



## Pop Goes the Woozle: Being Misled by Research on Child Custody and Parenting Plans

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## **Pop Goes the Woozle: Being Misled by Research on Child Custody and Parenting Plans**

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*Mental health professionals, lawyers, and judges whose work involves child custody decisions are often presented with social science research on issues related to which parenting plan is in the children's best interests. Unfortunately, this research can be misrepresented in ways that mislead these professionals and the children's parents, leading to child custody decisions that are not the most beneficial for the children. The process of misrepresenting the research in ways that create myths and misconceptions has been referred to as woozling. This article describes how social science data can be woozled, illustrating this with examples related to parenting plans for children under the age of 5 whose parents have separated.*

*KEYWORDS joint custody, overnighting, parenting plans, shared parenting, woozling*

When parents negotiate or litigate a child custody agreement, mental health professionals often present social science research on behalf of a particular parenting plan or custody recommendation. Understanding how the social science data can be manipulated and misrepresented helps professionals who are involved in child custody decisions make wiser decisions. It also reduces the likelihood of being led astray by misrepresentations and distortions of the research. This process of misrepresenting the data has been referred to as *woozing* and the mistaken beliefs it creates are referred to as *woozles* (Gelles, 1980).

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Several social scientists have written about how data can be misrepresented in family law, especially in regard to child custody issues (Cashmore & Parkinson, 2014; Johnston, 2007; Ramsey & Kelly, 2006). Lawyers and judges have also been warned about putting too much trust in custody evaluations because too many custody evaluators hold beliefs that are based on distorted, inaccurate, woozled versions of the research (Kelly & Johnston, 2005; Klass & Peros, 2011). Nielsen (2014c) provided a detailed account of woozling as it applied to one of the seven studies about parenting plans for very young children. This article expands on these ideas by providing examples from all seven studies that have compared the outcomes of various parenting plans for children under the age of five. Further, this article also describes how researchers can inadvertently or intentionally contribute to the woozling of their studies. This article's two central questions are as follows: How can professionals whose work involves making parenting plans for very young children be woozled by social science research? What can social scientists do to reduce the likelihood of woozles or to dismantle them once they have taken hold?

### WHAT IS A WOOZLE?

The term *woozle* was coined 35 years ago by sociologists Gelles (1980) and Houghton (1979). A woozle is a belief or a claim that is not supported—or is only partially supported—by the empirical evidence. Because the claim keeps “popping up,” though, the public and many policymakers come to believe it. As a result, inaccurate or seriously flawed data come to be accepted as the “research evidence” on that particular topic. Through a number of different woozling techniques, the findings from certain studies become magnified and widely disseminated, overshadowing those studies that would challenge the woozles. Eventually woozles can become so powerful that they have an impact on public opinion and public policy.

These distorted beliefs are similar to the imaginary woozle in the children's story, *Winnie the Pooh* (Milne, 1926). In the story the little bear, Winnie, dupes himself and his friends into believing that they are being followed by a scary beast—a beast he calls a woozle. Although they never actually see the woozle, they convince themselves it exists because they see its footprints next to theirs as they walk in circles around a tree. The footprints are, of course, their own, but Pooh and his friends are confident that they are onto something really big. Their foolish behavior is based on faulty “data”—and a woozle is born. More recently Gelles described woozling as “the use, abuse and misuse” of social science research (Gelles, 2007). Like Pooh and his friends who are led astray by their own footprints, we are misled by woozled data. Indeed, professionals, including a judge (Hutchins,

2014), a forensic psychologist (Franklin, 2014a), a developmental psychologist (Mercer, 2014), and a lawyer (Franklin, 2014b) have found the concept of woozling useful in understanding the myths that affect child custody decisions.

### POP GOES THE WOZZLE

Using another analogy from the children's nursery rhyme, *Pop Goes the Weasel*, a woozle often behaves like the weasel who keeps popping in and out of holes in the ground, evading the monkey who is frantically chasing it around a mulberry bush. Like the evasive weasel who keeps popping up no matter how hard the monkey tries to catch it, woozled data can seem to have disappeared, but keep reemerging and evading the people who are trying to "catch" them. Woozles are hard to catch for at least two reasons. First, certain aspects of a woozle might be true in that some studies' findings might lend support to some portions of the woozle. That is, there might be a small grain of truth buried in a bushel of untruths. Second, certain woozles are inherently appealing to a society's prevailing beliefs, so people are more willing to accept those woozles without questioning the data underlying them. Nobel Prize-winning economist and *New York Times* columnist Paul Krugman (2014) wrote about a similar concept that he called a "zombie"—a belief that "everyone important knows must be true, because everyone they know says it's true. It's a prime example of a zombie idea—an idea that should have been killed by evidence, but refuses to die. And it does a lot of harm" (p. A-21).

Woozling the data is not restricted to the social sciences, of course. For example, Nobel Prize-winning chemist Irvin Langmuir wrote about a similar process that he called "pathological science." By this he meant an area of research that will not go away because some researchers or the general public so desperately want those particular ideas to be true. Even though the theory underlying the ideas has been proven false by the majority of physicists, the "pathological science" lingers on (Langmuir, 1989). Similarly, Carl Sagan (1997) discussed processes similar to woozling in many fields of science. To reduce the odds of being led astray by pseudoscience, Sagan described ways we can improve our skeptical thinking. The skeptical thinking tools in his "baloney detection kit" include encouraging substantive debate, considering more than one hypothesis, and not getting overly attached to a hypothesis just because it is yours. Along the same lines physicist Robert Park warned us not to be duped by frauds—with frauds being similar to woozlers. There are frauds who are merely speculating, intending no harm, frauds who falsely claim that their work has a scientific basis when they know full well that it does not, and frauds who deliberately exploit bad science with the intent to deceive or to confuse people. As Park explained,

what might begin as the researcher's honest error can evolve from naive self-delusion to intentional fraud. In the beginning, some scientists honestly, although wrongly, believe they have made a great discovery. When it gradually becomes clear to them that they were wrong the unscrupulous researchers continue to defend and to woozle their findings rather than admit their errors and set the record straight (Park, 2000).

## HOW ARE WOZZLES BORN AND RAISED?

Although anyone who is involved in making child custody recommendations is subject to being woozled, being aware of how woozles come into being reduces the odds. By looking at examples from the research on parenting plans, we can more easily recognize woozles when we encounter them. As we will see, the woozling process involves a combination of factors, interacting with one another in ways that often are unpredictable and unforeseen. No single person and no single factor can be held responsible. Although researchers themselves might sometimes contribute to the woozling of their own data or the data of others, many other factors must come into play to keep the woozles alive.

The examples of woozled data in this article are all related to the same question: For infants, toddlers and preschoolers whose parents have separated, is spending overnight time in the father's care linked to any positive or negative outcomes? Put differently, should very young children spend all or almost all of the overnight time in their mother's home? Other issues and other studies could be used to illustrate woozling. But because there are only seven studies that have addressed this particular child custody question, this particular topic is well suited to a discussion of woozling.

Throughout this article, the term *overnighting* means children spending nights in their father's care while living almost exclusively with the mother when the children are under the age of five. The word *mother* is used instead of *primary care parent* or *residential parent*; and the word *father* is used instead of *nonresidential parent* or *secondary parent*. This more clearly reflects the fact that 95% to 100% of the children in the seven overnighting studies were living almost exclusively with their mother and overnighting with the father. The findings from several of the overnighting studies are mentioned briefly in this article to illustrate the woozling process. Extensive critiques and detailed comparisons are available elsewhere (Cashmore & Parkinson, 2011, 2014; Fabricius, 2014; Kelly, 2013, 2014; Lamb, 2012a; Ludolph & Dale, 2012; Milar & Kruk, 2014; Nielsen, 2014a, 2014b, 2014c; Pruett, Cowan, Cowan, & Diamond, 2012; Warshak, 2002, 2014).

So we turn our attention now to these questions: How does woozling occur and what examples of woozling are evident in the overnighting studies?

## MISREPORTING DATA OR WRONGLY CLAIMING SIMILARITIES AMONG THE FINDINGS

We begin with one of the most powerful and most common ways that woozles come into being—repeatedly misreporting or only partially reporting data from a few studies—a process Gelles (1980) referred to as evidence by citation. This can happen even among well-educated professionals who discuss or write about studies without ever having actually read them or having read only the synopsis or the abstract. These kinds of woozles have been referred to as scholarly rumors (Johnston, 2007). Often the few frequently cited studies are presented together, as if they all reached similar conclusions, when in fact they did not. This can mislead people into believing that there is an emerging consensus or a pattern in the data. As cognitive psychologists have documented, people tend to perceive patterns or consistency in data or in situations that are actually random and inconsistent (Chabris & Simons, 2010). In part this occurs because we are more likely to believe data that offer relatively simple, consistent explanations for complicated questions (Kahneman, 2011). Because we like a consistent story, when several research studies are presented together as though their findings are similar, we are inclined to believe there is a pattern or a trend even when none exists.

