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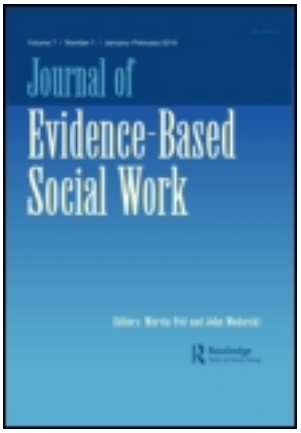

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Establishing CASA as an Evidence-Based Practice

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In this article the authors examine the evidentiary status of the Court Appointed Special Advocates (CASA) program through a review of current research findings and a critical analysis of the study methodologies used to produce those findings. Due to the equivocal research findings and widespread methodological weaknesses (most notably selection bias) in the literature base, it is determined that there is not currently enough evidence to establish CASA as an evidence-based practice. In spite of the challenges to the feasibility of such research, a future research agenda is suggested that calls for the execution of large randomized controlled trials in order to produce findings that will inform a deeper understanding of CASA effectiveness in improving child outcomes.

Keywords: CASA, Court Appointed Special Advocates, child welfare, foster care

Court Appointed Special Advocates (CASA) is a national network of nonprofit organizations which train and support community volunteers to serve as child advocates representing the best interests of abused and neglected children in the dependency court system. CASA volunteers are appointed by judges to serve as advocates who represent children independent of the court, the child welfare agency, and family members. CASA volunteers typically serve one child at a time, and they most often represent that child for the life of the case. Since 1977 when CASA was established in a single jurisdiction in Seattle, Washington, it has expanded to a network that includes 75,000 volunteers in 49 states, serving an estimated 240,000 children in 2010, according to the National CASA Association (2011).

There are different models of CASA involvement in court cases, with the primary distinction being the role of the CASA volunteer in court. In some jurisdictions, CASA volunteers act *as* the legally-mandated guardian ad litem (GAL) for the child, while in other court systems CASA volunteers are appointed as a "friend of the court" *in addition* to an attorney who serves as the GAL (Litzelfelner, 2000; Piraino, 1999). The model of involvement utilized is determined by individual states or localities; some states have legislative mandates that GALs must be attorneys, while other states have more general requirements allowing for the option of either a volunteer GAL or an attorney GAL (Piraino, 1999). The National CASA Association reports that, as of 2010, 59% of CASA programs were operating with a friend of the court model, 34% with a GAL model, and 7% with a team model combining both (NCASAA, 2010).

Regardless of the model used, there are several unique benefits that may be associated with the appointment of a CASA volunteer in a child's case. One such benefit is stability of representation. It is widely acknowledged that children in the child welfare system frequently experience turnover

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among the professionals with whom they interact. In one study of 25 children with CASA volunteers, 90% of cases enjoyed stability with their CASA volunteer while their child welfare social worker changed one or more times (Clark, 1988). Similarly, Brennan, Wilson, George, and McLaughlin (2010) found that among a large sample of children receiving CASA services, 68% had only one CASA volunteer while a mere 10% had only one social worker during their case. Another benefit of CASA is the independence of the volunteers. Charged with the task of bringing relevant information on the children's interests and circumstances to judicial officials, CASA volunteers typically function as advocates who provide recommendations independent of judges, attorneys, social workers, therapists, family members, or others who might be involved with cases (Litzelfelner & Petr, 1997; Piraino, 1999; Weisz & Thai, 2003). Finally, and at least equally importantly, CASA volunteers have very low "caseloads," usually representing only one child at a time (NCASAA, 2011). In contrast to professionals such as attorneys and child welfare workers, who frequently have large caseloads, CASA volunteers have the time to develop meaningful relationships with the children they represent, and therefore can bring a breadth and depth of information to the court that may not otherwise be available for consideration in decision making (Litzelfelner & Petr, 1997; Piraino, 1999; Weisz & Thai, 2003).

CASA is highly regarded among child welfare professionals and in the judiciary (Berliner & Fitzgerald, 1998; Collins-Camargo, Lemon, Ojha, Osborne, and Pieratt, 2005; Leung, 1996; Litzelfelner, 2008; ORS, 2005; Weisz & Thai, 2003). In one study (Collins-Camargo et al., 2005), the vast majority (88%) of judicial officers described CASA as helpful in securing additional services for children, and well over three-quarters indicated that they felt CASA volunteers have a positive impact on children's outcomes. Another survey of over 500 judges showed that the majority of judges use the input that CASA provides in their decision making, and find that CASA is useful and effective in monitoring cases and considering the best interests of children (ORS, 2005). This finding is echoed in other studies that have conducted survey research, with judges and other child welfare professionals reporting overall high satisfaction and regard for the performance of CASA volunteers in advocating for children (Berliner & Fitzgerald, 1998; Leung, 1996; Litzelfelner, 2008; Weisz & Thai, 2003).

The esteem with which CASA is held in the legal system appears to be grounded, at least in part, in the high-quality service volunteers provide in representing children. In studies directly comparing the performance and activities of CASA volunteers and attorneys, researchers have concluded that CASA volunteers perform at least as well as paid attorneys in representing the best interests of children in court (Condelli, 1988; Duquette & Ramsay, 1986; Poertner & Press, 1990; Weisz & Thai, 2003; Youngclarke, Dyer Ramos, & Granger-Merkle, 2004). In the earliest study looking at the relative performance of volunteer advocates and attorneys, Duquette and Ramsey (1986) found that lay volunteers (similar to CASA) spent more time on cases than paid attorneys with no specialized child advocacy training, and that there were no significant differences between the lay volunteers and paid attorneys on a host of measures related to legal activities (such as court processing time, placement orders, visitation orders, treatment orders, pleas, ward of court orders, and dismissals). Weisz and Thai (2003) conducted survey research indicating that CASA volunteers were significantly more likely than attorneys to have visited their child at home prior to court hearings, and to have investigated alternative services for the child or family, resulting in more detailed information being brought to court hearings. In a law review article on the efficacy of CASA volunteers versus attorney models of representation, Peterson (2005) states, "Of these several different models, the one that stands out above the rest is that of the Court Appointed Special Advocate. The CASA model ... has been consistently evaluated as the most effective at advocating the best interests of the child" (p. 4).

