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Juvenile Arrest and Later Economic Attainment: Strength and Mechanisms of the Relationship

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Abstract

Objectives We tested the impact of juvenile arrest on asset accumulation, debt accumulation, and net worth from ages 20–30. We also examined whether indicators of family formation, school and work attainment, and subsequent justice system contacts explained any effects.

Methods We used longitudinal data on 7916 respondents from the National Longitudinal Survey of Youth 1997 Cohort. Our treatment variable was a dichotomous indicator of whether respondents were arrested as juveniles. Our focal outcomes were combined measures of the values of 10 types of assets, 6 types of debt, and net worth (assets minus debt) at ages 20, 25, and 30. We used propensity score methods to create matched groups of respondents who were and were not arrested as juveniles, and we compared these groups on the outcomes using multilevel growth curve analyses.

Results Arrested juveniles went on to have lower assets, debts, and net worth during young adulthood compared to non-arrested juveniles. These differences were most pronounced at age 30. The differences were largely explained by educational attainment, weeks worked, and income.

Conclusions The fact that juvenile arrest predicted early adult economic attainment net of 43 matching covariates provides strong evidence that these effects are not merely artifacts of selection. The additional finding that education, employment, and income explain much of the juvenile arrest effect highlights several potential areas of intervention for protecting young arrestees' later net worth.

Keywords Juvenile arrest · Assets · Debt · Economic attainment

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Introduction

One in six youth in the U.S. will be arrested before their 18th birthdays (Brame et al. 2012). Most will be processed by the juvenile justice system, which was designed in part to protect juvenile offenders against the effects of long-term stigma and adult punishment. Yet these protections, and observers' confidence in them, may be eroding. In part this is because legal protections for juveniles are not as universal as is commonly assumed; for example, only half of states allow the sealing or expungement of juvenile police or court records (Shah et al. 2014). It is also because a small but growing body of research suggests that youthful contacts with the justice system can have long-term consequences (e.g., Kirk and Sampson 2013; Tanner et al. 1999; Wiesner et al. 2010).

This paper examines the potential for juvenile arrests to disrupt longer-term pathways of economic attainment. Specifically, we test whether juvenile arrest predicts asset and debt accumulation across early adulthood. We also attempt to explain any effect of juvenile arrest by testing whether family formation, educational and work attainment, and subsequent criminal justice system contacts mediate the effect.

A Focus on Juvenile Arrest

Since the inception of the juvenile court, U.S. states' juvenile justice systems have been guided in part by the mission of acting in the best interests of the child (Radice 2017). For instance, most states' juvenile justice purpose statutes explicitly list parent-like functions and rehabilitation as central purposes of their juvenile systems (Radice 2017). This special treatment of juvenile offenders is grounded in the beliefs that juveniles are less responsible for their behavior than adults, that they are more easily rehabilitated, and that they should have opportunities for reform. Indeed, research shows that adolescents are still developing neurologically and psychosocially, and that compared with adults they have lower impulse control, are more susceptible to peer influence, and are less able to consider their actions in context (Cauffman and Steinberg 2000; Steinberg et al. 2009). Several recent U.S. Supreme Court decisions have referenced this body of work when placing a ceiling on punishment for juveniles (Cauffman et al. 2018). Both researchers and the courts thus have recognized a potential role for the juvenile justice system in helping young offenders desist from crime and achieve healthy transitions to adulthood (Chung et al. 2005).

The idea that juveniles should be shielded from long-term negative consequences of their behavior is consistent with the labeling perspective, which highlights the lasting problems that can result from the cascading consequences of youthful delinquency (Sampson and Laub 1997). Under this perspective, societal—most notably justice system—reactions to delinquency can cause harmful shifts in identities, peer groups, and conventional opportunities, and these shifts can undermine behavioral and other life course outcomes. Several studies, for instance, have shown that labeling processes following from initial justice system contacts contribute to the stability of offending behavior over time (e.g., Liberman et al. 2014; Wiley and Esbensen 2016; Wiley et al. 2013).This perspective thus draws attention to ways in which contacts with the juvenile justice system, which ideally would leave few permanent scars, might have lasting consequences for well-being.

Despite the stated mission of the juvenile justice system, there is evidence that contacts with that system cause long-term harm in a variety of life domains. This appears to be due in part to informal labeling processes; for example, police stops and arrests cause shifts in

youths' deviant attitudes, delinquent peer affiliations, and prosocial activities, which in turn perpetuate deviance (Wiley et al. 2013). It also is due to formal labeling processes: Having a juvenile record can result in suspension or expulsion from school, non-acceptance by colleges, and disqualification from jobs, and can have additional consequences for life pursuits such as public housing and immigration applications, military enlistment, and even driver licensing (Radice 2017). Compounding this, get-tough-era policy changes increased the resemblance between the juvenile system and the adult criminal justice system, weakening the protections that juvenile defendants once had (Butts and Mitchell 2000; Willison et al. 2009). Accordingly, although the mission of the juvenile system should create age-graded effects of justice system contacts, such that juveniles are more protected from the collateral consequences of those contacts, juveniles still may experience negative outcomes.

Effects of Juvenile Arrest on Attainment

Sampson and Laub's (1997) formulation of the labeling perspective emphasizes cumulative disadvantage processes, which capture the pathways from juvenile delinquency to weakened social and institutional bonds to adult offending. Intermediate socioeconomic outcomes play a large role in this theorizing: They are a bridge between initial offending and justice system contacts and later adult offending and justice system contacts. Consistent with the first stage of the theoretical pathway, researchers have consistently found links between juvenile arrest and later socioeconomic attainment. For example, several studies have shown that teenage arrests and other forms of teenage criminal justice contact predict high school dropout and reduced educational attainment (e.g., Bernburg and Krohn 2003; Hirschfield 2009; Kirk and Sampson 2013; Sweeten 2006). A handful of others have linked teenage justice system contacts with later unemployment and reduced occupational attainment (De Li 1999; Tanner et al. 1999; Wiesner et al. 2003, 2010). Such links have contributed to concerns 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" (Sampson and Laub 1997: p. 147).

Despite these concerns, research on socioeconomic attainment outcomes has largely been limited to examining schooling and work status. Yet there is more to economic attainment and well-being than factors such as education, employment, and earnings (Martin 2011). Specifically, outcomes like assets, debts, and net worth are important in that they reflect accumulations of various socioeconomic experiences and in that they influence several other "life chances." Assets capture monetary and non-monetary things of value, such as the money in bank or retirement accounts, the values of stocks and bonds, and the values of houses and cars. Debts are amounts owed to lenders. Importantly, although many young adults have "bad" debt, which is spent on depreciating items, many also have "good" debt that has future value (Chiteji 2007; Houle 2014). This "good" debt helps young people achieve upward mobility (e.g., student loans enabling higher education) and key markers of adult status (e.g., mortgages enabling home ownership), and some amount of it may be necessary for the acquisition of important goods and experiences during this life stage (Chiteji 2007). Finally, net worth, a common measure of wealth, is the difference between an individual's or household's assets and debts (for a review see Killewald et al. 2017).

It is important to study assets, debt, and net worth in relation to juvenile arrest for four reasons. First, they are associated with adult offending (e.g., Aaltonen et al. 2016; Hoeve et al. 2016), and thus may be components of cumulative disadvantage processes. Second,

during the transition to adulthood they are only weakly correlated with conventionally examined indicators such as income (Killewald et al. 2017), and thus are distinct outcomes in their own right. Third, wealth influences several other life outcomes, including health, mortality, and household members' well-being (Killewald et al. 2017), as well as vulnerability to medical crises, layoffs, and other life shocks (Sykes 2003). And fourth, wealth is passed between generations; indeed, one generation's wealth influences the educational attainment, labor market outcomes, and wealth accumulation of not only the next generation, but also the one after that (Killewald et al. 2017; Pfeffer and Killewald 2018). Factors that impact wealth thus may have implications for the persistence of inequality across generations. In addition, because there are racial disparities in juvenile arrest (Brame et al. 2014), if these arrests do impact assets and debt, they may play a role in perpetuating racial disparities in wealth as well.

Harmful impacts of juvenile arrest on wealth outcomes may take the form of reduced assets, reduced "good" debt, increased "bad" debt, or reduced net worth. We are aware of only two studies that specifically examined the effects of arrests on young adult asset and debt attainment. First, Maroto (2015) used regression analyses and portions of the data that we use here to assess whether having ever been arrested by age 25 predicted various measures of age 25 wealth accumulation. She found that a previous arrest decreased financial assets by 45% and debt by 35%, though it did not decrease total net worth. In another study using a conditional change score analysis with the same data, Maroto and Sykes (2019) found that a new arrest between ages 25 and 30 decreased financial assets by 53% and debt by 52% over the same age range, but a new arrest did not decrease net worth.

