

# Shattered Dreams: Paternal Incarceration, Youth Expectations, and the Intergenerational Transmission of Disadvantage

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**Abstract:** Children's expectations and aspirations have a substantial effect on a variety of life course outcomes, including their health, education, and earnings. However, little research to date has considered empirically how expectations and aspirations are shaped by adverse events—such as experiencing a parent be incarcerated. In this article, I leverage Add Health's retrospective parental incarceration questions to employ an innovative analytic strategy that accounts for selection bias and unobserved heterogeneity above and beyond typical observational methods. Results indicate that paternal incarceration is associated with one-fourth to one-third of a standard deviation lower youth expectations and aspirations, and these results are robust to various methods and specifications. Given that paternal incarceration is both common and disproportionately experienced by disadvantaged youth, the large magnitude and robust nature of these results reveal an important pathway through which mass incarceration has contributed to the intergenerational transmission of inequality in the U.S. in recent decades.

**Keywords:** inequality; paternal incarceration; mass incarceration; youth expectations

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ADOLESCENTS' expectations, aspirations, and sense of agency play a central role in determining their educational attainment, labor force participation, and health (Bozick et al. 2010; Halleröd 2011; Hitlin and Johnson 2015; Johnson and Hitlin 2017; Vaisey 2010). Often referred to as future orientation, these perceptions about the future may be especially meaningful for youth as they transition into adulthood and navigate postsecondary opportunities, begin to participate in the labor market, and start families of their own (Hitlin and Johnson 2015; Johnson, Blum, and Cheng 2014; Proulx and Chandler 2009). Far from being illusory, expectations and aspirations permeate the fabric of youths' lives and influence a range of outcomes such as mortality, health and wellbeing, earnings, criminal activity, and educational attainment (Borowsky, Ireland, and Resnick 2009; Brezina, Tekin, and Topalli 2009; Halleröd 2011; Hitlin and Johnson 2015; Johnson and Hitlin 2017; Vaisey 2010). These future-oriented perceptions may also both reflect and exacerbate structural inequality: youth from disadvantaged populations have less stable (Bozick et al. 2010), less aligned (Ahearn 2021), more fatalistic (Borowsky et al. 2009), and generally lower (Johnson and Hitlin 2017) expectations for their futures compared to their more advantaged peers.

The fact that youth expectations and aspirations matter is well-established. But where do they come from? In the 1960s and 1970s, William Sewell and colleagues proposed an empirical framework, now commonly referred to as the "Wisconsin model of status attainment", in which they argued that socialization processes in

the home and at school lead to divergent expectations and aspirations between high- and low- SES youth. In turn, these disparate beliefs about the future help explain inequities in educational attainment and job status in adulthood (see especially: Haller and Portes 1973; Sewell, Haller, and Portes 1969; Sewell and Hauser 1975). Other foundational qualitative work by MacLeod (1987), Lareau (1987, 2003), and Willis (1977) suggests that children's social-structural position, family background, social context, and foundational childhood experiences forge their habits, ideals, and preferences while simultaneously influencing their interactions with social institutions and orientation to the future (also see Bourdieu 1973, 1977, 1984).

However, this literature primarily focuses on just two social institutions—the family and the school—and does not account for children's exposure to crime and the criminal justice system. This is consequential for a number of reasons. First, the criminal justice system is a now-pervasive social institution in the era of mass incarceration. Moreover, parental incarceration has become increasingly commonplace, especially for youth from historically marginalized racial/ethnic groups: 1 in 4 black children, compared to only 1 in 25 white children, have had a parent imprisoned (Wildeman 2009; also see Enns et al. 2019 and Turney 2014). Second, a burgeoning area of research demonstrates that children's exposure to crime, violence, and the carceral state is indeed a salient form of adversity for youth that exerts a negative influence on cognitive functioning (Sharkey et al. 2012; Sharkey 2010), psychological wellbeing (Ang 2020), academic performance (Burdick-Will 2018; Legewie and Fagan 2019; Schwartz et al. 2021), and outlook towards future milestones (Testa et al. 2021).

Yet, despite its widespread prevalence in recent decades, sociologists have not considered parental incarceration in the context of youth future orientation—a potential driver of persistent inequality that is especially urgent in light of the historically unprecedented rates of incarceration in the U.S. In this article, I use a unique methodological innovation and the most broadly representative dataset available—the National Longitudinal Study of Adolescent to Adult Health (Add Health)—to fill this gap in the literature. Across multiple analytic methods and under a variety of specifications, I find a substantial and consistent effect of paternal incarceration on children's expectations, aspirations, and hopes for the future. The large magnitude and plausibly causal nature of these results offers further insight into how mass incarceration has played a significant role in the intergenerational transmission of inequality over the previous few decades: by deflating the hopes and dreams of disadvantaged youth.

## Background

### *Future Orientation and Stratification*

Decades of sociological research illuminates how children's forward-looking perceptions (such as aspirations, expectations, and optimism) exert considerable influence on their life course trajectory. The empirical and theoretical import of these future-oriented beliefs (often referred to as future orientation) rests primarily on a body

of evidence that demonstrates their association with a myriad of health, labor, and educational outcomes.

Seminal stratification literature considers how future orientation is shaped and situated in a social context. This work posits that normative life events, structural constraints, and socialization processes in the home and at school forge children's habits, ideals, and preferences (Bourdieu 1973, 1977; Lareau 2003; Willis 1977). Subsequently, these factors impact how youth interact with social institutions and ultimately mold the way they think about their lives and their futures (MacLeod 1987). This scholarship also highlights that these social processes and structural conditions—and, subsequently, expectations and aspirations—diverge along class lines, which then plays a significant role in explaining long-run disparities in educational attainment, job status, and related outcomes (Sewell et al. 1969).

More recent work pushes these themes forward in an effort to understand how expectations and stratification are interwoven. Vaisey (2010) finds that teenagers' educational expectations and aspirations strongly predict odds of future attainment, but that expectations matter more for nonpoor youth, whereas aspirations matter more for poor youth. Moreover, aspirations were found to be of little consequence for nonpoor youth. Vaisey (2010, 94) concludes that "poor youth—unlike their more advantaged peers—cannot simply "go with the flow" and end up in college, [and so] their educational ideals might matter more for predicting their choices because they need to be motivated enough to overcome the surrounding social inertia". DeLuca et al. (2021) chronicle how economically disadvantaged black youth who have experienced "adverse shocks" can end up getting thrown off course educationally because they anticipate the occurrence of future negative incidents; these students often select into less opportune educational programs because hardships in the past lead them to recalibrate how they think about their future. Taken together, this body of work illuminates the need to meticulously investigate how future orientation develops across the early life course.

