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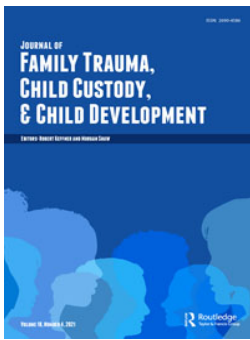


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The trouble with Harman and Lorandos's attempted refutation of the Meier et al. Family court study

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ABSTRACT

Harman and Lorandos assert that they have produced a study analyzing custody cases involving alienation allegations, which “disconfirms” the findings from our study of family court outcomes in cases involving abuse and alienation. In addition to pointing out the authors’ misrepresentation and mis-reporting of some of their findings, this Response details a series of profound flaws in their study’s design, dataset construction and variable coding, interpretations and analytic approach, as well as a series of statistical errors. The statistical analyses demonstrate that Harman and Lorandos’s five findings of a gender bias in favor of fathers are not supported by their data; the only statistically significant findings that persist after re-analysis of the correct data are consistent with the Meier et al. study. These pervasive design and methodological errors undermine both the appearance and assertion of rigor in their approach; these problems and the foundational differences in their dataset from our own disqualify their study from serving as any kind of credible test or disconfirmation of our study.

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In 2019, Meier et al. posted online the required final report (“final summary overview”) on their 5-year National Institute of Justice (NIJ)-funded study, *Family Court Outcomes in Cases Involving Abuse and Alienation Allegations* (Meier et al., 2019; hereafter “FCO study” or “final summary overview”). In 2020, Meier published an article reporting key portions of the study in a peer-reviewed journal. As reported in those two publications, the quantitative findings of the FCO study were consistent with widespread reports that family courts adjudicating custody displayed patterns of decisions that led to mothers having reduced or no custody, even when the father was allegedly abusive. We found that courts frequently reject mothers’ claims of fathers’ abuse of themselves and their children and that the courts often remove custody from mothers alleging abuse and award it to allegedly abusive fathers. The results we reported in these

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two articles showed accused fathers' alienation claims exacerbate these outcomes (Araji & Bosek, 2010; Bemiller, 2008; Berg, 2011; Silberg & Dallam, 2019; Stark et al., 2019). (While lacking any universal or scientific definition, "parental alienation" or "alienation" is generally understood to refer to one parent's undermining of the children's relationship with the other parent.) Most of the FCO study's hypotheses and findings focused on court responses to abuse and alienation allegations. In limited instances, we also were able to look at effects of gender on court outcomes and found some evidence of gender bias, as well as some similarities in mothers' and fathers' outcomes (Meier et al., 2019, p. 18-19).

Jennifer Harman and Demosthenes Lorandos assert in a 2021 article entitled *Allegations of Family Violence in Court: How Parental Alienation Affects Judicial Outcomes*, that the Meier et al. study is seriously flawed, and that their own study provides a "direct and thorough test" of its findings, which it "disconfirms" (Harman & Lorandos, 2021, pp. 185, 186, 191, 205). We rebut their critiques of the FCO study's methods in a forthcoming paper (Meier et al., under review, J. Fam. Ther., Ch. Cust. & Ch. Dev.). Here, we focus instead on Harman & Lorandos's claims about their own study. We will point out multiple serious problems in their methods and analytic strategies, and in their interpretation of their findings and conclusions. We will provide both qualitative and statistical analyses of some of the core errors in their study design, dataset construction and variable coding, interpretations and analytic approach. Our discussion refutes the suggestion that their results "soundly disconfirmed nearly all the findings [they] tested from Meier et al. (2019) report or discovered the findings to be in the opposite direction" (Harman & Lorandos, 2021, p. 22). We conclude by noting the risk that technical jargon, a deluge of statistical and technical detail, and the invocation of transparency (in the form of postings on the Open Science Framework) can mislead readers and reviewers into assuming the scientific validity of a published study that is actually deeply flawed.

Background on Meier, Dickson, Rosen, O'Sullivan & Hayes study

The FCO Study sought to provide an empirical test of widespread criticisms of family courts for their responses to women's abuse reports, found in an extensive qualitative literature (e.g., Jaffe et al., 2003; Silberg et al., 2013; Smith & Coukos, 1997; Stark et al., 2019); small quantitative studies (Silberg & Dallam, 2019; Berg, 2011; Bemiller, 2008; <http://www.CaProtParentsOrg/research>) and widespread public reports on social media and elsewhere (*One Mom's Battle*, 2021; *The Court Said*). These critics describe a resistant and sometimes hostile response from family courts to

mothers' reports of family abuse; the use of parental alienation claims by allegedly abusive fathers to defeat mothers' abuse allegations; courts' awards of unsupervised access and custody to allegedly abusive fathers; and particularly negative responses to mothers' and children's child sexual abuse allegations.

Building on a small pilot study of alienation cases (Meier & Dickson, 2017), the FCO Study proposed to expand Meier and Dickson's innovative approach to gathering data on family court practices: retrieving and analyzing electronically published decisions from across US custody courts to obtain a national picture of such courts' behavior. The National Institute of Justice (NIJ) funded the study. Law graduates were employed to "search for all electronically published decisions in the U.S. in which there were allegations of both abuse and parental alienation, allegations of abuse but not alienation, and allegations of alienation but not abuse" (Meier et al., 2014). The coders simply entered into a database the types of allegations, other case information, and court outcomes.

The reported findings are straightforward, consisting primarily of frequencies or percentages of different outcomes in different categories of cases. The Study found that courts reject mothers' claims of abuse more often than not, and that mothers' claims that the father abused the child are associated with her more frequent loss of custody. Fathers' claims that mothers alienated the child are associated with an increase in courts' disbelief in mothers' abuse claims and an increase in her loss of custody. We also reported some findings that support assertions by alienation theory proponents (Meier, 2020; Meier et al., 2019).