These are the best estimates we have of the impact of early arrests on asset and debt accumulation. Still, several questions remain unanswered. First, Maroto (2015) examined arrest histories up to age 25, so her findings may speak more to the effects of "youthful" arrests—those occurring up to early young adulthood—than to the lasting effects of juvenile arrests specifically. And Maroto and Sykes' (2019) findings speak to adult arrests. Thus, we do not yet know the implications of *juvenile* arrests for these outcomes. Second, Maroto (2015) was unable to conduct causal analyses of the impact of arrests on asset and debt accumulation, so it is possible that her findings overstated the presence or size of that impact. And third, the youthful arrest analyses of Maroto's (2015) study did not include mediation analyses, so they did not reveal why any association between youthful arrests and later economic attainment might occur.

Potential Mechanisms

The labeling approach posits that juvenile offending harms later life outcomes by undermining connections to key institutions of social control, especially during the transition to adulthood. Specifically, Sampson and Laub (1997) argue that absent or problematic transitions into marriage and employment, and continued involvement in the (adult criminal) justice system, both follow from juvenile offending and prevent grown juvenile offenders from transitioning away from crime as they age. Because several different institutions of social control are relevant to adult offending, there are several potential "bridges" between early and later offending. Developmental scholars highlight the interconnectedness of these bridges, describing spreading or diffusing effects of youthful problem behavior on a variety of later outcomes (Masten and Cicchetti 2010). Identifying the possible intermediate outcomes between juvenile arrest and our focal outcomes, asset and debt attainment, would

not only help account for any attainment deficits, but also might identify early warning signs for longer-term problems.

Indeed, the same institutional factors identified by Sampson and Laub have also been linked with asset and debt accumulation in adulthood. Being married, having children, being employed, and having more education and earnings all predict increased wealth, and criminal justice system contacts predict decreased wealth (Bricker et al. 2012; Dynan and Kohn 2007; Maroto 2015; Maroto and Sykes 2019). Conceptually, family structure, education, and income all influence wealth by influencing household resources and saving and spending decisions (Killewald et al. 2017). Many of these intermediate outcomes are associated with juvenile justice system contacts (De Li 1999; Hirschfield 2009; Kirk and Sampson 2013; Liberman et al. 2014; Tanner et al. 1999). If marriage, employment, and other potential bridges are associated with juvenile arrests, then they could explain any observed associations between those arrests and later asset and debt attainment.

Theoretically, juvenile justice system contacts should be associated with non-normative or "off-time" adult role transitions, which could include either precocious or delayed entry into those roles (Siennick and Widdowson 2017). For instance, under this perspective, arrested juveniles might have an increased risk of becoming teen parents, but might not marry until well after their peers, if they do so at all. There is mixed evidence on the association of juvenile justice contacts with family formation. An early study by Knight et al. (1977) found that delinquency did not predict marrying before age 21. Sampson and Laub's (1990) later study, though, suggested a harmful effect of official juvenile delinquency on the risk of separation or divorce by early adulthood. Studies using samples with wider age ranges have also found harmful effects of offending and justice system contacts on marriage (Apel et al. 2010; Apel 2016; Huebner 2005, 2007; King and South 2011; Raphael 2007; van Schellen et al. 2012). The association of juvenile justice contacts with subsequent parenthood is much less studied, though juvenile conduct problems predict early entries into parenthood (Woodward et al. 2006). Since both marriage and parenthood promote wealth accumulation, juvenile arrests could worsen socioeconomic outcomes either by delaying marriage or by speeding parenthood.

There is more evidence for the association of juvenile justice system contacts with markers of school and work attainment that predict later wealth, such as years of education, employment status, and earnings (Maroto 2015). Formally labeled youths have generally lower status achievement during the transition to adulthood (De Li 1999). More specifically, they leave school earlier than non-labeled youths (Hjalmarsson 2008; Kirk and Sampson 2013; Widdowson et al. 2016), and they have lower rates of employment during their late teens and early twenties (Apel and Sweeten 2010b; Bernburg and Krohn 2003; Tanner et al. 1999). Since education, employment, and earnings are all positively associated with wealth accumulation, any association of juvenile arrests with assets, debt, and net worth could operate through these intervening factors.

Finally, an even larger body of work has linked adult criminal justice system contacts with attainment. These contacts predict not only family, educational, and employment outcomes, but also wealth acquisition (Apel et al. 2010; Bushway 1998; Huebner 2005, 2007; Maroto 2015; Raphael 2007; Turney and Schneider 2016). Because there is continuity in justice system contacts over time (Liberman et al. 2014), it is possible that subsequent justice system contacts link initial juvenile arrests with later asset and debt accumulation. That is, even if a juvenile arrest alone results in insufficient intermediate collateral consequences to influence net worth, it could influence it by triggering continued involvement in the justice system. We test whether this is the case.

The Current Study

This study makes several notable contributions to the literature. First, it builds upon past work on the long-term effects of juvenile arrests by examining the understudied outcomes of asset and debt accumulation across the entirety of young adulthood. Second, it employs matching analyses to minimize the influence of selection and isolate the causal impact of those arrests. And third, it explains that impact through a series of mediation analyses that reveal the mechanisms by which juvenile arrests undermine later economic well-being.

The mechanisms that we examine, and socioeconomic attainment more broadly, represent key markers of adulthood: transitioning from school to work, supporting a family, and being generally self-sufficient (Furstenberg 2010). Each of these markers is strongly age-graded, such that studenthood and financial dependency are normative during the late teens and early twenties but work and financial independence are normative during the later twenties. This means that major life problems that might be examined in relation to juvenile arrests, including problems with asset and debt accumulation, may not yet have emerged among younger individuals. For this reason, we test for age-graded effects of juvenile arrests on our focal outcomes.

Method

Data

The data for this study came from waves 1–17 of the National Longitudinal Survey of Youth 1997 Cohort (NLSY97). The NLSY97 is a survey study of a national sample of 8984 youth who were living in the U.S. in 1997 and who were born between 1980 and 1984. The study contains two probability-based household samples: (1) a nationally representative sample of 6748 youths and (2) an additional over-sample of 2236 Black and Hispanic youths. NLSY97 respondents were interviewed annually from 1997 to 2011 (waves 1–15) and biennially in 2013 (wave 16) and 2015 (wave 17). The retention rate in the study is high, with 79% of participants being re-interviewed in 2015 (and 84% in either 2013 and/or 2015).

We made the following restrictions to the sample. First, from the full sample, we selected respondents who participated in at least one Asset interview (described below; $N = 8687$). Second, we selected respondents who did not report an arrest at wave 1 ($N = 7964$).[1] We did this to ensure that our control variables were measured prior to treatment in order to avoid endogeneity bias (Apel and Sweeten 2010a, b: pp. 558–559). Lastly, we removed a small number of respondents ($N = 48$) who were incarcerated as juveniles to ensure that our treatment variable—juvenile arrest—was not conflated with other forms of criminal justice contact. The final analytical sample was comprised of 7916 respondents. To address missing data, we implemented multiple imputation using chained equations

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[1] We compared our focal juvenile arrest group with the group of respondents who were arrested before wave 1. The groups differed on 12 of the 43 matching variables. Specifically, those arrested before wave 1 had more school problems, poorer health, less prosocial and more antisocial peers, and higher levels of deviant behavior. The groups had comparable demographic characteristics and socioeconomic and family backgrounds.

with the *mim* suite available in Stata 16 (StataCorp, College Station, TX).[2] In doing so, we created 20 imputed datasets. Standard errors were calculated using Rubin's (1987) rules which accounts for variance between and within the imputed datasets.

Measures

Focal Independent Variable: Juvenile Arrest

The key independent variable (or the *treatment*) in our study is a dichotomous indicator of whether respondents were arrested as juveniles (0 = no arrest, 1 = arrest). At each wave, respondents reported on their contact with the criminal justice system; those who reported being arrested were asked for the month and year of each arrest. We used the information on the dates of each arrest combined with information on respondents' state of residence at each wave to determine whether respondents were arrested as juveniles.[3] Information on respondents' states of residence was needed because different states set different age limits for juvenile versus adult adjudication. We used the age limits reported by Snyder and Sickmund (2006, p. 103), who found that the oldest age for original juvenile court jurisdiction in delinquency matters in 2004 was age 15 in Connecticut, New York, and North Carolina; age 16 in Georgia, Illinois, Louisiana, Massachusetts, Michigan, Missouri, New Hampshire, South Carolina, Texas, and Wisconsin; and age 17 in Alabama, Alaska, Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Florida, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Maryland, Minnesota, Mississippi, Montana, Nebraska, Nevada, New Jersey, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Dakota, Tennessee, Utah, Vermont, Virginia, Washington, West Virginia, and Wyoming. Unfortunately, we do not have information on whether respondents were individually transferred to adult court following juvenile arrests. However, these transfers account for only 10% of all cases in which juveniles are processed as adults (Griffin et al. 2011).

