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PUTTING SCIENCE AND REASONING BACK INTO THE “PARENTAL ALIENATION” DISCUSSION: REPLY TO BERNET, ROBB, LORANDOS, AND GARBER

Madelyn S. Milchman, Robert Geffner, and Joan S. Meier

This article presents our *Reply* to the *Responses* that Lorandos, Garber, Bernet, and Robb wrote to our *Critique* article in the *Family Court Review Special Issue*. Our *Reply* focuses on the repetition of the rhetoric that some of these authors used to distract from the issues we raised. The principal concern in our *Critique* article was the lack of adequate research methodology to support claims that parental alienation is a diagnosis, a condition, or a phenomenon that is directly observable rather than an inference that requires detailed behavioral descriptions, factual analysis, logical and scientific reasoning. In our *Reply*, we show how this concern was not properly addressed in the *Responses*, which does not facilitate meaningful dialog. However, we also recognize that some of the *Responses* facilitate improved dialog between the parental alienation and child abuse/domestic violence communities and we welcome that.

Keywords: *Abuse; Alienation; Child Custody; Child Maltreatment; Family Court; Research Methodology; Rhetoric.*

I. INTRODUCTION

Our main *Critique* article in this *Special Issue* sought to reduce the hostilities and lack of meaningful dialog between scholars who focus primarily on alienation and those who focus primarily on child abuse and domestic violence. We proposed that rhetoric in the service of ideology creates obstacles to reasoned intellectual exchange. Bernet, Garber, Lorandos, and Robb responded to our *Critique* of their articles. This *Reply* responds to their *Responses*, with an eye to whether they help advance future exchange or continue to use rhetorical strategies that fail to facilitate meaningful dialog about substantive issues. Due to time constraints we could not develop one narrative voice, so this *Reply* has separately authored sections; however, we all agree generally with each.

II. REPLY TO GARBER AND LORANDOS

A. MADELYN MILCHMAN, ROBERT GEFFNER & JOAN MEIER

We appreciate and welcome the *Responses* from Benjamin Garber and Demosthenes Lorandos, both of whom appear to agree that some of our concerns about the misuses of the alienation label warrant serious attention. We also appreciate and agree with Garber’s objection to the pathologizing of relationships as “disorders.” We are particularly pleased to see that Lorandos echoes our concerns about the importance in practice of protecting children from abuse and avoiding the use of the parental alienation (PA) label to sideline such concerns (Lorandos, 2020, p. 371), an acknowledgment we have seen in theory, but that too often in our experiences is ignored in practice.

We also welcome Garber’s call to move away from individual “diagnoses” to a focus on family systems. As noted in our initial *Critique* in this special issue, we too feel that family courts and

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evaluators rely far too much on pathologizing labels, especially those with dubious empirical foundations. However, we find that his hypothetical example (Garber, 2020, p. 369) raises questions: Garber posits a case where a child is resisting contact with dad, mom is seen as having a “personality disorder,” dad has committed intimate partner violence (“IPV”) and Billy has ADHD. We agree with him that the two diagnoses (the personality disorder and the ADHD) are both unhelpful and potentially misleading for determining a child’s best interests (especially given that “personality disorder” is the kind of “diagnosis” historically and routinely applied to survivors of domestic violence) (Erickson, 2005, p. 88).

Nevertheless, we want to emphasize that the history of IPV raised in Garber’s illustration needs to be the major focus of attention, even while recognizing multiple factors that could influence any particular case of resistance. Such a history is a matter of fact (presumably), and if Billy states that his dad scares him, we think there is little mystery to his contact resistance. Rather, where there has been family violence and the child is afraid of the perpetrator, the remedy to such a relationship breach will require the perpetrator of IPV to acknowledge the harm he has caused the family, accept Billy’s feelings about it, and work hard to regain his son’s trust and sense of security. In short, we would assert that histories of known abuse or IPV should be the dominant factor driving contact decisions, at least where a child is afraid or angry toward the perpetrator. While it is unclear to us how much Garber prioritized IPV in this particular hypothetical, his *Response* indicates common ground that helps to promote ongoing dialog.

