

## LABELING EFFECTS OF FIRST JUVENILE ARRESTS: SECONDARY DEVIANCE AND SECONDARY SANCTIONING\*

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*A growing literature suggests that juvenile arrests perpetuate offending and increase the likelihood of future arrests. The effect on subsequent arrests is generally regarded as a product of the perpetuation of criminal offending. However, increased rearrest also may reflect differential law enforcement behavior. Using longitudinal data from the Project on Human Development in Chicago Neighborhoods (PHDCN) together with official arrest records, the current study estimates the effects of first arrests on both reoffending and rearrest. Propensity score methods were used to control differences between arrestees and nonarrestees and to minimize selection bias. Among 1,249 PHDCN youths, 58 individuals were first arrested during the study period; 43 of these arrestees were successfully matched to 126 control cases that were equivalent on a broad set of individual, family, peer, and neighborhood factors. We find that first arrests increased the likelihood of both subsequent offending and subsequent arrest, through separate processes. The effects on rearrest are substantially greater and are largely independent of the effects on reoffending, which suggests that labels trigger “secondary sanctioning” processes distinct from secondary deviance processes. Attempts to ameliorate deleterious labeling effects should include efforts to dampen their escalating punitive effects on societal responses.*

The 1980s and the early 1990s were characterized by an “epidemic” of youth violence in the United States, which peaked in 1993–1994 (Cook and Laub, 2002). Policy responses to the epidemic included a shift from the traditional rehabilitative goal of juvenile justice toward more retributive goals (e.g., Allen, 2000) under the mantra of “old enough to do the crime, old enough to do the time.” The jurisdiction of the juvenile court was curtailed through lowered age of criminal responsibility, legislative exclusion of various age–charge combinations from juvenile court jurisdiction, and increased prosecutorial discretion to

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“direct file” cases in adult court (see Fagan and Zimring, 2000). The wisdom of retaining a separate juvenile court system was also debated (e.g., Ainsworth, 1995; Bishop, 2004; Butts and Mitchell, 2000; Dawson, 1990; Feld, 1998).

States have begun to step away from the punitive philosophy of late twentieth-century juvenile justice (National Conference of State Legislatures, 2012; U.S. Department of Education, 2014) after the decline in youth crime and violence over the past two decades. Juvenile violent offending rates are now at historic lows, and the latest arrest data from the U.S. Office of Juvenile Justice and Delinquency Prevention (2012) are down 55 percent from its mid-1990s peak. However, the decline in violent crime among juveniles outpaced the decline in arrests of violent juveniles through at least the early 2000s, so that the ratio of juvenile violent crime arrests to violent victimizations by juveniles increased from approximately .72 in 1980, to approximately 1.0 in the early 1990s, and to approximately 1.45 by 2003 (Snyder and Sickmund, 2006: 64). Despite massive declines in juvenile crime and violence over the past two decades, a convincing case can be made that U.S. society is still very much “governed through crime,” with a youth control complex that criminalizes juveniles at an extraordinary level (Rios, 2011; Simon, 2007).

The current study examines the collateral consequences of this criminalization of youth and revisits a question that has captivated and challenged criminologists for some time: What is the effect of arresting juveniles? Two theoretical perspectives provide opposing answers to this question. Deterrence theory predicts that arrests will have the specific deterrent effect of reduced offending (e.g., Smith and Gartin, 1989), whereas labeling perspectives predict that arrests will lead to increased offending and criminal sanctioning (e.g., Lemert, 1951). A third perspective, Gottfredson and Hirschi’s (2007) self-control theory, argues that a lack of self-control explains any apparent relationship between system responses such as arrest and subsequent behavior, so that the relationship between juvenile arrest and reoffending is spurious. To date, the empirical literature has revealed little support for specific deterrence. The literature is largely split between null findings, in accord with self-control theory, and findings that seem to show that arresting juveniles is associated with more subsequent offending, in accord with labeling theory.

In view of the literature, a preliminary aim of this study is to test the replicability of the labeling effects previously reported. Through the use of propensity score methods combined with the necessary sensitivity analyses, this study aims to minimize the selection-bias threats to validity that are common in nonexperimental studies. The study draws on the broad data on youth, family, peer, and neighborhood characteristics collected as part of the Project on Human Development in Chicago Neighborhoods (PHDCN). The comprehensiveness of the PHDCN allows us to account for many confounding influences that distinguish arrestees from nonarrestees in estimating the relationship between juvenile arrest and future offending.

The primary aim of this article is to distinguish between two types of potential labeling effects: the effects of labels on delinquent behavior versus the effects of labeling on societal responses to the label, particularly via future sanctioning. This broadens our exploration of labels to include their effects both on deviant behavior as well as on society’s response to misbehavior.

Per these two aims, we ask the following research questions: Does the first arrest of a juvenile increase the likelihood of future offending? Does it increase the likelihood of subsequent arrest? Does juvenile arrest increase the likelihood of subsequent arrest even after accounting for any increases in offending? Put differently, does a first juvenile

arrest increase subsequent arrests even if the arrestee does not engage in more subsequent offending than a similar nonarrestee?

## LABELING EFFECTS ON DELINQUENT BEHAVIOR AND ON SYSTEM RESPONSE

Generally, labeling theory predicts that an official response to delinquency promotes future delinquency (e.g., Lemert, 1951). Labeling theory includes two different mechanisms by which a label can lead to increased deviancy (Paternoster and Iovanni, 1980). In one strand of labeling theory, the primary mechanism is that a delinquent label redirects a youth's self-conception or personal identity toward a deviant self-concept, which is then self-fulfilling (e.g., Matsueda, 1992). Edwin M. Lemert's (1951) version of labeling theory is emblematic of this process, particularly his depiction of the progression from "primary deviance" to "secondary deviance." Individuals come to internalize the deviant status stemming from societal reaction to their behavior, and deviants come to organize their lives around this status (see also Becker, 1963; Schur, 1973). Labeled deviants may then associate with more deviant peers (Wiley, Slocum, and Esbensen, 2013), withdraw from conventional pursuits (Bernburg, 2009; Lopes et al., 2012), and ultimately engage in criminal offending at a higher rate than otherwise similar individuals who have not been labeled "deviant." With this higher rate of offending, stigmatized youth would presumably have more frequent interaction with the criminal justice system than nondeviants.

Another mechanism in labeling theory focuses more on external processes involving social and societal responses to the label, including increased surveillance as well as reduced social opportunities and interactions (e.g., Klein, 1986; Link et al., 1989; Paternoster and Iovanni, 1989). Here, the mechanisms are not internal to the labeled individual but are rather direct external social and societal responses. In a parallel to Lemert's terms of primary versus secondary deviance, we conceptualize the labeling event—here, an arrest—as a "primary sanction" and subsequent punitive societal responses resulting from the label as "secondary sanctions." This terminology is intended to capture the idea that two parallel processes may operate in reaction to a deviant label, one internal and one external.

Representative of this version of labeling theory, Sampson and Laub's (1997) life-course theory of cumulative disadvantage emphasizes that once an individual is labeled a deviant, a variety of detachment processes are set in motion that promote the likelihood of further deviance. The stigma of a criminal record undermines social control processes, whether or not the labeled deviant internalizes the deviant status as in the Lemert framework. Sampson and Laub (1997: 147) noted, "The theory specifically suggests a 'snowball' effect—that adolescent delinquency and its negative consequences (e.g., arrest, official labeling, incarceration) increasingly 'mortgage' one's future, especially later life chances molded by schooling and employment."

Several recent studies have shown evidence of such secondary sanctioning processes. For instance, Kirk and Sampson (2013) suggested that an arrest record officially marks a juvenile as a "criminal" and changes the way educational institutions treat the student. Students with criminal records are often pushed out of high school through exclusionary policies and segregated into specialized programs for problem youths. The result of the primary sanction (arrest) and the secondary sanction (school exclusionary policies and practices) is an increased likelihood of high-school dropout and diminished prospects

for going to college (e.g., Bernburg and Krohn, 2003; Hirschfield, 2009; Kirk and Sampson, 2013; Sweeten, 2006), thereby leading to a greater likelihood of future criminality. Similarly, the stigma of a criminal record drastically influences how former offenders are treated by potential employers, and the denial of employment represents a form of secondary sanctioning (Laub and Sampson, 2003; Pager, 2003; Schwartz and Skolnick, 1962).

