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Differential Effects of Adult Court Transfer on Juvenile Offender Recidivism

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Abstract

Prior research indicates that adolescent offenders transferred to adult court are more likely to recidivate than those retained in the juvenile system. The studies supporting this conclusion, however, are limited in addressing the issue of heterogeneity among transferred adolescents. This study estimates the effect of transfer on later crime using a sample of 654 serious juvenile offenders, 29% of whom were transferred. We use propensity score matching to reduce potential selection bias, and we partition the sample on legal characteristics to examine subgroup effects. We find an overall null effect of transfer on re-arrest, but evidence of differential effects of transfer for adolescents with different offending histories. These results suggest that evaluating the effects of transfer for all transferred adolescents together may lead to misguided policy conclusions.

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Keywords

Adult transfer; Juvenile justice; Sanctions; Propensity scores

The option to transfer an adolescent offender to adult court has been a feature of the juvenile court since its inception. There has always been a recognition that certain, usually older, adolescents may commit very serious offenses for which the juvenile system cannot provide a substantial enough penalty to satisfy the public's demand for punishment (Zimring, 2000). There may also be adolescent offenders who, despite the best efforts of the juvenile system, continue to offend, and for whom more of the same services seem to serve little purpose (Bishop & Frazier, 2000). The implicit assumption behind transfer policy has been that youth meeting either criterion are distinctly different from other youthful offenders and are better handled with the harsher sanctions they would find in a punitive-oriented criminal court as compared to the rehabilitation-minded juvenile court (Kupchik, 2003; Myers, 2003).

Perhaps the most dramatic change in the practice of transfer occurred with policy reforms in the 1990s and early 2000s, when nearly every state in the nation attempted to dramatically toughen laws governing criminal prosecution and sentencing of juveniles (Griffin, 2003). These statutory revisions both widened the net of eligibility and broadened the range of mechanisms by which transfer could be accomplished. Specifically, in many states, these reforms expanded the set of crimes that qualified an adolescent for transfer, lifted age restrictions, and added statutory exclusion and prosecutorial discretion as methods for achieving transfer to adult court. As a result of these changes, there was an increase in the rate of transfer and a likely increase in the heterogeneity of the youth sent to adult court, with considerable variability in the adolescents constituting this expanded pool of adult court cases (Schubert et al., 2009). Expansions of the transfer statutes made it easier for a broader group of adolescents to be processed by the adult court.

These changes have reinvigorated an ongoing debate about the proper role of transfer in the juvenile system (Fagan & Zimring, 2000). This critical decision has long been a flash point for disagreement between advocates of a more rehabilitation-focused juvenile system and those who see proportionality and retribution as equally (or more) legitimate goals that must be achieved when dealing with serious juvenile crime (Feld, 1999). Assumptions about adolescent culpability and amenability to treatment as well as the effectiveness of risk assessment all underpin societal determinations about when and how to transfer an adolescent offender to the adult system (Mulvey & Leistico, 2008; Steinberg & Scott, 2003). Recently, researchers have been particularly focused on one piece of information that goes into this debate—the relative impact of being transferred on outcomes for serious juvenile offenders.

The Effects of Transfer

While the theoretical intent of broader transfer provisions was clear (i.e., sufficient retribution for serious criminal behavior, deterrence through strengthened sanctioning and penalties), there has been only limited definitive empirical evidence regarding the effects of transfer on the future engagement in criminal and antisocial activities for these juveniles. There has certainly been an oft-voiced consensus that transfer policies uniformly produce negative outcomes (Redding, 2008). Based on a number of similarly designed studies in several locales, numerous commentators and scholars have come to the conclusion that transferred adolescents are more likely to “recidivate, recidivate at a higher rate, and be rearrested for more serious offenses, on average, than those retained in the juvenile system”

(Bishop & Frazier, 2000). The existing research behind this conclusion, however, is limited by its inability to address adequately the issue of heterogeneity in adolescents transferred to adult court.

It is debatable whether this research has fully addressed the issue of sample selection when assessing the impact of being transferred to adult court or retained in juvenile court. Several factors, including but not limited to age, offense, and number of prior petitions, may influence the likelihood that an individual's case is transferred to criminal court. Furthermore, some of these same factors associated with transfer may also be associated with higher levels of future recidivism. A comparison of offenders who do and do not get transferred to adult court thus 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. In formal statistical parlance, since there is no random assignment to the 'treatment condition' of transfer to adult court, the two groups are not directly comparable. This phenomenon prevents researchers from making confident causal inferences about the effects of adult court transfer on important outcomes.

