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**Juvenile Transfer and the Specific Deterrence Hypothesis:
Systematic Review and Meta-Analysis**

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Research Summary

We conducted a systematic review of recidivism outcomes for juveniles transferred to adult court, incorporating meta-analytic techniques. Nine studies—based on nine statistically independent samples—met the inclusion criteria. Pooled analysis suggests that juvenile transfer had no statistically significant effect on recidivism. However, the distribution of effect sizes was highly heterogeneous and, given the strength of the research designs, suggests that in some instances transfer may decrease recidivism and in others may increase it.

Policy Implications

The practice of transferring juvenile offenders to the criminal justice system has decreased from its peak in the mid-1990s, but it is still estimated to affect tens of thousands of juveniles in the United States each year. As such, a coherent rationale for transfer policy is needed. The present review casts doubt on one prominent justification for transfer, that it creates a specific deterrent effect for transferred juveniles. Indeed, the results suggest that transfer may in fact increase offending. More generally, the results underscore the need for more high-quality research to identify the conditions under which transfer may decrease or increase recidivism.

Keywords

juvenile transfer, specific deterrence, recidivism, systematic review, meta-analysis

The juvenile court was established in 1899 in Cook County, Illinois, borne out of the Progressive Movement and its emphasis on treating juvenile offenders outside of the punitive criminal justice system (Tanenhaus, 2004). Central to the court's mission was the assumption that juvenile offenders are not fully responsible for their conduct and can be rehabilitated and reformed into law-abiding adults. This rehabilitative focus involves a case-by-case informal approach to deciding how to intervene with each juvenile, and it stands in contrast to the formalism and retributive focus in criminal justice (Farrington, Loeber, and Howell, 2012; Feld, 2003).

Historically, the juvenile court has been guided by the assumption that it has “the knowledge and insight to make thoughtful, individualized judgments that will keep [society] safe and promote positive development for adolescents” (Mulvey et al., 2004: 214). Since its inception, however, the court has held that some juvenile offenders should be handled in adult court because of their dangerousness to other juveniles or to the public (Kupchik, 2006; Zimring, 2000). Accordingly, there have always been mechanisms for transferring the most serious juvenile offenders to the criminal justice system. The historical rationale for the practice of transfer is twofold. First, it serves as a “punitive necessity” for offenders who cannot be sufficiently punished in juvenile court (Zimring, 2000: 208). Second, it provides a mechanism for removing offenders who are “beyond rehabilitation” and whose presence in the juvenile system would interfere with the rehabilitative mission of the court (Mears, 2003: 160).

Expansion of Transfer Policy

In the 1980s and 1990s, states enacted a wide range of reforms aimed at “toughening” the juvenile justice system. Rising juvenile crime in the 1980s and 1990s created a moral panic that engendered a punitive response, with most states enacting legislation to make their juvenile justice systems more punitive during that time (Zimring and Rushin, 2013). One prominent reform consisted of new and expanded transfer laws, which provided courts with the ability to send a broader swath of the juvenile offender population to adult court. The tenor of the broader tough-on-crime reforms indicated a shift in the rationale for transfer that was more consistent with a punitive approach to juvenile crime and not centered on protecting the ability of the juvenile court to serve youth better (Kupchik, 2006; Kurlychek and Johnson, 2010). In addition to this retributive emphasis, transfer laws were justified on the grounds that they would have a specific deterrent and general deterrent effect (Howell, 2003; Mears, 2003). As Bishop (2006: 653) noted, a common view of the “get tough” movement was that harsher punishments, including transfer, would “be more effective in reducing crime.” Indeed, in recent years, some politicians have heralded the decrease in juvenile crime since the 1990s as evidence that the more punitive juvenile sanctions “worked” (Zimring and Rushin, 2013).

Increased use of transfer was central to the punitive turn and illuminated a more general trend toward “criminalizing” the juvenile court (Feld, 1999). The decision to transfer juveniles originally resided solely with the judge in what is now referred to as “judicial waiver.” New variants of transfer emerged in the

1990s, however, placing greater discretion or authority to transfer youth in the hands of prosecutors and legislators (Fagan, 2008; Zimring, 2010). For example, new laws enabled prosecutors to file certain cases directly in adult court or mandated that youth who commit certain crimes automatically must be processed as adults.

Prosecutorial direct-file and statutory exclusion statutes constitute a shift in power that, in turn, has transformed waiver from a decision guided by consideration of the “best interests” of youth and the juvenile justice system—the overarching mission of the juvenile court (Mears, 2003)—into one guided by the interests of the executive and legislative branches of state governments (Feld, 2000; Zimring, 2000). These interests can include a diverse range of sanctioning approaches and emphases, but in the 1980s and 1990s, they entailed a focus on tougher punishment. This shift occurred alongside of the expansion of scenarios under which prosecutors could request that judges permit transfer to occur. Although traditional judicial waiver represents a discretionary mechanism by which to send serious, violent, or chronic juvenile offenders to adult court, newer forms of transfer emphasize the offense rather than individualized consideration of the offender (Steiner, Hemmens, and Bell, 2006). Some scholars have argued that this shift—including the automatic transfer of youth for commission of certain types of crimes—likely results in more first-time offenders being waived to criminal court than what may occur through judicial waiver (Rainville, 2008; Verrecchia, 2011).

Although almost all states still allow for discretionary judicial waiver (45), prosecutorial direct-file (15) and statutory exclusion (29) are now common (Griffin, Addie, Adams, and Firestine, 2011). The Office of Juvenile Justice and Delinquency Prevention (OJJDP) estimates that approximately 4,000 juvenile defendants were judicially waived to adult court in 2013, representing approximately 4 in 1,000 referred delinquency cases nationwide (Furdella and Puzzanchera, 2015). Although judicial waiver has been steadily decreasing from its peak of more than 13,000 waived cases in 1994 (Hockenberry and Puzzanchera, 2015: 38), this trend underestimates the full impact of transfer. Unfortunately, the number of juveniles transferred to adult court by other mechanisms is unknown because only data on judicial waiver are collected nationally (Griffin et al., 2011). Some have estimated that during the 1990s, as many as 200,000 juveniles were transferred to the adult justice system for trial, sentencing, or incarceration every year (Woolard, Odgers, Lanza-Kaduce, and Dagleis, 2005). In fact, increased use of these other forms of transfer may be partially responsible for the decrease in judicial waiver since the 1990s (Hockenberry and Puzzanchera, 2015). It is unclear whether the total number of transferred juveniles has decreased in line with the decreased use of judicial waiver, however.

Even though precise estimates in total transfers do not exist because states do not track the frequency with which various types of transfer occur (Mears, 2003), it is reasonable to suggest that most transfers today do not involve judicial waiver. For example, the dataset “Juvenile Defendants in Criminal Courts

(JDCC): Survey of 40 Counties in the United States, 1998,” housed at the Inter-University Consortium for Political and Social Research (ICPSR), includes a sample of juvenile defendants in criminal court from 40 large urban counties ($N = 7,135$). Only 23.7% of transferred juveniles in the sample had arrived in criminal court by judicial waiver. Moreover, the six states that *do* track all nonjudicial juvenile transfers—Arizona, California, Florida, Michigan, Oregon, and Washington—reported 5,096 nonjudicial transfers in 2007 compared with the 8,500 judicial transfers reported across all 50 states in the same year (Griffin et al., 2011). Such statistics suggest that many more juveniles arrive in criminal court through prosecutorial direct-file and statutory exclusion mechanisms than through traditional judicial waiver (Fagan, 2008; Griffin et al., 2011).

