

**EXPENDITURES ON CHILDREN
AND VISITATION TIME
A Reply to Garfinkel, McLanahan, and Wallerstein**

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In their critique, Garfinkel, McLanahan, and Wallerstein raise concerns about the representativeness of the authors' sample, benchmark approach methodology, and historical review of guidelines, all of which lead them to discount the evidence presented opposing the cliff-model assumption of father expenditures on children, and to laud instead child support guidelines that give little monetary credit or adjustment for visitation. This article presents evidence that (a) this sample is at most little biased, and remains trustworthy for the main implications presented; (b) although only a beginning, the benchmark approach is highly useful and most of the concerns raised about it are ill-founded or implausible; and (c) the historical review suggesting that current guidelines assume zero visitation expenses is indeed accurate for the vast majority of states, according to the foremost authority. Thus, notwithstanding the critique, these findings have merit and importance and should be considered by policy makers. The authors also comment on the additional arguments against continuous and generous adjustments for visitation, finding them based on a weak foundation of evidence and reasoning.

Keywords: child support; father's financial support; parenting time; economic outcomes of divorce

At the invitation of the editor, Garfinkel, McLanahan, and Wallerstein (2004) have written a critique of our 2003 *Family Court Review* article "Non-Child Support Expenditures on Children by Nonresidential Divorced Fathers."¹ The journal has kindly allowed us the opportunity to respond to the critique.

SOME POINTS TO NOTE

Before we get to their specific complaints and criticisms, we wish to make four points about our report. First, the kind of evidence we were seeking, concerning the nature of financial outlays nonresident fathers (obligors) make directly on behalf of their children over and above any child support they pay to the residential parents (obligees), and how these outlays relate to the time they spend with their children, was seen as critically important by most authorities yet hitherto completely unknown. For example, we quote Robert Williams, the developer of the income shares approach used by most states and the president of Policy Studies Inc. (PSI) (the technical consultant to the majority of states' Child Support Committees), at length on this point, as saying that more research on this issue "would help states make more rigorous decisions concerning the appropriate design of shared parenting adjustments" (Fabricius & Braver, 2003, p. 324). When we notified other scholars in the child support field (such as Laura Morgan, Jane Venohr, and Ira Ellman) that we would be publishing data on this topic, most acknowledged how important yet unique it was to produce evidence of this sort.

Second, we recognized that the most desirable and defensible method of obtaining such evidence was to “conduct an exhaustive and expensive economic analysis similar to the . . . Consumer Expenditure Survey (CES) (on which all states’ child support guidelines ultimately rely in one form or another)” (Fabricius & Braver, 2003, p. 324), which tracks and categorizes the daily expenditures of a representative sample of thousands of families. Unfortunately, such studies are “multi-million dollar” projects, none existing examine even single parent families (let alone the expenditures of the obligors traceable to their child), none that can do so are planned or anticipated by federal agencies, and even the CES is suspected (even by staff of the Bureau of Labor Statistics, which conducts the CES) of being biased by systematic underreporting of income and overreporting of expenses in ways that distort child support guideline calculations (Branch, 1994; Ellman, in press; Venohr & Griffith, 2003b).² In the absence of anyone being able to obtain more definitive data in the near future, and in view of the importance of the issue, we decided to collect comparatively easier to obtain evidence as the “first empirical research insight . . . into what obligors actually and voluntarily bear in terms of child rearing expenses over and above mandated child support payments at different levels of time they have their children with them” (Fabricius & Braver, 2003, p. 324). Clearly, we regarded this evidence only as “a beginning” but of merit in and of itself.

Third, in view of its limitations, we were (we think) appropriately cautious in the conclusions we drew. For example, because ours was a college sample, we noted that “it is possible that young adult children of divorce at this state university are a select sample of those whose parents divorce” (Fabricius & Braver, 2003, p. 325). Again, we listed virtually a full paragraph of caveats, including questioning whether our findings would “be replicated with more precise measurement of noncustodial dollar outlays than the benchmark approach we used; for example, with actual expenditure diaries,” and wondering whether the results would “be upheld when applied to a more general sample than children of divorce who later made it into a large state university?” (p. 332). It is certainly possible that such a sample is unique or biased, and quite plausibly just because the fathers idiosyncratically somehow chose to spend money on their children when they did not have to” **WHERE SHOULD OPENING QUOTE BE PLACED?** (p. 332).