### *Youth and the Criminal Justice System*

Foundational stratification literature motivates how beliefs about the future can alter one's life course trajectory and contribute to enduring stratification. However, this scholarship—which largely predates the peak of mass incarceration—places the family, home, and school at the center of youth expectations and persistent stratification while paying little (if any) attention to the role of the modern carceral state. This is especially significant in the contemporary era given the unparalleled scope of the U.S. criminal justice system, as well as the fact that a burgeoning body of research suggests that various forms of contact with the carceral state are consequential for how children perceive their lives and futures.

A collection of recent work has established that exposure to the carceral state is a salient form of adversity that has the capacity to alter how children think about their lives and future. Testa et al. (2021) use fixed effects modeling to investigate how interactions with police influence adolescents' outlook towards future milestones. They find that these outlooks can be altered by both direct and indirect exposure to law enforcement—indicating that a more thorough investigation into the collateral

consequences of vicarious contact with the carceral state is warranted. In this vein, a substantial collection of research suggests that indirect exposure to the criminal justice system—in particular, via a parent being incarcerated—is negatively associated with a host of youth outcomes, including delinquency and/or criminal activity (Roettger and Swisher 2011; Porter and King 2015; Wildeman and Andersen 2017), educational achievement and attainment (Hagan and Foster, 2012a; 2012b), school readiness (Haskins 2014), behavioral problems (Haskins 2015; Geller et al. 2012; Wakefield and Wildeman 2011; Wildeman and Turney 2014), and physical and mental health issues (Lee, Fang, and Luo 2013; Turney 2014).

Previous literature delineates a variety of mechanisms through which having an incarcerated parent could influence children's perceptions about their future. Parental incarceration may be especially relevant to expectations and aspirations if children absorb some type of secondary stigma associated with the "mark" of a criminal record (Murray and Farrington 2008; Pager 2003). Children's expectations and aspirations may also be hindered by the multidimensional and interwoven thread of strains resulting from the incarceration of a parent (Agnew 1992; Turney 2022). The economic and material hardship that ensues in the aftermath of parental incarceration—whether in the form of financial destitution, housing instability, food insecurity, or other hardship (Geller, Garfinkel, and Western 2011; Schwartz-Soicher et al. 2011; Swisher and Waller 2008; Turney 2015; Wildeman 2014)—may additionally induce children to adjust how they think about their lives and futures. Finally, the stress and trauma experienced by children as a result of their exposure to violence and/or the carceral state may also unduly affect children's perceptions. Research shows that witnessing a parent's arrest and/or sentencing leads children to have elevated stress levels, heightened behavioral challenges, and increased emotional maladjustment (Dallaire and Wilson 2010; Muentner et al. 2021).

### *Barriers to Causal Inference*

Although a formidable array of research suggests negative effects of parent incarceration on a host of outcomes, most of this work only conditions on observed characteristics, and it may be the case that unobserved selection or heterogeneity in hard-to-measure constructs such as family troubles or household dynamics drives most of the associations. More specifically, Sampson (2011, 821) emphasizes the need to disentangle selection into incarceration while investigating the causal effects of incarceration when he notes that previous work in this area tends to "rely on observed indicators... [and as] a result it cannot be ruled out that [the] estimated effects of incarceration are instead being driven by correlated pre-existing characteristics of the father". To deal with these issues while using observational or survey data, researchers must take great care in identifying an appropriate comparison group, as children and families with an incarcerated father likely differ in important ways from children and families without an incarcerated father.

Consequently, I employ an innovative analytic strategy that accounts for both observed selection and unobserved heterogeneity in a rigorous way. To accomplish this, I build on a method first used in this area by Porter and King (2015), though a conceptually similar method has been used to assess the impact of incarceration

on the labor market (Western 2002) and to study crime victimization (Bindler and Ketel 2020). I exploit the longitudinal nature of a survey to compare youth who have *already* experienced paternal incarceration by the time the outcomes were measured to those who *will* experience it in the future. Although these two groups are not perfectly identical, leveraging the quasi-random timing of the event with this “strategic comparison group” strategy (Porter and King 2015, 421-3) allows me to “control” for otherwise unobserved variation in demographic, environmental, and behavioral characteristics more stringently than methods (such as propensity score matching) that only leverage differences in observed confounders to reduce pre-treatment differences between groups.

I make two key improvements on the strategic comparison group method (as previously used). First, I restrict the age range of those who have already and will later experience paternal incarceration to age 11 through 20, which makes the two groups even more similar. Second and perhaps most crucially, I additionally employ the covariate balancing propensity score (CBPS) method separately and jointly with the strategic comparison group strategy to address remaining variation between groups (see Frölich, Huber, and Wiesenfarth 2015 and Imai and Ratkovic 2014 for an overview of CBPS). This multifaceted and novel analytic strategy affords me the capacity to account for both observed and unobserved heterogeneity above and beyond any single non-experimental method on its own. The contribution of this article, then, is both substantive and methodological: in using the strategic comparison group strategy together with a covariate balancing procedure to analyze paternal incarceration’s impact on youth expectations and aspirations, I extend the literature by providing the strongest test to date of a core factor that simultaneously reflects and shapes inequality but had been previously overlooked in the literature.

## Data and Methods

The National Longitudinal Study of Adolescent to Adult Health (Add Health) is a nationally representative survey initiated during the 1994-95 school year.<sup>1</sup> Add Health primarily collects demographic, socioeconomic, familial, neighborhood, behavioral, and health data from respondents. Add Health is an ongoing study, and the original sample has been followed for five waves. In Wave I, respondents were in grades 7-12; in Wave IV, which was completed in 2008, respondents were aged 24-32. Add Health has a low attrition rate, with more than 80 percent of Wave I respondents still participating in Wave IV (Harris 2013).