While these studies suggest that CASAs and attorneys may be comparable on performance-based metrics, there remains a larger question about CASA's effectiveness as an intervention for improving child outcomes. Looking past the positive regard that CASA enjoys among profession-

als, as well as the performance-related successes that have been documented, there is the issue of outcome effectiveness. Do children who are represented by CASA have better outcomes than those who are not? Since the ultimate goal of the child welfare legal system is to ensure safety, permanency, and well-being, it is reasonable to ask whether the benefits of CASA intervention extend to producing better outcomes. In social work, as in many other disciplines that deal with human outcomes, there has been a sea change in recent years toward the use of evidence-based practice (EBP) in selecting interventions and making practice decisions. In resource-strapped social service systems, the movement toward using rigorous evidence to assess intervention outcomes is both necessary and desirable for assuring maximum benefit to recipients. In light of the emphasis on EBP, an important question is raised: Is CASA an EBP?

To begin to answer this question, it is necessary to define what constitutes an EBP. Gambrill (1999) defines EBP as a process that uses scientific evidence in conjunction with practice knowledge and client values as the basis of knowledge for decision making. She argues that social work interventions are often guided by practices that are based on "opinions of others, pronouncements of 'authorities,' unchecked intuition, anecdotal experience, and popularity (the authority of the crowd)" (Gambrill, 1999, p. 348). By contrast, "In EBP a sharp distinction is made between claims that rely on authority and consensus and those that have survived critical tests of their accuracy" (p. 346). Using an EBP framework, these "critical tests" refer to systematic appraisals of research regarding practice questions, with clinical trials as the gold standard in generating evidence of effectiveness. In other words, it is not enough for practitioners to believe that an intervention is effective because it is popular, anecdotally successful, or intuitively sound—there must be some demonstration of effectiveness that involves rigorous external evidence.

In this light, it is clear that the popularity of CASA among judicial professionals and the child welfare community is not alone sufficient to endorse CASA as an EBP for improving child outcomes. While its popularity is a good sign of a potentially promising practice—one could reasonably argue that CASA would not be so widely admired by those in child welfare circles if there were not much support for its perceived effectiveness—determining evidentiary status requires a critical evaluation of the available research literature in order to assess the strength of our current knowledge base, taking into account both the findings of the research and the limitations of the research designs used to obtain those findings. To this end, in this article we examine the current, somewhat limited body of research literature on CASA in order to summarize findings regarding its effectiveness. Subsequently, the methodology of the research literature is critically examined to draw conclusions about the strength and validity of the findings. Finally, these factors are weighed to assess whether CASA can be viewed as an EBP, with suggestions for future research.

REVIEW OF THE LITERATURE

There is a relatively small body of research studies that use treatment and comparison groups to answer the broad question "Does CASA work?" In this article we look at published studies that used treatment and comparison groups to examine indicators of CASA efficacy. Three broad categories of comparisons are examined in the literature to look at differences between children who receive CASA services and children who do not. The first category of group comparisons are simply measures of case characteristics; most studies look at some characteristics of the children assigned and not assigned a CASA in order to determine the degree of similarity or difference between groups. The second two categories of variables examine the differential outcomes of children in CASA and comparison groups; these categories are measures of process-related outcomes, and measures of child outcomes. Research findings are examined within each of these categories of variables.

Case Characteristics

Most studies have some component examining differences between samples of children receiving CASA services (treatment groups) and those not receiving CASA services (comparison groups) in terms of demographic and descriptive characteristics. These characteristics are used to assess whether treatment and comparison groups are equivalent on selected observables. However, there is wide variation in the number and types of variables used to assess comparability of groups. Some look only at basic demographics such as age and gender, while others have more expansive comparison variables and may assess for differences in a wider range of characteristics. The results of these comparisons indicate that there are substantial differences in case characteristics between treatment and comparison groups, particularly on indicators of case complexity and severity. These differences exist on the child, family, and case levels.

On the case level, one prominent finding is that children receiving CASA services typically have more extensive prior child welfare involvement than other children in court custody, including more prior referrals, substantiations, previous child welfare services, and prior court involvement (Abramson, 1991; Caliber Associates, 2004; Siegel et al., 2001). This can be viewed as an indicator that CASA cases involve more chronic or recurrent maltreatment than non-CASA cases. Another observed case difference between groups is severity and type of maltreatment. Caliber Associates (2004) found that children receiving CASA were more likely to have experienced abandonment, educational maltreatment, or exploitation than children not receiving CASA. The CASA children in the same study were also more likely to have been assessed as having experienced a severe level of harm and have more risk factors associated with their case (Caliber Associates, 2004). Other researchers have also found that there were different types of abuse experienced by the treatment and comparison groups. Siegel et al. (2001) found that CASA cases were more likely to involve physical abuse, while Poertner and Press (1990) found much higher rates of sexual abuse among their CASA sample. Litzelfelner (2000) found that CASA cases were more likely to involve neglect or a combination of abuse and neglect than abuse alone. Waxman, Houston, Profilet, and Sanchez (2009) not only found higher levels of risk, sexual abuse, and neglect among their CASA group, but also that CASA children had experienced more types of maltreatment than those in the non-CASA sample. While these findings do not show that CASA children are consistently experiencing one type of maltreatment more often than non-CASA children, they do indicate that in all of these studies, the treatment and comparison groups were not comparable because of different maltreatment experiences that led them into care. Information obtained from judges also affirms that CASA services are frequently assigned to cases that are more difficult than typical cases. In a national survey, judges indicated that they are more likely to make a CASA referral when there are placement issues, concerns about conflicting case information or implementation of services, or maltreatment factors such as extreme neglect or physical or sexual abuse (ORS, 2005).