Finally, we believe that we were circumspect and modest in drawing policy conclusions. We did not recommend that child support guidelines be generally lowered. We did not argue a general case in favor of continuous visitation adjustments. In a report that had nearly 6,000 words and seven figures, here is the sum total of what we said in the text about policy:

These findings are also relevant to policy makers charged with quadrennially revising state child support guidelines, the great majority of which do not include reductions for expenditures that accompany the amount of time the child spends in the noncustodial parent’s home. The current data suggest that at lower levels of contact than the cliff cutoff, substantial percentages of noncustodial fathers made nontrivial direct outlays for their children. . . . Granting credit for these outlays seems justified because fathers are assuming them already without any help or explicit responsibility to do so. (Fabricius & Braver, 2003, pp. 333-334)

Actually, our strongest policy recommendation did not concern child support systems at all; we recommended “granting fathers joint legal custody and more time with their children when they seek it” because they appeared to be “already making sacrifices to assume custodial financial responsibilities at their homes” (p. 334). Notwithstanding the Garfinkel et al. (2004) critique, we stand by our conclusions, caveats, and recommendations.

THE CRUX OF THE GARFINKEL ET AL. (2004) CRITIQUE³

Garfinkel et al. (2004) argue that there are problems with our sample, our methods, and our review of the background history that negate the conclusions that can be drawn from the evidence we presented. We address each of these in turn, then comment on some other relevant findings and assertions of Garfinkel et al.

THE REPRESENTATIVENESS OF OUR SAMPLE

Regarding our sample, they argue that any data from our sample have little evidentiary value because our sample is not typical of most children from divorced families. Young adults from divorced families who made it to college are the “success stories,” Garfinkel et al. (2004) argue, and one reason for their success is that their fathers had been especially committed to them all along: “They paid more child support, had more visitation, and spent more on their children than divorced fathers whose children did not go to college” (p. **PROOFREADER: PAGE?**), they assert (without documentation or citation). According to Garfinkel et al., these fathers will have made substantial financial contributions over and above child support when their children were younger, but fathers whose children did not go to college will not have done so to the same extent.

The representativeness of our sample—or any other researcher’s sample, including Wallerstein’s California Children of Divorce Study (Wallerstein & Blakeslee, 1989), from which Garfinkel et al. (2004) frequently draw conclusions but which has itself drawn criticism for being unrepresentative (e.g., Amato, 2003; Kelly & Emery, 2003; Walker, 2003)—is an important and relevant issue, and we welcome the opportunity to address it in more detail than was appropriate in our article.

Although we briefly reported that “the non-custodial fathers in this study generally did not have extraordinary amounts of visitation; indeed, 18% of students reported they did not see their fathers at all, and 45% reported that they saw them less than 15% of the time” (Fabricius & Braver, 2003, p. 332), Garfinkel et al.’s (2004) point is that whatever our rate, it is still more than the rate of visitation of other fathers. Garfinkel et al. suggest we “owe the readers of *Family Court Review* documentation” (p. **PROOFREADER: PAGE?**) in support of our statement; “however, since the findings in other respects (e.g., proportion of various visitation schedules) match those of more representative national samples, it is not unreasonable to assume the present estimates of substantial child expenditures are unbiased” (Fabricius & Braver, 2003, p. 332).

The nationally representative sample we use here is the one cited extensively in Garfinkel et al. (2004), the 1987-1988 National Survey of Family and Households (NSFH) as analyzed by Seltzer (1991).⁴ Because visitation may decline with time since divorce (a longitudinal effect) and increase by year of the divorce (fathers divorced in recent years appear to visit more, an historical or “cohort” effect), it is important to compare apples to apples by restricting attention to fathers divorced during the same time frame who are the same length of time postdivorce. Seltzer examined mothers’ reports of visitation by divorced, nonresident fathers “in the past year”; thus, visitation within the first 2 years (the period about which we most often asked our participants) is represented by divorces that happened between 1985 and 1987. Our participants averaged 19 years old when we queried them in 2000. Thus, for this comparison, we selected only students who were aged 4 to 9 years at the time of the divorce, which would have meant the divorces occurred between about 1985 and 1990, and who reported they lived with their mothers 55% or more of the time during the first 2 years. First,

we compared reports of no visitation across studies, defined in the NSFH by the category “not at all” in the past year, and by us as 0% of the time. In the NSFH, Seltzer found 4.3%, whereas we found 18.8%. Second, we compared reports of minimal visitation, defined in the NSFH by combining the categories “not at all” and “once a year,” and in our sample as less than 5% of the time (equivalent to 2.6 weeks per year). Seltzer found 12.8%; we found 25.6%. Third, we compared reports of frequent visitation, defined in the NSFH by combining categories “once a week” and “several times a week,” and for our participants as at least 15% of the time (equivalent to 1.05 days per week). Seltzer found 42.7%; we found 51.9%.

