Japanese and American participants found no differences between the two groups along this dimension (which is closely related to the presumed Japanese emphasis on interdependence—as compared with the presumed Western emphasis on independence—as proposed by Markus & Kitayama, 1991).

The existence of such emic concepts as *amae*, *wa*, and *kanjin* cannot be taken as evidence that the denoted attributes are unique to Japan or more prevalent in Japan than in other countries. For example, Kashima et al. (1995) conducted a survey on the *kanjin* orientation (which Hamaguchi, 1985, claimed is a unique, emic category) and found that Japanese participants scored lower on this orientation than did participants from any of the four other countries surveyed. Regarding the ostensibly emic *amae*, it is worth noting that Doi, trained as a psychoanalyst, developed his notion of *amae* on the basis of Western Freudian theory. Hence, the opposition of Japan and the West becomes more difficult to uphold in this respect.

Rothbaum et al.'s (2000) argument for greater attention to indigenous concepts in psychology may at first seem reasonable. However, it must be asked, indigenous to what or to whom? As Azuma (2000) noted, the term *indigenous* reflects a view of culture as "circumscribed, fixed, and internally homogeneous" (p. 9). The view of Japanese culture as internally homogeneous and externally distinctive has been widely discredited in contemporary anthropological and cultural studies (e.g., Gjerde & Onishi, 2000), and Rothbaum et al.'s references to Japanese culture and Japanese parents seem both essentialized and dated. But perhaps their most important fallacy is the failure to distinguish between cultural values and the actual lived experience of contemporary Japanese. Japanese cultural values, often ideologically tainted (Befu, 1993), may not accurately reflect the daily lived experiences of Japanese. Cultural values and subjective experience may be linked, but this linkage may not be perfect and should not be taken for granted without further investigation. As social scientists, we must not reify cultural values and mistake them for actual lived experience. For example, is *amae* experienced as true dependence, or is it sometimes a disingenuous or strategic behavioral expression of other attitudes or desires? We simply do not know. Until we do, an evaluation of the authors' arguments about *amae* and secure attachment is premature.

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Deeper Into Attachment and Culture

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We are grateful for the opportunity to respond to comments from a distinguished group of scholars (Chao, 2001, this issue; Gjerde, 2001, this issue; Kondo-Ikemura, 2001, this issue; Posada & Jacobs, 2001, this issue; van IJzendoorn & Sagi, 2001, this issue). In our comments below, we note areas of agreement and disagreement with their perspectives. First, though, we reiterate our central thesis (Rothbaum, Weisz, Pott, Miyake, & Morelli, October 2000). Attachment theorists maintain that cultural differences are relatively minor, and they focus on universals. We believe that culture warrants a more central role in the study of attachment, and we examine parent–child relationships in Japan and the United States. The evidence leads us to call into question three core hypotheses of the theory, involving the antecedents (sensitivity), consequences (social competence), and nature (secure base) of attachment. These hypotheses are based on measures that are biased toward Western values and meanings, in that they emphasize aspects of individuation such as autonomy and exploration. We advocate an approach that is grounded in concepts that are most meaningful to the peoples being examined.

We share the commentators' respect for attachment theory and its implications for human development and relationships. Indeed, it is our deep appreciation for the theory and its research that prompted our close attention to its tenets and the evidence. Ainsworth's field work in Uganda is particularly notable. Although we believe there has been a recurring Western bias in much of attachment research, we stated in our article (Rothbaum, Weisz, et al., 2000) that some of the core, general principles of the theory—involving the pursuit of proximity and protection and suffering resulting from loss—may well endure and prove universal. The extent of modification required to apply these general principles to other cultures remains a point of contention, but we do not doubt the contribution they make toward understanding fundamental aspects of human relatedness.

Other important points made by the commentators with which we agree are that intracultural differences and intercultural similarities are prevalent. We also agree that culture should not be treated as a discrete variable but rather as a process integral to the way people experience themselves and their social and physical worlds.

Some of the commentators (Kondo-Ikemura, 2001; Posada & Jacobs, 2001; van IJzendoorn & Sagi, 2001) pointed out our failure to mention a study in Japan, by Vereijken, Riksen-Walraven, and Kondo-Ikemura (1997), that in their view supports the sensitivity–security hypothesis; Posada and Jacobs (2001) also noted that we did not mention a study that failed to support this hypothesis. Our concern with the universality of the sensitivity–security hypothesis, as currently articulated, went far beyond whether correlations among the Western-based

measures of these constructs could be found. Japanese conceptions of sensitivity and secure base are so different from Western-based ways of construing and assessing these constructs that the latter are not transportable to Japan. The findings reported by Vereijken et al. (1997) are particularly difficult to interpret, in that the average correlation between maternal sensitivity and child security was much higher (r = .70 for concurrent measures) than the average correlation from a meta-analysis of Western studies (r = .24 in concurrent and brief longitudinal studies; De Wolff & van IJzendoorn, 1997) and much higher than findings from another study in Japan, which yielded nonsignificant effects (Nakagawa, Lamb, & Miyake, 1992).