Previous empirical research on the effects of transfer have attempted to address the problem of selection bias (Smith & Paternoster, 1990). Fagan (1995, see also Fagan, Kupchick, & Liberman, 2003) conducted a natural experiment of the deterrent effects of juvenile versus adult court sanctions by comparing recidivism among 15 and 16 year olds charged with robbery and burglary. This study was unique because differences in New York and New Jersey state transfer laws for these crimes permitted a comparison of outcomes for youth coming from otherwise comparable neighboring counties. Youth in New Jersey were compared to youth who committed the same crime in a matched community from New York, where the age of criminal responsibility is lower. They found that, for robbery offenders, transfer was associated with a higher likelihood of, and quicker time to, re-arrest. While significant, these effects could only be generalized to robbery offenses, as the authors found no such significant differences for burglary offenders. Bishop, Frazier, Lanza-Kaduce, and Winner (1996) and Winner, Lanza-Kaduce, Bishop, and Frazier (1997) also attempted to correct for selection bias in estimating the effects of transfer on future recidivism by matching a sample of Florida transfer cases to non-transfer cases on seven factors, i.e., number and seriousness of charges, number and seriousness of priors, age, race, and gender. They also found that transferred youth had an increased likelihood of recidivism and re-offended more quickly than their non-transferred counterparts. More recently, the issue of selection bias was addressed by Myers (2003) in his analysis of outcomes for 494 youth from Pennsylvania, 79 of whom were transferred to adult court and 415 who were retained in juvenile court. Using statistical controls for selection bias, Myers also concluded that transferred youth had higher rates of recidivism.

It is unclear, however, whether or not the statistical controls used to adjust for selection bias in these investigations achieved their goal. Many data sources are necessarily limited in their ability to characterize an adolescent offender's background or current functioning, and there is the possibility that some important group difference between the transferred adolescents and their juvenile court counterparts might be producing some of the observed effects. It is difficult to know with certainty if selection bias can be properly accounted for by matching cases on a limited range of potential confounders.

Heterogeneity in Response to Transfer

Prior research has paid only limited attention to the possible differential effects of transfer on subgroups of offenders. Much of the research and debate on transfer policy has pointed toward a singular, universal effect of transfer across all individuals and offense types. This view, however, may be myopic if expanding transfer laws have generated influential heterogeneity among the population of transferred individuals, as Bishop (2000) asserts and is demonstrated in another analysis from this study (Schubert et al., 2009). With expanding statutes creating a widening net to catch juvenile offenders for transfer to adult court, it is likely that we have what Zimring (1998) describes as an 'inappropriate aggregation'—that is, different types of offenders with different responses to transfer consequences being inadvertently combined together for analytic purposes. Thus, it is not clear that a singular effect across all transferred individuals is a sufficient metric to evaluate the merits of transfer policy. A more plausible scenario is that transfer to adult court has *differential* effects on individuals depending on both offender and offense characteristics. It might be, for example, that inexperienced offenders respond much differently to criminal sanctions compared to more seasoned ones (e.g., Loughran, Piquero, Fagan, & Mulvey, in press; Pogarsky & Piquero, 2003).

Current Focus

The goal of the present study is to assess the effects of transfer to adult court on serious adolescent offenders, expanding the findings of prior work by using a more stringent method of control for sample selection than in the prior literature. We use data from a group of participants in the Pathways to Desistance (Pathways) study, a large, longitudinal study of serious juvenile offenders which provides a wide array of relevant pre-adjudication information and outcome data regarding arrests and involvement in antisocial activity (see Mulvey et al., 2004; Schubert et al., 2004). Unlike previous analyses of the effects of transfer, the current empirical study uses propensity score methodology (Rosenbaum & Rubin, 1983), an alternative statistical method selected to account for selection bias. This approach, when used in conjunction with the richness of the Pathways data, allows us to consider and subsequently rule out *many* pretreatment variables which may be confounders, instead of direct matching on only a limited number of them. As a result, we are able to estimate the *causal* effects of transfer to adult court more stringently (though never definitively) than previously possible. Furthermore, the uniqueness of the sample allows us to search for evidence of possible differential effects of transfer on adolescents that differ based upon readily identifiable, legally relevant characteristics, including different presenting charges and prior history. In this way, we can assess how increased heterogeneity in the transferred group, promoted by more inclusive statutes, might produce both positive and negative effects simultaneously, depending on the subgroup of adolescent offenders considered. These analyses illustrate the importance of this issue when assessing the overall benefits of more expansive transfer criteria.

Methods

Sample

This study examined a subset of the research participants recruited for the Pathways to Desistance study, a longitudinal investigation of serious adolescent offenders. Participants in the Pathways study were adolescents, at least 14 years old and no older than 17 when they committed the offense that qualified them for enrollment. These adolescents were found guilty of committing a serious offense (almost entirely felony offenses) in either Maricopa County, AZ, or Philadelphia County, PA. Because drug violations represented such a large proportion of the offenses committed by this age group, the proportion of juvenile males

recruited with a drug offense was capped at 15% so that the sample would have sufficient heterogeneity regarding presenting offense. More information regarding the rationale and overall design of the study can be found in Mulvey et al. (2004); details regarding recruitment, a description of the full sample and the study methodology are discussed in Schubert et al. (2004).

The current study considers the $n = 654$ individuals in the Pathways study who were enrolled in Maricopa County, AZ, because they represent adolescents who could have been transferred to adult court in a locale using a "wide net" for transfer determination.[1] Consequently, a relatively high percentage (29%; 193/654) of Pathways participants enrolled in Arizona were processed in the adult system. Thus, this sample provides an opportunity to examine the effects of transfer applied at a relatively high rate and affecting a heterogeneous sample of adolescent offenders.