Impact of Transfer on Future Offending

Given the continued use of juvenile transfer, it is important to assess whether transfer reduces crime. Two major research questions can be posed. The first concerns a general deterrent effect of transfer: Does transferring more juvenile offenders lead to decreased offending in the juvenile population? Research on this topic has been limited, and a systematic review including three high-quality studies (through 2003) on general deterrence found inconsistent results (McGowan et al., 2007). More recent studies have found that stricter juvenile transfer policies (specifically, statutory exclusion) do not seem to contribute to significant reductions in juvenile crime (Steiner et al., 2006; Steiner and Wright, 2006; Zimring and Rushin, 2013). Although empirical evidence for general

deterrent effects is inconclusive, there are theoretical reasons to doubt that the increased risk of transfer would motivate serious juvenile offenders to refrain from delinquent behavior. For example, adolescents are less future oriented than adults and, thus, less likely to consider the consequences of their behavior (Caffman and Steinberg, 2012; Scott and Steinberg, 2008). In addition, would-be juvenile offenders would need to be aware of the existence of transfer laws, their likelihood of use, and the probability of being sent to prison if transferred to adult court (Mears, 2003).

The second research question concerns specific deterrence: Does transferring juvenile offenders to the criminal justice system decrease the future offending of those transferred juveniles? Or, more precisely, does it do so sufficiently to offset potential criminogenic effects that may be associated with transfer (Nagin, Cullen, and Jonson, 2009)? The argument for a potential specific deterrent effect of transfer is straightforward—youth who are transferred are assumed to receive more severe sanctions, which in turn deter them from future offending. This logic ignores other forms of deterrence that may be more effective in reducing crime, such as the certainty or celerity of punishment (Nagin, 2013: 205–207), but nonetheless, it provides a central foundation on which transfer has been anticipated to be effective (Mears, 2003; Redding, 2010). Although researchers have given greater attention to examining the specific deterrent effects of transfer than to its potential general deterrent effects, few studies have employed strong research designs. To date, one systematic review has been undertaken (McGowan et al., 2007). Based on six studies that had been published

as of 2003, McGowan and colleagues (2007) concluded that transfer had no specific deterrent effect, leading the authors to recommend the end of juvenile transfer as a means of reducing violence.

The review by McGowan et al. (2007) provided insight into the potential specific deterrent effect of transfer, but it did not systematically take into account the nature of the research designs. It did not, for example, incorporate meta-analytic techniques. In part, that was because only six studies existed at that point. Since then, several additional studies have emerged that, when coupled with an assessment using meta-analysis, create an opportunity to provide an updated and more rigorous evaluation of whether juvenile transfer reduces recidivism.

Methods

One challenge in assessing the specific deterrent effect of transfer is the possibility of selection bias, which presents a serious threat to internal validity (Shadish, Cook, and Campbell, 2002). The concern is that transferred and nontransferred juveniles are systematically different in their likelihood of reoffending. This difference makes it difficult to assess the causal impact of transfer on recidivism (Jordan and Myers, 2011; Myers, 2003). As Loughran and colleagues (2010: 477) observed:

A comparison of offenders who do and do not get transferred to adult court . . . involves a contrast of two groups that are inherently different in important, preexisting ways which may affect any comparison of the groups' patterns of re-offending. Consequently, observed differences (e.g.,

higher recidivism rates) in the transferred population cannot be accurately attributed to the transfer experience itself, as long as these differences in outcomes might also be partially or fully attributable to fundamental differences between the transferred and retained youth.

Failing to control for differences between transferred and nontransferred juveniles may bias estimates of the effect of transfer on reoffending (Mears, 2003; Nagin et al., 2009). It may even lead us to confuse cause and effect: If waived juveniles are sent to adult court because of their higher risk, the increased likelihood of reoffending might be better described as a cause, rather than as an effect, of transfer.

Accordingly, credible estimates of the impact of transfer on recidivism require strong research designs that adjust for potential confounding. Systematic reviews provide one strategy for locating, appraising, and synthesizing evidence from prior studies in ways that incorporate information about the strength of research design. They have explicit objectives and criteria for including or excluding studies, entail extensive searches for eligible studies, require careful extraction and coding of key features of studies, and involve a structured set of analyses and a detailed report of the methods and conclusions of the review. In so doing, they provide a stronger foundation than that of narrative reviews for identifying the conclusions that emanate from prior research and for quantifying uncertainty in estimated effects (e.g., Cooper, 2010; Lipsey and Wilson, 2001; Petticrew and Roberts, 2006).

With these considerations in mind, then, this article reports on the findings of a systematic review and meta-analysis of the specific deterrent effects of juvenile transfer. In so doing, and in contrast to the prior review undertaken by McGowan et al. (2007), it includes moderator analyses (Lipsey, 2003) to investigate differences across studies in the observed outcomes, including controls for selection bias. The article then discusses the implications of the analyses for future research and policy.

Criteria for Inclusion of Studies

In selecting studies for inclusion in this review, several criteria were employed. First, juvenile transfer—judicial waiver, prosecutorial direct-file, or statutory exclusion—was the primary focus of included studies. Second, the outcome measure for included studies was recidivism, here operationalized using postdispositional rearrests, readjudications, or reconvictions of juvenile offenders. For studies that examined several types of recidivism (e.g., felony, violent, and property), we selected the most general outcome (“all recidivism”) for the main study outcome to be coded. Third, the sample consisted of juvenile offenders, and the comparison was between transferred and nontransferred juveniles. Studies comparing juvenile offenders to similarly aged adult offenders, for example, were not included. Finally, the research design had to be of high quality methodologically, with the minimum design involving a quasi-experimental approach that compared a transfer group (“treatment”) and a nontransfer group (“comparison”).

Search Strategies

To locate studies meeting these criteria, six search strategies were employed. First, searches of electronic bibliographic databases were performed, including Criminal Justice Abstracts, National Criminal Justice Reference Service (NCJRS) Abstracts, ProQuest (all databases), Psychology Information (PsycInfo), Sociological Abstracts, PubMed Central (NBCI), Medline (EBSCO), Google Scholar, and Dissertation Abstracts. These databases were selected because they had the most comprehensive coverage of criminological, social science, and public health literatures. The following terms were used to search these databases: *transfer; waiver; direct file; exclusion; juvenile court; adult court; and criminal court*. When applicable, *recidivism, repeat offending, future offending, re-arrest, and re-offend* were added to narrow the search parameters.

