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Eileen A. Kohutis

To cite this article: Eileen A. Kohutis (2019): Commentary on: Joint versus sole physical custody: Outcomes for children independent of family income or parental conflict, Journal of Child Custody, DOI: 10.1080/15379418.2018.1539937

To link to this article: https://doi.org/10.1080/15379418.2018.1539937

Published online: 21 Jan 2019.
Commentary on: Joint versus sole physical custody: Outcomes for children independent of family income or parental conflict

Eileen A. Kohutis
Private Practice, Livingston, New Jersey, USA

ABSTRACT
The debate about joint physical custody rages in the scientific community as scholars and practitioners gather information about what is best for children when parents divorce. The field of child custody is vast and complex, and summaries are welcomed reviews. This commentary is a summary of some of the obstacles that may be encountered.

ARTICLE HISTORY
Received 18 July 2018
Accepted 23 September 2018

KEYWORDS
Joint custody; physical custody; shared parenting; sole custody

How custody of children should be allocated in the case of parental divorce is a matter of intense discussion today in our literature and in our courtrooms with efforts under way in many jurisdictions to establish joint physical custody (JPC) as the standard in law. As researchers try to study the relative merits of JPC and sole physical custody (SPC) in an objective and unbiased manner, advocates of both comb the scientific literature for findings that support their positions. Custody is such a vast and complex field and encompasses so many diverse factors that consolidations and reviews of the data are welcome and can be useful indeed. One consolidation is the following article by Linda Nielsen (2018), “Joint versus sole physical custody: Outcomes for children independent of family income or parental conflict.” Unfortunately, what this piece demonstrates most clearly is not the relative merits of custody arrangements, but the potential pitfalls of such reviews, for both authors and users.

As Nielsen (2018) points out, “authors who summarize the research must take great care to report the findings accurately and to include the results of all studies, not just those that support their particular point of view” (p. 38). If not, summaries may end up being misunderstood or misused by readers; this is a disservice to the researchers whose work the summary covers, and it undermines the usefulness of the summary itself. I would add that knowledgeable writers sometimes forget that readers do not
necessarily share their familiarity with a subject and may not have at their disposal information and assumptions that an expert takes for granted. Furthermore, readers who do not come from research fields—lawyers and legislators, for example—may interpret data in ways that more sophisticated statisticians would not. In keeping with these caveats, I will concentrate primarily on the aspects of Nielsen’s presentation that mitigate its usefulness for its likely readership, specifically some aspects of the research design, and the most unsettling aspects of her summary. This is not intended as an exhaustive list of the pitfalls of such works, or of this particular article’s shortcomings, but as an illustration of why it behooves us to examine such meta-studies carefully when they come our way.

In her first paragraph, Nielsen establishes JPC as the main focus of her summary and states that it is becoming more common. However, she never defines JPC. Researchers define JPC differently; and these definitions range from a truly equal 50% of time (Kelly, 2007) spent with the nonresident parent (Kelly, 2007), to a much less clearly “joint” custody of 33% of time spent. Nielsen’s readers never know exactly what she means when she talks about “joint” physical custody. One of the goals of research is to design studies that can be replicated. Nielsen states that three databases were employed in her summary and the key search words that were used; however, she does not tell us what the criterion was for a study’s inclusion and exclusion, and what years the studies in her summary covered.

One purpose of this kind of summary is to demonstrate the similarities and differences between various researchers’ results. Yet, Nielsen seldom reports the specifics that would allow her readers to assess the results; they have to take her at her word, which defeats the usefulness of this kind of summary. For instance, in discussing the meta-analyses of Baude, Pearson, and Drapeau (2016) and Bauserman (2002), she states that effect size was small, but she does not say what it was. Why not? What is small to one reader may not be to another. If a statistic is worth mentioning, it should be made explicit, so that readers can make their own assessments of the author’s conclusions. We do not have that opportunity here. Similarly, Nielsen goes on to say of the Bauserman (2002) study that the “benefits of JPC were much larger for children who lived 50% time [sic] with each parent than for JPC child who lived less than 50% time with parent.” But she does not tell us what “larger” means in the context of these “benefits” (p. 36). Are they large enough to be statistically significant? If so, tell us how much. If not, then what does “larger” matter? This kind of pervasive lack of clarity consistently undermines the points she is trying to make.

