

Examining the Divergence Across Self-report and Official Data Sources on Inferences About the Adolescent Life-course of Crime

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Abstract Both self-report and official crime data have known limitations, leading to the critical question as to whether inferences about the adolescent life-course of crime are different across these data sources. Using both official and self-report arrest data on a sample of subjects drawn from the Project on Human Development in Chicago Neighborhoods (PHDCN) longitudinal cohort study, this paper examines the extent to which individual age-arrest curves are comparable across these data sources. Particular attention is given to examining whether criminal career dimensions, namely participation, frequency of arrest, age of onset, and continuity in behavior, are similar across data sources. Additionally, this paper examines whether the key predictors of youth crime (e.g., family processes, peer influence, and neighborhood disadvantage) function similarly across measurement types. Findings reveal that a sizable number of youth self-report being arrested without having a corresponding official arrest record, and a sizable proportion of those youth with an official arrest record fail to self-report that they had been arrested. Despite significant differences across the two arrest measures on many criminal career dimensions, the effects of family supervision, parent-child conflict, and neighborhood disadvantage operate similarly across data types.

Keywords PHDCN · Self-report measures · Official measures · Criminal careers · Life-course

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Introduction

In a recent review of the state of life-course criminology, Piquero and colleagues (2003, p. 480) importantly ask whether measurement of criminal careers by self-report and official data sources produce similar findings with respect to key dimensions of the criminal career paradigm (i.e., onset of crime, prevalence, lambda, career length, crime-type mix). These authors ponder whether theoretical expectations derived from life-course studies apply equally well to self-report and official criminal records. For example, researchers can question whether the onset of arrest occurs at the same point across data sources, and whether the factors influencing the processes of persistence of and desistance from crime are similar across data sources. Answering these types of questions is fundamental to the advancement of life-course criminology.

This study addresses the issue of convergence across self-report and official data by examining the relation between arrest, age, and a number of relevant predictors of arrest. The objective of the study is to determine whether self-report and official indicators produce the same conclusions about arrest, paying particular attention to criminal career dimensions and whether theoretical expectations about key predictors of youth crime derived from prior research (e.g., family processes, peer influence, and neighborhood disadvantage) function similarly across measurement types.

Strengths and weaknesses of self-report and official data

Criminal behavior is generally measured through three different types of data collection: victimization surveys, self-report surveys, and official data from law enforcement agencies and criminal courts. Because of the emphasis in this study on arrest, focus is put upon comparing the latter two methods. This section briefly reviews the strengths and weaknesses of these two data types in cross-sectional and longitudinal research. To be clear, this comparison is made in order to illustrate that both self-report and official measures of arrest have strengths and weaknesses, which sets up the research question as to whether these sometimes fallible measures produce divergent findings about the adolescent life-course of crime.

One of the primary benefits of self-report survey data is the capacity to examine the etiology of crime and criminality by means of collecting comprehensive information on individual, familial, and environmental characteristics and influences (Thornberry and Krohn 2003). However, self-report indicators of offending and arrest are plagued by a number of problems, which results in substantial over- and under-reporting of events and behaviors. Here, over-reporting can generally be defined as self-reporting more events or behaviors than actually occurred, while under-reporting refers to reporting fewer events or behaviors than occurred. To name but a few of the problems associated with self-report surveys, researchers have long been concerned with the reliability and validity of measures (e.g., Short and Nye 1957, 1958), including the biases associated with recall error and response falsification. Also, the issues of sample attrition, testing effects from repeated measurement of the same subjects, and lack of construct continuity all potentially plague measurement in longitudinal designs (Thornberry and Krohn 2000, 2003). Regarding arrest, problems with questionnaire design may contribute to erroneous self-reported arrest counts if question wording does not properly distinguish between actual arrests versus police contact (Blumstein et al. 1986). Further, the more frequently an individual is arrested, the less salient any one arrest will be in memory,

such that recall may be most problematic for those arrestees with the greatest number of contacts with the criminal justice system (Blumstein et al. 1986).

Of course, official records are not without fault either. Proponents of victimization and self-report surveys argue that official records severely underestimate the true volume of crime. Findings from analyses of victimization surveys consistently show that victims of crime often do not report victimizations, with reporting rates varying by crime type. Self-report surveys also indicate that most crimes are not detected by law enforcement personnel.

In addition to underestimating the volume of crime, it is also true that some crimes detected by police do not lead to arrest and that some arrests made by police officers will not be counted in official statistics. Black and Reiss (1970) find that only 15% of police contacts with juveniles resulted in an official arrest, thus providing evidence of considerable discretion on the part of police. In a more recent study, Worden and Myers (1999; reported in National Research Council and Institute of Medicine 2001) similarly find that 13% of police contacts with juveniles result in arrest and 14% of contacts with adult suspects. However, a suspect may mistakenly report police contact as an actual arrest in survey data, thus producing divergent results between official and self-report data on arrest. For instance, as part of the same project on policing, Myers (2002, p. 126) observes that in 3.7% of police contacts with juvenile suspects, the juvenile was handcuffed but not officially arrested. It is an open question as to whether these juveniles interpreted this police action as an arrest or not, but the overall point is that the disposition of police-suspect encounters is often ambiguous and that there is a potential for misinterpretation on the part of suspects as to whether they were formally arrested.

Another problem with official data that contributes to discrepancies with self-report arrest data is the handling of aliases and misidentification of arrestees (Geerken 1994). Geerken (1994) notes that 1.1% of arrestees in his sample lied about their names and were discovered through subsequent fingerprint checks. It is unknown how many of the arrestees in his sample actually gave aliases because the fingerprint check only detected the use of aliases for prior arrestees and not first time offenders. Geerken (1994) also notes that the same individual may appear in criminal history records as multiple individuals because of law enforcement data entry errors (e.g., names misspelled, race/ethnicity entered incorrectly). In sum, official data arguably undercounts the true volume of crime and, to a lesser extent, the true volume of arrests.

One important advantage of official data in longitudinal studies is the fact that arrests and criminal case processing are recorded at specific points in time, as opposed to typical self-report surveys which ask about behavior and events during a window of time (often 12 months prior to the interview date) (Kazemian and Farrington 2005). In self-report surveys, even when subjects are asked about the specific timing of events, there are substantial recall errors in reporting. Particularly problematic is the issue of telescoping. Because of difficulties recalling the timing of events, respondents of self-report surveys often over-report behaviors that did not actually occur in the twelve month window, or under-report behaviors that did occur during the window.

Additional advantages of the use of official data in longitudinal studies include the length of the time period covered, and the lack of gaps in recording of criminal events. These are key reasons why most knowledge about criminal careers has been obtained from official data sources (Farrington et al. 2003). Self-report survey data collections are often designed to have gaps in the reporting period, in order to compensate either for the telescoping issue mentioned in the preceding paragraph, or for practical reasons associated with the cost of doing research. Furthermore, self-report surveys of youth typically follow

subjects for a limited number of years, usually into late adolescence or early adulthood. However, life-course research shows that offenders do continue committing crimes well into adulthood, suggesting that truncating analyses to early adulthood may lead to false conclusions about the true age-crime relation (Laub and Sampson 2003).

