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INFLUENCES OF JOHN B. WATSON'S BEHAVIORISM ON CHILD PSYCHOLOGY

LAS INFLUENCIAS DEL CONDUCTISMO DE JOHN B. WATSON EN LA PSICOLOGÍA INFANTIL

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Abstract

Watson's 1913 manifesto, and later elaborations of it, changed child psychology into a natural science based on experimental research and stimulus-response theorizing. These influences probably resulted partly from the philosophical and theoretical attractiveness of a natural science approach, partly from the objectivity and persuasiveness of an experimental approach, and partly from misunderstandings and misrepresentations of his behaviorism. These points are discussed in the first two major sections of this paper, respectively on Watson's influence on child psychology in general and, as a concrete illustration, his influence specifically in the domain of emotions and emotional development. The latter section shows, for example, that misinterpretations of Watson's theory of emotions led to many experimental investigations in an area that had been overwhelmingly nonexperimental. The final section is a ruminative summary in that its conclusions come largely from considerations given in the first two sections but also partly from considerations not covered there.

Keywords: John B. Watson, behaviorism, child psychology, emotions, emotional development

Resumen

El manifiesto de Watson de 1913, y las elaboraciones posteriores de éste, cambiaron la psicología infantil a una ciencia natural basada en investigación experimental y la elaboración de teorías de estímulo-respuesta. Estas influencias

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probablemente fueron el resultado en parte del atractivo filosófico y teórico de un enfoque de una ciencia natural, en parte de la objetividad y la persuasión de un enfoque experimental y en parte de los malos entendidos y las malas interpretaciones de su conductismo. Estos puntos se discuten en las dos primeras secciones principales del presente trabajo: La influencia de Watson en la psicología infantil en general y como un ejemplo concreto, su influencia específica en el dominio de las emociones y del desarrollo emocional. Esta última sección muestra, por ejemplo, que las malas interpretaciones de la teoría de Watson de las emociones condujeron a muchas investigaciones experimentales en un área que ha sido abrumadoramente no experimental. La sección final es un resumen reflexivo en el que las conclusiones se derivan en gran medida de las consideraciones que se establecen en las primeras dos secciones, pero también se derivan en parte de consideraciones que no se abarcaron en ellas.

Palabras clave: John B. Watson, conductismo, psicología infantil, emociones, desarrollo emocional

John B. Watson's "Psychology as the Behaviorist Views It" has often been called his "behaviorist manifesto" — Woodworth (1931, p. 69) seems to credit the phrase to himself — but the article had little direct impact on traditional child psychology. Perhaps the reason is that most child-study professionals in that era would not have read much if any beyond the first two sentences: "Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior" (Watson, 1913, p. 158). The traditionalists' primary goal was to discover norms of development, which requires observational research. Of course, all psychological research is "observational" and "behavioral" in that it involves observation of behavior or behavioral artifacts, but "observational" is used here in the sense contrasted with "experimental" (e.g., Anderson, 1931; Reese & Lipsitt, 1970, p. 13; Watson, 1914, pp. 30-31 & title of chap. 4, p. 106) and "behavioral" is used to refer to Watsonian behaviorism and later behaviorisms.

Watson remained keenly interested in making psychology a natural science (e.g., 1924-1925, p. 119; 1925a, p. 127; 1930, p. 161; p. 1 in 1919a, 1924, 1929a, 1929b). As a natural science, psychology would be an experimental science without mentalistic ghosts in the behavioral machine (Bergmann, 1956). This program was maintained by later behaviorists (Horowitz, 1991, 1992) and it included an emphasis on environmental determinants, primarily stimulus-response conditioning (he did not call it that in Watson, 1914, but he endorsed the gist of the method, which he called *substitution*, pp. 65-68, 269-273). The emphasis meshed well with the experimental approach because in humans environmental determinants are obviously more likely than hereditary determinants to be controllable experimentally — real experiments, not the *experiments of nature* that flourish in behavioral genetics (the italicized phrase is an oxymoron because *nature* is an abstract superordinate con-

cept and therefore it cannot do experiments). Environmentalism also meshed well with Watson's call for observability and rejection of instincts and other kinds of will-o'-the-wisp for which an assumed hereditary basis was an excuse for ignorance of causes rather than a testable explanation. Nevertheless, in the 1913 manifesto Watson gave heredity a role (p. 167) and he later (1916c, p. 485) said that humans have "instincts" that are "hereditary." In the 1920s Kuo Zing-Yang criticized him for not being more of an environmentalist. Kuo said, "There are only one and a half true behaviorists in this world; Watson is the half. I am the only true behaviorist" (quoted in Chin & Chin, 1969, p. 8). Kuo (1921) gave heredity no role, until later (Kuo, 1967/1976).

Watson's emphasis on environmentalism and the ubiquitous role of conditioning became immensely popular in psychology, in part because, as Kagan (1983) said, "Conditioning emphasized the role of experience, not biology, in promoting both change and stability of psychological attributes" (p. 550). Kagan also noted the rise of philosophical pragmatism in the early 20th century and he said, "Benevolent environmental intervention, which is central to the pragmatic view, is obviously in accord with the central premise of behaviorism" (p. 551). Stevenson (1983, p. 215) implicitly made the first of these points and omitted the second one. He said that Watson's behaviorism was an "applied science," and "In the 1920s, such a scientific, pragmatic approach was readily acceptable to Americans, for whom science seemed to have limitless potential." That is, Stevenson emphasized Watson's natural science approach and used "pragmatic" only in its meaning of *practical*. Relevant to the second point, Watson said in the 1913 manifesto that behaviorists can and should do experimental research on matters related to advertising, drug abuse, forensics and jurisprudence, pedagogy and test construction, and psychopathology. However, he added that behaviorists are not concerned with application of the facts discovered and that the practitioners must "decide whether these facts are ever to be applied" (p. 169). These remarks indicate that at first, Watson envisioned behaviorism not as an applied science but as a pure science. These points are discussed further in the two major sections of this paper, respectively on Watson's influence on child psychology and, as a concrete illustration, Watson's influence on the study of emotions. The study of emotions is especially instructive for two major reasons covered in the latter section and a third major reason developed in the final section. First, in the 1913 manifesto Watson seems to dismiss this domain, yet his two most famous experiments (Watson & Morgan, 1917; Watson & Rayner, 1920) dealt directly with emotions in human infants and indirectly with emotional development throughout childhood and in adulthood. Second, these two experiments and his other writings about emotions and emotional development nicely exemplify his influence on child psychology in general, leading in this domain to a shift from traditional observational research and an emphasis on genetic determination to experimental research and an emphasis on learning. Third, later changes in this domain illustrate how his influence became modernized without abandonment of the behavioral base.