My study uses data from Waves I and IV of Add Health. The outcome variables, as well as demographic (e.g., age, race, gender), personal, and family background control variables, come from Wave I. Wave IV included retrospective data on parental incarceration, asking respondents about their own age when their parent was first incarcerated. Because the age of the respondent at Wave I is known, it is possible (in most cases, see Section B of the online supplement) to ascertain whether or not the respondent had a parent already incarcerated prior to the administration of the Wave I survey. My main strategic comparison group model specifications have a sample size of  $N = 422$  respondents who experience paternal incarceration at some point between a specified age range (explained in detail below); 62 percent

experience paternal incarceration for the first time before Wave I (but after they turn 11) and 38 percent experience it after Wave I (but by age 20). The full pooled sample is approximately 51 percent female, 77 percent white, 14 percent black, and 11 percent Hispanic (see Table 1 in the Results section for more detailed sample characteristics).

### *Outcome Variables*

At Wave I, respondents were asked six questions about their futures. These six questions are: (1) how much they *expect to attend college*, (2) how much they *want to attend college*, (3) how *hopeful they feel* about their future, (4) how likely they think they are to *live to age 35*, (5) how likely they think they are to be *killed by age 21*, and (6) how likely they think it is that they will be *married by age 25*. Expecting to attend college and wanting to attend college are measured on a 1 to 5 scale, with 1 = low and 5 = high. Feeling hopeful about the future is measured on a 0 to 3 scale, with 0 = never/rarely and 3 = most/all of the time (over the past week). Likelihood of living to age 35, being killed by age 21, and being married by age 25 are all measured on a 1 to 5 scale where 1 = almost no chance and 5 = almost certain. I reverse code the variable for being killed by age 21 to align it with the other variables, where a higher score corresponds to a more positive outlook. I then construct the outcome variable from these six questions. First, I standardize each of the six forward-looking variables (to account for differing scales) by converting them to have a mean of 0 and standard deviation of 1 and then, similar to previous work (see Jaynes et al. 2021 and Testa et al. 2021), sum them to produce an index of future orientation. I also standardize the index measure itself, and therefore all estimates presented throughout the article can be interpreted as standard deviation differences in future orientation.

### *Key Independent Variable and Controls*

At Wave IV, respondents were asked if their parent had been in jail or prison, and if so, how old the respondent was the first time their parent went to jail or prison. Responses range from pre-birth to age 30. All respondents answered questions about incarceration for their biological mother and father. Subsequently, respondents were prompted with: “We would like to know about the man/woman you feel raised you”, and if respondents indicated someone other than a biological parent, they answered questions about incarceration for their father and/or mother “figure”. Out of 422 respondents in the ever-incarcerated sample (explained in detail below), 54 indicated that only their father figure (and not their biological father) had spent time in jail or prison. I include these father figures alongside biological fathers because, if the respondent is indicating that this person raised them, it stands to reason that they would still be impacted by that individual’s incarceration. I also consider respondents regardless of father’s resident status: Haskins (2016, 142) notes that “a change in a father’s presence is a significant but not necessary condition for children to be harmed by paternal incarceration”, and Geller et al. (2012) find that deleterious effects of paternal incarceration occur regardless of resident status.

Therefore, for the ever-incarcerated sample, I create a variable that has two levels: one indicates whether a respondent's father had already been incarcerated prior to Wave I, which serves as the primary "treatment" group (referred to as "Pasts"), whereas the second indicates if a respondent experienced paternal incarceration for the first time after Wave I (i.e., the "control" group, referred to as "Futures"). This serves as the primary explanatory variable.

Although this approach seeks to address potential unobserved confounding, I additionally control for several relevant individual and family-level characteristics. Demographic variables include race (coded as mutually exclusive indicator variables for white, black, Hispanic, and Other) as well as sex and age of both the child and focal parent. I also include two socioeconomic variables: economic hardship (dichotomous variable for the focal parent's ability to pay bills, as reported by the focal parent) and parent education level. Parent education is a four-level ordinal variable where 1 = did not complete high school, 2 = graduated high school, 3 = had some post-high school education, and 4 = possesses a college degree (or higher). This variable is included in the models as a series of indicator variables with college degree (or higher) as the reference category. I also include a measure of the adolescent's cognitive skill—the Peabody Picture Vocabulary test (standardized scores)—as a control variable, which may otherwise confound the relationship between paternal incarceration and future orientation if a child's higher skill level causes them to expect more from their futures. Therefore, including this as a covariate helps assuage concerns about the role individual disparities in aptitude may play in relation to future orientation. Finally, I use multiple imputation with chained equations (MICE) to preserve observations with missing covariate values (see Section B of the online supplement for more detail).

### *Analytic Strategy*

Typical methodological complications regarding unobserved and observed variation between treatment and control groups are magnified when using survey data to study the criminal justice system, as most commonly used nationally representative longitudinal surveys are not designed with incarceration as a primary focus and do not lend themselves to conventional econometric methods for causal inference such as a difference-in-differences design. Previous studies on parental incarceration have typically used one of two analytic strategies: (1) between-person matching/weighting or (2) within-person fixed effects.

Although between-person matching/weighting strategies (e.g., propensity score matching) have significant advantages, their utility is attenuated when used with surveys that are not comprehensive enough to adequately address unobserved heterogeneity—and, importantly, surveys typically do not have sufficient information about parent behavior and criminal activity, which could be a key factor in explaining divergent outcomes (Sampson 2011). The limited slate of covariates available to researchers is a hindrance for even the most comprehensive matching/weighting strategy, given that they can only match or weight on observed characteristics. Fixed effects modeling is also a promising and widely used technique for dealing with bias and selection issues. However, it may be less suited for studying parental

incarceration because of the high likelihood that relevant observed and unobserved factors are not time-invariant, and additionally because even time-stable characteristics can only be accounted for in their influence on respondents. In short, even when using these methods, significant barriers still hamper one's ability to make especially strong causal claims with observational data.

Therefore, I use an innovative and multifaceted estimation strategy to assess the impact of paternal incarceration on youth future orientation. For my primary analysis, I employ a version of a strategic comparison group strategy (first used in this context by Porter and King 2015; also see Bryan 2017 and McCauley 2020) where I use ordinary least squares (OLS) regression to compare future orientation of youth whose fathers have already been incarcerated before the outcome is measured to future orientation of youth whose fathers would be incarcerated after the outcome is measured. This counterfactual approach is beneficial in that it allows me to compare very similar families and youth who simply experience the "treatment" (i.e., paternal incarceration) at slightly different points in time. Leveraging variation in the *timing* of the event, as opposed to the mere *occurrence* of the event, helps overcome the confounding and unobserved variation that imperils many attempts by sociologists to make causal inferences with survey data.