Convergence on individuals' records of arrest

Because of the problems addressed above concerning measurement of arrest in official and self-report data, it is an open question as to whether these two data types will show convergence on arrest, in the sense that self-report and official arrest records for the same person will show agreement on whether the given individual has ever been arrested, their frequency of arrest if they have been arrested, and the timing of arrest. Past research has produced mixed findings about the convergence on the same outcome measure across data sources.

Hirschi (1969) finds that only 60% of individuals in his study with official records admitted being picked up by the police. Hardt and Petersen-Hardt (1977) find that 78% of the juveniles in their study with an official police record did self-report having a criminal record, and that 95% of those juveniles who reported that they did not have a criminal record did not in fact have an official record. Huizinga and Elliott (1986) find that between 36 and 48% (depending on the matching criteria) of individuals in the National Youth Survey with an official arrest record misreported at least some of their behavior, and between 22 and 32% of official arrests were not reported in self-report data. Hindelang et al. (1981, p. 172) similarly find that a large number respondents in their sample failed to report being picked up by the police, and also that the failure to report varies by race and gender. White males failed to report 24% of the occasions when they were picked up by police; the figure for black males is 50%; for white females, 48%; and for black females, 70%. In a more recent study, Maxfield et al. (2000) find that 73% of subjects with an official arrest record self-reported having an arrest record, and that roughly 21% of subjects without an official record self-reported having been arrested. Given these findings, it is questionable whether self-reports and official data will produce similar conclusions about the prevalence and incidence of arrest.

Similarity of results with respect to criminal careers

Recently, a growing number of studies have examined the extent of agreement across self-report and official data sources in regards to key criminal career dimensions. Most of this research compares different domains of criminal behavior (i.e., self-reported offending versus official arrest, conviction, or court referral), while the present study examines the same criminal outcome (arrest). In a recent article in this journal, Brame et al. (2005) provide one of the first systematic analyses of whether key research findings from life-course criminology are dependent upon a certain type of data source. More specifically, they examine the association between past and future offending in both self-report offending data and official data (police contact and arrest), and find evidence across data sources for both population heterogeneity and state dependence explanations for continuity in behavior.

In another relevant study, Farrington et al. (2003, p. 954) compare self-report offending data from the Seattle Social Development Project with court referral data and conclude, that “present analyses indicate that criminal career research based on self-reports would yield different theoretical implications from research based on official records.” Specifically, they find that the prevalence of both self-reported offending and court referral increase with age, though the increase in court referrals with age is much steeper. Not surprisingly, they find that prevalence of offending is greater than prevalence of court referral. Farrington and colleagues also find much continuity in criminal behavior over time, in both self-report and court referral data, though they observe that the continuity is greater with court referrals. Additionally, they find that the frequency of self-reported offending is greater than the frequency of court referrals, and that the frequency of offending increases with age, but that the frequency of court referrals does not. As one would expect, onset of self-reported offending occurs earlier than onset of court referral. This finding of earlier onset in self-reports has been replicated in numerous other studies (see, e.g., Moffitt et al. 2001, Loeber et al. 2003), though one study did find consistency across data sources in terms of age of onset when analyses were restricted to onset of serious offending (Kazemian and Farrington 2005). Interestingly, Farrington et al. (2003) find that early onset predicts a high yearly frequency of subsequent court referrals, but not a high frequency of self-reported offending.

Finally, in perhaps the only other study besides the present one that compares self-reports of arrest and official arrest data longitudinally, Thornberry and Krohn (2003) find a high degree of concordance between self-report and official data. Using data collected as part of the Rochester Youth Development Study, these authors conclude that subjects with an official contact with the police or an arrest record were generally willing to report that contact during the self-report interview. Moreover, the degree of concordance is stable across waves of data collection.

These prior studies offer a compelling examination of the dependency of criminal career and life-course research findings on data types. That said, in addition to confirming these prior research findings, the present study provides a number of unique contributions beyond what has already been learned from comparisons of longitudinal self-report and official data. First, whereas self-reported *offending* data is usually compared with official data in these prior studies, the present study focuses on comparing the same measure of crime (i.e., arrest) for the same subject across two data types. It has long been recognized that self-reported offending and official data actually measure different “domains” of behavior, where official data tends to capture more serious behavior than self-report offending measures (Hindelang et al. 1979). Thus, it is necessary to determine whether comparative findings described in preceding paragraphs hold when the same domain of behavior (i.e., arrest) is examined across data sources. Second, in addition to comparing findings on key criminal career dimensions, the present study also examines whether the key correlates of arrest and key predictors of arrest function similarly across the two arrest measures. The next section introduces the relevance of a number of these key predictors.

Predictors of youth crime

Family effects, peer effects, and neighborhood effects all have been given prominent focus in criminological research. Sampson and Laub’s (1993) groundbreaking reanalysis of the

Gluecks' data offers a framework for examining the effect of these factors on youth crime. Sampson and Laub (1993, p. 7) argue that structural context (e.g., social class, race, ethnicity, neighborhood poverty) mediated by informal social controls (e.g., family supervision, parent-child conflict, deviant peer associations) explains delinquency in childhood and adolescence. Regarding the family, Loeber and Stouthamer-Loeber (1986) provide an extensive review of the family predictors associated with juvenile crime. These authors conclude that family socialization variables, like parent-child conflict and supervision, are among the most important predictors of juvenile delinquency.

Moving to peer influence, a great deal of research has examined the effect of peers on individuals' criminal behavior. This research has consistently shown a substantial positive association between peer behavior and delinquency, though the reason for this association is debatable (Sampson and Laub 1993; Warr 1993). In his classic statement on differential association, Sutherland (1947) makes the argument that criminal behavior is learned in intimate social groups. In contrast, Glueck and Glueck (1950) argue that the association between peers and crime arises from a selection effect (i.e., birds of a feather flock together). In a more recent study which is relevant for the present analysis, Warr (1993) examines the relation between age, crime, and peer influence, and finds that the relation between age and crime is weakened after controlling for peer influence.

Over the past 20 years, the neighborhood effects approach to studying social phenomena has gained widespread popularity. Arguably, this current popularity owes its rise to the influence of Wilson's (1987) research on the detrimental effects of residing in concentrated poverty and social isolation, although, criminological research has long emphasized the role of ecological context in influencing criminal behavior (see, e.g., Shaw and McKay 1942). The present study focuses on the role of neighborhood disadvantage as a predictor of criminal behavior. A number of studies have likewise examined the effect of neighborhood disadvantage, and consistently find that neighborhood disadvantage is a positive predictor of crime (for recent discussions, see McNulty and Bellair 2003; Sampson et al. 2005).

In sum, the broader purpose of this study is to examine whether inferences about the adolescent life-course of crime are dependent upon the way crime is measured. As such, three main research questions guide this analysis: (1) Are there differences across data sources on the same sample of respondents in terms of the prevalence, frequency, onset, and continuity of arrest? (2) Is the association between key demographic correlates of arrest (e.g., age, gender, and race/ethnicity) and arrest trajectories similar across the two data sources? (3) Are inferences about key predictors of arrest (e.g., neighborhood structure, family structure, family process, peer influence) similar across data sources?

Data and research design

The study sample is drawn from the Project on Human Development in Chicago Neighborhoods (PHDCN), a multi-wave study of the factors influencing human development and antisocial behavior of Chicago youth. In this project, longitudinal data was collected on 7 cohorts of subjects, defined by age at baseline (0, 3, 6, 9, 12, 15, and 18), with subjects and their primary caregivers interviewed up to three times. Wave 1 of the survey was completed between 1994 and 1997; wave 2 was completed between 1997 and 2000; and wave 3 of the survey was completed between 2000 and 2002. The interval between interviews was about 2.5 years.

