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Maternal Attachment, Paternal Overnight Contact, and Very Young Children's Adjustment: Comment on Tornello et al. (2013)

We are compelled to respond to Tornello et al. (2013) because of concerns about measures and theory and because some findings of their study were misleadingly presented. Their results, as described in the abstract and DISCUSSION section, were reported in news media aimed at the general public in the United States ("Divorce Study," 2013; Hallas, 2013; HealthDay News, 2013b) and overseas (Asian News International, 2013; Furness, 2013), physicians (HealthDay News, 2013a; Scutti, 2013), and psychologists (British Psychological Society, 2013; Wood, 2013), making these inaccuracies more serious in their impact. We critique the measure used for attachment in Tornello et al.'s study, the results of the investigation with respect to attachment and adjustment, and the authors' argument about a burden of proof. Finally, we summarize our concerns with this highly publicized yet problematic article.

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MEASURE OF ATTACHMENT

Tornello et al.'s study introduced a new, never-before-used measure for attachment: the Toddler Attachment Q-Sort (TAQS). The authors did no validation for this new measure in comparison to the measure from which it was derived (the Attachment Q-Set [AQS; Waters, 1995]). The TAQS is derived from maternal self-reports rather than from an independent assessor. No published analysis of the validity of this measure is presented, although an abstract of a poster presentation (Howard, Brooks-Gunn, & Lubke, 2008) is cited as evidence that the distribution of the TAQS resembles that of the AQS. Hence, we have little reason to believe that the measure of one of the key concepts in this study, attachment, is valid or reliable: The authors built their evidence using very shaky materials. They list this as a limitation of their study, but only after making strong claims for their evidence.

The measure of attachment used here (i.e., the TAQS) is gender specific; that is, it measures only attachment to mothers, despite previous research that attachment to fathers is important and is related to maternal attachment (van IJzendoorn & Wolff, 1997). Thus, the measure prevents the understanding of how parenting arrangements affect the attachment to fathers and mothers. This deficiency cannot be attributed to Tornello et al., because this appears to have been designed into the Fragile Families survey

(*QSORT Release Memo*, n.d.; Reichman, Teitler, Garfinkel, & McLanahan, 2001), but the authors fail to note this anomaly in the survey and sometimes cloud the gendered nature of their results by referring to “nonresident parents” (p. 882) when their study is confined to nonresident fathers.

ATTACHMENT RESULTS

The hypothesis for Tornello et al.’s study was that “very young children who had frequent overnights with their fathers would have more insecure attachments with their mothers” (p. 874). The authors first tested this hypothesis with a bivariate analysis but with part of the main variable of interest removed; that is, they chose to exclude the category of “no visits” in their results, presenting only categories with visits in their comparative analysis. Because of this, we cannot tell whether the children were more secure with visits or without visits (see Table 3 of Tornello et al.). The stated reason for this is: “Theory and research tend to focus on the difference in attachment security of very young children who experience frequent overnights versus less frequent overnights or day contact” (p. 878). Yet earlier the authors complained about the paucity of research on this topic, finding only three extant articles. If Tornello et al. were keen to show the effects of visits, then why would they not illustrate the benefits of no visits by showing this category? Moreover, this category is excluded in the regression shown in their Table 5 (predicting attachment insecurity). So, a partial variable is in the bivariate analysis, but the complete variable is in the univariate analysis.

The omission of the attachment values from Table 4 is especially strange, given the stated hypothesis/research question, and does not allow us to compare the bivariate relationship with the multivariate relationship in Table 5. Furthermore, the logistic regression model in Table 5 shows that frequent overnight visits *reduce* attachment insecurity relative to some overnights, a finding directly contradicting the hypothesis and statements by the authors, such as “The reported findings are consistent with our hypothesis that frequent overnights away from the primary attachment figure are associated with greater attachment insecurity among infants” (p. 882). Moreover, the logistic regression shown in Table 5 shows no effects from such factors as mother’s depression and poverty, factors that

previous research would lead to an expectation of an effect (Diener, Casady, & Wright, 2003; Millar, 2009). Of course, there is nothing in the literature review that would help the reader develop an expectation with respect to most of the variables in Table 5. Last, as we discuss below, assessing effects on maternal attachment without also measuring attachment to other figures important in a child’s life may give misleading results.

ADJUSTMENT RESULTS

Tornello et al. tested the effect of different modes of father contact on 14 measures of children’s adjustment. Only one of these (presented in Table 6) produced a significant predictor, and that predictor suggests that frequent overnight father contact is beneficial for prosocial behavior. If there were no difference in 13 of 14 outcomes, it would be more consistent with the evidence to report that variation in contact did not generally affect children’s adjustment, except, perhaps, positively.