The specific approach used in this article, however, makes a number of key improvements on the strategic comparison group strategy as used in previous literature in this area. One improvement is that I use a more restrictive age range: I only include respondents as part of the "treatment" group if they experienced paternal incarceration between age 11 and Wave I (and had not experienced incarceration of another parent figure prior to age 11). This higher age cutoff makes the "treatment" and "control" groups more similar along key background and familial characteristics. This innovation also addresses potential concerns about potential underreporting or misreporting of first time parent incarcerations that occurred while the child was very young (see Table A1 in the online supplement and Alternative Specifications section below for further discussion and analysis; also see Bryan 2017). I also impose an upper-bound age cutoff of 20 for the Futures group, as families where the father's first incarceration occurs in their child's 20s are likely to be substantively different from a family that experiences first time paternal incarceration during their child's adolescence.<sup>3</sup> Therefore, unless otherwise noted, all descriptive statistics and model results use age 11 as the lower-bound cutoff for the Past group and age 20 as the upper-bound cutoff for the Future group. I also conduct sensitivity analyses where I vary the lower-bound age cutoff from age 8 up to age 13 and restrict the sample to a very narrow age window of age 14 to age 18 (results can be seen in Figure 2 and Table A1 in the online supplement, respectively; for a discussion on bandwidth sensitivity, see Cattaneo and Titiunik 2022, 845).

The other significant methodological improvement stems from incorporating the CBPS separately *and together* with the strategic comparison group strategy.<sup>2</sup> That is, after restricting to a more appropriate comparison group, I further strengthen the analysis by applying a covariate balancing technique to make the "treatment" and "control" groups even more similar—thus addressing both unobserved *and* observed heterogeneity in a rigorous way. Previous studies that assess the impact of parent incarceration with observational data often use propensity score matching



or inverse probability of treatment weighting, but CBPS has been found to perform better than most commonly used matching and weighting techniques (Frölich et al. 2015; Imai and Ratkovic 2014). CBPS is a semiparametric method that mitigates model misspecification in typical matching and weighting techniques by using an empirical likelihood approach to jointly maximize the covariate balance and the prediction of the treatment.

Put succinctly, I estimate a series of OLS regression models across a range of covariates, samples, and analytic methods that are designed to address both observed and unobserved heterogeneity in a careful and stringent way. Additionally, across all models, I use robust standard errors that are clustered at the school level, in an effort to take seriously (1) that Add Health uses a school-based sampling design and (2) that the risk and consequences of incarceration tend to be concentrated spatially (Simes 2021). And finally, to be especially vigilant and comprehensive regarding model choice and uncertainty (see Young and Holsteen 2017), I execute a series of robustness checks which demonstrate that the results are not sensitive to a given age cutoff, are not driven by Pasts experiencing more frequent stints of incarceration, and are not reliant on other particular empirical specifications, covariate choices, or model selection. This estimation strategy—buoyed further by a battery of robustness checks—provides sufficient leverage to make the strongest empirical claim of the impact of paternal incarceration on children to date.

## Results

### *Descriptive Results*

Table 1 presents weighted descriptive statistics for three groups: “Pasts”, “Futures”, and “Nevers” (i.e., those who never had a parent incarcerated). Descriptively, the data reveal that youth who experience paternal incarceration at any point in time are more likely to be non-white and have lower socioeconomic status, a finding that is consistent with a long line of prior research (see Enns et al. 2019; Turney 2014, Table 3 on p. 307; Wildeman 2009). The data show that the mean respondent age at Wave 1 was 15.42 years old, with the Pasts (16.00) and Futures (14.58) groups being slightly above and slightly below the overall mean, respectively. The mean age of the respondent at the time of incarceration was 12.82 years old for the Pasts and 16.99 years old for the Futures. Reassuringly for the strategic comparison group strategy, the Pasts and Futures are highly similar along many key background characteristics: differences between them in percent female, percent Hispanic, Peabody Vocabulary Test scores, parent age, parent education (across all four education levels), and parent ability to pay bills are not statistically significant.

Table 2 displays the means for each of the six individual future orientation variables for Pasts, Futures, and those who never had a parent incarcerated (the “Nevers”). A descriptive pattern emerges where Pasts and Futures have lower mean scores than Nevers, and Pasts (mostly) have lower mean scores than Futures. This descriptive trend is consistent with prior research (see Bozick et al. 2010), as the Nevers represent a more advantaged group (see Table 1) of youth who, on average, expect and hope for more out of their lives. Additionally, the fact that the Pasts

**Table 1:** Sample characteristics by parent incarceration status.

	Nevers Mean (SE)	Pasts Mean (SE)	Futures Mean (SE)	p-value <sup>a</sup>
Child characteristics				
Age at W1	15.42 (0.12)	16.00 (0.17)	14.58 (0.20)	< 0.01
Age at Incarceration	- -	12.82 (0.13)	16.99 (0.24)	< 0.01
Female	0.51 (0.01)	0.56 (0.04)	0.57 (0.06)	0.764
White	0.77 (0.02)	0.60 (0.05)	0.73 (0.05)	0.023
Black	0.14 (0.02)	0.28 (0.05)	0.14 (0.04)	0.008
Hispanic	0.11 (0.02)	0.18 (0.04)	0.19 (0.05)	0.858
Cognitive Test Score	102.6 (0.58)	96.60 (1.14)	96.69 (1.42)	0.959
Parent education				
HS Dropout	0.14 (0.01)	0.33 (0.04)	0.25 (0.05)	0.203
HS Degree	0.32 (0.01)	0.28 (0.05)	0.34 (0.05)	0.370
Some post-HS	0.30 (0.01)	0.24 (0.05)	0.31 (0.06)	0.273
College Degree	0.25 (0.02)	0.14 (0.03)	0.09 (0.03)	0.293
Pay Bills	0.86 (0.01)	0.72 (0.04)	0.74 (0.05)	0.718
Parent Age	41.83 (0.17)	39.13 (0.62)	37.99 (0.81)	0.275
Num obs.	11647	268	168	

*Note:* Uses Add Health survey weights. W1 = Wave 1 of Add Health survey. HS = high school.

<sup>a</sup> p-values are for differences between Pasts and Futures.

have a consistently lower mean score than the Futures indicates the relationship of interest goes in the expected direction but could plausibly be a culprit of selection bias if Pasts are simply a more disadvantaged group than the Futures. Therefore, I put this descriptive pattern through a series of rigorous empirical tests to determine whether the observed pattern represents a plausibly causal relationship.