Note that the difference in rates are in opposite directions and cancel each other out. Although it is true that we found about 10% more of our sample reporting the higher level of visits (once a week or more), we also found about 10% more participants reporting no visitation whatever or little of it. Similar findings were obtained for every way we examined the representativeness of our sample against the NSFH.⁵ These analyses suggest that, overall and on average, our reported rates of visitation are quite comparable to those found in national samples.

Garfinkel et al. (2004) also make the assertion that “there can be no question that college undergraduates are a select sample of the children of divorce. . . . The direction of bias is clear. The only question is how big is the bias? . . . The literature is quite clear: divorce reduces college attendance (McLanahan & Sandefur, 1994)” (p. **PROOFREADER: PAGE?**). Again, we were open about recognizing and admitting that our college student sample was likely to be a somewhat select sample. But is the bias big enough to completely undermine our findings? Garfinkel et al. did not take the step of quantifying the exact extent of “how big the bias” is for the readership, but we detail what they found here: McLanahan and Sandefur examined five nationally representative studies and found across all of these that about 50% of children of two-parent families go to college, whereas about 40% of children of “one-parent families” do so.⁶ Even this rather slight 10% discrepancy is an overestimate of the extent of bias between married and divorced families, because the one-parent families they counted included never-married parents (as well as divorced parents), and McLanahan and Sandefur later show (in Figure 5) that children of never-married parents are about 6% less likely to graduate high school than children of divorced parents. Overall, one could fairly say that, of children from divorced families, those who made it to college appear a very slightly select group.

So, admittedly, our sample is likely biased, but only slightly so, and indeed we appear to have slightly more involved fathers than a representative sample might have. But we also have more completely uninvolved ones than a representative sample might have. We appear to underrepresent only the fathers in the middle.

Most important, note that any impact of our findings does not really hinge on whether we slightly over- or underrepresent any group of fathers because we examine the provision of the benchmark items in relation to time with father. The primary force of our findings concerns not the overall level of fathers’ provision of benchmarks, but instead two aspects of this relationship of provision of benchmarks with time with father: its shape (cliff model vs. relatively smooth increasing function) and the extent of the outlays for the least involved fathers. Even if there had been fewer of the latter in our sample than in a more representative sample, our findings would be dismissible only if the least involved fathers among those we sampled provide more of the benchmark items than would the least involved fathers in a fully representative sample. Although readers may arrive at their own conclusions, we believe this is most improbable.

Nonetheless, it is important that our findings be replicated as soon as possible by other researchers. It may be that such future research may not find the same linear, non-cliff-like relation between expenditures and time with fathers in noncollege samples, but we think it is unlikely to be because we did not have a fairly representative range of visitation levels in our sample.

OUR METHOD: QUERYING COLLEGE STUDENT CHILDREN OF DIVORCE ABOUT BENCHMARK ITEMS

Garfinkel et al. (2004) express several concerns about our method of assessing non-child support expenditures by fathers, presenting the prevalence of benchmark outlays by the fathers as retrospectively reported by their now young adult offspring. Garfinkel et al. argue the following: (a) that we cannot tell how “appreciable” those expenses were because we cannot assign a dollar value to them, nor can we compare them to outlays made by the mother; (b) that we cannot tell if some of those expenditures were actually made by mothers, such as buying clothes for the children to take to dad’s house, or were actually deducted from child support; (c) that we cannot tell whether mothers agreed that the adolescent should have a car when students reported that they had a car “that your dad mostly paid for”; and (d) that reports from college students cannot be trusted because they are “colored by wishes, by the fantasies of childhood, by nostalgia, by the wish to deny hurt, by anger or hurt, or by the wish to please the professor who wants answers in their first college class” (p. **PROOFREADER: PAGE?**).

With respect to (a), it is true we do not have dollar values for these expenditures, nor do we compare them to mothers’ outlays. The point of using benchmark items was not to determine the dollar amount of non-child support expenditures but to determine the nature of the association of such expenditures with the amount of visitation, after taking into account fathers’ standard of living and legal custody arrangement. The benchmark items we used were chosen because they seemed like the kinds of things children could be expected to readily and accurately remember about their fathers’ houses. Readers can judge for themselves whether the mere presence of benchmark items, rather than dollar value, has important meaning. We think it does.