We excluded from these analyses 65 cases that had been transferred to adult court because the case was either dismissed in adult court ($n = 15$), there was missing court record information ($n = 8$), a missing follow-up interview prevented us from knowing when the youth was first released from his/her disposition stay ($n = 14$), or the adolescent was not released from their disposition stay before the end of the follow-up period ($n = 28$). This yields a working transfer sample of $n = 128$. We consider the implications of excluded cases in the discussion below.

The Maricopa County sample is 17 years old on average (with a range of 14–18 years old at the time of the baseline interview), and provides an ethnic mix of offenders (59% Hispanic, 21% Caucasian, 12% African-American, and 8% other). The majority of these offenders were adjudicated on felony offenses against a person (56%). Adolescents in the sample were followed from the date of their baseline interview (within 45 days of their juvenile court adjudication or within 90 days of their adult court arraignment) through the date of their 4-year follow-up interview. The average follow-up period was 3.8 years ($SD = 7.3$ months). There was no significant difference between the transferred and retained group in average days of follow up. Time at risk in the community, however, was greater for the group retained in the juvenile court. Youth transferred to the adult court spent, on average, 33% of the recall period in a facility while youth retained in the juvenile system spent, on average, 21% of their time in a facility. More detailed descriptive information regarding the sample and the distinguishing characteristics of the transferred and juvenile cases can be found in Schubert et al. (2009).

Measures

Outcome variable—We used *rate of re-arrest* as a measure of subsequent involvement in criminal activity. Indicators of arrest prior to age 18 were based on reports of petitions to juvenile court recorded in the Juvenile On-Line Tracking System (JOLTS) used in Maricopa County. Arrests after age 18 were based on nationwide FBI arrest records. Probation violations, without an additional criminal charge being filed, were not counted as a rearrest for this analysis.[2] For adolescents given probation at disposition, the rate of re-arrest was calculated as the total number of arrests divided by total time on the street for the period after disposition to the end of the 48-month follow-up period. For adolescents who were sent to an institution, the rate of re-arrest was the total number of arrests divided by total time on

[1]Arizona law provides three main mechanisms by which a juvenile can be transferred, including judicial, prosecutorial, and statutory. The Arizona statute delineates a broad range of offenses that qualify an adolescent for automatic transfer, and the age of exclusion from juvenile court can be young (i.e., 8 years old) in some situations. There is no automatic waive-back provision under the Arizona statute, and once a juvenile is prosecuted as an adult, all subsequent arrests come under adult court jurisdiction. For a more detailed discussion of these various forms of transfer in Arizona, refer to Schubert et al. (2009).

the street for the period after release from the institutional stay ordered at disposition until the end of the 48-month follow-up period. In each case, the rate was standardized such that the outcome would be in terms of yearly rate. The main benefit of this measure was that it controls for exposure time in the community, as opposed to either a binary marker for re-arrest or total number of arrests post-disposition, both of which may be confounded with exposure time. The importance of controlling for exposure time has been noted elsewhere (Piquero et al., 2001).

Covariates and predictors—The Pathways data allowed us to consider—and subsequently eliminate—a wide range of baseline variables as potential confounders related to selection bias. Specifically, we considered 59 covariates measured at baseline, including demographic, family history, peer, legal, psychological, substance abuse, psychosocial maturity, and prior adjustment factors. Each of these factors was specifically selected to be ruled out as a potential confounder within the analysis. We list each of these covariates and consider the potential for each to be a confounder below. The covariates were selected to account for the influence of individual, situational and developmental factors on juvenile crime (Mulvey et al., 2004).[3] Table 1 reports variable descriptions, scales, and conditional means (by adult and juvenile cases) for each of the 59 covariates.

Selection Bias: Initial Imbalances Among Covariates

As mentioned previously, there may be important preexisting differences between the group of transferred individuals and those retained in the juvenile system. Furthermore, these differences themselves may be affecting the outcomes and introduce a substantial bias when trying to equate a basic difference in means in each outcome to an ostensible treatment effect. Fortunately, we were able to check for *covariate balance*, that is, differences between groups prior to adjudication. When a covariate is in balance, we may reasonably rule it out as a potential confounder; when it is not, it may be biasing the estimate of the treatment effect. As mentioned, the main advantage of the Pathways data was the wide range of pre-adjudication covariates over which we were able to create balance, and thus, ultimately eliminate as potential confounders.

We checked for initial balance in two ways. First, we compared the differences in covariates between the adult and juvenile groups using an ordinary difference-in-means test, considering the associated *F*-statistics (i.e., the square of a normal *t*-statistic for a test of difference in means) for each test, so as to simplify later comparison. The null hypothesis in each case was no difference between means between adult and juvenile cases. Any *F*-statistic exceeding a value of 3.84 would correspond to a significant difference in the sample means at $\alpha = .05$, and for our purposes, was considered to be out of balance. Second, we also considered the standardized bias statistic (SBS) advised by Rosenbaum and Rubin (1985), to ensure that differences between the conditional variables were not dependent upon sample size. This measure, reported in this paper as a percentage, was the mean difference as a percentage of the average standard deviation, $100\left(\bar{x}_{\mathrm{T}} - \bar{x}_{\mathrm{C}}\right)/\left[\left(s_{\mathrm{T}}^2 + s_{\mathrm{C}}^2\right)/2\right]^{1/2}$, where for each covariate, $\bar{x}_{\mathrm{T}}$ and $\bar{x}_{\mathrm{C}}$ were the sample means in the treated group and the control group, respectively, and s_{T}^2 and s_{C}^2 were the corresponding sample variances. Rosenbaum and Rubin