I initially conduct a series of OLS regressions with a naïve comparison group—that is, I compare Pasts to the rest of the analytic sample (i.e., a pooled group of Futures plus Nevers). Table 3 displays these results. Model 1 considers the association without any control variables whereas Model 2 includes a comprehensive set of controls (for all tables with regression output, only the main paternal incarceration

**Table 2:** Means of individual outcome variables by parent incarceration status.

	Nevers	Pasts	Futures
Expect College	0.09 (0.95)	-0.31 (1.15)	-0.16 (1.06)
Want College	0.06 (0.95)	-0.22 (1.18)	0.01 (0.96)
Live to Age 35	0.06 (0.96)	-0.29 (1.24)	0.07 (0.94)
Killed by Age 21	0.01 (0.97)	-0.18 (1.10)	0.03 (0.99)
Married by Age 25	0.05 (0.97)	-0.08 (1.07)	0.00 (1.00)
Hopeful for Future	0.04 (0.98)	-0.14 (1.00)	-0.06 (0.95)

Note: Variables standardized to have a mean of 0 and standard deviation of 1. Standard deviation in parentheses.

coefficients are shown for the sake of brevity; see Section C of the online supplement for full tables with all coefficients displayed). Both models imply a strong baseline relationship between paternal incarceration and future orientation. In addition to establishing this baseline relationship, the results in Table 3 offer two important contextual insights.

First, the magnitude of the coefficient for Model 1 (sans covariates) is extremely large—it suggests that paternal incarceration is associated with a decrease in future orientation of roughly half of a standard deviation. To appreciate the consequence of an effect size of this magnitude, consider that an effect size of exactly half a standard deviations implies that about 69 percent of the entire control group (in this analysis, the Futures and Nevers pooled together) would be below the average person in the treatment (i.e., the Pasts) group (see Coe 2002). Second, the effect size drops precipitously (by roughly 0.17 SDs) after adding covariates to the model. This is unsurprising given that the control variables take on added importance and absorb a larger portion of the effect when comparing extremely dissimilar

**Table 3:** Full (pooled) sample results predicting youth future orientation.

	Model 1	Model 2
Paternal Incarceration	-0.48** (0.07)	-0.31** (0.07)
R <sup>2</sup>	0.01	0.11
Num obs.	11901	11901

Note: Asterisks and coefficients represent differences between those who have already experienced paternal incarceration (Pasts) and a pooled group of those who will later experience paternal incarceration (Futures) plus those who never experience parental incarceration (Nevers). Robust SEs (clustered at the school level) in parentheses. \*\*  $p < 0.01$ ; \*  $p < 0.05$ . Two-tailed tests.

**Table 4:** Ever-incarcerated sample results from strategic comparison group models predicting youth future orientation.

	Model 1	Model 2	Model 3	Model 4
Paternal Incarceration	-0.32** (0.09)	-0.29** (0.10)	-0.31** (0.09)	-0.35* (0.16)
R <sup>2</sup>	0.02	0.06	0.11	0.13
Num. obs.	422	422	422	399

*Note:* Asterisks and coefficients represent differences between those who have already experienced paternal incarceration (Pasts) and those who will later experience paternal incarceration (Futures). Model 4 incorporates Add Health survey weights. Robust SEs (clustered at the school level) in parentheses. \*\*  $p < 0.01$ ; \*  $p < 0.05$ . Two-tailed tests.

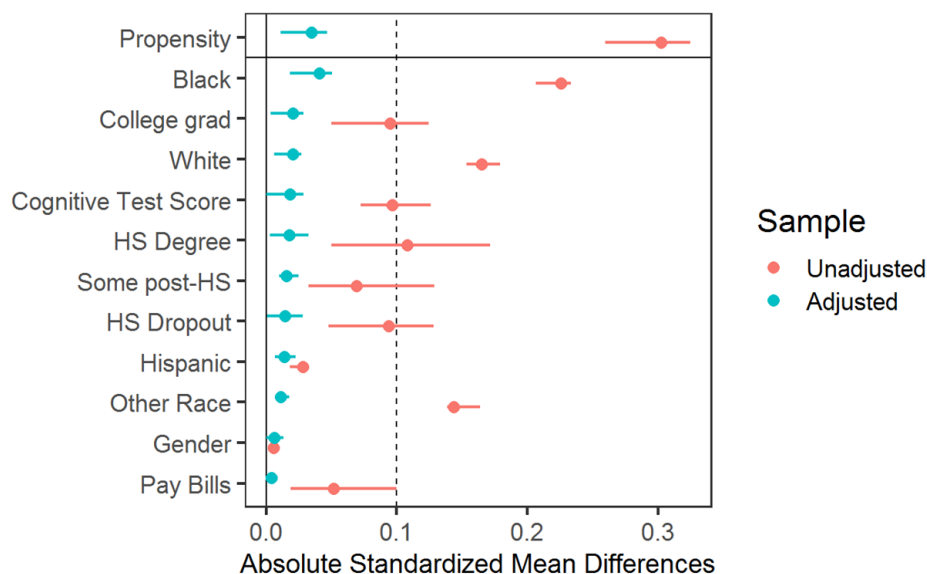
groups. Considered together, the pooled sample comparison group results in Table 3 demonstrate how powerful selection is—those who experience paternal incarceration undeniably differ in essential ways from those who do not.

### *Strategic Comparison Group Results*

These insights reinforce the importance of using a stronger control group and directly comparing Pasts and Futures to each other, which I do in Table 4. This strategy diminishes potential *unobserved* confounding in the pooled sample estimate in Table 3—because families who experience paternal incarceration are very different than families who do not, limiting the sample to only families who experience paternal incarceration at some point *within a compressed time window* allows for a more appropriate comparison. Table 4 displays a series of multivariate results estimating the impact of paternal incarceration on youth future orientation. For all four models, I use the strategic comparison group strategy and directly compare Pasts to Futures (with Futures as the reference category). Model 4 incorporates Add Health survey weights whereas Models 1-3 are unweighted and use robust standard errors clustered at the school level.