Watson's Influence on Child Psychology

As already mentioned, in the early years of behaviorism most child-study professionals were not interested in pure objectivity and experimentally verified methods of predicting and controlling children's behaviors. However, Watson's 1913 manifesto had a major indirect impact on the development of a new child psychology, mediated by his later papers and books that expanded upon the behavioral psychology he envisioned in the manifesto. Some of his works that might have been especially effective in this respect are his 1917 chapter "Practical and Theoretical Problems in Instinct and Habit" (Watson, 1917), his 1919a *Psychology from the Standpoint of a Behaviorist*, and his 1925a and 1930 editions of *Behaviorism*. Interest in these works might have been generated by his publications in professional journals outside psychology, such as *The Kindergarten and First Grade* (Watson, 1920), *The Scientific Monthly* (Watson & Watson, 1921), and *The Pedagogical Seminary* (Watson, 1925b, 1925c, 1925d), and his books produced for the popular market-*Psychological Care of Infant and Child* (Watson, 1928a) and *The Ways of Behaviorism* (Watson, 1928b). The traditional child-developmentalists might have been more likely to read these journals and books than the scientific psychology journals and books. Incidentally, *Psychological Care* has been called a bestseller (e.g., Cairns, 1983, p. 71; B. Harris, 1984), but I have found no citation of sales figures or other relevant evidence. Perhaps it was a bestseller, or perhaps some early commentator remarked that it was a popular book, intending to mean a book written for *the general public* rather than for academics or professionals, but misread by some readers to mean *widely accepted*, that is, widely purchased (the italicized phrases are from *Merriam-Webster's*, 1998, p. 906). The point is that factual statements tend to need documentation more often than it is given.

Background on Child Psychology

The history of child study in the first few decades of the 20th century indicates two main approaches: child psychology as a descriptive science versus a natural, experimental science (e.g., D. B. Harris, 1956/1963; Laidlaw, 1960; Penney, 1960a, 1960b; for fuller discussion and other references, see Reese, 1970, 1991, 1993). These approaches have been given various names; for example, "developmental psychology" versus "behavior theorist working in child psychology" or "child psychology" (D. B. Harris, 1956/1963), "traditional child development" versus "experimental child psychology" (Penney, 1960a), "normative"-"historical"/"semihistorical"-"naturalistic" versus "explanatory"-"ahistorical"-"manipulative" (McCandless, 1961, pp. 36-39), and "developmental" versus "experimental manipulative" (Lipsitt, 1970b, pp. 20-24, 28-30). These names seem too cumbersome or too vague. The first approach in the contrasts was the historically earlier one and therefore "traditional" is an appropriate adjective. Calling the second approach "experimental" is misleading in that much of

its research was observational, but nevertheless its distinguishing characteristic was experimental research. Most experimental child psychologists were behaviorists, but calling the approach "behavior theorist" or "behavioral" would be loose because some were Watsonian behaviorists (e.g., H. E. Jones, 1931; M. C. Jones, 1924a, 1926), and others were, for example, Skinnerian behaviorists (e.g., Bijou, 1955) and Tolmanian cognitive behaviorists (e.g., Tolman, 1932). On balance, I think the best short names are "traditional approach" and "experimental approach."

The traditional and experimental approaches had different preferences regarding primary general concerns, associated specific topics, research aims, and methods, and different theoretical-explanatory preferences. These characteristics are discussed in the following two subsections. To forestall surprise in the third of the following subsections, I would emphasize here that the differences were not absolute but rather were in *preferences*--traditionalists did not completely eschew experimental research and experimentalists did not completely eschew observational research.

The Traditional Approach

The major concern in the traditional approach was broadly "Psychology in the Service of Man," as Laidlaw (1960) said, and more specifically, normal development (ontogenesis), child rearing, child welfare, and other home-economics kinds of issues related to child development. The dominant research themes were psychological stages, or states, and age changes in states; the dominant research aims were norm-gathering to describe the states and the changes with age; and the dominant research methods were observational (Bijou, 1955).

These characteristics are nicely reflected in slogans of early institutes of child development. I cite four examples. The *Teachers College* in New York was incorporated in 1892 and by the turn of the century it had become a unit in Columbia University. Its focus was on "marrying a humanitarian concern to help others with a broad-based scientific approach to human development" ("TC: A legacy of innovators," n.d.; "Teachers College," n.d.). The *Iowa Child Welfare Research Station* was founded in 1917 at the State University of Iowa "with the social goal of providing the people of Iowa with information that would assist them in developing their greatest resource: their children" (H. H. Kendler, 1991, p. 3) or more fully, "for the purpose of obtaining scientifically valid and practically useful data on children between 2 and 6 years of age" (T. S. Kendler, 1991, p. 93). The *Merrill-Palmer School of Motherhood and Home Training* (later named the Merrill-Palmer Institute) was founded in Detroit Michigan in 1920 "to prepare young women as wives and mothers, but under Edna Noble White's leadership [she was its director until 1947], it expanded its services to include a student program of college level courses in child development, family life, parenting skills, and nursery education" ("Merrill-Palmer Institute," n.d.). The *Institute of Child Development and Welfare* at the University of Minnesota was originated in 1925 for "the promotion of child welfare" ("Institute," n.d.).

The traditionalist observational methods included direct or indirect observations in the home, hospital nursery, school, research laboratory, and so on, done by either relatively untrained observers such as parents (already mainly the mothers) or more or less well trained observers. Observation is *direct* if done in the natural environment with no imposed intervention or intrusion other than the presence of the observer(s), thus excluding the use of questionnaires, interviews, standardized tests, and experiments (Wright, 1960). Little of the traditionalist observational research involved direct observations (Wright, ibid.) and most of the rest seems to have involved *indirect* observations via questionnaires, interviews, and standardized tests (observations in experiments are also indirect, but this point is irrelevant here). Questionnaires were popularized in child psychology by G. Stanley Hall; interviews of children were popularized by Jean Piaget, but most often the interviews were of third-party informants such as mothers rather than the children themselves. The popularity of tests reflected the testing movement that followed from Alfred Binet's research and intelligence tests (Anderson, 1931; Cairns, 1983, pp. 46-51).

The earliest use of direct observation in child psychology seems to have been in the "baby biographies," for which a self-selected observer kept daily or otherwise periodic records of a single infant's behavior (paraphrased from Anderson, 1946, p. 2, who cited 11 examples). The observer was usually a parent who was sometimes also a scientist; an example is Professor William Preyer (1890/1973). A related direct method, much more popular than baby biographies as such, was to ask mothers or other observers to keep diaries on situations and reactions presumably related to specified topics, such as emotions (e.g., in Jersild & Holmes, 1935b). However, the usual method was indirect observation in questionnaires (e.g., in Hagman, 1932) or interviews (e.g., in Jersild & Holmes, 1935c, 1935e) involving informants who were assumed to be well informed but who were not necessarily well trained in objectivity. The informants were usually the mothers but sometimes were the children themselves, teachers, or siblings or other relatives. The information about emotions, for example, was frequencies of reported occurrences of emotion-relevant situations and reactions and sometimes other reported details such as durations, and in some studies techniques that informants reported were used to deal with emotions and the reported efficacy of these techniques. The aim was to obtain norms about occurrences of emotions and techniques for controlling emotions.

The favored explanations were hereditarian — also called "genetic," which is confusing because "genetic" means not only "hereditary" as in Watson's reference to "The Genetic System" (1930, p. 52) but also "developmental" as in Watson's reference to "the genetic psychologist" (1930, p. 96), in Pratt, Nelson, and Sun's (1930, p. 6) phrase "genetic development" contrasted with cross-sectional development (not their phrase), and in the title of *The Journal of Genetic Psychology*.