Dependent Variables: Young Adult Assets and Debts

Our focal dependent variables are three measures that capture respondents' asset and debt accumulation across young adulthood. At approximately ages 20, 25, and 30 respondents were asked about the types and amounts of different assets and debts that they held.[4] *Assets*

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[2] We imputed all variables with item missingness (i.e., cases where respondents refused or skipped the question, or did not know the answer; or in some cases, where respondents were purposely skipped as part of the NLSY97 design). Each predictive equation included the other study variables, both those with no missingness (i.e., gender, age, region, concentrated disadvantage, percent Black, number of siblings, and an indicator for asset interview) and those that had missingness. The treatment variable (juvenile arrest) was not included in the imputation model. The average amount of missingness across all variables was low (2.7%). There were some variables with notable missingness, including parental education (5.0%), assets (9.9%) and net worth (11.7%), ASVAB score (19.9%), household income (26.6%), and income-to-poverty ratio (26.9%). As noted below, the substantive conclusions were the same under listwise deletion.

[3] Information on respondents' state of residence is not in the public use version of the NLSY97; instead, the second author applied to the Bureau of Labor Statistics for access to the restricted geocode file and was granted access. The geocode file contains respondents' the state and county of residence at each wave.

[4] The NLSY97 also collected asset and debt information at age 35. However, because only a small percentage (14.4%) of respondents had reached age 35 by wave 17, most respondents had missing information on the age 35 asset/debt interview. We therefore excluded the age 35 asset/debt observations.

includes the total value across 10 different categories of financial, non-financial, and housing assets (i.e., retirement/pension accounts, bank or money market accounts, bonds or certificates of deposit, stocks held, trusts or annuities, motor vehicle value, home furnishing value, business/partnership assets, real estate assets, and primary housing assets). *Debts* includes the total owed on 6 different categories of debt (i.e., amount owed on a motor vehicle loan; amount owed on government or family student loans; amount owed on a personal loan borrowed from family and/or friends; amount owed on a primary housing mortgage; and balances carried on store bills, credit cards, loans obtained through a bank or credit union, margin loans, or other installment loans). In supplemental analyses we also examine each type of debt separately. *Net worth* is calculated by subtracting respondents' total debt from their total assets (assets–debts). Most of these measures come from what is known as the NLSY97 Assets interviews.[5] We adjusted these measures to 2015 dollars to account for inflation. We also log transformed our measures of assets and debts to reduce skewness; supplemental analyses repeated our main models using untransformed versions of these variables. Net worth was left untransformed due to the high number of cases with zero or negative values (see Killewald et al. 2017). On this measure, 16% of cases had negative net worth, 2% had zero net worth, and 82% had positive net worth.

Mediating Variables

We include a number of variables that might explain differences between arrested and non-arrested juveniles with respect to young adult asset and debt accumulation. These include demographic, economic, and criminal justice variables. Each variable is time-varying and measured at the time of respondents' age 20, 25, and 30 Asset interviews. *Marital status* is a dichotomous variable indicating whether respondents were married (0 = no, 1 = yes). *Parenthood* is a dichotomous variable indicating whether respondents had a child in their household (0 = no, 1 = yes). *Highest grade completed* is a continuous variable reflecting the highest grade of education respondents completed. *Weeks worked* is a continuous variable reflecting the number of weeks respondents reported working since the date of the last interview (typically 1 year prior, except for the last two waves, which were 2 years apart); this variable was log transformed to reduce skew. *Personal income* is a continuous variable reflecting respondents' total wages in the year preceding each interview; this variable was inflation-adjusted to 2015 dollars and log transformed to reduce skew. *Subsequent arrest* is a dichotomous variable indicating whether respondents reported having been arrested by police or taken into custody for an illegal or delinquent offense excluding minor traffic violations since the date of the last interview (0 = no, 1 = yes). *Subsequent conviction* is a dichotomous variable indicating whether respondents reported having been convicted or adjudicated delinquent or having pled guilty to any charges since the date of the last interview (0 = no, 1 = yes). *Subsequent incarceration* is a dichotomous variable indicating whether respondents reported having been sentenced to spend time in a jail or an adult corrections institute since the date of the last interview (0 = no, 1 = yes).

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[5] Although almost all of the items come from the Asset interview section of the survey, the items for age 20 government and family student loan come from the education section of the survey. We did this because the NLSY97 did not collect student loan information until wave 7 in the Asset interview section, and using student loan information from that section would have resulted in about half of cases having missing student loan data at age 20.

Pretreatment Control Variables

We used a wide range of background variables measured at the first wave to model respondents' propensity for juvenile arrest. Covariates were selected based on prior research in criminology and income stratification. Altogether, our analyses include 43 pretreatment control variables covering demographic characteristics, household structure, parenting practices, cognitive ability, prior school performance and engagement, and behavior, including delinquency and substance use. We do not discuss the coding of each variable here; rather, we refer readers to the appendix for a full description of each variable ("Appendix A"). Table 1 contains descriptive statistics on all study variables.

Analytical Strategy

We used propensity score matching to adjust for preexisting differences between arrested and non-arrested juveniles that may bias our estimate of the effect of juvenile arrest on asset and debt accumulation in young adulthood. This approach approximates an experimental design by comparing a treatment group (here, arrested juveniles) with an otherwise similar control group (non-arrested juveniles) that differs only on treatment status (Apel and Sweeten 2010a, b; Shadish et al. 2002). This is accomplished by matching treated and control cases based on their conditional probability of treatment given a vector of observed characteristics (Heckman and Hotz,1989; Rosenbaum and Rubin 1983). If matching is successful, groups will be balanced on the observed covariates, and the treatment effect can be estimated without bias. However, this strategy does not rule out the possibility of hidden biases from unobserved heterogeneity (Shadish et al. 2002).

Propensity score matching follows a series of steps. The first step involves assessing balance on the pretreatment covariates between treated and control cases prior to matching. We evaluated balance using t tests and standardized bias (SB) statistics; a variable is considered imbalanced if it has a t score great than the absolute value of 1.96 or a SB statistic greater than the absolute value of 20 (Rosenbaum and Rubin 1983). The second step involves modeling respondents' propensity for treatment (in our case, juvenile arrest) as a logit function of the pretreatment covariates. The propensity score, which is bounded between 0 and 1, represents respondents' predicted probability of being arrested as a juvenile. The third step involves using a matching algorithm to match each treated case to one or more control cases with similar propensity scores. We present the results from kernel density matching with replacement using a bandwidth of 0.03 because it resulted in the best fit; this algorithm matches each control case based on its distance to its matched treated case, giving more weight to cases that are closer. During this step, cases are discarded if they do not have a match within the designated bandwidth. The final step involves reassessing balance on the pretreatment covariates between treated and control cases after matching. If matching is successful, all group differences in the covariates should have t scores and SB statistics less than the absolute value of 1.96 and 20 respectively.