As we will soon see, repeatedly misreporting certain studies and then presenting them as if they all reached similar conclusions has happened in regard to five of the overnighting studies (Altenhofen, Sutherland, & Biringen, 2010; McIntosh, Smyth, Kelaher, & Wells, 2010; Pruett, Ebling, & Insabella, 2004; Solomon & George, 1999; Tornello et al., 2013). Scholars who have critiqued and compared these five studies have concurred that there was no pattern in the findings and that there were very few similarities among them (Cashmore & Parkinson, 2011; Fabricius, Sokol, Dizen, & Braver, 2015; Kelly, 2013; Lamb, 2012a; Ludolph & Dale, 2012; Nielsen, 2014a; Pruett et al., 2012; Warshak, 2014).

Nevertheless, as woozles are prone to do, the assertion keeps popping up that these five studies reached a similar conclusion: Frequent overnighting (defined differently in each study) is linked to more insecure attachments, more emotion (affect) regulation problems, or more behavioral problems for children younger than 4 years old. For example, Tornello et al. (2013, p. 883) concluded that their study “is the third of four studies on the topic that show some evidence of increased insecurity among very young children who have frequent overnights.” Likewise, the Australian researchers (McIntosh et al., 2010) who conducted one of the overnighting studies repeatedly stated that their findings overlapped with or were similar to four of the other overnighting studies (McIntosh, 2011b, 2012a, 2014c; McIntosh, Smyth, & Kelaher, 2013; McIntosh et al., 2010). As McIntosh (2012b) told an

audience in a keynote address: “To cut a long story short, we took these findings, looked at the other studies, and *saw a pattern*” (p. 5, emphasis added). But as we will see, there is no pattern.

To begin, the Altenhofen et al. (2010) study could not possibly have reached conclusions similar to any of the studies that compared overnighting to nonovernighting children for one simple reason: *All* of the children in the study were overnighting. There was no other group in the study. Likewise, this study could not possibly overlap with any other study in finding a link between affect regulation or attachment and overnighting for two obvious reasons. First, affect regulation was not a variable in the study. Second, there was no significant correlation between the number of times these children overnights and their attachment scores, as would have happened if overnighting was having any impact. The researchers simply found that 54% of these children of divorce had insecure attachment classifications—which is comparable to the 47% of insecurely attached children from single-parent families and higher than the 35% of insecurely attached children from intact families in general population surveys (Mulligan & Flanagan, 2006). In short, this study tells us nothing about differences between overnighting versus nonovernighting on attachment classifications.

The second study by Solomon and George (1999) has been woozled many times over the past 15 years. The many scholars who have critiqued this study concur that there was no significant link between overnighting, insecure or disorganized attachment classifications, or the toddler’s performance on a challenging task with his or her mother in the laboratory playroom (Cashmore & Parkinson, 2011; Fabricius, 2014; Kelly, 2013; Lamb, 2012a; Lamb & Kelly, 2001; Nielsen, 2014a; Pruett et al., 2012; Warshak, 2014). Solomon (1998) summarized their results clearly and succinctly years ago: “Attachment security with the mother was not related to . . . the number of overnights per month, the number of consecutive nights away from the mother, or how well the schedule had been followed. Overnight separation from the mother in and of itself is not necessarily seriously disruptive to the mother–child attachment” (p. 5). In the second phase of the study, there was no way to determine whether the overnighting babies were more distressed when briefly separated from their mother in the laboratory playroom because the overnighting and nonovernighting babies were never directly compared to one another. Given the persistent misrepresentations of their study, Solomon (2013) is still having to clarify their findings: “Neither the particular patterns of overnight visits nor the total amount of time away from mother predicted disorganized attachment” (p. 169). Keeping in mind that 37 of the 44 overnighting babies *very rarely* overnights (1–3 times a month) and that the researchers emphasized that the baby’s reactions to brief separations from the mother in the laboratory procedure were not any indication of how babies would react to overnight separations from their mothers in

divorced families, we have to wonder why the woozles emanating from this study have been so persistent.

Part of the answer lies in the fact that the study has been repeatedly misreported for so many years by other social scientists—an excellent example of what Gelles (2007) called “evidence by citation.” For example, for more than a decade the authors of the Australian study (McIntosh et al., 2010) have made statements that might unwittingly lead people to believe that Solomon and George found overnighting was linked to a number of serious, negative outcomes in the babies’ day-to-day lives and in the overall quality of their relationships with their mothers. Among these statements were that the overnighting babies were more irritable and more watchful and wary of separation (McIntosh et al., 2010), had a greater propensity for anxious, unsettled behavior when reunited with the primary caregiver and a greater propensity for insecure and disorganized attachment (McIntosh, 2011b, 2011d), had more difficulties in emotional regulation (McIntosh, 2012), more developmental strain (McIntosh & Smyth, 2012), more irritability and fretful behaviors and more vigilant monitoring of the whereabouts of their mother (McIntosh, 2013), and more unsettled, volatile and angry behavior and breakdown on reunion with the primary caregiver following a separation (McIntosh, 2014b). As far back as 2003 (Martin, 2003), McIntosh was quoted in a newspaper article as having said that researchers had found that babies who *live alternately* with their divorced parents develop *long lasting psychological problems*, that those arrangements caused *enduring* disorganized attachment, and that *as older children and adults*, they have *alarming* levels of emotional insecurity and poor ability to regulate strong emotion. In 2003, Solomon and George were the only researchers who had conducted an attachment study with overnighting infants, so McIntosh could only have been referring to their study—a study that did not reach any of the conclusions reported in the newspaper article. Even as recently as 2013, McIntosh still held the view that “The value of this study cannot be underestimated: the first to take a *deep, observational lens* to examine *how infants responded to mothers from whom they were frequently* separated overnight. It has inspired all the studies since conducted” (McIntosh, 2013), emphasis added.

Reporting the Solomon and George data out of context or in exaggerated ways can unintentionally lead people to believe the woozle that spending even one night a month away from their mother caused the babies to become so insecure and anxious that they got upset whenever they were separated from her; had “breakdowns” when she returned; and became more fretful, angry, and irritable in their day-to-day lives. In sum, this study is an excellent example of a woozle that has been harder to catch than the weasel running around the mulberry bush.

As for the next overnighting study (Pruett et al., 2004), because the researchers did not measure attachment security or emotion regulation, this study cannot be part of a “pattern” linking overnighting to insecure



attachments. Nor can this study be similar to others in finding that frequent overnighting had a more negative impact than occasional overnighting because that comparison was never made. In fact, this study cannot be similar to any study that found negative outcomes linked to overnighting because it found none. The overnighters were no different from the nonovernighters on five measures of well-being, with two exceptions. First, when the 4- to 6-year-old boys had inconsistent schedules and also had multiple caretakers, they were more anxious than the girls their age, a finding that the researchers attributed to boys having less advanced social skills than girls their age, not to overnighting. Second, overnighting appeared to benefit the 4- to 6-year-old girls because they were less withdrawn than the nonovernighting girls. Moreover, having multiple caregivers (because they were overnighting) had no impact whatsoever on any measure of well-being for the 2- to 3-year-olds and had a positive impact on the 4- to 6-year-old girls: “The worry about implementing overnights and parenting plans with multiple caretakers for infants and toddlers is misplaced” (Pruett et al., 2004, p. 55). Despite stating their findings very clearly, their data were still cited to support the woosle that overnighting puts children at greater risk. For example, McIntosh (2013) told a seminar audience that Pruett found “having multiple caregivers was a *significant problem* for young children” (emphasis added) and that Pruett et al.’s findings “overlapped with” the findings from their Australian study (McIntosh, 2013)—which was not true for the 2- to 4-year-olds. The only “overlap” was that neither study found any negative outcomes linked to overnighting for the 4- and 5-year-olds.

The Australian overnighting study (McIntosh et al., 2010) also had very little in common with the other studies and did not reach similar conclusions. First and foremost, the study did not include any measures of attachment or emotion regulation so it cannot possibly be similar to the only two studies (Solomon & George, 1999; Tornello et al., 2013) that did include those two measures. Second, four of the six measures had no established validity (Nielsen, 2014c; Warshak, 2014). Consequently, these data cannot be compared to findings from other studies because it is not clear what was actually being measured. It would also be difficult to find similarities with other studies as there was no clear or consistent relationship between the frequency of overnighting and the outcomes on most measures. For example, the babies who overnights more than three times a month were more irritable, according to their mothers, than those who overnights less often. But they were not less irritable than babies who never overnights or babies who lived in intact families. The limitations of the study and the problems related to interpreting its data have been enumerated elsewhere, leaving very little interpretable data for children under the age of four to compare to the findings from other studies (Cashmore & Parkinson, 2011, 2014; Fabricius, 2014; Kelly, 2014; Lamb, 2012a; Ludolph & Dale, 2012; Nielsen, 2014a, 2014b, 2014c; Pruett et al., 2012; Warshak, 2014). In short, this study did not, as its

authors have stated, “overlap with similar findings from Solomon and George and Althenhofen” (McIntosh, Smyth, & Kelaher, 2015, p. 114).

