

a broad culture none the less deep because of its breadth, even if he has had time for the analysis of but one specimen, while the other almost inevitably results in confining his labors and his attainments within narrow limits. Whenever it can be done, a determination of any sort should be made by two processes as nearly independent of each other as possible. For example, the radius of curvature of a lens might be determined by comparing the size of an object, as a scale, with that of its image formed by reflection from the lens surface; and it might be calculated from spherometer measurements. While there are some points in physics which the progressive method would reach and the method of analysis miss, the latter would the more readily lend itself to such twofold determinations.

There are operations such as the calibration of a thermometer, determining a rate of vibration, adjusting special forms of apparatus and determining their constants, etc., that cannot be classified in any simple manner. An attempt to adhere strictly to any clearly defined method throughout the whole course of physics would be unwise and unprofitable. The recognition of a method and of its legitimate limits, however, cannot fail to be of service to a judicious instructor. The limitations of the laboratories themselves in many cases compel a departure from any method and cause the work to degenerate into an unsystematic performing of experiments. It must be admitted, too, that such is the character of the work in some instances where the equipment is very complete.

A NEW METHOD OF CHILD STUDY.

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THE current discussions of the more elementary mental processes show that we lack clearness in our conceptions of the earlier stages of mental life. This is evident enough to call out frequent appeals for "scientific" child study. The word "scientific" is all right, as far as it goes; but as soon as we come to ask what constitutes scientific child study, and why it is that we have so little of it, we find no clear answer, and we go on as before accepting the same anecdotes of fond mothers and repeating the inane observations of Egger and Max Müller.

Of course there are only two ways of studying a child, as of studying any other object—observation and experiment. But who can observe, and who can experiment? Who can look through a telescope and "observe" a new satellite? Only a skillful astronomer. Who can hear a patient's hesitating speech and "observe" aphasia? Only a neurologist. Observation means the acutest exercise of the discriminating faculty of the scientific specialist. And yet most of the observations which we have in this field were made by girls who, before their marriage, knew less about the human body than they did about the moon or a wild flower (having got this latter information from Steele's "Thirteen Weeks") or by a father who sees his child when the boy is dressed up, for an hour a day, and who has never slept in the same room with him in his life; by people who never heard the distinction between reflex and voluntary action, or that between nervous adaptation and conscious selection. Only the psychologist can "observe" the child, and he must be so saturated with his information and his theories that the conduct of the child becomes instinct with meaning for mind and body.

And as for "experiment," greater still is the need. Many a thing a child is said to do—a little judicious experimenting—a little arrangement of the essential requirements of the act in question—shows it is altogether incapable of doing. But to do this we must have our theories, and have our critical moulds arranged beforehand. That most vicious and Philistine attempt in some quarters to put science in the straight-jacket of barren observation, to shut out the life-blood of all science—speculative advance into the secrets of things—this ultra positivistic cry has come here as everywhere else, and put a ban upon theory. On the contrary give us theories, theories, always theories! Let every man who has a theory pronounce his theory! This is just the difference between the mother and the psychologist—she has no theories, he has. She may bring up a family of a dozen and

not be able to make a single thrustworthy observation: he may be able from one sound of one yearling to confirm theories of the neurologist and educator, which are momentous for the future training and welfare of the child.

In the matter of experimenting with children, therefore, our theories must guide our work—guide it into channels which are safe for the growth of the child, stimulating to his powers, definite and enlightening in the outcome. All this has been largely lacking, I think, so far, both in scientific psychology and in applied pedagogy. The implication of physiological and mental is so close in infancy, the mere animal can do so much to ape reason, and the rational is so helpless under the leading of instinct, impulse, and external necessity, that the task is excessively difficult—to say nothing of the extreme delicacy and tenderness of the budding tendrils of the mind. Experiment? Every time we send a child out of the home to the school, we subject him to experiment of the most serious and alarming kind. He goes into the hands of a teacher who is not only not wise unto the child's salvation, but who is on the contrary a machine for administering a single experiment, to an infinite variety of children. It is perfectly certain that two in every three children are irretrievably damaged in their mental and moral development in the school; but I am not at all sure that they would fare any better if they stayed at home! The children are experimented with so much and so unwisely, anyhow, that it is possible that a little experiment, intentionally guided by real insight and psychological information, would do them good.