Harman and Lorandos's study does not test, replicate or disconfirm Meier et al.'s findings

Harman & Lorandos acknowledge that Meier et al.'s search string and User Manual were publicly posted months before they published their study. They chose not to replicate the study's method, apparently deeming the large dataset unmanageable (Harman & Lorandos, 2021, p. 7, note 3). Harman and Lorandos assert that the fact that they did not literally replicate our methods nor use our data is not a problem because their study, "as designed, provides a stronger and more transparent test of the [Meier et al.] hypotheses" (Harman & Lorandos, 2021, p. 192). We explain their study was neither strong nor transparent, and that its design made testing Meier et al.'s hypotheses or findings impossible.

We will discuss Harman and Lorandos's article's misrepresentation of the data they posted on OSF, as well as their representation of findings they knew were incorrect. We discuss why their study could not test the

FCO study's findings and analyze key methodological (non-statistical) flaws in their approach, which cast doubt on many of their findings. Lastly, we focus on the statistical errors that compound these design flaws and further impugn their findings.

Lack of transparency and data suppression

Harman and Lorandos acknowledge in a footnote that at some point they discovered, a pattern of mis-coded data in their dataset: Numerous cases in which the court determined parental alienation occurred or did not occur had not been “originally classified in that way” (p. 196, note 5). After correcting the mis-codes, they state that they analyzed their hypotheses again and posted the output on the Open Science Framework. The article then reports that the “research results were similar to those presented in this article; however, the effects were often stronger (see footnote 1)” (Harman & Lorandos, 196, n. 5). An examination of their postings on Open Science Framework reveals this description to be false and misleading.

Over the course of their paper, Harman and Lorandos report five purportedly significant regression results showing that fathers accused of alienation are more likely to lose custody or parenting time than mothers so accused (Harman & Lorandos, 2021, pp. 197-98, 199, 200, 202). As shown in [Table 1](#) below, however, every one of these findings is *not* significant according to the authors' *own regression analyses of the corrected data posted on OSF*. Harman and Lorandos's core gender finding – that fathers accused of alienation are more likely to lose custody of their children than mothers so accused – is an artifact of their miscoded data.

For instance, Harman and Lorandos assert in their analysis of Hypothesis 1 “that fathers had 1.73 greater odds (63.30% greater likelihood) of losing custody of their child(ren) than did mothers ($p = .002$)” (Harman & Lorandos, 2021, pp 197-98). Yet, in their posted analysis of the corrected data, the effect is smaller (OR 1.55) and not significant: $p = 0.053$ (Harman & Lorandos OSF, 2020, *Analyses, CourtDetermined PA or not as predictor, Hypothesis 1*) (see [Table 1](#)). Similarly, the authors assert under Hypothesis 1a that fathers are more likely to lose custody to abusive mothers than the reverse: OR 2.33, $p = 0.037$ (p. 200), claiming this finding is “opposite ... the one that Meier et al. reported” (p. 204). Yet their posted corrected odds ratio suggests mothers lose custody more often than fathers (OR = 0.15), though the difference is not significant ([Table 1](#), $p = 0.999$; Harman & Lorandos OSF 2020, *Analyses, CourtDetermined PA or not as predictor, Hypothesis 1a*). And last, in their Hypothesis 6 findings, Harman and Lorandos assert that “alienated fathers had almost 6 times the odds (85.67%

Table 1. Differences in effect size and significance of independent variables in subsets of Harman and Lorandos's hypotheses 1, 1a, 3, and 6, using reported data with coding mistakes and unreported data with coding corrections.

Predictor	Hypothesis 1 – Outcomes Overall, without Accounting for Abuse Claims			Hypothesis 1a – Outcomes in Founded Abuse Cases			Hypothesis 3 – Outcomes when Alienating Parent Alleges Abuse						
	Alienated Parent Lost Custody		On OSF	Alienated Parent Lost Custody		On OSF	Alienated Parent Lost Custody		On OSF				
	Reported (miscoded data)	Odds Ratio	p	Reported (miscoded data)	Odds Ratio	p	Reported (miscoded data)	Odds Ratio	p				
Gender	1.725	0.002	1.549	0.053*	2.331	0.037	0.015	0.999*	1.6	0.048	1.537	0.171*	
Alienation Founded	2.406	<0.001	3.824	<0.001	2.445	0.028	387.799	0.999*	2.823	<0.001	4.157	<0.001	
Interaction term	1.2	0.296	1.074	0.752	1.238	0.599	163.875	0.999	1.277	0.302	0.986	0.964	
Reported N	953 [^]				122 [^]				386 [^]				
Actual N	189		162		48		35		108		91		
			Hypothesis 6 – Outcome for Alienated Parent, Considering Count of Unfounded Abuse Claims										
			Alienated Parent Lost Parenting Time		Alienated Parent Lost Custody		Alienated Parent Lost Custody		Alienated Parent Lost Custody		Alienated Parent Lost Custody		
			Reported (miscoded data)	Odds Ratio	On OSF (corrected data)	p	Reported (miscoded data)	Odds Ratio	On OSF (corrected data)	p	Reported (miscoded data)	Odds Ratio	p
Count of Unfounded Abuse Allegations	0.564		0.022	0.612	0.064*		0.845	0.204	0.943		0.758		
Gender	5.98		0.031	3.196	0.181*		0.547	0.036	0.675		0.223*		
Alienation Founded	10.08		<0.001	23.536	<0.001		0.396	0.001	0.294		<0.001		
Interaction term	0.661		0.097	0.68	0.146		0.969	0.808	1.094		0.628		
Reported N	348						NR						
Actual N	348			292			93		79				

* Change in significance with corrected data. [^] Number of cases was misreported. Corrected data refers to analyses performed by Harman and Lorandos using their own corrections to the data; these analyses were posted on Open Science Foundation (OSF) but were not included in the manuscript. In the reported data, N was reported as the number of cases submitted to the model; "Actual N" is the number of cases included in the model after missing data were dropped.