Moreover, if labeling effects operate through differential social or societal responses to those labeled as deviant, then a labeled individual may have more frequent interactions with the criminal justice system even if his or her criminal offending does not increase after an arrest (relative to otherwise similar “nondeviants” who avoided an arrest record). As Petrosino, Turpin-Petrosino, and Guckenburg (2010: 9) put it, “The same actions that resulted in police turning a blind eye to misconduct may now result in an arrest.” Such secondary sanctioning processes fit broadly under the realm of labeling theory but offer slightly different predictions than classic versions of labeling that stress identity internalization, or even Sampson and Laub’s (1997) version, which stresses a decline in social controls. The essential difference is that the stigmatized deviant may not engage in crime at a higher rate after an arrest relative to an otherwise similar individual who managed to avoid arrest, but the stigmatized deviant would still be rearrested and sanctioned more often because of the intensified gaze, or declining tolerance, of the criminal justice system.

## PRIOR RESEARCH ON THE LABELING EFFECTS OF JUVENILE ARRESTS

Few studies of the effects of arrest, whether in experimental or observational studies, have simultaneously examined both secondary deviance (subsequent delinquency) and secondary sanctioning (subsequent justice-system responses). Most experimental studies have relied solely on administrative outcome data and have generally taken official data (arrests) as an indicator of offending behavior *per se* without distinguishing between effects on offending behavior (secondary deviance) versus effects on later system response (secondary sanctioning). Petrosino, Turpin-Petrosino, and Guckenburg (2010) recently conducted a meta-analysis of the effects of formal responses to juvenile delinquency limited to studies with random assignment (or quasi-random assignment) of juveniles to either traditional processing versus release or some form of diversion. Overall, the meta-analysis found that formal sanctioning was associated with more reoffending across self-report and official measures. Of the studies reviewed, 13 addressed the question of immediate interest in this study by comparing juveniles who received traditional processing—beginning with a formal arrest—with juveniles who were “released” or “counseled and released” without additional programming (see Petrosino, Turpin-Petrosino, and Guckenburg, 2010; table 8.6).<sup>1</sup> All but 1 of these 13 experimental studies used official arrest measures.

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1. The extent of formal processing varied among these studies. “Because the system processing condition is usually the control group in the experiments, it often is not described further” (Petrosino, Turpin-Petrosino, and Guckenburg, 2010: 13), and it was described just as “processing” in nearly two thirds of the studies reviewed (p. 22). Among studies in which the control group was “released” or “counseled and released,” the commonality among the traditional processing conditions seems to have been a formal arrest. This is similar to studies of “arrest” *per se*, in which system penetration after arrest is generally unknown.

One of the few experimental studies to measure both self-reported offending (SRO) and official arrests was conducted by Klein (1986). Youth identified by police were randomly assigned to be counseled and released, petitioned, or referred to one of two diversion conditions. Nine months later, no effects were found on youths' SRO or their agreement with descriptions of themselves that "encapsulated" a delinquent label, but formally petitioned youth were more likely to have been rearrested.

In contrast to the experimental studies, most longitudinal studies have relied on SRO outcomes and have not explored system responses. In a recent review, Huizinga and Henry (2008) identified 19 longitudinal studies of the effects of arrest with reasonable attempts to control for selection; most of these studies relied solely on SRO. Approximately half of these studies found no effect of arrest; the other half seemed to find labeling effects on SRO.

Longitudinal studies can allow confident establishment of temporal ordering. However, selection bias remains a persistent challenge to nonexperimental labeling findings, even in longitudinal designs. In addition to lower self-control (Gottfredson and Hirschi, 1990), arrested youth typically differ from nonarrestees in many ways that predispose them to greater offending, including individual-level risk factors, as well as family, peer, and neighborhood risk. To control this selection bias, approximately half of the studies reviewed by Huizinga and Henry (2008) included predisposing factors to arrest as control variables in regression models, and approximately half used matching strategies.

One of the most extensive matched longitudinal studies was conducted by Huizinga et al. (2003). Arrest during adolescence was examined in two longitudinal samples, from Denver (Colorado) and from Bremmen (Germany). Each arrestee was matched to a nonarrestee who was most similar on gender, age, minority status, annual delinquency since 14 years of age, history of arrest, and annual history of delinquent peer involvement.<sup>2</sup> With this matching, adolescent arrest showed little effect on subsequent SRO but sanctions as an adolescent were related to increased unemployment as a young adult.

Propensity score methods have recently been used to improve control over selection bias by allowing matching on many risk variables simultaneously (Rosenbaum, 2002). Rather than matching on a few select variables, one matches on a summary measure (i.e., the "propensity" of arrest) that is computed from many variables simultaneously. This approach addresses one limitation of traditional (exact) matching, where one typically can only match on a few variables simultaneously. Three recent longitudinal studies have applied propensity-score methods to the question of the labeling effect of arrest. Wiley and Esbensen (2013; also Wiley, Slocum, and Esbensen, 2013) used student survey data from the second national evaluation (2006–2013) of the Gang Resistance Education and Training (GREAT) program. Controlling for 17 prearrest covariates, they found that youth who report having been arrested subsequently report significantly more offending.

McAra and McVie (2007) used data from the Edinburgh Study of Youth Transitions and Crime to explore the effects of three stages of formal processing after police contact (charging by police, "referral to Reporter," and being brought to a hearing). Charging and referral did not affect SRO, but being brought to a hearing was associated with significantly more SRO. Thus, in a rare study where arrest and prosecution could be disentangled, the study found no effect of arrest *per se* but a labeling effect of prosecution.

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2. Neighborhood type also was used as a matching variable in the Denver sample.

Theoretically, labeling effects should be strongest for the first labeling event, and each repeated labeling event should have a smaller marginal effect. The difference between having an arrest history or not should be larger than the difference between having three versus four prior arrests. Conversely, deterrence should also be strongest for the first arrest. Because most empirical studies fail to distinguish those newly labeled from those being labeled repeatedly, they may have inadvertently conducted weak tests of the marginal effects of additional arrests (Paternoster and Iovanni, 1989). This weak test may be one contributing factor to the many null findings in the literature.

One of the few studies to isolate first arrests was Morris and Piquero's (2013) analysis of National Youth Study data. Selection was controlled by using propensity score matching within groups with similar prior-offending trajectories (see Haviland et al., 2008). The first arrests reported at wave V (1980) were found to increase SRO at wave VI (1983). This effect was most substantial with chronic delinquents and negligible for youth with little prior offending.

In sum, the prior literature generally has found that arrest increases subsequent offending, as predicted by labeling theory, or has found no effect at all. Generally, experimental studies have shown labeling effects on rearrest but have not distinguished offending from sanctioning. Longitudinal studies have shown effects on SRO. Some recent studies have reaffirmed the basic labeling effect using propensity score methods to control for selection bias, but none of these recent studies has examined secondary sanctioning.

## THE CURRENT STUDY

The literature has not clearly delineated whether the effect of arrest on rearrest is primarily the product of the indirect effect of arrest through subsequent offending or whether the first juvenile arrest independently increases rearrest beyond any effect through future offending. This article aims to distinguish labeling effects on reoffending (secondary deviance) from labeling effects on rearrest (secondary sanctioning).

To study the effects of juvenile first arrests on both subsequent SRO and rearrest, we use data from the PHDCN, linked to official arrest data from the Chicago Police Department (CPD) and the Illinois State Police (ISP). Using the PHDCN data, we construct propensity scores for matching arrestees and nonarrestees using a rich set of covariates, including measures of prior offending, temperament, family circumstances, demographics, education, peer influences, and neighborhood characteristics. In contrast to most prior studies, we restrict this study to first arrests.

We test three hypotheses:

*Hypothesis 1:* A first juvenile arrest has an independent positive effect on subsequent delinquency and criminal offending above and beyond the influence of individual, family, peer, neighborhood, and school correlates.

*Hypothesis 2:* Arrested adolescents are more likely to be arrested in the future than otherwise similar youth without arrest records.

*Hypothesis 3:* The effect of first arrest on future arrests is independent of the effect of a first arrest on criminal offending. That is, adolescents with a previous arrest are more likely to be arrested in the future than comparable nonarrestees even if they engage in similar levels of future delinquency.

## DATA AND METHODS

### SAMPLE

Our sample comes from the PHDCN's longitudinal cohort study, which involved three waves of data collection from seven cohorts of youth at 3-year age intervals (i.e., 0, 3, 6, 9, 12, 15, and 18 years of age at wave I). The first wave of interviews was conducted in 1995 through 1997, and subsequent waves were separated by approximately 2.5 years, with the third interviews occurring approximately 5 years later (from January 2000 to January 2002).

For the longitudinal study, the PHDCN selected a sample of 80 neighborhood clusters, stratified by racial/ethnic composition (seven categories) and socioeconomic status (high, medium, and low), from a total of 343 neighborhood clusters in Chicago (Sampson, Raudenbush, and Earls, 1997). Within these 80 neighborhood clusters, a simple random sample of households yielded a total sample of 1,517 youth in the 12-year-old and 15-year-old cohorts. We specifically focus on these two cohorts because our study examines the effect of arrest as a juvenile.