[2]A reviewer commented that in some states, probation violations may arise as a result of fairly serious (felony) behaviors but are simply charged as probation violations. Thus, our exclusion of probation violations in the numerator of the rate could be potentially problematic, as it might understate the true count of arrests. Unfortunately, we are unable to discern the cause of the probation violation from the data. Therefore, we proceed without the inclusion of parole violations in the rate calculation, and recognize this as a possible limitation of the findings.

[3]Additional information about the instruments used to measure these covariates or the derivation of the scores used can be obtained from the corresponding author.

offer that a standardized difference percentage value greater than 20 for any covariate would suggest that the covariate is out of balance.

Table 2 reports associated *F*-statistics and standardized bias statistics, for each of the 59 baseline covariates considered. Notice that initially, 16 out of the 59 covariates are out of balance. Furthermore, some of those which are out of balance could also potentially lead to selection biases, such as number of prior petitions, perception of punishment costs, and exposure to violence. Also, several important demographic factors including age, race, and gender are out of balance. In summary, there are multiple factors which may be simultaneously affecting both later recidivism in offenders as well as the likelihood that that an offender is transferred to adult court. Thus, any basic associations relating transfer to adult court to later recidivism may, in fact, be due to the confounding of these factors and not by a causal mechanism.

Creating Balance Over Observable Covariates: Propensity Scores

The propensity score represents the probability that an individual received some treatment conditional on a vector of observed covariates (Rosenbaum, 2002). Rosenbaum and Rubin (1983) show that, conditional on two individuals, one treated and one control, having an identical propensity score, the difference in treatment status becomes independent of all observable characteristics. The idea is to estimate a propensity score for each individual, and in turn use this estimate as a method for creating balance on key covariates that may be confounding the treatment effect estimate. We used a binary logistic regression model to estimate the propensity score, with the binary transfer status as the dependent variable, and some combination of the 59 initial covariates, including some squares and interactions, as explanatory terms. Model selection is conducted through an iterative process, with the primary goal of achieving covariate balance afterwards (Heckman, Ichimura, Smith, & Todd, 1998; Rosenbaum & Rubin, 1984), as opposed to explicit modeling of the selection process. The predicted probability from the final model for each individual is thus that individual's estimated propensity score.

After estimating a propensity score for each individual, instead of conventional matching, we employed subclassification (i.e., stratification), where subjects are divided into equally sized subgroups based on the propensity score distribution. Stratification can be thought of as a special form of matching where subjects are grouped, rather than paired, with other individuals within a certain range of propensity scores. Rosenbaum and Rubin (1984) contend that stratification using quintiles (i.e., five equally sized subgroups) can remove approximately 90% of the initial imbalance in each of the baseline covariates. The subsequent average effect of treatment on the treated (ATT) can be estimated as a weighted average of within-stratum probation-minus-placement mean differences in outcome, *Y*,

$$\mathrm{ATT}_{\mathrm{transfer}} = \sum_{j=1}^{J} \frac{n_{\mathrm{transfer},j}}{n_{\mathrm{transfer}}} \left[\bar{Y}_{\mathrm{transfer},j} - \bar{Y}_{\mathrm{juv},j} \right]$$

where there are $j = 1,\ldots, J$ number of strata, $n_{\mathrm{transfer},j}$ and n_{transfer} represents the total number of transferred individuals in stratum j and overall, respectively, and $\bar{Y}_{\mathrm{transfer},j}$ and $\bar{Y}_{\mathrm{juv},j}$ denote the mean rate of re-arrest for transferred and retained individuals, respectively, in stratum j.

Results

Covariate Balance After Subclassification

After subclassification on the estimated propensity score, it is necessary to reassess covariate balance, in order to determine if, after stratification, any covariates are still able to strongly predict transfer. If this is the case, there still may be residual selection bias. To reevaluate balance, we estimated a two-way ANOVA for each covariate, where binary transfer status was one factor, the propensity score quintile was a second factor, and the covariate itself was the dependent variable. If balance was achieved, then there would be neither a statistically significant main effect of transfer status on the covariate nor a statistically significant interaction effect of transfer status by quintile. Any *F*-statistic exceeding 3.84 would suggest a significant effect, and hence, indicate that the covariate was still out of balance. If these two conditions were not met, the propensity score was re-estimated by adding quadratic or interaction terms with those covariates which remained out of balance to the propensity score model specification. It is important to note that covariate balance, and not model parsimony, is the primary goal of estimating the propensity score (Rubin & Thomas, 1992).

