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Rejoinder to Millar and Kruk (2014): Who Assumes the Burden of Proof When There's No Neutral Null Hypothesis?

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We welcome the chance to respond to Millar and Kruk's (2014) comment primarily because this gives us an opportunity to expand on an issue that we think deserves broader consideration: Where does the burden of proof lie when there is no neutral null hypothesis? Tests of statistical significance continue to rely on the null hypothesis testing premise. Scientists do not reject the null hypothesis unless statistically significant ($p < .05$) support is found for some alternative. But the null hypothesis is not empty substantively when one is addressing many questions of interest to social scientists. This includes the topic of our research: whether frequent overnights in both mothers' and fathers' households are beneficial or harmful to very young children (Tornello et al., 2013). As a result, advocates of one position or the other often try to capture the null hypothesis, shifting the burden of proof on to the other side. Millar and Kruk do precisely this in their comment, as they have attempted to do elsewhere (e.g., Kruk, 2012; Millar, 2009). They say, "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" (p. xx). We suggest that Millar and Kruk are free to advocate for this position on any ground they wish, except for one: the weight of scientific evidence. Before elaborating, we summarize our study and discuss several questionable and simply wrong assertions made in the Millar and Kruk commentary.

Vehement debates have erupted among child custody experts and advocates about whether it is potentially harmful or beneficial for very young children to spend frequent overnights away from the residential parent with their nonresident parent, typically overnights spent with a father away from a mother (e.g., Lamb & Kelly, 2001; Sroufe & McIntosh, 2011). To date, arguments have been based largely on interpretations of attachment theory or studies of attachment security in relation to other topics (e.g., child care). With only three, limited prior studies on the specific issue, we conducted a secondary analysis using the Fragile Families data set, which had the advantages of including (a) a representative sample (albeit only of 20 major U.S. cities with a population over 200,000), (b) measures of overnight contact and attachment security (assessments rarely obtained in large-scale demographic studies), and (c) data collected for the purposes other than testing a highly charged issue that might be influenced by experimenter bias. Our own interpretation of attachment theory, research, and clinical experience led us to hypothesize that frequent overnights would, in fact, predict an increased risk for attachment insecurity among infants and toddlers.

Consistent with our hypothesis, a statistically significant univariate analysis found that attachment insecurity was highest among infants (birth to age 1) who had frequent overnights with their nonresident parents (43% insecure) compared to infants with some overnights (16% insecure) or day contact only (25% insecure). Of course, families were not

randomly assigned to contact groups, so we used multivariate analyses to control for multiple, potentially relevant selection variables (while noting throughout that correlation does not mean causation). In this analysis, the comparison between the frequent- and some-overnights groups remained statistically significant in the predicted direction (frequent overnights were associated with higher rates of attachment insecurity), even though fathers in the frequent-overnights group were rated by mothers as being better fathers and as having a better relationship with the mother.

In their critique, Millar and Kruk reiterate some methodological limitations of our research, make two clear misstatements in interpreting statistical results, and repeatedly misrepresent how we presented our findings. Let us address each of these issues.

We do not object to Millar and Kruk's methodological concerns about our study. We raised the same points (and more) in the original article when discussing several inevitable limitations on our research. We do find it interesting, however, that Millar and Kruk worry about methodology in regard to our predicted results regarding attachment insecurity, but they do not appear similarly concerned about our one finding that is consistent with their advocacy position. In an exploratory analysis, we found that frequent overnights during the second and third years of life were associated with *more* positive behavior rated at age 5. We analyzed and discussed this finding, which is, to our knowledge, the first and only direct research evidence linking frequent overnights for children age 3 or younger with a positive outcome. We were cautious about interpreting the result, however, because it was one of 28 separate exploratory analyses and the only statistically significant one. (One in 20 should be significant by chance.) Despite their concerns about methodology and our *hypothesized* attachment results (where one of two tests was statistically significant and the other, though not significant, also was in the predicted direction), in their conclusion about our study's findings, Millar and Kruk say, "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" (p. xx).