III. REPLY TO BERNET

A. MADELYN MILCHMAN

Bernet (Bernet, 2020, p. 362) states he wants to create “constructive dialog,” but we believe his *Response* to our article does not foster this. He decries “the publication of cascading criminations, recriminations, and re-recriminations,” including the publication of our article. However, Bernet is a contributor to this cascade because his response to our *Critique* creates just such a “cycle of rhetoric.”¹ He largely repeats and justifies his original arguments, doubling down on them rather than acknowledging any merit in our comments, or attempting to identify any significant common ground, as Garber and Lorandos do in this *Special Issue* (Garber, 2020; Lorandos, 2020).

Bernet criticizes the editors, Bala and Fidler, of this *Special Issue* for attempting to promote a dialog among writers with these different perspectives by publishing articles from each perspective. He calls instead for face-to-face discussion. We do not agree that face-to-face discussion should replace published versions of diverging opinions. We agree with the editors that published critiques have particular value because they leave participants time to reflect before they respond. Furthermore, published critiques reach a wider audience than private discussions among those with differing perspectives. Finally, publication of conflicting ideas within a single journal is an effective way to reach readers who do not usually read journals publishing articles supporting one side of a debate. That said, we now address Bernet’s (Bernet, 2020, 362–367) specific responses to our *Critique*, pointing out the use of communication strategies that obstruct substantive dialog.

1. Bernet Trivializes the Issue of Labeling

Bernet (Bernet, 2020, p. 363) dismisses the issue of labeling as “a matter of ‘semantics’” and does not appreciate or agree with our point that using capitals to denote “Parental Alienation” implies that it is a diagnosis. Perhaps Bernet does not mean to imply a clinical diagnosis by using the term “PA.” Perhaps he is only using capitals as an acronym to describe alienating behaviors, akin to the common use of “DV” to refer to “domestic violence” or “CSA” to refer to “child sexual abuse.”

However, this does not seem to be the case because in his original article he states, “for purposes of this article, PAS and PA are meant to be *synonymous*” [emphasis added] (Bernet, 2020, p. 293). He also defines both the same way, with the “eight characteristic behaviors identified by Gardner” for “diagnosing” PAS (Bernet, 2020, p. 294). He also says, “despite different points of view, almost every scholar who addresses these topics would agree with this generic definition of *both* PAS and PA” [emphasis added] (Bernet, 2020, p. 294). Further, he states, “critics and detractors of PAS/PA routinely lump these concepts and terms together,” and concludes “thus, the concepts of PAS and PA have much more in common than they differ” (Bernet, 2020, p. 294). Finally, he states, “both terms are used” in his article (Bernet, 2020, p. 294). In short, in his article he uses “PA” interchangeably with PAS and PAD, which he treats as diagnoses. As a result, in our article we referenced “PAS/PAD/PA” to indicate that these were alternative terms for the same purportedly diagnostic construct.

In his *Response*, however, Bernet denies that he uses PA as a diagnostic term. Objecting to our statement that he claimed the American Academy of Child and Adolescent Psychiatry (AACAP), Association of Family and Conciliation Courts (AFCC), and American Academy of Pediatrics (AAP) “recognize PAS/PAD/PA as a clinical diagnosis” (Bernet, 2020, p. 363), he said he meant that these organizations merely “recognize the concept or the phenomenon of PA,” which he argues means they recognize “the reality of PA” (Bernet, 2020, p. 366). There are two problems with changing his claim in this way.

First, these two statements are contradictory. Either he is using the terms synonymously to refer to a diagnosis, as he said repeatedly in his original article, in which case our point is justified, or he has now decided to use “PA” as an acronym for mere behavior, which would suggest that he is no longer advocating his original position that “PA” is a diagnosis synonymous with PAS and/or PAD. This kind of shifting claim makes constructive dialog more difficult.