Second, we performed searches of literature reviews on recidivism outcomes for transferred juveniles. The only systematic review to date was by McGowan et al. (2007), with coverage through 2003. The following narrative reviews were identified: Farrington et al. (2012); Redding (2010); Fagan (2008); Howell (2003); Mears (2003); Redding (2003); Bishop and Frazier (2000); and Howell (1996). Third, searches of bibliographies of impact evaluations on juvenile transfer were performed. Fourth, we performed searches of the bibliographies of OJJDP reports on juvenile transfer. Fifth, forward citation searches were performed with Google Scholar to identify any additional research that cited studies meeting our inclusion criteria. Finally, contacts with leading

researchers allowed us to obtain or clarify studies that we collected, as well as to solicit recently published, in-press, or unpublished papers. The searches did not place any restrictions based on publication status or language.

Coding

Multiple variables were coded from the original studies to facilitate both the meta-analysis and the moderator analyses. The key features of the included studies coded for these analyses included author and date; outlet (published or not); location; study period; sample (number of participants, age, offense type); intervention and comparison group sample sizes; type of transfer mechanism; outcome measure(s); and study design and follow-up period.

Meta-Analysis

Meta-analytic techniques were used to determine the size, direction, and statistical significance of the effect of juvenile transfer on future offending. Study effect sizes were weighted on the variance of the effect size and the study sample size (Lipsey and Wilson, 2001). Where multiple studies used the same sample over time, the latest follow-up was used. The main measure of effect size was the odds ratio (OR), which is most appropriate when studies compare two groups (i.e., treatment and control) in terms of “the relative odds of a status or event” (Lipsey and Wilson, 2001: 52).

OR effect sizes could be extracted from all nine studies in one of two ways. For three studies (Loughran et al., 2010; Myers, 2003; Podkopacz, 1996),

logits reported by the authors were used. For the other six studies, ORs and standard errors were calculated based on the data provided. This approach was used to create ORs with similar interpretations across studies and to avoid issues resulting from how different studies used statistical controls for reported effects. Computations were performed with Biostat's Comprehensive Meta Analysis version 2.2.

Results

On the basis of the search strategies, we identified an estimated 600 references related to the research question. Of these, 164 studies were identified as potentially relevant. One hundred forty-three of these studies were excluded because they did not report an impact evaluation or original research findings, did not measure offender recidivism as an outcome, or did not use a comparison group. Full-text screening was performed for the remaining 21 studies to determine whether they met the inclusion criteria. Altogether, nine studies—based on nine statistically independent samples—met these criteria. The other 12 studies were excluded for four main reasons: (1) no comparison group was used ($n = 2$); (2) either the treatment or control group did not consist of juvenile offenders ($n = 2$); (c) recidivism was not the dependent variable ($n = 2$); and (d) the same sample was used in another study that was already included ($n = 6$). Where multiple studies used the same sample, the study with the longest follow-up time to measure recidivism was selected. Where multiple papers used the same analysis (i.e., duplication), the paper with the most descriptive information was used.

Details of the Included Studies

Table 1 summarizes the key features of the nine included studies. All studies took place in the United States, with two samples based in New York and New Jersey, two based in Florida, and two based in Pennsylvania. Type of transfer mechanism varied across these jurisdictions. Four studies included juvenile defendants who were automatically transferred to criminal court via statutory exclusion, whereas two studies examined cases transferred by judicial waiver. In the remaining three studies, multiple mechanisms of transfer were used (Lanza-Kaduce, Lane, Bishop, and Frazier, 2005; Loughran et al. 2010; Winner, Lanza-Kaduce, Bishop, and Frazier, 1997); the two Florida studies included mostly direct-file cases. Two studies used reconviction or readjudication as a measure of recidivism, whereas seven used rearrests. Three studies used felony recidivism as their outcome measure, whereas the other six studies included all (i.e., nonfelony) recidivism. The follow-up period to measure recidivism ranged from a low of 18 months (Myers, 2003) to a high of 8 years (Jordan, 2012). Samples ranged in size from 102 matched pairs (Jordan, 2012) to 2,700 matched pairs (Winner et al., 1997). Although three studies included all petitioned juvenile offenders in a given time period, the other six included only juveniles arrested for serious felonies.

All nine studies attempted to address the threat of selection bias. To this end, the included studies employed quasi-experimental designs of varying methodological rigor. Two studies used nonequivalent comparison groups with statistical controls (Myers, 2003; Podkopacz, 1996), two used conventional

matched-pair designs (Lanza-Kaduce et al., 2005; Winner et al., 1997), two used propensity-score matching designs (Jordan, 2012; Loughran et al., 2010), and three used natural experiment designs (Drake, 2013; Fagan, 1996; Fagan, Kupchik, and Liberman, 2007).

[Table 1 about here]

Meta-Analysis

Figure 1 summarizes the results of the nine included studies in a forest-plot graph. It can be observed that most studies (five) found evidence of increased recidivism for transferred juveniles compared with retained juveniles. Three studies identified no significant difference in recidivism outcomes between transferred and retained juveniles. Only one study identified an effect of transfer consistent with the specific deterrence hypothesis: Transferred juveniles had lower odds of recidivating compared with nontransferred juveniles (Jordan, 2012). In pooling the data from the nine studies, there was evidence that juvenile transfer had a small but nonstatistically significant effect, such that transferred youth had higher odds of recidivating; the weighted mean OR was 1.183 (95% confidence interval [CI]: 0.932, 1.501).

[Figure 1 about here]

The analysis to this point has focused on the effect of transfer on reoffending and, to this end, has used a general recidivism measure because it was available for all nine studies. However, several studies employed additional outcome measures. Accordingly, we ran two additional meta-analyses with these

different measures: felony recidivism only and violent felony recidivism only. Five studies (Drake, 2013; Fagan, 1996; Fagan et al., 2007; Lanza-Kaduce et al., 2005; Winner et al., 1997) provided requisite data for felony recidivism, whereas three studies (Drake, 2013; Fagan et al., 2007; Myers, 2003) provided requisite data for violent felony recidivism.

The results are summarized in Figures 2 and 3. For the meta-analysis of felony recidivism, Figure 2 shows that three studies found evidence that juvenile transfer increased recidivism, one study found that it decreased recidivism (Winner et al., 1997), and another identified no significant difference in recidivism (Fagan et al., 2007). In pooling the data from the five studies, there was evidence that juvenile transfer had a small but nonstatistically significant effect on recidivism; here, the weighted mean OR was 1.276 (95% CI: 0.905, 1.799). For the meta-analysis of violent felony recidivism, Figure 3 shows that two studies found that juvenile transfer increased recidivism and one study identified no effect on recidivism (Myers, 2003). In pooling the data from the three studies, there was evidence that juvenile transfer had a statistically significant but small effect; the weighted mean OR was 1.785 (95% CI: 1.542, 2.066).