One kind of hereditarian explanation was the attribution of observed behaviors and inferred mental processes to *instincts*, as in William McDougall's "purposive psychology" (e.g., 1926) and in William James's (1890, Vol. 2, chap. 24) discussion of some 32

instincts, from acquisitiveness to walking. Hereditarianism is also seen in the attribution of changes with age to "maturation," championed by Arnold Gesell (e.g., 1946) but never adequately defined (Kessen, 1984; McGraw, 1946). Early examples include H. E. Jones and M. C. Jones (1928, pp. 137, 142) and Shirley (1933, p. 238). Later examples include Michel and Moore (1978), who defined maturation as contrasted with experience (p. 47) and then said it is inseparable from experience (pp. 48, 93); and McNeil (1966, p. 18), who said, "maturation describes that part of development that takes place in the absence of specific experience or practice" (p. 19). Contrary to McNeil, maturation does not *describe* anything; it is intended to name the process by which change occurs when the process is not learning via experience or practice. The problem is that naming this nonlearning process does not indicate what the process is. The name was borrowed from physiology; it is well defined in that field when it refers to, for example, the myelinization of neurons (Parmelee & Sigman, 1983), but it is still problematic when it is contrasted with experience, because gene-environment interactions make maturation and experience interactive rather than contrastive (Gottlieb, 1983).

The Experimental Approach

Experimental methodology. Experimental methodology has been described in various ways, but one common element is that the experimenter uses active manipulations to create one or more "independent" variables or conditions (e.g., Lipsitt, 1970b; Reese, 1997; Schulze, 1912, p. 5; Woodworth, as documented by Winston, 1988). Accurate observation of outcomes — changes in "dependent" variables — is often mentioned and is at least as often implicit, as in requiring objectivity (e.g., Hart & Risley, 1995, chap. 2), repeatability (e.g., Gilmer, 1970, p. 26; Schulze, 1912, p. 3), or validity (Jersild & Holmes, 1935b, although referring explicitly to observational methodology). An element in some descriptions is that the experimenter must also hold other relevant conditions constant (e.g., Schulze, 1912, p. 235; Winston, 1988). This is an element in John Stuart Mill's Method of Difference (1872/1973, Bk. 3, chap. 8, sect. 2; 1973, p. 391), but he said that if the manipulated condition is introduced so quickly that the other conditions cannot reasonably be assumed to have changed, these other conditions do not need to be controlled formally. Nevertheless, he added a disclaimer — this variant leaves open the possibility that "the effect may have been produced not by the change [i.e., the manipulated condition], but by the means employed to produce the change" (p. 393; index to collating footnote deleted). Mill's method, variant, and disclaimer are seldom mentioned in reports of psychological research, perhaps because the element can easily be finessed by using a comparison condition (often misnamed a *control* condition) assumed to contain all constituents of the manipulated condition except the constituent being investigated. In group research, this finesse requires random assignment of research participants to each condition (e.g., Vasta, 1979, pp. 16-17) or, in both group and single-case research, administration of every condition to each research participant. In short, then, the key

elements are experimental control, objectivity, and random (usually quasi-random) or comprehensive assignment of conditions.

The description in the preceding paragraph is an idealization of laboratory research that most psychologists endorse on the mistaken belief that it is the model of research in the physical sciences (Smith, 1970). Many psychologists try to use this model and continue an experiment with the planned goals and predetermined procedures regardless of whether the goals are being approached and the procedures are effective. For example, English (1929) doggedly persisted with a stimulus that turned out to be ineffective. In contrast, "bench research" methodology is widely used in physical sciences such as physiology and chemistry and in behavior analytic psychology. In bench-research experiments, the researcher does not march lockstep through predetermined procedures, but rather develops the procedures as the study proceeds, modifying prior plans and introducing new plans in response to the observations. As Bakan (1967) said, "Truly good research means that one allows the investigation to be guided by the experiences of the investigation" (p. xiv). Psychologists who used bench-research methodology include Cason (1922), Holmes (1936), Hunter (1917), M. C. Jones (1924b), Moss (1924), Watson (1907, e.g., pp. 42-45, 98-100), Watson and Rayner (1920), and many operant conditioning researchers such as Orlando (1965 — see Ryan, 1970, pp. 131-132, for an informative summary).

Applications in child psychology. The major concern in the approach formally called "experimental child psychology" was about empirical laws of psychological phenomena, without any inherent interest in the child as such (Penney, 1960b). A major research theme of this approach was learning; the dominant research aim was to determine empirically how behaviors are affected by learning; the preferred research method was experimental (Penney, 1960b); and the favored explanations were environmentalistic. However, many experimental child psychologists studied the child as such in two variants of the main approach. In one variant the aim was to establish norms of development experimentally (e.g., Levinson & Reese, 1967); in the other the aim was to test hypotheses about the nature or effects of stages and changes in stages during childhood (e.g., Kuenne, 1946). Both of the parenthetically cited examples were *quasi*-experimental because although the experimental conditions were created by active manipulations, age was also an independent variable and it can be assessed but not actively manipulated. This is an unsolvable problem in all age-based research. The *Journal of Experimental Child Psychology* contained examples of the main approach and both variations.

Garrett (1930, p. 147) said that "a large share" of modern interest in the experimental approaches in child psychology was stimulated by "early experiments of Watson and his students" (1941, p. 292), later (1951, p. 127) modified to "much of the interest" and without mention of "his students." Two child psychologists also gave credit to Watson. Anderson (1931, p. 3) — a traditionalist — said that experimental child psychology began "in large measure" with the works of Thorndike and Watson. Stevenson (1983, p. 315) — an experimentalist — attributed the beginning only to

Watson. I think Stevenson's attribution is more accurate than Anderson's, but two points may merit emphasis: First, scientific psychological research with children began before Watson, and much of it was instigated by G. Stanley Hall's research with children late in the 19th century (Cairns, 1983, pp. 51-54). However, Hall's work was primarily observational, via questionnaires rather than experiments. Second, some truly experimental work was done with children before Watson's work began. For example, Wesley (1968) noted that in 1903 a German ophthalmologist, E. Raehlmann, demonstrated color discrimination beginning at age 6 months using a differential conditioning procedure (not Wesley's nor Raehlmann's phrase) with pairs of differently colored opaque flasks, one containing milk and the other empty, and with reaching and grasping as the instrumental response (Raehlmann, 1903). Another example is that Krasnogorskii (1907/1967) had already begun research in 1907 in Russia on salivary conditioning in children, but by 1913 it had been only briefly summarized in English (see Razran, 1933, chap. 1). These studies were not cited as influencing the trend that in fact Watson's work led to. Watson (1916a) emphasized the role of conditioning in behaviorism and he mentioned Bechterev's method of aversive conditioning in children (1916b, p. 94; 1919a, p. 29; 1920, p. 15; 1924, p. 29; 1929b, p. 29; 1930, p. 35), but he did not mention Raelmann's and Krasnogorskii's research on appetitive conditioning.

A final point in this subsection is that researchers in the experimental group were often found in psychology departments, but they were also found in institutes such as the *Iowa Child Welfare Research Station*, later renamed the Institute of Child Behavior and Development, and the *Institute of Child Development and Welfare*, renamed without "and Welfare." They were also found in nonpsychology departments such as the University of Kansas Department of Human Development and Family Life, which was transformed in 1964 by its chairwoman, Frances D. Horowitz, from a Department of Home Economics into a department of behavior analysis and experimental child psychology. It was eventually renamed the Department of Applied Behavioral Science ("Department," n.d.).