Similarly, the more recent study by Tornello et al. (2013) does not fit into a pattern of similar findings with the other overnighting studies. The Australian researchers (McIntosh et al., 2015, p. 113) were incorrect in asserting that the Tornello study was “*similar* to ours” and “*replicated many* of the Australian findings” (emphasis added). First, unlike any other overnighting study, Tornello et al.’s data came from a very distinct, atypical group of U.S. families: minority, inner-city, impoverished, poorly educated, never married parents with high rates of incarceration, mental health problems, and substance abuse—families where the mothers often had children by several different men with whom they had never even lived. Second, the study could not have much in common with the others because it used entirely different measures of children’s well-being and because it was one of only two studies (Solomon & George, 1999) that had used an attachment measure. Whereas Solomon and George found no link between overnighting and attachment classifications, Tornello et al. did find a link between frequent overnighting and insecure attachment classifications. Because they used an attachment procedure that was not valid, however, we cannot know what was actually being measured—which means their attachment data cannot be used to make any comparison to the Solomon and George study. As for the statement that Tornello et al. replicated the Australian findings, the word *replicated* is generally defined as repeating a study in all its important details to establish the reliability of the initial finding. None of the overnighting studies have replicated one another. Again then, Tornello et al.’s findings did not overlap or fit into a pattern with the other overnighting studies.

Given how often these five overnighting studies are misrepresented in academic journals and seminars, it is not surprising that the data are also misreported or woozled on the Internet by some social scientists. For example, a professor of human development and family studies at the University of Illinois (Hughes, 2014) wrote on his “Divorce Science” blog: “There is a growing body of scientific evidence that suggests young children’s well-being may be adversely affected by frequent overnight stays. At the moment 4 out of 5 studies of this issue have found that overnights stays lead to attachment issues.”

### CHERRY PICKING

Another common way that data can become woozled is to report only those studies or only particular findings from one study that support one particular point of view—a bias referred to as *cherry picking* (Johnston, 2007). Johnston (2007) also noted another version of cherry picking: researchers acknowledging the limitations of their own study in the presence of other

researchers, but minimizing or ignoring those limitations when discussing their findings with the media or with more naive audiences who do not have the expertise to recognize that they are being woozled.

One example of cherry picking occurred at a national conference for family court professionals. One panelist told the audience that the beneficial effects of shared parenting were “small” and that shared parenting plans were “less stable” (not as long lasting) than mother residence plans (Emery, 2012). Although he might have been referring to the magnitude of the statistical difference being small, Emery (2012) cited only 1 of the 38 studies that had compared the effects of shared parenting to mother residence—36 of which showed better outcomes for the shared parenting children. He also cited only 2 of the 10 studies on the stability of shared parenting plans—8 of which showed the majority of shared parenting families were long lasting (Nielsen, 2013a, 2013b). For other examples of cherry picking in regard to the research on overnighting and shared parenting see Nielsen (2014c).

Journal or book editors can also cherry pick in what they choose to emphasize in their introductions or summaries and in which people they invite to contribute to the volume. For example, for a special issue of *Family Court Review (FCR)*, the guest editor, Jennifer McIntosh, was criticized for having presented what many scholars considered to be a one-sided, inaccurate presentation of the research on babies’ attachments to their parents and the implications this might have for overnighting (Lamb, 2012b; Ludolph, 2012). For example, in her introductory summary article for the issue, McIntosh (2011a) presented only Schore’s opinions (Schore & McIntosh, 2011) as if they were the “generally agreed upon view” in neuroscience: “From current neuroscience the dominant mother care of infants is not just sociologically informed; in normal development, the female brain is specifically equipped for the largely nonverbal, affiliative, nurturing aspects of attachment formation with an infant” (McIntosh, 2001a, p. 424). McIntosh did not mention the views of Siegel, who in the same journal issue (Siegel & McIntosh, 2011), voiced the opposite opinion: “I know people say women are more integrated because their corpus callosum is thicker. So what? That does not mean you cannot have as loving relationship as a male does with an infant. The primary caregiver is someone who is tuned in to the internal experience of the child, not just the child’s behavior. . . . Males can do it, and females can do it. And some females cannot do it, and some males cannot do it.” Three years later, the executive director of the Association of Family and Conciliatory Courts (AFCC), which publishes *FCR*, acknowledged that cherry picking had occurred: “AFCC and *FCR* were criticized for allowing one side of a controversial issue to be represented in *FCR* without counterpoint in the same issue. . . . In retrospect we would have made adjustments in order to create the best possible discussion” (Salem & Shienvold, 2014, p. 146).

Cherry picking is also evident when certain studies are all but ignored or are underemphasized in articles about overnighting. For example, the

very first study (Maccoby & Mnookin, 1992) to compare overnighting to nonovernighting babies is rarely if ever mentioned in the literature. At the outset of the study, 54 of the babies under the age of three had no contact with their father, 50 had daytime contact only, and 60 had more than 25% overnight time in their father's care. Three years later, all but one of the children who had been overnighting before the age of three still had fathers who were fully involved in their lives. In contrast 70% of the children who had not overnighted before the age of three no longer had any contact with their fathers. Given this, the researchers concluded that overnighting might be an important incentive for keeping fathers involved in their children's lives—a benefit that might not be immediately apparent, but would emerge as the children aged. Yet this important finding is rarely mentioned in discussions of the research on overnighting.

The question in regard to the overnighting studies is this: Why has the cherry picking favored the negative outcomes? That is, why has the focus been mainly on the few negative findings associated with overnighting rather than on the majority of positive or neutral findings? In part this negative cherry picking might be related to another woozling process referred to as confirmation bias.

#### CONFIRMATION BIAS: I'LL SEE IT WHEN I BELIEVE IT

Woozles are more likely to thrive when they confirm beliefs that people already hold—an effect referred to as *confirmation bias* (Chabris & Simons, 2010). Operating with this bias, we are overly critical and dismissive of data or ideas that contradict our existing beliefs. Kagan (1998) used the term “seductive ideas” to describe beliefs that are so appealing to the general public that most people readily believe any study that supports them. As the British philosopher, scientist, and statesman Francis Bacon wrote: “For what a man had rather were true he more readily believes,” (Bacon, 1620). Or as a more modern idiom puts it, “I’ll see it when I believe it.”

Confirmation bias might help explain why the few negative outcomes that have been linked to overnighting have received more attention than the majority of positive or neutral findings, especially in the media. Many people still believe that females are, by nature, better than males at raising, nurturing, or communicating with children—especially infants and toddlers. For those individuals, studies with any negative findings linked to the baby's being away from the mother overnight would be more appealing and more readily accepted. In fact, however, there is no empirical evidence that human females have a maternal “instinct”—an inborn, automatic, natural, built-in set of skills that better equips them to take care of babies. A mother's

responsiveness and nurturance of a baby—just like a father’s—is largely acquired through experience, not through instinct or through some unique feature in her brain (Hrdy, 2009). In fact the same areas of the brain become activated in mothers and in fathers when they are interacting with their baby or when they hear their baby cry (Atzil, Hendler, Sharon, Winetraub, & Feldman, 2012; Mascaro, Hackett, & Gouzoules, 2013; Swaim & Loberbaum, 2008). Likewise, fathers are just as capable as mothers of matching and understanding their baby’s nonverbal signals and emotions—a skill referred to as *synchronicity* (Feldman, 2003). In fact, among gay male couples, the father who was doing most of the daily caregiving was better at synchronizing and understanding the baby’s signals and had more neural activity in those parts of the brain associated with nurturing behaviors (Abraham, 2014). Then, too, both the father’s and the mother’s oxytocin levels (the amino acid associated with nurturing behavior) increase when they are interacting with their baby, and the father’s testosterone levels (the hormone associated with aggression) decrease (Gordon, Sharon, Leckman, & Feldman, 2010; Kuo, Carp, Light, & Grewen, 2012).

Another possible reason why the few negative findings linked to overnighting attract more attention is that those particular findings confirm several of the commonly held beliefs about babies and their mothers: first, that babies are naturally more attached to their mothers than to their fathers; second, that the infant’s bond with the mother is more primary and more influential than its bond with the father; and third, that their bond will be weakened if the baby spends too much time away from the mother. According to contemporary research, however, these beliefs are not supported by the empirical data. Babies form important attachments to both parents at around 6 months of age, and a secure bond with the father is just as beneficial and just as primary in importance. Among a few of the findings from specific studies are that infants and toddlers seek comfort equally from both parents (Bretherton, 2011); that although most 12- to 18-month-olds turn first to their mothers when they are distressed, there is no overall preference for either parent (Lamb & Lewis, 2013); that fathers support children’s sense of security as much as mothers do (Freeman, Newland, & Doyl, 2011); and that the link between an insecure relationship with a parent at age 15 months and subsequent behavioral problems at age eight is just as strong for the relationship with the father as it is with the mother (Kochanska & Kim, 2012). In sum, when it comes to their susceptibility to being woozled, people will more readily believe studies that confirm their preexisting beliefs about babies and mothers—even when those data are weak, flawed, or inconclusive.