Any comparison to mothers’ analogous outlays is meaningless for the point we are interested in. Suppose a “better” study would show that mothers’ households had even more children’s clothes than fathers’, a fact we concede is exceedingly likely. This would not change the fact that the cliff-model child support guideline assumes that below a high threshold of shared parenting, fathers expend virtually nothing on children, and this assumption appears quite completely incorrect.

Garfinkel et al. (2004) make the argument in (b) that some benchmarks, such as clothing, may have actually been paid for by mothers and simply brought to fathers’ houses. While we think it highly probable that much clothing is brought from the mother’s house, for a respondent to properly give an affirmative answer to our question, substantial amounts of such mother-purchased clothing would have needed to have been permanently left at the father’s,⁷ which we regard as less plausible. Their argument is even more implausible for bicycles, and does not apply at all to providing a bedroom for the child, which together constituted our strongest findings and evidence against the cliff model, in terms of percentage of fathers making the outlay at low levels of visitation. If so many fathers paid by themselves for these major benchmark items, we think it is reasonable to assume that fathers did so as well for the

other reported expenditures that children could have otherwise simply carried from house to house.

Regarding the possibility that fathers may have deducted some of the expenditures that they did make from child support, this was simply not allowed under state child support guidelines, which generally automatically withhold child support from the father's paychecks. The contrary findings Garfinkel et al. (2004) cite from the California Children of Divorce Study (Wallerstein & Blakeslee, 1989) occurred long before state child support guidelines were instituted and child support strictly enforced and paychecks garnished. Additionally, it is hard to see why increasing amounts of visitation (and higher levels of fathers' standard of living) are related to increasing percentages of fathers who provide these benchmark items if they are deducting them from child support. Why wouldn't fathers at the lower levels of visitation also provide these items if all could, in essence, get out of paying for them by correspondingly deducting them from child support?

Regarding (c), whether the father's purchase of a car for the student was an expense that the mother deemed unnecessary, although this would be interesting to know, it does not have bearing on the central issue at all.⁸ Furthermore, about half of the students who reported that their father contributed to car expenses reported that the contribution was in the form of using his car or both his and the mother's car. Overall, among students who reported that they had some arrangement in which either they themselves or at least one parent provided a car, only 15.7% reported that it was a car that "dad mostly paid for." About the same number (17.4%) reported that it was a car that "mom mostly paid for." Similarly, only 21% reported that the father was the sole provider of car insurance.

Finally, in (d), Garfinkel et al. (2004) raise doubts about the reliability of college students' reports of the presence of such benchmark items in their father's homes, because those reports could be colored by wishes, fantasies, nostalgia, anger, and a wish to please the professor. They wonder why "there was no attempt by the investigators to check any of the memories of the youngsters by interviewing either parent or consulting records of any purchase" (p. **PROOFREADER: PAGE?**). We actually answered this question:

Inventorying the home was impractical, but there is evidence that the two parents commonly provide very different responses to questions about child support payments, visitation, and other aspects of post-divorce life, with the possibility that each one exaggerates in both a self-serving and "ex-spouse bashing" fashion. . . . A reasonable solution is to ask the children (presuming they are old enough to be trustworthy) because they have fewer obvious systematic biases in their responses (albeit they may have distortions in their memory). (Fabricius & Braver, 2003, p. 325)

But we have two more reasons to trust these college students' reports.⁹ The first involves the statistically reliable correlations between these reports and the amount of visitation and the father's standard of living. For example, it is hard to see why unreal wishes and fantasies about having a bike at dad's house would be related to reports of his standard of living. It seems implausible, for example, that the more college students fantasized they had a bike at their dad's house, the more they fantasized they lived with him, and the more money they fantasized he had.

Second, in a separate but analogous investigation we referenced (Pasley & Braver, 2003), we do have comparative reports by matched mothers, fathers, and young adult children of divorce about some relevant benchmarks, such as a bedroom for the child at father's house. Parents' reports did indeed appear exaggerated, in self-serving and ex-spouse bashing direc-

tions: Mothers report their own contributions are higher than fathers', whereas fathers say their contributions are greater than mothers'. Their matched college-age children are intermediate; they report that mothers' contributions are less than mothers say they are, but more than fathers say mothers' contributions are. They say fathers' contributions are less than fathers say they are but substantially more than mothers report fathers contribute. Because children's reports are intermediate, it is reasonable to conclude that they are less biased and therefore to be preferred to parents' reports.