Relations Between the Two Approaches

The traditional and experimental groups did not take kindly to one another, as well-illustrated by a brief exchange between Penney (1960a, 1960b) and Laidlaw (1960). Penney (1960b) said that the traditionalist is interested in child behavior and that the behaviorist is interested in behavior independent of the organism, and therefore "Some question arises as to whether an experimental child psychologist should be labeled a 'child psychologist' at all" (p. 81). He also said, "Perhaps, the child developmentalists themselves have come to realize that they have contributed to theory very little that is grounded in 'sound' research. . . . [T]he majority of [traditional] researchers are not only shockingly naive with respect to statistical techniques but are largely ignorant of the basic philosophy of science and research" (p. 83).

Laidlaw (1960) responded in kind, beginning with the title of his response "Appreciate or Perish" and ending with "Working with, and for, children will keep us engaged beyond the call of duty. We contain multitudes. Why, we may well be able to contain child experimentalists, and even embrace them to our tender hearts — if they will promise us one thing: to be guided by the concept of child welfare, and subordinate physicalistic scientific rigor to an overriding concern for the Service of Man" (p. 56). Penney's (1960a) main reply was that the traditional versus experimental disagreement is largely about values versus facts and that the traditionalist "confuses a value statement with scientific knowledge" (p. 92). Referring to related approaches in general psychology — field and laboratory — Watson (1914) was somewhat conciliatory but ended up giving field research a secondary role: "The field is both the source of problems and the place where the laboratory solutions of these problems are tested" (p. 31). Baer (1973) made the same point, expressed differently, regarding child psychology.

Despite mutual distancing, the differences identified in the preceding two subsections were tendencies and preferences rather than absolute rules. For example, some of the baby biographies included simple experiments (Anderson, 1946, pp. 1-2) and the traditional approach in general sometimes included more formal experiments (e.g., in Holmes, 1935, 1936). Also, traditionalist explanations utilized not only genetic but also environmental variables such as education and parents' child-rearing techniques. On the other side, the experimental approach involved much observational, norm-gathering research. For example, Watson did some observational fieldwork with nonhuman animals (Todd & Morris, 1986), but he did little in his work with humans. An example in his human research is on handedness; it was in part observational and in part experimental in that he sometimes observed spontaneous occurrences of reaching and sometimes presented stimuli and observed the responses (Watson, 1919a, pp. 241-242; Watson & Watson, 1921, pp. 500-501). Another example is a study by Blanton (1917) that Watson directed. It was mostly an observational norm-gathering study, but it also included many small experiments — examples are spontaneous and deliberately elicited occurrences of the grasping reflex (pp. 465-466) and responses to painful stimuli that occurred naturally and pinpricks that she applied to the wrist (p. 473). I did some questionnaire norm-gathering research (Reese, 1961a, 1961b, 1962, 1966) and Hart and Risley (1995) conducted a monumental observational study of language development in young children.

The experimentalists also recognized some hereditary effects. Watson himself said:

> Is there nothing then in heredity? How absurd. Certainly there is. We are born men not kangaroos. We are born with two eyes situated close together, not like fowls and horses where the two eyes, except for a narrow range, never view the same object at the same time. [Etc.] (1927b, p. 231)

Actually, this and other of Watson's references to heredity are quibbling, and Kuo's assertion quoted earlier — that Watson was only half a behaviorist — got the propor-

tion wrong in that Watson was more than half a behaviorist. The real proportion can be seen in Watson's paragraph after the one containing the above quoted statement: "The behaviorist admits all this but he says, contrasted with what the human infant has to learn (be conditioned to), it is all *unimportant*" (his emphasis).

Despite the overlaps, the differences in concerns and preferences warrant characterizing the respective approaches as consistent with field versus laboratory (Watson, 1914, pp. 30-31) or natural history versus natural science (Reese, 1999) or humanistic versus scientific (Horowitz, 1986).

Watson's works influenced both the traditional and the experimental groups, by inciting some traditionalists to explicate the aims and methods of their approach to child-study and by recruiting other psychologists to the experimental approach. The traditional approach was dominant in child psychology in the era of Watson's 1913 manifesto (e.g., White 1970), but the experimental approach eventually became dominant. In 1931, Anderson wrote, "The great body of investigations in the child field at the present time are behavioristic in their approach" (p. 6), and "It is to the credit of Thorndike and Watson that they developed a conception of psychology which put genesis [i.e., development] as well as status [i.e., state or stage] within the field and started a series of researches which have already revolutionized the traditional definition of the science" (p. 8; bracketed material added).

Watson on Emotions

The domain of emotions and emotional development provides a good illustration of Watson's influence on child psychology. It is discussed in the present section.

Watson's Stance on Emotions

Citing no documentation, Whiten (2002, p. 1720) commented that emotion was "a topic that was nearly taboo during behaviorism's dominance." This comment is superficially consistent with comments in the 1913 manifesto, in which Watson criticized the concepts of "sensation, perception, affection, *emotion*, volition" (p. 164; emphasis added). However, Watson was criticizing the way these concepts were used in classical psychology. Regarding this usage he said, "The psychological object of observation in the case of emotion, for example, is the mental state itself. The problem in emotion is the determination of the number and kind of elementary constituents present, their loci, intensity, order of appearance, etc." (p. 158). Later, Watson devoted two chapters to emotions and emotional development as conceived in behaviorism in all three editions of *Behaviorism* (1924-1925, Pamphlets 7 & 8; 1925a, 1930, chaps. 6 & 7); he also dealt with these topics in much of his (1928a) *Psychological Care of Infant and Child* and in other works cited later (subsection *Watson on Fear, Rage, and Love*); and his two most famous experiments-the Watson and Morgan (1917) and Watson and Rayner (1920) studies — dealt respectively with fear, rage, and love as primary and innate

emotions and with fear conditioning and generalization. Evidence for fame is that Watson and Morgan's report was one of 13 articles selected to be reprinted as "benchmark papers" in a Special Centennial Issue of the *American Journal of Psychology* (Watson & Morgan, 1917/1987); and in a survey of 130 introductory psychology textbooks published between 1920 and 1989, Todd (1994) found that Watson and Rayner's report was the most frequently mentioned single experiment.

A further consideration is that emotion was a fairly popular topic before and during behaviorism's dominance and remained so after the rise of cognitivism and then neurophysiological psychology. Stated more precisely, *emotion* is at best a superordinate concept and at worst only a chapter-heading word (see Bentley, 1928, for relevant discussion); but either way, behaviorists extensively studied and discussed the specific topics it encompasses — specified emotions and their natures, functions, development, and so on (for discussion and examples see, e.g., Jersild, 1946, 1954; M. C. Jones, 1931; Lindsley, 1951; Lipsitt, 1970a; Munn, 1965, chap. 14). These topics have not been popular in behavior analysis, Skinner (1953, p. 160) having interdicted emotion as an explanatory fiction. He said, "The names of the so-called emotions serve to classify behavior with respect to various circumstances which affect its probability. The safest practice is to hold to the adjectival form . . . so by describing behavior as fearful, affectionate, timid, and so on, we are not led to look for *things* called emotions" (1953, p. 162). That was all Watson asked for, as indicated by Watson and Morgan's (1917) saying that the stimulus-response combinations they called fear, rage, and love might better be called X, Y, and Z (see also the next paragraph). For Watson, the stimulus-response combinations *are* the emotions and because the combinations are observable, they are *things* called emotions. Both Watson and Skinner wanted to avoid the excess baggage that folklore, fiction, and nonfiction had loaded onto names of the emotions.