After deriving matching treated and control cases, we estimated a series of random effects models among the matched sample predicting asset and debt accumulation in young adulthood from juvenile arrest and dummy variables for each interview (i.e., age 25 interview; age 30 interview). Our main models featured the logged versions of the assets and debt variables, but supplemental models featured the untransformed versions to aid interpretation. Our random effects models consisted of two levels, where waves (level 1) were nested in individuals (level 2). Due to this nested data structure, the ordinary least squares

Table 1 Descriptive statistics on study variables ($N = 22{,}101$ observations on 7916 respondents). *Source*: NLSY97

	Mean	SE	Min	Max
Dependent variables				
Assets	76,257	1079	0	3,456,925
Assets (logged)	9.88	0.02	0	15.06
Debt	37,433	567	0	661,616
Debt (logged)	6.62	0.03	0	13.40
Net worth	38,824	778	− 399,006	3,262,767
Focal independent variable				
Juvenile arrest	0.08		0	1
Mediating variables				
Married	0.26		0	1
Parenthood	0.32		0	1
Highest grade completed	13.30	0.02	2	20
Weeks worked	56.48	0.40	0	889
Weeks worked (logged)	3.45	0.01	0	6.79
Personal income	22,424	187	0	316,456
Personal income (logged)	8.14	0.03	0	12.66
Adult arrest	0.05		0	1
Adult conviction	0.03		0	1
Adult incarceration	0.02		0	1
Pretreatment covariates				
Male	0.50		0	1
Age	14.31	0.02	12	18
Black	0.15		0	1
American Indian	0.01		0	1
Asian or Pacific Islander	0.02		0	1
Other race	0.08		0	1
Hispanic	0.13		0	1
1981 birth cohort	0.19		0	1
1982 birth cohort	0.20		0	1
1983 birth cohort	0.20		0	1
1984 birth cohort	0.21		0	1
Northeast	0.19		0	1
Midwest	0.26		0	1
West	0.21		0	1
Central city	0.26		0	1
Suburbs	0.54		0	1
Concentrated disadvantage	− 0.13	0.01	− 1.71	3.97
Percent Black	0.12	0.00	0.00	0.76
Two parent household	0.55		0	1
Number of siblings	1.53	0.01	0	5
Parental education	13.63	0.04	1	20
Household income (logged)	10.21	0.03	0	12.42
Income-to-poverty ratio	3.30	0.04	0	16.27
Mother's age at respondent's birth	25.80	0.07	12	54

Table 1 (continued)

	Mean	SE	Min	Max
Mother supportive	0.78		0	1
Mother strict	0.54		0	1
ASVAB score	50.22	0.36	0	100
School tardies	1.78	0.05	0	30
School absences	4.26	0.06	0	30
School suspension	0.22		0	1
Fought at school	0.14		0	1
School attachment	2.83	0.01	1	4
Property stolen at school	0.23		0	1
Threatened at school	0.20		0	1
Private school	0.08		0	1
Victimization index	0.41	0.01	0	3
Youth's health	4.11	0.01	1	5
Antisocial peer association	2.12	0.01	1	5
Prosocial peer association	3.07	0.01	1	5
Perceived risk of arrest	61.31	0.48	0	100
Gang member	0.03		0	1
Delinquency	0.88	0.02	0	6
Substance use	0.99	0.01	0	3

assumption of independent observations would be violated and standard error estimates would be biased, usually downward (Osgood 2010; Raudenbush and Bryk 2002). The random effects models corrected for this clustering by including a random variance component for each level of analysis.

Results

Propensity Score Matching

We begin by examining balance on the pretreatment covariates prior to matching. The four left-most columns of Table 2 display mean values on the covariates, standardized bias statistics, and *t* tests for arrested versus non-arrested juveniles. Prior to matching, arrested and non-arrested juveniles differed on 29 of the 43 pretreatment covariates. Compared with non-arrested juveniles, juveniles who would later be arrested were more likely to be male, to be younger, to live in a western state (and not in a northeast state), to live in a central city (and not in the suburbs), to live in a home without two parents, to have parents with fewer years of education, to come from a lower income household, and to be born to a mother who was younger. They reported lower levels of parental support, more school tardiness and absences, and more suspensions and school fighting. They scored lower on a test of cognitive ability and reported lower school attachment. They were more likely to be threatened and victimized at school, to attend a public school, and to have been exposed to violence in the past, and they were less likely to have prosocial peers. Finally, arrested youths self-reported higher levels of delinquency, substance use, and gang involvement.

Table 2 Differences between arrested and non-arrested juveniles on pretreatment covariates assessed at wave 1. *Source*: NLSY97

	Before matching (N=7916)				After matching (N=7908)			
	Arrest	No arrest	t test	SB	Arrest	No arrest	t test	SB
Male	0.63	0.49	7.04	30.20	0.63	0.65	− 1.05	− 4.41
Age	13.54	14.37	− 13.23	− 59.69	13.54	13.56	− 0.44	− 1.88
Black	0.17	0.15	1.05	4.36	0.17	0.16	0.63	2.64
American Indian	0.01	0.01	0.25	1.05	0.01	0.01	0.06	0.24
Asian or Pacific Islander	0.02	0.02	− 0.42	− 1.83	0.02	0.02	0.05	0.21
Other race	0.09	0.08	0.68	2.82	0.09	0.10	− 0.98	− 4.23
Hispanic	0.13	0.13	− 0.13	− 0.57	0.13	0.14	− 1.24	− 5.36
1981 birth cohort	0.09	0.20	− 6.37	− 30.18	0.09	0.10	− 0.33	− 1.40
1982 birth cohort	0.20	0.20	− 0.36	− 1.53	0.20	0.20	− 0.31	− 1.30
1983 birth cohort	0.29	0.19	5.74	22.79	0.29	0.30	− 0.32	− 1.37
1984 birth cohort	0.35	0.20	8.69	33.95	0.35	0.34	0.69	2.90
Northeast	0.14	0.19	− 2.96	− 13.13	0.14	0.15	− 0.60	− 2.55
Midwest	0.24	0.26	− 1.54	− 6.59	0.23	0.23	0.17	0.74
West	0.27	0.20	4.09	16.51	0.27	0.28	− 0.27	− 1.15
Central city	0.30	0.26	2.21	9.16	0.30	0.30	− 0.17	− 0.72
Suburbs	0.45	0.55	− 4.82	− 20.37	0.45	0.47	− 0.75	− 3.19
Concentrated disadvantage	− 0.14	− 0.13	− 0.13	− 0.57	− 0.14	− 0.15	0.41	1.68
Percent Black	0.12	0.12	0.34	1.39	0.12	0.12	1.02	4.13
Two parent household	0.40	0.56	− 7.79	− 33.15	0.40	0.42	− 1.21	− 5.14
Number of siblings	1.55	1.53	0.50	2.11	1.56	1.59	− 0.63	− 2.67
Parental education	13.11	13.67	− 4.55	− 19.72	13.11	13.05	0.58	2.47
Household income (logged)	9.95	10.23	− 3.03	− 12.41	9.95	9.96	− 0.17	− 0.70
Income-to-poverty ratio	2.79	3.34	− 4.51	− 19.63	2.80	2.78	0.16	0.65
Mother's age at R's birth	24.92	25.87	− 4.22	− 17.86	24.93	24.92	0.03	0.14
Mother supportive	0.70	0.78	− 4.89	− 19.70	0.70	0.69	0.27	1.15
Mother strict	0.50	0.54	− 1.78	− 7.49	0.50	0.52	− 0.65	− 2.73
ASVAB score	40.75	50.99	− 8.57	− 37.08	40.83	40.70	0.12	0.49
School tardies	2.47	1.72	4.13	16.01	2.48	2.39	0.37	1.61
School absences	4.89	4.21	3.08	12.69	4.90	4.65	1.07	4.56
School suspension	0.42	0.20	12.33	47.46	0.41	0.40	0.92	3.88
Fought at school	0.31	0.13	12.84	46.27	0.31	0.32	− 0.25	− 1.04
School attachment	2.79	2.83	− 2.17	− 8.93	2.80	2.79	0.22	0.92
Property stolen at school	0.33	0.22	5.93	23.73	0.33	0.32	0.36	1.50
Threatened at school	0.36	0.19	9.84	37.96	0.35	0.34	0.73	3.07
Private school	0.05	0.08	− 2.37	− 10.79	0.05	0.05	0.01	0.06
Victimization index	0.63	0.39	8.90	34.19	0.63	0.59	0.99	4.20
Youth's health	4.07	4.11	− 1.25	− 5.27	4.07	4.04	0.81	3.50
Antisocial peer association	2.08	2.12	− 1.10	− 4.64	2.07	2.07	0.09	0.40
Prosocial peer association	2.96	3.08	− 3.95	− 16.30	2.97	3.00	− 1.25	− 5.25
Perceived risk of arrest	58.36	61.55	− 1.90	− 8.03	58.28	58.30	− 0.01	− 0.04
Gang member	0.08	0.03	6.99	23.31	0.08	0.07	1.35	5.49
Delinquency	1.50	0.83	13.22	50.74	1.49	1.41	1.29	5.50

Table 2 (continued)

| | Before matching (N=7916) | | | | After matching (N=7908) | | | |
	Arrest	No arrest	t test	SB	Arrest	No arrest	t test	SB
Substance use	1.35	0.96	8.49	35.06	1.35	1.29	1.14	4.84
Mean absolute t/bias			4.61	18.77			0.56	2.37
$N_{respondents}$	606	7310			605	7303		

Thus, these findings indicate that arrested juveniles had a collection of liabilities that are associated with both involvement with the criminal justice system and lower socioeconomic attainment.