Table 2. Comparison of frequency of alienation founding by gender in Harman and Lorandos's uncorrected, reported data and corrected, unreported data.

Reported Data, with		Unreported Data, with Coding Corrections					
Coding Mistakes		Father Accused of Alienation			% Incorrectly Coded in Original Regression	Not Addressed	Grand Total
		Alleged	Founded	Total			
Father Accused of Alienation	Alleged	86	55*	141	39%	72	213
	Founded	17*	215	231	7%	13	245
	Total	103	270	373	19%	85	458
Mother Accused of Alienation							
Mother Accused of Alienation	Alleged	120	44*	164	27%	84	248
	Founded	24*	206	230	10%	17	247
	Total	144	250	394	17%	101	495

* Cells with coding mistakes in reported data. Corrected data refers to analyses performed by Harman and Lorandos using their own corrections to the data; these analyses were posted on Open Science Foundation (OSF) but were not included in the manuscript. In the data reported in the manuscript, N represents the number of cases in the model; "Actual N" is the number of cases included in the model after missing data were dropped.

greater likelihood) of mothers of getting a decrease in custody than did alienated mothers ($p = .031$)” (p. 202). In their corrected data, though, this effect drops to odds of 3.20 and is not significant (Table 1, $p = 0.181$; Harman & Lorandos OSF, 2020, *Analyses, CourtDetermined PA or not as predictor, Hypothesis 6*).

In short, Harman and Lorandos's core gender finding – that fathers accused of alienation are more likely to lose custody of their children than mothers so accused – is simply an artifact of their miscoded data, which is seen in the authors' own *posted but unreported* analyses.

Based on Harman and Lorandos's own analyses of their corrected data posted on OSF, the only significant gender effects that remain significant with their corrected data are (i) that mothers accused of alienation are more likely to lose their case than fathers so accused (Hypothesis 1; corrected data OR 0.68, $p = 0.001$), and (ii) that courts are less likely to credit mothers' abuse allegations than fathers': Hypothesis 2; corrected data Beta -0.20 , $p = 0.004$ (Harman & Lorandos OSF 2020, *Analyses, Found or Alleged as Predictor, Hypothesis 1, 2*; also shown in Tables 1 and 2 below). Both of these findings are consistent with and not “in the opposite direction” (Harman & Lorandos, 2021, p. 204) of Meier et al.'s findings (see generally Meier et al., 2019, p. 18; Meier, 2020, p. 100). Both results indicate that courts disfavor mothers, as we found, not fathers, as they claim to have found.

Oddly, Harman and Lorandos acknowledge in a later footnote (note 7, p. 200) that they did not find gender effects in a separate set of analyses which they characterize as based only on whether or not the court found parental alienation. However, such a variable is not described elsewhere in the paper - and there were only two sets of analyses posted on the

Open Science Framework. If the authors were referring to the second (corrected) analyses on OSF, footnote 7 appears to directly contradict their claim in footnote 5 that the corrected analyses show similar or stronger gender effects.

Further examination reveals why Harman and Lorandos incorrectly found bias against fathers: Their coding errors themselves were systematically biased by gender. As shown in [Table 2](#) below (constructed from Harman and Lorandos's own postings on OSF), the corrected data indicate that at least 39% of fathers' cases were erroneously coded as alleged rather than founded alienation; yet only 27% of mothers' cases were so miscoded. Harman and Lorandos's posted data show that nearly 1 in 5 cases (18%, 140/767) in their data were miscoded. They do not report the extent of this miscoding in the article, and only reference the issue obliquely in the footnote quoted above. The original coding incorrectly coded alienation allegations against fathers as unfounded 46% more often than mothers (RR 1.46, $p=0.032$). This produced Harman and Lorandos's finding of bias against fathers in the courts, mistaking the impact of founded alienation for a gender effect.

Non-statistical methodological flaws

In numerous respects, key codes in the authors' study appear to be internally contradictory or erroneous, with important implications for the accuracy of their tests of their own data. In addition, several of their assertions about their findings are inaccurate.

Problem 1: Harman and Lorandos's dataset is both too narrow and too heterogeneous to produce any useful findings or to test the FCO study findings

Too narrow

The FCO study specifically included abuse cases with and without alienation claims, in order to compare them. Harman and Lorandos's dataset consists solely of alienation cases; they have no abuse cases without alienation claims. Consequently, their dataset cannot test the impact of claims of alienation on court outcomes for parents alleging abuse relative to cases *without* abuse claims.

Too heterogeneous

Harman and Lorandos included every possible sort of case referencing alienation, whether it was a private custody case or a case filed by the

state (e.g., for child neglect), a case in which both parents claimed the other was abusive, or a case occasioned by relocation, incarceration, or any other issue. Harman and Lorandos criticize the FCO study's deliberate exclusion of such cases (Harman & Lorandos, 2021, p. 186), but the FCO study was explicitly focused on *custody litigation between parents* precisely because that is where the field's controversies over abuse and alienation have arisen, and where the particular gender dynamics of mothers vs. fathers are found. Alienation claims are designed for use against an opposing parent, not the State. State cases, in contrast, are not comparing two parents but determining whether children should be removed altogether, often due to neglect by one or both parents. In addition, State abuse and neglect cases are governed by restrictive standards for "substantiation" or "founding" of allegations – these standards are entirely different from custody courts' determinations (by a preponderance of evidence) of allegations of abuse and alienation (Kornblum & Pollack, 2018). These cases are not comparable to parent vs. parent custody battles over abuse and alienation claims, and their inclusion unavoidably compromises any conclusions that may be drawn about custody courts' responses to parents' alienation and/or abuse claims.

The problem with Harman and Lorandos's mixing State and non-State cases can be seen vividly in their definition of their "custody loss" variable – which includes state terminations of parental rights (Harman & Lorandos, 2021, p. 196). In their study, State removals of children into foster care, and even termination of all parental rights, are treated as equivalent to one parent losing primary parenting or most visitation in favor of the other parent.