For rearrest outcomes, our sample consists of 1,249 youth—58 arrestees (the treatment group) and 1,191 nonarrestees—who completed the wave I SRO questionnaire and consented to the official records search. This sample excludes 34 youth who failed to respond to the wave I SRO questionnaire, as well as 234 youth who did not consent to the official records search. Prior research comparing PHDCN respondents who did or did not consent to the records search found no systematic difference on a measure of self-reported arrest (Kirk, 2008).

Wave III participation was not necessary for our rearrest outcomes; for SRO outcomes, wave III attrition slightly reduced the sample to 53 arrestees and 951 nonarrestees. Wave III attrition was lower among arrestees (8.6 percent; 5 youth) than nonarrestees (20.2 percent; 240 youth). The sample of arrestees was distributed across 39 of the 80 neighborhood clusters; only 1 neighborhood cluster contained more than three arrestees.

### DESIGN

Reliably establishing the temporal order of pretreatment propensities, treatment, and outcomes is a key requirement for a methodologically strong quasi-experimental study of the causal effects of arrests. We capitalize on the three-wave structure of the PHDCN longitudinal study and dates in the official arrest data to ensure that measures used to predict our treatment condition—first arrest—were indeed measured prior to treatment and that the reoffending and rearrest outcomes follow the treatment. Pretreatment characteristics used in propensity models were measured at wave I; the treatment was restricted to a window between waves I and III, and our self-report outcome is measured at wave III. Arrest outcomes are contemporaneous with the wave III SRO period.

### TREATMENT VARIABLE

Administrative data on arrests from the CPD and the ISP were obtained and merged with the PHDCN data. These data span 1995 to 2001 and include both juvenile and adult

arrest data throughout Illinois. Identifying information used in matching the data sets includes social security number, name, birth date, county and zip code, race and ethnicity, and gender. To construct our treatment variable, we determined whether each given PHDCN survey respondent was officially arrested as a juvenile (per the CPD and ISP data) for the first time sometime between their wave I interview date and 15 months preceding the wave III PHDCN interview. This approach allows a 3-month buffer against recall error before the beginning of the 12-month recall period used for the SRO outcomes in wave III (i.e., at the wave III interview, respondents were asked about offending during the prior 12 months). With approximately 5 years between the first and third waves of data collection, this arrest window was approximately 45 months (60 months minus 15 months).

## OUTCOME VARIABLES

Subjects were asked at the wave III interview whether they had engaged in each of 22 behaviors during the preceding 12 months and, if so, how many times. As a general offending measure, we calculate the variety of offending across the 22 items, which counts the number of different types of criminal acts in which the person engaged at least once. The 22 items consist of 6 violent offenses, 8 property offenses, 3 drug-selling offenses, 3 public-order offenses, and 2 status offenses.<sup>3</sup> In addition to the offending variety score, we use as outcome variables separate measures of the prevalence of violent, property, and drug-sales offending.<sup>4</sup>

Official arrest records were used as the secondary sanctioning outcome, using the same arrest data from CPD and ISP described earlier. We constructed a binary prevalence measure indicating whether each individual was arrested at any point from 12 months prior to the wave III interview date and the end of 2001, which was the last available extract of data on arrests from CPD and ISP.

## PROPENSITY SCORE MATCHING

Most adolescents commit some delinquent acts (Porterfield, 1943; Short, 1958; Short and Nye, 1957; Wallerstein and Wyle, 1947), but as Tannenbaum observed (1938: 19), “Only some of the children are caught.” Only a minority of delinquent acts is detected and fewer lead to arrests. Law enforcement officers exercise considerable discretion regarding whether and when to initiate a formal arrest. In their classic study, Black and Reiss (1970) found that only 15 percent of police contacts with juveniles resulted in an arrest. To a large extent, then, the arrest decision lies with the police and is based on a

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3. *Violent Offenses* include the following: carried a hidden weapon, hit someone not lived with, attacked someone with a weapon, used force to rob, threw objects at people, and was in a gang fight. *Property Offenses* include the following: damaged property, set a fire, broke into a building to steal, stole from a store, stole from household member, snatched a purse, stole from a car, and bought/sold stolen goods. *Drug-Selling Offenses* include the following: sold marijuana, sold cocaine/crack, and sold heroin. *Public-Order Offenses* include the following: caused trouble in public, paid for sex, and received a driving ticket. *Status Offenses* include the following: ran away from home overnight and drove without a license.
  4. We explored the frequency of offending in these three items. The results from an analysis of offending frequency were substantively similar to the prevalence results.



host of external factors beyond the criminal behavior of the arrested individual. In this sense, juvenile arrest has a random component, making it likely that for each arrested individual in the PHDCN sample, there are one or more equivalent nonarrestees, in terms of criminal offending and other pretreatment covariates, who were fortunate enough to avoid arrest. We exploit this randomness in juvenile arrest to determine whether arrest is causally related to subsequent offending and rearrest.

Using the PHDCN's extensive data on pretreatment covariates, we used propensity score matching to identify comparison youth who were otherwise similar to treated (i.e., arrested) youth by modeling the probability of arrest. Propensity score methods allow the creation of balanced treatment and control groups who are equivalent on all measured covariates (Rosenbaum and Rubin, 1983). Given a set of covariates that account for the features associated with selection into the treatment condition, this approximates an experimental design (Morgan and Winship, 2007; Rosenbaum, 2002). Importantly, however, whether the set of covariates is sufficient to account for selection into treatment cannot be empirically determined. Instead, as with other regression or matching approaches, judgments of the sufficiency of the control variables must be assessed on a priori grounds based on theory and understanding of the treatment under consideration. In addition, sensitivity tests allow one to examine the sensitivity of the results to possible omitted variables. The use of sensitivity analyses is critical when employing propensity score methods, particularly in analyses where relatively few covariates are used to estimate the propensity score, but even in our analysis that draws on a vast array of pretreatment covariates. We use Rosenbaum's (2002, 2010) bounding approach for this purpose.

Propensity scores were constructed from 79 wave I covariates. SRO variables from the first wave of interviews, constructed identically to the SRO outcome variables from wave III interviews, were included as pretreatment covariates predicting the probability of arrest. We also included similarly constructed wave I SRO variables concerning status and public order offending in constructing the propensity score. In addition, pretreatment variables included drug use, temperament, household composition, parenting characteristics, socioeconomic status (SES), demographics, education, and peer influences. Neighborhood characteristics also were included, from the 1995 PHDCN Community Survey, U.S. Census data, and reported crime data (Sampson, Raudenbush, and Earls, 1997). To estimate the effect of a first juvenile arrest on future offending and arrest, we statistically match and then compare arrested and nonarrested sample members who are otherwise similar to one another in their frequency of criminal offending and all the pretreatment characteristics.

Before creating propensity scores, multiple imputation procedures were used to impute missing values for wave I pretreatment variables. Approximately 24 percent of the cases had at least one missing value. Multiple-chain imputation was implemented through Stata 12's (StataCorp, College Station, TX) MI procedure to create five imputed data sets. We followed Hill's (2004: 13) multiple-imputation matching strategy and calculated a propensity score for each observation in each imputed data set, using the `mi estimate` and `mi predict` commands in Stata 12. We then averaged the propensity scores for each respondent across the five imputed data sets.

Propensity score matching was done using nearest-neighbor 3:1 matching, with replacement, with a caliper set at .02. That is, each arrested youth was matched with up to three otherwise similar nonarrested youth who had a propensity of arrest (i.e., a predicted

probability of arrest) within .02 of the arrested youth. Matching was accomplished via Stata's `psmatch2` routine. The resulting matched samples for arrest outcomes consisted of 43 arrestees and 126 nonarrestees; because we matched with replacement, the 126 control matches include 103 unique control cases. For SRO outcomes, the matched samples consisted of 38 arrestees and 111 nonarrestees; these control matches include 80 unique control cases.<sup>5</sup>

Linear models of the effect of first arrest on offending variety were estimated directly by `psmatch2`. With binary outcomes (the prevalence of offending as well as rearrest), `psmatch2` was used to identify matches; then, the matched samples were analyzed via logit models while accounting for matching. That is, each treatment observation and its corresponding control matches represent a cluster, and logit models were estimated while accounting for this clustering.<sup>6</sup>

## RESULTS

### PRETREATMENT DIFFERENCES

An important early question concerns the similarity or difference among youth who are and are not arrested. Arrested youth significantly differed from nonarrestees on 34 of 79 covariates examined. Table 1 shows differences in individual-level factors in the left panel, and tables 2 and 3 display differences among family, peer, and neighborhood-level factors.