In Model 1, I consider the relationship between paternal incarceration and future orientation using the ever-incarcerated sample and strategic comparison group strategy, but with no covariates. The results imply that those who have already experienced paternal incarceration (Pasts) have future orientations that are about 0.32 standard deviations lower than those who will (Futures) experience paternal incarceration later ( $p < .01$ ). This coefficient, from a model with no covariates, is essentially identical to the coefficient from the pooled Model 2 in Table 3, which has a comprehensive set of covariates. This is generally encouraging evidence that the strategic comparison group strategy is indeed successfully addressing heterogeneity. And an effect size of one-third of a standard deviation is substantial—but even with the strategic comparison group strategy, carefully chosen covariates may be needed to further account for selection bias and remaining observed variation.

Adding race and gender (Model 2), socioeconomic status and personal characteristics (Model 3), and survey weights (Model 4) does very little to alter the



**Figure 1:** Covariate balance before and after adjustment.

relationship observed in Model 1 (which, to reiterate, has no covariates). The association remaining effectively unchanged further suggests that the strategic comparison group strategy is working as intended: the “treatment” and “control” groups are already highly similar, and therefore adding covariates does very little to alter the primary association of interest. Moreover, using a strict strategic comparison group method on a strategically restricted sample with a comprehensive set of covariates and survey weights, results indicate that Pasts have expectations and aspirations that are about one-third of a standard deviation lower than that of Futures.

### *CBPS Results*

Next, to enhance the rigor of my methodological approach and improve on the initial efforts of Porter and King (2015) and others, I use a semiparametric modeling strategy—the CBPS—to further assess the impact of paternal incarceration on future orientation. Table 5 displays the results of two CBPS models, which both incorporate the full set of covariates. Model 1 presents results of Pasts compared to the pooled group of Futures plus Nevers (analogous to models in Table 3), whereas in Model 2 I restrict to the ever-incarcerated sample and present results of Pasts compared only to Futures (analogous to models in Table 4). For Model 2 (with the ever-incarcerated sample), visual representations of the improved covariate balance for the treatment and control groups can be seen in Figure 1.

Model 1, which uses CBPS on its own with the pooled sample (and is therefore comparable to the main modeling strategy used in many previous parental incarceration articles), suggests that Pasts have future orientations that are 0.32 standard deviations lower than Futures ( $p < .01$ ). Model 2, which is restricted to

**Table 5:** Pooled sample and ever-incarcerated sample results from covariate-balancing propensity score models predicting youth future orientation.

	Model 1	Model 2
Paternal Incarceration	-0.32** (0.07)	-0.26* (0.11)
R <sup>2</sup>	0.10	0.12
Num. obs.	11901	422

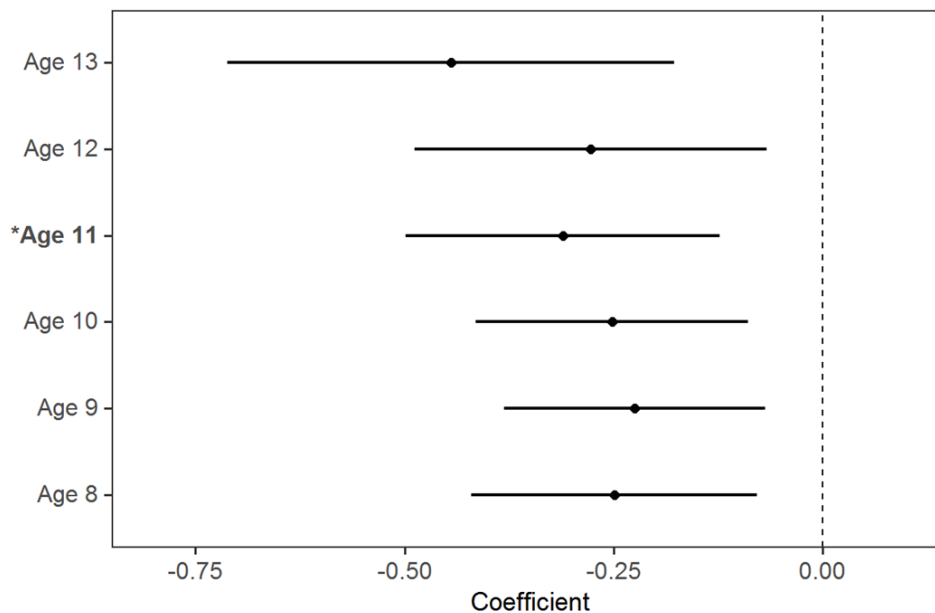
*Note:* Model 1 represents differences between those who have already experienced paternal incarceration (Pasts) and a pooled group of those who have never experienced parental incarceration (Nevers) plus those who will later experience paternal incarceration (Futures). Model 2 represents differences between the Pasts and Futures group exclusively. Robust SEs (clustered at the school level) in parentheses. \*\*  $p < 0.01$ ; \*  $p < 0.05$ . Two-tailed tests.

the ever-incarcerated sample and is therefore the strictest model in my analysis because it best accounts for both observed *and* unobserved heterogeneity, demonstrates that Pasts have future orientations that are 0.26 standard deviations lower than Futures ( $p < .05$ ). Methodologically, the drastically improved balance across treatment and control groups (as seen in Figure 1) combined with Model 2 having the smallest association of any model in the article reveals that selection on *observed* characteristics persists with the strategic comparison group strategy and, thus, that incorporating the covariate balancing procedure offers a meaningful improvement over the strategic comparison group strategy alone. Substantively, the results of these two models in Table 5 provide yet more compelling evidence of a substantial effect of paternal incarceration on children's expectations and aspirations.

Up to this point, I have established a baseline association between paternal incarceration and future orientation using a conventional comparison group, shifted to a more rigorous comparison group, and tested the relationship in a variety of ways using two stringent methodological approaches separately and together. Across all methods and modeling strategies employed thus far, the estimated impact of paternal incarceration on future orientation has remained persistently large and statistically significant.

### *Alternative Specifications*

The results in Tables 4 and 5 show evidence of robustness across covariates and methods. In Figure 2, I additionally demonstrate that the aforementioned results are not contingent on my choice of age cutoff for the Pasts group. Figure 2 displays the relationship between paternal incarceration and future orientation, with a comprehensive set of control variables, for six model specifications that have varying lower-bound cutoffs (age 8–13). One can observe visually from Figure 2 that a general pattern unfolds whereby the magnitude of the coefficient tends to increase as the age cutoff increases. Note that as the age cutoff increases, the estimate is also less precise—intuitively, as I “cut out” more respondents, the sample size decreases in turn. Yet the effect size remains large—as high as more than two-fifths of a



**Figure 2:** Results of alternate age cutoffs predicting youth future orientation. *Note:* \*Main specification.

standard deviation for one specification (Age 13)—and is statistically significant across all models, regardless of the choice of age cutoff.