Aside from characteristics related to the nature of the maltreatment, some studies have also looked at family characteristics and found significant differences between CASA and non-CASA groups. Siegel et al. (2001) and Litzelfelner (2000) both found that there was more parental substance abuse among families of children in CASA. Siegel et al. (2001), who compared groups on a wide range of characteristics, found that families of CASA children had more mental health issues, housing, and financial problems. Also relating to family characteristics, Poertner and Press (1990) and Siegel et al. (2001) both found that CASA children had more siblings in care than children not receiving CASA.

The differences on individual child characteristics are less clear, though distinctions have been found in several studies. Regarding age, Mensing (2008) reports that in California, children being served by CASA are older than the average age of children in care, however Poertner and Press (1990) and Siegel et al. (2001) found that the children in their CASA samples were younger on

average than the children in the comparison samples of children without CASA. These differences may reflect variations in state and local practices more than variability in study approaches. Regarding race/ethnicity, Abramson's (1991) CASA sample (assigned randomly to the treatment condition) had significantly more Hispanic/Latino children and fewer White children than the control group. In contrast, the children with a CASA volunteer in the Caliber Associates (2004) study were *less* likely to be Hispanic/Latino than children without a CASA. Waxman et al. (2009) reported more White and biracial children in the CASA group than the comparison group, which had more African American and Hispanic/Latino children, but they did not report any statistical testing for significance on these differences. There were no differences in age or race found in studies by Calkins & Millar (1999), Leung (1996), or Poertner and Press (1990), nor in the study by Litzelfelner (2000), who used age and race as matching variables in the sampling process.

In all, the research literature soundly shows that CASA cases generally appear to be more difficult than non-CASA cases, with only three studies reporting no differences between treatment and comparison groups on examined variables (Calkins & Millar, 1999; Duquette & Ramsay, 1984; Leung, 1996). This strongly suggests that children served by CASA are distinctive from children otherwise served by the child welfare system, with particular indications that their cases may be more complicated, more severe, or otherwise present special challenges. The distinctive nature of CASA cases suggests the potential for significant selection bias. This problem of selection bias, which results in non-comparable treatment and comparison groups (and which will be discussed at greater length in the subsequent section on methodological problems), presents one of the major barriers to determining CASA effectiveness, as systematic differences between groups make it difficult or impossible to determine whether outcome differences are due to the intervention or to pre-existing differences between groups.

Process Variables

Process variables are those that focus on measurable outputs that occur during the course of a legal case, including those related to child placements, case duration, and service provision. In other words, these are the outputs that occur *during* the case, prior to the ultimate permanency-related outcomes. These are vital to examine because they directly relate to the quality of a child's experience while in the system.

One such variable that is frequently examined in the literature is the number of services that are ordered for children and their families. This is one of the areas in which the findings across studies are quite clear: In aggregate, children (and their families) who are assigned to a CASA volunteer receive more services than children who are not assigned to a CASA (Caliber Associates, 2004; Condelli, 1988; Litzelfelner, 2000; Poertner & Press, 1990; Seigel et al., 2001). Only one study (Snyder, Downing, & Jacobson, 1996) did not find that CASA children received more services (they found no difference in number of services between CASA and non-CASA children) but that study still found that the parents of the CASA children received more services. In sum, the finding that CASA children receive more services is one of the strongest in the literature, however because of the issue of selection bias, the higher numbers of services observed among CASA cases could be attributable to existing group differences rather than an effect of CASA intervention. In other words, it is possible that CASA cases get more services simply because they are more serious cases that warrant more services regardless of CASA involvement.

Another commonly examined process variable relates to case duration. This variable is operationalized in different ways—such as days that a child is in protective custody, total length of time that the court has oversight of the case, length of time in out-of-home care—yet all are concerned with the central concept of whether CASA is associated with a reduced length of involvement for children in the dependency court system. Case duration is one of the variables for which there are truly equivocal findings in the literature; some studies find no difference in

case duration, others find that CASA cases are open longer, and some find that CASA cases close more quickly. In four studies researchers looking at this question did not find significant differences between CASA and non-CASA groups regarding duration of court cases or children's length of time in the system (Caliber, 2004; Litzelfelner, 2000; Poertner & Press, 1990; Siegel et al., 2001). Of those studies, three (Caliber, 2004; Litzelfelner, 2000; Poertner & Press, 1990) found that the raw average duration of CASA cases was slightly longer than non-CASA cases, and one (Siegel, 2001) found that the CASA cases were very slightly shorter in duration, but none of these differences were found to be statistically significant. In contrast, Calkins and Millar (1999) found that among those cases that achieved permanency during the study, children with CASA had substantially shorter stays in care, with CASA children averaging 8 fewer months in care than non-CASA children. Supporting this finding, two other studies (Powell & Speshock, 1996, as cited in Youngclarke et al., 2004; McRoy & Smith, 1998) also found shorter case durations associated with their CASA samples. However, two studies (Condelli, 1998; Smith, 1993, as cited in Youngclarke et al., 2004) found the opposite—that length of time in the system was longer for CASA children.