Prevalence of offending at wave I among arrested youth was significantly greater across all offending categories (violence, 60 percent; property, 22 percent; drug-sales, 14 percent; public order, 21 percent; status, 64 percent; mean offending variety = 4.28) than among control youth (violence, 15 percent; property, 9 percent; drug sales, 2 percent; public order, 11 percent; status, 12 percent; mean offending variety = 1.80). In addition, arrestees differed significantly on variables in other domains. Arrested youth were significantly more likely to smoke cigarettes, with a marginally significant difference in marijuana use as well. In terms of temperament, arrested youth had lower inhibitory control, were more sensation seeking, and were quicker to make decisions (i.e., a form of impulsivity). As noted by Gottfredson and Hirschi (1990), these aspects of self-control are likely confounders related to both the likelihood of first arrest and future behavior. Accounting for these aspects of control and temperament are essential when attempting to estimate the unbiased effects of arrest.

Among demographic variables, arrested youth were significantly more likely to be male, older, and African American, and they were less likely to be Mexican or second-generation immigrants (compared with third or later generations). First-generation

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5. Reestimating the rearrest model using the smaller SRO sample generated equivalent results to those presented (available upon request).

6. Stata code for the logit models is as follows: `logit outcome_var treatment_var (pweight = controlweight), vce(cluster clusterid)`. Each treated case and its matched control cases have a common, unique cluster identification (`clusterid`); `vce(cluster)` accounts for this clustering. Control cases are downweighted if more than one match to a given treatment case; `controlweight = 1` for one match, .5 for two matches, and .333 for three matches. Treatment cases' weight = 1.

**Table 1. Differences Between Arrestees and Nonarrestees, Before and After Matching: Individual-Level Factors**

| Wave I Variable:<br>Individual Factors | Unmatched        |                         |                  |                             | Matched          |                         |                  |                             |
|--|------------------|-------------------------|------------------|-----------------------------|------------------|-------------------------|------------------|-----------------------------|
|  | Arrested<br>Mean | Not<br>Arrested<br>Mean | <i>t</i><br>Test | Percent<br>Absolute<br>Bias | Arrested<br>Mean | Not<br>Arrested<br>Mean | <i>t</i><br>Test | Percent<br>Absolute<br>Bias |
| SRO and Aggression                     |                  |                         |                  |                             |                  |                         |                  |                             |
| Violent offending                      | .60              | .15                     | 9.30*            | 84.9                        | .45              | .42                     | .17              | 4.5                         |
| Property offending                     | .22              | .09                     | 4.54*            | 45.3                        | .15              | .16                     | -.15             | 3.4                         |
| Drug selling                           | .14              | .02                     | 5.78*            | 41.4                        | .07              | .06                     | .10              | 2.0                         |
| Public order offending                 | .21              | .11                     | 2.68*            | 32.2                        | .18              | .26                     | -.94             | 26.0                        |
| Status offending                       | .64              | .12                     | 10.13*           | 97.9                        | .42              | .47                     | -.38             | 9.1                         |
| Variety score                          | 4.28             | 1.80                    | 7.42*            | 82.5                        | 3.50             | 3.40                    | .13              | 3.2                         |
| Delinquent score (Achenbach)           | 4.98             | 3.24                    | 4.34*            | 56.1                        | 4.34             | 3.97                    | .53              | 12.1                        |
| Aggressive score (Achenbach)           | 9.83             | 8.83                    | 1.16             | 15.0                        | 9.61             | 9.74                    | -.08             | 2.0                         |
| Temperament                            |                  |                         |                  |                             |                  |                         |                  |                             |
| Inhibitory control                     | 2.80             | 2.41                    | 2.94*            | 39.1                        | 2.68             | 2.68                    | .01              | .1                          |
| Impulsivity                            | 3.19             | 2.98                    | 1.87             | 25.8                        | 3.06             | 3.01                    | .25              | 6.0                         |
| Sensation seeking                      | 3.07             | 2.71                    | 3.33*            | 47.7                        | 2.93             | 2.84                    | .52              | 12.5                        |
| Activeness                             | 3.80             | 3.57                    | 1.77             | 26.2                        | 3.70             | 3.47                    | 1.12             | 26.5                        |
| Emotionality                           | 2.81             | 2.67                    | .93              | 12.9                        | 2.74             | 2.58                    | .64              | 14.5                        |
| Sociability                            | 3.69             | 3.64                    | .48              | 7.3                         | 3.66             | 3.61                    | .28              | 6.6                         |
| Shyness                                | 2.25             | 2.48                    | -1.91            | 28.5                        | 2.17             | 2.47                    | -1.64            | 35.6                        |
| School and Education                   |                  |                         |                  |                             |                  |                         |                  |                             |
| Ever repeated grade                    | .22              | .15                     | 1.30             | 18.3                        | .25              | .18                     | .64              | 16.8                        |
| Ever remediation class                 | .33              | .29                     | .58              | 8.6                         | .31              | .32                     | -.03             | .8                          |
| School truancy                         | .50              | .16                     | 4.37*            | 50.1                        | .30              | .45                     | -.81             | 22.1                        |
| IQ (standardized)                      | 96.89            | 100.78                  | -1.83            | 28.8                        | 97.86            | 98.94                   | -.34             | 8.0                         |
| School mobility                        | 3.13             | 2.48                    | 3.69*            | 49.9                        | 2.82             | 2.74                    | .27              | 5.7                         |
| Drug Use                               |                  |                         |                  |                             |                  |                         |                  |                             |
| Days marijuana last month              | 1.30             | 1.11                    | 2.05*            | 23.4                        | 1.21             | 1.12                    | .74              | 10.8                        |
| Days cigarettes last month             | 1.64             | 1.22                    | 3.07*            | 33.5                        | 1.58             | 1.43                    | .47              | 11.7                        |
| Days alcohol last month                | 1.25             | 1.12                    | 1.88             | 25.3                        | 1.24             | 1.12                    | .93              | 23.2                        |
| Demographics                           |                  |                         |                  |                             |                  |                         |                  |                             |
| Gender                                 | .72              | .45                     | 3.75*            | 55.1                        | .66              | .63                     | .28              | 6.4                         |
| Age minus 14                           | .51              | -.55                    | 5.00*            | 75.1                        | .27              | .07                     | .58              | 13.9                        |
| African American                       | .64              | .34                     | 4.57*            | 63.7                        | .63              | .66                     | -.24             | 5.5                         |
| Mexican                                | .17              | .32                     | -2.32*           | 35.7                        | .18              | .18                     | .00              | .0                          |
| Puerto Rican or other Hispanic         | .08              | .14                     | -1.28            | 19.9                        | .08              | .10                     | -.27             | 5.7                         |
| Other race                             | .06              | .03                     | .89              | 11.0                        | .03              | .02                     | .26              | 4.2                         |
| First-generation immigrant             | .09              | .15                     | -1.07            | 16.2                        | .11              | .07                     | .53              | 10.8                        |
| Second-generation immigrant            | .15              | .31                     | -2.42*           | 37.7                        | .13              | .10                     | .48              | 8.5                         |
| N                                      | 53               | 951                     |                  |                             | 38               | 111                     |                  |                             |

\**p* < .05.

immigrants were descriptively less likely to be arrested than third generation or later, although the difference was not statistically significant.

Table 2 shows differences in family and parent variables. Arrested youth were less likely to have married parents, had lower levels of parental supervision, and came from families with more parent-child and family conflict as well as less developmental stimulation and parental warmth.