Another problem with the inclusion of State cases is their equating of neglect allegations with "abuse" (Harman & Lorandos, 2021, p. 194). Neglect claims are common in State cases, but, in our experience, are seldom raised in private custody litigation. "Neglect" typically lacks a clear, objective definition, particularly in child custody statutes. That is why the FCO study excluded them from its definition of abuse (Meier, 2019, App. A, p. 1). State findings of parental neglect, typically addressing failure to feed and/or clothe, get them to school, etc., cannot be compared to the physical or sexual abuse allegations against which alienation cross claims are often brought to bear. In short, inclusion of State-initiated neglect claims in the dataset makes it impossible to draw any conclusions from this dataset about how custody courts addressing alienation respond to actual abuse claims by a parent.

Additional categorization and coding confusions further muddy the waters of Harman and Lorandos's study. Not only do the authors treat a switch of primary parenting from one party to the other the same as a

State termination of parental rights, they also bring into the “loss of custody” category significant limitations of visitation time (Harman & Lorandos, 2021, p. 196). A reduction in visitation or a switch to supervised visitation with one’s child is not comparable to a loss of primary custody, nor to a complete termination of all parental rights. Only four cases were coded in this way, but the decision to code termination of parental rights in two contradictory ways – both as a loss of custody for both parents and as reduced parenting time for one parent –making the findings difficult to interpret on their own, and impossible to compare to ours. Review of their postings reveals that Harman and Lorandos include cases in which *both* parents’ parental rights were terminated in their analysis of rates at which alienating parents lost parenting time (Harman & Lorandos OSF, 2020), further obscuring meaning of results. The FCO study carefully defined loss of custody to mean loss of primary care of the children (Meier et al., 2019, p. 8).

Harman and Lorandos extoll their decision to include every possible type of case, including mutual abuse cases, in their dataset as a means of increasing “external validity” (Harman & Lorandos, 2021, p. 186). Yet the inclusion of not only State cases, but also cases with mutual abuse claims, or where a parent accused of alienation was also accused of abuse, unavoidably confounds their conclusions. Such cases are not comparable to cases where one parent alleges abuse and the other alienation; in the more complex ones, it is impossible to know which allegations drove a court’s decision. Rather than increasing “external validity,” then, Harman and Lorandos’s failure to exclude or control for cases with mutual abuse claims or co-occurring abuse and alienation claims in their regression analysis *decreases* the validity of their findings because they have conflated cases in which an alienating parent lost custody *due to abuse* with those in which that parent might well have lost custody due to alienation.

Harman and Lorandos’s data posted on OSF indicate that only 25% of mothers accused of alienation were simultaneously accused of abuse (124/495), but 38% of fathers were accused of both (176/458), i.e., fathers were 53% more likely than mothers to be accused of both abuse and alienation (RR 1.53, CI 1.21-1.95, $p < 0.001$; Meier et al., 2021, p. 26). If courts were likely to remove children from parents accused of both alienation and abuse more often than parents accused of just alienation, this would be obscured by the authors’ characterization of both types of cases as custody losses for fathers “accused of alienation.” (Harman & Lorandos, 2021, p. 199, 200, 202). Both the study’s exclusions and inclusions render their findings uninterpretable, and their conclusions likely wrong.

Problem 2: Conflating trial court and appellate decisions in coding outcomes

Because the FCO Study's goal was to examine the decision-making of trial courts, which are responsible for evaluating evidence and issuing discretionary decisions about custody and access, that study focused solely on trial court decisions, although it used both trial and appellate opinions as the source of that information (Meier, 2019 App. B, p. 1). In contrast, although their paper is opaque on this point, Harman and Lorandos appear to have coded appellate and trial court determinations interchangeably. For example, they write that the loss of custody "had to occur or be affirmed at the end of the appellate decision" (Harman & Lorandos, 2021, p. 194), and refer to reductions in parenting time "after the trial-level and appeals process" (Harman & Lorandos, 2021, p. 196). Review of their dataset on OSF confirms that they coded custody losses only if the appeals court affirmed them; they coded no custody loss if the appeals court reversed such a ruling (Harman & Lorandos, 2020, OSF, *Data and Process Notes*). They also assert that their "findings indicate that *appellate* courts do not take all claims of alienation or domestic violence or child abuse at face value. These claims are evaluated based on the evidence presented" (Harman & Lorandos, 2021, p. 23; emphasis added).

This last statement evinces a fundamental misconception about appellate process. Appellate courts do not "evaluate the evidence" like trial courts. Nor do appellate courts determine whether abuse or alienation claims are valid, as all first-year law students learn. Unlike trial courts, which take testimony and admit evidence, appellate courts do none of that: They are limited to reviewing the written record to determine if the trial court properly *interpreted the law* and used the correct *procedures* when considering the case (Council of California, 2021). An appellate court could thus (in theory) agree with a trial court's factual finding of abuse or alienation and also agree with a decision not to change custody, but still be compelled to reverse the decision on other legal or procedural grounds. Treating such a procedural reversal as a "loss of custody" is fundamentally erroneous - because appeals courts do not exercise discretion to determine custody, as do trial courts. Rather, appeals court reversals typically simply tell trial courts they did something procedurally or legally wrong, usually resulting in a remand to the trial court to re-decide. Appeals court reversals (or affirmances) thus have no bearing on the substantive questions driving both studies: how *fact-finding* and discretion-exercising courts treat abuse and alienation allegations when deciding custody. By conflating appellate and trial court outcomes, the study makes it impossible to discern how courts are applying their discretion to the facts and claims, the question both studies seek to answer.

Table 3. Comparison of Harman and Lorandos’s original coding of custody loss and re-coding for regression analysis.