Such equivocal findings can be hard to evaluate for conclusions, and different conclusions have indeed been reached on this variable. On one hand, in the only systematic review of CASA research to date, the authors concluded that while the across-study averages showed longer mean durations for CASA cases, the lack of statistical significance and the number of contradictory findings made the evidence so ambivalent that they concluded "no difference" was ascertainable on case duration (Youngclarke et al., 2004). However, in the U.S. Inspector General's audit of CASA, one of the main outcome findings that they report is that length of time in care is longer for CASA children than non-CASA children (OIG, 2006). They base their finding on their assessment that, despite the lack of statistical significance, the raw means in the existing literature and in their own research support the conclusion that stays in care are longer for CASA children than both non-CASA study samples and the national average for all foster children (OIG, 2006), although again, it is uncertain whether this is due to the fact that CASA cases are more generally more complicated in the aggregate. Another source of potential selection bias in regard to case duration is that some studies (Caliber Associates, 2004; Calkins & Millar, 1999; Siegel at al., 2004) only looked at case duration for an exit cohort, excluding cases that were still open and had not yet reached a permanency outcome. This introduces potential bias in the findings by excluding the lengthiest cases and thereby adds to the difficulty of drawing solid conclusions from the research on this question

Most existing CASA studies also include variables that examine the nature of child placements, either in number (number of total placements or placement changes while in care) or type (e.g., foster homes, kinship homes, institutional homes), as indicators of safety and stability for dependent children. This is another question where findings are ambiguous. Calkins & Millar (1999) and Litzelfelner (2000) both found that children in the CASA samples had significantly fewer placements than children in the comparison groups. Waxman et al. (2009) found that children in the first year of their 3-year study had fewer placements, but this difference did not last through subsequent years.

Brennan et al. (2010), Caliber Associates (2004), Leung (1996), Poertner and Press (1990), and Siegel et al. (2001) found no statistically significant differences in number of placements between CASA and non-CASA samples. In the study by Brennan et al. (2010), which evaluated CASA in Washington State, there were some regional differences seen in number of placements, but no differences between CASA and no-CASA groups in the statewide aggregate. In their systematic review of CASA research, Youngclarke et al. (2004) report that only two studies have found a higher average number of placements for CASA children compared to non-CASA children (McRoy, 1998; Smith, 1993), but these were both assessed as having poor research designs with limited conclusive strength.

Regarding placement type, the Caliber Associates (2004) report found that among all cases (open and closed at the end of the study), children in the CASA group were more likely to be in out-of-home placements than those in the non-CASA group; among only those cases still open at the end of the study, CASA children were less likely to be in kinship placements and more likely to be in non-relative placements. According to a study conducted by the Judicial Council of California, children assigned a CASA volunteer were more likely to live in group homes or other institutional settings compared to children not assigned a CASA volunteer (Mensing, 2008), but these differences again may reflect state and local policies relating to CASA selection than to outcomes of CASA cases. Conversely, Litzelfelner (2000) found that CASA children were *less* likely to be in institutional placements, but only at the 24-month data collection period (the difference in placement types was not seen at previous points in the study).

On balance, though the findings in this area are contradictory, one might tentatively conclude that there may be fewer placements associated with children who receive CASA services. While some studies have found no differences between CASA and non-CASA groups, others have found fewer placements associated with CASA, and the only studies to find that CASA children had more placements are of questionable quality. Both Youngclarke et al. (2004) and the Office of the Inspector General (2006) have concluded that the available evidence suggests that CASA children experience fewer placements. A caveat is that none of the studies examining placement stability used length of time in care as a denominator in calculating number of placements or placement changes. A good deal of evidence suggests that the longer children remain in care, the greater the likelihood they will experience placement instability (James, 2004), yet these issues do not appear to have been taken into account.

Child Outcomes

Perhaps the most salient outcome that researchers hope to assess in relation to CASA is permanency. Accordingly, nearly all research on CASA effectiveness includes an examination of permanency plans (for cases still open at the end of the study) and/or permanency outcomes (for closed cases), with findings suggesting that CASA cases may be more likely to involve adoption as an ultimate permanency outcome. Both Abramson (1991) and Poertner and Press (1990) found that CASA assignment was associated with a significantly higher likelihood of a case ending in adoption. In the study by Poertner and Press (1990), the authors suggest that the difference was substantial: 21.7% of CASA cases ($n = 13$) ended in adoption, compared to 7.1% of non-CASA cases ($n = 7$), but this conclusion is based on a very small subset of cases, raising questions about the generalizability and certainty of the findings. In Abramson's (1991) study, CASA children were more likely to have been adopted and less likely to have been reunified with their parents than control group children among cases that had been dismissed by the close of the study. Findings relating to permanency in the Abramson study, however, should be viewed with considerable caution. Only 77 children out of a very small total sample ($n = 122$) achieved case closure by the end of the study, and of these, only 5 cases resulted in adoption, a number so small that any conclusions should be viewed tentatively.

For cases still open at the end of the study, Abramson found that children in the CASA condition were significantly more likely to have a permanency plan of reunification and significantly less likely to have a plan of long-term foster care than children in the control condition. In contrast to Abramson's study, Caliber Associates (2004) found the opposite among permanency plans for still-open cases; CASA children in that study were *less* likely to have reunification as the case plan. No significant differences were found in permanency plans or outcomes in studies by Litzenfelner (2000) and Siegel et al. (2001).