Figure 1 displays distributions of *F*-statistics for tests of balance both prior to and after subclassification. Notice that after subclassification, the distribution of associated *F*-statistics for both main and interaction effects is considerably reduced. Overall, only one of the 59 main effects, and none of the interaction effects, is significant, suggesting that we have achieved sufficient covariate balance. Note that this is actually better balance of observable covariates than one might expect from randomization, since, at $\alpha = .05$, we would expect about 3 of these 59 to be out of balance.

Treatment Effect of Transfer to Adult Court

Prior to subclassification, the mean rate of re-arrest for those individuals transferred to adult court was about .91 arrests per year, compared to about .93 arrests per year for those individuals retained in the juvenile system. The difference in arrests per year is essentially negligible, suggesting that there is no difference between rate of re-arrest as an outcome for these two groups overall. As elaborated above, however, selection problems prevent us from interpreting this difference or lack of difference as a valid indicator of the causal treatment effect of transfer.

Instead, we may estimate the average treatment effect after subclassification on the propensity score. After accounting for the potential selection biases in the initial covariates using subclassification, the estimated ATT is −.23 (SE = .22; *t*-statistic = −1.03). This difference can be interpreted as the average treatment effect of being transferred on those individuals who were actually transferred, as opposed to the average treatment effect across the entire population of juvenile offenders. However, not only is this estimate not statistically significant at conventional levels, but its small magnitude (indicating only about one less arrest every four years for adult cases as compared to those retained in juvenile court) suggests that it is not very meaningful in either a practical or policy sense. Thus, we conclude that this result points to a null effect of transfer.

Exploring Potential Treatment Effect Heterogeneity

While the above results demonstrate a null effect of transfer when averaging across the entire group of transferred individuals, if this large group is indeed heterogeneous in its makeup, it is plausible that different subgroups of individuals may also have different responses to transfer. The overall null effect may actually be masking differential effects within different groups. We explore this possibility by partitioning our sample into subgroups two different ways and examining subgroup-specific effects on subsequent re-

arrest conditional upon transfer status. We selected variables upon which to partition the sample based on their suitability as legally relevant criteria for differentiating among serious offenders and our previous findings regarding factors that differentiated outcomes within the transferred group (Schubert et al., 2009).

First, we partition the sample based on the most serious adjudicated charge on the study index petition[4]—property crimes (e.g., burglary and theft) versus person crimes[5] (e.g., assault). Second, we partition the sample by number of prior petitions, as recorded on official record files from Maricopa County. Following the rationale of Blumstein, Farrington, and Moitra (1985), we divide the sample between those individuals with either 0 or 1 prior petition and those with 2 or more priors.

The choice to divide the sample by type of charge and number of prior petitions, as opposed to specific individual factors, is logical since policy debate about "redrawing the line" for transfer to adult court will likely begin with a consideration of identifiable subgroups based on legally relevant variables. Current charge and past offending history serve this purpose well. While obviously proxies for a variety of other offender characteristics, they are both readily ascertainable and legally relevant, as well as demonstrated indicators of likely future success in transferred youth (Schubert et al., 2009).

After dividing the full sample into subgroups based on charge group and priors, we are unable to actually estimate the conditional ATT, a group-specific treatment effect. This is due to the very limited sample size in each subgroup, which prevented us from employing propensity score methods as we do above. For the division based on charge type, there are only 45 transferred individuals in the property crime subgroup (with another 138 juvenile cases), and only 63 transferred individuals in the person crime subgroup (136 juvenile cases). For the division based on prior petitions, there are 65 transferred individuals with either 0 or 1 prior petitions (with another 180 juvenile cases), and there are 63 individuals with 2 or more (205 juvenile cases). These relatively small samples preclude the possibility of creating reasonable covariate balance in each divided sample, and thus determining a causal treatment effect. The breadth of covariates is too great for the restricted sample sizes to estimate a unique propensity score for each sub-sample.

Thus, we are limited to using a somewhat less sophisticated analytic approach. We simply regress future rate of re-arrest on binary transfer status, binary charge type (i.e., person as opposed to property), and an interaction of these two factors. Also, we control for the 16 covariates which were initially out of balance between the adult and juvenile groups to provide some reasonable attempt at controlling for selection (though by no means eliminating it). We repeat this same procedure replacing a binary marker for 0 or 1 prior petition, and its interaction with adult court, for charge type, again controlling for the 16 out of balance covariates.

Table 3 (column I) reports coefficients for the regression using person crime and its interaction with court status as explanatory terms. Column II reports the same coefficients controlling for the 16 covariates initially out of balance. Since the regressors of interest are each binary, their coefficients may be interpreted as differences in conditional mean rates,

[4]Note that there is a unique third group defined by charge type that includes minor sex offenders (not rape) and drug cases. We omit these cases from this section of the analysis for two reasons. First, the size of the group (n = 20) is too small to make valid inferences. Second, the grouping of offenses presents a wide array of offenses with little coherence, limiting interpretability.