Original Custody Loss Coding	Harman and Lorandos’s Re-Coding of Custody Loss for Regression						Total
	Mother Accused of Alienation Custody Loss			Father Accused of Alienation Custody Loss			
	No	Yes	Missing	No	Yes	Missing	
No Custody Loss	0	0	394*	1	0	367*	762*
Mother Lost Custody	0	50	0	22	0	0	191
Father Lost Custody	49	0	0	0	67	0	
Other Party Lost Custody	0	0	2*	0	0	1*	3*

*Cases were improperly coded as “Missing” instead of “No Custody Loss” for regression analysis.

Another legal confusion appears in the authors’ reference to family courts’ findings of “guilty” or “not guilty.” These terms and concepts do not apply in custody litigation, but only to criminal prosecution (Harman & Lorandos, 2021, p. 208 Table A2, p. 195 Table 3).

Problem 3. Additional problem with custody loss variable

Harman and Lorandos’s definition of “Total Loss of Custody” includes awards of extremely limited supervised visits. They offer no rationale for this decision (Harman & Lorandos, 2021, p. 194). Cases coded as “custody losses” thus include the following examples, among others: “Father’s visitation [was] cut down to four hours of supervised visitation every other weekend,” “Father was given limited supervision with his son,” and “Mother’s petition to relocate granted. Father allowed continued supervised visitation” (Harman & Lorandos OSF, 2020, Cases 547, 597, 631). In some of these cases, fathers only had visitation to begin with, yet these outcomes are still coded as a “total loss of custody.” As far as we can tell from a review of the coding notes, these codings appear for fathers but *not* for mothers in the study’s dataset. The net effect is that mothers’ actual losses of primary custody are equated with fathers’ visitation *reductions* or unchanged restrictions, creating a systemic gender bias.

Problem 4. Treating professional opinions as equivalent to court opinions in defining “founded” alienation

The FCO Study explicitly set out to analyze *court* rulings, defining “crediting” of alienation and abuse allegations to mean judicial findings or the equivalent, i.e., party admissions or criminal convictions for abuse (Meier, 2020, 103 note 8; User Guide, Appendix B). Harman and Lorandos code alienation as “founded” if *either* a judge *or* a neutral appointee (custody evaluator or guardian ad litem) believed a parent had alienated a child from the other parent (Harman & Lorandos, 2021, pp. 194, 200-201). They treat alienation

as “founded” even when the court *disagreed* with the appointed professional on that point (Harman & Lorandos, 2021, p. 194). But when a court disagrees with a professional’s testimony, it is the court’s finding that drives the court’s decision. There are a number of important statistical tests that could have been affected by this problematic coding decision. For instance, both their Hypotheses 1 and 1a address the impact of “founded” alienation on court outcomes (Harman & Lorandos, 2021, pp. 197-199), yet their analysis includes cases where the court did *not* find alienation.

A similar problem may be embedded in Harman and Lorandos’s discussion of “founded” abuse (Harman & Lorandos, 2021, p. 194). As with alienation, the authors speak of coding allegations as founded or not based on decisions of *either* “investigators or the court” (Harman & Lorandos, 2021, p. 196). They refer to abuse allegations in terms that reflect outside evaluations, not court findings: They code them as “substantiated” or “unsubstantiated,” “false” or “unknown” after an “investigation” (Harman & Lorandos, 2021, Appendix A2). “Investigation” is a term used for evaluators, not courts’ fact-finding. The authors also remark that a finding of abuse can be based on “something as little as an “inarticulable hunch” of a CPS caseworker” (Harman & Lorandos, 2021, p. 194). They also describe “founded” abuse as a “highly discretionary and unconstrained conclusion drawn by investigators,” without mentioning courts’ concurrence or lack thereof (Harman & Lorandos, 2021, p. 205). It appears that they have deemed abuse claims founded or unfounded based on the results of outside “investigations” by evaluators or agencies, as well as by courts. Therefore, it seems possible that, as in their handling of alienation, they coded as “unfounded” abuse allegations that were deemed unfounded by outside evaluators or “investigators,” *even if the courts, after hearing more evidence, deemed them credible*. If so, it would help to explain their odd conclusion that courts award *more* parenting time to parents who make allegations coded as false, and less parenting time to parents subject to “unfounded” accusations (Harman & Lorandos, 2021, p. 202). They call such unfounded claims a “silver bullet” strategy that rewards the parent making false accusations (Harman & Lorandos, 2021, p. 205). Given the illogic and unlikelihood of such an outcome, we wonder if those cases they describe as “rewarding false claims” actually involved abuse allegations coded as “unfounded” by an outside investigator but validated by the *court*.

Problem 5. Inapplicable evaluator/GAL analysis (Harman & Lorandos hypothesis 4)

Harman and Lorandos assert that their analysis of the impact of court-appointed professionals on mothers’ vs. fathers’ outcomes undermines faith

in Meier et al.'s finding that the presence of court-appointed neutral professionals, i.e., Guardians Ad Litem (GALs) or custody evaluators, significantly disfavors mothers (Harman & Lorandos, 2021, pp. 200-201; Meier et al., 2019, pp. 21-23). But the FCO study compared cases with these professionals to those without: Harman and Lorandos simply compare mothers' vs. fathers' outcomes in cases with these professionals and find no difference. This finding offers the surface appearance of gender equality but says little about true gender equity because that would depend at minimum on whether the mothers' and fathers' cases involve comparable facts and postures.

Harman and Lorandos contradict themselves regarding which cases they actually analyzed when they were looking at the effects of professionals testifying. They state at one point that their analysis was restricted to cases in which a custody evaluator or GAL made a finding of alienation (Harman & Lorandos, 2021, p. 200), but then state that the finding of no gender difference in outcomes applies "whether they were found to be alienating parents or not" (Harman & Lorandos, 2021, p. 201). If they did limit the analysis to "founded" alienation, such an analysis is of questionable meaning and has no bearing on the FCO study's finding.