We next estimated respondents' propensities for treatment by modeling juvenile arrest as a logit function of the 43 pretreatment covariates. The resulting propensity score had a mean of 0.077 and ranged from 0.001 to 0.769. As expected, the mean propensity score for arrested youths (0.177) was higher than that for non-arrested youths (0.068). Next, we excluded 8 cases who did not have a match within the designated bandwidth (0.03) of propensity scores. We then used the kernel density matching algorithm to match 605 arrested juveniles to one or more control cases with propensity scores within 0.03, resulting in a matched sample of 7908 respondents.

We next determined whether our matched groups were balanced on the pretreatment covariates. As noted above, prior to matching, the arrested and non-arrested juveniles differed on 29 out of 43 covariates. After matching, the arrested juveniles and their matched controls were balanced on all variables. The two right-most columns of Table 2 show that all covariates had a SB statistic less than the absolute value of 5.5 (mean |SB|=2.37) and a t score less than the absolute value of 1.35 (mean |t score|=0.56). In total, the matching procedure eliminated 87% of the initial bias.

Effect of Arrest on Asset and Debt Accumulation

We next estimated the effect of juvenile arrest on asset and debt accumulation in young adulthood among the matched sample. This was done by estimating three random effects linear regression models predicting assets, debts, and net worth from juvenile arrest, dummy variables for time (interview), and interactions between juvenile arrest and time. Each control case was weighted by the distance to its matched treated case, and all cases were weighted by the NLSY97 survey weights.

Models 1 of Tables 3, 4, and 5 present the results of this analysis. The intercepts (a=9.10, 4.49, and 16,634 in the assets, debt, and net worth models respectively) represent the means on the outcomes at age 20 for respondents without juvenile arrest histories. The juvenile arrest coefficients—which represent the effects of juvenile arrest when respondents were age 20—indicate that arrested juveniles had significantly lower assets at age 20 (b=−0.23, p=0.025) than non-arrested juveniles; arrested and non-arrested juveniles did not differ statistically with respect to debt (b=−0.40, $p>0.05$) or net worth (b=−2426, $p>0.05$) at age 20. Given that the measures of assets and debt are log transformed, exponentiating the coefficients from these models yields the percentage change in the outcome for a one-unit change in the independent variable. As such, the results suggest that at age 20, arrested juveniles had 20% lower assets (100 * [exp[−0.23] − 1]=20.2%). Next, the interaction term coefficients indicate that the effect of juvenile arrest on all

Table 3 Random effects regressions predicting assets from juvenile arrest and mediators ($N = 22{,}081$ observations on 7908 respondents). *Source*: NLSY97

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	−0.23*	−0.42, −0.03	−0.28**	−0.48, −0.07	−0.02	−0.22, 0.17	−0.11	−0.31, 0.09	−0.03	−0.22, 0.17
Age 25	0.48***	0.36, 0.60	0.17**	0.04, 0.29	0.13	0.00, 0.26	0.47***	0.35, 0.59	−0.17*	−0.31, −0.03
Age 30	1.06***	0.92, 1.19	0.45***	0.29, 0.61	0.54***	0.40, 0.69	1.05***	0.92, 1.19	−0.07	−0.24, 0.10
Juvenile arrest* Age 25	−0.25	−0.54, 0.04	−0.21	−0.49, 0.07	−0.17	−0.45, 0.11	−0.29*	−0.58, −0.01	−0.17	−0.45, 0.10
Juvenile arrest* Age 30	−0.42*	−0.73, −0.10	−0.23	−0.53, 0.08	−0.29	−0.59, 0.02	−0.44**	−0.76, −0.12	−0.14	−0.44, 0.16
Married			1.51***	1.32, 1.69					1.20***	1.02, 1.37
Parenthood			0.30***	0.11, 0.49					0.61***	0.42, 0.79
Highest grade completed					0.22***	0.18, 0.25			0.22***	0.19, 0.25
Weeks worked (logged)					0.15***	0.08, 0.23			0.18***	0.11, 0.25
Personal income (logged)					0.13***	0.10, 0.16			0.11***	0.08, 0.14
Subsequent arrest							−0.28	−0.73, 0.17	−0.09	−0.48, 0.31
Subsequent conviction							−0.18	−0.76, 0.40	−0.18	−0.72, 0.37
Subsequent incarceration							−1.61***	−2.38, −0.85	−0.94***	−1.63, −0.25
Intercept	9.10***	9.01, 9.20	8.94***	8.84, 9.04	5.10***	4.60, 5.61	9.17***	9.07, 9.27	4.99***	4.47, 5.51

Results are adjusted for kernel density weight and NLSY97 population weights

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 4 Random effects regressions predicting debt from juvenile arrest and mediators ($N = 22{,}081$ observations on 7908 respondents). *Source*: NLSY97

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	−0.40	−0.82, 0.02	−0.47*	−0.89, −0.05	−0.04	−0.46, 0.38	−0.34	−0.76, 0.08	−0.17	−0.58, 0.25
Age 25	2.26***	2.03, 2.50	1.76***	1.52, 2.00	1.60***	1.35, 1.84	2.25***	2.02, 2.49	1.07***	0.83, 1.32
Age 30	2.72***	2.47, 2.97	1.75***	1.45, 2.04	1.83***	1.56, 2.10	2.70***	2.45, 2.95	0.77***	0.46, 1.07
Juvenile arrest* Age 25	−0.42	−0.94, 0.10	−0.34	−0.86, 0.17	−0.26	−0.77, 0.26	−0.44	−0.96, 0.08	−0.19	−0.70, 0.32
Juvenile arrest* Age 30	−0.94**	−1.51, −0.37	−0.61*	−1.16, −0.05	−0.68*	−1.24, −0.11	−0.96**	−1.53, −0.38	−0.38	−0.93, 0.17
Married			2.73***	2.34, 3.12					2.19***	1.81, 2.56
Parenthood			0.22	−0.14, 0.57					0.88***	0.55, 1.22
Highest grade completed					0.47***	0.40, 0.54			0.48***	0.41, 0.55
Weeks worked (logged)					0.07	−0.04, 0.18			0.12*	0.01, 0.23
Personal income (logged)					0.22***	0.18, 0.27			0.20***	0.16, 0.24
Subsequent arrest							−0.57	−1.25, 0.10	−0.17	−0.83, 0.49
Subsequent conviction							0.47	−0.55, 1.48	0.54	−0.48, 1.55
Subsequent incarceration							−1.09	−2.26, 0.09	−0.02	−1.21, 1.16
Intercept	4.49***	4.29, 4.68	4.24***	4.04, 4.43	−2.90***	−3.75, −2.04	4.53***	4.34, 4.73	−3.35***	−4.17, −2.52

Results are adjusted for kernel density weight and NLSY97 population weights

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 5 Random effects regressions predicting net worth from juvenile arrest and mediators (N = 22,081 observations on 7908 respondents). *Source*: NLSY97

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	− 2426	− 7000, 2148	− 2867	− 7625, 1892	101	− 4596, 4799	− 1215	− 5634, 3204	− 324	− 5028, 4380
Age 25	10,622***	7067, 14,177	4830*	1137, 8524	5635**	1651, 9618	10,397***	6823, 13,972	415	− 3782, 4613
Age 30	41,366***	35,790, 46,942	30,256****	24,267, 36,245	35,086****	29,179, 40,993	40,996****	35,403, 46,589	24,013****	17,554, 30,473
Juvenile arrest* Age 25	1017	− 7941, 9975	2155	− 6861, 11,172	2203	− 6839, 11,245	559	− 8261, 9378	2849	− 6116, 11,813
Juvenile arrest* Age 30	− 14,757*	− 28,525, − 988	− 10,200	− 24,146, 3746	− 12,843	− 26,843, 1158	− 15,088*	− 28,714, − 1463	− 9033	− 23,081, 5016
Married			39,646***	30,390, 48,901					35,906***	26,869, 44,943
Parenthood			− 4910	− 12,084, 2263					− 1109	− 7790, 5571
Highest grade completed					3128***	1391, 4865			2717**	1035, 4400
Weeks worked (logged)					250	− 1903, 2403			1071	− 1033, 3174
Personal income (logged)					1936***	997, 2874			1501**	571, 2430
Subsequent arrest							− 13,974	− 35,356, 7409	− 10,681	− 31,418, 10,056
Subsequent conviction							10,149	− 11,748, 32,046	10,928	− 10,606, 32,461
Subsequent incarceration							− 15,499	− 44,536, 13,537	− 6903	− 36,703, 22,898
Intercept	16,634***	15,224, 18,044	14,238***	12,479, 15,998	− 35,268**	− 55,366, − 15,169	17,614***	16,030, 19,198	− 32,056**	− 51,395, − 12,717