## THE EXPERTS SAY: THE CONSENSUS IS

Another way that data can inadvertently become woozled is to use phrases like “the experts agree” or “the consensus is” to make it appear as if there is general agreement on a particular topic in situations where there is not. Statements such as these should be documented by citing the research, otherwise the “expert” claims are not trustworthy. For example, in a widely read issue of *FCR*, Schore stated that babies should not spend overnight time in their father’s care after the parents separate because “The science suggests” that one parent needs to be a “constant source of nightly bedtime routines” (Schore & McIntosh, 2011, p. 508). Yet Schore did not cite a single empirical study to support his dramatic assertion.

Similarly, because consensus reports do play an important role and merit special attention, the word *consensus* should be used judiciously and in the proper context. For example, the AFCC sponsored a Think Tank meeting of 32 professionals in law and social science to see if they could reach any consensus about legal presumptions for shared parenting and about overnighting. The group reached no consensus. “The discussion . . . stalled” (Pruett & DiFonzo, 2014, p. 163). One year later three of the people who had attended the meeting wrote an article expressing their views on overnighting (Pruett, McIntosh, & Kelly, 2014). The authors included the phrase “a consensus view” in their subtitle, and in their abstract they alluded to the previous year’s Think Tank meeting. This might unintentionally have created the impression that their coauthored paper represented a consensus of the opinions of the Think Tank, which it did not. One of the authors also announced in a keynote address at a national conference (McIntosh, 2014a, p. 5) and in an article for the Australian Psychological Society’s magazine (McIntosh, 2014b, p. 4) that “The work of progressing toward a consensus on infant overnights was then tasked by AFCC to a trio of developmental and divorce researchers.” Again, the words *consensus* and *tasked by* might be misunderstood to mean that AFCC or the people at the previous year’s meeting agreed with these three authors’ views on overnighting—or that AFCC had commissioned them to write an article that represented the organization’s position. Because a paper that represents the views of a group of scholars is likely to have more influence and more credibility than a coauthored paper, the word *consensus* needs to be used judiciously and precisely. For example, a paper entitled “Social Science and Parenting Plans for Young Children: A Consensus Report” was endorsed by 110 scholars who all agreed on a set of recommendations and who all endorsed the review of literature on which those recommendations were based (Warshak, 2014). These 110 scholars reached the consensus that regular and frequent overnights for infants and toddlers need not be postponed until the children are older. To ensure that people were not misled by the word *consensus*, Warshak (2014) went even

further by clarifying that the paper did not represent a consensus of all scholars in regard to the topic of overnighting.

The word *consensus* might also lead to woozling by creating the impression that certain articles deserve special attention or are more trustworthy and credible because they represent the opinions of a group of experts, when in fact that is not the case. For example, McIntosh et al. (2015) contended that an article McIntosh wrote with Kelly and Pruett (McIntosh, Pruett, & Kelly, 2014) was commendable for representing “important elements of consensus writing” (p. 117). By their definition, any paper written by more than two people who make certain concessions to reach mutual agreements on particular issues would be deemed noteworthy as an example of “consensus” writing. In contrast, McIntosh et al. (2015) contended the Warshak (2014) paper that was read, critiqued, and endorsed by 110 scholars was not a “consensus” paper—and was nothing more than a “petition” (McIntosh et al., 2015). In that vein, it is worth noting that in the group that endorsed the Warshak paper, there were 11 people who had held major office in professional associations, such as a former president of the American Psychological Association; 5 university vice presidents, provosts, or deans; 14 professors emeriti; 17 department chairs; 61 full professors; 16 members of the American Board of Professional Psychologists (ABBP), and eight professors with endowed chairs. Others were leading attachment researchers, the current editor of the major journal on attachment, and leading day care and early child development researchers.

Clearly then, the word *consensus* can be confusing. Perhaps the best way to reduce the likelihood of woozling with this word is to use it as defined by *Webster's Dictionary*: “a *group* decision making process that seeks the consent of *all* participants; a professionally acceptable resolution that can be supported by each individual in the *group*; a general agreement of a *group's solidarity*” (emphasis added). By *Webster's* definition, Warshak's (2014) paper is a consensus report, but coauthored papers are not.

## PRESENTING DATA OUT OF CONTEXT

Data can also become woozled when presented out of context, especially in press releases and abstracts. One way this happens is reporting the few negative findings without giving equal attention to the nonsignificant or positive findings. Another is ignoring or failing to emphasize the unique, atypical characteristics of the people in the study, which woozles people into believing that the findings are applicable to the general public, when in fact they are not. Or the data can be presented in alarming ways, when in fact the findings are not particularly unusual for the sample in the study.

The press release (Samarrai, 2013) and the abstract for the study by Tornello et al. (2013) illustrate all three of these problems. First, both

focused almost exclusively on the one negative finding: 43% of the frequently overnighting infants had insecure attachment ratings versus 25% of the nonovernighters. Not mentioned was the fact that on six of the seven measures, there were no differences among the various groups of overnighters or nonovernighters. Also not mentioned was the fact that these findings were not applicable to the vast majority of separated parents because these parents were members of racial minorities who were living in abject poverty in inner cities, with high rates of incarceration, drug and alcohol abuse, and mothers having children with several different men out of wedlock. Further, in the general population of impoverished single-parent families, 61% of the babies and 43% of the toddlers have insecure attachment ratings (Andreassen & Fletcher, 2007). Put into context, this means that the babies in the study had “normal” attachment classifications for children living in similar types of families. Finally, 26 of the 51 frequently overnighting babies assessed on the attachment measure lived primarily with their father—a highly atypical situation. Unless we know why these babies were not living with their mothers, we cannot put these attachment data into proper context. Other examples of presenting data out of context have been discussed elsewhere in regard to the Australian overnighting study (Nielsen, 2014c; Warshak, 2014), and earlier in this article in regard to the Solomon and George (2009) study.

#### INVALID OR UNRELIABLE TESTS AND PROCEDURES

All research studies have limitations. But as long as they are acknowledged frequently and openly whenever the data are presented, especially in media interviews, woozling is less likely to occur. One of the most serious limitations is using a scale or a procedure that has no established validity or reliability—or using a valid instrument but not following the correct procedures in administering it. More important still, data from invalid measures have to be presented as speculative at best because there is no way to determine what is actually being measured.

One example of woozles that can arise from invalid measures is the Australian study that concluded that frequent overnighting had a “deleterious impact” on children under the age of four (McIntosh et al., 2010). Based on their “visual monitoring scale,” these researchers reported that the frequently overnighting toddlers were significantly stressed, worked harder to monitor their mother’s presence, and had an “added degree of vulnerability” (McIntosh et al., 2010, p. 144). However, the three-item scale was one the researchers had created for their study without being able to establish its validity or reliability. They had merely extracted three questions from the Communication and Symbolic Behavior Scales (CSBS), which measures children’s readiness to learn to talk (Wetherby & Prizant, 2001). On the CSBS, frequently gazing at and trying to get the attention of the mother are positive



signs that the baby is ready to learn language. The Australian researchers, though, used the three questions to measure how anxious, stressed, or vulnerable the baby felt in the mother's presence—which they interpreted as signs of insecurity and anxiety. The findings from these three questions were widely reported from 2010 to 2014 as evidence that babies who overnights were more stressed, more anxious, and more insecurely attached to their mothers (Nielsen, 2014). In responding to scholars who had pointed out that their scale was not valid (Nielsen, 2014c; Warshak, 2014), the authors defended it as a “utilitarian compromise” that was “theoretically derived” from attachment theory (McIntosh et al., 2015, p. 116). This position not only ignores the accepted standards in social science for using valid measures, but it is also at odds with McIntosh's own statements in a keynote address, which she ended with her “prayer to the secular God/Goddess of Family Law”: “Give us sensitive research tools and deliver us from shallow methodologies” (McIntosh, 2012b).

Similarly Tornello et al. (2013) relied on an invalid procedure for measuring attachment. Although they acknowledged this problem, they presented the attachment findings as if they were extremely important—especially in the press release and in the abstract of their paper. They diminished the importance of this serious limitation by stating that the scale “can be *called into question*” . . . “can *perhaps* fail to detect true effects,” and might “*perhaps be potentially* biased” (emphasis added, p. 883). In fact, however, data from invalid procedures are definitely “in question” because it is not clear what has been measured. If Tornello and her colleagues had emphasized this problem and then presented their attachment data more tentatively, perhaps their findings would not have become woozled to the extent they were in the media, as we will soon see.

### OVERSTATING THE PRACTICAL SIGNIFICANCE OF DATA: MAKING MOUNTAINS OUT OF MOLE HILLS

Another way that woozles can arise is overstating the practical significance of the findings, especially when the scores are within normal range for that particular population. There might be a statistical difference on a measure between the groups in the study, but the practical significance of that difference is relatively minor. Similarly, the findings might be presented as if they have a considerable impact on people in real-life situations, when there is little to no evidence that the factor has much impact at all.