For an even stricter sensitivity analysis, I limit the age range more narrowly to 14–18, or essentially high school age. Results can be seen in Table A1 in the online supplement. The coefficients are *much* larger in magnitude, though also much less precisely estimated (yet still statistically significant). Limiting to such a tightly restricted age range makes the groups even more similar along background characteristics while also further reducing any potential concerns that recall bias may differ between Pasts and Futures, given how similar they are in age under this specification.

Thus far, these modeling strategies have primarily dealt with selection effects and unobserved heterogeneity. However, the strategic comparison group strategy incidentally leads to a new potential issue: that respondents experience a different *dosage* of the “treatment” (i.e., paternal incarceration) due to the variation in age. That is, because Pasts are younger on average when they first experience paternal incarceration, it may be the case that they experience more subsequent incarcerations after the initial incarceration (prior to Wave 1), and that this *cumulative* disadvantage induces the previously observed results. To address this problem, I limit the sample to Pasts and Futures that experience only one stint of paternal incarceration.<sup>3</sup> Table 6 shows the results of two models (Model 1 sans covariates; Model 2 with a comprehensive set of covariates). These models reveal that the effect of paternal incarceration for those who only experience it one time is statistically significant at  $p < .01$  and is actually *larger* in magnitude than analogous results in

**Table 6:** Once-incarcerated sample results from strategic comparison group models predicting youth future orientation.

	Model 1	Model 2
Paternal Incarceration	-0.41** (0.13)	-0.46** (0.14)
R <sup>2</sup>	0.04	0.16
Num obs.	215	215

*Note:* Asterisks and coefficients represent differences between those who already experienced paternal incarceration only one time and those who will later experience paternal incarceration only one time. Robust SEs (clustered at the school level) in parentheses. \*\*  $p < 0.01$ ; \*  $p < 0.05$ . Two-tailed tests.

Tables 4 and 5. These findings address the concern that Past experiencing more frequent stints of paternal incarceration drives the observed relationship.

Finally, Table A2 in the online supplement displays results of an additional key robustness check: using the strategic comparison group strategy, I iteratively re-run the regressions with one individual future orientation variable dropped from the summed index. Across all six resulting models, the range between the largest and smallest coefficient is only about 0.06. The consistent and statistically significant effect for each subsequent model implies that no one individual future orientation question is driving the observed relationship.

Consequently, across a formidable collection of model specifications, methods, samples, and covariates, the deleterious effect of paternal incarceration on youth future orientation is strong and consistent, ranging anywhere from about one-fourth to more than one-third of a standard deviation across the main models. The fact that results from both the strategic comparison group and CBPS methods—employed separately and concurrently across various specifications—offer essentially identical conclusions affords me considerable capacity to make a strong empirical claim about the relationship between paternal incarceration and youth future orientation.

## Discussion

Expectations and aspirations are fundamental aspects of one's early life course trajectory. Previous work demonstrates that children's future-oriented perceptions have a substantial impact on a wide variety of outcomes related to educational attainment, labor, earnings, and health (Halleröd 2011; Hitlin and Johnson 2015; Johnson and Hitlin 2017; Vaisey 2010). These impacts are consistent across both general (Johnson and Hitlin 2017) and context-specific (Domina, Conley, and Farkas 2011) expectations, and are also meaningful in how they potentially reflect and reproduce socioeconomic inequality (Bozick et al. 2010; Vaisey 2010). Furthermore, the large magnitude of these previous findings suggests that future orientation is a powerful force: Vaisey (2010) shows that higher expectations lead to roughly three and a half times greater odds of graduation, whereas Halleröd (2011) finds that children who



think they will do “much worse” than others have a 13 percent higher risk of lack of income than those who think they will do “just as good” as others.

This body of evidence implies that youth expectations and aspirations are vital to understanding the shape of systematic inequality. Related work shows that social programs and structural conditions may have the capacity to exacerbate or ameliorate these tendencies: historically, social initiatives such as housing vouchers (Chetty, Hendren, and Katz 2016), early childhood investment (Heckman and Karapakula 2019), and Supplemental Nutrition Assistance Program (SNAP) benefits (Gassman-Pines and Bellows 2018) have been found to directly improve the life course trajectory of disadvantaged youth and attenuate intergenerational inequality as a result. On the other hand, structural conditions such as macro-level job loss (Ananat et al. 2011), neighborhood disadvantage (Sampson 2012), aggressive policing tactics (Legewie and Fagan 2019), and mass incarceration (Wakefield and Wildeman 2013) have proven to be deleterious to youth and, subsequently, contribute to ongoing stratification.

However, despite the preponderance of evidence regarding expectations and aspirations’ influence on youth outcomes and its potential implications for bolstering our understanding of inequality more generally, we have limited empirical evidence about how it is shaped or altered by acute adverse events, social and policy conditions, and larger institutional forces. My study begins to address this important gap in the literature, as I consider how experiencing the incarceration of a father impacts youth future orientation. Consequently, this article represents a novel entry in the “incarceration ledger” (Sampson 2011) while illuminating a previously unstudied but highly consequential process through which disadvantage is transmitted from parent to child. The large magnitude and thoroughly tested nature of the effects—in conjunction with the fact that parental incarceration is increasingly common and unevenly distributed across the population (Wildeman 2009)—provide a deeper understanding of how mass incarceration has contributed to persistent stratification over the previous few decades.

In recent years, a compelling body of research has illustrated the deleterious effects of parental incarceration on children and families. My study extends this literature by considering how paternal incarceration influences the way children think about their future. Most previous work in this area relies heavily on propensity score matching, which has significant limitations when used on its own—particularly when relying on surveys that were not designed with the criminal justice system in mind. My novel and robust estimation strategy advances the literature by systematically addressing causal identification concerns in a rigorous and innovative way. I leverage the longitudinal nature of Add Health—and its unique retrospective questions regarding parent incarceration—to conduct a series of highly stringent tests on a strategically chosen sample. In the process, I address the concerns of Sampson (2011), Wildeman (2020) and others by meticulously accounting for two of the most prevalent threats to causal inference using observational data: (1) selection bias, or the fact that families who experience parental incarceration simply differ from those who do not, and (2) omitted variable bias, or the fact that these families differ in ways that are not sufficiently captured in typical survey data. By comparing those who have already experienced paternal incarceration to those

who will experience paternal incarceration later, I analyze nearly identical children and families who merely receive the “treatment” at varying points in time—some right before and some right after the outcome is measured. Limiting the sample with this counterfactual framework in mind effectively holds constant the various background and family-specific characteristics that may differ between those who do and do not experience paternal incarceration. I additionally show that these results remain large and statistically significant when employing the CBPS method separately and together with the strategic comparison group strategy.