Notwithstanding the above defense, as we said at the beginning of this response, we regard the benchmark approach we used as only a beginning step. We think our most serious error was to forget to include the ubiquitous phrase "more research is needed," preferably with more ideal methods. We strongly suspect, however, that our basic results will be replicated in these "better" studies (which are not anticipated to be conducted in the foreseeable future, anyway), simply because the cliff model is improbable on its face. It is simply naïve, if not demeaning, to assume that today's fathers whose children visit a "normal" amount (about 25% of the time¹⁰) spend nothing appreciable on them. Nonetheless, at this point in time, the only available evidence of the relationship between expenditures and time with fathers is ours, and ours is clearly inconsistent with the cliff model.

OUR HISTORICAL REVIEW

Garfinkel et al. (2004) argue that our review of the history of guidelines is misleading. They contend that we are incorrect in our assertion that few states' guidelines assume any visitation, nor do states credit its associated expense to the obligor. We had stated that Czapanskiy (1994) had made the assertion that "when guidelines were initially formulated, they were based on the assumption that the child will spend about 20% of his/her time with the obligor and a corresponding reduction in child support was already incorporated in guidelines for this standard visitation" (Fabricius & Braver, 2003, p. 323), but that Czapanskiy's assertion was "actually a mistake." In rebuttal, Garfinkel et al. quote a long passage from Garfinkel and Melli (1990) that they believed we "were unaware of" and argue that it shows that "the architects of the Wisconsin child support guidelines believed that non-resident parents did incur visitation costs" (p. **PROOFREADER: PAGE?**) and incorporated these costs into their guidelines. So Czapanskiy, they argue, was correct after all.

Although we indeed were aware of and familiar with the Garfinkel and Melli (1990) article, we readily concede that Garfinkel knows the background and history of the Wisconsin guidelines far better than we do (or most anyone else in the world) because he and his colleagues were the prime architects of the guideline. So we also concede, if he says so, that in Wisconsin some degree of visitation credit or adjustment was in fact built in there. We are curious to know from these architects exactly how much visitation was assumed in Wisconsin's guidelines and exactly how much cost was associated and credited, because this was not apparent from the quoted passage despite its length, nor have we found the exact assumption/adjustments in any of the published literature authored by the developers.¹¹ One independent analyst (Rogers, 2003), however, by comparing the final percentages Wisconsin arrived at to the economic data on which they were based (Van der Gaag, 1982), concludes that the visitation adjustment in Wisconsin was "equivalent to a Saturday afternoon visit with the child with no overnight costs" (Van der Gaag, 1982, p. 5).

But whatever the case in Wisconsin, it is just one state. Even if one makes the debatable assumption that the same is true in the 11 other states that use the basic percent-of-obligor-income approach that Wisconsin pioneered,¹² as Garfinkel et al. (2004) imply but do not

explicitly assert, that would still be far from “most” states. What about the much larger (and growing) number of states that do not use Wisconsin’s approach?

It was not we who called Czapanskiy mistaken. As we thought we made clear, it was Robert Williams, President of PSI, who was the architect of the rival income shares approach used (now) in 35 states. In Venohr and Williams (1999), he writes that Czapanskiy “*erroneously* [italics added] reported that *most* [italics added] states assume” the 20% visitation figure. “In truth,” he continues, “*most* [italics added] states make no assumption about visitation in their basic schedule” (p. 29). In a written response to an inquiry by the Virginia Child Support Study Commission (R. G. Williams, personal communication, August 28, 1992), in which he was asked about the extent to which visitation expenses had been incorporated into his system, he was even more explicit: “The answer to this question is ‘none.’ . . . Our review of other states’ guidelines indicates that *most* [italics added] states have not made adjustments for what might be considered normal visitation.” He added, “To my knowledge there are no data that would allow us to include in the schedule of child support obligations an adjustment for visitation costs,” underscoring once again the importance of the kind of data we tried to collect and present. In sum, our statement that most states’ guidelines assume no visitation expenses was accurate, and we stand by it.