Problem 6: Apparent misinterpretation of own finding

Harman and Lorandos's Hypothesis 2 reads as follows: "When a mother claims intrafamilial abuse and the father claims PA, her reports of abuse will be deemed unfounded more often than if the father claimed abuse and the mother claimed PA" (Harman & Lorandos, 2021, p. 199). This hypothesis, while not identical to ours, does parallel one of the FCO study's gender-specific findings. We found that when a mother accused a father of abuse and he accused her of alienation, outcomes for mothers were significantly worse than if he did not make the alienation claim; the same was not true for fathers when they alleged abuse by the mother and the mother accused them of alienation (Meier et al., 2019, pp. 18-19, 22-23).

Harman and Lorandos conclude that they "did not find support for Hypothesis 2," because "fathers were more likely than mothers to have unfounded allegations of abuse made about them" (Harman & Lorandos, 2021, p. 200). This statement actually *confirms their hypothesis* that mothers' abuse allegations are "deemed unfounded" more often than fathers'. That is, if allegations against fathers are more often "unfounded" than allegations against mothers, which means that *courts disbelieve mothers' abuse allegations more often than fathers' abuse allegations*. We can only surmise that Harman and Lorandos's failure to understand their own verbiage reflects their confirmation bias and determination to disprove the FCO Study's

findings and others' critiques of court biases against mothers. An objective reading of *their own words* compels recognition that their finding is actually *consistent with* both the FCO Study's findings and the abuse field's critique.

Problem 7. Additional concordant findings denied

Harman and Lorandos fail to acknowledge the apparent convergence of at least two more of their findings with the FCO study's. In their analyses of Hypotheses 1 and 1a, they conclude that parents found to be alienating are more likely to lose custody or parenting time, regardless of gender. They cite this finding as "fail[ing] to...support the conclusions made by Meier et al." (Harman & Lorandos, 2021, p. 183). Yet the FCO study produced virtually the same finding (Meier et al., 2019, p. 18). Their finding related to Hypothesis 1a is also consistent with the FCO Study (Meier et al., 2019, p. 17). Both concluded that even "founded" abuse is sometimes outweighed by findings of alienation (Meier et al., 2019, pp. 15, 18). It is hard to understand Harman and Lorandos's refusal (even after it was pointed out to them, <https://osf.io/j9bh5>) to recognize that these findings are convergent.

The appearance of bias also arises from Harman and Lorandos's conclusion that "the majority of courts carefully weigh allegations of all forms of family violence" (Harman & Lorandos, 2021, p. 183). This was not a question that was asked or answered in any of their hypotheses or statistical analyses - nor ours. Their assertion is based on a handful of interviews with founded abusers' attorneys (Harman & Lorandos, 2021, pp. 204-205). Such opinions provide no objective assessment, let alone scientifically valid evidence.

Statistical errors

Statistical error 1. Improper interpretation of the effects and significance of key independent variables

In their regression analyses, Harman and Lorandos improperly interpret the effects and significance of their independent variables in two regards: 1) they fail to consider the interaction term when interpreting the effect of gender and alienation as individual covariates, and 2) they interpret the interaction term in their non-linear models as though they had used a linear model. These mistakes cause them to state the effect of key variables incorrectly as well as to improperly test several of their hypotheses which turn on the significance of the interaction term.

Misinterpretation of main effects

In discussing Hypothesis 1, Harman and Lorandos report that they “found a statistically significant gender main effect, such that fathers had 1.73 greater odds (63.30% greater likelihood) of losing their child(ren) than did mothers ($p=.002$.)” Similarly, in regard to alienation they found that, “if there was a known alienating parent, this parent had 2.41:1 greater odds (70.64% greater likelihood) of losing custody of their children than was an alleged alienating parent ($p = .002...$)” (Harman & Lorandos, 2021, p. 197). Both of these findings are drawn from their regression output for the individual gender and alienation variables. However, their regression model also includes an interaction variable for gender and alienation, which is the gender variable multiplied by the alienation variable. Because the gender and alienation variables are also present in the interaction variable, it is not accurate to report the effect of the individual variables without factoring in their presence in the interaction variable. In its most simple format, Harman and Lorandos’s regression equation for custody loss takes this form (omitting the intercept and error terms for simplicity):

$$\text{Custody Loss} = \beta_1 * \text{Gender} + \beta_2 * \text{Alienation} + \beta_3 * (\text{Gender} * \text{Alienation}).$$

In their article, Harman and Lorandos report the parameters for β_1 and β_2 as the effects of gender and alienation (Harman & Lorandos, 2021, p. 98, Table 7), but they have neglected to account for the effect of β_3 , causing them to misreport the effect of gender and alienation in the model (Darlington & Hayes (2017, p. 433). Jaccard & Turrisi (2003, p. 24) specifically caution that these coefficients should not be interpreted as main effects, as Harman and Lorandos have done, when there is an interaction variable in the equation.

Moreover, because Harman and Lorandos’s model is (necessarily) non-linear, this error cannot be rectified by simply adding the interaction effect (β_3) to report the effect of gender or alienation as in a linear model. In a non-linear model, this addition would have to take place before transforming the regression coefficients into an odds ratio, resulting in an uninterpretable “ratio of ratios.” Due to this complexity, *when a non-linear model includes an interaction term, odds ratios are considered an inappropriate means of interpreting the effect of any particular variable* (Karaca-Mandic et al., 2012, 266-7). Rather than interpreting main effects and interactions as though the model were linear, the researcher must estimate *average marginal effects* predicted under the model for the variable of interest, here, gender (Karaca-Mandic et al., 2012, 270). We perform this analysis in the final section of this article.