With this preamble, I wish to call attention to a possible method of experimenting with young children, which has not been before noted to my knowledge. In endeavoring to bring questions like the degree of memory, recognition, association, etc., present in an infant to a practical test, considerable embarrassment has always been experienced in construing safely the child's responses. Of course the only way a child's mind can be studied is through its expression—facial, lingual, vocal, muscular; and the first question, i.e., What did the infant do? must be followed by a second, i.e., What did his doing that mean? And the second question is, as I have said, the harder question, and the one which requires more knowledge and insight. It is evident, on the surface, that the farther away we get in the child's life from simple inherited or reflex responses, the more complicated do the responsive processes become, and the greater becomes the difficulty of analysing them, and arriving at a true picture of the real mental condition which lies back of them.

To illustrate this confusion, I may cite about the one problem which psychologists have attempted to solve by experiments on children, i.e., the determination of the order of rise of the child's perception of the different spectral colors. Preyer starts the series of experiments by showing a child various colors and requiring the child to name them, the results being expressed in percentages of true answers to the whole number. Now this experiment involves no less than four different questions, and the results give absolutely no clue to their analysis. It involves, 1, the child's distinguishing different colors simultaneously displayed before it (i.e., the complete development of the child's color sensation apparatus); 2, the child's ability to recognize or identify a color after having seen it once; 3, an association between the child's color-seeing and word-hearing memories, by which the name is brought up; 4, equally ready facility in the pronunciation of the various color names which the child recognizes: and there is the further embarrassment, that any such process which involves association, is as varied as the lives of children. The single fact that speech is acquired long after objects and some colors are distinguished, shows that Preyer's results are worthless as far as the problem of color perception is concerned.

That the fourth element pointed out above is a real source of confusion is shown by the fact that children recognize many words which they cannot pronounce readily. Binet, who represents the second phase in the development of this experimental problem, realized this, and varied the conditions by naming a color and then requiring the child to pick out the corresponding color. This gave results different not only from Preyer's, but also

from those which Binet reached by Preyer's method. For example, Preyer's child identified yellow better than any other color, a result which no one has confirmed.

The further objection that colors might be distinguished before the word association is established at all, is also seen by Binet, and his attempt to eliminate that source of error constitutes what we may call the third stage in the statement of the problem. He adopts the *méthode de reconnaissance* as preferable to the *méthode d'appellation*. This consisted, in his experiments, in showing to a child a colored counter, and then asking the child to pick out the same color from a number of different colored counters.

This reduces the question to the second of the four I have named above. It is the usual method of testing for color-blindness. It answers very well for color-blindness; for what we really want to learn in the case of a sailor or a signal-man is whether he can recognize a determined color when it is repeated; that is, does he know green or red to be the same as his former experience of green or red. But it is evident that there is still a more fundamental question in the matter—the real question of color perception. It is quite possible a child might not recognize an isolated color quality when he could really very well distinguish color qualities side by side. It is the question just now coming to the front, the question of absolute *vs.* relative recognition, or immediate *vs.* mediate recognition. The last question is this: When does the child get the different color sensations (not recognitions) and in what order?

A further point of criticism of Binet's results serves to illustrate my argument. Binet rules out the influence of the word memories which were necessary to Preyer's results by his *méthode de reconnaissance*. The child recognizes again the color just seen. Now any one who has followed the course of recent discussions of recognition must know that the mediation of word associations is not ruled out in these cases in children of 3 to 5 years old or even younger. Lehmann finds colored wools are recognized when the colors are those whose names are known (*Benennung's association*), and that shades which have not peculiar names, or whose names are not known, are not recognized. Scripture has shown that an unobserved or unintelligible element—a *Nebenvorstellung*—may serve as the link of recognition without rising again to clear consciousness a second time. It is, of course, useless, if these results be trustworthy, to attempt to get recognitions clear of word memories after color names have once been learned by the child. It would seem that the question ought to be taken up with younger children. Binet's experiments were in the interval between the child's 32d and 40th weeks. It is perhaps a confirmation of Lehmann's position, that the colors least recognized in Binet's list are shades whose names are less familiar to children: his list, in order of certainty of recognition, is red, blue, green, rose, maroon, violet, and yellow by the *méthode d'appellation*; and, by both methods together, red, blue, orange, maroon, rose, violet, green, white, and yellow.¹