Watson's Definition of Emotions

Watson defined emotions as empirically specified sets of stimuli that arouse empirically specified sets of responses (e.g., Watson & Morgan, 1917). The responses can be overt behaviors or physiological phenomena: He interpreted visceral responses that are involved in thinking as "emotional" (Watson, 1930, p. 237), and more formally he theorized that "*visceral and glandular factors predominate*" in emotions, but that emotions also include "overt factors such as the movement of the eyes and the arms and the legs and the trunk" (p. 165). That is, an emotion includes a set of responses involving the whole body; in fact, Watson made this a basic principle of his behaviorism: "Man for us is a *whole animal*. *When he reacts he reacts with each and every part of his body*" (p. 94; his italics).

Watson's General Influence

Some of the investigations Watson's work on emotions stimulated or at least influenced were theoretical (e.g., Campos, Barrett, Lamb, Goldsmith, & Stenberg, 1983:

Hilgard & Marquis, 1940, pp. 293-294), many were observational (e.g., Jersild, 1935; Jersild & Holmes, 1935b, 1935c, 1935d, 1935e), but more significantly, I think, is that many were experimental (e.g., Bregman, 1934; W. Dennis, 1940; English, 1929; Gauger, 1929; Holmes, 1935, 1936; Irwin, 1932a, 1932b; H. E. Jones, 1930; M. C. Jones, 1924a, 1924b, 1926; Munn, 1940; Sherman, 1927a, 1927b, see also Sherman & Sherman, 1929; Valentine, 1930). Unfortunately, as shown in the rest of this paper much of this follow-up work was based on mistaken beliefs about Watson's findings, empirical conclusions, and theory. Also, some of the experiments were "quite amateurish in character," to borrow Watson's (1930, p. 203) epithet about experiments by Gesell and Thompson (1929), or were at least suboptimal in design or execution. Examples are three studies that were frequently cited, and a fourth seldom cited, as failing to replicate Watson and Rayner's (1920) demonstration of conditioning and generalization of fear in "Little Albert." The studies are discussed in the following paragraphs.

B. Harris (1979, p. 155) said that Bregman (1934) failed to obtain conditioning using a disagreeable noise and said "see Thorndike, 1935." Actually, Bregman used a differential conditioning procedure with intended "agreeable" and "disagreeable" unconditioned stimuli (UCSs) that Thorndike (1935, p. 195) said caused, respectively, "relief, contentment, and interest" and "fear or startle." The actual findings showed that differential conditioning did not occur, but also showed that the "agreeable" UCSs — a rattle and a 10 sec. melody — were at most only mildly agreeable, and although the "disagreeable" UCS — an electric cow-bell — was disagreeable, it did not cause fear or startle. Aside from being irrelevant to the Watson and Rayner study because of the procedural difference, the study was flawed by few training trials, multiple would-be conditioned stimuli (CSs), a very complex response-coding system, and other problems.

English (1929) obtained no fear conditioning in more than 50 trials with a loud sound. However, the loud sound produced no fear in the infant he studied (one of his daughters) and as B. Harris (1979, p. 156) noted, English himself acknowledged this problem in his report.

B. Harris (1979, p. 155) said that Valentine (1930) "used extensive naturalistic observations and failed to find conditioned fear of infants to loud noises." Valentine's naturalistic observations, involving his own five children, were intermittent rather than continuous and therefore were very unlikely to provide information about paired occurrences of neutral stimuli with an unconditioned fear-evoking stimulus. This information would be required for inferring that conditioning occurred. He also gave some experimental presentations of selected stimuli in natural settings, interspersed among the naturalistic observations, but they also revealed nothing about conditioning. He also conducted two experiments with one of his daughters. In an experiment with opera glasses as the intended CS, the intended UCS was ineffective and in the other one the intended CS (a caterpillar) was aversive before the UCS was presented.

Hagman's (1932) main study was observational, but his report included an experimental study with preschool children. Each child was given a single training trial with a UCS (artificial thunder) that Hagman himself said may have been ineffective — only 1 of 25 children exhibited fear.

Brooks (1937, p. 287) cited a 1930 report by Irwin as inconsistent with Watson's theory of emotions, but the study was not relevant. The study was experimental but was unusual in that it involved not experimenter-controlled presentations of stimuli but experimenter-controlled nonstimulation. Newborn infants were observed in an experimental cabinet (p. 11) that controlled external stimulation by making it "constant, or approximately constant" (p. 13) except for variations of faint sounds (p. 14). Irwin said, "It is assumed that the activities of infants under the conditions of this research are due to stimuli originating within the body of the infant" (p. 13).

Watson on Fear, Rage, and Love

Watson and Morgan (1917) concluded from research on infants that three emotions are primary in the sense that they were the only three observed in neonates and are "unlearned" in the sense that they were observed to occur without any known training. They called these emotions fear, rage, and love "with a good deal of hesitation" and added, "The reader is asked to find nothing in them which is not fully statable in terms of situation and response. Indeed we should be willing to call them original reaction states X, Y, and Z" (p. 165). Watson often described and sometimes expanded upon the original report in, for example, the following publications. I call this Watson List 1 for ease of later reference.

> Watson List 1: Watson, 1917, pp. 63-67; 1919a, pp. 199-202; 1919b, pp. 169-172; 1920, pp. 15-16; 1924, p. 219-221; 1924-1925, pp. 113-117; 1925a, pp. 120-125; 1925b, pp. 337-341; 1926c, pp. 46-50; 1927a; 1928a, pp. 42-43, 71, 74, 96, fig. captions on pp. 26, 27, 34; 1928b, pp. 53-56; 1929b, pp. 229-231; 1930, pp. 152-157; Watson & Watson, 1921, pp. 506-508.

He also sometimes mentioned only the conclusion about fear, rage, and love (e.g., Watson, 1926a; 1926b; 1927c, only fear & rage; Watson, 1929a; Watson & Rayner, 1920).

Citations of Watson and Morgan's study. Watson and Morgan's (1917) study stimulated extensive commentary and follow-up research by child psychologists; it was rather frequently cited — I found 12 explicit citations of it in articles and books I read for other purposes: Blanton (1917, footnote 1, p. 469), Watson (1917, footnote 1, p. 66), Watson and Rayner (1920), English (1929), M. C. Jones twice (1931, pp. 71, 93, 1933, pp. 271, 301), Taylor (1934, p. 69), Garrett twice (only in reference section in 1941, p. 447, 1951, p. 147; not cited in 1930), Loring (1967), and Buckley (1989, index note 30 on p. 121, with full reference p. 211), and Schultz and Schultz (1992,

p. 206, but only for using the concept of "drive" before Woodworth used it). Stoffels referred obliquely to Watson and Morgan: In discussing the study of emotional reactions in young infants, Stoffels said, "Watson was the first to employ this method (1917)" (p. 92; my translation), but he neither cited nor listed Watson and Morgan's report. Many others cited Watson's later summaries; for example, Irwin (1932a, 1932b) cited Watson 1919a and 1928a, and Stoffels (1940, p. 93) cited Watson 1924 (the 2nd ed. of 1919a) and 1928b (Watson did not cite Watson and Morgan in these works). Still others cited the gist about fear, rage, and love without citing any source (e.g., Pratt, Nelson, & Sun, 1930, pp. viii, 211; Sherman, 1927a, 1927b, 1927c; Watson List 1 above). Pratt et al. listed Watson (1919a, 1926b) and Watson and Watson (1921) in their reference section, thus perhaps implicitly citing them as sources. Sherman and Sherman (1929) did not cite any specific source, but in their preface they clearly implied Watson and Morgan's report or one of the summaries of it by Watson. They said, "The pioneer experimental investigations of John B. Watson on the emotions of infants provided the stimulus for the work on emotional responses reported in this volume. Grateful acknowledgement is made for the use of some of his methods of initiating such responses" (p. 10).