This paper focuses on the 12, 15, and 18 age cohorts. In the data collection, a random sample of 80 neighborhood clusters, stratified by racial/ethnic composition and SES, were

selected from a total of 343 neighborhood clusters in Chicago (Sampson et al. 1997). Within these 80 clusters, a simple random sample of households yielded a total sample of 2,150 youth in the 12, 15, and 18 cohorts. Overall, 76.2% of the PHDCN participants in the 12, 15, and 18 cohorts were interviewed at all three waves.

Dependent variables

Two measures of arrest are used as outcome variables in both descriptive and inferential analyses. At the first PHDCN interview, youth subjects were asked to report whether they had been arrested during the previous twelve-month period. If so, they were then asked when and where the arrest occurred, the reason for the arrest, and whether they went to court for the arrest. At the second and third interviews, youth subjects were asked to report any additional arrests since the first interview date. The present analysis uses a subset of the total sample ($n=1775$) who consented to have their official records searched. This subset showed no significant difference in the average number of self-reported arrests per wave compared to those youth subjects who did not consent to have their criminal records searched ($F=0.925$; $df=1, 2149$).

Official arrest data were provided by the Chicago Police Department (CPD) and the Illinois State Police (ISP), and cover the time span from 1995 to 2001. Both juvenile and adult arrest data were provided for arrests recorded throughout the State of Illinois. An automated matching procedure was used to compare the data files from the criminal justice agencies with identifying information on youth subjects from the PHDCN data. This probabilistic method calculates the likelihood that records across different data sources belong to the same person by matching as many pieces of identifying information across sources as possible. Identifying information used in the matching includes social security number, name, birth date, county and zip code, race and ethnicity, and gender. With the use of multiple identifying variables, records can be matched across data sources even if an alias was used in the official arrest data.

With the official data, person-year observations were constructed by calculating the age of a given subject as of December 31st of a given year, and summing the count of arrests over the previous twelve-month period. With seven years of data, there are exactly seven official observations per subject. For the self-report data, person-year observations start with the subject's age at the first wave of data collection. Calculating arrests per person-year is possible given that subjects were asked at waves 2 and 3 about the timing of arrests since wave 1. On average, there were five years between the first and third interview. For an individual with 5 years between their first wave self-report and third wave self-report, there would be a total of 6 self-report observations for that subject. The maximum number of self-report observations for any subject was seven. If a subject did not report at wave 2 and wave 3, then they only have one self-report observation. If a given subject reported at wave 3 but not wave 2, then they would still have a full set of observations given that the wave 3 arrest question asked subjects about arrests since wave 1. Note that self-report data were cleaned to eliminate duplicate arrest reports.

Independent variables

Included in the statistical models are a number of individual-, family-, peer-, and neighborhood-level predictors of arrest. Key demographic factors include age, cohort, gender, and race and ethnicity. Five dummy indicators of race and ethnicity are employed in the analyses: black, Mexican, Puerto Rican/Other Latino, other race, and white. Black, white, and other

race groups are all non-Latino. Two measures of family structural characteristics are included as explanatory predictors of arrest: family socioeconomic status and parental marital status. Marital status is described with a binary variable reflecting the marital status of a youth's biological parents.

Finally, neighborhood concentrated disadvantage is included in statistical models, along with three self-reported scales of family process and peer influence, all of which are derived from the wave 1 PHDCN survey: family supervision, parent–child conflict, and peer deviance. Construction of the neighborhood concentrated disadvantage measure is informed by previous work (Sampson et al. 1997), and derived from 1990 census data. Family supervision is a 24-item scale derived from the Home Observation for Measurement of the Environment (HOME) (Bradley et al. 2000; see also Browning et al. 2004). Primary caregivers of each subject in cohorts 12 and 15 responded to a series of questions such as whether curfews are set and adhered to, whether the family has behavioral rules, whether there is access to alcohol in the home, and whether the primary caregiver is consistent in applying family rules. Parent–child conflict is a 7-item scale that measures physically aggressive behavior of primary caregivers towards youth subjects. The 7 items are a subset of the Conflict Tactics Scales (Straus 1979). Primary caregivers were asked the frequency with which they did the following in the twelve-month period prior to the interview: (1) threw something at the subject, (2) pushed, grabbed, or shoved the subject, (3) slapped or spanked the subject, (4) kicked, bit, or hit the subject, (5) hit or tried to hit the subject with an object, (6) beat up the subject, and (7) burned or scalded the subject. Peer deviance is derived from 17 items asking subjects how many of the people they spend time with engage in various behaviors. These behaviors include, among others, substance use, sale of drugs, theft, property damage, and assault. Items for the respective scales were combined using an item response model (IRT) with the STATA GLLAMM program (Rabe-Hesketh et al. 2004).

Statistical models

Studies using multiple data sources or informants often produce results separately for each data source. However, there is considerable benefit to combining data sources in one model in order to evaluate the similarity of results across data sources. In addition to descriptive statistics, this study uses what is known as a bivariate outcome modeling approach to compare arrest measures, which combines official and self-report data into a single statistical model (for a detailed discussion of this modeling approach, see Horton and Fitzmaurice 2004; Kuo et al. 2000). For the purposes of this study, the primary reason for using this modeling strategy is in order to statistically compare the size of coefficients of the same predictor of arrest, where arrest is measured by both self-report and official data. For example, in analyses to follow, I compare the size and direction of the coefficient for family supervision as a predictor of arrest for both self-reported arrest and official arrest. If there is a significant difference in the size and direction of the coefficient, then it can be concluded that the effect of family supervision on arrest is dependent upon which data source is under investigation.

With the bivariate modeling approach, a baseline quadratic growth model is first specified, with arrest as the outcome, and age and a squared age term as covariates. In the analyses, age is centered at 18. This age was chosen because it provides an overlap in the

observation periods for all cohorts (i.e., age 18 is the end of the observation period for the 12 year-old cohort, and the beginning of the observation period for the 18 year-old cohort). With this centering, model coefficients are used to assess the expected count of arrests at age 18 and the rate of change in arrest at age 18. The baseline model is first expanded with the addition of demographic covariates, followed by family structural characteristics. The final model also includes neighborhood characteristics, and family process and peer influence measures.

Each model just described assumes that Y_{ij} , which is the observed number of self-reported or official police arrests for person i in neighborhood j in the twelve months immediately prior to age t , follows an overdispersed Poisson distribution. Thus, the data are structured to where each observation represents a person-year, with a total of t observations per person i . In each model, random intercepts are added in order to account for the correlation among observations within the same subject, and the correlation between subjects living in the same neighborhood. Given the addition of random intercepts, each subject has their own estimated arrest trajectory. This modeling strategy is undertaken in order to assess the individual change in arrest with age. Furthermore, random effects account for the heterogeneity between subjects (and neighborhoods) due to unobserved factors. Random slopes (i.e., for the age terms) were also included in preliminary analyses of all models described in this paper. However, results indicate that there is no significant variability across subjects and neighborhoods in the growth or change in arrests at age 18. Therefore, in the interest of parsimony, all analyses reported in the paper were estimated only with random intercepts.