Second, Bernet’s use of terms that have different meanings as if they were synonymous is not limited to PA/PAS/PAD, and consideration of the other terms he uses interchangeably shows that making them into synonyms supports his claim that PA has “reality.” In addition to PA/PAS/PAD, Bernet uses the term “concept” interchangeably with the terms “phenomenon” and “condition” to refer to PA. However, a concept is an idea, whereas a phenomenon is an observable fact (*Webster’s seventh new collegiate dictionary’s*, 1963). Similarly, in the vernacular, the term “condition” refers to “a state of being” (*Webster’s seventh new collegiate dictionary’s*, 1963), and in *DSM-5* (pp. 715—727), with few exceptions (e.g., spiritual problems), “conditions” are external events with observable facts.

However, “alienation” is not directly observable or objectively knowable or indisputable. It is a concept based on the observer’s *interpretation* of behaviors, which are the only facts that can be directly observed. His interchangeable use of these terms despite their different meanings conceals that he is making an inference which could be disputed.

Garber’s *Response* (Garber, 2020, p. 369) recognizes the power and danger of labeling when he cites cognitive science’s recognition that language shapes thinking because “words reify experience.” Applying a label such as “parental alienation” to a child’s contact resistance or rejection misleads decision-makers into believing that “alienation” (i.e., blameworthy conduct by a preferred parent) has been directly observed as an objective fact. It masks the reality that it is a conclusory opinion, which depends on an *interpretation* of the facts. It encourages decision-makers to: (1) resist accepting such an opinion at face value but assess whether the opinion is logical or tainted by bias and subjectivity (Saunders, Faller, & Tolman, 2012); (2) independently assess the facts as objectively as possible; and (3) consider other interpretations that might be legitimate.

For those asserting that alienation contributes to parent resistance or rejection in a particular case, the critical requirement is to demonstrate with sufficient factual evidence that a child’s parental rejection is unjustified and that the preferred parent is *at fault* and *to blame* for that rejection. Blame and the causes of a child’s rejection of a parent are not directly observable or objectively knowable, and they are frequently disputed, as is attested to by conflicting expert opinions. Any legitimate conclusion that “alienation” is responsible for a child’s resistance or rejection of one parent,

therefore, requires thorough assessment and factual analysis of the parents' and child's behaviors and that of others, and the context upon which the conclusion is based. Even then, it is an opinion, an inference, from facts and not the facts themselves.

Future terminology should make clear that alienation is a concept and not a phenomenon or a condition, which goes beyond the issue of capitalization and should not be dismissed as mere semantics. Writers should explicitly call attention to the status of alienation as a concept, using such phrases as "the concept of alienation" rather than just "alienation" so readers do not slip into thinking that "alienation" has been observed directly rather than appreciating that it has been inferred from observable behaviors. This might sound petty, but it is not because inferences warrant careful scrutiny of whether they are actually supported by the behaviors on which they are purportedly based, whereas observable facts can be directly assessed by fact finders themselves. Bernet's shifts in terminology change the foundation for his argument, supporting advocacy over balanced dialog.

2. Bernet Argues that DSM-5 Did Not Reject the Diagnosis of PA

Bernet's *Response* (Bernet, 2020, p. 364) states, "in the end, the concept of PA was expressed in DSM-5, but not the actual words." He goes on to claim, "PA is an example of the novel *diagnosis*, child affected by parental relationship distress" (Bernet, 2020, p. 364) [emphasis added]. There are several problems with Bernet's assertion that *DSM-5* includes PA as an example of this new "diagnosis."

First, while *DSM-5* includes a new section titled "Child Affected by Parental Relationship Distress" (p. 716), it is in the chapter titled "Other Conditions That May Be a Focus of Clinical Attention." The introduction to this chapter pointedly states "the conditions and problems listed in this chapter are not mental disorders" (*DSM-5*, p. 715). If they are not mental disorders, they are not diagnoses. That is why they were put in a separate section. "Parental Relationship Distress" is not a novel diagnosis – it is not a diagnosis at all.