ADDITIONAL COMMENTS

Although they are not strictly speaking criticisms of our study, we wish to respond to three additional points made by Garfinkel et al. (2004) in their section, “A Few Facts That Make Generous and Continuous Adjustments a Bad Idea.” First, Garfinkel et al. argue that

given that visitation declines systematically over time, finely calibrating all or even most child support obligations to the amount of visitation is a terrible idea. It sets up the custodial parent with an increasingly inadequate amount of child support unless she proactively pursues a modification of the agreement based on the actual behavior of her ex-spouse. (p. **PROOFREADER: PAGE?**)

The argument that visitation will decline over time, although widely believed, is actually somewhat questionable. In most of the reports, including two of the studies that Garfinkel et al. (2004) cite (Huang, Han, & Garfinkel, 2003; Seltzer, 1991), researchers compare visitation 2 years after divorce, for example, versus 12 years after divorce. However, because they all work with cross-sectional data sets that were collected at one point in time, in which mothers were asked to report the amount of visitation in the past month or year, they compare divorces that occurred 2 years ago versus those that occurred 12 years ago. Thus, length of time after divorce is confounded with year of divorce. This is an important problem because in the past, if fathers obtained less visitation to begin with than they do more recently, then even if they maintained it, researchers would still see less visitation in the older divorces and could erroneously conclude that fathers dropped out as time went on.

This confound is recognized by most authors but not acknowledged by Garfinkel et al. (2004). Seltzer (1991) cautions, “The most serious disadvantage to this cross-sectional analysis is that the sample of families separated for longer periods may represent a different population from those separated for shorter periods” (p. 96). Huang et al. (2003) caution, “The parents divorced 8 years or more did so in the 1980s or the early 1990s. . . . Indeed, the bias observed here may come from either duration [of the divorce] or a cohort effect” (p. 274). Furstenberg, Hoffman, and Shrestha (1995), who found the original evidence of visitation

decline using cross-sectional data sets, now warn, "It may well be that the consequences of divorce will be different for nonresidential fathers today because they sustain more continuous contact than did their counterparts a generation ago" (p. 331).

The problem is also present in the few studies, such as Wallerstein's, that have tracked the same families over long periods of time and found decreases in visitation, because in those long-term studies the divorces generally occurred in the 1970's. However, the final study Garfinkel et al. (2004) cite, Maccoby and Mnookin (1992), does not suffer from this conundrum because it is a rather recent longitudinal, rather than cross-sectional, study. The researchers recruited families from Northern California who all filed for divorce in 1984 and 1985, and interviewed them three times in the next 3 years. Do they report a drop off in visitation over time? Not for overnight visits. They found that, among those mother-residence families that started off with overnights during the school year, "In only 16 percent of these families were the children no longer visiting their fathers at Time 3; about two-thirds were still making overnight visits; and 19 percent had shifted to dual or father residence" (Maccoby & Mnookin, 1992, p. 174). Thus, similar percentages appeared to lose (16%) and gain (19%) overnights. Additionally, "For children who spent overnights with their non-residential parent at all three time periods, the number of overnights in a typical two-week period did not drop over the three years of our study" (Maccoby & Mnookin, 1992, p. 174).

Although overnights did not decrease, a decrease was found for daytime visits during the school year (which require less outlays by fathers than overnights anyway). The most common reason was a residential move by one or both parents, but "for mother-residence families, father contact dropped off only when driving distance increased by over an hour" (Maccoby & Mnookin, 1992, p. 183). Thus, Maccoby and Mnookin (1992) found that overnight visits remained surprisingly stable, whereas daytime visits were disrupted primarily by substantial parental moves. These findings are replicated in the other recent large-scale representative longitudinal investigation, Braver and O'Connell (1998), who also found that the number of overnight visits did not decline 3 years after the divorce, and that other loss of time with the father was largely a result of parental moves of more than 60 miles away.

Second, Garfinkel et al. (2004) suggest that most fathers do not need the visitation credit as much as mothers need the additional child support, because "after payment of child support is subtracted from the dad's income and added to the mother's and child's income, the standard of living of non-resident fathers is still about twice that of the mothers and children" (p. **PROOFREADER: PAGE?**), citing Garfinkel, McLanahan, and Hanson (1998). Although there are some other investigations with similar findings (e.g., Weitzman, 1985), in actuality all these earlier estimates are dubious. In Braver (1999) and Braver and O'Connell (1998), we start with a representative data set that has similar findings (on average, fathers have 1.6 times the standard of living of mothers in our sample vs. the 1.77 ratio—3.9 for fathers divided by 2.2 for mothers—that Garfinkel et al. [1998] find) based on identical methodology (dividing income by the poverty level for that type of household). But both of these results work with gross income. If using the more appropriate after-tax (i.e., spendable) income, which corrects for the far more beneficial tax advantages that custodial parents enjoy, the ratio reduces to 1.36. If one corrects further for the time the child actually spends in each household, the ratio reduces still further, to 1.1, close to even. Any remaining disparity would be reduced still further today, because child support guidelines are now substantially higher than in either study. In fact, Braver and Stockburger (in press) show that it is quite likely that the majority of custodial parents today have higher standards of living than noncustodial parents, even without taking into account longer term influences that will further disproportionately advantage custodial parents, such as remarriage and faster upgrading

of job skills and income.¹³ Thus, fathers arguably do need the credit as much as or more than mothers need the child support income.