For example, in the Australian overnighting study (McIntosh et al., 2010) the researchers and the journalists reported that the toddlers who frequently overnights “showed severely distressed behaviors in their relationship with their primary parent” (Nielsen, 2014c, p. 9). These behaviors included biting, kicking, and hitting their mother; refusing to eat and gagging on food; and

clinging to her when she tried to leave. As alarming as this sounds, half of the 4,400 mothers in the Australian national survey from which the sample was drawn reported that their toddlers engaged in this relatively normal toddler behavior (Smart, 2010). More recently a Norwegian study with 1,159 toddlers also found that this kind of behavior is fairly frequent for 1- to 2-year-olds, and that it decreases by age 3 (Naerde, Ogden, Janson, & Zachrisson, 2014). In other words, the Australian researchers were overstating the significance of these kinds of fairly common toddler behavior by interpreting them as signs of “severe distress” that they attributed to overnighting.

Overstating the data’s practical significance is especially common in studies that include attachment measures. Except for those social scientists who know what the term *insecure attachment* means, many people are likely to believe insecure attachment means the baby is not closely bonded to the mother or that their relationship is weak, unloving, or unhealthy. In fact, however, insecure attachment means none of these things. First, as Waters and McIntosh (2011), who created one of the most widely used attachment measures, explained, “There is no way of measuring the strength of attachment” (p. 476). Attachment procedures merely assess how infants and toddlers react when stressed by the presence of a stranger or by new or challenging situations, which parent they seek out first for play or for comfort, and how confident they feel about exploring their surroundings (Newland, Freeman, & Coyle, 2011). Second, there is not a strong, consistent link between a baby’s attachment classification and his or her future behavior or relationships with peers, romantic partners, or parents (Ludolph & Dale, 2012). In other words, babies’ attachment classifications do not reliably predict future outcomes. In contrast, a baby’s having a loving, attentive, responsive relationship with both parents is linked to better outcomes later in the child’s life. Again, though, this is not what attachment measures are assessing. Third, a baby who is classified as having a secure attachment does not necessarily have a loving, healthy relationship with the mother. For example, babies with abusive or neglectful parents and babies who are overly dependent on their mothers can still be classified as securely attached (Zeanah & Emde, 1994). Fourth, in U.S. research studies only about 60% of the children are classified as securely attached (Rutter, 1997). But this does not mean that the 40% with insecure classifications have troubled, unloving, or weak relationships with their mothers. Finally, attachment measures are not designed to assess how children will adapt to overnighting after their parents separate or to assess which parent is better at taking care of the child. For all of these reasons, the practical significance of a baby’s attachment classification is of limited value in regard to custody issues. Unfortunately there are judges and family court professionals who are misled or confused by the terms *attachment* and *bonding* (Arredondo & Edwards, 2000).

Developmental psychologist Jane Mercer (2011) compared people's fears about babies becoming insecurely attached to the fears of the heffalump in *Winnie the Pooh*. Pooh's friends were afraid of an unidentified creature they called a heffalump. As it turned out, the feared heffalump was nothing more than poor old Pooh stumbling around with his head stuck in a honey pot. In regard to babies' overnighting after the parents separate, Mercer (2014) wrote, "My bet is that the heffalumps have been romping with the wozzles to create an unnecessarily increased tension about attachment." In sum, we need to keep in mind that statistically significant findings on attachment measures might be of little practical significance in real-life situations.

### SMALL STUDIES WITH BIG DATABASES

Woozling can also occur when people are misled to believe that a study included far more people than it actually did. If the size of the database is emphasized—especially in press releases, abstracts, or keynote addresses—people can easily get the impression that there were large numbers of people in the actual study, which makes the findings seem more important or more credible. For example, McIntosh (2012b, p. 4) told an audience at a national conference that "our study explored a large randomly selected general population data set which amounts to 10,000 children." What was not mentioned was that there were as few as 14 children in some groups in the study and that small sample sizes were a major limitation of their study. Likewise, Tornello was quoted in the widely disseminated press release (Samarrai, 2013) as saying that their study "analyzed data" from "a national longitudinal study of about 5,000 children." What was not stated was that the numbers were much smaller in the actual study, especially on the attachment measure—the one finding that was most widely reported in the media. In fact, the findings on insecure attachments and frequent overnighting were based on data from only 55 frequently overnighting babies—a far cry from 5,000 children. To show how distorted data can become, one journalist reported that: "The University of Virginia study *assessed the attachment of 5,000 young children*" (Rowlands, 2013, emphasis added). In sum, the likelihood of woozling is reduced if social scientists and journalists emphasize or only report the actual numbers of people in the study.

### MISLEADING TITLES, ABSTRACTS, AND PRESS RELEASES

Abstracts and press releases can also accidentally contribute to the woozling of a study's findings. Busy professionals and journalists are apt to read only the initial press release announcing the results of the study. It is often

only the abstract of a paper that is picked up by search engines and provided for free on a journal's Web site, as opposed to the entire article that must be purchased. This places a special burden of responsibility on the researcher to provide a balanced press release and abstract, summarizing the major findings and specifying the unique characteristics of the sample.

One illustration of this was the study by Tornello et al. (2013). The university's press release (Samarrai, 2013) began with the alarming title "Overnights Away from Home Affect Children's Attachments," and then reported that "infants who spent at least one night per week away from their mothers had more insecure attachments to the mother compared to babies who had fewer overnights or saw their fathers only during the day." The most alarming statement, which ended up in dozens of newspapers worldwide, was that: "43% of babies with weekly overnights were insecurely attached to their mothers versus 16% with less frequent overnights." Further, Tornello was quoted as saying that the study *could be used by judges "to help decide whether babies are better off spending overnights with a single caregiver"* (emphasis added). Keep in mind that attachment measures do not assess how securely attached babies are to their mothers and are not designed to be used in making custody decisions about overnighting.

The press release might also have inadvertently contributed to woozling by not mentioning six important facts and findings. First and foremost, there were no significant links between overnighting and six of the seven measures of children's well-being. Given this, a more accurate title for the press release and ensuing media reports would have been "Overnighting Makes Very Little Difference." Second, these findings were not applicable to the vast majority of separated or divorced parents given the unique characteristics of the sample. Third, the most frequently overnighting 3-year-olds were better behaved at age five. Fourth, attachment measures do not assess how securely bonded babies are to their mothers. Fifth, the 55 frequently overnighting babies' attachment scores were well within normal range for children from poor, single-parent families (Andreassen & Fletcher, 2007). Sixth, the attachment measure used in this study was not a valid one, meaning that we do not know what these data mean.

The press release also stated that the second author, Robert Emery, who was Tornello's thesis advisor, "advocates parenting plans where day contact with fathers occurs frequently and overnights away from the primary caregiver are minimized in the early years" (Samarrai, 2013). Long before conducting this study, Emery held the view that overnighting should be restricted for the first 3 years of a child's life: "Secondary attachment figures (fathers) can have frequent but *relatively brief contacts* with their baby during the first year of life, but the contact can become longer and more frequent as babies become toddlers" (Emery, 2011, emphasis added). Further, Emery believes it is sufficient for the father's brief daytime visits to

take place at his child's day care center or in the mother's home (Emery, 2004, p. 180). To reduce the potential for woozling, regardless of their own opinions on issues related to their study, researchers should strive to avoid making recommendations for the general public if their study is based on a sample that is not representative of the general population.

In sum, the Tornello et al. (2013) study illustrates several of the pitfalls that the AFCC Think Tank warned against—pitfalls that contribute to the creation of woozles. First, one finding from a study should not be given disproportionate attention. Second, one negative outcome should not be presented without presenting the other nonsignificant findings that are equally revealing. Third, when the scores fall within the normal range for that particular population, the differences should be interpreted as less relevant for making recommendations about child custody or parenting plans (Pruett & DiFonzo, 2014).

### WOOZLING IN THE MEDIA

Data can also be misrepresented in the media, becoming increasingly woozled as the story travels around the world. This might happen when researchers present their findings directly to the media and use dramatic stories to make their data more memorable (Park, 2003). There are a number of reasons why some social scientists are more likely than others to woozle their data—or why some studies are more susceptible to woozling regardless of the researchers' best efforts to prevent it (Thompson & Nelson, 2001). First, some social scientists benefit from media attention because it provides public and professional recognition, which, in turn, can increase their incomes. For example, if the researchers are generating income from counseling services, speaking engagements, or seminars that are largely dependent on their own study's findings, they might be more inclined to exaggerate or to overreach their data to boost people's desire for their "product." Second, social science appears to be easier to understand than disciplines like neurology or physics. Given this, reporters might not ask social scientists the kinds of probing questions that would reduce the odds of the data becoming woozled. Third, it is easy to frame social science data in ways that relate to public concerns, even if the studies were not designed to address those particular issues. In these situations the media can more easily spin the data in ways that support a particular position or policy. Then, too, confirmation bias can affect the way journalists report the findings. For example, reporters' own feelings about overnighting can influence the kinds of questions they ask and which researchers they choose to interview. Given the media's influence, it is incumbent on social scientists to inform the media when their own data or when other scientists' data have been inaccurately reported, overstated, or distorted. The more media coverage a study receives, the more potential

impact it has on public opinion or policy. So these studies should be even more carefully scrutinized by researchers who want to protect the public from woozles.

