

ethnic minority populations was double that for White majority populations. Most MI therapists in the clinical trials were non-Hispanic Whites, and it is possible that an experience of the respectful, collaborative, empathic style of MI is a greater contrast for minority populations than for White clients.

A wide variety of external factors might mediate or moderate the efficacy of MI (or of any psychotherapy). Our article focused on the therapeutic interaction, not on a comprehensive model of all that influences behavior change. The domain of “social context” encompasses a broad range of factors (such as employment, family history, peer influence, and religious involvement), and any number of other components might also be considered in predicting substance use outcomes (e.g., age, conceptual level, severity of dependence, comorbidity). The model that we proposed (Miller & Rose, 2009) was focused on interpersonal and intrapersonal factors involved when a therapist interacts with an individual client. MI as an individual intervention has been found to be efficacious across a broad range of problem areas. As the processes and efficacy of MI become better understood, it will also be possible to explore how these operate within the person’s ongoing social context.

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Little Albert Still Missing

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Beck, Levinson, and Irons (October 2009) presented a fascinating account of how they seemingly solved the mystery of what ever happened to Little Albert, the infant in whom Watson and Rayner (1920) claimed to have conditioned a rat phobia. Using government census data, the authors identified a woman, Arvilla Merritte, who worked as a wet nurse at Johns Hopkins Hospital during the period that Watson and Rayner were conducting research there. Hospital records revealed that Arvilla gave birth to a son, Douglas, in the hospital several months earlier, such that the child’s age closely matched, within a critical two-week time period, the reported age of Albert. On the basis of this and other evidence, the authors concluded that Douglas very likely was Albert (the published name, Albert B., apparently having been a pseudonym). However, there are several deficiencies in the authors’ analysis that seriously undermine their conclusion.

Beck et al. (2009) offered various forms of evidence in support of their thesis. These included a report of a biometric comparison between some poor-quality film images of Albert and an old photograph of Douglas; unfortunately, this comparison seems to have been, at best, highly inconclusive. Instead, the strongest evidence by far was the congruence between Douglas and Albert in gender, race, and age. As Beck et al. (2009) put it,

How likely was it that a child born to a Johns Hopkins wet nurse would meet these three criteria? . . . It seemed reasonable to estimate that half the wet nurses’ children would be male, that half would be Caucasian, and that their births would be randomly distributed throughout the year. If these assumptions were correct, then the odds were 1 in 104 ($1/2 \times 1/2 \times 1/26$) that a

child of a 1920 Johns Hopkins wet nurse would be male, Caucasian, and born between March 2 and March 16. (p. 610)

But herein lies a problem. Why assume that half the infants born to a wet nurse at Johns Hopkins would be Caucasian, especially when another potential wet nurse identified in the census was described as being the only Black among the 379 individuals registered in that enumeration district? If so, any additional wet nurses in the hospital would almost certainly have been Caucasian. In fact, the authors did find evidence of another Caucasian wet nurse, Pearl Barger (whose surname interestingly begins with B), in the hospital at that time. The odds of this woman having had a Caucasian child is therefore 1 (or close to it), and the probability of that child also being a male ($1/2$) and in the correct age range ($1/26$) is therefore 1 in 52. Add to that the possibility of yet a third Caucasian wet nurse residing in the hospital—there were apparently up to four wet nurses in the hospital at any one time—and the likelihood of one or more children in the hospital meeting these criteria is, while admittedly low, not as low as the authors’ calculations would suggest.

The second difficulty for Beck et al.’s (2009) thesis relates to the record of Douglas’s birth at Johns Hopkins. According to Watson and Rayner (1920), Albert “was reared *almost from birth* [emphasis added] in a hospital environment [and was] one of the best developed youngsters *ever brought to the hospital* [emphasis added]” (p. 1). The emphasized phrases indicate that the real Albert was *not* born in the hospital but was brought to the hospital soon after his birth. Of course, it is possible that Watson and Rayner were careless in their choice of words or were misinformed about Albert’s history, but the passage also suggests that they had carefully perused Albert’s medical records and would therefore have been aware if he had been born in the hospital. (Interestingly, no record was found of the other wet nurse, Pearl Barger, having given birth in the hospital, which means that she may have given birth elsewhere and then brought her child to the hospital!)

The greatest difficulty for the notion that Douglas was the real Albert relates to the purported rumor that Albert was later adopted. As Beck et al. (2009) put it, “None of the folktales we encountered during our inquiry had a factual basis. There is no evidence that the baby’s mother was ‘outraged’ at her son’s treatment . . . and he was not adopted by a family north of Baltimore” (pp. 612–613). The authors rejected the adoption story on the basis of their discovery that Douglas left the hospi-

tal with his mother and remained with her until he died at age 6. The problem here is that the adoption story is *not* a folktale but is actually a direct quote from Watson (1924/1925): “No further tests could be made upon Albert B . . . because he was shortly adopted by an out-of-town family [emphasis added]” (p. 167). In the absence of any evidence that Douglas was similarly adopted (and his mother later retrieved him), or that Watson was mistaken about Albert’s whereabouts (despite his interest in doing further research with him), this statement seriously calls into question the notion that Douglas Merritte was Little Albert.