Second, Bernet's claim is based on *DSM-5*'s inclusion of "disparagement" as an example of parental relationship distress. However, while disparagement may be a core concept in theories of alienation (Rowen & Emery, 2014), disparagement alone is not sufficient to satisfy most scholars' definition of alienation since multiple behaviors are purportedly involved in alienation. Also, in *DSM-5*, disparagement is only one behavior indicative of parental relationship distress but multiple behaviors are needed to make a diagnosis. *DSM-5* (p. 715) includes other examples of bad parenting, some of which have been proposed to be associated with alienation. They are in the section titled "Parent Child Relational Problem" in the same Other Conditions chapter. These examples are also descriptions, not diagnoses. Describing examples of bad parenting is a far cry from including a "diagnosis" of "PA" or even recognizing the concept, as Bernet claims.

Bernet's *Response* (Bernet, 2020, p. 364) states the *DSM-5* Task Force "never said that they doubted the reality of PA." He says that individuals with whom he communicated recognized relational problems as "mental conditions" though not as "mental disorders." However, there are no "mental conditions" in *DSM-5*. There are only two sections that reference "conditions:" the "Other Conditions" section (*DSM-5*, p. 715) discussed above, and a section titled "Conditions for Further Study" (hereafter "Conditions to Study") (*DSM-5*, p. 783). As we said above, the Other Conditions section expressly states that the relationship problems it describes are *not* mental disorders, and that section does not recognize PA. Nevertheless, Bernet (Bernet, 2020, p. 365) makes the incorrect statement that "relational problems are considered diagnoses." He bolsters this claim by citing other relational problems included in the Chapter (such as various kinds of child abuse and neglect and domestic violence), asserting "all of those conditions are considered diagnoses." This appears to us to be nothing more than proof by repeated inaccurate assertion.

The second Conditions for Study section does not contain anything related to PA. The conditions it includes are all based on symptoms within an individual. Individual "location" defines mental disorder in *DSM-5* (p. 20), as Bernet (Bernet, 2010, p. 495) acknowledges. But, as Bernet

(2010) also insists, PA is a relational problem not an individual one. He rightly observes there are other mental disorders/diagnoses in *DSM-5* that begin with relational problems and progress to individual ones (e.g., Reactive Attachment Disorder). He argues that since PA is similar, it *could* have been included in *DSM-5*. Be this as it may, it has no bearing on whether *DSM-5* *did* recognize PA as an individual mental disorder. It is clear that the *DSM-5* Task Force did not doubt the reality of some destructive parental behaviors that have been proposed to indicate alienation, but they *did* doubt and rejected “alienation” as a mental disorder or a diagnosis.

Bernet (Bernet, 2013, p. 488) acknowledges the term “disorder” was created for advocacy purposes, “to conform to the vocabulary in *DSM*.” However, the difference between descriptive examples and diagnostic categories is substantive. Like the difference between descriptive language and labeling discussed above, it is not a matter of mere “vocabulary” or “semantics.” Diagnosing, like labeling, is an epistemological act. It denotes a factual reality. Changing the nomenclature did not persuade the *DSM-5* Task Force that “alienation” had achieved sufficient empirical or clinical evidence to be considered a factual entity, which is what the *DSM* mental disorders are presumed to be (*DSM-5*, p. 21). Bernet’s ongoing insistence that *DSM-5* accepted PA, when what they accepted was descriptions of some of the behaviors that proponents argue are associated with alienation, blurs the differences between describing and diagnosing, which makes analysis of the unresolved issues related to developing a new PA diagnosis impossible to address.

3. Bernet Criticizes Us for Failing to Address His Research on the Parental Acceptance-Rejection Questionnaire (PARQ)

Bernet (Bernet, 2020, p. 365) accuses us of using “hyperbole” and of being “misleading” when we said, “no one has found ‘scientifically validated criteria’ to help identify PA”. He bases this accusation on the fact that we did not mention his research on the Parental Acceptance-Rejection Questionnaire (PARQ) (Khaleque & Rohner, 2002), which he claims contributes to the scientific validity of PA. We did not address the PARQ in our *Critique* because Bernet’s article in this *Special Issue* did not discuss the PARQ, and it was not relevant to our *Critique*’s focus on rhetoric. However, his arguments compel us to do so here.