There is another example of this in the section entitled “Negative outcome for JPC children,” where Nielsen says that “Despite the more positive outcomes overall for JPC children, in 6 of the 60 studies JPC children had
worse outcomes than SPC children on one, but not all, measures [sic] of well-being” (p. 44). How much worse? A little? A lot? What is worse? Again, no statistics are reported. And, a reader who turns back to the table to try to figure out which measures she is referring to quickly discovers that the measures are not listed; she lists only generic categories, such as “academic and cognitive development,” and “depression, anxiety, overall life satisfaction, self-esteem.” Analyzing conflict among couples, Nielsen writes, “Compared to SPC couples, in 3 studies JPC couples had less conflict; in one study they had more, and in one study the conflict differences depended on the age of the children. In short, cooperation and low conflict are not likely to account for JPC’s children’s better outcomes” (p. 46). Here, she does not even identify which studies she is talking about; the reader must decipher which studies she means before it is even possible to search for the citations.

Obviously, it is crucial that interested readers of a summary are able to identify easily and accurately the studies discussed; researchers will want to delve deeper into concerns that are relevant to their own work, while legislators need to be able to assess whether a study is of sufficient quality to shape the law and its application. Yet, Nielsen makes it difficult to identify her data sources. She tells us that this current article is the third in a series of three; in each she has summarized 20 studies examining outcomes of children in shared and joint physical custody, and in this third one she has updated the summaries of the 40 previous studies published in the first two. However, she does not indicate in the text which 20 studies she is examining in this particular piece; it is not until the end that we learn, in a note at the head of the reference list that they are marked with an asterisk there. It would have been easy and helpful, and it would have saved this reader a lot of frustration if there had been a note to that effect early in the text as well.

Even more frustrating, many of the studies she discusses in this piece are not listed in the references at all. Nielsen tells us that this is for reasons of space and assures us that the references are available in her previous review articles. However, this is no help to a reader struggling with the massive four-page table into which Nielsen has distilled her analysis, and it goes against the time-honored and wise scholarly convention that references be provided in a article for any material directly discussed therein. Sometimes a name and date in the table can be found in the references, but without the full range of authors listed, it is impossible to tell whether this is in fact the study Nielsen has in mind. Furthermore, since there is no indication in the table whether or not a study is among the 20 included in the present article, each and every listing requires a hunt for asterisks in the references to figure that out. For instance, Nielsen lists Cashmore as having authored
three studies but there is no citation for any of these studies in the references; as a result, the reader has no indication to which articles Nielsen is referring. This is particularly problematic since Cashmore has numerous publications, some in which she is the sole author and in others where she has collaborators.

The converse problem exists too: although Fabricius & Suh (2017) is listed in the references, that article is not cited anywhere, either in the article or in the table. In short, a reader engaged in the fundamental task of tracking these studies down will find many bibliographic obstacles in the way. An example: she makes a general statement that “In 6 studies, there were no significant differences between the two groups on any measures” (p. 39). But the six studies are identified only by shorthand names, and only sometimes with dates, in the table’s leftmost column. Clearly, this has been done in the interest of saving space, and clearly space-saving is an important priority in such a huge table. But an expansion of the shorthand could have been offered as well, either in a subsidiary table, in the text, or in the references. Without that, it is impossible for the reader to know for certain which studies she is referring to in this paper without referring to another one—or possibly two—because how can we tell which of the two earlier studies will turn out to be the one containing the mystery reference?

As researchers, lawyers, and clinicians, we are accustomed to looking up the other writings of authors who interest us, and to availing ourselves of libraries when we need an article that we do not have at hand. Hunting up references when delving deeper into a subject is standard scholarly operating procedure and one of the pleasures of study. It is not standard operating procedure for an article to fail to provide the references relevant to its own immediate argument. I should add too that this practice occurs many pages into the article; if Nielsen’s other articles are the sources of all of the missing references, it would be good to be told that when the problem first arises.

As I have said, the heart of this article is a lengthy and extensive table. It, too, presents serious problems to a reader who wants to use the material as an entrée to further work, to deeper understanding, or even simply to confirm that he or she sees things the same way that Nielsen does. Nielsen combines categories of findings into columns, but provides no clue as to how these (sometimes perplexing) combinations interact. For example, one column is labeled “Depression, anxiety, overall satisfaction, self-esteem”; in some studies, the table tells us, the outcome of one group or another was “better.” But what does “better” refer to? Depression? Anxiety? Both? Is self-esteem included? What happens if overall satisfaction is better, but anxiety is not? What if the outcome is listed as “Better life satisfaction?” Does this mean depression and anxiety are not better? The reader has no
way to know which comments apply to which conditions. The table would have been more helpful, and less subject to misinterpretation, if that single column had been broken down into at least two: depression and anxiety, and satisfaction and self-esteem. In that context, the word “better” would have had a clearer meaning. Alternatively, a general category of emotional health with a description of each study’s findings would have given the reader more help in understanding the results. It will take inexperienced readers some study to figure out that the table is established with JPC as the point of reference. A clear statement to that effect would have been useful, as the structure of the table itself does not make clear that the two adjacent columns, JPC and SPC, indicate not only the relevant data, but also the direction of the comparison.