This color question may suffice to make clear the essentials of a true experimental method. Only when we catch the motor response in its simplicity is it a true index of the sensory stimulus in its simplicity. I have accordingly attempted to reach a method of child study which would yield a series of experiments whose results would be in terms of the most fundamental motor reactions of the infant, which could be easily and pleasantly conducted, and which would be of wide application. The child's hand-movements are, I think, the most nearly ideal in this respect. The hand reflects the first stimulations, the most stimulations, and, becoming the most mobile and executive organ of volition, attains the most varied and interesting offices of utility. We have spontaneous arm and hand movements, reflex movements, reaching-out movements, grasping movements, imitating movements, manipulating movements, and voluntary efforts—all these, in order, reflecting the development of the mind. The organs of speech are only later brought into use, and their use for speech involves an already high development of mind, hence the error in Preyer's results. It has accordingly seemed to me worth while to find whether a child's reaching movements would reflect

with any degree of regularity the modifications of its sensibility, and, if so, how far this could be made a method of experimenting with young children.

Before speaking, however, of applications, I may adduce one or two other considerations which tend to show that some such dynamogenic method is theoretically valid. Féré showed that sensory stimulations of all kinds increase the maximum hand-pressure. Colors (seen) have regular and each its peculiar effect upon movement. Tones have similar influence. The ticking of a watch is more clearly perceived if a sound is heard at the same time. Further, the reaction-time of hand-movements is shorter if the stimulus (sound, etc.) be more intense. There is an enlargement of the hand, through increased blood-pressure, when a loud sound is heard. These, and a variety of other facts upon which the law of dynamogenesis rests, seem to afford justification for the view that the infant's hand-movements (say) in reaching and grasping will be an index of the kind and intensity of its sensory experiences. Magendie² long ago suggested measuring changes in sensibility by the corresponding changes in blood-pressure.

Further, it is not necessary to embarrass ourselves with the question whether the hand-movements are voluntary or not. However we may differ as to the circumstances of the rise of volition, it is still true that after its rise the child's reactions are for a long time quite under the lead of its sensory life. It lives so fully in the immediate present and so closely in touch with its environment, that the influences which lead to movement can be detected with great regularity. In this case the sensations, which are movement-stimuli, become what we may call "effort-stimuli," and the child's hand-efforts become our indications of the relative degree of discrimination, attractiveness, etc., of the different sensations.

Suppose we hang a piece of meat up over Carlo's head and tell him to jump for it. His first jump falls short of the meat. He jumps again and clears a greater distance. Why does he jump farther the second time? Not because he argues that a harder jump is necessary to secure the meat, but because by the first jump he got more smell, blood-color, and appetite-stimulus from the meat. Now suppose it be a red rag instead of meat, and Carlo refuse to jump a second time. This is not because he concludes the rag would choke him, but because he gets a kind of sensation which takes away what appetite-stimulus he already had. The thing is a thing of sensational dynamogeny or "suggestion," and the child-state up to his 24th month (more or less) is just about the same.

The following questions, I think, might be taken up by this method:—

1. The presence of different color-sensations as shown by the number and persistence of the child's effort to grasp the color.
2. The relative attractiveness of different colors measured in the same way.
3. The relative attractiveness of different color combinations.
4. The relative exactness of distance-estimation as shown by the child's efforts to reach over distances for objects.
5. The relative attractiveness of different visual outlines (stars, circles, etc.) cut in the same attractive color, etc.
6. The relative use of right, left, and both hands.
7. The rise of imitative movements.
8. The rise of voluntary movements.

I am quite aware of the meagreness of this list; but one has only to remember the fact that there is no such thing yet as a psycho-physics of the active life, that this side of psychology is *terra incognita* to the experimentalist.³ If the method proves reliable in one-half of these questions, then so much gain. I have applied it to questions 1, 2, 4, 6, and 7 with results, some of which I have already published in this journal. Other papers will be devoted to these detailed applications.

² Féré, "Sensation et Mouvement," p. 56.

³ I see no reason that a method could not be devised for testing the motive influences of presentations in terms of the time elapsed since their experience. I have announced elsewhere (*Proceedings of Congress for Exper. Psychology*, London, 1892), the first results of a research conducted upon adults by such a method.

¹ Calculated from Binet's detailed results (*Revue Philosophique*, 1890, II., 582 ff.) by Mr. F. Tracy.