[5]Again, note that those individuals charged with the most serious person crimes (i.e., murder, rape) are already excluded from this sample because they lack an observable outcome, since they are locked up for the entire follow-up period. The group used in these analyses, therefore, contains individuals who have committed relatively serious crimes against persons, but it does not necessarily contain those offenders found guilty of the gravest person crimes.

controlling for ostensible confounders. We again caution that these results cannot necessarily be interpreted as group-specific causal treatment effects, but rather exploratory evidence that such differential effects indeed exist. Notice that the coefficient for adult court is positive, suggesting that for individuals charged with property crimes (i.e., the base category) the rate of re-arrest is actually higher for those transferred than those retained in juvenile court, although this difference fails to approach conventional statistical significance. In contrast, note that there is a negative and significant interaction effect between adult court and person crime, which suggests that there is a reduction in offending associated with transfer for those who engage in person crimes, even when controlling for primary selection factors. This difference suggests that there may indeed be a differential effect of transfer, as the reduction in offending is greater for transferring person offenders as compared to the effect gained from transferring property offenders.

Columns III and IV of Table 3 report the same model using a binary delineation for 0 or 1 prior petitions (i.e., a low prior group) and its interaction with transfer status as the key explanatory terms of interest. Notice in this case, while there is no significant interaction effect of low priors and transfer, which would suggest a differential effect of transfer, there is a significant main effect of low priors which remains even after controlling for the initial selection factors. This negative effect can be interpreted as low priors being associated with a lower rate of future re-arrest for individuals in both adult and juvenile court, and it suggests that individuals with low priors tend to fare better than those with many priors regardless of the court setting. However, we are unable to reject a null hypothesis of a differential effect of transfer conditional on number or priors.

Discussion

This paper informs the debate over transferring serious juvenile offenders to adult court by estimating the effects of being transferred on future recidivism, in a manner designed to more rigorously control for selection biases than that found in previous analyses. Using data from a large sample of serious youthful offenders, we find two main results. First, across the entire sample of transferred individuals, there is a null effect of transfer on rate of re-arrest. Second, and more importantly, despite this overall null effect, we show initial evidence of possible differential effects of transfer, conditional on type of charge. The differential effects suggest that transferred adolescents charged with person crimes show *lower* rates of re-arrest, even after controlling for those covariates which are statistically significantly different between the adult and juvenile groups. The same result does not necessarily hold when the sample is split based on number of priors, as those individuals with 0 or 1 prior petition tend to fare better regardless of court jurisdiction.

These findings are compelling because the data from the Pathways study provide an excellent opportunity to study this question more carefully than possible in prior investigations. First, given that the sample is comprised entirely of serious juvenile offenders, the counterfactual outcomes are more appropriate than if we were to compare a sample of transferred individuals to less serious offenders. Also, as discussed above, the ability to rule out such a wide range of confounders is an improvement over much of the existing literature which had limited data on which to define a control group. Perhaps most importantly, the Pathways data provide a unique opportunity to more fully explore the issue of heterogeneity among transferred individuals, the importance of which has been emphasized by others (Bishop, 2000; Zimring, 1998). A considerable amount of variability exists within the Pathways sample of transferred youth in AZ, in both legal and certain risk-need factors as well as adjustment following involvement in the adult system (Schubert et al., 2009). As shown here, these differences in outcomes, at least in some types of offender subgroups, may also be attributable to whether the case was transferred or not.

Before addressing the relevance of these results to broader policy issues, though, we have to recognize the limitations of these analyses. As mentioned, we need to interpret our results, in particular the estimated coefficients for the charge group category, with ample caution, given the limited sample size involved. Furthermore, for the group-specific effects, we can control for, but not completely rule out, selection biases due to observable covariates. Thus, these group-specific results should be thought of as largely exploratory, and they should be replicated in a larger sample and in other jurisdictions. In addition, these analyses do not address the impact of transfer on drug or sex offenders. Although drug offenders may constitute a sizable proportion of transferred youth in some locales, the design of the Pathways study limited the number of drug offenders in the sample, thus limiting the applicability of these findings for generalized legal reform.

Another possible limitation is the screening of the most serious offenders from the sample (i.e., those charged with murder, rape or arson, all of whom spent the entire recall period in confinement), as it may be creating a biased estimate of the overall treatment effect of transfer. We argue, however, that this is not problematic, since there is little ambiguity regarding the issue of transfer for these individuals. Indeed, an individual in this age group charged with such a serious crime will almost always be transferred to adult court, regardless of the statutory scheme governing this practice. Thus, while the treatment effect we estimate does not include these individuals, it is unlikely that knowing the effect of transfer on this specific group would carry much sway in a discussion about recalibrating the transfer policy (c.f., Loughran & Mulvey, in press).

Furthermore, with respect to age, we caution against extrapolating these results outside of the age range of our transfer sample (ages 14–18, with less than 5% 14 year olds). It is important to note that only within this limited age spectrum do we find age to be unimportant. Further, we emphasize that, even if the current results were replicated and/or strengthened in future studies, these results still provide no relevant empirical basis for policy regarding transfer of youth younger than age 14.

Even in light of these issues, the present findings have considerable implications for juvenile justice policy. The dramatic differences in the effects suggest that, at the very least, researchers and policy makers must account for the heterogeneity within the group of transferred youth and the very real likelihood of differential effects related to this variability. Such an awareness redirects the policy debate away from a concern with whether transfer is “good” or “bad”, and toward a focus on where to “redraw the line” for determining transfer to do the most good and the least harm. This paper considers two specific forms of heterogeneity, person versus property crime charge and number of prior petitions. There are, however, several other policy-relevant ways to examine this heterogeneity, including, but not limited to, age, race, and gender.