Hopefully, Beck et al. (2009) have additional information that can address these difficulties. If not, one is forced to conclude that Douglas Merritte might very well not have been Albert and that Albert was instead another infant residing in the hospital at that time. Thus, while the authors should be applauded for their persistent detective work and the tantalizing leads they have uncovered, it appears that the case of Little Albert remains unsolved. To put a twist on the authors’ own words, psychology’s lost boy is still missing.

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- ## Regarding Little Albert
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- H. P. Beck, Levinson, and Irons (October 2009) concluded from intensive detective work that Watson and Rayner’s (1920) “Albert B.” was Douglas Merritte, born at the Johns Hopkins Hospital on March 9, 1919. However, they overlooked one supporting consideration (see paragraph 4c below) and some contradictory considerations (see the remaining paragraphs).
1. Several authors have wondered why Albert disappeared, but Watson said he was “adopted by an out-of-town family” and taken out of the Baltimore area (Cohen, 1979, p. 144; Watson, 1930, p. 167). Douglas was not adopted; he left with his mother and lived with her (H. P. Beck et al., 2009).
2. Albert was 8 months, 26 days old in the first pretest and 12 months, 21 days old in the last (fifth) experimental session. In the segments of the extant film that include him, he is dressed lightly in the pretest and warmly in Session 5, respectively implying a warm day and a cold day. Douglas was the cited ages on December 5, 1919—presumably not a warm day—and March 30, 1920 (1920 was a leap year).
3. H. P. Beck et al. (2009) calculated that Albert was born between March 2 and March 16, 1919, on the basis of their assumption that (1) the filming depended on funding that the Budget Committee at Johns Hopkins authorized on November 19, 1919, and (2) “the first [pretest] filming session occurred within a two-week period between November 28 and December 12, 1919” (p. 607). However, Watson had evidently begun filming his work with infants earlier in 1919, without the cited funding, and this filming could have included the pretest footage. The next two paragraphs provide the evidence:
- (a) L. F. Beck (1937, 1938) cited and summarized a Watson film, *Experimental Investigation of Babies*, that he said was available in 1919. A supplement to the 1937 catalog of the C. H. Stoelting Company, which distributed the film, listed it as still available in 1937 (C. H. Stoelting Co., 1937, p. 54), but Cohen (1979, p. 142) said it was subsequently lost. I have not located any film with the title *Experimental Investigation of Babies*; for example, it is not in the Historic Film collection of the National Library of Medicine (Stephen Greenberg, personal communication, February 6, 1996), not catalogued in the film collection of the Library of Congress, and not in the University of Akron Child Development Film Archives. The title is on the labels of two metal film cans in the Akron Archives, but the title shown within the film is *Studies Upon the Behavior of the Human Infant*, which is also on the two-line label of a videotape cassette converted from the reels of the film by the Akron Archives.
- (b) L. F. Beck’s (1937, 1938) summaries of the lost film *Experimental Investigation of Babies* do not even hint at any footage with Albert. In contrast, the extant film *Studies Upon the Behavior of the Human Infant* is obviously not lost, contains footage with Albert, and—based on L. F. Beck’s summaries—seems to include footage from *Experimental Investigation of Babies*. Evidently,
- then, the lost film was made much earlier than November 28 to December 12, 1919.
4. Watson and Rayner’s (1920) report was published in the February issue of the *Journal of Experimental Psychology (JEP)*; but if Albert was Douglas, the issue could not have been published before April because the study would have ended on March 30, 1920 (see paragraph 2). The following considerations are relevant:
- (a) H. P. Beck et al. (2009) located stamped receipt dates at the libraries of Cornell University, Harvard University, and Kansas State University, but the dates are inconclusive: (1) The date at Cornell was blurred and could have been August 23, 1920 or 1921, and the date at Harvard was for Issues 1 through 5; (2) they did not report the date for Kansas.
- (b) H. P. Beck et al. (2009, p. 608) cited a letter dated December 14, 1922, in which Watson told Adolf Meyer that “the issues now come out on time.” However, the delays were not necessarily as pronounced as required if Albert was Douglas, and the letter does not specifically refer to the February 1920 issue.
- (c) H. P. Beck et al. (2009) found no evidence within the February issue implying lateness, but they overlooked two relevant footnotes. Coleman R. Griffith, who also had an article in the February 1920 issue of *JEP*, cited a “paper read before the Illinois State Academy of Sciences, February, 1920” (Griffith, 1920b, footnote 1, p. 41) and cited page 135 in another article of his (Griffith, 1920a) published in the March 1920 issue of *The Laryngoscope* (Griffith, 1920b, footnote 1, p. 43). Griffith’s use of “read” instead of “to be read” and the March date of *The Laryngoscope* article are consistent with April or later publication of the February *JEP*. However, the February *JEP* could have been published in March if Griffith cited page 135 from page proofs or from the published article if the March issue of *The Laryngoscope* was published on time. I have not found the actual publication date of the March issue of *The Laryngoscope*.
- (d) Publication of the February *JEP* could have been delayed because it was the first issue published after a two-year hiatus caused by World War I (H. P. Beck et al., 2009). H. P. Beck et al. (2009) said Watson had returned to Johns Hopkins by December 1918, and they speculated that he “needed to solicit articles and reestablish subscriptions that had lapsed during the war” (p. 607). However, he had 14 months available between December 1918 and February 1920, which as a long-time journal editor I would say is plenty of time to allow publication of the February 1920 issue on time. Consistent with my speculation, the February issue was