Bernet (Bernet, 2020, p. 364–365) says Milchman should not have overlooked his research (Bernet, Gregory, Reay, & Rohner, 2018) on the PARQ in her *Reply* (this issue) because she has previously written about it. In our view, this criticism is unjustified, since the previous article by Milchman (2019) *rejected* his claim that the PARQ sheds light on alienation. It explained that the PARQ does not even measure children’s rejection of parents; it measures only children’s *perception of parents’ rejection of them*. In fact, *there is no item on the PARQ that measures any of the behaviors that Gardner proposed or Bernet and his colleagues (Lorandos, Bernet, & Sauber, 2013) use to define the concept of alienation*. It is misleading for Bernet to write that Milchman’s awareness of his research on the PARQ means that we were disingenuous by not discussing it in our *Critique*. Instead of responding to the issues she raised, he simply repeats his claim that we ignored the relevant scientific research. He is relying on proof by assertion, a rhetorical strategy we have previously identified that makes constructive dialog impossible.

4. Bernet Accuses Us of Misrepresenting His Position Regarding Reliability in Assessing Alienation

We criticized Bernet for implying that Saini et al. (2012, 2016) agree with him regarding the accuracy of identifying alienated children and differentiating them from abused and other badly parented children. Saini et al. (2016, p. 423) had written that there is “remarkable agreement about parents’ “alienating strategies” and that children’s alienation symptoms “can also be *reliably* identified” [emphasis added] (p. 423). Bernet (Bernet, 2020, pp. 362–367) disputes our claim that he is wrong when he implies these authors agree with him about accuracy in identifying alienated

children. He says he and these authors are using “reliability” in the same way, to refer to “consistency” and “stability.”

These concepts are scientific terms that indicate whether scores on test items agree with each other, whether test-takers who are similar to each other score similarly on the test, or whether their scores remain stable over time (Bernet, 2020, p. 365). However, the consistency and stability of a test does not assure the test’s *accuracy*. That is, regardless of whether test scores are consistent in these various ways and remain so over time, the test may not measure what it is intended to measure.

Saini and Drozd (2019) have clarified the difference between scientific reliability and real-world accuracy with an analogy based on a research study by Goertzel (1994), who designed a measure to assess people’s beliefs that they were abducted by aliens. The measure achieved good “scientific reliability.” The respondents who believed they had been abducted overwhelmingly agreed on their abduction experiences. They told very similar stories, so their memories and beliefs were found to be consistent, that is, scientifically “reliable.” Nevertheless, it is obvious their consistency did *not* constitute proof of accuracy. Measuring their beliefs about their alien abductions did not verify that they actually *were* abducted by aliens.

However, we are concerned that Saini et al.’s (2016) language about alienation leaves room for confusion on the part of readers who are not well-versed in scientific terminology. The statements that there is “remarkable agreement” about parents’ alienation “strategies” and children’s alienation “symptoms or behaviors” (Saini et al., 2016, p. 423) could suggest to the average reader that “reliable” means “dependable,” as it does in common usage (*Webster’s new world dictionary of synonyms*’s, 1984). Despite this possible source of confusion, other statements in Saini et al.’s (2016) chapter make it clear they are not asserting that alienated children can be accurately identified.

In our *Critique*, we quoted Saini et al. (2016) at length to counter the misimpression Bernet was creating that they agreed on the accuracy of identifying alienated children. Based on their comprehensive analysis of 58 empirical studies, Saini et al. (2016) concluded that there were *virtually no studies that supported claims of accurate identification or scientific validity*. They also stated:

There have been growing efforts to develop assessment tools for parental alienation, but further research is needed to produce tools with adequate psychometric properties (Saini et al., 2016, p. 423).

Saini et al.’s (2016, p. 423) statement about the “remarkable agreement” on the behavior and strategies of alienating parents and the symptoms and behaviors of alienated children must be understood *technically* in the context of the difference between scientific reliability and scientific validity. This is the appropriate interpretive context because the chapter in which the “remarkable agreement” quote appears is a technical scientific literature review. In that context, “reliability” or “agreement” is a statement about *the consistency in people’s beliefs, not about the accuracy of their beliefs*. Bernet should have explicitly acknowledged that he and Saini et al. (2016) do not agree that alienation can be “reliably,” that is *accurately* identified, because Saini et al.’s (2016) denial of scientific validity is made in the two paragraphs that are directly below the paragraph in which he and his colleagues made their statement about “remarkable agreement.”