Table 3 shows peer characteristics and neighborhood differences. Arrested youth reported significantly more peer deviance, more peer pressure, and less peer attachment. At

**Table 2. Differences Between Arrestees and Nonarrestees, Before and After Matching: Family Factors**

| Wave I Variable:<br>Family Factors           | Unmatched        |                         |                  |                             | Matched          |                         |                  |                             |
|--|------------------|-------------------------|------------------|-----------------------------|------------------|-------------------------|------------------|-----------------------------|
|  | Arrested<br>Mean | Not<br>Arrested<br>Mean | <i>t</i><br>Test | Percent<br>Absolute<br>Bias | Arrested<br>Mean | Not<br>Arrested<br>Mean | <i>t</i><br>Test | Percent<br>Absolute<br>Bias |
| Household Composition                        |                  |                         |                  |                             |                  |                         |                  |                             |
| Parents married                              | .23              | .54                     | -4.45*           | 67.3                        | .24              | .24                     | .00              | .0                          |
| Extended family in<br>household              | .26              | .19                     | 1.28             | 17.1                        | .21              | .31                     | -.95             | 22.9                        |
| Number of children                           | 3.55             | 3.36                    | .77              | 10.0                        | 3.29             | 3.24                    | .10              | 2.5                         |
| Single parent                                | .45              | .31                     | 2.17*            | 29.5                        | .47              | .54                     | -.53             | 12.7                        |
| SES and Residence                            |                  |                         |                  |                             |                  |                         |                  |                             |
| Years current address<br>(subject)           | 5.78             | 5.87                    | -.14             | 1.8                         | 5.22             | 6.88                    | -1.37            | 33.5                        |
| Years current address<br>(primary caregiver) | 7.78             | 6.98                    | .76              | 9.8                         | 6.64             | 8.67                    | -1.09            | 24.9                        |
| Caregiver occupational<br>status (SEI)       | 41.42            | 42.29                   | -.35             | 5.2                         | 41.26            | 39.23                   | .56              | 12.1                        |
| Caregiver education                          | 3.08             | 3.04                    | .21              | 2.9                         | 3.03             | 2.92                    | .37              | 8.1                         |
| Household income                             | 3.79             | 4.25                    | -1.73            | 25.7                        | 3.82             | 3.85                    | -.09             | 2.0                         |
| SES composite                                | -.22             | -.06                    | -.81             | 11.9                        | -.24             | -.34                    | .34              | 7.2                         |
| Home interior physical<br>environment        | -.25             | .05                     | -1.14            | 16.0                        | -.24             | -.36                    | .30              | 6.8                         |
| Home exterior physical<br>environment        | -.28             | .04                     | -1.72            | 25.4                        | -.21             | -.52                    | 1.04             | 24.3                        |
| Parent Risk                                  |                  |                         |                  |                             |                  |                         |                  |                             |
| Father criminal involvement                  | .09              | .11                     | -.45             | 6.6                         | .11              | .14                     | -.46             | 11.4                        |
| Father substance use                         | .21              | .14                     | 1.29             | 16.9                        | .24              | .26                     | -.26             | 6.9                         |
| Mother substance use                         | .08              | .05                     | .88              | 11.2                        | .05              | .11                     | -.96             | 25.4                        |
| Mother depression                            | .19              | .14                     | .94              | 12.5                        | .16              | .09                     | .92              | 18.8                        |
| Family and Parenting<br>Processes            |                  |                         |                  |                             |                  |                         |                  |                             |
| Family supervision                           | -.45             | -.05                    | -3.67*           | 47.6                        | -.29             | -.13                    | -.86             | 18.3                        |
| Parent-child conflict                        | .27              | -.07                    | 3.21*            | 41.2                        | .19              | .25                     | -.29             | 7.2                         |
| Family conflict                              | 51.08            | 47.48                   | 2.49*            | 32.5                        | 49.24            | 50.00                   | -.32             | 6.9                         |
| Disciplined child rearing                    | 58.36            | 58.31                   | .04              | .5                          | 58.26            | 58.78                   | -.29             | 6.1                         |
| Religiosity score                            | 62.77            | 60.44                   | 2.18*            | 35.8                        | 62.94            | 62.22                   | .53              | 11.2                        |
| Family support                               | -.12             | .01                     | -1.14            | 15.7                        | .08              | -.06                    | .70              | 16.0                        |
| Lack of hostility (primary<br>caregiver)     | -.15             | .34                     | -.79             | 11.9                        | -.07             | -.26                    | .22              | 4.5                         |
| Developmental Environment                    |                  |                         |                  |                             |                  |                         |                  |                             |
| Access to reading                            | -.38             | -.03                    | -1.32            | 17.9                        | -.22             | -.07                    | -.35             | 8.0                         |
| Developmental stimulation                    | -.48             | -.07                    | -2.87*           | 36.2                        | -.38             | -.03                    | -1.46            | 31.5                        |
| Family outings                               | -.10             | -.03                    | -.59             | 7.9                         | -.03             | -.09                    | .35              | 7.8                         |
| Parental verbal ability                      | -.26             | .06                     | -1.28            | 16.6                        | -.01             | .03                     | -.11             | 2.2                         |
| Parental warmth                              | -.55             | .04                     | -2.53*           | 33.8                        | -.48             | -.50                    | .04              | .8                          |
| <i>N</i>                                     | 38               | 951                     |                  |                             | 38               | 111                     |                  |                             |

\**p* < .05.

the neighborhood level, arrested youth lived in neighborhoods with proportionally more African Americans residents, fewer Hispanics, and fewer foreign-born residents, as well as greater concentrations of poverty and higher violent crime rates. The neighborhoods had more social and physical disorder, and residents had significantly more cynicism toward the law.

**Table 3. Differences Between Arrestees and Nonarrestees, Before and After Matching: Peer and Neighborhood Factors**

| Wave I Variable                      | Unmatched     |                   |               |                       | Matched       |                   |               |                       |
|--------------------------------------|---------------|-------------------|---------------|-----------------------|---------------|-------------------|---------------|-----------------------|
|                                      | Arrested Mean | Not Arrested Mean | <i>t</i> Test | Percent Absolute Bias | Arrested Mean | Not Arrested Mean | <i>t</i> Test | Percent Absolute Bias |
| Peer Factors                         |               |                   |               |                       |               |                   |               |                       |
| Peer pressure                        | .47           | -.06              | 3.55*         | 46.6                  | .47           | .50               | -.10          | 2.5                   |
| Peer attachment to school            | .11           | -.01              | 1.98*         | 25.7                  | .04           | .00               | .37           | 7.6                   |
| Friend support                       | .06           | .06               | .04           | .6                    | .07           | .02               | .39           | 9.3                   |
| Deviance of peers                    | .55           | -.05              | 5.33*         | 72.0                  | .42           | .35               | .37           | 8.3                   |
| Peer attachment                      | -.18          | .07               | -2.55*        | 34.3                  | -.09          | -.21              | .68           | 16.9                  |
| Neighborhood Factors                 |               |                   |               |                       |               |                   |               |                       |
| Legal cynicism                       | 2.55          | 2.50              | 2.73*         | 33.3                  | 2.53          | 2.55              | -.71          | 17.3                  |
| Tolerance of deviance                | 4.24          | 4.24              | .14           | 2.1                   | 4.24          | 4.25              | -.25          | 5.6                   |
| Perceived social disorder            | 2.13          | 2.00              | 2.54*         | 36.2                  | 2.09          | 2.13              | -.46          | 10.4                  |
| Perceived physical disorder          | 1.74          | 1.65              | 2.34*         | 31.4                  | 1.71          | 1.75              | -.56          | 12.8                  |
| Neighborhood organizations           | -.29          | -.41              | 1.53          | 22.1                  | -.30          | -.25              | -.44          | 9.9                   |
| Services for youth                   | -1.63         | -1.74             | 1.08          | 14.7                  | -1.68         | -1.55             | -.78          | 18.0                  |
| Collective efficacy                  | 3.84          | 3.89              | -1.49         | 20.4                  | 3.87          | 3.84              | .47           | 10.6                  |
| Residential stability                | .06           | -.01              | .46           | 6.3                   | .13           | .03               | .43           | 10.2                  |
| Resident victimization last 6 months | .39           | .42               | -1.25         | 18.1                  | .36           | .37               | -.17          | 3.6                   |
| Percent foreign born                 | 13.44         | 21.28             | -3.66*        | 52.4                  | 13.03         | 12.37             | .20           | 4.4                   |
| Concentrated poverty                 | .28           | -.14              | 4.12*         | 52.0                  | .22           | .44               | -.97          | 26.7                  |
| Concentrated affluence               | -.28          | -.21              | -.81          | 11.7                  | -.26          | -.32              | .35           | 7.8                   |
| 1995 official violent crime          | 9.20          | 8.85              | 3.98*         | 57.3                  | 9.11          | 9.27              | -1.05         | 25.2                  |
| Percent Black                        | 55.61         | 31.37             | 4.60*         | 62.4                  | 53.95         | 57.79             | -.40          | 9.9                   |
| Percent Latino                       | 22.13         | 32.11             | -2.41*        | 34.3                  | 20.91         | 20.91             | .00           | .0                    |
| <i>N</i>                             | 53            | 951               |               |                       | 38            | 111               |               |                       |