[Figures 2 and 3 about here]

If we turn our attention to the general recidivism measure and associated estimated effects, it is apparent that the distribution of the nine effect sizes is highly heterogeneous ($Q = 52.61$, $I^2 = 84.79$, $df = 8$, $p < .001$). Specifically, the null hypothesis that the study effects are homogenous can be rejected. The usual

test statistic, Cochran's Q , represents the summed squared deviations of each study's weighted effect from the weighted mean effect. A significant Q value indicates a significant degree of inconsistency across the studies. As a result of statistical power concerns, the I^2 statistic is often more informative. Here, it indicates that 84.79% of the dispersion in effects across studies results from heterogeneity as opposed to chance (Altman, Deeks, Higgins, and Thompson, 2003). This finding in turn suggests the presence of moderators, which can be either methodologically or substantively related to the studies (Lipsey and Wilson, 2001). In accordance with other meta-analyses, we identified several key potential moderators to explore variability in effects across studies. These moderators included study design, sample size, sample offense type, mechanism of transfer, outcome measure, and follow-up time to measure recidivism outcomes.

All nine studies attempted to address the threat of selection effects but employed designs of varying methodological rigor. These nine studies could be characterized by one of four quasi-experimental designs: nonequivalent comparison group with statistical controls ($n = 2$); conventional matching ($n = 2$); propensity matching ($n = 2$); and natural experiment ($n = 3$). Although randomized experiments have the highest internal validity (Weisburd and Hinkle, 2012), the various quasi-experimental designs employed are intended to achieve higher internal validity compared with nonexperimental designs. Accordingly, they potentially address selection bias more effectively (Nagin and Weisburd, 2013; see also Farrington, 2003).

As shown in Table 2, the conventional matching and natural experiment designs identified nonstatistically significant and highly heterogeneous effects. The other two designs had statistically significant effects and low heterogeneity. For the nonequivalent comparison group with statistical controls, the weighted mean OR was 2.089 (95% CI: 1.270, 3.435), indicating that the odds of recidivism were significantly greater for transferred youth compared with for nontransferred youth. The heterogeneity statistics indicated that effects were not heterogeneous ($Q = 0.143$, $I^2 = 0.000$, $df = 1$, $p = .706$). For the propensity matching design ($n = 2$), the weighted mean OR was 0.690 (95% CI: 0.488, 0.976), indicating that the odds of recidivism were significantly lower for transferred youth compared with nontransferred youth. The heterogeneity statistics indicated that effects were not heterogeneous ($Q = 1.042$, $I^2 = 4.052$, $df = 1$, $p = .306$). Notably, there was also significant heterogeneity *between* the groups ($Q = 13.814$, $df = 3$, $p = .003$), suggesting that study design contributed to variation in the estimated effects across the different studies.

[Table 2 about here]

To explore further the potential influence of study design on the estimated effects of transfer, we conducted sensitivity analyses in which each of the four designs was removed from the meta-analysis (Table 3). Only removing the propensity matching studies resulted in a significant effect (OR = 1.346, 95% CI: 1.036, 1.748). As such, the inclusion of these two studies—Jordan (2012) and Loughran et al. (2010), both of which were not included in the prior systematic review—can be interpreted as altering the overall conclusion on the effects of

juvenile transfer. All four sensitivity analyses remained highly heterogeneous, however, indicating that no one subgroup of studies was responsible for the overall observed heterogeneity among studies.

[Table 3 about here]

Table 4 reports on additional moderator analyses. Studies measuring recidivism as reconviction ($n = 2$) had a significant effect on recidivism outcomes (OR = 1.452, 95% CI: 1.146, 1.841), and studies assessing judicial waiver ($n = 2$) also had a significant effect (OR = 2.090, 95% CI: 1.271, 3.436). The other study characteristics—offense type, sample size, and length of follow-up—did not show any significant relationship to recidivism outcomes. No moderator analysis revealed significant heterogeneity between subgroups, although transfer mechanism came closest to reaching significance ($Q = 5.090$, $df = 2$, $p = .078$).

[Table 4 about here]

Given that all nine included studies were published, there is potential for publication bias to affect the estimated impact of transfer on recidivism. We used the “trim-and-fill” procedure developed by Duval and Tweedie (2000), which is designed to estimate the effect of potential data censoring, such as publication bias, on the findings of meta-analyses. This procedure uses a diagnostic funnel plot to display the distribution of individual effect sizes around the mean effect size, where the y -axis represents sample size (standard error) and the x -axis represents effect size (logged OR). In the absence of publication bias, the plot should take the shape of a funnel evenly distributed around the mean effect (Lipsey and Wilson, 2001). The funnel plot is illustrated in Figure 4. When a

random effects model is used, it shows that one missing effect size (small, negative) was imputed, which had the effect of slightly reducing the overall effect size, from an OR of 1.183 (95% CI: 0.932, 1.501, non-significant [NS]) to an OR of 1.134 (95% CI: 0.898, 1.432, NS). The substantial overlap of confidence intervals suggests that the mean effect sizes approximate one another and that publication bias is not likely a problem.

[Figure 4 about here]

Discussion

Summary

The main aim of this article was to report the results of a systematic review and meta-analysis of the effects of juvenile transfer on recidivism. The motivation for this review stems from the continued widespread use of transfer in the United States, which results in tens of thousands of juvenile offenders being processed in adult court every year—as a result, in part, of a belief that it will reduce recidivism. Prior reviews have cast doubt on the effectiveness of transfer in achieving this goal, but they have left open the possibility that it may in fact be effective.

The results of the review and meta-analysis, which focused on all extant methodologically rigorous evaluations of transfer, suggest, first, that transfer of juvenile offenders to criminal court leads to a small but nonstatistically significant increase in future offending. This finding, which provides a more robust and empirically based assessment of prior research, lends greater credence to the

consensus in the extant literature that transfer does not have a specific deterrent effect (e.g., Farrington et al., 2012; McGowan et al. 2007). At a minimum, it suggests that the balance of various causal mechanisms—for example, the deterrence and rehabilitation available through adult court sanctions versus juvenile court sanctions, respectively—does not result in an overall reduction in recidivism and may even increase it.

Second, the results suggest that transfer effects are highly heterogeneous. This heterogeneity does not seem to be a result of sampling error around an average effect, one that holds across all places and times (Borenstein, Hedges, Higgins, and Rothstein, 2009; Wilson, 2010). Rather, it seems to be a result of transfer potentially exerting different effects, whether because of the type of transfer, offenders, offenses, or sanctions that youth receive in juvenile court or adult court, respectively (Loughran et al., 2010). The potential for differential effects is one that exists not only for transfer but also for other types of sanctions (Mears, Cochran, Greenman, Bhati, and Greenwald, 2011).

Although some caution is needed in interpreting the results of moderator analyses (Lipsey, 2003), they provide a useful way to investigate effect heterogeneity. Two interesting findings emerged from the moderator analyses here. The first is that the type of transfer may in fact be associated with whether transfer has an effect. In particular, the analyses suggest that judicial waiver may increase recidivism. That may mean that judicial waiver, for whatever reason, has no deterrent effect. Indeed, it is possible that judicial waiver may be criminogenic; for example, it may be perceived by youth as unfair and so result in increased

offending out of defiance (Sherman, 1993). Alternatively, judicial waiver may be a more selective mechanism of transfer that places higher risk youth in the adult system. From that perspective, the increased recidivism would not be causal; instead, research on judicial transfer may not have sufficiently addressed selection effects associated with transfer.