Research by Pratt et al. and Taylor. Loring (1967) cited Watson (1925b, 1925d) and Watson and Morgan (1917) on unlearned emotions, but his 1925d ("1925a" in her cit.) is irrelevant — it is on instincts. She said, "There were a number of attempts to replicate his work under more controlled conditions and with larger groups of infants (Pratt et al., 1930; Irwin, 1932; Taylor, 1934; Stoffels, 1940-1941). None of these attempts at replication was successful" (p. 431; the correct date for Stoffels is 1940). All four of these sources are problematic.

Pratt, Nelson, and Sun (1930, p. 168) cited Watson (1919a, p. 242), who said that holding a young infant's nose arouses a "defense reaction" of raising the hands and pushing at the examiner's fingers (Watson, 1920, p. 72, repeated the point). Taylor (1934, p. 69) cited Watson and Morgan (1917), who said that holding the nose arouses rage (p. 167). Pratt et al. and Taylor did not confirm these findings, but relevance to the present topic is debatable. On one side, Watson (1919a, p. 242; 1920, p. 71) classified the holding-nose situation and defense response not as an emotion but as an instinct (also implied in Watson, 1928b, p. 95) and on the other side, Sherman and Sherman (1929, pp. 144-145) implied that emotions are instincts. Furthermore, although Watson and Morgan (p. 167) said in the last sentence in their section on rage that holding the infant's nose for a few seconds arouses rage, Watson omitted nose-holding as a source of rage in all of his later summaries of Watson and Morgan's report except one (all but Watson, 1917, p. 72) in Watson List 1 above).

Pratt et al. (1930) and Taylor also studied the efficacy of restricting infants' arm movements as a stimulus for rage. Watson (e.g., 1919a, p. 200) said, "Almost any child from birth can be thrown into a rage if its arms are held tightly to its sides." Pratt et al. said their stimulus consisted of "pressing the arms firmly against the body and *holding them there against whatever energy the infant would exert*" (p. 177; emphasis added).

The italicized clause is an overstatement, because arm movements were part of several reaction types Pratt et al. reported. Another issue is that Pratt et al. aggregated reactions across varying numbers of trials per infant and varying numbers of infants per age group, which makes the reported percentages hard to interpret. Nevertheless, they found that 58% of 358 trials given to 66 infants from birth to 21 days old showed no response to the stimulus. This percentage is so far from Watson's "almost any infant" that Loring's conclusion is reasonable — Pratt et al. did not confirm Watson's statement.

Taylor also did not confirm Watson's statement, but Taylor restricted arm movements for only 20 seconds and said that Watson suggested a full minute in a personal communication Taylor received after his study had begun. Taylor said, "I do not believe this is an important difference" (footnote 10, p. 70), but I would not bet on it — 20 seconds are 67% fewer than 60.

Taylor also used dropping the infant and a loud sound, neither of which aroused the behavior Watson and Morgan described. For dropping, instead of holding the infant above a bed and dropping him or her onto a soft pillow, as Watson and Morgan had done (their p. 166), Taylor placed the infant on a board, raised it about 18 inches, let it fall about 12 inches, then caught it and lowered it such that "the speed was decelerated slowly and brought gently to rest on the table top" (p. 71). The slow and gentle lowering was done in only the last 6 inches of the fall. Taylor did not estimate the loudness of the sound he used, but he said its intensity "no doubt varied widely from time to time" (pp. 71-72), that is, from infant to infant. In footnote 11 (p. 72) he changed "no doubt varied" to "may have varied."

In his introductory comments, Taylor said, "The critical experiment, to prove or disprove the question, must perforce reproduce in so far as possible the conditions originally used by Watson" (p. 70). Although Watson and Morgan and, later, Watson in his summaries did not provide much information about the experimental situations, the foregoing comments indicate that Taylor did not closely reproduce the few details they provided. Not mentioned above is that in his criticism of the Sherman and Sherman (1929) report, Watson (1930) alluded to the need for multiple trials with each infant. Taylor noted that Watson criticized Sherman and Sherman, but said only that "Watson rejected the Sherman results because the observers were not trained in infant observation" (p. 69).

Research by Irwin. Loring's (1967) reference section cited the title of Irwin, 1932a, but the page spread of Irwin, 1932b. Both studies are relevant and in both, Irwin cited Watson's 1919a and 1928a as sources. Both studies failed to replicate parts of Watson's findings about neonates' response to loud sounds and the dropping version of loss of support. Irwin found that a loud sound (1932b) and dropping (1932a) elicited startle, as Watson had reported (1919a, pp. 199-200; 1928a, fig. and fig. cap. p. 26, fig. p. 27), but contrary to Watson (1919a, ibid.; 1928a, fig. cap. p. 26, fig. p. 27) crying never (Irwin, 1932b) or almost never (Irwin, 1932a) occurred. A problem is that Watson and Rayner (1920) found that in the pretest the loud sound aroused crying only on the third presentation and Watson and Watson (1921) said that crying occurs

"in some cases." However, in all the other works cited in Watson List 1, Watson implied that crying is always part of the response (in some works, beginning in 1924-1925, p. 114, he added that depending on age, the response includes "crying, falling down, crawling, walking or running away"; his 1919b p. 170 implies that crying does not depend on age). Another problem is that Irwin's loud sound was a pure tone with a frequency of 581 cps, which is quite low-pitched (Licklider, 1954, fig. 12, p. 1003). Watson (1925a, p. 121; 1930, pp. 152-153) said that "extremely low pitched, rumbling noises" do not arouse fear in infants and pure tones "are not very effective." Irwin's dropping situation — infant raised supine above the experimenter's head, dropped about 2 feet, then caught — may have been more drastic than Watson's — described by Watson and Morgan (p. 166) only as "the child is held over a bed upon which has been placed a soft feather pillow."

Research by Sherman. Sherman (1927a, 1927b, 1927c) did not mention Watson at all, and only the first two of these articles contributed experimental data — his 1927c was a theoretical account, but he mentioned some descriptive anecdotal evidence analyzed below. Sherman and Sherman's (1929, chap. 5) summaries of the 1927a and 1927b results indicate either that those reports were incomplete or that Sherman and Sherman conflated some results from other studies. A point that I believe has been overlooked by other commentators is that if fear, rage, and love are really innate and primary, they are innate and primary *combinations* of specific sets of stimuli with specific sets of behaviors; that is, they are functionally stimulus-response *compounds* and therefore they cannot be identified without observing both the stimulus and the behavior (see Watson List 1 except 1927a and 1928a).

M. C. Jones (1933, p. 273) evidently missed this point in saying that Sherman's experiments "were well designed to test the behavioristic theory of specific primary emotional patterns." Actually, these experiments were poorly designed if Sherman's intention was to test Watson's conclusion that fear, rage, and love are primary and innate. However, this intention was not stated in Sherman's 1927a and 1927b reports, nor in his 1927c theoretical article nor in Sherman and Sherman's (1929) book, so the possibility remains that the experiments were well designed to serve some other intention.