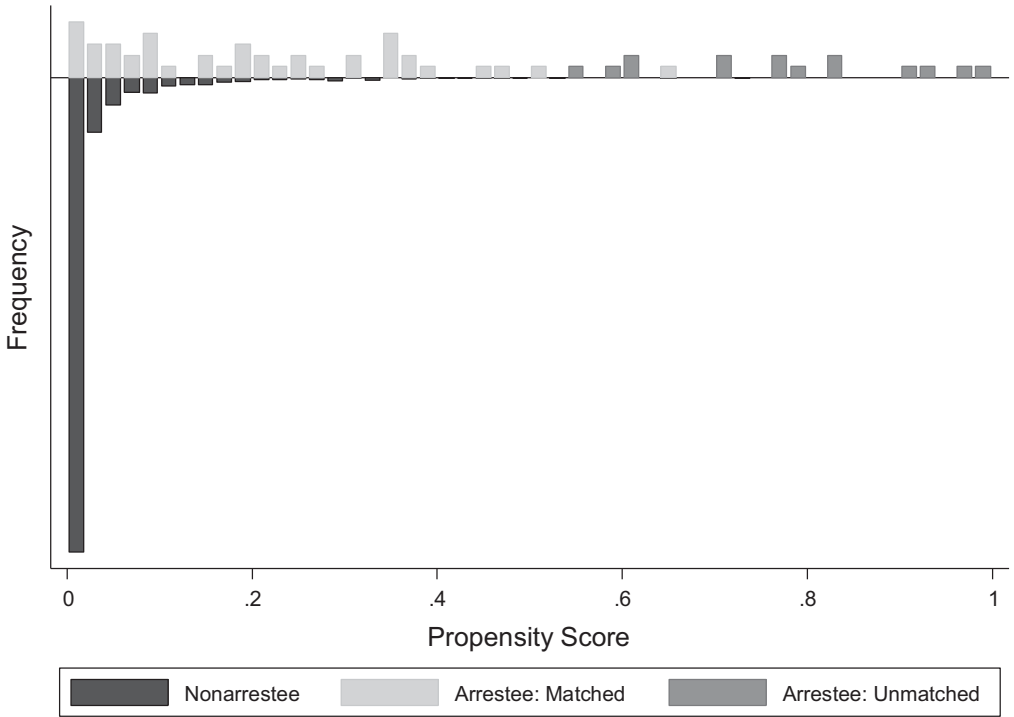
\**p* < .05.

**PROPENSITY SCORES AND BALANCE**

The maximum propensity score, expressed as the probability of being arrested, found for the nonarrestees was .738, whereas the lowest propensity for the treatment group was .003 (see figure 1). Following the recommendations of Ho et al. (2007) and Stuart (2010), we restricted our analyses to individuals with propensities in the ranges found in *both* groups, known as the region of common support. Thus, we excluded four arrestees with propensity scores greater than any controls (i.e., greater than .738). We did not extrapolate our results to individuals with the highest propensities to be arrested, which led us to estimate only the average effect of the treatment on the treated (ATT; the effect of treatment for those subjects who actually received the treatment). These effects may not generalize to individuals with very high probabilities of arrest.

Once matched to arrestees, the resulting sample of nonarrestees did not differ from the arrestees on any of the covariates. The postmatch *t* statistics and corresponding *p* values on the right side of tables 1–3 reveal that among the 79 covariates used to estimate the propensity of arrest, none showed a significant difference between the treated and controls in our final matched sample. In addition, matching on the propensity score reduced absolute bias across all covariates

**Figure 1. Distribution of Propensity Scores of First Arrest, By Treatment Status**



by 63 percent, from a mean of 30.3 down to 11.5 (median bias was reduced from 25.8 to 11.1).<sup>7,8</sup>

**EFFECT OF ARREST ON OFFENDING AND REARREST**

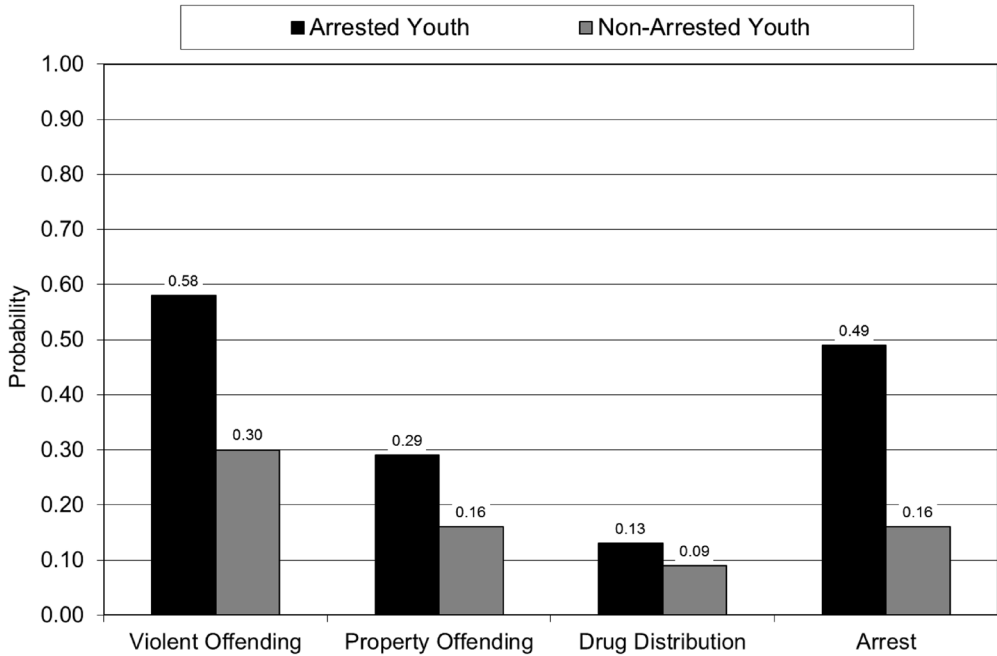
Having established the effectiveness of our propensity score matching to produce equivalent samples of arrestees and nonarrestees, we turn now to the effect of first juvenile arrests on self-reported reoffending at the third wave of PHDCN subject interviews.

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7. Bias represents the mean differences across groups as a percentage of the square root of the average of the sample variances:  $100 \times (\bar{x}_T - \bar{x}_C) / (s_T^2 + s_C^2)^{1/2}$ , where  $\bar{x}_T$  and  $\bar{x}_C$  are the sample means in the treated group and the control group, respectively, and  $s_T^2$  and  $s_C^2$  are the respective sample variances (Rosenbaum and Rubin, 1983). Absolute bias is unsigned and facilitates comparison across variables.

8. The matching results in tables 1–3 are based on the sample of youth with wave III SRO data on violent offending. With the slightly larger sample of youth with official (re)arrest data, 3:1 matching reduced mean absolute bias by 66 percent, from 29.1 to 6.9, and no significant covariate differences remained.

**Figure 2. Effect of First Arrest of the Probability of Subsequent Offending and Rearrest, Individually Matched Arrested and Nonarrested Youths**



As expected, in the absence of matching, the prevalence of offending at wave III was considerably greater among arrested youth (violence, 57 percent; property, 30 percent; drug sales 17 percent) than among control youth (violence, 23 percent; property, 19 percent; drug sales, 6 percent); offending variety, too, was considerably greater for arrestees (2.08 vs. .90). Those already arrested were much more likely to be arrested later (55 percent vs. 9 percent). These differences, all highly significant, reflect both the preexisting differences in propensity between those who had and had not been arrested (i.e., selection effects) as well as any effects of being arrested.

The key question is whether these differences persist once the selection effects are reduced through propensity score matching. We find that arrestees continued to report significantly more offending variety at wave III than matched nonarrestees (2.03 vs. 1.04; standard error [SE] = .40;  $t = 2.44$ ;  $p = .016$ ). In addition, as shown in figure 2, arrestees were more likely to report committing violent, property, and drug-distribution offending than matched nonarrestees. These prevalence differences were significant in logit models for violent offending (odds ratio [OR] = 3.23; SE = 1.45;  $z = 2.61$ ;  $p = .009$ ) and marginally significant for property offending (OR = 2.17; SE = .98;  $z = 1.72$ ;  $p = .086$ ). In addition, as shown in the figure, arrestees were much more likely to be rearrested than equivalent nonarrestees (OR = 5.20; SE = 1.85;  $z = 4.63$ ;  $p < .001$ ).

**Table 4. Rosenbaum Bounds, Effect of First Arrest**

| $\Gamma$ | Future Arrest |                | Violent Offending |                |
|----------|---------------|----------------|-------------------|----------------|
|          | Q+            | <i>p</i> value | Q+                | <i>p</i> value |
| 1.00     | 4.320         | <.001          | 2.550             | .005           |
| 1.25     | 3.720         | <.001          | 1.992             | .023           |
| 1.50     | 3.236         | .001           | 1.533             | .063           |
| 1.75     | 2.836         | .002           | 1.148             | .125           |
| 2.00     | 2.496         | .006           | .817              | .207           |
| 2.25     | 2.200         | .014           | .526              | .299           |
| 2.50     | 1.938         | .026           | .266              | .395           |
| 2.75     | 1.703         | .044           | .031              | .488           |
| 3.00     | 1.490         | .068           | -.183             | .573           |

NOTE: The estimates for future arrest are based on 43 arrestees and 103 nonarrestees, and the estimates for violent offending are based on 38 arrestees and 80 nonarrestees.  $\Gamma$  refers to the odds ratio of the effect of unobserved variables on the likelihood of first arrest for youth who were arrested versus youth who were not arrested.