The raters of maternal sensitivity in the Vereijken et al. (1997) study were aware of the child's security, and the raters of child security were aware of the mothers' sensitivity. Vereijken et al. (1997) commented,

> As the same observers rated both maternal sensitivity and child security, the correlations might be artificially high. In order to avoid this possibility, we computed two cross-observer-correlations: one between observer one's scores for maternal sensitivity and observer two's scores for child security; the other between observer two's scores for maternal sensitivity and observer one's scores for child security. The average was taken to be the correlation between maternal sensitivity and child security in further analyses. (p. 42)

Despite this precaution, each rater scored both measures and was aware of both scores. Although the unusually high correlations may be due to methodological factors, a more interesting possibility is that they are due to the different meanings of sensitivity and security in Japan than in the West.

We are also criticized for our allegedly insufficient treatment of findings that, according to the commentators (Posada & Jacobs, 2001; van IJzendoorn & Sagi, 2001), support the validity of the secure base construct in Japan. We disagree with their conclusion for the reasons mentioned in our article (Rothbaum, Weisz, et al., 2000): Their studies "used instruments developed in the United States . . . and they either ignored concepts indigenous to Japan (e.g., *amae*) or they operationalized those concepts in Western terms" (p. 1100). We believe that in other cultures, competing and more powerful secure base dynamics exist that have not been examined. Specifically, we challenge the primacy, not the existence, of the link between attachment and exploration, and we highlight the primacy of the link between attachment and the dependency or *amae* system in Japan.

Several of the commentators (Gjerde, 2001; Posada & Jacobs, 2001) claimed that the evidence we reviewed about fundamental cultural differences in parental sensitivity and social competence is not relevant to the key attachment theory hypotheses regarding these constructs. They argued that the hypotheses are in no way compromised if sensitivity and competence are experienced and expressed very differently in different cultures. We disagree. It is unclear what the hypotheses mean when the constructs with which they are concerned are poorly defined. As the commentators noted, we do not provide evidence that directly contradicts the attachment theory hypotheses. From our perspective, it is not clear what such evidence would look like, given the commentators' acknowledgement that sensitivity and competence can vary so markedly with culture.

Part of the difficulty may be that one theorist's fundamental difference can be another's minor or specific difference. Perhaps psychologists would all do well to avoid such debatable labels and seek instead to identify the ways in which sensitivity, competence, and the secure base are conceptualized and manifested in different cultures. This would allow researchers to determine which processes are local and which extend beyond local boundaries. A much more textured conception of similarities is likely to emerge if psychologists are not blinded by prevailing (Western) assumptions.

Most of the commentators (Chao, 2001; Gjerde, 2001; Posada & Jacobs, 2001; van IJzendoorn & Sagi, 2001) criticized our call for indigenous psychologies of attachment on the grounds that it discards valuable contributions made by Western theories of attachment. However, an indigenous approach should neither ignore nor impede the development of theory. Instead, it should focus on widely accepted concepts and beliefs from the culture under examination, while remaining mindful of extant theory—in our case, attachment theory. For example, research currently being conducted in Japan (Rothbaum, Kakinuma, Makoshi, & Azuma, 2001) is examining concepts, such as *amae* (depending on others' benevolence) and *isshin-dotai* (one heart, same body), in part because they are relevant to attachment theory. (These Japanese concepts do not have exact parallels in English. Indeed, it is difficult to convey their meaning in English.) This study draws on concepts, practices, and settings that are important to both attachment theory and the culture under consideration.

A particularly effective methodology for exploring indigenous psychologies of attachment might be to videotape naturalistic parent–child interactions in stressful situations (as defined by participants in the different cultures studied). Methods used by Tobin, Wu, and Davidson (1989), who examined respondents' reactions to and interpretations of videotaped observations of participants in their own and other cultures, would be especially illuminating. In contrast to the commentators, who expressed concern that culturally specific theories of attachment undermine broad theorizing, we believe that such theories and research will lead to a new and enriched theory of attachment. At its best, an enriched theory would describe propensities for sensitivity, competence, and the secure base that are common to all humans; it would also address how those propensities take on different meaning and expression in different contexts.

Finally, we respond to the criticism (Gjerde, 2001) that we view Japan as being different from the rest of the world, at least with regard to our concerns about attachment theory. We do not. But Japan is an important case to consider, because it is matched with the United States with regard to technological and educational achievements, and because more research compares Japan and the United States than any two other countries. As a consequence, we have an understanding of ways in which Japanese sensitivity, competence, and security may differ from those phenomena in the United States. The findings we highlighted in the *American Psychologist* article (Rothbaum, Weisz, et al., 2000) and elsewhere (Rothbaum, Pott, Azuma, Miyake, & Weisz, 2000) constitute far more than specific differences in relatedness—a conclusion echoed by five Japanese experts who commented on the latter article—but we do not believe that Japan is the only country that raises profound challenges for attachment theory.

Theories grounded overwhelmingly in studies from one part of the world are not easily exported to other cultures, nor should they be. Because of Westerners' hegemony in the international market of ideas, they are too prone to apply theories they devise, based primarily on research with people like themselves, to people throughout the world. The best antidote for this problem is a systematic study of cultural variation, drawing heavily from indigenous concepts. The study of cultural variation is especially likely to enrich attachment theory if it is championed by attachment theorists themselves.