A few studies have looked at differences in maltreatment recurrence and reentry into care between CASA and non-CASA samples. While there have been lower observed rates of recidivism

and reentry among the CASA samples, none of the studies looking at this issue have found differences that were statistically significant, which makes it difficult to conclude whether the trends are merely due to random variation in the data (Abramson, 1991; Caliber Associates, 2004, Poertner & Press, 1990). Abramson (1991) found that for the subset of cases that had been dismissed by the end of the study period, the comparison group had more subsequent referrals ($n = 8$) than the CASA group ($n = 6$), but the difference was not statistically significant. In looking at the proportion of total new referrals to referrals that resulted in a new court petition for each group, the difference approached but did not attain statistical significance in showing that CASA re-referrals were less likely to result in court involvement. Similarly, Caliber Associates (2004) found that among those cases still open at the end of the study, the CASA group was less likely to have had a subsequent referral, but the difference was not significant. There was no substantial difference in new referrals among closed cases. Poertner and Press (1990) reported that in their study, CASA cases had a reentry rate (into court dependency) of 6.7%, compared to 12.2% for the comparison cases, but this was also not statistically significant.

Finally, it is worth noting that in considering child outcomes, only one study has looked at child outcomes not related to permanency, placements, or court processes. Waxman et al. (2009) used standardized scales and qualitative child and caregiver interviews to longitudinally examine a broader range of well-being outcomes including protective factors, family functioning, self-esteem, and school functioning. They found that CASA children had more protective factors and some indicators of better family functioning (although, importantly, it is unclear whether this refers to birth families or foster families). They also found that children in the CASA group had fewer school problems and better school performance, but not at a statistically significant level. Further research is needed for any clear picture of what influence CASA might have in improving non-court-related outcomes.

LIMITATIONS OF CURRENT RESEARCH

The reviewed literature provides some tentative indications of differential outputs and outcomes for children who receive CASA and children who do not. While there are many equivocal findings, several seem less ambiguous at first glance, including that children in CASA samples receive more services, may experience fewer placements while in care, and may be more likely to reach adoption as a final outcome than children in comparison samples. However, even in those variables that produce clearer findings, there are major methodological limitations that must be considered against the results of the research, and which may inhibit the ability to draw any valid conclusions at all about CASA effectiveness from the current literature base.

As has been demonstrated and discussed, selection bias is clearly the major limitation hindering conclusive understanding of CASA effectiveness based on current studies. Briefly stated, CASA cases are more severe and complex, differing systematically from cases that are not referred to CASA. When selection bias exists and results in non-comparable samples, it is impossible to deduce whether differences in outcomes are due to CASA intervention or existing differences between groups. While the selection bias issue is discussed in many studies and reviews of CASA research, findings that are questionable due to this bias are repeated in subsequent studies without proper acknowledgement of the way in which they undermine the ability to assert conclusions about effectiveness. In this way, the literature base seems to affirm the legitimacy of findings that may not be legitimate at all.

Of studies finding that the treatment and control groups were not comparable, some used statistical controls (Caliber Associates, 2004; Litzelfelner, 2000) or exclusion of certain cases from the analysis (Poertner & Press, 1990; Siegel et al., 2001) to account for the systematic differences between groups, while others either did not use any such methods or did not specify

(Abramson, 1991; Waxman et al., 2009). However, even where statistical control methods are used, this still does not eliminate the selection bias problem, as researchers cannot ensure that they are controlling for all variables that might differ. These methods can only control for *known* bias, not for hidden bias that is present as the result of unmeasured variables. Due to the pervasiveness of selection bias in the literature base, all findings (positive and negative) are called into question and cannot be relied upon to give meaningful information about CASA's effectiveness. In addition, some researchers make the salient point that selection bias likely masks positive effects of CASA intervention, since it appears that CASA children are more at risk and face more challenges than their non-CASA counterparts (Caliber Associates, 2004; Youngclarke et al., 2004). Thus, it seems reasonable to believe that designing research that eliminates selection bias might produce evidence of positive impact among children receiving CASA.

Small sample size is also a potential problem among current studies. Of the studies examined in this review, only the Brennan et al. (2010) and Caliber Associates (2004) studies have a large total sample size ($n = 3013$ and 2831, respectively). Other studies range in their total study sample sizes (treatment and control combined) from a low of 43 (Weisz & Thai, 2003) to a high of 483 (Siegel et al., 2001). Many studies have questionable sample sizes sufficient for detecting true differences between groups, in particular when considering the large standard deviations typical to many child welfare outcome measures. Only Litzelfelner (2000) directly addresses the sample size limitation, noting that a power analysis indicated that a much larger sample than was available would have been needed to adequately detect differences in her study. Further, when statistical control methods are used to account for between group differences, small sample sizes become even less effective at accurately detecting differences in outcome variables.

The central strategy for eliminating selection bias is utilizing a randomized controlled experimental design. To date, Abramson (1991) has conducted the only randomized controlled trial on this subject, and that study has design flaws that inhibit the validity of the findings. In this trial, 28 families were randomly assigned to the treatment condition of receiving an Amicus (comparable to CASA) volunteer, and 28 families were assigned to the control group receiving conventional child welfare services. A few issues are worthy of consideration. First, the total sample size is relatively small (56 families, 122 children), and there is no discussion of a power analysis in the report. Because of the small total study sample size, there are several subgroup analyses (including those examining permanency outcomes and re-referrals) that use statistical tests with insufficient numbers of subjects to yield valid findings. Second, even with the randomized design, it is reported that there were statistically significant differences between groups that made them non-comparable; a larger sample size might have allowed for the randomization process to smooth out group differences. Third, the samples were created through assignment by family unit, but the findings are reported on individual children, violating assumptions of data independence in the research design. Finally, the author reports that the Amicus program coordinator was made available to the families in the control condition as a "resource person" (p. 480), but no information is provided on what, if any, services or resources these families received, which raises concerns about possible contamination of the control group through treatment diffusion.