## SENSITIVITY AND ROBUSTNESS

### SENSITIVITY

We explored the sensitivity of our results to possible unobserved variables. The rich set of covariates used in our propensity score analyses suggests that our matched results control for pretreatment differences between the treatment and control groups. Nonetheless, the potential for hidden biases in our estimation of the effect of arrest remains. We used Rosenbaum's (2002) bounding strategy, which explores how large the bias of an omitted variable would need to be to affect our results substantively (see the online supporting information for methodological details).<sup>9</sup>  $\Gamma$  in table 4 refers to the assumed increase in the odds of treatment (first arrest) resulting from hypothetical unobservable factors.

We begin with the effect on future arrest. At  $\Gamma = 1$ , we assume there are no hidden biases and conclude that arrest has a significant positive effect on future arrest ( $Q+ = 4.320$ ,  $p < .001$ ). Hypothetical unobserved variables that would bias the results in the direction of the observed effect are explored with values of  $\Gamma$  larger than 1. At  $\Gamma = 1.25$ , we assume that an unobserved variable increases the odds of being arrested (receiving the treatment in the current study) by an additional 25 percent after accounting for the propensity score. Under this scenario, we still find a significant positive effect of arrest on future arrest ( $Q+ = 3.720$ ,  $p < .001$ ). At  $\Gamma = 2.00$ , we assume an unobserved variable that doubles the odds of being arrested, and we still find a significant positive effect of arrest on future arrest ( $Q+ = 2.496$ ,  $p < .01$ ). To render the effect on future arrest insignificant would require a  $\Gamma$  value of greater than 2.75. As an example, increasing the probability of arrest from .50 (odds = 1.0) to .7333 (odds = 2.75) would produce a  $\Gamma$  of 2.75.

For the effect on future violent offending, we find that at  $\Gamma = 1.25$ , the effect persists ( $Q+ = 1.992$ ,  $p = .023$ ). To render the treatment effect of arrest on violent offending no longer significant, at  $p < .05$ , would require a  $\Gamma$  value of nearly 1.5. As an example,

9. Additional supporting information can be found in the listing for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2014.52.issue-4/issuetoc>.



increasing the probability of arrest from .50 (odds = 1.0) to .60 (odds = 1.5) would produce a  $\Gamma$  of 1.5.

As a comparison, we find that wave I violent offending increases the odds of first arrest by an additional 20 percent after controlling for a propensity score that excludes this factor. Thus, to produce the effect on future violent offending spuriously, an unobserved factor would need to be related to first arrests somewhat more strongly than is prior violent offending. To produce the effect on future arrest spuriously, an unobserved factor would need to be related to first arrests much more strongly than is prior violent offending. Given that we already control for a full range of offending behavior and substance use in developing the propensity score, it is challenging to conceive of an omitted factor that would yield such a sizable increase in the probability of arrest, particularly for the effect of arrest on future arrest.

### ROBUSTNESS

We explored the robustness of the effects found in the matching approach already described (3:1 nearest-neighbor matching, with caliper = .02), through four other propensity-score-matching specifications. We widened the caliper for 3:1 matching to .04, used 1:1 matching, and used kernel matching with bandwidths set at .06 or .10. The alternative matching approaches and their results are presented in the online supporting information.

The matching approach already described, which we term our “primary” specification, was the most efficient in removing bias. This primary specification was more stringent than most of the other specifications on matching, so that 43 of the 58 arrestees were matched (a.k.a. “on support”), and 38 of the 53 arrestees with SRO data. The other specifications were somewhat less efficient in reducing bias but were able to match more treated cases to controls.

The estimates of the effect on future arrest were highly significant across all specifications, and effect sizes (ORs) were larger under the alternative specifications than with our primary specification. However, for the effects on future offending, the picture was more mixed. The effects on violent offending were largest under our primary specification and were reasonably robust, although the ORs and significance were somewhat reduced under other specifications. The results on property offending and on offending variety score were less robust, and the effects were considerably diminished and no longer significant using kernel matching rather than nearest-neighbor matching.

In sum, we find that the effects on future arrests are robust and are not sensitive to omitted variable bias. The effects on future offending, however, were both less robust and somewhat more sensitive. The largest offending effect, on violent offending, is reasonably robust and reasonably insensitive to omitted variable bias.

### THE RELATIONSHIP BETWEEN SECONDARY DEVIANCE AND SECONDARY SANCTIONING

Up to this point, we have evidence in support of our first two hypotheses: Arrest has an effect on offending, particularly violent offending, and on rearrest. We now consider whether the effect of arrest on rearrest is independent of its effect on offending. Put differently, are individuals with an arrest record more likely to be arrested in the future even if they engage in comparable rates of offending as nonarrestees? Or, are

**Table 5. Effects of First Arrest on Rearrest**

| Variable                   | Model 1 |     |      |          | Model 2 |     |      |          | Model 3 |     |      |          |
|----------------------------|---------|-----|------|----------|---------|-----|------|----------|---------|-----|------|----------|
|                            | Coeff   | SE  | OR   | <i>t</i> | Coeff   | SE  | OR   | <i>t</i> | Coeff   | SE  | OR   | <i>t</i> |
| First arrest               | 1.60    | .41 | 4.97 | 3.92***  | 1.63    | .41 | 5.10 | 3.98***  | 1.40    | .55 | 4.06 | 2.55*    |
| Wave III violent offending |         |     |      |          | -.21    | .44 | .81  | -.48     | -.58    | .54 | .56  | -1.07    |
| Arrest × offending         |         |     |      |          |         |     |      |          | .55     | .81 | 1.73 | .68      |
| Constant                   | -1.33   | .27 | .26  | -4.87*** | -1.24   | .32 | .29  | -3.86*** | -1.11   | .33 | .33  | -3.41*** |
| <i>N</i>                   |         |     | 581  |          |         |     | 581  |          |         |     | 581  |          |

ABBREVIATIONS: Coeff = *b* coefficient; OR = odds ratio; SE = standard error.

\* $p < .05$ ; \*\*\* $p < .001$  (two-tailed).

prior arrestees more likely to be rearrested in the future primarily because of greater offending?

That the difference in rearrest reported in figure 2 is considerably larger than the differences in SRO suggests that the two effects are distinct. To explore this question more formally, we conducted an analysis of rearrest while controlling for the SRO at wave III. For this purpose, we use our most robust effect of offending, violent offending, as a mediating variable. We weight cases by the inverse of their propensity score and estimate the effect of arrest on subsequent arrest, net of criminal offending, in a logistic regression model (see the online supporting information for methodological details).

The results are shown in table 5. Model 1 replicates our earlier propensity score matching results with propensity weighting and confirms that first arrests significantly increase the likelihood of later arrest (OR = 4.97).<sup>10</sup> Model 2 then explores the extent to which this effect is accounted for by increased SRO.<sup>11</sup>

Consistent with our third hypothesis, the effect of first arrest on future arrest is essentially independent of the effect on reoffending. That is, offending at wave III is not predictive of rearrest (with a nonsignificant negative coefficient) once common influences are controlled through propensity scores. The clear implication is that the secondary sanctioning effect of increased rearrest is distinct from the secondary deviance effect on reoffending.

Finally, model 3 examines whether the system response effect (i.e., the significant effect of arrest on subsequent arrest) is concentrated among the more active offenders by interacting arrest with the level of offending. Focusing on the interaction term, we find

10. This OR is slightly different than that model reported in table 4 and results presented earlier because of differences in the estimation method. Propensity score weighting uses the full sample under common support in estimating the effect of arrest, including control and treated cases not used in the matched samples.