The second, and related, finding from the moderator analyses is that study design may be associated with the estimation of transfer effects. As a logical matter, study design should not influence the actual effect of transfer. However, it may well contribute to varying estimates of the effect. Quasi-experimental techniques can work toward minimizing selection bias, but they cannot eliminate the possibility altogether (Blumstein, 2010). To date, no studies of juvenile transfer seem to have fully captured the ways in which transferred and nontransferred youth may be different. The challenge of comparability is not unique to transfer research and represents a larger obstacle in studying juvenile court sanction effects (Mears, 2003).

How was study design related to the estimated effects of transfer? The nine included studies employed a range of quasi-experimental designs. Two designs—propensity-score matching and nonequivalent comparison groups with statistical controls—identified statistically significant but opposite estimates of transfer effects on recidivism. The propensity-score matching studies found that transfer was associated with decreased recidivism, whereas the two nonequivalent comparison group studies found that transfer was associated with increased recidivism. Sensitivity analyses revealed that when the propensity-score matching

studies were removed, the overall effect size increased and achieved statistical significance. In short, if propensity-score matching studies were removed, the main conclusion of this study would be that transfer increases recidivism.

Research design can clearly affect estimates of policy and program impact (Weisburd, Lum, and Petrosino, 2001; Welsh, Peel, Farrington, Elffers, and Braga, 2011). Although the ideal research design for testing the specific deterrence hypothesis would be a randomized experiment (Nagin et al., 2009), none have been performed in the context of juvenile transfer. Others have posited that compared with other quasi-experimental designs, propensity-score matching most closely approximates randomization and offers the highest internal validity (Stuart, 2010). It is thus conceivable that the propensity-score matching analyses, when coupled with potentially more and better variables to address confounding, in fact provided more accurate estimates of impact. If so, then the true effect of transfer may be that it decreases recidivism. Nevertheless, it is not demonstrably clear that the propensity-score matching studies addressed selection bias better than did the other three types of quasi-experimental designs.

Limitations

Although this review and meta-analysis included studies that employed strong research designs, several limitations warrant emphasis because of their relevance both for interpreting the overall results and for understanding limitations in extant research on transfer. First, the number of included studies is small. This limitation, however, highlights a central tension for policy research and for policy

makers. In short, how many studies must be undertaken before policy makers can feel that credible scientific evidence of an effect, or a lack of an effect, exists? In writing about this issue in the context of evaluating after-school programs (ASPs), Taheri and Welsh (2015: 14) described the two competing perspectives:

One view holds that until such time that there are a sufficiently large number of evaluations of an intervention, there should be no conclusions drawn about its impact (effective, ineffective, or so on). Another view recognizes that it is important that policymakers, practitioners, and researchers have access to the best available information. Moreover, if one were to wait for a large number of evaluations of an important and widely used intervention (perhaps like ASPs) before conducting a systematic review, one could be waiting for many years, and in the meantime the intervention is being marketed and used, quite possibly in an inadequate or, worse yet, harmful way.

What emerges from the present review is that, after nine strong research evaluations of transfer have been undertaken, there is no reliable basis on which to argue that scientific evidence “now” exists for or against transfer as a policy of specific deterrence. Rather, the evidence is that transfer may decrease recidivism, but it also may increase it. The precise conditions under which such effects might arise remain unknown.

A second limitation is that a meta-analysis is only as valid as the studies on which it rests. Although the studies examined here relied on strong research

designs, they may have omitted important confounders that in turn resulted in biased estimates of impact (see, generally, Mears et al., 2014; Schubert et al., 2010). These confounders include such measures as differences in demeanor and attitude, gang involvement, weapon use, and family and community risk factors that may influence transfer decisions as well as recidivism. An important confounder that almost no studies address is the precise sequence of juvenile court sanctions that transferred youth have experienced (Johnson, Lanza-Kaduce, and Woolard, 2011). Additionally, different studies reflect samples of juvenile offenders from different states, each of which has a juvenile justice system and transfer laws.

A third limitation is that the studies did not include information about actual sanctions. Juvenile court processing does not mean that youth are *not* sent to prison, and likewise, criminal court processing does not mean that youth are convicted and sent to prison. In addition, the studies did not include information about the conditions associated with particular sanctions. Moreover, juvenile sanctions and interventions vary greatly, making it difficult to assess the effectiveness of various alternatives (Mears et al., 2011). Accordingly, even if a “true” average effect of transfer were to exist, the causal mechanism contributing to the effect would remain unclear.

Fourth, different studies typically have not used equivalent “time at risk” for the groups being compared (see, however, Loughran et al., 2010), which is a problem that plagues studies of recidivism (Nagin et al., 2009). If transferred juveniles are incarcerated for longer periods, then nontransferred groups will have

more time on the street in which they can be rearrested (Lanza-Kaduce et al., 2005), which would result in an underestimate of the effect of transfer on recidivism.

A final limitation involves the different mechanisms of transfer. The effects of transfer may well vary by transfer type, especially if the mode of transfer targets different youth (e.g., those who commit more serious crimes) or results in different sanctions. This issue raises a related caution for future research. To the extent that different types of transfer are used for different types of youth or offenses, studies will need to ensure that they match carefully to ensure that comparison groups resemble those of transferred youth. In addition, they will want to be careful in generalizing their results regarding the effects of transfer. It may be that any given study only generalizes to the effects of a specific type of transfer, or even a specific type of transfer for a specific population of youth or type of offenses.

Implications for Research and Policy

The effects of juvenile transfer have important theoretical and policy implications. Yet the policy implications of the present review remain tentative given the dearth of studies and the highly heterogeneous dispersion of effects. This conclusion echoes an observation made more than a decade ago: “Despite the wealth of research, the fact remains that we know relatively little about the true effects of waiver” (Mears, 2003: 157). It is also consistent with the observation that research on specific deterrence more generally “is not nearly sufficient for making firm

evidence-based conclusions for either science or public policy” (Nagin et al., 2009: 121). The same must be said of the effects of transfer on reoffending: Extant research does not provide a clear or consistent basis for determining whether transfer increases, decreases, or has no effect on recidivism.

The continued lack of empirical evidence poses some concern given the importance of developing a coherent legal rationale for juvenile transfer (Zimring, 2010). It remains possible that transfer achieves appreciable reductions in juvenile crime rates; however, little empirical basis exists to support that claim. If in fact transfer does not have a general deterrent or specific deterrent effect, then its continued use may be questionable. At the same time, it is conceivable that even if transfer has no direct effect on juvenile crime, it may enable the juvenile court to operate more effectively and in keeping with its original mission focused on child welfare. No empirical studies have assessed that possibility. Given the considerable growth of the juvenile justice system in recent decades and the fact that only a small percentage of court referrals are transferred, it seems unlikely that such a benefit exists. However, current studies have provided little basis for assessing the impact of transfer on recidivism when transfer is used to plea bargain youth to various juvenile court sanctions, many of which may differ from what otherwise would have happened without transfer as an option (but see Burrow and Lowery, 2015).