With the Poisson distribution, it is assumed that the conditional variance and mean are equal, though this is often not the case with arrest data. Thus, a dispersion parameter is added to all models in order to allow for conditional variance that is larger or smaller than expected.

In Eq. (1), a total of four random effects are included in a bivariate outcome model that combines data sources instead of treating them separately. Two random intercepts are specified for self-report arrests (one at the person-level and one at the neighborhood-level), and two separate random intercepts are specified for the official police arrests. Therefore in these models, each subject has two trajectories, one for self-report data and one for official police data.

Model 1 in analyses to follow displays results estimated by Eq. (1), where arrest from both data sources is modeled as a function of age:

$$\begin{aligned} \log E(Y_{ij}) = & \pi_{1ij}SR_{ij} + \pi_{2ij}POLICE_{ij} + \pi_{3ij}SR_{ij} * (AGE - 18)_{ij} + \pi_{4ij}POLICE_{ij} \\ & * (AGE - 18)_{ij} + \pi_{5ij}SR_{ij} * (AGE - 18)_{ij}^2 + \pi_{6ij}POLICE_{ij} \\ & * (AGE - 18)_{ij}^2 + r_{1ij}SR_{ij} + r_{2ij}POLICE_{ij} + u_{1j}SR_{ij} + u_{2j}POLICE_{ij} \end{aligned} \quad (1)$$

where

SR_{ij} is an indicator function taking the value of 1 when the record for person i in neighborhood j at age t is from PHDCN self-report data, and 0 otherwise;

$POLICE_{ij}$ is an indicator function taking the value of 1 when the record for person i in neighborhood j at age t is from ISP or CPD police data, and 0 otherwise;

r_{1ij} and r_{2ij} are the two person-level random effects, one for the self-report arrest trajectory and one for the official arrest trajectory;

u_{1j} and u_{2j} are the two neighborhood-level random effects, which capture the dependence of the respective measures of arrest between residents in the same neighborhood.

Equation (2) shows that the expected count of arrests at age 18 is modeled as a function of additional covariates, where $X_{ij}\beta$ is a vector of demographic, family, and peer characteristics, and $W_{j\gamma}$ represents neighborhood concentrated disadvantage:

$$\begin{aligned}\pi_{1ij} &= \mu + X_{ij}\beta + W_{j\gamma} \\ \pi_{2ij} &= \mu + X_{ij}\beta + W_{j\gamma}\end{aligned}\quad (2)$$

The two linear and two quadratic growth terms are also modeled as a function of demographic, family, peer, and neighborhood characteristics, where k references coefficients 3, 4, 5, and 6 from Eq. (1):

$$\pi_{kij} = \mu + X_{ij}\beta + W_{j\gamma} \quad (3)$$

As noted, one important advantage of using bivariate models is that they can be used to test whether the size of the effect of predictors of arrest are a function of the data source utilized. As such, a series of hypothesis tests will be used to compare the coefficients from Eqs. (2) and (3) above. For comparison of the q demographic, family, and peer coefficients:

$$H_0 : \beta_{1q} = \beta_{2q} \quad (4)$$

For comparison of the neighborhood concentrated disadvantage coefficients:

$$H_0 : \gamma_1 = \gamma_2 \quad (5)$$

Results

Descriptive summary of arrests

Tables 1 and 2 present a descriptive summary of self-reported and official arrests, with an emphasis on prevalence and frequency. A total of 341 PHDCN youth subjects from cohorts 12, 15, and 18 were officially arrested at least once from 1995 to 2001, equating to 19.2% of the sample. Of this number, 148 were arrested one time (8.3%), and the remainder arrested at least twice during the time frame. A total of 1,093 arrests of the PHDCN youth were officially recorded in the State of Illinois from 1995 to 2001, which equates to an average of 3.21 arrests for those subjects ever arrested. The average age of first arrest among those 341 subjects ever arrested was 18.3. The partial correlation (controlling for cohort) between age of first official arrest and the total number of arrests equals -0.381 ($P < 0.001$), and the partial correlation between age of first arrest and imprisonment in the Illinois Department of Corrections equals -0.220 ($P < 0.001$). Similar to some of the classic studies in criminology (e.g., Glueck and Glueck 1950; McCord 1978; Wolfgang et al. 1972), these correlations suggest that earlier onset of crime, in this case measured by arrest, is related to persistent and serious criminality.

In comparison, 21.4% of the sample self-reported at least one arrest across the three waves of data collection. Of this number, 9.5% reported one total arrest across the three

waves. The remainder reported being arrested two or more times. A total of 1,173 arrests were self-reported by a total of 379 arrestees, for an average of 3.09 arrests among those ever arrested. The average age of first arrest among those subjects ever arrested was 17.2, which is statistically different than the onset of arrest in the official data ($F = 40.757$; $df=1,719$). The partial correlation between age of first self-reported arrest and the total number of arrests equals -0.248 ($P < 0.001$), but the partial correlation between age of first arrest and imprisonment in the Illinois Department of Corrections is not significant ($r = -0.080$, $P = 0.121$).

Comparing self-report and official data reveals that more subjects reported being arrested than actually found in the official data, and more arrests were reported. However, some of the over-reporting, though not all, is due to reporting of arrests that did not occur in Illinois, which is not captured in the ISP or CPD data. Twenty-four subjects self-reported at least one arrest outside of Illinois, and 38 of the 1,173 (3.2%) self-reported arrests occurred outside of Illinois.

In their review of the literature, Blumstein et al. (1986) find substantial differences across race on participation in crime, particularly when participation is measured by official data. However, they find that the frequency of arrest is generally comparable across race. Therefore, they conclude that race differences in criminal behavior, whether measured by offending, arrest, or some other outcome, are generally due to differences in participation and not due to differences in frequency. Results from Tables 1 and 2 point to similar conclusions, though with some differences across data sources. With official arrest data, roughly 30% of the sampled black youth were arrested at some point between 1995 and 2001, compared to roughly 13–14% for the other groups (a ratio of roughly 2.3:1). With self-report data, it can be seen that a slightly lower percentage of black youth self-reported an arrest than found in the official data (27.9% vs. 29.6%). In contrast, much higher percentages of youth from the non-black groups reported an arrest than found in the official data. The ratio of participation for black youth relative to other groups ranges from 1.3:1 to 1.8:1.