Another column is labeled “Peer behavior, substance use, hyperactivity.” What does "better" mean there? If there is better integration into a peer group that smokes weed at parties, does that count as better peer behavior, or worse substance abuse? As always, I am assuming that these categories have been collapsed in the interests of space and efficiency. But in this case the results undermine Nielsen’s own efforts. Nielsen cites an article by Fransson under the category “JPC equal or better outcomes than SPC 14 studies” and according to her the better outcomes indicate, “equal psychological, better stress, equal drinking, better smoking, better bullying” (p. 41). This example illustrates the neat compactness of the table that gives rise to a tangle of uncertainty and complication. Some categories really are better dealt with separately.

As to Nielsen’s representation of the data itself, I will comment here on one study only, which demonstrates at best some serious confusion as to citation, and at worst a serious divergence between Nielsen’s report of the study findings and the report of the researchers themselves. In the table summary of Fransson, Turunen, Hjern, Ostberg, and Bergstrom (2016), Nielsen reports that "depression, anxiety, overall life satisfaction, self-esteem" were “better” in children between the ages of 10 and 18 in JPC. She elaborates:

Having a parent with a graduate degree was more closely linked to children’s stress and anxiety than was the physical custody plan. The researchers speculated that highly educated, higher income parents might put more academic and social demands on their children, which, in turn, increase children’s stress and anxiety (p. 48).

The authors, however, say something quite different: first, that “Low parental education and parental worry/anxiety were associated with more psychological complaints for children” (p. 180; emphasis added); and second, that “the differences between joint physical custody and sole parental care was not explained by socioeconomic factors or by parental
ill-health” (p. 183; emphasis added). Although they do distinguish between “low level” and “above low level” incomes and among levels of education, Fransson et al. make no mention at all in this article of social “demands” or graduate “degrees.”

I can think of only two reasonable explanations for this discrepancy; one is that Nielsen had in mind an article other than the one cited. Perhaps the date was listed incorrectly and she meant the other Fransson article in the table, or perhaps there is another article by these authors that is not listed, or the article in question was by someone else altogether. The other, which I entertain reluctantly and, in this instance, consider less likely, is that Nielsen’s own views on the JPC/SPC controversy distorted her interpretation of the article she was discussing. Either way, however, she weakens her own effort: a reader familiar with Fransson’s work will note the inconsistency and conclude that the summary cannot be used with confidence. An unfamiliar reader may well be seriously misled. This is not a reassuring finding, and it is made even more irritating by the fact that the truncated reference list gives us no direct way of checking whether there were other studies by Fransson in Nielsen’s database (in addition to the two listed here, one of which has a different date) that might have been confused with this one. There is another Fransson study on the references. When I looked at that study, it is not about parental ill-health. Clearly, she was thinking of an article by another writer.

It is standard practice for scholarly publications to cite the limitations of their manuscript. Curiously, although Nielsen discusses the limitations of the studies she reviewed, Nielsen does not make any statement at the conclusion of her article about any limitations of her own summary. My primary point in this commentary is that there are seemingly nonsubstantive issues in this article that severely limit its usefulness to the reader, who turns to it as a guide to an important field. My secondary point is that inconsistencies and inaccuracies, even small ones, diminish a reader’s trust in an author. The imprecision of Nielsen’s summary—both the skimpy references and the overly collapsed data categories—make it difficult for her readers to follow her arguments, or feel secure in her conclusions. I have called attention to an instance where I believe that either a study has been cited incorrectly, or that there has been a misrepresentation of its findings. As I have said, my intention in this article is not to detail every possible problem that can be found in it, but to demonstrate the kinds of problems that readers should look for, both for their own protection and to encourage care and “user-friendliness” in authors. I am in no position to speculate on why such problems haunt this particular article. However, they do, and readers therefore need to consider it seriously flawed and lacking reliability.
References