Misinterpretation of interaction term

In addition to misinterpreting the main effects of gender and alienation because they are also present in the interaction term, Harman and Lorandos misinterpret the significance of their gender-alienation interaction term in several of their analyses. They conclude that their Hypothesis 1 is unsupported because the p-value of the interaction term does not reach statistical significance (Harman & Lorandos, 2021, p. 198). Because their model is non-linear, the significance of the interaction term varies at each point on the distribution, which means one cannot interpret the significance of the interaction term based solely on the p-value reported in the regression output, as one would with a linear model. This treatment of a non-linear model as though it were linear is considered a “common mistake” (Buis, 2010, p. 305; Ai & Norton, 2003, p. 129). The appropriate test of significance for an interaction term in a non-linear model is to consider the “difference-in-differences” in the average marginal effects predicted for the interaction of the two variables of interest. We perform this analysis for Hypothesis 1 in the final section of this article.

Statistical error 2. Improper construction of custody loss variable

Compounding the errors in interpreting main effects and interactions, Harman and Lorandos (presumably unintentionally) deviated from their analytic plan when they constructed their custody loss variable for the regression. This error results in over 80% of the cases in their data set being omitted from the analysis, as can be seen in [Table 3](#) below.

The authors’ analytic plan states that the dependent variable is coded as “custody loss” if the alleged alienating parent lost all parenting time and “no custody loss” if the parent did not lose all parenting time (Harman & Lorandos, 2021, p. 195, [Table 3](#)). Yet, when they construct the custody loss variable for the regression, they instead compare custody losses to custody *gains* by an allegedly alienating parent (rather than to *no custody loss*). [Table 3](#) below compares Harman and Lorandos’s original coding of custody loss with their re-coding of the custody loss variable used in the regression analysis.

The dataset posted on OSF contains 953 cases with a coded custody outcome (p. 196) of which 191 cases result in a custody loss and 762 do not result in a custody loss (Harman & Lorandos OSF, 2020, “Data and Process Notes, SPSS cleaned appellate case dataset”). As shown in [Table 3](#) above, only 191 cases with a custody loss are included in the custody loss dependent variable in the regression posted on OSF. Because the regression equation only compares one parent’s custody

Table 4. Comparison of effect and standard coding of gender and alienation as predictors of loss of custody in binary logistic regression.

Covariates	Results with Effect Coding (+1/-1)		Results with Standard Coding (1/0)	
	Odds Ratio	p-value	Odds Ratio	p-value
Gender	1.73	0.002	2.07	0.119*
Alienation	2.41	<0.001	4.02	0.001
Interaction Term	1.2	0.296	2.07	0.296
Model Fit Statistics				
N	189		189	
Chi-Sq	39.91		39.91	
Pearson	<0.0001		<0.0001	
Log-Likelihood	-105.64		-105.64	
Pseudo R ²	0.16		0.16	

* Change in significance due to use of “effect coding”.

Note: This table shows that the coding change from 0/1 to -1/1 affected the regression output for the Gender, Alienation, and Interaction terms. Harman and Lorandos erroneously did not change their interpretive approach, reporting the results of their regression as though Gender and Alienation were coded as 0/1 rather than as -1/1.

losses to the other parent’s custody losses in order to determine in how many cases custody was lost, it leaves out all of the cases where *neither* parent lost custody.

Harman and Lorandos report in their [Table 3](#) an N of 953 for custody loss regression, but regression output on OSF shows that 764 of those cases were not included in the regression, having been omitted and called “missing data.” Similar discrepancies are seen between their OSF data and their published article’s reports of the Ns for Hypotheses 1a, 3, and 6 ([Table 1](#)).

The same error also leads to systematically biased gender findings. The erroneous regression coding of custody loss versus custody gain, rather than custody loss versus no custody loss, overstates the rate at which fathers lost custody relative to mothers. Comparing losses to gains, as Harman and Lorandos did, fathers accused of alienation lose custody 74% (67/89) of the time compared to mothers 51% (50/99) ([Table 3](#)). When all the actual cases with no custody loss are included in the regression variable for “no custody loss,” as envisioned by the analysis plan, fathers lose custody only 15% of the time (67/457), compared to mothers’ loss of custody 10% of the time (50/495) ([Table 3](#)). Thus, under erroneous analysis, fathers have 2.98 times greater odds of losing custody than mothers ($p < 0.001$). Corrected analysis reduces these odds by approximately 50%, to 1.53 ($p = 0.041$).

This significant gender effect is a simple effect and does not control for whether alienation was founded, a variable with enormous impact on custody outcomes according to both studies. In [Table 4](#) of their article, Harman and Lorandos report that 72% of fathers compared to 63% of mothers were found to be alienators (Harman & Lorandos, 2021, p. 197). This means that fathers had 1.5 times the odds of mothers of being found to be alienators ($p = .01$); thus, the significant gender difference in custody loss. After we corrected for Harman and Lorandos’s compounded errors,

Table 5. Logistic regression of custody loss, by gender, alienation alleged, alienation found, and interaction; summary statistics and predicted frequency of custody loss (average marginal effects).

Observations N = 767		Pseudo R-Squared = 0.06		Log Likelihood -281.98
Predictor	Average Marginal Effects – Predicted Custody Loss		Frequency in Each Scenario	p-value
	Margin	Difference		
Mother	11.69%	3.11%		0.194
Father	14.81%			
Alienation Alleged	3.67%	14.07%		<0.001
Alienation Found	17.74%			
Mother, Alienation Alleged	3.47%	12.13%		<0.001
Mother, Alienation Found	15.60%			
Father, Alienation Alleged	3.88%	16.12%		<0.001
Father, Alienation Found	20.00%			
Difference-in-Differences (Mother vs. Father, Alleged vs Found)			3.99%	0.335

we found no significant main or interaction effects involving gender in our regression model. (See Section IV below.)