This brings us to Millar and Kruk's first error in interpreting/describing our statistics. They state that we found no significant results for 13 of 14 outcome measures, but our exploratory analyses actually were not significant for 27 of 28 tests. (7 measures $\times$ 2 assessments [age 3, age 5] $\times$ 2 overnight periods [infancy, toddlerhood] = 28.) Their "13 of 14" number also omits our hypothesized, significant finding for an eighth measure: attachment insecurity. Of course, finding statistically significant results to support a hypothesized finding is very different from picking one significant correlation out of a data table.

In their second error concerning statistics, Millar and Kruk misinterpret the multivariate findings regarding attachment insecurity in Table 5 in our paper. They say, "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" (p. xx). In fact, Table 5 shows consistent, statistically significant differences between frequent overnights and some overnights in predicting attachment security in the hypothesized direction across three different models (each introducing more controls), completely consistent with the univariate analysis. For the most complex model, the odds ratio of secure/insecure attachment was 5.62 *greater* given some overnights (the numerator) than given frequent overnights (the denominator; $p < .05$; for overnights during the first year of life).

To be fair, the title of Table 5—"Predicting Attachment Insecurity"—is confusing. Attachment *insecurity* was our substantive interest, but higher odds ratios in Table 5 actually indicate greater attachment *security* in a given target group relative to the reference group of

frequent overnights. A clearer title would have been, “Predicting Attachment Security.” Still, Millar and Kruk could have double-checked with us about the correct interpretation, which we would have offered while acknowledging our poor wording. Instead, they chose to believe the univariate findings were completely reversed in multivariate analyses and that we misrepresented the multivariate findings throughout the article. Again, both the univariate and multivariate analysis found significantly more attachment insecurity in the frequent-overnights group, with the multivariate analysis pinpointing significant differences to the frequent- versus some-overnight comparison. We correctly interpreted these results throughout our article.

Millar and Kruk also complain that we did not include nonresident fathers who had no contact with their children in our analyses, and they wrongly claim that our analyses in Table 3 excluded the no-contact group while our analyses in Table 5 included this group. In reality, Table 3 and Table 5 are based on identical contact categories, both excluding the no-contact group. As we noted in our article, the relatively frequently studied issue of no contact (vs. some involvement) is categorically different from the rarely studied issue of frequent overnights (vs. less frequent overnights/day contact only). We chose to include the no-contact group for descriptive statistics, so the reader would have a complete understanding of the sample, but we did not include this grouping when predicting attachment insecurity because (a) the comparison of theoretical interest is between frequent overnights and less frequent overnights/day contact only; (b) none of the three previous studies of this topic included a no-contact group; and (c) no one involved in child custody decisions wants to encourage *no* contact between very young children and their nonresident parents, except perhaps in cases of terribly extreme abuse.

We conclude by returning to the issue of the burden of proof when there is no neutral null hypothesis. In this regard, Millar and Kruk make their position clear (as quoted earlier). They suggest that our position is clear too, and write about our article, “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” (p. xx). In reality, this is what we wrote:

We do not know where the burden of proof should lie in the present debate, but we do think it is important to call attention to this general issue as well as the box score. To date, we know of no research showing that frequent overnights are associated with better adjustment among very young children (under age 4) other than one result we report here. On the other hand, the present investigation is the third of four studies of the topic that show some evidence of increased insecurity among very young children who have frequent overnights, perhaps particularly in the face of parental conflict (McIntosh, Smyth, Kelaher, Wells, & Long, 2010; Solomon & George, 1999). Although no study, including this one, has shown that frequent overnights with the nonresident parent *causes* [italics added] greater attachment insecurity, the onus for showing otherwise may be shifting with growing empirical findings. (p. 882)

Millar and Kruk employ a common rhetorical tactic: They try to capture the null hypothesis for the position they advocate. Although such efforts to shift the burden of proof on to one’s opponents are commonly recognized in legal debates, surprisingly the issue is rarely discussed in relation to all kinds of scientific topics where there is no neutral null hypothesis. We are glad to have a second opportunity to point to the importance of this issue. The first was in our original article.

The strong language of advocacy can be more convincing rhetorically than the cautious language of science. We put our faith in science, which is why we conduct research.

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