Results are adjusted for kernel density weight and NLSY97 population weights

*p < 0.05; **p < 0.01; ***p < 0.001

Fig. 1 Predicted Assets, Debt, and Net Worth at Ages 20, 25, and 30 among Respondents who Were and Were Not Arrested as Juveniles
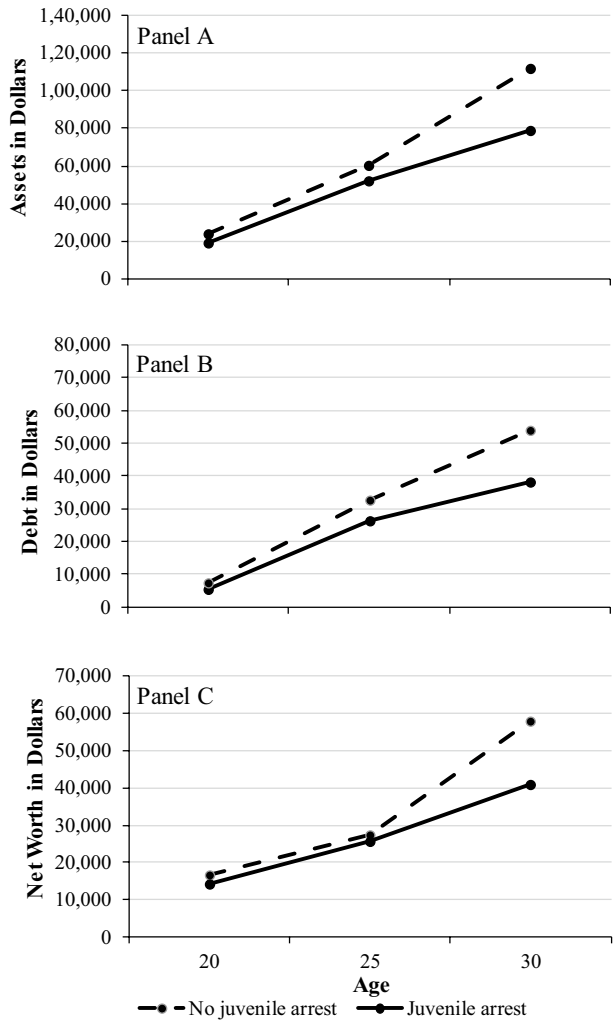
three outcomes became more negative over time. By age 25, statistically significant differences favoring non-arrested juveniles emerged for the assets (for joint test of arrest and arrest * age 25 coefficients, $F=6.24$, $p=0.002$) and debt ($F=6.18$, $p=0.002$) outcomes, but not the net worth outcome ($F=0.57$, $p>0.05$). This corresponds to 38% lower assets and 56% lower debt among arrested juveniles at age 25 (e.g., for the asset model, $1 - \exp[-0.23 + (-0.25)] = 0.38$). By age 30, the effect of juvenile arrest grew to -0.65, -1.34, and $-\$17,183$ for assets, debt, and net worth respectively (e.g., for the asset model, $-0.23 + [-0.42] = -0.65$). In other words, arrested juveniles had 47% lower assets, 74% lower debt, and $17,183 lower net worth at age 30 than non-arrested juveniles.

To further illustrate these findings, Fig. 1 shows predicted assets, debts, and net worth at ages 20, 25, and 30 for arrested and non-arrested juveniles. To facilitate interpretation, the figure is based on similar models that used untransformed versions of the assets and debt outcomes. Panels A, B, and C indicate that the differences between arrested and non-arrested juveniles' assets, debts, and net worth, while minor at age 20, are apparent by age

30. Overall, these models indicate that arrested juveniles will go on to have lower assets, debts, and net worth in young adulthood, with the differences growing larger over time.

Mediating Models

We next examined whether the effect of juvenile arrest on financial assets and debt was mediated by family formation, school and work, and criminal justice factors. As a first step, we assessed whether juvenile arrest predicted each of the potential mediators among the matched sample. "Appendix B" shows the results. Net of the covariates, respondents with juvenile arrest histories appeared to enter parenthood early but to have lower rates of marriage by late young adulthood. They completed less education, worked less, and had lower incomes. Finally, they were more likely to have been arrested, convicted, and incarcerated as adults. These significant associations mean that these indicators might explain why juvenile arrests predict lower assets, debt, and net worth by late young adulthood.

Models 2 through 5 of Tables 3, 4 and 5 present a series of random-effects linear regression models where assets (Table 3), debt (Table 4), and net worth (Table 5) were regressed on juvenile arrest, time dummies, arrest by time interactions, and mediators among the matched sample. As before, cases were weighted by the distance to the matched treated case (for control cases) and by the NLSY97 survey weights. If there were a mediating effect, then the introduction of the mediating factors should attenuate the size of the juvenile arrest and age interaction coefficients compared with those coefficients in models 1.

Models 2 introduce the family formation mediators: marital status and parenthood. Respondents who were married had higher assets, debt, and net worth. Respondents who had a child had higher assets. The introduction of these mediators into the models increased the effects of juvenile arrest on assets and debt at age 20 by 22% and 17% respectively, had little impact on the age 25 associations, and attenuated the effects of juvenile arrest on assets, debt, and net worth at age 30 by 22%, 20%, and 24% respectively (e.g., for age 30 assets $((-0.23 + [-0.42]) - (-0.28 + [-0.23])/(-0.23 + [-0.42]) = 0.22$).

Model 3 introduces the school and work mediators: highest grade completed, weeks worked, and personal income. Higher scores on education and income were associated with significantly higher assets, debt, and net worth, and more weeks worked were associated with higher assets. The introduction of the school and work mediators into the models attenuated the effects of juvenile arrest on assets and debt at age 20 by 89%; attenuated its effects on assets and debt at age 25 by 59% and 64% respectively; and attenuated the effects of juvenile arrest on assets, debt, and net worth at age 30 by 52%, 46%, and 26% respectively.

Model 4 introduces the criminal justice mediators: subsequent arrest, conviction, and incarceration. Subsequent incarceration was associated with significantly lower financial assets; there were no other statistically significant associations between criminal justice contacts and the outcomes. The introduction of these criminal justice mediators into the models attenuated the effects of juvenile arrest on assets at age 20 by 51%; attenuated its effects on assets and debt at age 25 by 15% and 5% respectively; and attenuated its effects on assets, debt, and net worth at age 30 by 14%, 4%, and 5% respectively.

Finally, model 5 introduces all of the mediators together. The full set of mediators attenuated the effects of juvenile arrest on assets at age 20 and age 25 by 89% and 58% respectively; and attenuated its effects on age 30 assets, debts, and net worth by 74%, 59%, and 46% respectively.

Supplemental Analyses

In additional analyses we repeated the model shown in the first column of Table 4, but substituting measures of specific types of debt as the outcomes. ("Appendix C") shows the results. To summarize, juvenile arrest predicted lower motor vehicle loan debt, government student loan debt, and mortgage debt, but it did not predict debt from student loans from family, personal loans from family and friends, or installment loan (consumer debt) balances. This suggests that juvenile arrestees went on to hold less "good" debt than non-arrestees, and that they did not have any more or less "bad" debt than non-arrestees.

We also conducted several robustness checks. These included examinations of whether the results differed under listwise deletion (versus multiple imputation), whether they differed when lagged (by one wave) measures of the mediators were used, and whether they differed when the 723 excluded respondents whose arrests preceded the matching variables were included in the analysis. The substantive findings were the same under the first two alternate specifications. However, when respondents who were arrested before wave 1 were included in the analyses, the results changed in two ways: First, the association between juvenile arrest and debt was present at all ages, and second, juvenile arrest no longer predicted net worth. Thus, the main findings were generally robust to different sample selection strategies, treatments of missing data, and timings of measurement of the mediators, though the association between juvenile arrest and later assets was the most robust.

Finally, to assess the extent to which our analytical strategy addressed confounding, we compared models 1 of Tables 3, 4, and 5 with the results of unadjusted models that were estimated without propensity score matching (full results available upon request). Although the substantive findings from these unadjusted models were similar to the main findings, the magnitude of the effects was much larger. For example, the associations between juvenile arrest and assets ($b=-0.45$, $p<0.001$), debt ($b=-0.69$, $p=0.001$), and net worth ($b=-7039$, $p=0.002$) at age 20 were approximately twice as large in the unadjusted models. We found similar differences between the adjusted and unadjusted associations at age 25, and at age 30 the unadjusted associations were approximately two-thirds higher than the adjusted associations. These results underscore the importance of accounting for sources of spuriousness in studies of this topic.