One recent example of how the media contribute to woozling is the study by Tornello et al. (2013). As already explained, the initial press release and the abstract focused primarily on the one negative finding about insecure attachments. Not surprisingly then, the study was soon being woozled internationally under alarming headlines such as these: “Overnight Separation Linked to Weaker Bond” (Preidt, 2013), “Babies Who Spent More Than One Night Away from Mother Are More Insecure” (Furness, 2013), “Nights Away from Mum Leave Babies Less Secure: New Findings Could Affect Custody Rulings for Young Children” (Nights away, 2013), “Divorce Study Shows Infants’ Attachment to Caregivers Affected by Joint Custody” (Divorce study, 2013). Woozling the data even further, one journalist wrote, “A new study suggests parents make or break their child’s ability to form healthy relationships for life before the baby’s first birthday. When babies spend even one night away from their primary caregivers in that first year those babies may be in for tough times building relationships as adults” (Hallas, 2013). Similar stories appeared in India (Spending Nights Away from Home Affects Baby’s Attachment, 2013) and Australia (Infants Who Overnight, 2013), as well as on a medical news Web site (Scutti, 2013), a law firm’s Web site (Kenny, 2013), and *Psyche Central’s* Web site (Wood, 2013). More noteworthy still, the British Psychological Society (2013) reported the study on their Web site under the headline, “Staying Away Affects a Baby’s Attachment.” These alarming and distorted media reports were reminiscent of what had happened only a few years earlier with the Australian overnighting study (McIntosh et al., 2010) whose findings had been woozled worldwide in the media since 2010 (Nielsen, 2014c). In the case of both studies, shortly after their release, the woozles started running around the mulberry bush, popping up in various forms in the media around the world.

In the wake of the media woozling of the Tornello et al. (2013) study, another researcher (Sokol, 2014) reanalyzed their data. Sokol correlated each child’s attachment rating with the actual number of nights each child spent in the father’s care, instead of separating the children into groups before analyzing the data as Tornello et al. had done. Sokol found no significant correlation between the number of overnights and the children’s attachment scores. Sokol’s study, however, has not yet made its way into the media—and has not been able to catch the woozles emanating from the Tornello et al. study.

## ISSUING WARNINGS WITH AMBIGUOUS TERMS

Data can also become distorted into wozzles when researchers use ambiguous terms when they issue warnings based on their research. As a hypothetical example, assume that a research team repeatedly states in media interviews and in their academic papers that the babies and toddlers in their study who “frequently” ate food containing peanuts were more irritable, anxious, unmanageable, and insecure than the babies who ate peanut products “less frequently,” “rarely,” or “occasionally.” They reassuringly report, though, that the children who were older than four had no adverse reactions to frequently eating peanut products. Unless the researchers make it abundantly, emphatically, and repeatedly clear from the outset that they are not recommending that children under four should never eat peanut products, the study runs the risk of being taken to mean: “Never feed children any peanut products until they are at least 4 years old.” It is highly unlikely that most people would assume it was a good idea to occasionally feed babies and toddlers peanut products—unless the researchers repeatedly and emphatically publicized that fact. Especially if the study is repeatedly represented in the media with frightening headlines like “Babies Struggle After Eating Too Many Peanut Products,” an anti-peanut-eating wozzle is almost inevitable.

In fact, this hypothetical example is not so hypothetical after all. Until 2000 the American Academy of Pediatrics had recommended withholding food containing peanuts until children were 3 years old. In 2008 they revised that position and said there was no conclusive evidence on the topic. Not surprisingly, most parents were still hesitant to feed their very young children any food containing peanut products. Meanwhile children’s peanut allergies continued to increase in the United States and Britain, while they remained low in Israel where most babies routinely ate peanut products. Finally, in 2014 British researchers conducted a study where one group of babies at high risk of developing peanut allergies were given foods containing peanut product until they were 5 years old. The other high-risk group was denied peanut products. At the age of five, the peanut-eating children had significantly fewer peanut allergies (DuToit et al., 2015).

The point is that responsible researchers should take extra precautions to clarify their findings when they are issuing warnings, especially when using ambiguous words like *occasional* or *frequent*. For example, in the Australian study (McIntosh et al., 2010), one group of babies had “regular” and “frequent” overnights. People might assume that regular meant at least weekly and that frequent meant far more than one overnight a week. In fact, however, regular and frequent merely meant overnighiting more than three nights *a month*—not necessarily weekly or on any regular basis. Similarly when researchers issue warnings with phrases like “contra-indicated,” “caution against,” “generally best avoided,” or “only when necessary,” people are

likely to understand this to mean: *Do not ever do this*. Researchers might be perceived as disingenuous or dissembling if they claim at some later point that they never imagined that their warnings could possibly be construed as a “never do this” message. It might be true that, when the researchers issued their warnings, they briefly mentioned that there might be “exceptions” or that their warnings might not apply in “all” cases. As in the peanut warnings, though, most people respond to warnings or contra-indications as if these are rules that apply in all cases.

One example of this is the Australian study (McIntosh et al., 2010) that was widely interpreted as a warning against overnighting under the age of four (Nielsen, 2014c). Over a period of years, having made such statements as “in early infancy overnight stays are contra-indicated, undertaken when necessary or helpful to the primary caregiver” (McIntosh, 2011d, p. 4), the lead author was often perceived and often reported in the media as being opposed to overnighting. For more than half a decade the “anti-overnighting” woozles emanating from this study circulated worldwide in the media and in the academic community (Nielsen, 2014c). Four years after the study’s release, two academic papers (Nielsen, 2014c; Warshak, 2014) pointed out the woozling and the limitations of the study, followed by an article in a major Australian newspaper (Arndt, 2014). It was at that point that McIntosh posted a statement on her counseling center’s Web site (McIntosh, 2014d) and coauthored an article (McIntosh et al., 2014) in which she stated that she had never recommended that very young children should “never” overnight.

#### SUGGESTING STATISTICALLY SIGNIFICANT FINDINGS WHERE NONE EXIST

Data are also more easily woozled when the findings are worded as if they are statistically significant, when in fact they are not. For example, in studies where there are no statistically significant differences, people might report the findings with misleading phrases like “greater propensity for,” “some evidence of,” “difference in the expected direction,” “indicative of,” or “more likely to.” For example, in regard to the Solomon and George (1999) study, it would not be lying to say that “more” of the babies who overnighted had disorganized and insecure attachments than babies who did not overnight. Lying, no; but woozling, yes, because the difference was not statistically significant. Given this, it can mislead people to report that the overnighting babies had “*a greater propensity* for insecure and disorganized attachments (McIntosh, 2011a, 2011b, emphasis added). In social science research, the differences between the groups or the correlations between the factors either are or are not statistically significant. Given this, people can be led astray when researchers report that their study found “*evidence of more* problematic behaviours . . . and a *trend* with respect to the rare overnights group”



when there were no statistically significant differences between these groups (McIntosh, Smyth, Kelaher, & Wells, 2011, emphasis added). Likewise, it is fertile ground for woozles when researchers state that several studies “*show some evidence of* increased insecurity among very young children who have frequent overnights” (Tornello et al., 2013, p. 883, emphasis added) when the studies they have cited either found no statistically significant differences or had no measures of insecurity at all.

### COMPELLING ANECDOTES, ANALOGIES, AND CREDENTIALS

How the research is presented can also contribute to the eventual woozling of the data. For example, when the findings are presented along with anecdotal stories, emotionally laden photographs, or case studies, we are more likely to remember, to repeat, and to believe them. These techniques are beneficial when used to make a presentation more entertaining. However, anecdotal stories or case studies can contribute to woozling if they are exaggerations or anomalies that might incline people to adopt a view that is not supported by the empirical data (Best, 2001). By arousing people’s emotions, these techniques increase the odds that the data will be more widely disseminated and, in too many cases, more widely woozled (Kahneman, 2011; Stanovich, 2003).

Several of these emotionally charged approaches can be found in presentations of the research on shared parenting and overnighting—approaches that might contribute to people’s receptivity to woozled data. For example, at a national conference a speaker who was making an argument against shared parenting (Emery, 2012) compared these children to the baby who was almost chopped in half in the biblical story about King Solomon. On his slide entitled “Solomon’s Sword” were the Bible passages from the King Solomon story. The analogy, of course, is that those separated parents who want to continue living with their children at least a 35% of the time are “sacrificing” them to meet their own selfish needs. Metaphorically, these unloving parents are willing to “chop the children in half,” just like the selfish mother who stole another woman’s newborn and was willing to let King Solomon slice the baby in half rather than returning it to its real mother. The message is far from subtle: Unless both parents agree to share, if you love your children, you will not ask for—or go to court for—a shared parenting plan where your children will be “split in half” by living in two homes.