Like other studies focusing on the effects of incarceration on subsequent criminal activity (Bhati & Piquero, 2007; Piquero & Blumstein, 2007), our results do suggest that, for some individuals, transfer may reduce criminal activity and, for others, it may increase it, although these increases are not statistically significant. As discussed above, the limited power of our sample precludes us from drawing sweeping policy conclusions about the absolute magnitude of either of these effects in the subgroups examined. If, however, following replication, it turns out that transfer increases the likelihood of offending for some subgroups of individuals and significantly reduces it for others, then this must be at the forefront of policy debate about the refashioning of transfer statutes.

The debate about the utility of transfer has to consider other factors not addressed in this study. In addition to the risk for increased re-offending, transferring youth charged with less

serious crimes might also promote several other problems, including labeling (Sampson & Laub, 1997; Smith & Paternoster, 1990) and developmental barriers (Steinberg & Cauffman, 2000), which are potentially costly to both the individual and society. In addition, any debate must address the substantial economic costs involved with housing juveniles and adults in correctional facilities. Whether expanding laws to include these less serious individuals has placed an unnecessary strain on the already thin resources of the criminal courts and adult correctional facilities is a key point (Bishop & Frazier, 2000). All of these issues deserve consideration in a newly energized discussion of how to reform, rather than refute, the practice of transferring juveniles to the adult court system.

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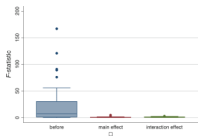

Fig. 1.
Balance tests before and after stratification—distributions of *F*-statistics

Table 1

Covariate descriptions and summaries

Construct	Covariate (scale)	Mean—juvenile	Mean—adult
Demographic	Age	16.22	17.00
	Male (%)	83	91
	White (%)	36	23
	Black (%)	7	9
	Hispanic (%)	51	59
	Other race (%)	6	9
	Parent's education	5.80	6.96
Household composition	Both biological parents present (%)	21	19
	Both biological parents present (%)	21	19
Intelligence	IQ	89.16	89.66
Employment	Employed (%)	26	30
Official record information	No. of priors—ever	2.89	3.46
	No. of priors—past year	1.85	1.62
	No. of priors—past 6 months	1.35	1.30
	Age of first prior petition	14.86	15.19
Gang involvement	Gang membership (%)	31	34
Early onset of behavior problems	No. of early onset behavioral problems	1.62	1.69
Services	Any overnight stays in a facility (%)	65	68
	Any involvement in community service (%)	56	55
Risk-need factors	Risk-need antisocial history	.03	.05
	Risk-need antisocial attitudes	.12	.01
	Risk-need mood/anxiety problems	.20	.18
	Risk-need parental antisocial history	.27	.26
	Risk-need association with antisocial peers	.04	.11
	Risk-need school difficulties	.42	.45
	Risk-need substance use problems	.97	.87
Trait anxiety	Total anxiety score—RCMAS	10.45	11.31
Substance use and mental health disorders	Alcohol abuse or dependency (%)	31	27
	Drug abuse or dependency (%)	46	44
	Presence of a selected mental health diagnosis (%)	16	15
Psychopathy	Psychopathy check list (PCL)—factor 1	5.28	5.20
	Psychopathy check list (PCL)—factor 2	8.28	8.95
Acculturation	Multi-group measure of ethnic identity—overall (1–4)	2.70	2.85
	Multi-group measure of ethnic identity—affirmation and belonging (1–4)	2.96	3.08
	Multi-group measure of ethnic identity—identity achievement (1–4)	2.35	2.53
Exposure to violence	Exposure to violence as a victim	1.59	1.98
	Exposure to violence as a witness	3.15	3.73
Psychological development	Consideration of others—Weinberger adjustment inventory (A.I.; 1–5)	3.33	3.51

Construct	Covariate (scale)	Mean—juvenile	Mean—adult
	Impulse control—Weinberger A.I. (1–5)	2.68	2.80
	Suppression of anger—Weinberger A.I. (1–5)	2.67	2.95
	Temperament—Weinberger A.I. (1–5)	2.68	2.87
	Psychosocial maturity index (PSMI)	2.95	2.92
	Resistance to peer influence (1–4)	2.83	2.93
Emotional reactivity	Walden—Self-regulation (1–4)	2.67	2.70
Social & personal costs and rewards of punishment	Certainty of punishment—yourself	5.73	5.21
	Certainty of punishment—others	5.91	5.47
	Punishment cost—variety	7.05	10.72
	Punishment cost—freedom issues	3.32	3.65
	Punishment cost—Material issues	3.73	7.07
	Social costs of punishment (1–5)	2.94	2.98
	Personal rewards to crime (0–10)	3.19	2.89
Perceptions of procedural justice	Legal cynicism (1–4)	2.05	2.06
Social support	Domains of social support (#)	6.62	6.78
Academic motivation	Motivation to succeed (1–5)	3.30	3.22
Moral disengagement	Moral disengagement (1–3)	1.66	1.62
Community involvement	Involvement in community activities—past 6 months (%)	27	24
Routine activities	No. of unsupervised routine activities	3.85	3.77
Personal capital and social ties	Social capital—closure and integration (1–4)	2.35	2.28
	Social capital—perceived opportunity for work (1–5)	3.44	3.33
	Social capital—social integration (1–4)	1.98	2.00