Bernet’s selective quotation from Saini and his colleagues (2016) misleads readers into believing they are agreeing with his own position – which they are not. His selective quotation is particularly misleading because Saini et al. (2016) emphasize that the lack of scientific validity for the concept of alienation means it is necessary to exercise “considerations and cautions when applying the empirical evidence on alienation to cases involved in child custody disputes” (p. 423). Further, they advise clinicians to “be wary of the numerous knowledge claims in this field” because “strongly supported empirical findings are relatively few” (p. 423). As we discuss below, Bernet used the same rhetorical strategy of selective quotation with Saini and his colleagues that he used with us when he incorrectly accused us of not acknowledging his scholarship on multiple causes of parent rejection.

Bernet should have informed his readers, many of whom are not researchers, that the scientific reliability about which those authors *do* agree does not mean scientific *validity or accuracy*, about which they do *not* agree. Judges rely on experts to be scrupulous in presenting and interpreting scientific evidence because they do not have the technical knowledge to evaluate technical claims. In the absence of clearly stated acknowledgment that scientific “reliability” is different from and does not imply accuracy, and that it is largely irrelevant to child custody disputes, claims of “agreement” are misleading. The legal system ultimately does not care about whether people agree on how to identify alienation, about whether they are consistent with each other or are consistent over time; it cares about whether their opinions or judgments are accurate (i.e., scientifically valid). Bernet’s reliance on the vernacular rather than the scientific meaning of “agreement” once again uses language to blur substantive differences between terms, which makes useful dialog impossible.

5. Bernet Objects to Our Assertion that He Misrepresented Endorsement by Authorities

a. American Bar Association (ABA). Bernet (Bernet, 2020, p. 365–366) says that his claim of endorsement by the ABA is based on the fact that Clawar and Rivlin (2013) told him “their books were ‘commissioned’ by the Section on Family Law of the American Bar Association.” He goes on to say, “that suggests some level of endorsement” (Bernet, 2020, p. 366). Bernet should have explained what “commissioned” means rather than claiming institutional “endorsement.” They are not the same thing, especially given that different sections of a large organization like the ABA have their own values, beliefs and biases. In fact, it is our understanding that obtaining any ABA organizational statement of “endorsement” or opposition to any particular proposition is an extraordinarily difficult process; clearly this was not done for PAS/PAD/PA.

b. American Psychological Association (APA). Bernet (Bernet, 2020, p. 366) now says he does not claim that APA accepts PA and the organization has made it clear that “it has no official policy regarding PAS.” He then quotes (Bernet, 2020, p. 366) a paragraph that he “recently wrote” (no citation) which has a disclaimer at the end that also states, “As an organization, APA apparently has ‘no official position’ regarding PAS, but it has not made a statement regarding PA” (Bernet, 2020) Nevertheless, numerous APA publications object to the misuse of the “alienation” label to deny abuse in families (APA, 1998; APA, 2008; APA Task Force, 1996).

It is not clear what Bernet’s (2020) revised position means. It could be a more limited statement that APA has not *rejected* PA, which is accurate, but potentially misleading. As an organization, APA takes no official position on many diagnoses (Meier, 2005). The absence of rejection isn’t acceptance. His revision, however it is to be interpreted, is also inconsistent with his claims of official endorsement at the International Association of Law and Mental Health Conference in 2017 (Bernet, 2017) and to the Connecticut Legislature as recently as 2019 (Bernet, 2019). We would welcome information about the reason for his changed position and its effect on his future advocacy. We believe that such an explanation could contribute to a more productive dialog about the state of the field, whereas simply revising inaccurate claims when challenged makes productive intellectual exchange difficult if not impossible.

c. American Academy of Child and Adolescent Psychiatry (AACAP), Association of Family and Conciliation Courts (AFCC), and American Academy of Pediatrics (AAP). Bernet’s Response (2020, p. 366) states that he cited support by AACAP, AFCC, and AAP in his original article to assert the general acceptance of alienation pursuant to a Daubert or Frye (legal admissibility) standard. If this were his purpose, he should have explained that general acceptance does not necessarily mean scientific validity. Without such an explanation, some readers might have been left with the impression that these organizations had recognized the scientific validity of PA, which they did not. This appears to be another example of shifting a position when challenged, undermining constructive dialog.