Watson's theory can be tested only by evidence that the stimulus side of the stimulus-response combinations does not accurately predict the behavior side and vice versa. Sherman provided no relevant evidence; he showed his observers live or filmed reactions of young infants with and without observable stimulating conditions, and he asked the observers to name the emotion exhibited rather than to describe the responses. The observers were much more "accurate" in naming the responses when the stimulus was observable, and Sherman concluded that the infants' behaviors were generalized rather than specific. That is, he "blamed" the infants rather than the observers. Sherman (1927c) mentioned some descriptive evidence based on the 1927a and 1927b experiments, but this evidence was anecdotal because it consisted of retrospective memories of apparently unsystematic observations of "many infants over a period of three years" (p. 389).

Watson (1930, pp. 155-156) also said that fear, rage, and love "are at first quite indefinite. Much work remains to be done to see what the various part reactions are in each and how much they differ." This point is consistent with an empirical conclusion by Bridges (1932) and a speculation by Sherman (1927c, p. 392; Sherman & Sherman, 1929): The primary, innate emotion is undifferentiated general excitement.

Research by Stoffels. Stoffels (1940) studied newborn infants' responses to restriction of head movement and to hunger, cold (cold metal pressed on the skin), and "non-stimulation" (p. 100; all translations herein are mine). The observers generated descriptions of the part-responses using a fairly complex coding system developed from a preliminary study of responses to head restraint (pp. 96-97). The system consisted of 42 codes divided into five groups (head, arms and hands, legs and feet, body, and vocalizations; p. 97), and the aim was to do the codings in real time and in sequence of occurrence. The data indicated large differences within and between infants (p. 112), but strong correlations between the numbers of part-responses across the stimulation and control conditions and across the stimulation conditions (pp. 110-111, 116-118). Stoffels interpreted the correlations as indicating similarity — a common error (see below) — but also observed some similarities in part-responses and their sequences (pp. 116, 127). Given the alleged similarity between the reactions to head restraint, hunger, and cold, Stoffels concluded that anger "seems to be a generalized reaction, a simple exaggeration of habitual reactions" (p. 127); but he added, "That does not mean that there is no difference between the reactions provoked by these stimuli: These reactions are very similar but not identical" (p. 128).

I would make three points: First, correlation almost always indicates not absolute similarity on a measure but, as here, similarity of relative position on a measure. The measure correlated in Stoffels's study was the *number* of part-reactions to two stimuli or on two trials or in two infants, but the numbers could be identical without any overlap of the actual part-reactions. Stoffels's remarks quoted at the end of the preceding paragraph are consistent with this point. Second, the coding system was derived from observation of responses to head restraint, and therefore it would miss part-responses unique to the other stimuli. Third, Stoffels's conclusion is consistent with the comment by Watson (e.g., 1930, pp. 155-156) quoted in the last paragraph of the preceding subsection.

Watson on Emotional Development

Watson's interest in emotional development is not evident in his 1913 manifesto, but it is implicit in his 1916a "Behavior and the Concept of Mental Disease," especially in an anecdote about a "hypothetical 'neurasthenic dog'" (pp. 593-594; see also p. 596), and Watson and Morgan's (1917) saying that the path to understanding the "hundred or so of [adult] emotional reactions" is "to put them under experimental control" (p. 168) using "the method of conditioned reflexes" by which "emotional reactions can be called out by situations (stimuli) which do not at first call them out"

(p. 171). Watson and Morgan's proposal was repeated in Watson's later textbooks (e.g., 1919a, pp. 211-214) and most notably in his (1930, pp. 157-158) critique of Sherman and Sherman's (1929) report. A noteworthy point about the proposal is that it is less consistent with the traditionalistic precept that "The way to understand a phenomenon is to see how it develops" than with the experimentalistic precept "The way to understand a phenomenon is to see how to develop it" (compare pp. 21-22, 71-73 in Reese, 1989).

Several experiments have dealt with aversive conditioning in infants, but none with a stimulus aversive enough to arouse fear. Several of these were successful (e.g., H. E. Jones, 1930, 1931; Moss, 1924) and several were unsuccessful (e.g., Bregman, 1934; Gauger, 1929; Hagman, 1932; Herring, 1930; Valentine, 1930). Only two experimental attempts at infant conditioning of fear have been reported — the Watson and Rayner (1920) study and the English (1929) study. Watson and Rayner's experiment was successful, but English's was a failure because the intended UCS did not arouse fear. B. Harris (1979, 1980) asserted that Watson and Rayner's experiment was unsuccessful, or at least overstated, but he did not fully understand their method and findings. For example, he did not understand the value of single-subject research and he did not understand that Watson and Rayner used bench-research procedures with a version of the standard reversal design, which has built-in control conditions. Bench research encourages serendipitous deviations from predetermined procedures; English (1929) should have tried it.

The standard reversal design begins with baseline observation of reactions to various stimuli (the pretests in Watson & Rayner), followed by a treatment phase (conditioning trials) in which a stimulus (a rat) that had not aroused the target response (fear) in the baseline tests is followed by a baseline (pretest) stimulus (a loud sound) that had aroused this response, followed by a reversal to the baseline phase with stimuli that had not aroused the target response in the first baseline phase (probes for generalization). Sometimes (as in Watson & Rayner) the reversal is modified by interspersing treatment (conditioning) trials among the probe trials. The reversal probes that Watson and Rayner used were blocks, cotton wool, dog, mask, and rabbit; Watson and Rayner did not mention blocks as pretest stimuli, but Watson's extant film shows Albert playing with blocks in the pretest (Watson, 1919c, second segment in Reel 2; regarding the film, see H. P. Beck, Levinson, & Irons, 2009, 2010; L. F. Beck, 1937; 1938, pp. 131, 142-143, 148, 168; Reese, 2010). The blocks did not arouse fear; but the other stimuli did. This pattern demonstrates generalization of conditioned fear because the pretest demonstrated that Albert had not feared these stimuli before the conditioning of fear of the rat. Furthermore, the strength of the generalized fear was not the same for all of the stimuli, which together with nonfear of the blocks demonstrates a strong gradient that confirms generalization by ruling out other potential causes of these fears. Watson and Rayner also used other probe stimuli that had not been pretested (a sealskin coat, Watson's hair, the hair of two observers); only the coat and Watson's hair aroused fear, but because they had not been pretested, fear of them did not demonstrate generalization.

Several studies of the elimination of fears have been reported. Many were observational (e.g., Jersild, 1935; Jersild & Holmes, 1935d); but some were experimental (e.g., M. C. Jones, 1924a, 1924b, under Watson's guidance; Holmes, 1935, 1936, apparently under Jersild's guidance).