Third, although Garfinkel et al. (2004) mount the case and build a full argument against “generous and continuous adjustments,” it is important to note that we did not really attempt to make the case for them. We took as our goal instead simply presenting the first empirical evidence relevant to the key assumption on which they were based, the cliff model of nonresident father expenditures on children. But, for the sake of debate, we present below a stronger case for adjusting child support for visitation expenses (see also Miller, in press).

The primary argument is simple fairness. Guidelines were propounded to arrive at some fixed and clear system to fairly apportion the costs of the children between the parents (Blumberg, 1999; Garrison, 1999). By federal law, states’ guidelines must be “just” (45 C.F.R. 302.56). In building these guidelines, most states did not adjust for any child-related expenses of the nonresident parent for the child or give any monetary credit against the child support owed. But this was simply because they assumed there were no such expenses, and there existed no data to the contrary. Now we presented such data, and it is only fair to make some reasonable credit for them.

Garfinkel et al. (2004) also argue that primary parents are more likely to resist visitation if it costs them. Our position is that the cliff model greatly raises the financial stakes for such battles at levels of contact near the cliff, whereas a continuous adjustment lowers the stakes at all levels. Why should an obligor seek more visitation¹⁴ and an obligee seek less if only a few dollars are at stake? But there is no need to speculate blindly about this. There is a fair amount of experience with the issue in Arizona, which has had such a system of continuous credits for visitation for 8 years. One of us is on the Arizona Committee reviewing these guidelines; they present little difficulty in practice and parents, attorneys, and judges generally support and endorse them. And a review of past awards by Venohr and Griffith (2003a) concluded that “the evidence suggests that there probably was not much gaming of parenting days to lower support award amount” (p. 17); “the . . . shared parenting adjustment appears to be working. . . . There is no evidence of a cliff effect” (p. 22).

CONCLUDING THOUGHT

It is important to note that the basic thrust of the findings is corroborated by our related findings (Fabricius, Braver, & Deneau, 2003) on college financial support by divorced mothers and fathers. College support is a substantial, post-child support voluntary expenditure for which we do have an exact dollar value, and for which we do have comparative data on mothers’ and fathers’ contributions, and in Pasley and Braver (2003), matched parents and child reports. What we found is that overall, mothers and fathers voluntarily contribute about equally to their students’ college expenses, even after controlling for mothers’ and fathers’ possibly different ability to pay, and that fathers’ contributions increase with the joint legal custody and amount of visitation they had to the point that with high levels of visitation, fathers contribute proportionately more than mothers. As we reported in Fabricius et al.:

Figure 2 presents the father’s college expense support across access arrangements, from saw fathers “minimally or not at all,” through “saw father some” and “a moderate amount,” to “saw father a lot”. . . . As the amount of contact the father was allowed to have progressed from one level to the next, the amount of college expense support increased by about \$981 per year. (p. 233)

Thus, father's voluntary contributions to their children's college expenses follow the same pattern as their "benchmark" expenditures, and in both cases show a linear increase with increasing amounts of visitation that disconfirms the predictions of the cliff model. We believe that these college support data lend validity to the benchmark expenditure data, making a strong case that increasing amounts of visitation are associated with real and important expenditures by fathers over and above child support. Despite Garfinkel et al.'s (2004) views to the contrary, we see no reason so far to doubt that argument.

NOTES

1. The invitation was actually something of a mistake. With our concurrence, Wallerstein was to be invited by the editor to respond to a different article we published in *Family Court Review* (Fabricius, Braver, & Deneau, 2003). The latter used a *Family Court Review* paper by Wallerstein and Lewis (1998) as its primary point of departure and presented findings strongly at odds with its conclusion, so it was appropriate to engage in dialogue with them. By accident, however, the invitation to Wallerstein erroneously invited her to respond to our article, a paper with the same authors and a similar title and related topic published in the next 2003 issue of *Family Court Review* (however, we never mentioned Wallerstein). The error was not recognized until after Wallerstein had enlisted Garfinkel and McLanahan as coauthors and they had written and submitted their critique, at which point it was mutually decided to let it stand.