6. Bernet Criticizes Us for Citing Anecdotal Evidence

Bernet (Bernet, 2020, p. 366) misses our point when we commented on his use of anecdotal evidence. We critiqued his reliance on anecdotes because he uses them to attempt to *prove* the theory of alienation; anecdotes cannot prove a theory scientifically. It is well known in the philosophy of science that “it is easy to obtain confirmations, or verifications, for nearly every theory – if we look for confirmations,” but “every genuine *test* of a theory is an attempt to falsify it,” that is to disprove it (Popper, 1963). Bernet does not discuss any effort to find anecdotes that might falsify the alienation anecdotes cited as proofs. That is, he did not report any attempt to seek information disconfirming his theory. In contrast, the two anecdotes we cited were included to *raise doubts* about theoretical principles that seem to inform the concept of “alienation.” That is, we were citing examples in the service of falsification of a theory, which, as explained above, is a very different proposition than citing uncorroborated anecdotes to prove a theory.

7. Bernet Selectively Quotes Us

Bernet (Bernet, 2020, p. 363) says that we failed to credit his recognition that there are multiple possible causes for parent rejection. In our *Critique* (p. 341), we said, “[S]ome alienation theorists still appear to propound a single factor explanation of children’s rejection of a parent because they do not give weight to causes other than alienation *in their approach to practice*” [emphasis added] and cited his co-authored handbook on parental alienation (Lorandos et al., 2013). Bernet corrects us by citing Chapter Two in that book, which does discuss multiple causes of parent rejection. We acknowledge that in citing the whole book, we were imprecise and created the misimpression that Bernet and the other authors fail to recognize multiple causes.

However, Bernet quotes our statement out of context; we maintain the context (*Critique*, p. 341) makes it clear that we were not asserting that he or other alienation scholars fail to acknowledge multiple causes *in theory*. We were asserting that this theoretical recognition is not consistently carried over into practice. This limitation in our criticism is clear in our article because in the paragraph directly above Bernet’s partial quote, we cited 17 articles by 30 different alienation scholars that recognized multiple causes. We also said that Bernet agreed with them (*Critique*, p. 341), and we did not include him in our references to single-factor proponents (*Critique*, p. 342). The full context shows we were seeking common ground and included Bernet in that effort. By quoting us selectively and ignoring the full context for our statement which made its meaning clear, Bernet may distract readers from recognizing our substantive point about the overidentification of alienation in practice, a point that Warshak (2020) has also forcefully recognized.

IV. REPLY TO ROBB

A. JOAN MEIER & ROBERT GEFFNER

Robb’s *Response* to our *Critique* includes some legitimate points. However, he seems to impute malice and “hypocrisy” (Robb, 2020, p. 373) where only oversights occurred, and he fails to acknowledge the accuracy of at least one of our critiques.

First, our *mea culpas*. Robb’s original article was correct in pointing out an error in Meier and Dickson’s (2017) article: that is, the latter did incorrectly cite the Leadership Council’s 58,000 estimate of cases of “unsupervised access” in support of a statement about custody reversals (Robb, 2020, p. 314). Meier regrets that mistake. In addition, Robb is right that our *Critique* in this *Special Issue* mistakenly conflated his reference to “lost custody” being undefined (in his discussion of a survey the Meier and Dickson (2017) article cited in passing), with his critique of the Meier and Dickson (2017) pilot study itself. This too was an oversight, perhaps an understandable one, given the number of pages he devoted in his critique of the pilot study to discussing one passing

background citation which has nothing to do with the study’s findings, itself a questionable rhetorical strategy in our view.