Table 1 Official arrest summary by race/ethnicity: PHDCN waves 1–3, cohorts 12–18 ($n = 1775$)

	Total ($n = 1775$)	Black ($n = 641$)	Mexican ($n = 560$)	Puerto Rican/ Other ($n = 227$)	White ($n = 279$)	Other Race ($n = 68$)	Hypoth. Test	<i>P</i> -value
Number of arrestees	341	190	74	32	36	9		
% of Total <i>n</i>	19.2%	29.6%	13.2%	14.1%	12.9%	13.2%	70.447	0.000
Participation ratio: Black to other Groups			2.2	2.1	2.3	2.2		
Number of arrests	1093	659	223	89	102	20		
Mean # of arrests, All years (Total <i>n</i>)	0.62	1.03	0.40	0.39	0.37	0.29		
Mean # of Arrests, All years (Active arrestees)	3.21	3.47	3.01	2.78	2.83	2.22	0.780	0.539
Frequency ratio: Black to Other Groups			1.15	1.25	1.22	1.56		

Note: Chi-Square tests used to compare mean participation ratios across groups. *F*-tests used to compare the mean number of arrests across groups

Table 2 Self-report arrest summary by race/ethnicity: PHDCN waves 1–3, cohorts 12–18 ($n = 1775$)

	Total ($n = 1775$)	Black ($n = 641$)	Mexican ($n = 560$)	Puerto Rican/Other ($n = 227$)	White ($n = 279$)	Other Race ($n = 68$)	Hypoth. Test	<i>P</i> -value
Number of arrestees	379	179	88	39	58	15		
% of Total n	21.4%	27.9%	15.7%	17.2%	20.8%	22.1%	29.097	0.000
Participation ratio: Black to other Groups			1.8	1.6	1.3	1.3		
Number of arrests	1173	512	333	116	186	26		
Mean # of arrests, All years (Total n)	0.66	0.80	0.59	0.51	0.67	0.38		
Mean # of arrests, All years (Active arrestees)	3.09	2.86	3.78	2.97	3.21	1.73	1.509	0.199
Frequency ratio: Black to Other Groups			0.76	0.96	0.89	1.65		

Note: Chi-Square tests used to compare mean participation ratios across groups. *F*-tests used to compare the mean number of arrests across groups

As for the frequency of arrest among active offenders, it can be seen in Table 1 that the official arrest frequency for active black arrestees is higher than the frequency for other youth. In Table 2, it can be seen that the frequency of arrest for active arrestees is lower for black youth than all other race and ethnic groups, with the exception of the “Other Race” grouping. However, in all cases there are no statistically significant differences in the frequency of arrest between black youth and youth from the other racial and ethnic groups.

One logical explanation for the finding that the participation and frequency of arrests for black youth are lower in self-report data than official data, while participation and frequency is higher in self-report data for other groups, is because of reporting biases. Much research has questioned whether self-reporting biases are comparable across race. Generally, research confirms that under-reporting is significantly related to race and ethnicity, and that the validity of self-reported delinquency is lower for blacks than whites (Hindelang et al. 1981; Huizinga and Elliott 1986; Maxfield et al. 2000). A further examination of reporting is necessary to untangle the patterns of participation and frequency found in Tables 1 and 2.

One hundred fifty-five out of the 341 (45.5%) PHDCN youth officially arrested did not report any arrests in the self-report survey during any of the three interview periods. Furthermore, 195 out of the 379 (51.5%) youth that self-reported arrest did not have an official record during the 1995 to 2001 time period. Put another way, of the 834 subjects (80.8% of the sample) not officially arrested, 195 out of the 834 (23.4%) nonetheless reported being arrested. As reviewed in Section “Convergence on Individual’s records of arrest”, this figure is comparable to what has been found in other studies (Hardt and Petersen-Hardt 1977; Hirschi 1969; Maxfield et al. 2000). Still, there are evident inconsistencies across data sources on exactly which members of the sample were arrested.

Reporting does vary substantially by race and gender, where under-reporting is defined as self-reporting fewer arrests than found in official data and over-reporting is defined as reporting more arrests. Because subjects who were not officially arrested at any point from 1995 to 2001 cannot by definition under-report their arrests, findings described next are given only for the subset of subjects who were officially arrested at least once at some point from 1995 to 2001. Black youth are significantly more likely to under-report the number of

times they have been arrested than non-blacks ($\chi^2=5.250$, $df=1$, $P = 0.022$), but are not any more or less likely to over-report the number of arrests ($\chi^2=1.287$, $df=1$, $P = 0.257$). Whites are significantly less likely to under-report than non-whites ($\chi^2=7.500$, $df=1$, $P = 0.006$), but they are not any more or less likely to over-report ($\chi^2=0.066$, $df=1$, $P > 0.500$). Mexicans are not any more or less likely to under-report or over-report than other ethnic and racial groups ($\chi^2=0.018$, $df=1$, $P > 0.500$ for under-report; $\chi^2=0.173$, $df=1$, $P > 0.500$ for over-report). Similarly, Puerto Ricans are not any more or less likely to under-report or over-report than other ethnic and racial groups ($\chi^2=1.655$, $df=1$, $P = 0.198$ for under-report; $\chi^2=0.792$, $df=1$, $P = 0.374$ for over-report). Finally, males are significantly more likely to over-report than females ($\chi^2=5.406$, $df=1$, $P = 0.020$), but are not any more or less likely to under-report ($\chi^2=0.010$, $df=1$, $P > 0.500$).

Research consistently finds much continuity in criminal behavior with age, such that arrest at one age is highly associated with, or highly predictive of, arrest at subsequent ages (Farrington et al. 2003). This continuity in behavior implies that repeated measures of arrest are positively correlated, and two goals of longitudinal research are to describe the correlation between measures of a dependent variable across multiple time points and to account for the correlation structure. Presented in Table 3 are the autocorrelation functions between arrest frequency at different time points for each of the two data sources. Recall that there are exactly seven observations per-person in the official data, and up to seven observations per-person in the self-report data. The first row of the table displays the correlation between time points spaced one year apart (e.g., between time 1 and time 2; between time 5 and time 6). The second row displays the correlation between time points spaced two years apart (e.g., between time 1 and time 3); and so on for subsequent rows of the table. Findings illustrate that the correlations between arrest observations in the official data are substantially greater than in the self-report data. Thus, there is greater continuity in arrest revealed in official data than in self-report data.

The descriptive findings just presented answer the first question from Piquero et al. (2003), whether the use of different measurement approaches provides similar conclusions about criminal career dimensions. In summary, participation and frequency are higher in self-report data than in the official data except for black youth, and the average age of onset is lower in the self-report data. There is much greater continuity in arrest in the official data. Given these discrepancies across data sources on reporting, prevalence, frequency, onset, and continuity, it is necessary to now determine whether theoretical expectations derived from life-course studies apply equally to self-report and official criminal records.

Results: within-person convergence

Table 4 shows results for Models 1, 2, and 3. Findings from Model 1 can be used to determine if the shape of age-arrest trajectories are similar across data sources. Results from Model 1 demonstrate that there is a moderate difference in the initial level of arrest

Table 3 Autocorrelation between time points, Self-report and official arrest counts

Time lag	Official arrest	Self-report arrest
1	0.459	0.177
2	0.303	0.205
3	0.225	0.107
4	0.168	0.064
5	0.129	0.065
6	0.123	0.005

Table 4 Demographic and family correlates of age-arrest trajectories for self-report and official measurement sources, PHDCN Cohorts 12–18

