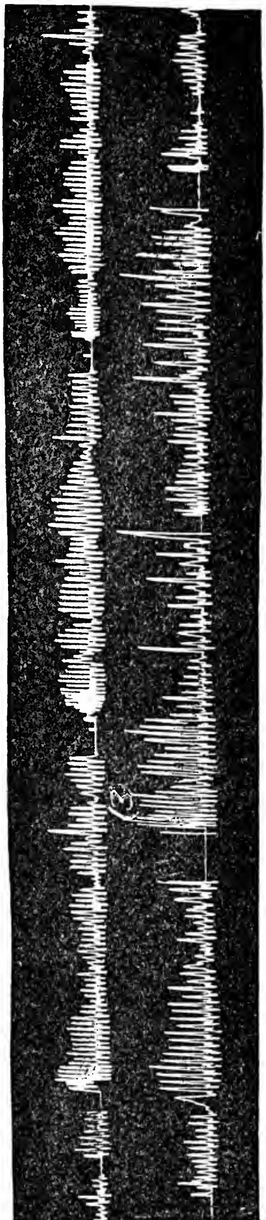

Fig. 10.

In Fig. 10 is given a part of the kymograph record of the excess pressure used in tapping. It shows that the curve runs in a wave-like form and consequently the excess, or surplus force was expended in rhythmic or pulse like beats. One curve running for five seconds began at an excess of 500 grams which increased after about five taps to an excess of from 800 to 1,200 grams, then dropped again to 50 grams and then rose again to the original height. The kymograph record shows that the greatest excess pressure was made at intervals of from ten to twenty taps. The increase or decrease of a series of averages of excess is usually regular, yet isolated high and low averages sometimes occur.

From the above tables of results, we may draw the following conclusions:

1. Reaction time for the positive movements, within the limits employed, is lengthened or shortened, respectively, when the resistance of the reacting key is increased or decreased. The resistance of the key should, therefore, be made definite, and should be stated when reaction measurements with this type of movement are taken.

[5] Most of the research studies in reaction time have been made upon the Hipp chronoscope. Fortunately this requires a key that breaks the circuit and since the breaking is accomplished most easily by simply releasing the ordinary keys usually used, the readings, for the most part, have been for the release type of movement.

2. For the release type of movement, it is evident that the variations of resistance affect the time very little, therefore, if comparable data are desired, this is the better type of reaction to use.[5]

3. The rate of tapping is fastest when minimal resistances are employed, and it increases when the resistance is increased.

4. The excess force used in a reaction movement does not seem to vary with the resistance, in other words, it is largely independent of it.

5. The graphic records showing the excess exhibit a tendency towards rhythm, especially in tapping.

6. The excess diminishes (more or less regularly) with practise.

A peculiar fact (perhaps worth noting) is that with the ordinary key M.'s records were longer, while with the ergograph his records were shorter than H.'s. The evidence will be found in detail in Tables XXII. and XXIV.

Some work on this and closely related problems has already been done. In 1892, Féré[6] arrived at the following conclusions: For one and the same subject, the reaction time is longer when the weight to be lifted is heavier—provided that the weight is not known beforehand. When however the weight to be lifted is known to the subject beforehand, the length of the reaction time does not vary with the weight, but with the capability of the subject to adapt his attention. It was found in our experiment, that after the subject had learned the resistances his reaction time still increased as the resistance was increased.

Helmholtz found that the total muscular force was not developed instantaneously. Haycraft,[7] working upon this problem, found that if the muscle is lightly loaded, the muscular force sufficient to raise the weight will be developed approximately in one hundredth of a second; if it be loaded with a heavier weight, the greater muscular force required to raise it will not be developed for some three or four hundredths of a second.

The amount of pressure was measured by Delabarre[8] in his experiment on the force and rapidity of reaction movement. He made a study of temperaments by taking the reaction time itself, and the degree of pressure used by the subject, and the rapidity with which he contracted his reacting muscles. The degree of pressure in this case was measured by the height of a mercury column forced up by the reacting movement. In this experiment as well as the one reported by Féré, we have the force of inertia to overcome at the be-

[6] *Comptes Rendus de la Société de Biologie*, 1892, 9th series, **4**, 432.