Woozles might also gain support when social scientists tell anecdotal stories or present dramatic case studies without presenting any empirical data. For instance, in its national newsletter, the AFCC published a story by McIntosh (2010, p. 6) whose own overnighting study had been widely interpreted as a warning against overnighting (Nielsen, 2014c). According to

McIntosh's story, she had missed her flight and found herself in a New York City train station where a "distraught" teenage mother asked to use her cell phone to call her mother for help. The teenager was trying to get her baby back from its father, a "smooth talker with drug friends who can hurt her." When the baby was only 8 weeks old, a judge had allegedly ordered that the child spend alternating weeks with each parent. "Between sobs" the teenager confided in McIntosh: "I moved up here from the south so his dad could see him more. The court said I got to." So now the teenage mother was living alone in New York, working and going to school, and taking three trains to transport the baby for the week-long stays with the father. After buying the teenage mother a cup of coffee and making sure she got on her train, McIntosh "started dreaming about a family court system . . . with rulings that prioritized adequate care-giving experiences for babies." Although emotionally engaging, such anecdotal stories might inadvertently incline people to be more receptive to ideas that are not grounded in the research—in this case, to the idea that most judges are inclined to grant 50/50 physical custody even when the mother is a teenager and the baby is only 8 weeks old.

As cognitive psychologists have also demonstrated, we are more likely to be woozled by data that are presented by a confident or a well-known person (Chabris & Simons, 2010) or by a person who has important sounding titles and prestigious affiliations (Johnston, 2007). For example, when people refer to themselves as professors or fellows or list university affiliations next to their names, others are likely to assume that they hold a full-time faculty position or have been awarded a prestigious research fellowship. In some cases, however, this is an exaggeration of the actual status or nature of their affiliations. Inflating their credentials in these ways decreases the chances that other people will challenge their ideas or question their research—which, in turn, makes it easier for woozles to arise.

#### GOING BEYOND LIMITED DATA TO SUGGEST POLICY OR OFFER RECOMMENDATIONS

Another aspect of woozling is making policy recommendations or issuing guidelines for the general public by relying on only a few studies and ignoring the vast body of research relevant to the issue. For example, McIntosh (2011c), in the section of her paper, "Implications for Parents, Practice and Policy," recommended that "In early infancy overnight stays are contra-indicated, undertaken when necessary or helpful to the primary caregiver and when the second parent is already an established source of comfort and security for the infant" (p. 4). At the time, there were only four overnighting studies, three of which had not attributed any negative outcomes to overnighting (Maccoby & Mnookin, 1992; Pruett, Ebling

& Insabella, 2004; Solomon & George, 1999) and one (McIntosh et al., 2010) that provided only limited evidence of negative outcomes for the overnighting children. More important, there were more than four decades of research directly relevant to the topic of overnighting that McIntosh's policy statement ignored. That body of research "provides a growing and sophisticated fund of knowledge about the needs of young children, the circumstances that best promote their optimal development and the individual difference among children regarding their adaptability to different circumstances, stress and change" (Warshak, 2014, p. 46).

Since 2010 the Australian overnighting study, to the exclusion of the findings from the other overnighting studies, has often been cited in support of the position that overnighting is harmful to very young children (Nielsen, 2014c). For example, the Australian Attorney General cited the study as part of the "strong evidence base" for his proposed amendment to revoke the 2006 Family Law Act—a law that was more favorable toward shared parenting (Jackman, 2010). Likewise, a paper by McIntosh (2011b) was the only one cited as the "background paper" for the Australian Association for Infant Mental Health (2011) guidelines: "Prior to the age of two years, overnight time away from the primary caregiver should be avoided, unless necessary" (p. 1). On the basis of the Australian study, eight British professors and directors of eight British organizations wrote a letter to the Minister of Education requesting that proposed changes to the custody laws not be enacted (Hamilton, 2012). More recently still, the Chair of the Child Psychiatry Department at St. Etienne's Children's Hospital wrote an article in France's largest newspaper stating that he and 4,800 other professionals had signed a petition opposing any court-ordered shared parenting when children were under the age of six, citing only the Australian overnighting study as their evidence (Berger, 2014). For more examples of the worldwide impact of this one study see Nielsen (2014c).

In regard to the potential for woozling, the point is that policy recommendations and organizations' guidelines should not be issued on the basis of only a few studies—and certainly not on the basis of any one study.

## HOW TO DISCOURAGE WOOZLING

One of the most effective ways to prevent data from being woozled or to counteract woozles is to encourage scholarly debate. In a scholarly environment, debates, critiques, and differences of opinion are encouraged and welcomed—and willingly shared with the media when the issues involve matters of public concern. Social scientists contribute to this scholarly approach by welcoming critiques of their work and by encouraging expressions of differences. Conversely they discourage this academic or

“anti-woozling” environment by chastising those who disagree with them for creating dissent or for being troublemakers and by reacting to critiques and debates as if they were personal insults or personal rebukes. For example, referring to the fact that 32 scholars were unable to reach any consensus on certain issues related to shared parenting, the co-editors of the *FCR* journal affirmed that “Despite our preference for cooperative dispute resolution, we embrace conflict in the development of public policy. . . . Honest, direct, respectful disagreement moves us forward, not backward” (Emery & Schepard, 2014, p. 143). Or as Kuhn (1962) explained, for science to advance, some individuals have to be bolder than their colleagues in challenging the existing views.

So how do social scientists either encourage or discourage woozling by the way they react to criticisms of their work? One of the least sophisticated and most obvious tactics is to attack the people who are criticizing your work, rather than address their arguments—a defensive reaction known as making an *ad hominem* (Latin for “to the man”) argument. For example, you might accuse other scholars of being part of some secret conspiracy or a tangled web deliberately working to sully your reputation (Park, 2000). Or you might imply that your critics have ulterior motives for criticizing your work—for example, accusing them of being fathers’ rights advocates (Johnston, 2007). You can also make personally insulting, belittling comments about your perceived “enemies.” Similarly, you can conjure up a “straw man” argument—caricaturing and exaggerating your opponent’s position. An even more aggressive approach is to attack the integrity of other scholars, accusing them of being impassioned, biased zealots with hidden agendas and nefarious motives. More combative still, you might hire a lawyer and literally threaten to sue journalists, editors, authors, or organizations who have published articles that are critical of your work.

In this defensive and deprecating context, words like *advocate* and *impassioned* are meant to insinuate that people who express a different view from one’s own are so biased and so blinded by their emotions that they deliberately misreport data to buttress their preconceived beliefs or positions. Their intense emotions override their ability or their willingness to think rationally or objectively about the topic at hand. In contrast, when these words are not being used to silence or to belittle others, an advocate is merely someone who upholds, supports, backs, proposes, or endorses a particular position. In accusing others of being biased, impassioned advocates, the underlying presumption, of course, is that one’s own work and opinions are objective and bias-free. In short, others are woozling the data, but you yourself are not. Sociologist Joel Best (2001, 2008, 2013) has written extensively about the fact that all researchers have certain biases. Cognitive psychologists have also repeatedly documented that our preexisting beliefs

and our experiences influence our thinking processes, rendering all of us “biased” in that sense (Chabris & Simons, 2010).

One illustration of reactions that might unintentionally discourage a scholarly and welcomed exchange of ideas are the responses of the Australian researchers (McIntosh et al., 2010) to scholars who have criticized their study or have pointed out how their data have been woozled. In a keynote address at a national conference, McIntosh (2014a) began by making disparaging remarks about two of the scholars who had critiqued her study (Nielsen, 2014c; Warshak, 2014). McIntosh (2014a) told the audience that Nielsen and Warshak were waging a “war” against her: “Sadly the use of the word war is no exaggeration. . . . For good measure, let’s add a few more adjectives: dull, unnecessary, divisive and retrograde” (p. 1). McIntosh went on to disclose that another social scientist who shared her low opinion of the Warshak paper had written a letter of complaint to the editor of the journal that had published the paper and to the American Psychological Association, which publishes the journal: “Professor Robert Emery describes the Warshak piece as ‘shoddy scholarship’ . . . ‘undeserving of time and attention’” (p. 2). If what McIntosh reported about Emery’s conduct was correct, it can be contrasted with what he had written previously as the coeditor of *FCR*: “Honest, direct, respectful disagreement moves us forward, not backward” (Emery & Shepard, 2014, p. 143). McIntosh then posted her keynote speech on her Web site ([www.familytransitions.com.au](http://www.familytransitions.com.au)). The Warshak paper that McIntosh and, according to her, Emery had so harshly denounced was endorsed by 110 prominent, international experts who agreed that the available research supports the recommendation that regular and frequent overnights for infants and toddlers need not be postponed—a recommendation that ran counter to both McIntosh’s and Emery’s opinions and counter to the way they had interpreted data from their own overnighting studies (McIntosh et al., 2010; Tornello et al., 2013). While telling the audience that the Warshak paper had an “attacking tone” and provided “an unbalanced literature review” (p. 2), McIntosh described her own work as providing “a thorough review” (p. 5) of the research (McIntosh, 2014a)—reiterating her previous statements that she, unlike other scholars including the 110 who endorsed Warshak’s paper, provides a “clear and balanced view” of the overnighting studies (McIntosh, 2014b). The statements in the keynote address echoed her earlier comments in a major Australian newspaper article, in which she was quoted as accusing Warshak and Nielsen of being “impassioned advocates who have sought to discredit me . . . to further political agendas” (Arndt, 2014).