Fixed effect	Model 1				Model 2				Model 3			
	Self-report	Police	Hypoth.	Hypoth.	Self-report	Police	Hypoth.	Hypoth.	Self-report	Police	Hypoth.	Hypoth.
	Coef. (SE)	Coef. (SE)	Chi-Square	test	Coef. (SE)	Coef. (SE)	Chi-Square	test	Coef. (SE)	Coef. (SE)	Chi-Square	test
Expected count of arrest, age 18												
Intercept	-3.055 (0.084)***	-3.694 (0.098)***	56.673***		-3.505 (0.138)***	-3.932 (0.110)***			-3.552 (0.140)***	-3.958 (0.105)***		
White					-0.676 (0.295)*	-1.439 (0.217)***	8.745**		-0.438 (0.304)	-1.086 (0.221)***		
Mexican					-0.698 (0.210)***	-1.400 (0.209)***	11.567**		-0.441 (0.241)	-1.199 (0.226)***		
Puerto Rican/					-0.661 (0.256)**	-1.038 (0.271)***	1.742		-0.644 (0.256)**	-1.022 (0.253)***		
Other Latino					-0.967 (0.382)*	-2.085 (0.489)***	4.996*		-0.722 (0.383)	-1.866 (0.433)***		
Male					1.745 (0.193)***	1.739 (0.186)***	0.001		1.813 (0.186)***	1.799 (0.181)***		
Cohort 15					0.106 (0.307)	-0.663 (0.200)***			0.114 (0.316)	-0.687 (0.206)***		
Cohort 18					0.577 (0.318)	-0.848 (0.254)***			0.606 (0.330)	-0.859 (0.250)***		
Family SES									-0.022 (0.072)	-0.182 (0.072)*	5.263*	
Married parents									-0.798 (0.233)***	-0.687 (0.206)***	0.016	
Age/growth (per year)												
Intercept	0.030 (0.032)	0.423 (0.031)***	389.670***		-0.058 (0.091)	0.557 (0.064)***			-0.058 (0.095)	0.572 (0.061)***		
White					-0.132 (0.073)	0.138 (0.153)	13.717***		-0.112 (0.085)	0.072 (0.163)		
Mexican					0.002 (0.085)	-0.064 (0.091)	2.560		0.013 (0.081)	-0.071 (0.086)		
Puerto Rican/					0.204 (0.134)	0.082 (0.111)	2.539		0.217 (0.128)	0.098 (0.110)		
Other Latino												
Other race					0.126 (0.196)	0.065 (0.284)	0.169		0.126 (0.205)	0.018 (0.244)		
Male					0.170 (0.060)**	0.007 (0.064)	11.773***		0.176 (0.061)**	0.010 (0.065)		
Cohort 15					-0.151 (0.189)	-0.011 (0.149)			-0.120 (0.197)	-0.007 (0.150)		
Cohort 18					-0.450 (0.266)	-0.408 (0.169)*			-0.386 (0.279)	-0.396 (0.169)*		
Family SES									-0.026 (0.028)	0.042 (0.019)*	18.021***	
Married parents									-0.035 (0.070)	0.060 (0.069)	4.890*	
Age ²												
Intercept	-0.035 (0.007)***	-0.085 (0.007)***	113.653***		0.005 (0.019)	-0.102 (0.019)***			0.001 (0.020)	-0.113 (0.021)***		
White					-0.008 (0.021)	-0.050 (0.027)	8.238**		-0.006 (0.024)	-0.036 (0.027)		
Mexican					-0.017 (0.018)	0.008 (0.018)	6.819**		-0.041 (0.017)*	0.019 (0.016)		

Table 4 continued

Fixed effect	Model 1			Model 2			Model 3		
	Self-report Coef. (SE)	Police Coef. (SE)	Hypothesis test Chi-Square (SE)	Self-report Coef. (SE)	Police Coef. (SE)	Hypothesis test Chi-square (SE)	Self-report Coef. (SE)	Police Coef. (SE)	Hypothesis test Chi-Square (SE)
Puerto Rican/Other Latino				-0.019 (0.025)	-0.069 (0.032)*	6.369*	-0.020 (0.025)	-0.069 (0.032)*	
Other race				-0.005 (0.037)	0.025 (0.048)	0.727	-0.013 (0.039)	0.052 (0.043)	
Male				-0.003 (0.019)	0.010 (0.018)	1.389	-0.072 (0.018)	0.008 (0.018)	
Cohort 15				0.068 (0.042)	-0.163 (0.048)***		0.076 (0.040)	-0.162 (0.047)***	
Cohort 18				0.042 (0.045)	-0.019 (0.042)		0.029 (0.049)	-0.014 (0.042)	
Family SES							-0.022 (0.008)**	-0.002 (0.005)	30.177***
Married parents							0.037 (0.016)*	-0.041 (0.018)*	60.881***

* $P < 0.05$ ** $P < 0.01$ *** $P < 0.001$

Note: Unit of analysis is the person-year, and the outcome is the person-year count of arrests

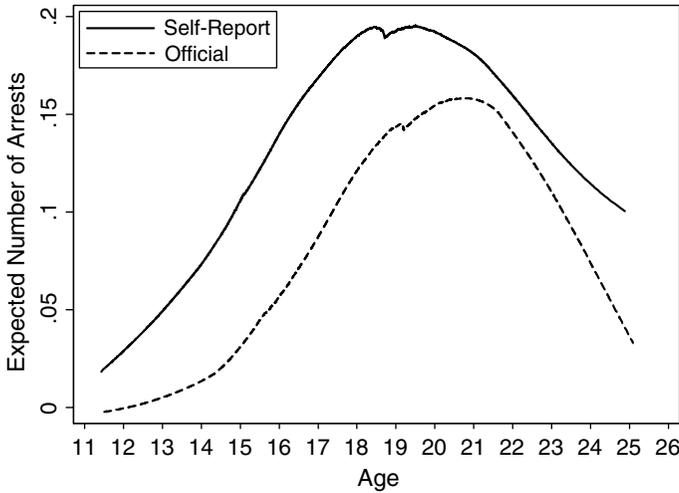


Fig. 1 Age-Arrest curves by data source; PHDCN Cohorts 12–18

(at age 18) across data sources (the intercepts). It can be seen that there is significant growth in the official trajectory. However, the linear growth coefficient for the self-report trajectory is not significantly different than zero. Importantly, hypothesis tests reveal that the differences in coefficients across outcome variables (i.e., self-reported arrest versus official arrest) are statistically significant. Substantively, these findings suggest that, on average, each individual's self-report age-arrest trajectory is statistically different than their official age-arrest trajectory.

To further demonstrate the differences in arrest across data sources, Fig.1 displays the expected age-arrest curves for ages 11 to 26, constructed from fitted values.¹ Here, the self-report curve peaks earlier than the official curve, and there is a constant gap between the two curves until around age 20. Also noteworthy is that the self-report trajectory remains fairly flat from ages 18 to 20. This finding visually illustrates why the linear growth coefficient in Model 1 is close to zero for the self-report trajectory. Overall, the two curves appear to have similar shapes up until the peak, but the expected count of arrests is substantially lower with the official data, and the peak of the curve is located to the right of the self-report trajectory. Furthermore, after the peaks, the official trajectory has a much steeper decline in arrest.

Results: person-level effects

Results from Model 2 in Table 4 demonstrate that the expected count of arrest for males is significantly greater than the expected count for females in both data sources. Furthermore, there are also significant gender differences in the growth of arrests in the self-report data, though not with the official data. Hypothesis tests show that there is not a statistically significant difference in the association between gender and the expected count of arrest at age 18 across data sources, but the difference in the relation between gender and change in arrest is significant.

¹ All plots of age-arrest trajectories are constructed with fitted values from the Level-1 residual file in HLM.

Moving to the issue of race and ethnicity, findings from Model 2 in Table 4 reveal substantial differences between black youth and youth from other racial and ethnic groups on the expected count of arrests at age 18. This finding holds for both self-reported arrests and official arrests. However, the size of the gap in arrest between groups does vary by data source. For example, hypothesis testing reveals that the official arrest gap between white and black youth is significantly greater than the self-report gap ($\chi^2=8.745, df=1$). In other words, there is significantly less disparity in arrest in the self-report data than in the official data. Thus, inferences about black–white differences in arrest depend upon the type of data examined. The same conclusion is true about the differences between black and other racial and ethnic groups.