Discussion

This study examined the association of juvenile arrest with the understudied outcomes of financial assets and debts across young adulthood. The findings are important for two main reasons. First, it is important to know the extent to which juvenile "indiscretions" have lasting effects on individuals' life chances. And second, although many studies have examined arrests and outcomes such as work status, assets and debts have distinct precursors and unique impacts on several domains of adult and intergenerational well-being. This study also incorporated several methodological strengths, including a longer outcomes window than the only other studies on the topic, a strong causal analysis, and tests of three categories of mediators that potentially could explain any effect of juvenile arrests. Our data and methods gave us the rare opportunity to observe whether, when, how much, and why asset and debt trajectories diverge for individuals who were arrested as juveniles.

The results from this study yielded two main conclusions. First, our findings indicate that arrested juveniles go on to have lower assets, debts, and net worth during young

adulthood compared to non-arrested juveniles. Although only one of these differences had emerged by age 20, all three were visible by late young adulthood. By the time respondents reached age 30, arrested juveniles had 47% lower assets, 74% lower debt, and $17,183 lower net worth than non-arrested juveniles. Second, our findings indicate that much of the difference between arrested and non-arrested juveniles' asset and debt accumulation was explained by school and work mediators. Having lower education, hours worked, and income explained between 26 and 89% of the difference, suggesting that juvenile arrest leads to lower asset and debt accumulation because arrested youth are less likely to hold degrees and jobs that contribute to overall economic well-being. We also found some evidence that differences in incarceration at age 20 and in marriage at age 30 helped to explain the juvenile arrest effect, but these were less powerful explanations than were the work and school indicators.

Our findings are consistent with three major elements of the labeling perspective, which predicts long-ranging outcomes of juvenile justice system contacts. First, they show that juvenile arrests can have long-term harmful collateral consequences for life domains not directly related to offending—here, socioeconomic attainment. Others have shown this with respect to school and work attainment (e.g., Bernburg and Krohn 2003; Hjalmarsson 2008; Tanner et al. 1999; Widdowson et al. 2016); this study shows it with respect to wealth. Second, our findings confirm the first step in a multistep process of cumulative continuity, which specifies the ways in which early deviant behavior may come to perpetuate itself. Here, early arrests lead to socioeconomic disadvantage in young adulthood. Our theoretical framework would predict that that disadvantage will in turn predict continued involvement with the justice system. Future research should test the full pathway from initial arrests to collateral consequences to additional arrests.

Third, our results suggest that some of the harmful consequences of juvenile arrests may not fully emerge until late young adulthood. Although age-graded effects are a main theme in the life course literature, the delayed onset of effects is perhaps less studied. Yet such effects are especially relevant for our study because age positively predicts wealth (Maroto 2015) and because financial independence typically develops later in young adulthood (Furstenberg 2010). More generally, our age-graded findings suggest that studies of the collateral consequences of justice system contacts should use extended follow-up periods and should take into account the underlying age trends of those consequences. For example, the life problems that often are examined in relation to arrests, such as problems in the labor market and in family formation, may not yet have emerged among the youngest young adults (Siennick and Widdowson 2017). Our findings indicate that not only may juvenile arrests have lasting consequences, but also it might take some time before those consequences become visible.

One contribution of this study is its identification of the mechanisms by which juvenile arrests may eventually harm assets and debt attainment. The findings are consistent with the ideas of "snowballing" consequences of these arrests, as their effects appear to operate largely through their effects on the intermediate outcomes of school, work, and income attainment. Labeling theory would anticipate that blocked conventional opportunities would account for lasting negative effects of justice system involvement. However, we could not explicitly test other mechanisms anticipated by labeling theory, such as changes in identities. In addition, our analysis of the family formation mediators may partly tap changes in peer groups following justice system contacts, but indicators such as marriage may not fully index these changes. Future research should examine additional potential mechanisms of the key effects identified here.

The special protections of the juvenile justice system are aimed in part at helping young offenders achieve healthy transitions to adulthood (Chung et al. 2005). Our findings identify several life domains—specifically, family formation, education, employment, income, and wealth attainment—where that goal appears unmet. Some of the policy implications of this paper follow from the fact that institutional policies and practices may contribute to juvenile arrestees' attainment deficits. For example, in the realm of education, the Higher Education Act of 1998 denies financial aid to some convicted drug offenders (Lovenheim and Owens 2013; U.S. Government Accountability Office 2005). In addition, many colleges use criminal records in admission decisions (Pierce et al. 2013). In the realm of employment, employers often have access to juvenile records, and many have reservations about hiring previously arrested youth (Pham et al. 2015). In addition, the continuity that we found between juvenile and adult justice system contacts means that juvenile arrestees may be at risk for collateral consequences from their adult records even if their juvenile records are sealed or otherwise protected. These possibilities highlight ways in which policy and practice might contribute to the cascading effects of early arrests.

Our finding that arrested juveniles go to have lower debt in young adulthood may be interpreted as a positive outcome given that too much debt is associated with negative outcomes such as economic insecurity and stress (Dwyer 2018). Nevertheless, we urge caution in interpreting this finding in a positive light. Most of the debt categories collected by the NLSY are considered "healthy" forms of debt that assist young people in achieving upward mobility (e.g., student loan debt and mortgage debt). Yet, it could still be the case that arrested juveniles are more likely to have other less advantageous forms of debt that the NLSY97 does not measure well (e.g., debt from payday loans). It could also be the case that arrested juveniles hold debt that is subject to less favorable terms, which would not be captured by our measures. For example, arrested juveniles might hold credit card debt at a higher interest rate, and the impact on economic well-being might not be visible until after years of compounding.

Although we examined several different types of debt, this study did not address the role of criminal justice-related debt in labeling processes. Offenders who are processed by the justice system may emerge holding various forms of this debt, ranging from fines to court costs to various fees related to sanctioning (Ruback and Bergstrom 2006). Although the dollar amounts of these debts are often relatively modest, the people holding them often experience more than their share of employment problems, which may hamper their ability to repay them (Link 2019). Given that criminal justice debts specifically have been linked with persistence in offending (Aaltonen et al. 2016), studies should include them in investigations of the collateral consequences of justice system contacts and of cumulative continuity more broadly.

Our study was limited in that it examined asset and debt accumulation only up to age 30. Although the NLSY97 data eventually will include farther-reaching information on these outcomes, at the time this study was conducted the age 35 Asset interviews had not yet been fielded for most respondents. When they are available, those additional data will provide a more complete picture of the long-term economic impacts of juvenile arrests. Our finding that arrested and non-arrested juveniles' asset and debt trajectories increasingly diverge over time suggests that arrested juveniles may continue to experience economic disadvantage compared to their non-arrested counterparts well into their 30 s and that this disadvantage may continue to grow over time. Even so, future research should consider following respondents further into adulthood.

Our study also had additional limitations. For example, although we constructed our measure of juvenile arrest to account for states' varying top ages of juvenile court

jurisdiction, some of the youth in our study may have been processed in the adult system through mechanisms such as waivers. This probably affected only a small number of juvenile arrestees (Griffin et al. 2011), but to the extent that our focal predictor erroneously blended juvenile and adult justice system contacts we could have over- or understated the impact of juvenile arrests. In addition, we did not examine the crimes for which respondents were arrested. Perhaps different crimes have different implications for labeling and different associations with later economic attainment. For example, because drug offenses are linked with student loan restrictions and education is linked with wealth attainment, drug crimes might have stronger associations with later assets and debts. Our estimates capture an average effect across crime types.

In conclusion, our study is one of the first to quantify and to attempt to explain the impact of juvenile arrests on multiple important forms of economic attainment. We found that this impact was modest in size at age 20 but grew considerably by age 30. These effects on assets, debt, and net worth were found net of 43 matching covariates, which is twice the number included in many other studies of justice system involvement and school, work, and economic outcomes (e.g., Brayne 2014; Sharlein 2018). This provides strong evidence that they are not merely artifacts of selection. Our additional finding that education, employment, and income explain much of these effects highlights several potential areas of intervention for protecting young arrestees' later economic wellbeing.

Compliance with Ethical Standards

Conflict of interest The authors declare that they have no conflict of interest.