Finally, some discussion of the Youngclarke et al. systematic review of CASA studies is warranted. While they used inclusion and exclusion criteria to examine all comparison studies on CASA (up to the date of the review), there are some issues with the conclusions drawn based on their assessment of the studies reviewed. Specifically, Youngclarke et al. assert some strong conclusions that are questionably warranted based on the findings of the studies they reviewed. They state, "[T]here is just a small body of available literature with generally poor methodological quality" (p. 121), yet they go on to draw conclusions about CASA effectiveness that do not seem supported by this literature. One example is their assessment that reduced recidivism/re-entry is "the most profound finding" of their review. Only three studies were evaluated on this outcome (Abramson, 1991; Poertner & Press, 1990; Powell & Speshock, 1996), and two of them

(Abramson, 1991; Poertner & Press, 1990) had findings that were not statistically significant. The third study (Powell & Speshock, 1996) is not published and could not be located for primary review in this article, nonetheless even if it had positive significant findings on reduced recidivism, it is unclear how Youngclarke et al. (2004) concluded that the finding on reentry was "consistent" and "large" (p. 121) when two of the three cited studies had no statistically significant results on this measure. This example illustrates the point that though a systematic review has produced findings asserting evidence of CASA effectiveness in producing positive outcomes, we feel that these conclusions are not fully supported by the literature and should be viewed with caution.

Some of the persistent methodological problems of the reviewed studies are summarized in Table 1. Examining the design flaws in the research makes clear the fundamental problem with the body of research literature on CASA effectiveness: Randomized controlled trials using large, representative samples of children are needed to deal with the methodological weaknesses endemic to studies on this topic. Where does this leave the state of our evidence about CASA effectiveness? Unfortunately, the equivocal and contradictory findings coupled with the widespread selection bias, small sample sizes, and other methodological problems in the research literature strongly suggest that it is not possible to currently draw valid conclusions about the effectiveness of CASA as an intervention for improving child outcomes. This does not mean that CASA is not valuable, or that it does not *actually* improve outcomes. On the contrary, there are many indications that CASA may be a viable alternative to attorney representation, as CASA volunteers and attorneys appear to perform similarly in the court system. In addition, just because we cannot currently draw meaningful conclusions from the extant evidence regarding CASA effectiveness, this does not mean that CASA is ineffective, only that we have not yet produced the research required to answer the effectiveness question adequately.

CONCLUSION: WHAT IS NEEDED TO ESTABLISH CASA AS AN EBP?

This assessment of the current research indicates that some findings may be more promising than others when it comes to child outcomes. Our tentative assessment in this regard is drawn from the relative consistency with which these findings are reported, rather than from our confidence in the study methodologies. Some of these findings are that CASA children and their families receive more services, that CASA may be associated with a reduced number of placements while in care, and that CASA cases may be more likely to end in adoption. However, even these findings are sorely impaired by the limitations of the study methodologies, particularly in regard to selection bias. On the whole, it seems clear that CASA cannot currently be considered an EBP. Given that this is the case, the question becomes: What is needed to establish the evidentiary status of CASA as a child welfare intervention?

As discussed above, research utilizing randomized controlled designs would be the gold standard in producing evidence from which causal conclusions could be drawn. While random assignment is the antidote to many validity threats in research, it is especially needed in this realm because of the selection bias that appears to exist in the assignment of children to CASA programs. Because children referred to CASA have been demonstrated to differ systematically and significantly from children not referred to CASA, any findings that are based on comparison of non-random samples from these two groups will not produce useful results, as it will always be impossible to determine whether differences in outcomes are due to the intervention or to pre-existing group differences. Provided that sample size is adequate, random assignment to treatment and control conditions eliminates selection bias, as participant characteristics are randomly distributed over conditions.

Previous studies have used matching and statistical control methods to attempt to overcome the selection bias problem, however these methods are not adequate. While it makes intuitive sense to

TABLE 1
Summary of Selected Research on CASA Effectiveness*

Study	*Design*	*Sample Sizes*	*Matched Samples? If YES, Variables Used for Matching*	*Variables Used to Compare Treatment and Comparison Groups Differences Between Groups?*	*If Samples not Comparable, Were Statistical Controls Used?*	*Major Findings*	*Limitations to Causal Inference*
Abramson (1991)	Randomized controlled trial	CASA = 28 families (60 children) No CASA = 28 families (62 children)	No	Race/ethnicity, age, gender, abuse type, previous CPS referrals. Groups differed on previous CPS involvement and race/ethnicity.	Not specified	Adoption more likely, reunification less likely, for CASA children whose cases closed during study. Reunification more likely, long term foster care less likely as permanency plans for CASA children with open cases.	Subjects assigned to condition by family, but results reported on individuals. Non-comparable groups. Small sample sizes (no power analysis). Threat of treatment diffusion.
Brennan et al. (2010)	Retrospective case comparison	CASA = 1431 No CASA = 1582	No	Differences between treatment and comparison groups not examined.	N/A	Among reunified cases, children with no representation spent less time in care than those with CASA or mixed representation. Among cases ending in adoption, children with mixed representation spent more time in care than those with CASA or an attorney GAL. Among adoption cases, CASA cases took an average of 5 months less to finalize, but this was not statistically significant. Children with no representatives were more likely to have their cases still open at the end of the study period than those with a CASA volunteer, and those with a CASA volunteer more likely to still be open than those with CASA staff or mixed representation.	No assessment of comparability of treatment and control groups. Wide variation in outcomes when statewide statistics are broken down into smaller regions.
Caliber Associates (2004)	Retrospective case comparison	For the case comparison portion of the study: CASA = 429 children No CASA = 2398	No	Age, race/ethnicity, gender, abuse type, history of maltreatment, risk and protective factors. Groups differed on race, abuse type, level of harm, severity of risk.	Yes	More services for children and families among CASA cases. CASA children more likely to be in out-of-home placements among open and closed cases. Among open cases of those ever in out-of-home care, CASA kids less likely to be reunified or in kin care.	Selection bias appears to persist despite utilization of statistical control methods (p. 48).