Visually, the black–white differences in arrest trajectories can be seen in Fig.2, which is a plot of the estimated trajectories for males for each group. This Figure shows that the black–white gap in the expected count of arrests at age 18 is greater in the official data than in the self-report data. It is not until after age 18 that the black and white self-report trajectories diverge. However, even more interesting than the difference in trajectories at age 18 is the overall shape of the trajectories. The black and white official trajectories peak at roughly the same age, but there is a considerable gap in arrests between the two trajectories. These two trajectories are very similar until age 15, and then the official black trajectory abruptly accelerates. With the self-report data, after the black and white trajectories diverge at age 18, the white trajectory has a much steeper decline in arrests, albeit flat by comparison to the official trajectories.

Figure 2 also allows for an assessment of the similarities between white self-report versus white official trajectories, and also between the two black trajectories. First, the two black trajectories cross on two occasions. With the white trajectories, the official trajectory is always lower. Second, the peak count of arrest occurs at roughly the same age in the self-report and official data for blacks, but not for whites. Third, for both blacks and whites, the official data depicts what is generally accepted to be the shape of the age-crime curve, with a sharp increase and rapid decline after the peak level of offending. With the self-report data, the decline in arrests is very gradual for whites and almost non-existent for blacks.

Model 2 in Table 4 demonstrates that there are significant cohort differences in arrest trajectories in the official data, though not in the self-report data. These findings reveal that,

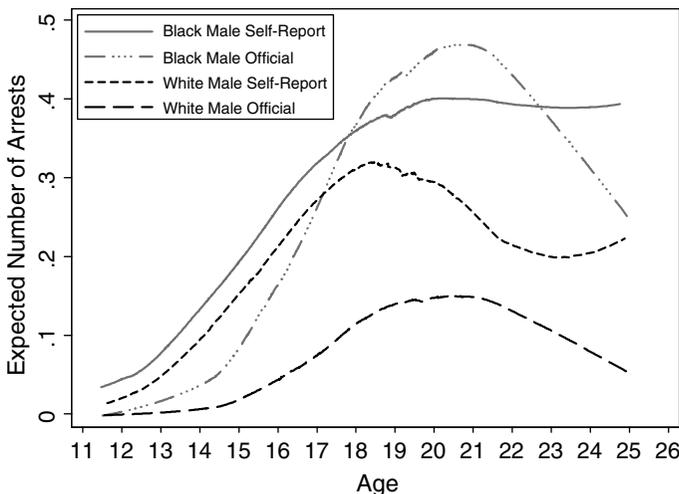


Fig. 2 Age-Arrest Curves by data source, gender, and race; PHDCN cohorts 12–18

on average, the 12 year-old cohort had significantly more official arrests at the age of 18 than did the 15 and 18 year-old cohorts, when members of these cohorts were age 18. The next section addresses potential reasons for these findings.

Results from Model 3 in Table 4 reveal that there is a significant negative association between family socioeconomic status and the expected arrest count at age 18 in official police data, but not in self-report data. While hypothesis testing reveals that the association between family socioeconomic status and arrest does vary across data sources, the association between the marital status of biological parents and arrest at age 18 does not vary across data types. With both data sources, there is a substantial difference in the count of arrests between youth with married parents and those without married parents.

Results to this point suggest that there are some key differences across data sources in the patterning of arrest over the course of adolescence and early adulthood, and also key differences in the association between arrest and both demographic and family structure correlates of arrest. What remains unanswered is whether the key predictors of arrest are similar across data sources. Even if the onset, level, and decline in arrest are different for self-report and official arrest data, the predictive power of family process, peer influence, and neighborhood characteristics may still be comparable across data types.

Results presented in Table 5 concentrate on the intercept term in Eq. (2). Thus, analyses focus on assessing the comparability of predictors of arrest at age 18. Note that analyses exclude data from cohort 18, as self-report measures of family processes were not collected for this cohort. Results illustrate that the key predictors of arrest function similarly across arrest measures, with the exception of the role of deviant peers. It can be seen that family supervision has little effect on either measure of arrest, after controlling for other relevant predictors. Parent–child conflict is a significant predictor of both measures of arrest, indicating that youth subject to greater levels of conflict and abuse are more likely to be arrested than youth with comparably lower levels of family conflict. Results also illustrate

Table 5 Family, peer, and neighborhood predictors of self-report and official arrest, PHDCN cohorts 12–15

Fixed Effect	Model 4				Hypoth. Test Chi-Square
	Self-Report		Police		
	Coef.	(SE)	Coef.	(SE)	
Expected count of arrest, Age 18					
Intercept	-2.713	(0.114) ^{***}	-2.882	(0.112) ^{***}	
White	0.183	(0.266)	-0.442	(0.244)	
Mexican	-0.040	(0.228)	-0.513	(0.213) [*]	
Puerto Rican/Other Latino	-0.330	(0.228)	-0.881	(0.234) ^{***}	
Other race	-0.854	(0.299) ^{**}	-0.891	(0.442) [*]	
Male	1.041	(0.138) ^{***}	1.489	(0.177) ^{***}	
Cohort 15	-0.085	(0.177)	-0.991	(0.178) ^{***}	
Family SES	-0.052	(0.060)	-0.081	(0.072)	
Married parents	-0.340	(0.180)	-0.818	(0.165) ^{***}	
Family supervision	-0.124	(0.101)	-0.041	(0.100)	0.539
Parent–Child conflict	0.339	(0.091) ^{***}	0.277	(0.095) ^{**}	0.350
Deviant peers	0.656	(0.086) ^{***}	0.295	(0.088) ^{***}	10.333 ^{**}
Neighborhood disadvantage	0.118	(0.110)	0.220	(0.103) [*]	0.582
Age/growth (per year)	0.063	(0.069)	0.464	(0.042) ^{***}	
Age ²	-0.027	(0.013) [*]	-0.105	(0.019) ^{***}	

* $P < 0.05$ ** $P < 0.01$ *** $P < 0.001$

Note: Unit of analysis is the person-year, and the outcome is the person-year count of arrests

that official police arrest is significantly more likely for youth who live in disadvantaged neighborhoods, after controlling for the composition of those neighborhoods. However, neighborhood disadvantage is not significantly associated with self-reported arrest. That said, the size of the neighborhood disadvantage coefficient is not significantly different across data sources. Finally, results in Table 5 show that association with deviant peers increases the likelihood of arrest across both arrest measures. However, the coefficient is significantly greater for the self-report measure of arrest than for the official measure. Thus, the association between deviant peers and arrest is greater in the self-report data.

Discussion and implications

The primary objective of this study was to compare and contrast inferences about the age-arrest relation across data sources, and to examine whether the association with key covariates and predictors is the same across data sources. Similar to prior research (e.g., Farrington et al. 2003), findings reveal a number of similarities and differences across data sources.