The theoretical basis for anticipating the beneficial effects of transfer in reducing recidivism remains unclear. Few youth presumably know about or understand what transfer may entail. Although some youth may have been in

juvenile custodial facilities, they will not have direct knowledge of adult sanctioning outcomes or conditions in adult prisons. In addition, they may view the sanctions differently than do adults (Loughran, Piquero, Fagan, and Mulvey, 2012). One of the only studies that addressed this issue comes from interviews that Bishop and Frazier (2000) conducted with juveniles in juvenile and adult prisons; the youth in adult prisons felt that they had been treated less fairly or humanely. How these different experiences translate into deterrent effects is uncertain. At the same time, scholars have identified many other mechanisms that may contribute to worse rather than to improved outcomes. For transferred youth who go to adult prisons, for example, the experience of incarceration may be more criminogenic through exposure to more hardened or “seasoned” offenders and less exposure to rehabilitative programming (Lambie and Randell, 2013; Redding, 2010). In addition, communicating to juvenile offenders that they are adult criminals may create a “self-fulfilling prophecy” (Fagan et al., 2007: 70).

As noted, these different effects may apply to different kinds of juvenile offenders. It is possible that transferring juvenile offenders to criminal court represents an effective means of furthering the mission of the juvenile court (i.e., for serious, older offenders who represent a danger to other juveniles) but an ineffective or even harmful strategy for younger or first-time offenders. Alternatively, juvenile transfer may be more effective with low-risk youth. Loeffler and Grunwald (2015), for example, used a regression discontinuity design to assess the impact of adult court processing for low-risk, transfer-ineligible juvenile offenders arrested for felony drug crimes. They found

a small decrease in recidivism for young drug offenders processed in adult court. Whether such findings generalize to other types of crimes or places remains unknown.

As newer forms of transfer, such as prosecutorial or legislative waiver, may send more first-time offenders to adult court, it may be that judicial waiver targets a different class of juvenile offenders (e.g., Verrecchia, 2011). Moderator analysis revealed that studies of judicial waiver were associated with the highest recidivism level. One explanation for that finding is that judicial waiver does not act as a specific deterrent. Another is that the finding is spurious, a result of unaddressed selection effects. If judicial waiver is a more selective transfer mechanism, it may send higher risk juvenile offenders (i.e., more likely to recidivate) to criminal court. Indeed, this use represents the traditional role of judicial waiver. Despite whether the effect is spurious, the more general possibility is that juvenile transfer may not be “good” or “bad” for all juvenile offenders. Instead, its effect may vary based on the mechanism of transfer, characteristics of the transferred juveniles, and the punishments and interventions associated with juvenile court sanctions and adult court sanctions, respectively.

It is possible, too, that transfer achieves a more appropriate level of retribution, one consistent with what society may view as desirable. However, what society expects of transfer and whether transfer provides the desired amount of retribution for particular offenses remains unknown (Bishop, 2006). In addition, transfer does not guarantee that punishment actually occurs (Howell, 1996). A related consideration is that even if transfer were to achieve a desired

level of retribution, it is possible that this may be unconstitutional. A growing body of literature has suggested that juveniles are not as culpable as adults because of differences in their neurological, cognitive, and psychosocial development (e.g., Cauffman and Steinberg, 2012, Scott and Steinberg, 2008). The U.S. Supreme Court has referred to these substantial differences between adults and juveniles in recent landmark decisions where certain adult punishments for juveniles were held to be unconstitutional because juveniles were, as a class, viewed as less culpable (e.g., *Graham v. Florida* [2010]; *Miller v. Alabama* [2012]; *Roper v. Simmons* [2005]). A retributive rationale for the increase in juvenile transfer, thus, appears to be tenuous.

This state of research can be viewed in two ways. On the one hand, the absence of clear and consistent evidence of appreciable benefits of transfer may be viewed as a basis for greatly reducing its use. On the other hand, transfer can be viewed as somehow necessary, and so it should be retained. The lack of evidence of such benefits may be viewed as temporary. More and better research may help to adjudicate between the two views if it includes assessment of the effect of transfer on a diverse array of outcomes, including retribution, juvenile crime rates, recidivism, and other outcomes central to the juvenile court (Farrington et al., 2012; Mears et al., 2011; Myers, 2003). Of particular importance will be research that identifies the precise conditions under which transfer may decrease recidivism and the conditions under which it may increase recidivism. Until additional high-quality evidence on the empirical consequences of juvenile transfer emerges, we will not be able to determine “the good and the

harm we do through routine criminal justice operations” (Blumstein, 2010: 556)
or, in this case, routine juvenile and criminal justice operations.

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Table 1. Summary of Included Studies

Author (Year)	Location	Population (Date)	Treatment Group	Comparison Group	Transfer Mechanism	Study Design (Follow-up)	Outcome
Podkopacz (1996)	Hennepin County, Minnesota	All juveniles motioned for transfer (1986–1992)	175 transferred youth	115 retained youth	Judicial waiver	QExp, nonequivalent comparison with statistical controls (7 yrs.)	Adjudication or conviction
Fagan (1996)	New York and New Jersey (2 counties each)	Juveniles aged 15–16 arrested for robbery or burglary (1981–1982)	400 youth in NY criminal court	400 youth in NJ juvenile court	Statutory exclusion	Qexp, natural experiment (7 yrs.)	Any rearrest; Violent rearrest
Winner et al. (1997)	Florida	All juveniles adjudicated (1987)	2,700 transferred youth	2,700 retained youth	Multiple (mostly direct-file)	QExp, with matched pairs (7 yrs.)	Any rearrest; Felony rearrest
Myers (2003)	Pennsylvania	Juveniles aged 15–17 arrested for robbery or assault (1994)	79 transferred youth	415 retained youth	Judicial waiver	QExp, nonequivalent comparison with statistical controls (18 mos.)	Any rearrest
Lanza-Kaduce et al. (2005)	Florida (6 judicial circuits)	All juveniles adjudicated (1995–1996)	475 transferred youth	475 retained youth	Multiple (mostly direct-file)	QExp, with matched pairs (4 yrs.)	Felony rearrest
Fagan et al. (2007)	New York and New Jersey (3 counties each)	Juveniles aged 15–16 arrested for felony robbery, assault, or burglary (1992–1993)	1,321 youth in NY criminal court	1,061 youth in NJ juvenile court	Statutory exclusion	QExp, natural experiment (7 yrs.)	Any rearrest; Violent rearrest
Loughran et al. (2010)	Maricopa County, Arizona	Serious juvenile offenders aged 14–17 (2000–	128 transferred youth	461 retained youth	Multiple mechanisms	QExp, with group propensity matching	Any rearrest

			2003)			(4 yrs.)	
Jordan (2012)	Pennsylvania (3 counties)	Juveniles aged 15–17 arrested for violent crime, deadly weapon, or prior offense	102 transferred youth (not decertified)	102 nontransferred youth (decertified)	Statutory exclusion (decertification to juvenile court)	QExp, with propensity score matched pairs (8 yrs.)	Any rearrest
Drake (2013)	Washington	Juveniles aged 16–17 arrested for serious felony (pre- and post-1994)	770 transferred youth (post- 1994)	446 nontransferred youth (pre- 1994)	Statutory exclusion	QExp, natural experiment (3 yrs.)	Any reconviction