(*continued*)

TABLE 1
(*Continued*)

Study	*Design*	*Sample Sizes*	*Matched Samples? If YES, Variables Used for Matching*	*Variables Used to Compare Treatment and Comparison Groups Differences Between Groups?*	*If Samples not Comparable, Were Statistical Controls Used?*	*Major Findings*	*Limitations to Causal Inference*
Calkins & Millar (1999)	Retrospective case comparison	CASA = 68 No CASA = 121	No	Gender, ethnicity, case severity. No significant group differences on these variables.	N/A	Among cases achieving permanency, CASA children spent less time in care.	Treatment and comparison groups were compared on limited variables. Fairly small CASA sample.
Duquette & Ramsay (1986)	Prospective case comparison	Three "treatment" groups with representatives getting specialized training in child advocacy = 53 children total: Law students = 16 Lay volunteers = 22 Attorneys = 15 Comparison group of children with attorneys with no specialized advocacy training = 38	No	Race/ethnicity, sex, number of children in family, case severity. No significant group differences on these variables.	N/A	No differences in legal performance or outcomes between three treatment groups. Combined, the three treatment groups spent more time on cases than the comparison group.	Small sample sizes. Cases receiving lay volunteers (similar to CASA) were combined with other non-lay volunteer groups for comparison.
Leung (1996)	Prospective longitudinal case comparison	CASA = 66 children No CASA = 131	No	Number of children in case, disabilities among parents/children, financial/housing stability, prior relinquishments, child deaths in family, age of caregiver, type of maltreatment, severity of maltreatment, maltreatment history, caregiver substance use. No significant group differences on these variables.	N/A	"[N]one of the placement comparisons show statistical significance" (p. 277). Despite lack of statistically significant results, the author asserts (based on differences in raw averages) that the data "tend to support the belief" that CASA reduces length of time in out-of-home placements and number of placements, and that CASA intervention is most effective prior to case disposition.	No statistically significant results to support conclusions. Fairly small sample of CASA children.
Litzelfelner (2000)	Prospective case comparison	CASA = 119 No CASA = 81	Yes. Samples matched on age, race, maltreatment type, and time of entry. When a matched comparison case was not available at time of a CASA case entry, the treatment case entered study without a corresponding matched comparison case.	Age, race, gender, maltreatment type, number of siblings in care, single-parent home status. Group differences were found on maltreatment type and number of siblings in care even after matching.	Yes	CASA children had fewer placements. After 24 months, CASA children more likely to be placed in reunification, kinship, or adoption, and less likely to be in institutional placements. Among open cases, children in CASA received more services.	Selection bias may still be affecting results despite statistical controls. Power analysis indicated much larger sample size would have been needed to accurately detect differences between groups.

(*continued*)

TABLE 1
(*Continued*)

Study	*Design*	*Sample Sizes*	*Matched Samples? If YES, Variables Used for Matching*	*Variables Used to Compare Treatment and Comparison Groups Differences Between Groups?*	*If Samples not Comparable, Were Statistical Controls Used?*	*Major Findings*	*Limitations to Causal Inference*
Poertner & Press (1990)	Retrospective case comparison	CASA = 61 No CASA = 148	No	Age of child, age of mother, number of children in family, number of children in court jurisdiction, sex of child, type of maltreatment, number of maltreatment type, relationship of child to abuser. Group differences were found on type of abuse and age. Certain cases were excluded to adjust for these substantial differences, but smaller differences still found in number of children in family and under court jurisdiction.	Yes	More services for CASA children and families. CASA cases significantly more likely to end in adoption.	Small sample size for CASA cases.
Siegel et al. (2001)	Prospective case comparison	Full sample: CASA = 253 No CASA = 230 For outcome analysis subset: CASA = 139 No CASA = 143	Yes, for the outcome analysis portion of the study. Samples were matched on age, gender, number of siblings on petition, maltreatment type, and placement status.	Age of child, petition allegations, prior child welfare and court history, placement status, and child and family problems. Group differences were found on abuse type, court and child welfare history, substance abuse and mental health issues in the family, housing/financial problems, and incarceration of parents.	Not specified	More services for CASA children and families. No statistically significant differences on any outcome or process measures.	Selection bias due to persistent differences in case complexity despite matched pair sampling.

(*continued*)

TABLE 1
(*Continued*)

Study	*Design*	*Sample Sizes*	*Matched Samples? If YES, Variables Used for Matching*	*Variables Used to Compare Treatment and Comparison Groups Differences Between Groups?*	*If Samples not Comparable, Were Statistical Controls Used?*	*Major Findings*	*Limitations to Causal Inference*
Waxman et al. (2009)	Prospective longitudinal case comparison	CASA = 327 No CASA = 254	Unclear. The report states: "These [comparison group] children were matched with treatment children first by gender, then by age (within two years) and finally by type of abuse." (p. 31) However in another section, it states that comparison children were sampled randomly from the full population in a specific time period, and it is unclear why there are different numbers of treatment and comparison children and why the groups varied by race and abuse type if matched pair sampling was used.	Race/ethnicity, maltreatment type. Group differences on type and severity of maltreatment.	Not specified	CASA group had more protective factors and some indicators of better family functioning. Fewer placements for CASA children in early years of the study, but not lasting through end of study period. Indicators of better school performance for CASA children, but not statistically significant.	Selection bias. Attrition over the course of the longitudinal study period.
Weisz & Thai (2003)	Case comparison of survey data	CASA = 21 No CASA = 22	No	Groups were only compared on one variable—number of siblings involved in the case. CASA cases had a slightly smaller mean number of siblings.	Not specified	Judges reported higher rate of receiving court reports from CASAs compared to GALs, and rated CASA reports as more helpful. CASA volunteers performed more activities to prepare for hearings.	Low response rate from GALs. Comparability of groups unknown. Very small sample sizes. All data was subjective survey data; no objective indicators.