Descriptive summary of arrest

Addressing the research questions posed at the outset of the paper, descriptive results indicate that more respondents self-reported an arrest (21.4% of the sample) than found in the official data (19.2%). Frequency of arrest is also higher in self-report data than in the official data except for black youth, and the average age of onset is lower in the self-report data. Furthermore, there is much greater continuity in arrest in the official data. Results from the bivariate model analysis (as shown in Fig. 1) also show a wide gap in the average age-arrest trajectories across data sources, particularly until the age of 21. Thus, self-report and official data yield contrasting inferences about the age-arrest relation, in the sense that the expected number of yearly arrests is statistically different. Additionally, the peak age of arrest is later for the official data, and the decline following the peak of the age-arrest curve is much steeper. It should not be overlooked that 45.5% of youth officially arrested did not report any arrests in the self-report survey during any of the three interview periods, and that 23.4% of those subjects without an official record nonetheless self-reported being arrested. Taken together, these results imply that self-report indicators of arrest utilized in this study likely suffer from a number of problems common in self-report survey designs, namely response falsification and recall error. Furthermore, similar to Geerken's (1994) findings, the use of aliases may account for a portion of the instances when subjects self-reported an arrest that was not contained in official data.

Correlates and predictors of arrest

Results suggest that there are some significant and substantial differences in the correlates of arrest. While race and ethnicity tend to be strongly associated with both self-reported and official arrest, the gap in the expected count of arrest between black youth and other youth is significantly greater in the official data. Additionally, results presented in Tables 1 and 2 reveal that participation and frequency of arrest are greater in the official data than in the self-report data for all groups except for blacks. One plausible conclusion to draw from these findings is that under-reporting is relatively more severe for black youth, a conclusion consistent with prior research (Hindelang et al. 1981; Huizinga and

Elliott 1986). Findings also reveal that, for SES, there is a significant negative association with the initial level of arrest in official data, but not in self-report. The association between the marital status of parents and arrest at age 18 does not vary across data types.

As for the family, peer, and neighborhood predictors, findings demonstrate that the effect of family supervision, parent–child conflict, and neighborhood disadvantage appear to operate similarly across arrest measures. However, the effect of deviant peers on arrest at age 18 differs across data sources, such that the association between deviant peers and arrest is much greater for self-reported arrest. Still, association with deviant peers is a significant predictor of both measures of arrest, so the difference is simply one of magnitude.

Findings reveal significant cohort differences in arrest in the official data, with the youngest cohort having more predicted arrests at age 18. If anything, it reasons that the opposite would be true given the decline in crime in the 1990s, which is the time frame of the data. One potential reason for this finding is reform of the juvenile justice system in Illinois. The Illinois Juvenile Justice Reform Act of 1998 made a number of changes to the way juvenile arrestees are processed, which may have influenced the reporting of arrests even if the actual number of arrests (reported and unreported) remained the same. For example, disposition of juveniles arrested for a crime are handled a number of ways by juvenile police officers, who generally decide between issuing a “station adjustment” or referring the case to juvenile court. A station adjustment is an informal handling of arrests for youths with a limited prior history of delinquency, where the adjustment most often leads either to the unconditional release of the youth without any prosecution or supervision, or to the conditional release of youth with a community service or supervision component stipulated. Reforms in 1998 introduced a distinction between formal and informal station adjustments, and put a limit on the number of station adjustments a juvenile could receive (Illinois Criminal Justice Information Authority 2005). Whether these or other changes altered reporting practices by police is unknown, but it offers one potential reason for why there were significantly more officially reported arrests for the 12 year-old cohort at age 18 than the other cohorts. Furthermore, this example offers one justification for combining data sources when examining arrest. Policy reforms and changing police practices can potentially create inconsistencies in official arrest data. Self-report data can then be used to examine whether official arrest patterns do show any irregularities (e.g., significant cohort differences).

In sum, descriptive findings illustrate that a sizable number of youth self-report being arrested without having a corresponding arrest record, and a sizable proportion of those youth with an official arrest record fail to self-report that they had been arrested. Results also illustrate that the age–arrest relation and the association between demographic characteristics and arrest trajectories tend to vary across the two data sources. That said, despite significant differences across the two arrest measures on many criminal career dimensions, the effects of family supervision, parent–child conflict, and neighborhood disadvantage are not dependent upon the type of arrest data researchers choose to utilize. In other words, even if there are inconsistencies across arrest measures on who was arrested, when, and how often, it is still the case that arrestees are more common in abusive families who reside in disadvantaged neighborhoods. At a more general level, results suggest that research questions designed to address within-individual change in crime may produce divergent findings across data sources. However, research questions that aim to explain between-person variability in crime (e.g., because of parent–child conflict) are more apt to produce similar results across official and self-report crime measures.

Future research

Findings presented in this study are suggestive of numerous extensions. First, the present study has provided a partial glimpse as to whether theoretical expectations derived from life-course studies apply equally to self-report and official criminal records by examining the family, peer, and neighborhood predictors of arrest in late adolescence. Future research should proceed by examining whether the effects of predictors during adulthood are similar across data sources. More generally, researchers should examine whether the factors influencing the processes of persistence of and desistance from crime are similar across data sources.

Second, while many studies have used official records as a check on reporting in self-report surveys, particularly under-reporting, little has been done to assess how reporting bias varies over time (an exception is Thornberry and Krohn 2003). If an individual's under- or over-reporting is stable over time, then it will not affect inferences about the shape of their self-reported trajectory. If reporting is a function of time or age, then inferences about the shape of the age-arrest curve may be biased. Recall that blacks are more likely to under-report arrest relative to other groups, and evidence from Fig. 2 shows that the expected number of self-reported arrests for blacks is lower than official arrests at the peak of their arrest trajectories, from age 18 to 22. One logical conclusion that follows from these findings is that under-reporting is more severe for blacks at around the peak arrest level. Further research is needed to determine how reporting biases vary over time.

A third extension relates to the divergence in the prevalence, incidence, onset, and continuity of arrest across data sources. Much attention and debate in recent years has been placed on defining typologies of criminals, for example chronic versus low-rate offenders. In one typology, Moffitt (1993) argues that there are developmentally distinct groups of offenders (i.e., adolescent-limited and life-course persistent), with each group having a distinct developmental etiology and associated risk factors. Other researchers argue that it is impossible to define such groupings prospectively based on a set of childhood risk factors (Laub and Sampson 2003). Regardless of whether chronic offenders can be prospectively identified, research has consistently shown that most crimes are committed by a small group of offenders (see, e.g., Wolfgang et al. 1972). Nevertheless, because of differences in the prevalence, incidence, and onset of arrest in self-report versus official data, one could question whether the identification of chronic offenders would be the same across data sources. The same is true for finding other offending types (e.g., late onset, desisters, persisters, intermittent offenders). Dunford and Elliott (1984) provided perhaps the first comparison across data sources on the grouping of subjects into criminal career typologies, and found stark inconsistencies across data sources as to whether individuals were classified as career offenders, non-career offenders, or non-offenders. However, Dunford and Elliott's typology is somewhat crude, in that they defined career offenders as those youth who committed offenses for just two or more consecutive years. Use of statistical tools like finite mixture models (Nagin and Land 1993) make it possible to identify approximate criminal types from longitudinal data without having to arbitrarily define the number of criminal types in advance, or the number of offenses and the duration of criminal activity. Future research on offender typologies using such advances in methodology and statistical tools should examine whether findings are a function of the type of crime data used (i.e., self-report or official).

Clearly more research must be done examining whether inferences about criminal careers are robust to the type of data used, in this case self-report arrest versus official arrest. Findings thus far suggest that there are some differences across data types, and that the integration of both data sources is beneficial in order to understand the life-course of crime.

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