2. Noted in their Technical Appendix I: "Staff at the Bureau of Labor Statistics . . . suggest that income reported in the CEX is too low relative to expenditures" (p. 3).

3. We also wish to distance ourselves from the ad hominem attacks and often derogatory tone in their comments. We feel that passages like "in addition to using their own bad data and covering up its inadequacy, Fabricius and Braver" (p. **PROOFREADER: PAGE?**) and "these data are so poor as a basis for making child support policy that it is hard to know where to begin or end" (p. **PROOFREADER: PAGE?**) add nothing to their argument, detract from the serious and scholarly nature of the debate, and ultimately are a disservice to readers and the field. We take particular exception to the charge that we attempted to cover anything up. We have striven in our reply to maintain a civil and scholarly tone in return.

4. Garfinkel et al. (2004) cite Seltzer's 1998 paper, but this must be mistaken because no relevant figures appear therein. Instead, the figures they quote are actually in her 1991 paper.

5. For example, we also compared Seltzer's rates of father visitation after 3 to 5 years, or for divorces that occurred between 1982 and 1985, to students' reports of time spent with dad "considering the whole time since the divorce" for those whose parents divorced between 1985 and 1990. Rates of minimal visitation were 18.1% in the National Survey of Family and Households (NSFH) and 19.4% in our study, and rates of frequent visitation were 33.2% and 43.1%, respectively.

6. The calculations from one of their databases, High School and Beyond (HSB), for example, are as follows: according to their Figure 1, 91% of children from two-parent families graduate high school; of these, Figure 2 shows that 57% started college—57% of 91% yields 52%. For one-parent families, 84% graduate high school, and 50% of those start college—50% of 84% yields 42%.

7. Our response alternatives were "none; a little; a fair amount; a lot *but you still frequently needed to bring some from your primary parent's home* [italics added]; and a sufficient amount so that *you rarely needed to bring some from your primary parent's home* [italics added]" (Fabricius & Braver, 2003, p. 326).

8. Which parent has the legal right to decide whether the child should have a car is an open question in joint legal custody families, which characterized about half of the sample.

9. Here we generally side more with Wallerstein's previous position, in which she has argued so passionately for hearing children's voices (Wallerstein & Lewis, 1998), than her present one, in which she doubts their credibility. For additional arguments concerning attending to the voices of children of divorce, see Fabricius (2003), L'Heureux-Dube (1998), and Warshak, (2003).

10. In Arizona's system of counting, for example, the following schedule of time in father's care would amount to 32.6%: alternating weekends, Friday from dinner time to Sunday at dinner time; every Wednesday from dinner time to school the next morning; 2 weeks during winter and spring vacation and 3 weeks in the summer—62% of a random sample of decrees specified at least 24% (Venohr & Griffith, 2003a).

11. Garfinkel's coarchitect and coauthor Melli (1999) wrote a paper focusing on an analysis of child support and time-sharing by parents. This would have been the perfect opportunity to be explicit about Wisconsin's assumptions. Instead, she writes only that

the issue in revising child support guidelines is, of course, whether or not the basic support formula considers the amount spent by the noncustodial parent when that parent spends time with the child. There is very little statutory or guideline statement on this point, but increasingly . . . case law assumes the expenses of [normal] visitation have been considered in the basic award. (pp. 223-224)

12. The categorization of states' guidelines is from the National Conference of State Legislatures Web site: <http://www.ncsl.org/programs/cyf/models.htm>. The percentage of obligor income states are Alaska, Arkansas, Georgia, Illinois, Massachusetts, Minnesota, Mississippi, Nevada, North Dakota, Tennessee, and Texas.

13. Fabricius, Braver, and Deneau (2003) present evidence that remarried mothers' standard of living is noticeably higher than remarried fathers', presumably because mothers, when they remarry, gain more income than they do expenses, whereas fathers, when they remarry, do the reverse. The majority of divorced mothers and fathers will remarry (Cherlin, 1992 **NOT IN REFS**), yet these changed circumstances almost never change child support amounts.

14. In our article, we also allude to this argument:

A third rationale for the cliff approach is that offering credits or offsets to child support obligations at lower levels of time with the child would only encourage bad faith efforts on the part of obligors to bargain for more time simply to reduce the amount of child support ordered. (p. 323)

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