None of the models predicting children’s adjustment used attachment security as a predictor. Therefore it is difficult, on the basis of this evidence, to make claims as to the importance of attachment for children’s adjustment and, hence, their best interests. The question simply was not addressed empirically. Yet the authors reported that “attachment security predicted child adjustment concurrently and prospectively” (p. 882). Given that no hypothesis was stated with respect to the relationship of visitation frequency and child adjustment, there was no contradiction between the hypothesis and the conclusions for adjustment.

ATTACHMENT THEORY AND THE BURDEN-OF-PROOF ARGUMENT

Tornello et al. argued that the burden of proof for allowing frequent overnight access or visitation to young children should be on those who wish this to occur; that is, that, in the absence of evidence for the benefit of this practice, it should not be considered in children’s best interests. When married, a father is considered beneficial and unproblematic, but after a separation his parenting and relevance to a child become suspect. Yet there is good reason to believe that—in general—contact with the noncustodial father and/or shared custody produces better

attachment, adjustment, or outcomes for children (Kruk, 2005, 2012; Laumann-Billings & Emery, 2000; van IJzendoorn & Wolff, 1997). Coparenting has been associated with increased maternal attachment (Caldera & Lindsey, 2006) and, similarly, using the same survey that Tornello et al. used, so has paternal presence (Martin, Brazil, & Brooks-Gunn, 2013). Even nonparental caregivers can form attachments with children that are important to their development (van IJzendoorn, Sagi, & Lambermon, 1992). Thus, we should have the expectation that, absent evidence to the contrary, visitation and attachment to a father will, on average, be in children's best interests. Also, given that there is evidence to believe that children are often attached to their father when not securely attached to their mother (Fox, Kimmerly, & Schafer, 1991), the discarding of a potential resource to children—who may have greater attachment to their fathers than to their mothers—is difficult to defend. The very contention that young children should be prevented from frequently seeing their fathers overnight unless it is proven that this is not harmful would suggest that Tornello et al. feel they have little empirical evidence to support their position. If there were ample evidence to support this position, no such claim need be made: The evidence would be clear and stand on its own.

Tornello et al. addressed the debate in the literature with respect to attachment to a single caregiver (*monotropy*, a concept that even Bowlby apparently abandoned; Lamb, 2012) versus the importance of multiple attachment figures, yet they argued that a “minority of researchers” support this view, implying that this has a bearing on the veracity of the theory. We contend instead that either multiple attachment figures have a bearing on children's best interests or they do not. The preponderance of evidence has a bearing on this, not the preponderance of researchers. We agree with Tornello and colleagues that the extant research on this question is limited, and we applaud their attempts to add to this literature. But the debate needs a focus on evidence that is correctly and impartially interpreted.

A MINOR ISSUE

There is another, minor, issue that Tornello et al. could have addressed. They highlight child-related costs, such as “expensive baby equipment,” as an impediment to overnight

visits for young children versus older children. This belies evidence that older children cost, on average, far more than younger children (Finnie, Giliberti, & Stripinis, 1995; Millar & Gauthier, 2002). Costs are less of a concern for toddlers and infants than for older children, not more so.

SUMMARY

This article misleadingly states in its abstract: “Frequent overnights were significantly associated with attachment insecurity among infants” (p. 871), giving the impression that this study found that frequent overnights were harmful to infant attachment. Instead, we see in the models presented in Table 5 that frequent overnights were not different than no overnights in predicting attachment insecurity. There was also a tantalizing effect size, which, although not statistically significant, might be something to investigate in future research. This effect suggests the opposite of the authors' conclusions: that no overnight visits increase maternal attachment insecurity compared to frequent overnights. In other words, in this sample (but not in the population from which it was drawn), very young children were more secure, not less, when there were frequent overnight visits with their fathers. One assumes that the authors relied on the data from Table 3 for their claim, as mentioned above, but this is a bivariate analysis, which excluded part of the main variable of interest and did not adjust for other factors expected to influence attachment. A similar statement is found in the DISCUSSION section. To state the results in this way, especially in the abstract, mischaracterizes the findings and could easily lead to a casual reader forming a mistaken impression. The abstract further states that “frequent overnights were not directly linked with adjustment problems at older ages” (p. 871). Frequent overnights were also not *indirectly* linked to adjustment problems—there was no linkage, except to a *reduction* of adjustment problems. To equivocate in this way, especially in the abstract, is again misleading. The problems with Tornello et al.'s study were magnified not only by wide reporting of the results but also by many of the headings broadening the conclusions of the study to “overnight visits.” This article investigated an important subject and presented new data on the distribution of paternal contact with children after separation or divorce. This aspect

of the article is an important contribution. The conclusions reached and the characterizations of the evidence with respect to attachment and child adjustment, particularly in the abstract and the DISCUSSION section, give false impressions of the evidence presented within.

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