However, Robb’s criticism of us for not addressing his critique of that survey is misplaced: the survey was merely one of numerous citations in a background section, cited to support an introductory statement that “service providers and advocacy organizations... report what appears to be a trend” of mothers losing custody after reporting abuse in family courts. It is reported in a one-page internet summary of findings and in a separate poster presentation (Stahly et al., 2014; Stuebner, Krajewski, & Stahly, 2014). That the survey is non-scientific and could potentially be unreliable is neither contested nor relevant to the point for which it was cited: that *reports by advocates and providers* of this troubling outcome are widespread. Our decision not to respond to Robb’s critique of the survey was neither a “tactic” nor an example of “misdirection” or “hypocrisy” (Robb, 2020, pp. 373–374). On the contrary, Robb’s making such an issue of the reference to this survey appears to be another rhetorical strategy, an attempt to convince his readers that our study is illegitimate by focusing on a minor citation rather than the study itself.

In addition, Robb asserts that our description of his attack on the study’s “win” rates, which we characterized as a form of “*reductio ad absurdum*,” is wrong. It is true that the pilot study’s operationalization of “winning” a family court case, as reflecting whenever a party obtained all or part of the relief requested or defeated the opposing party’s request, could mean that some relatively minor custody/visitation changes were coded as “wins” (Robb, 2020, pp. 373–374). However, the study’s authors believe litigants’ experiences of winning and losing are almost never minor to them, especially in custody and abuse cases. Space limits prevent us from deeper engagement with Robb’s misplaced focus on the “win” factor, but our point is and was that the attack on the “win” element focuses on a single minor element of the study in order to discredit the entire study, rather than engaging with the pilot study’s core, troubling findings. These included, among other things, that:

“[n]ot only did fathers alleging alienation manage to negate abuse reports from mothers and children in the majority (72%) of cases, they did so in every case but two (36 of 38) when mothers alleged child sexual abuse; when courts believed mothers were alienating, they switched custody to the father 69% of the time;” and “even when the alienation claim was rejected or not decided, they transferred custody of the children to an allegedly abusive father 25%–50% of the time,” with all mothers losing custody approximately half the time (Meier & Dickson, 2017, p. 331).

We believe that Robb made an incorrect statement about the study. In footnote 12 of his article he stated that Meier and Dickson’s (2017) definition of winning “essentially encompassed fathers getting even an iota of outcome that the mothers opposed, *such that mothers ‘lost custody’* even if they maintained a counterfactual request...” [emphasis added] (Robb, *Methodological Challenges*, 2020, p. 318). This is a critical mis-statement, which, if true, would powerfully discredit the study. In fact, as our *Critique* correctly pointed out (*Critique*, p. 351–352), Robb himself erroneously conflated the study’s definition of wins (which were not custody-based), with its very precise definition of custody reversals as a change from primary custody in one parent to primary custody in the other.

Further, Robb incorrectly claims that Meier and Dickson’s (2017) survey treated minor visitation changes as equivalent to losses of custody. It is possible Robb was conflating Meier and Dickson’s (2017) pilot study with the survey (cited in passing) when he stated:

Lumping together cases where issues are so detrimental to a child as to lead to the termination of parental rights in the same category as cases where a parent gained an hour of parenting time over the objections of the other parent may be comparing apples to oranges... (Robb, 2020, p. 313–314).

No doubt. However, neither the survey nor the study did this. Is he conflating Meier and Dickson’s (2017) definition of “wins” (distinct from custody losses) with the cited survey’s failure to define “lost custody” (in its one-page internet summary of findings)? It is not clear.

Criticism of research is important for researchers and readers. However, distortions and conflation that appear to discredit research, but are incorrect, do not contribute to constructive dialog. Hopefully future dialogs between professionals with different viewpoints about alienation can incorporate more careful analysis and better-grounded criticisms. We acknowledge that professionals on the child abuse and domestic violence “side” should also be held to this standard.

ENDNOTE

1. Chris Silleza, CNN, 10/1/19, 2:33 PM described a cycle in which baseless claims are constantly repeated and debunking is met with an ongoing lack of apology or regret until it is too late to correct the claim. The term “Cycle of Rhetoric” is our adaptation of Silleza’s commentary.

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