These reactions to the Nielsen and Warshak critiques were reminiscent of the way the Australian researchers had castigated two other scholars who had written a negative critique of their study (Cashmore & Parkinson, 2011). In their rebuttal paper Smyth, McIntosh, and Kelaher (2011) reprimanded Cashmore and Parkinson for giving “intense scrutiny” and “highly critical treatment” (p. 269) to their study, for making a “*deliberate a priori* analytic

decision” (p. 265, emphasis added) and for “casting a shadow of doubt” over their study’s “value” (p. 269). In response, Parkinson and Cashmore (2011) pointed out that only a small portion of their article had been devoted to the McIntosh et al. study and characterized the reactions as “ruffled feathers.” Similarly, after Lamb had written about the weaknesses of their study, McIntosh’s response (2012c) might be construed, correctly or incorrectly, as her implying that Lamb had ulterior motives: “*For reasons unclear*, Lamb’s account contains many and significant factual errors which may mislead the reader” (p. 499, emphasis added). These reactions to other scholars seem out of sync with their espoused belief that “It is fundamental to the scientific method and to the development of evidence based policy and practice that ideas emanating from all studies are scrutinized and subjected to robust debate” (McIntosh et al., 2015, p. 112). Nor do these reactions to their study’s critics square with McIntosh’s responses when her own work as guest editor of a controversial issue of *FCR* was criticized for being biased and unbalanced. In that situation, she praised the journal for its “willingness to uphold the legitimacy of academic debate and to source different points of view on complex issues” (McIntosh, 2012c, p. 212).

In sum, woozles tend to scurry back into their holes when scholars’ debates and critiques of one another’s work are forceful, but tactful; blunt, but not belittling; critical, but not insulting; and—in situations where the same woozled data keep doggedly resurfacing year after year—relentless, but not malicious.

### CATCHING THE WOZZLES: ETHICAL RESPONSIBILITIES

“A lie will go around the world, while the truth is still pulling its boots on,” Mark Twain supposedly wrote in the early 1900s (Shapiro, 2006). In 1710, satirist Jonathan Swift expressed the same thought: “Falsehood flies, and the truth comes limping after it” (Shapiro, 2006). By whatever means data from certain studies become distorted into woozles, social scientists are ethically obligated to try to correct the misinformation and to do so as quickly and as diligently as possible, regardless of whether the data came from their own studies or from someone else’s.

Professional organizations have made it abundantly clear that social scientists are obliged to try to prevent their data from being misunderstood or misused—in short, to prevent woozling. For example, the Australian Psychological Society (2010) puts this responsibility squarely on the researchers’ shoulders: “Psychologists take reasonable steps to correct *any* misrepresentation made by them *or about them* in their professional capacity *within a reasonable time* after becoming aware of the misrepresentation. Statements made by psychologists in announcing or advertising the availability of psychological services, products or publications must not

contain *any* statement which is false, fraudulent, *misleading* or deceptive or *likely to mislead or deceive*" (p. 26). Likewise, the American Psychological Association (2013) states that "forensic practitioners do not, either by commission or omission, participate in misrepresentation of their evidence nor participate in partisan attempts to *avoid, deny or subvert* the presentation of evidence contrary to their own position or opinion" (p. 16, emphasis added).

Social scientists do not all respond in similar ways, of course, when data from their study or from others' studies are being woozled. For example, in response to two papers that had documented the extensive woozling and misuse of their study over a period of years (Nielsen, 2014c; Warshak, 2014), McIntosh et al. (2015) still felt that the "evidence" that their study had any impact whatsoever "is not strong" (p. 116). They also felt that the "suggestion" that their study had ever been linked with efforts to discourage overnights was nothing more than a "notion" (p. 114). Further they stated that "if" their study had any negative impact on fathers' overnight time with their children since 2010, a newspaper article written by Arndt (2014) that discussed the Warshak (2014) consensus paper might be responsible: "Media coverage of the Warshak article in Australia may have contributed to the disenfranchisement of fathers seeking shared-time arrangements with statements echoing the Warshak misrepresentation of our study" (McIntosh et al., 2015, p. 112). After the Warshak and Nielsen articles were published in 2014, followed by Arndt's (2014) newspaper article, McIntosh coauthored an academic article (McIntosh et al., 2014) and posted a statement on her Web site stating (McIntosh, 2014d) that their study was never meant as a warning against "all" overnighting—only against "frequent" overnighting (which in their study meant any more than three overnights a month for babies). Further these Australian researchers stated, "While researchers have limited—if *any*—ability to control who uses their research and how their research is reported by others, our research team has exercised due diligence in informing others when we have felt that statements made by them misrepresent or overstate the findings of our research" (McIntosh et al., 2015, p. 116, emphasis added). Metaphorically, they believed they had done a commendable job trying to catch the woozles emanating from their study.

Other scholars have responded quickly and assertively as soon as they become aware that data are being woozled and misused to make recommendations related to child custody issues. One example is Professor Patrick Parkinson at Sydney University's School of Law (Parkinson, 2012). In 2011 the British Parliament received a report from the Norgrove Committee that recommended against revising custody laws to be more favorable toward shared parenting (Norgrove, 2011). The Norgrove Committee had relied largely on two Australian studies (Kaspiew et al., 2009; McIntosh et al., 2010). Within a year of the Norgrove report's release, Parkinson had published a paper in which he meticulously documented the many errors in the Norgrove paper—errors that could easily have become woozles if Parkinson

had remained silent or had waited several years to write the paper. Two of the wozzles that he swiftly and assertively dismantled were that, as a result of the 2006 Custody Reforms in Australia, the rates of litigation in court had increased and more judges were making shared care orders in cases where there was a history of violence in the family. As Parkinson wrote, “It is at this point that I really start to hear warning bells ringing. The picture painted in the report states that the presumption has led to upholding of father’s rights over children’s needs and mothers being unable to disclose violence and abuse. This is disingenuous to say the least and to my mind, illuminates the agenda behind the research itself” (Parkinson, 2012, p. 5). In the end, the British Parliament was not swayed by the Norgrove report. Parkinson had succeeded in catching the wozzles.

Another example of being a responsible “wozzle catcher” is provided by Marsha Pruett, who had conducted one of the overnighting studies (Pruett et al., 2004). Referring to the fact that other researchers had been wozzling their study by claiming that it reached similar conclusions to the studies by McIntosh et al. (2010) and Solomon and George (1999), Pruett confronted the wozzle in *FCR*: “Comparisons of these studies have led to distorted conclusions that result from faulty assumptions made that these studies look at similar outcome measures in similar ways, which they do not. . . . Responsible scholarship acknowledges and elaborates on these differences so that they are clearly articulated” (Pruett & DiFonzo, 2014, p. 165). Pruett also presented a workshop at a national conference on the topic of misrepresenting the research on child custody and parenting plans. Robert Emery, second author of the Tornello overnighting study (Tornello et al., 2013), presented the workshop with her (Emery & Pruett, 2015). Neither of them made any mention of how their own studies had been wozzled and misused as evidence against overnighting.

The most impressive example of scholars shouldering the responsibility of trying to “catch” wozzles related to overnighting is the Warshak (2014) consensus paper endorsed by 110 international scholars. These scholars were trying to corral two wozzles that had reemerged in recent years: first, that overnighting should be delayed until children are older than four and, second, that babies should not spend more than three nights a month in their father’s care. These scholars debunked these two wozzles with a large body of empirical research on infants’ attachments to their parents, the impact of babies being separated from their mothers in day care centers, the importance of fathers in the earliest years of childhood, and the findings of the overnighting studies. Their consensus was that there was no reason to postpone frequent and regular overnights for infants and toddlers. In terms of scholars working together to reach a consensus by debunking many of the wozzles that had arisen in regard to overnighting, this paper exemplifies consensus building and “wozzle catching” at its best.



## CONCLUSION

In closing, several points are worth reiterating. First, woozling the data from research studies occurs in all fields of science, not just in social science and not just in those studies related to overnighting for infants and toddlers whose parents have separated. Second, no single person, no one event, or no one study can be held responsible for the creation or the promotion of a woozle. A constellation of factors, including the media and advocacy groups, carry the woozle along its path. Third, although some social scientists or journalists contribute more than others to the woozling of data from particular studies or on particular topics, their behavior is not necessarily intentional or self-serving. Some people who contribute to woozling the data are as naive and unaware as Winnie the Pooh who duped himself into believing he was being followed by the dangerous woozle. Finally, social scientists and family court professionals whose work involves child custody issues should be on the alert for woozles by becoming familiar with the various woozling techniques that too often lead us astray. More important still, we should be persistent in chasing the child custody woozles around the mulberry bush until we catch them—or at the very least, until we force them to scurry back into their holes. To do otherwise is to do a grave disservice to the millions of children whose lives are affected by child custody decisions and parenting plans that are based on distorted, inaccurate, woozled data.

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