Table 2. Meta-analysis by Study Design

Study Design	Study	Odds Ratio	Lower Limit	Upper Limit	<i>p</i>
Nonequivalent Comparison with Statistical Controls	Myers (2003)	2.344	1.077	5.2104	.032
	Podkopacz (1996)	1.929	1.010	3.683	.046
	All 2 studies	2.089	1.270	3.435	.004
<i>Q</i> = 0.143, <i>I</i> ² = 0.000, <i>df</i> = 1, <i>p</i> = .706					
Natural Experiment	Fagan (1996)	1.579	1.156	2.157	.004
	Drake (2013)	1.390	1.077	1.793	.011
	Fagan et al. (2007)	0.850	0.713	1.105	NS
	All 3 studies	1.214	0.811	1.818	NS
<i>Q</i> = 16.52, <i>I</i> ² = 87.89, <i>df</i> = 2, <i>p</i> = .000					
Matched Pairs	Lanza-Kaduce et al. (2005)	1.774	1.393	2.261	.0001
	Winner et al. (1997)	0.948	0.852	1.055	NS
	All 2 studies	1.284	0.695	2.373	NS
<i>Q</i> = 21.51, <i>I</i> ² = 95.35, <i>df</i> = 1, <i>p</i> = .000					
Propensity-Score Matching	Loughran et al. (2010)	0.795	0.516	1.223	NS
	Jordan (2012)	0.553	0.320	0.955	.033
	All 2 studies	0.690	0.488	0.976	.036
<i>Q</i> = 1.042, <i>I</i> ² = 4.052, <i>df</i> = 1, <i>p</i> = .306					
Between-Group Heterogeneity	<i>Q</i> = 13.814, <i>df</i> = 3, <i>p</i> = .003				

Note. Random effects model used. NS = non-significant.

Table 3. Sensitivity Analyses of Study Design

Studies Removed	Odds Ratio	Lower Limit	Upper Limit	<i>p</i>	<i>n</i>
Nonequivalent Comparison with Statistical Controls	1.086	0.849	1.390	NS	7
					$Q = 45.057, df = 6, p = .000$
Natural Experiments	1.176	0.808	1.711	NS	6
					$Q = 35.879, df = 5, p = .000$
Matched Pairs	1.156	0.843	1.583	NS	7
					$Q = 31.078, df = 6, p = .000$
Propensity-Score Matching	1.346	1.036	1.748	.026	7
					$Q = 45.234, df = 6, p = .000$

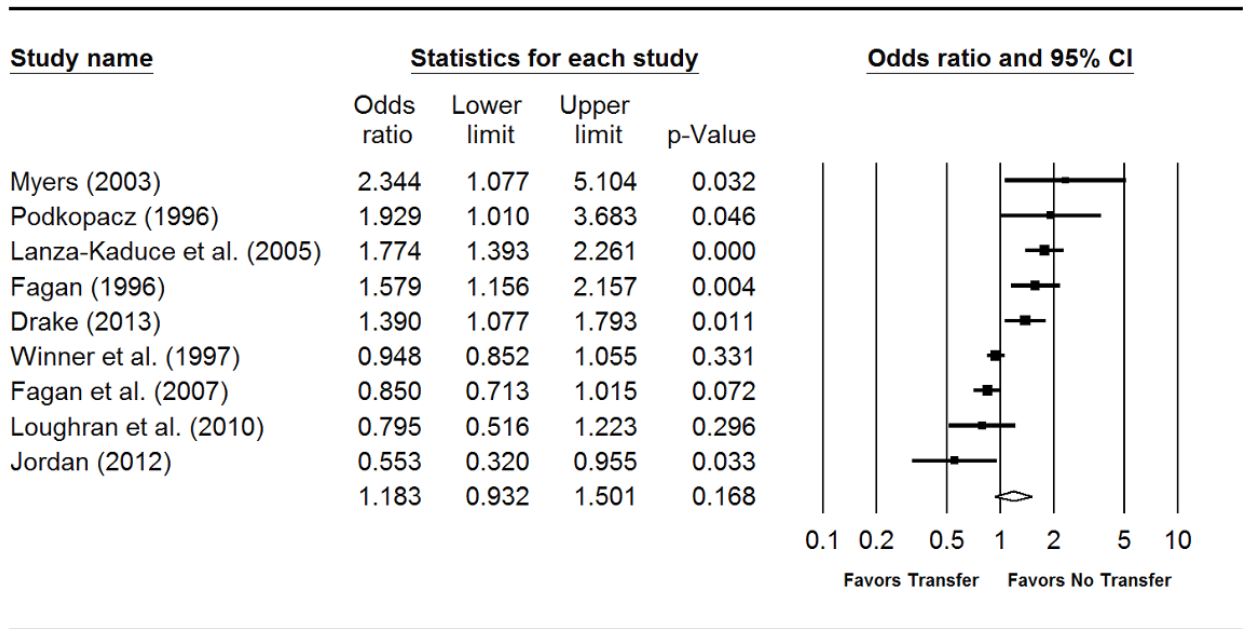
Note. Random effects model used. NS = non-significant.

Table 4. Effect Sizes by Study Features

Study Feature	Odds Ratio	Lower Limit	Upper Limit	<i>p</i>	<i>n</i>
Sample Offense Type					
All juvenile offenders	1.420	0.839	2.403	NS	3
Serious juvenile offenders	1.085	0.781	1.508	NS	6
<i>Q</i> = 0.721, <i>df</i> = 1, <i>p</i> = .396					
Sample Size					
Large (treatment ≥ 400)	1.235	0.939	1.624	NS	5
Small (treatment < 400)	1.133	0.601	2.136	NS	4
<i>Q</i> = 0.060, <i>df</i> = 1, <i>p</i> = .807					
Transfer Mechanism					
Statutory exclusion	1.047	0.710	1.543	NS	4
Judicial waiver	2.089	1.270	3.435	.004	2
Multiple	1.118	0.704	1.775	NS	3
<i>Q</i> = 5.090, <i>df</i> = 2, <i>p</i> = .078					
Length of Follow-Up					
≥7 years	1.031	0.794	1.340	NS	5
<7 years	1.408	0.986	2.011	NS	4
<i>Q</i> = 1.904, <i>df</i> = 1, <i>p</i> = .168					
Outcome Measure					
Reconviction	1.452	1.146	1.841	.002	2
Rearrest	1.102	0.840	1.446	NS	7
<i>Q</i> = 2.252, <i>df</i> = 1, <i>p</i> = .133					