*Selection criteria: This table only includes studies that: (a) used treatment *and* comparison groups to examine indicators of CASA efficacy, and (b) were available for primary review by the authors. Unpublished studies that could not be obtained by the authors, non-comparison survey data, and purely descriptive reports were among those excluded.

match children on variables such as gender, ethnicity, and case type or severity, these methods do not capture the unobservable variables that uniquely characterize CASA children. That is, while they capture some of the necessary dimensions associated with children referred and not-referred for CASA, they likely do not capture the range of subtle and complex characteristics that typically make children eligible for CASA services.

Unfortunately, using randomized designs for CASA research is an aspiration that is not easily realized. In the real-world functioning of dependency courts, child welfare services, and CASA advocacy, there tend to be ethical qualms about the random denial of services that would be required for a randomized design (Heuertz, 1996). To be certain, denial of CASA services already occurs in CASA agencies. CASA is a service for which demand routinely far outstrips supply. Nationally, CASA volunteers serve less than half of children in foster care (NCASAA, 2011). In California, only about 10% of children are assigned a CASA volunteer (L. Collier, California Administrative Office of the Courts, personal communication, December 2010). As such, there are typically children on waitlists for CASA services, many of whom will never be served. Despite the fact that even under routine circumstances services will be denied to some, judges and other child welfare and CASA personnel appear averse to denying services randomly, rather than allotting limited resources according to some characteristics of the applicants. Judges and CASA agencies are likely to prefer to prioritize limited services to those who are perceived as the neediest or the most likely to benefit from having a volunteer. However, using such prioritization methods introduces bias in a sampling design because it results in non-random differences between treatment and comparison groups that preclude causal inference, as is the situation in the current research base.

It is apparent that a randomized experimental design would provide the best evidence for evaluating the effectiveness of CASA programs, and it is hoped that a random design might be feasible for a future research agenda. There are some reasons to think that this could be the case. In any program where demand exceeds supply, there will be individuals who do not receive services, resulting in "treatment" being denied to some. This fact could assuage ethical concerns in agencies/communities where there is a major imbalance of supply and demand. In addition, there are unfortunate realities about CASA funding that may help sway some judges toward approving randomized trials. In any environment, CASA agencies struggle to maintain adequate funding, as evidenced by the limited capacity that results in service waitlists. In the current economic climate, CASA agencies are fighting all the harder for limited funding. At the same time, funders are increasingly calling for demonstration that programs are evidence-based. Currently, there is no external evidentiary proof that CASA is an effective intervention program. A sophisticated, randomized research design has the potential to produce valuable information about CASA's effectiveness, which could be convincing to some judges in positions to approve such a design. Further, there is precedent for a randomized design. The study by Abramson (1991) utilized random assignment to condition, and was undertaken with the approval of a juvenile court in Fresno, California. While that particular study has aforementioned impediments to good evidentiary conclusions, it establishes that there is a precedent of a dependency court agreeing to a randomized design for research purposes.

Despite these arguments for approval of randomized designs to study CASA effectiveness, it is possible that judges will still be averse to such methods, and that randomized designs really might not be possible. In this case, researchers will have to look for a "next best" design that allows for greater causal inference. One such possibility is using the waitlist typical to CASA agencies as a naturally-occurring control group. The benefit of such a design is that both the children receiving services (the treatment group) and the children on the waitlist due to lack of service capacity (the control group) would both have been referred to CASA by a judge, so both groups would presumably have the same aggregate (and often intangible) qualities that make children likely to be referred. Such a design would still have significant problems, however, if children

are prioritized for services based on systematic child/case characteristics (e.g., severity of need) rather than taken on a first-come, first-served basis, again introducing considerable selection bias.

The third-tier possibility for achieving better research data would be using a sophisticated quasi-experimental matching design, such as propensity score matching. Such a design would assign children a composite score reflecting their probability of having been referred to CASA; this score would be based on a multitude of observable factors going well beyond the basics of age, race, gender, and case severity that do not seem to capture the subtleties of difference between children who are referred to CASA and those who are not. Children would then be matched in treatment and control groups according to their composite score, resulting in a greater likelihood of between-group comparability than has previously been seen. While this design would improve upon prior research, it too has drawbacks and limitations. The design would require an extremely large sample size (probably more than could ever be feasible in a CASA study) to be effective (Peikes, Moreno, & Orzol, 2008) and it is still vulnerable to the hidden bias of unmeasured observables that is problematic for matching designs in general (Peikes et al., 2008; Shadish, Cook, & Campbell, 2002).

Researchers hoping to establish CASA as an EBP have a clear road forward. The clear, unquestionably best research agenda is to find ways to make large, randomized experimental designs feasible. Randomized designs are likely the only way to truly establish causal conclusions about the effectiveness of CASA in improving child outcomes in the dependency system, and such causal evidence is vital for producing the external evidence needed to make the determination about CASA as an evidence-based practice. Without more sophisticated research designs, the outlook on the efficacy of CASA as an intervention targeting child outcomes is likely to remain in question.

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