Ethical Approval All procedures performed in studies involving human participants were in accordance with the ethical standards of the authors' institutional research committees and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

Appendices

Appendix A

Description of Pretreatment Covariates Used in Matching Algorithm. *Source*: NLSY97

Variable	Definition
Demographic characteristics	
Male	Respondent's gender is male (0 = no, 1 = yes)
Age	Respondent's age (in years) at wave 1
Race	Set of dummy variables with indicators for White (0 = no, 1 = yes), Black (0 = no, 1 = yes), American Indian (0 = no, 1 = yes), Asian or Pacific Islander (0 = no, 1 = yes), and Other race/Something else (0 = no, 1 = yes) White is the reference category
Ethnicity	Respondent is Hispanic (0 = no, 1 = yes)
Birth cohort	Set of dummy variables with indicators for 1980 cohort (0 = no, 1 = yes), 1981 cohort (0 = no, 1 = yes), and 1982 cohort (0 = no, 1 = yes), 1983 cohort (0 = no, 1 = yes), and 1984 cohort (0 = no, 1 = yes). 1980 cohort is the reference category

Variable	Definition
Community characteristics	
Census region	Set of dummy variables with indicators for South (0=no, 1=yes), Northeast (0=no, 1=yes), Midwest (0=no, 1=yes), and West (0=no, 1=yes). South is the reference category
Residential location	Set of dummy variables with indicators for rural (0=no, 1=yes), central city (0=no, 1=yes), and suburban (0=no, 1=yes). Rural is the reference category
Concentrated disadvantage	Mean of the county's proportion of families living below the poverty line proportion of female-headed households, the median family income (reverse coded and logged), unemployment rate, proportion of the population without a high school diploma, and the proportion of households receiving public assistance, from the 1990 Census (α=.89)
Percent Black	Percentage of the county's population that was non-Hispanic Black, from the 1990 Census
Household characteristics	
Two parent household	Respondent lives with two parents (0=no, 1=yes)
Number of siblings	Number of siblings in respondent's home
Parental education	Highest level of education attained by a parent
Household income	Total household income in logged dollars
Income-to-poverty ratio	The ratio of gross household income variable to the previous year's federal poverty level (for households of that size)
Mother's age at R's birth	Mother's age at respondent's birth
Family characteristics	
Mother supportive	Respondent's mother figure is supportive (0=no, 1=yes)
Mother strict	Respondent's mother figure is strict (0=no, 1=yes)
Educational characteristics	
ASVAB score	Cognitive abilities were assessed by the Armed Service Vocational Aptitude Battery (ASVAB). Scores reflect percentiles
School tardies	Number of days late to school without an excuse in the past semester
School absences	Number of days absent from school in the past semester
School suspension	Suspended from school in the past year (0=no, 1=yes)
Fought at school	Been in a physical fight at school in the past year (0=no, 1=yes)
School attachment	Mean index based on 7 items assessing whether: (1) teachers are good, (2) teachers are interested in students, (3) there are disruptions by other students, (4) students are graded fairly, (5) there is a lot of cheating on tests, (6) discipline is fair, and (7) feels safe at school (α=.68)
Property stolen at school	Belongings stolen at current school (0=no, 1=yes)
Threatened at school	Threatened at current school (0=no, 1=yes)
Private school	Attended a private school at wave 1 (0=no, 1=yes)
Youth background	
Victimization index	Variety score indicating the number of types of victimization respondents experienced before age 12: (1) home burglarized, (2) bullied, and (3) witnessed violence
Youth's health	Respondent's general state of health (1=poor, 5=excellent)
Peer influences	
Antisocial peer association	Mean index based on 5 items assessing the percentage of respondents' peers who (1) smoke, (2) get drunk, (3) belong to a gang, (4) use illegal drugs, and (5) skip class (α=.84)

Variable	Definition
Prosocial peer association	Mean index based on 4 items assessing the percentage of respondents' peers who (1) go to church, (2) participate in sports, (3) plan to go to college, and (4) volunteer ($\alpha = .59$)
Antisocial characteristics	
Perceived risk of arrest	Percent chance respondent believes he/she would be arrested if stole a car
Gang member	Respondent reports belonging to a gang (0 = no, 1 = yes)
Delinquency	Variety score indicating the number of different delinquent acts ever committed: (1) vandalism, (2) theft under \$50, (3) theft over \$50, (4) other property crime, (5) sold or helped sell drugs, and (6) assault ($\alpha = .70$)
Substance use	Variety score indicating the number of different substances ever used: (1) cigarettes, (2) alcohol, and (3) marijuana ($\alpha = .74$)

Appendix B

Random Effects Regressions Predicting Mediators From Juvenile Arrest ($N = 22{,}081$ observations on 7908 respondents). *Source*: NLSY97

	Married[a]		Children[a]		Highest grade completed[b]		Weeks worked[b]		Personal income[b]	
	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	0.24	−0.10, 0.59	0.45***	0.21, 0.70	−0.56***	−0.73, −0.38	−0.20**	−0.35, −0.06	−0.38	−0.76, −0.00
Age 25	1.34***	1.18, 1.51	1.05***	0.93, 1.17	0.85***	0.79, 0.92	0.06	−0.02, 0.15	1.19***	1.00, 1.38
Age 30	2.07***	1.89, 2.24	1.79***	1.66, 1.93	1.29***	1.20, 1.37	0.60***	0.52, 0.68	1.09***	0.87, 1.30
Juvenile arrest* Age 25	−0.28	−0.62, 0.05	−0.11	−0.35, 0.13	−0.24**	−0.39, −0.10	0.02	−0.16, 0.20	−0.22	−0.68, 0.24
Juvenile arrest* Age 30	−0.68**	−1.07, −0.29	−0.33*	−0.61, −0.05	−0.40***	−0.60, −0.20	0.00	−0.20, 0.20	−0.34	−0.86, 0.17
Intercept	−2.45***	−2.62, −2.28	−1.71***	−1.83, −1.58	11.85***	11.77, 11.92	3.20***	3.13, 3.26	7.26***	7.10, 7.42

	Adult arrest[a]		Adult conviction[a]		Adult incarceration[a]	
	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	0.93***	0.64, 1.22	1.15***	0.79, 1.52	1.19***	0.63, 1.75
Age 25	−0.28*	−0.54, −0.01	−0.07	−0.42, 0.27	0.00	−0.54, 0.53
Age 30	−0.58***	−0.87, −0.29	−0.10	−0.43, 0.23	0.19	−0.34, 0.72
Juvenile arrest* Age 25	−0.28	−0.71, 0.14	−0.44	−0.97, 0.08	−0.26	−0.98, 0.47
Juvenile arrest* Age 30	−0.01	−0.46, 0.45	−0.23	−0.74, 0.28	−0.23	−0.97, 0.52
Intercept	−2.42***	−2.60, −2.24	−3.01***	−3.26, −2.76	−3.87***	−4.30, −3.43

Results are adjusted for kernel density weight and NLSY97 population weights

[†]$p < 0.10$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

[a]Logistic coefficients shown

[b]Linear coefficients shown

Appendix C

Random Effects Linear Regressions Predicting Each Measure of Debt from Juvenile Arrest ($N = 22{,}081$ observations on 7908 respondents). *Source:* NLSY97

	Motor vehicle debt		Consumer debt		Government student loan debt		Family student loan debt		Personal loan debt		Mortgage debt	
	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI	b	95% CI
Juvenile arrest	− 0.58**	− 0.93, − 0.23	0.25	− 0.10, 0.59	− 0.33**	− 0.56, − 0.11	− 0.02	− 0.11, 0.07	0.07	− 0.09, 0.24	− 0.14	− 0.30, 0.02
Age 25	0.92***	0.69, 1.15	2.03***	1.82, 2.24	0.87***	0.75, 0.99	0.02	− 0.02, 0.06	− 0.10	− 0.20, 0.00	1.24***	1.07, 1.42
Age 30	1.10***	0.87, 1.32	1.38***	1.16, 1.60	1.28***	1.11, 1.45	0.08*	0.02, 0.14	− 0.18***	− 0.26, − 0.09	2.67***	2.44, 2.90
Juvenile arrest* Age 25	− 0.10	− 0.58, 0.37	− 0.32	− 0.84, 0.19	− 0.51***	− 0.79, − 0.23	− 0.08	− 0.18, 0.02	− 0.07	− 0.28, 0.15	− 0.16	− 0.55, 0.22
Juvenile arrest* Age 30	− 0.52*	− 1.01, − 0.04	− 0.26	− 0.76, 0.25	− 0.33	− 0.72, 0.06	0.00	− 0.16, 0.16	− 0.03	− 0.25, 0.20	− 1.01***	− 1.50, − 0.52
Intercept	2.27***	2.10, 2.44	1.94***	1.80, 2.08	0.95***	0.84, 1.05	0.13***	0.10, 0.16	0.38***	0.31, 0.45	0.34***	0.25, 0.43

Results are adjusted for kernel density weight and NLSY97 population weights

$*p < 0.05$; $**p < 0.01$; $***p < 0.001$

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