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Table 2

Initial covariate imbalance

Covariate	*F*-stat	SBS	Covariate	*F*-stat	SBS
Age	65.851	80*	Personal rewards to crime	1.355	12
Punishment cost—material	35.204	64*	Motivation to succeed	1.158	11
Punishment costs—variety	26.078	55*	Moral disengagement	1.151	11
Ethnic identity—achievement	9.601	32*	Domains of social support	1.136	10
Ethnic identity—overall	8.782	31*	Employed?	1.044	11
Exposure to violence—witness	8.162	29*	Parent's education	1.002	11
Male	7.815	26*	No. routine unsupervised activities	0.935	10
Weinberger—suppression of anger	7.803	29*	Risk-need: peer	0.921	10
White	7.651	27*	Other race	0.897	10
Exposure to violence—victim	5.984	25*	Alcohol use/dependency	0.713	9
Ethnic identity—affirmation	5.639	24*	Psychosocial maturity	0.556	7
Weinberger—temperament	5.585	24*	Any overnight stays in secure facility?	0.423	7
No. of priors in the past year	5.569	22*	Gang membership?	0.375	6
Weinberger—consideration of others	5.455	23*	Risk-need: mental health	0.332	6
No. of priors—ever	5.059	24*	Both biological parents present	0.321	6
Certainty of punishment—others	4.336	21*	Early onset?	0.275	5
Age at first prior	3.320	19	Walden—self-regulation	0.264	5
Certainty of punishment—self	3.279	19	No. of priors—past 6 months	0.260	5
Punishment cost—freedom issues	2.819	18	Risk-need: parent	0.232	5
Hispanic	2.811	17	Social cost of crime	0.228	5
PCL factor 2	2.554	17	Black	0.220	5
Resistance to peer influence	2.228	15	IQ	0.171	4
Risk-need: attitudes	2.013	14	Involved in community activity	0.167	4
Weinberger—impulse control	1.946	14	Social capital—social integration	0.158	4
Total anxiety scores	1.762	14	Mental health diagnosis—ever	0.152	4
Social capital—closure and integration	1.712	14	Drug use/dependency	0.101	3

Covariate	*F*-stat	SBS	Covariate	*F*-stat	SBS
Social capital—perceived opportunity for work	1.679	14	Any community service?	0.094	3
Risk-need: substance use	1.474	12	Risk-need: antisocial history	0.094	3
Risk-need: schooling	1.374	12	PCL factor 1	0.047	2
			Legal cynicism	0.018	1

* Denotes covariate is initially out of balance

Table 3

Differences in re-arrest by subgroup

	I	II		III	IV
Adult?	0.248 (0.269)	0.134 (0.271)	Adult?	0.016 (0.210)	–0.157 (0.215)
Person crime?	–0.262 (0.19)	–0.105 (0.182)	0 or 1 prior?	–0.495 (0.148)	–0.479* (0.200)
Adult* person	–0.509 (0.360)	–0.875** (0.345)	Adult* 0 or 1 prior	–0.106 (0.296)	–0.236 (0.285)
Age		0.123 (0.077)	Age		0.134* (0.062)
Punishment cost—material		–0.016 (0.067)	Punishment cost—material		–0.006 (0.055)
Punishment cost—variety		0.062 (0.053)	Punishment cost—variety		0.042 (0.044)
Ethnic identity—achievement		–1.384 (5.381)	Ethnic identity—achievement		–1.335 (4.733)
Ethnic identity		3.920 (12.940)	Ethnic identity		3.913 (11.378)
Exposure to violence—witness		0.054 (0.050)	Exposure to violence—Witness		0.052 (0.041)
Male?		0.395 (0.236)	Male?		0.477** (0.172)
Suppression of anger		0.095 (0.166)	Suppression of anger		0.051 (0.131)
White?		0.038 (0.180)	White?		0.058 (0.140)
Exposure to violence—victim		0.001 (0.068)	Exposure to violence—victim		–0.029 (0.054)
Ethnic identity—achievement		–2.497 (7.560)	Ethnic identity—achievement		–2.584 (6.643)
Temperament		–0.185 (0.196)	Temperament		–0.130 (0.157)
No. of priors past year		0.324** (0.074)	No. of priors past year		0.296** (0.064)
Consideration of others		–0.056 (0.095)	Consideration of others		–0.090 (0.080)
No. of priors ever		0.008 (0.041)	No. of priors ever		0.093* (0.046)
Certainty of punishment—others		–0.083* (0.039)	Certainty of punishment—others		–0.066* (0.031)
Intercept	1.142 (0.134)	–1.502 (1.400)	Intercept	1.195 (0.108)	–2.021 (1.163)
R-squared	0.022	0.225	R-squared	0.032	0.190

*Denote statistical significance at 0.05, 0.01, respectively

**Denote statistical significance at 0.05, 0.01, respectively