[7] *Journ. of Physiol.*, **23**.

[8] Delabarre, Logan and Reed, *Psychol. Rev.*, 1897, **4**, 615.

ginning of the reaction movement, while in the experiment reported in this paper, all resistances were made by the varying tensions of springs, thus reducing the effect of inertia.

If we attempt an explanation of the effects observed, one of the possible ones is that the tip of the finger is capable of considerable compression before the key actually moves, which would make the reactions to the heavier resistances longer than to the lighter. This may be true to a slight extent in all such experiments, but it plays only a minor rôle where the ergograph is used, since here the finger is placed very snugly in a thimble and thus compression is prevented.

It has been shown in physiological experiments that muscles have a certain amount of elasticity. Lombard, in speaking of the effect of different weights on the gastrocnemius muscle of a frog, says: "There can be no movement of the lever until the inertia of the weight has been overcome and the first effect of the contraction is to stretch the muscle, a part of the energy of the contraction being changed to elastic force, which on the recoil assists in raising the weight. Thus the myograph may fail to reveal the instant that the contraction starts. In as much as the tension increases the activity of muscle protoplasm, it is probable that the presence of the weight really hastens the liberation of energy at the same time that it delays the recording of the contraction." This seems to be a very probable explanation for the longer reaction times which appear when the resistance is increased.

A further explanation may lie in the fact that the nervous impulse itself is a thing of volume and requires time for formation and for conduction, consequently then the greater the strength of the impulse demanded by the movement the longer the reaction time will be. The subjects all reported that they felt the constantly increasing sense of effort when reacting against the greater resistances.

An explanation of the results in the release type of reactions involves a further discussion of the psychology of movement.

Since the finger is relaxed in both the ergograph and in the downward pressure type of reaction and since the key is at rest, no sensations anticipatory of the amount of innervation to be put forth can arise; consequently the first impulse from the motor centers may not be sufficiently strong to release a movement powerful enough to overcome the resistance employed. When this impulse is found to be insufficient, reinforcement immediately occurs and the proper releasing movement occurs. In reacting against resistances a summation effect is thus required which is greater the greater the resistance employed. This process of summation requires time and thus the longer reaction times to increasing resistances are explained.

On the contrary, in the release type of reactions there are coming continually certain sensations indicative of the amount of effort required to keep the circuit closed. If the pressure is reduced below the minimal point a premature reaction takes place and since very few such reactions occurred we must conclude that the subjects were most of the time exerting more than the minimum pressure on the key. Yet the excesses in any case probably were not exceedingly great for in every instance the subject pressed the key down until the contact was made and held it there until the stimulus was given for him to release it. So for each reaction the subject had received sensations anticipatory of the movement required to release the weight. According to the results of Féré[9] the fact that the weight is known beforehand causes the increase in resistance to cease. This may be the important factor in causing uniformity in this type of reaction. In the positive movement, however, it would seem that the subject would soon learn his weight by reason of the fact that he employed each weight in a series of reactions.

In the course of practise it is noted that the excesses do decrease on successive days as the experiment progresses. But this additional knowledge did not serve to compensate for or to destroy the variations caused by changing the resistance of the keys.

The factor of elasticity of the muscle is eliminated to a great extent in this type of reaction, which accounts partially for the differences found.

There is another important fact to be considered in explaining the uniformity of the release type of reaction. Experiments show that the subject in virtue of the approximate accuracy of his anticipatory sensations is rarely exerting more than two hundred grams of excess pressure. Thus if the actual resistance is two hundred grams he is usually exerting three hundred to four hundred grams while if the actual resistance is fifteen hundred grams he exerts only sixteen hundred to seventeen hundred grams of pressure. Accordingly the first innervation sent to the muscles upon the incidence of a stimulus is approximately the correct one; time-consuming summation processes are consequently not necessary to the release type of reaction.

[9] *Op. cit.*