Ruminative Summary

History Writing

Some history writers have asserted that behaviorism would have arisen even if Watson had not had a hand in it. For example, Schultz and Schultz (1992, pp. 260-261) said, "By the time Watson set to work on behaviorism, objectivism, mechanism, and materialism were strong. Their influence was so pervasive that they led inexorably to a new kind of psychology. … The science of behavior … was the inescapable result." That is, Watson's assistance was not needed. One problem with this assertion is that inexorability and inescapability exist in philosophical idealisms such as Hegel's and in some speculative histories, but not in actual history. The error is not in noting historical trends that antedated an historical change, but in interpreting the correlation of prior events with a later event as indicating that the prior events caused the later event (Gould, 1996, p. 32). This is the fallacy called *post hoc ergo propter hoc* — "after this, therefore because of this" (Werkmeister, 1948, pp. 51-52). Another example is that Leahey (1992, p. 317) said Watson's 1913 behaviorist manifesto "simply marks the moment when behavioralism became ascendant and self-conscious, creating for later behavioralists a useful 'myth of origin'." … But all of these things would have happened had Watson never become a psychologist." Actually, behaviorism did not spring ascendant and self-conscious from the manifesto, but this is not the basic problem with such assertions. The basic problem is that they are *silly* (in a nonmalicious sense that Bergmann, 1956, p. 266, said philosophers use). After Watson became a psychologist and published the manifesto, a world in which Watson did not become a psychologist and did not publish the manifesto did not exist and never can exist. Therefore, in the real world the assertions are untestable and therefore meaningless; as James (1907/1981, Lecture 2, p. 26) said, "Whenever a dispute is serious, we ought to be able to show some practical difference that must follow from one side or the other's being right." If this condition is not met, the dispute has no empirically defensible solution and therefore it is pragmatically meaningless — that is, meaningless in the real world.

The history of child psychology indicates that this field changed from a norm-gathering, observational natural history to a theory-testing, experimental natural science. The antecedents included Watson's 1913 manifesto, his early subsequent textbooks and scholarly articles, and the Watson and Morgan (1917) and Watson and Rayner (1920) reports of experiments on emotions. The conclusion given herein is that these antecedents are among the causes of the change. One might argue that this

conclusion is an instance of *post hoc ergo propter hoc*; but two further points invalidate such an argument. A crucial point is that many of the new wave child psychologists indicated in reports of their own experiments that the intention was to follow up the Watson experiments, or one or more of Watson's later summaries of them. A second point is that the change in child psychology was multi-faceted and as some new wave child psychologists said, the facets were consistent with Watson's manifesto or his later elaborations of it. These points indicate that the conclusion in question is not a mere conjecture but rather follows from evidence.

Watson and Child Psychology

General influences. Histories of psychology in general indicate a shift from the classical experimental psychology of consciousness, based on subjective introspections by specially trained self-observers (e.g., James, 1890, Vol. 1, chaps. 1, 7; Ladd, 1896, Introduction) to a natural-science experimental psychology of stimulus situations and behaviors, devoid of consciousness and based on manipulations of the stimulus situations and objective observation of the behaviors. The histories also indicate an interest of classical psychologists in identifying and listing instincts and emotions, often without clearly distinguishing between these categories, and the histories indicate a shift to identifying instincts and emotions as stimulus-response combinations and determining experimentally when they emerge during infancy and, normatively or theoretically, how they change in childhood and adulthood. The shifts, in other words, were toward the behaviorism that Watson had outlined in his 1913 behaviorist manifesto.

The goals of child psychology in the 19th century were broadly consistent with the goals of classical psychology — description and understanding of the mind (e.g., Preyer's first preface in Preyer, 1890/1973; Hall, 1973), but this traditional child psychology dealt with mental development as well as mind, and mental development included aspects of mind that are not necessarily conscious — sensory processes, instincts, and emotions. Because these child psychologists could not use classical introspection to study early stages of mental development, they used observational methods, especially those of natural history but often assisted by natural science methods involving experimental manipulation of stimuli (e.g., in Preyer, 1890/1973). The approach to development was descriptive, in the form of norm-gathering, and the basic explanatory concept was heredity (e.g., first preface in Preyer) or hereditary mechanisms such as "maturation." Preyer said, "*Heredity*,... to be sure, explains nothing; but dim as the notion is, much is gained toward our understanding of the matter, in the fact that some functions are inherited while others are not" (op. cit., p. xi; Preyer's italics). The concept of maturation is equally dim, and my take is that both concepts yield only an illusion of understanding except in some branches of biology such as physiology and genetics (but not behavioral genetics, which is a branch of psychology).

The interests of child psychologists changed gradually during the decades following Watson's 1913 manifesto and became more consistent with Watson's behaviorism. For example, the main motive for gathering norms shifted from norms for practical use or for their own sake to norms for theory testing; the use of traditional observational methods sometimes aided by experimental methods was largely replaced by the use of experiments sometimes aided by traditional observational methods; and explanations of development shifted from vaguely specified hereditary mechanisms to well-specified environmental mechanisms such as classical conditioning and generalization.

Emotions and emotional development. The foregoing changes are seen with special clarity in research on emotions and theories of emotional development. In classical psychology the emphasis was on emotions as conscious contents — the issue of how emotions are experienced. Watson rejected this issue in his 1913 manifesto and called for the study of emotions as behaviors. In traditional child psychology the question of how emotions are experienced in consciousness became obsolete, but the issue of describing and listing emotions remained and catalogues of "innate" emotions grew larger and larger. Eventually, this botanizing approach was replaced by stimulus-response experiments aimed at the issues of identifying emotions that are innate and the stimuli that elicit them, and determining experimentally — or predicting theoretically — how innate emotions develop into the myriad of specific emotions that emerge in childhood and adulthood. Again, the shift was toward Watson's position in his 1913 manifesto.

In the domain of emotions, then, and in child psychology in general, Watson's behaviorism brought about an emphasis on experimental evidence. This emphasis has persisted despite a decline in attention to his behaviorism as such. Schultz and Schultz (1992, pp. 326-327) provided a possible reason: Behaviorism grew up. They said, "Boring said in 1929 that behaviorism was already past its prime as a movement. Because movements depend on protest for their existence and strength, it is an effective tribute to Watson's behaviorism that only sixteen years after its introduction, it no longer needed to protest."

More specifically, the view of emotions that Watson (1913) challenged was that they are akin to instincts and the meaningful research issues are to identify them, their mental contents, and their effects on consciousness. Watson's behavioral influence shifted the view to emotions as combinations of observable stimuli and responses that express underlying but unobserved physiological processes. The major research issues shifted accordingly to determining which stimulus-response combinations identified as emotions are innate and primary and how they develop into new emotions — that is, new stimulus-response combinations — by conditioning, generalization, and other stimulus and response mechanisms. This view gradually faded as the field shifted back to a pre-Watsonian emphasis on emotions as physiological processes — an aspect of emotions that Watson liked (e.g., Watson & Morgan, 1917, p. 170; Watson, 1930, pp. 38, 165, 237). This view eventually gave way to a view of emotions as social psycho-

logical phenomena (Campos et al., 1986), followed by a return of the physiological view. The most recent incarnation is a neurophysiological view described in a monograph edited by T. A. Dennis, Buss, and Hastings (2012b), in which Campos et al. and Watson are not cited. It has no consensus on what emotions are, hence none on the relation between emotions and neurophysiological phenomena. The relation might be neurophysiological phenomena as constituting emotions, as nondefining concomitants of emotions, or as merely correlated with emotions (T. A. Dennis et al., 2012a).

Watson also gave no consistent statement. Paradoxically, he defined emotions as stimulus-response combinations but he also said that "*visceral and glandular factors predominate*." The paradox is resolved by an earlier statement: "Observations seem to show that combinations or integrations occur among emotional, instinctive, and habit activities," many of which "are constrained by social factors" (1919a, p. 216). My rewording is that specifiable physiological phenomena constitute emotions, which are formally defined by specified stimulus-response combinations in which the response sets are instinctive for innate emotions, and variations in the observed response sets are habits conditioned by social factors. In short, the modern neurophysiological view is reprising a basic consistency with Watson's view.

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