Statistical error 3. Improper use of “effect coding” for gender and alienation

Harman and Lorandos note that they had originally intended to dummy code the independent variables “Gender of (alleged) Alienator” and “Founded or Alleged PA” as 0/1, but they changed the coding to $-1/1$ “to compare the two groups with the interaction term” (Harman & Lorandos, 2021, p. 195, Table 3), a technique called ‘effect coding’ (Hardy, 1993, p. 64). Harman and Lorandos wanted to “compare the two groups with the interaction term” yet they seem to have overlooked the change in dummy coding when interpreting their regression output, rendering their interpretation of the odds ratios incorrect.

“Effect coding” is used when “one wants to contrast subgroups with a sample average,” (Hardy, 1993, 64), as the authors say they intended to do (Harman & Lorandos, 2021, p. 195). Effect coding evaluates the odds of an outcome occurring for a particular group compared to the population as a whole, rather than comparing all effects to a particular reference group (the standard approach). When using effect coding, the interpretation of the ‘1’ value, in this case “fathers,” must be compared to the average value for the overall sample, here represented by zero, *not* to the reference group coded as -1 , in this case “mothers” (Hardy, 1993, 64). In testing the relationship of religious identity to political beliefs, a researcher may choose to code Catholics as -1 , Lutherans as 1, Jews as 2, and Methodists as 3 to compare the beliefs of each group to the average beliefs across the entire population (represented as ‘0’), rather than only comparing Lutherans, Jews, and Methodists to a reference population of Catholics.

Harman and Lorandos's switch to the effect-coded approach means that when they attempt to interpret the coefficient of the "gender" term, they are interpreting the odds of something happening for fathers compared to average odds of that event happening overall (for fathers and mothers). Though they claim to be comparing fathers to mothers, saying, for example, "fathers had 1.73 greater odds ... of losing custody of their child(ren) than did mothers" (197), this interpretation relies on the reported odds ratio that compares the value of '1' (fathers) to '0' (sample mean of mixed population), not '1' (fathers) to '-1' (mothers) (Jaquard and Turrisi, 24).

This error provides an independent reason that their conclusion about the relative odds of custody loss for father compared to mothers is incorrect, apart from the previously described errors. In Harman and Lorandos's coding approach, fathers are significantly more likely to lose custody than mothers, OR 1.73, $p=0.002$ (Harman & Lorandos, 2021, p. 198, Table 5), but when we replicate the analysis using the coding detailed in the analytic plan (Table 4 above), this effect is not significant: OR 2.07, $p=0.119$.

Harman and Lorandos's oft-repeated conclusion that fathers accused of alienation lose custody more often than mothers is an artifact of multiple statistical and coding errors. They fail to account for the presence of an interaction term when interpreting their regression output; they exclude 80% of cases from the custody loss analysis, injecting systematic bias; and they misinterpret their regression output due to deviations from the variable construction in their analytic plan. They report analyses from data they know contain significant mis-codings, which happen to be systematically biased by gender, while failing to report the corrected (and not statistically significant) results. These compounded errors mean their reported conclusions are not supported by their underlying data, *even if their dataset were appropriate*.

Re-analysis of part of hypothesis 1 with corrected coding and regression methodology

Given that we are providing a commentary and not a replication study, we do not re-assess all of Harman and Lorandos's regression analyses here. We provide a re-analysis of the custody loss component of their Hypothesis 1, with the errors corrected (Meier et al., 2021).

In Table 5, we show the regression analysis correcting the data set to include all "no custody loss" cases, rather than just "custody gain" cases, and using Harman and Lorandos's analytic plan's standard coding of 0/1 for gender and alienation crediting. Rather than report the odds ratios and coefficients for the independent variables, which, as we described previously, cannot be properly interpreted on their own due to the presence of the interaction term, we present the predicted average

marginal effects and their significance. The corrected findings indicate that, regardless of gender, “founded” alienation increases the frequency of custody loss by 14.07% as compared to *alleged* alienation, a statistically significant increase. This finding that custody loss is driven by founded alienation, not gender, is consistent with, not the opposite of, Meier et al.’s finding that “[b]oth mothers and fathers lose custody at identical rates when the court deems them an alienator” (Meier, 2020, p. 100; Meier et al., 2019, p.19). Contrary to Harman and Lorandos’s claim, the corrected analysis shows that there is no significant gender difference in custody losses in response to an alienation claim: the predicted 3.11% difference between mothers’ and fathers’ custody losses is not statistically significant ($p=0.194$). We show our analysis only for Hypothesis 1, but it demonstrates that Harman and Lorandos’s key conclusions that mothers fare better than fathers in court, and their study contradicts Meier et al.’s findings, are unsupported when the errors enumerated above are corrected.

Conclusion

We have argued that Harman and Lorandos’s study constitutes neither a “direct and thorough test” of Meier et al.’s research, nor a credible analysis of the issues both studies seek to address. The statistical and non-statistical problems we have detailed are easily overlooked by those who are not steeped in social science research methodology or who are lulled by Harman and Lorandos’s confident tone and presentation, replete with complex (if erroneous) claims about both their data and their analyses. Our labor-intensive deconstruction of their sampling method, coding, and analyses and interpretations of both their own and our results reveals fundamental problems at every stage.

We conclude by noting that the state of the discourse around these matters of critical importance to our courts and the well-being of children (and parents) is regrettable. There are genuinely important questions here – most fundamentally, how often are abuse allegations true, false, or knowingly fabricated in family court? How often are parental alienation claims deployed to nullify credible risk? Neither study can directly answer these questions, but they are at the root of the divide between those who espouse the view that parental alienation and false abuse allegations are common phenomena. Answering these questions requires reasoned and deliberate research and consideration by objective scholars and practitioners acting in good faith.

As is detailed in the second part of our rebuttal of Harman and Lorandos’s article (Meier et al., under review, JFT), we believe the FCO

study's empirically clean, careful accounting of court findings and decisions in these cases invites and compels this conversation. Rather than engage directly with our study's findings and implications, we fear that Harman and Lorandos's article is agenda-driven, filled with error, and does more to obfuscate the issues than to shed light on courts' practices.

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