11. Our regression estimation of the indirect path to rearrest via increased offending does not control for possible alternative indirect pathways. Its identification is therefore weaker than for our propensity-score matching estimation of the primary effects. However, because this mediating path is the critical pathway suggested by theories of secondary deviance, it warrants particular scrutiny. By failing to control alternative pathways, we likely overestimate the magnitude of this pathway. In which case, this produces a conservative estimate of the residual effect that we take as evidence for a secondary sanctioning path.

no evidence that arrest is any more or less consequential for high-rate versus low-rate offenders.<sup>12</sup>

## DISCUSSION

This study explored the effects of arresting juveniles on subsequent offending and on rearrest, and it tested two types of labeling effects. Supporting our first hypothesis, we found that arrest led to a greater likelihood of offending, which is consistent with labeling theory. Supporting our second hypothesis, first arrests increased the likelihood of rearrest, and this effect was considerably larger than the effects on subsequent offending. Supporting our third hypothesis, we found that the increased likelihood of subsequent arrest was not caused by the increase in offending. Rather, a first juvenile arrest seems to increase subsequent law enforcement responses to those youth compared with other youth who offend at a comparable level but have managed to evade a first arrest. This could result from increased scrutiny of the individual's future behavior, by police as well as other actors such as teachers and school staff, as well as from reduced tolerance by police and other actors of an arrestee's future transgressions.

These findings extend prior labeling research in several important ways. The increases in SRO after arrest confirm findings from studies with other longitudinal data. Using propensity score methods with the PHDCN's extensive set of covariates from the individual, family, peer, and neighborhood domains provides grounds for believing that we have substantially reduced the threat to validity from selection bias that is a concern in many labeling studies. Varied specifications and sensitivity tests confirm that our results are reasonably robust, especially the effect on violent offending, and relatively insensitive to bias from unobservables. These findings are consistent with two other recent propensity score studies of SRO outcomes, Morris and Piquero's (2013) study of arrests circa 1980 and Wiley and Esbensen's (2013) study of arrests in the 2000s, both of which supported the conclusion that arrests lead to secondary deviance.

The current study also confirms earlier findings, mostly from experimental studies, that an arrest tends to generate more subsequent arrests. This finding is robust across model specifications and is quite insensitive to the possibility of omitted variable bias. Perhaps our most important finding concerns the relationship between the effect on SRO and the effect on rearrest. It often is assumed that rearrest is largely a product of the perpetuation of offending associated with secondary deviance. Our findings cast doubt on this common interpretation. Instead, we find a considerably larger effect on arrest than on SRO, which is consistent with Klein (1986).<sup>13</sup> Moreover, we find that the arrest effect is not diminished after accounting for the potential mediating effect of wave III SRO, which leads us to conclude that the effects of secondary deviance and secondary sanctioning are essentially independent.

One limitation of the current study is that it excludes the highest rate offenders for whom no matched nonarrestees were found with a comparable propensity to be arrested.

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12. Similar results were found in equivalent propensity-weighted analyses using the variety score as the control variable or using offending from wave I (rather than wave III).

13. Although Klein found no effect on secondary deviance, we find such an effect on offending perhaps because we restrict our study to first arrests, which theoretically should show the largest effects on secondary deviance.

Hence, we have no empirical basis to estimate how such extremely high-propensity youth would fare if not arrested because all such youth in the PHDCN were arrested. Possibly, the highest propensity youth are already so firmly on a deviant trajectory that a formal arrest has little effect either on their behavior or on societal responses. Or perhaps cumulative disadvantage makes first arrests especially likely to result in secondary deviance and/or secondary sanctioning for such high-propensity youth.

## IMPLICATIONS

Our results suggest that the large labeling effect found on rearrest truly reflects secondary sanctioning—that is, differential societal response to a youth with an “arrestee” or “delinquent” label—and that this societal response is not mediated by the differential offending behavior of the juvenile. This process of secondary sanctioning, in which initial arrests beget subsequent arrests, suggests a cascading effect of deepening involvement in the justice system.

How does this secondary sanctioning come about? Does it reflect only the differential behavior of law enforcement or might some differential youth behavior also be implicated? One limitation from applying data collected for broader purposes to these labeling questions is that we have limited information about the arrests, their particular circumstances, or what offending behavior may have prompted those arrests, let alone how the arrestee interacted with law enforcement. Thus, although the secondary sanctioning effects were not mediated by differential offending, we cannot confidently assume that no other behavioral differences between youth with and without prior arrest experiences played a role in their differential subsequent arrests. Nonetheless, the major expected behavioral contributor of youth would seem to be offending differences. The absence of any indication that offending behavior mediates the increase in arrest suggests, therefore, that secondary sanctioning effects are caused in considerable part by differential societal responses. Whether this reflects police actively scrutinizing and/or monitoring “the usual suspects,” being more likely to take formal action rather than issue a warning to youth with a prior arrest, or some other changed response remains a question for future research.

Labeling effects on youth behavior and on police behavior also may be interdependent. Secondary sanctioning processes may be partly co-produced in the interaction between two actors whose expectations are conditioned by the earlier arrest, with police having greater expectations that labeled youth will offend, whereas youth have greater expectations that police will carry out arrests. Long-standing experimental work has found that experimentally induced expectations of students’ academic performance influence teachers’ and students’ interactions in ways that are expectancy confirming (Jussim and Harber, 2005; Rosenthal and Jacobson, 1968), especially when teachers do not yet have much direct experience with the student (Raudenbush, 1984). Similar expectancy processes may operate in how labels affect the interactions between labeled offenders and justice system actors.

The current study thus suggests that understanding labeling processes will require future work on mechanisms of secondary sanctioning processes as well as on secondary deviance. This will require broadening the labeling perspective to include studying the behavioral response of societal actors to labels.

The policy implications of labeling findings are twofold. The most obvious implication for curtailing the destructive effects of labeling is to restrict formal law enforcement responses to serious delinquency and to resist the temptation to criminalize minor misbehavior, such as school discipline problems (e.g., Kupchik, 2010). Although few U.S. policy makers have been willing to go as far as Schur (1973) in considering “radical nonintervention” by law enforcement, there are some promising signs in the school domain. Following the lead of several school districts, notably in Broward County, Florida (see Stucki, 2013), the U.S. Department of Education (2014) issued a set of guiding principles with respect to school discipline that marks a fundamental shift away from the tough-on-crime school policies that led to the criminalization of so many minor school infractions over the past two decades.

The second type of policy implication concerns how to decrease the detrimental effects of labeling. This typically focuses on how to ameliorate secondary deviance effects (e.g., Bernberg, Krohn, and Rivera, 2006; Wiley, Slocum, and Esbensen, 2013). The current study, however, suggests that in the short term, the effects of arrest through secondary deviance may be dwarfed by the detrimental effects that operate through secondary sanctioning. Ameliorative policy efforts that address the secondary societal responses may be as important as those that try to ease the deviance-amplifying effect of the primary sanction. In the law enforcement realm, these would involve efforts to prevent the compounding effect of increasingly punitive law enforcement responses to equivalent misbehavior, especially minor misbehavior. These results highlight the importance of policies and practices to maintain the confidentiality of juvenile records, and to expunge and remove such records after an appropriate period of redemption (Blumstein and Nakamura, 2009).

One way for an offender to lessen the risk of secondary sanctioning is to simply move residences or change schools. There may be some benefit to putting physical distance between an arrestee seeking to reform his or her behavior and the increasingly watchful eye of the authorities. Recent research on prisoner reentry has found that residential relocation can potentially provide a turning point for ex-prisoners by helping sever ties to former peers and neighborhoods, thereby lessening some of the risk factors that propel individuals toward crime (Kirk, 2009, 2012). Residential change also may lower the risk of rearrest by separating individuals from the watchful gaze of local police who might have a decreasing level of tolerance for a given person’s transgressions. In accord with this line of reasoning, Keels (2008) found that male youth from families participating in the Gautreaux housing mobility program in Chicago who moved to the suburbs were significantly less likely to be arrested for drug, theft, and violent offenses than male youth who moved internally within Chicago. A move to the suburbs means moving outside of the jurisdiction and surveillance of the Chicago Police Department, thereby providing more of an opportunity for a fresh start.

In conclusion, among otherwise equivalent youth with similar levels of criminal offending, those youth unlucky enough to become ensnared by the criminal justice system face a daunting task of steering clear from future interaction with the system. Not only is the likelihood of future offending increased for a host of reasons; the likelihood of future sanctioning also increases even if criminal behavior does not escalate relative to nonarrested counterparts. Therefore, policy solutions to the detrimental consequences of a delinquent label must address not only ways to reduce secondary deviance but also ways to reduce secondary sanctions. Of course, two ways to avoid the necessity of countering the consequences of criminal stigma are to reduce primary deviance and primary

sanctioning. Fortunately, the federal government has recognized the far-reaching consequences of a criminal stigma and has taken steps to reduce the criminalization of misbehavior in schools (U.S. Department of Education, 2014). The U.S. Department of Education's new guiding principles are a refreshing alternative to the culture of control that has characterized the U.S. criminal justice and education systems for too long.

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## SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

**Table S.1.** Alternative Specifications