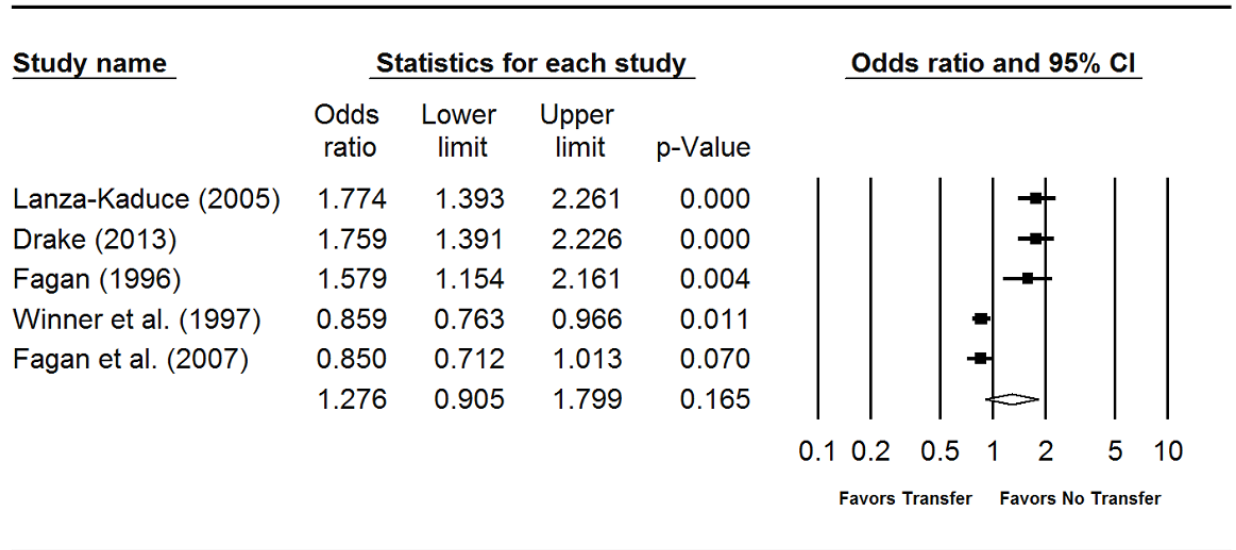
Note. Random effects model used. NS = non-significant.

Figure 1. Forest Plot of the Distribution of Effect Sizes for All Recidivism



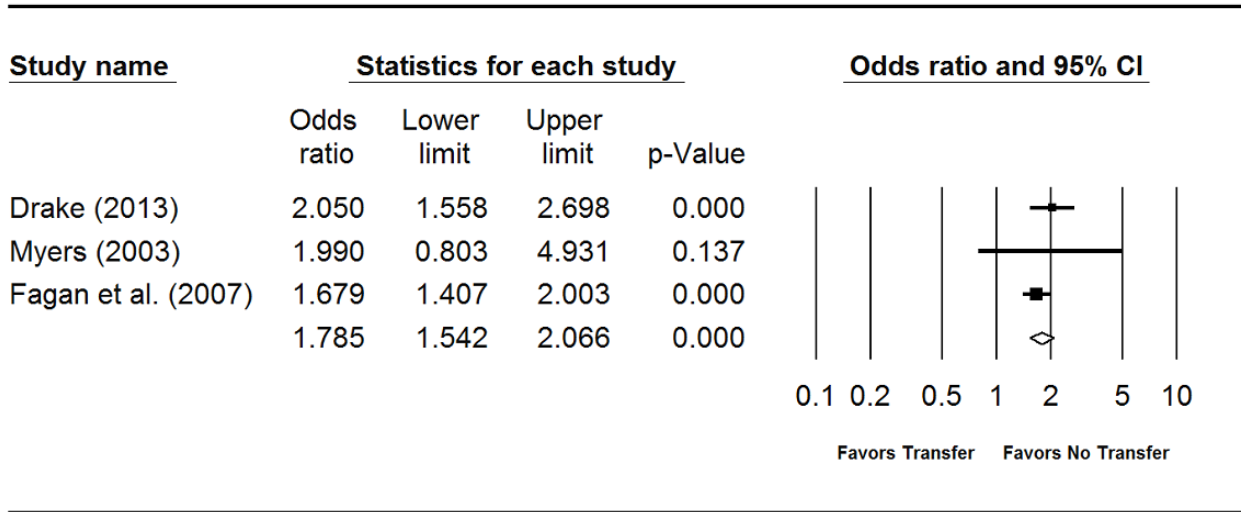
Note: Random effects model used

Figure 2. Forest Plot of the Distribution of Effect Sizes for Felony Recidivism



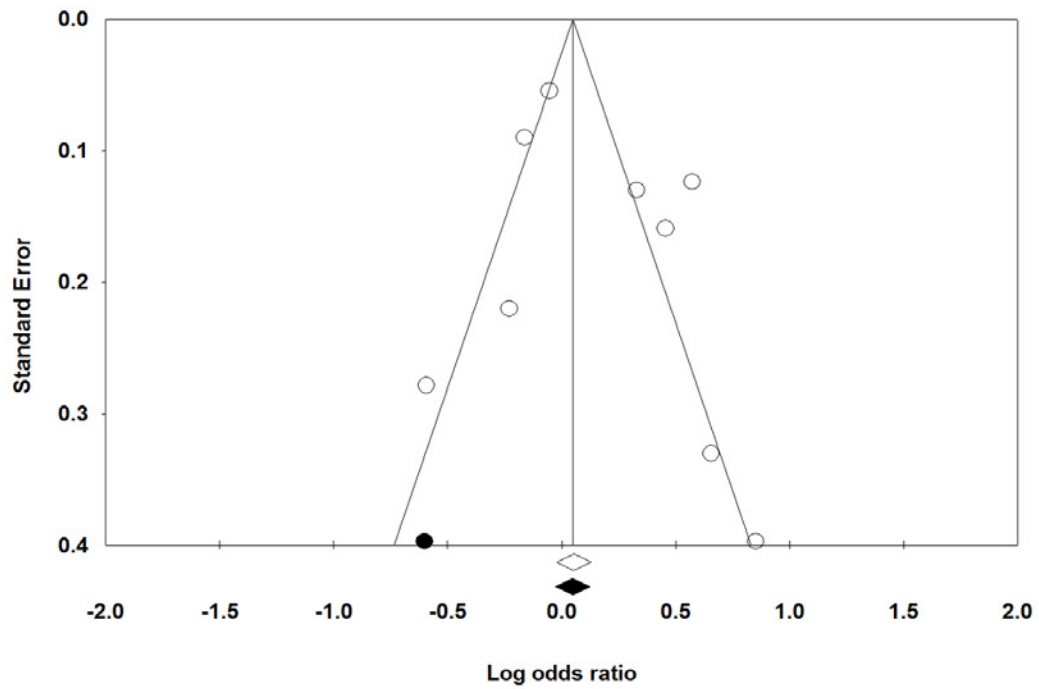
Note: Random effects model used

Figure 3. Forest Plot of the Distribution of Effect Sizes for Violent Felony Recidivism



Note: Random effects model used

Figure 4. Funnel Plot of Nine Included Studies with Imputed Value from Trim-and-Fill Analysis



Notes. Random effects model used; empty circles represent the nine included studies.