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The Accuracy of Reference Lists in Five Experimental Psychology Journals

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Reference lists appended to published journal articles are of critical importance to readers of academic literature. They allow readers to retrieve cited sources, either to increase their own knowledge or to substantiate the authors' claims, and in so doing help to establish the judgment and credibility of the authors (McLellan, Case, & Barnett, 1992). However, erroneous references detract from the intended function of reference lists by impeding retrieval searches and providing misleading bibliographic information (de Lacey, Record, & Wade, 1985; McLellan et al., 1992). It has also been suggested that such errors may undermine the credibility of the authors of the erroneous material (de Lacey et al., 1985; McLellan et al., 1992; Nishina, Asano, Mikawa, Maekawa, & Obara, 1995). Although alarmingly high error rates have been detected in medical journal reference lists (see Evans, Nadjari, & Burchell, 1990; McLellan et al., 1992), similar empirical investigations have not yet been reported within the psychological literature. With the present study, we aimed to assess the accuracy of reference lists in the five experimental psychology journals published by the American Psychological Association.

All 1999 issues of the five experimental psychology journals published by the American Psychological Association were analyzed (i.e., *Journal of Experimental Psychology: Animal Behavior Processes*; *Journal of Experimental Psychology: Applied*; *Journal of Experimental Psychology: Learning, Memory, and Cognition*; *Journal of Experimental Psychology: Human Perception and Performance*; *Journal of Experimental Psychology: General*). All reference list citations in these issues were sequentially numbered (N = 14,291), from the first reference in the January 1999 issue of *Journal of Experimental Psychology: Animal Behavior Processes* to the last reference in the December 1999 issue of *Journal of Experimental Psychology: General*. Using computer-generated random numbers, we selected 100 references from each of the five journals. All references to nonjournal items were excluded, leaving a total of 355 references for analysis. This selection procedure was in accordance with that commonly used in the medical literature (see Asano, Mikawa, Nishina, Maekawa, & Obara, 1995; McLellan et al., 1992; Nishina et al., 1995).

The accuracy of each reference was verified by comparing the chosen reference with the original publication (i.e., the primary source). The six standard elements of bibliographic citation were checked for each reference: authors (including the correct order, initials, spelling, and inclusion of all authors), year of publication, article title, journal title, volume number, and page numbers.

Of the 355 references surveyed, 112 had an error in at least one element of the reference: ninety-four (26%) had one error, and 18 (5%) had errors in at least two elements of the reference. Errors in the article title (15%) and the authors (12%) were the most common, followed by errors in the page numbers (6%), volume number (3%), and journal title (2%). There were no errors in the year of publication. A chi-square test of independence revealed no significant differences across the five journals in terms of the frequency of errors, $\chi^2(4, N = 355) = 3.27, p > .05$.

The results of this study suggest that experimental psychology journals are not immune to the problem of reference list inaccuracy. Although the rate of error in the present survey (32% across all journals surveyed) may not have been as high as reported in some medical journals (see Evans et al., 1990; McLellan et al., 1992), it is sufficiently high to warrant concern. Nearly a third of all references surveyed in the selected experimental psychology journals contained at least one error.

Although all cited articles were successfully retrieved, this was only achieved with much effort in some cases. For instance, one article was cited as appearing in *Psychology and Aging* when it actually appeared in *Journal of Experimental Psychology: Learning, Memory, and Cognition*. Another article was cited as appearing in *Journal of Comparative Psychology* when it actually appeared in *Journal of Comparative Physiology*. Significant errors in article titles were also detected. The title of one article was cited as "Recognition of Japanese Kanji in the Left and Right Visual Field" when the correct title was "Visual Field Differences in Semantic Comparative Judgments With Digits and Kanji Stimulus Materials." Such errors reflect poorly on the authors of the erroneous material and the journals in which such errors are published (Asano et al., 1995; de Lacey et al., 1985; McLellan et al., 1992).

Perhaps the most disturbing aspect of the present study is the finding that authors were incorrectly cited in 12% of reference list citations, with errors in the initials of the authors being the most common. In one instance, an article by J. Larkin, J. McDermott, D. P. Simon, and H. A. Simon was attributed to H. A. Simon, J. H. Larkin, and J. H. McDermott. The surname of an author on another article was cited as de Bordes Galai rather than de Bordes Galdi. Such errors have the capacity to affect citation indexes and consequently may have implications for the academic careers of the incorrectly cited authors. This is also the case when initials are incorrectly reported, such as when we found T. D. Wilson incorrectly cited as T. C. Wilson.

One unfortunate feature of the high rate of citation error in the published academic literature is that citation errors tend to perpetuate themselves because authors rely on secondary sources when preparing reference lists. This problem is most clearly illustrated in the classic case of O. Uplavici. In 1887, the title of a Czech language article ("O uplavici" or "On Dysentery") was mistaken for the author's name when cited in the medical literature. The work of Uplavici came to be cited throughout the literature on amoebic