Using this innovative modeling approach, I find that, net of social-structural factors and personal characteristics, experiencing a father be sent to prison or jail during a child’s early adolescent or teenage years leads to anywhere from one-fourth of a standard deviation to as high as two-fifths of a standard deviation reduction in their expectations, aspirations, and hopes for the future. These effects are not just empirically meaningful, they are substantial in magnitude—suggesting that parental incarceration’s contribution to social inequality is immense. Comparing these effect sizes to those of other similar studies of parental incarceration provides additional context for appreciating their magnitude: while using propensity score matching or inverse probability weighting, the largest statistically significant effect size for the full sample in Haskins (2015, Table 2) is 0.187 standard deviations, the largest statistically significant effect size for the full sample in Haskins (2016, Table 2) is 0.12 standard deviations, the largest statistically significant effect size in Wildeman and Turney (2014, Table 6) is 0.17 standard deviations, and the largest statistically significant effect size in Turney (2022, Tables 3 and 4) is 0.17 standard deviations.<sup>4</sup>

This consideration takes on additional theoretical and empirical importance given that previous literature suggests low-SES youths’ expectations are less stable across childhood (Bozick et al. 2010). Thus, there may be significant measurement error in my estimates—yet even the *lower-bound estimates* in my primary models are comparable to the *main effects* of these prior studies. Said another way, even if we were to accept only the most conservative lower-bound of the 95 percent confidence interval—whether for CBPS using the entire analytic sample (which would be -0.18 for Model 1 of Table 5),<sup>5</sup> or for strategic comparison group models using the ever-incarcerated sample (-0.13 for Model 3 of Table 4) or a tightly restricted age range sample (-0.37 for Model 1 of Table A1 in the online supplement)—these would still be similar (and even larger in some cases) in magnitude to the main effects in prior parental incarceration literature. In sum, this article’s results are appreciable in magnitude and highly robust across both strategic comparison group and CBPS models as well as a multitude of samples, covariates, and models. In addition to the main finding, I consider the implications of two alternate specifications.

The first alternative specification (see Figure 2) shows that the effect of paternal incarceration may increase with age. Given that the Pasts and Futures become more similar as the age cutoff increases, this result is both methodologically solid and substantively intriguing. A number of potential explanations emerge. Perhaps as youth become older, they are able to grasp more fully the nature of incarceration and interpret its potential ramifications. It is also the case that as the age cutoff increases, the average incident of incarceration is temporally closer to Wave I of the

Add Health survey. This means the father is more likely, on average, to still be in jail or prison at the time of the survey, which could be exerting an ongoing influence on the respondent's mindset. And even if the father has since been released, the closer temporal nature of the incarceration to Wave 1 may lead to the incarceration event being more proximate in the respondent's mind. Adjudicating between these hypotheses is beyond the scope of this article and the available data but is a promising avenue for future research.

I also consider the competing explanation that there is a differing *treatment* effect taking place—that is, that Pasts simply experience more frequent bouts of paternal incarceration prior to Wave 1 because they are (by design) younger, on average, than the Futures when they first experience paternal incarceration. It is theoretically conceivable that this greater accumulation of adverse experiences could be driving the observed main results—but I assuage these concerns by conducting additional tests where I limit the sample to those who only experience paternal incarceration one single time (see Table 6). In fact, these results suggest that there may be a *larger* effect for Pasts who only experience paternal incarceration once. Although I lack the necessary data to speculate extensively on the reasons underpinning this finding, it may be the case that those who experience paternal incarceration one time live in more stable households with a lower propensity to experience paternal incarceration and, therefore, suffer a larger “shock” from the incident. If true, this would be consistent with Turney and Wildeman (2015), who find that the deleterious effects of maternal incarceration on child wellbeing are concentrated amongst those who are less likely to experience maternal incarceration.

My study makes clear that experiencing a father be incarcerated is a salient adverse event that fundamentally reshapes how youth perceive their future. This may have a number of theoretical implications for our understanding of the life course and divergent youth outcomes. Kirk and Sampson (2013, 55), for instance, contend that juvenile arrest serves as a crucial negative turning point in the early life course—this suggests that contact with the criminal justice system is a direct “life course trap” for those who are arrested. This article's results imply that vicarious contact with the criminal justice system may also be an *indirect* life course trap, affecting not only the person involved, but their children as well. I posit that experiencing paternal incarceration serves as a potent life course disruption for children which, troublingly, occurs earlier and more frequently in the life course than juvenile arrest and has a highly influential but primarily *intangible* consequence: in contrast to more visible outcomes such as arrests, test scores, GPA, behavior problems, or health issues, future orientation is rarely measured or observed, which may be why it has yet to be considered empirically in the context of parental incarceration and social inequality. The findings of this article, therefore, reveal a new way of thinking about this fundamental turning point early in the life course of many disadvantaged youth—an adverse experience that may underlie commonly studied impacts of parental incarceration on educational achievement, health and wellbeing, criminal activity, labor, and other important outcomes.

Despite the contributions this article makes, it is not without limitations. First, I am limited by the fact that, despite best efforts, quasi-experimental methods can never fully replicate a randomized controlled trial or natural experiment. In

the context of my study, the Pasts and Futures, although extremely similar, are not perfectly equivalent groups. In an ideal world, I would have a large enough sample size to deal with this more thoroughly by comparing only Pasts and Futures that are identical in the age at which they first experience paternal incarceration. Second, my data on paternal incarceration occurrence and timing are reported by the child, and therefore may be subject to errors of memory and recall. Although this would not be an issue if recall errors are random, it may be troublesome if recall is differentially correlated with future orientation by treatment status (but see Table A1 in the online supplement for results that mitigate this concern). I also do not know the duration of incarceration or what type of criminal activity led to incarceration. This may be significant if different lengths of sentences and types of crime have divergent consequences for families and children, although the strategic comparison group strategy should help account for much of this otherwise unobserved variation. Additionally, Add Health does not provide detailed data on how a father's residential status may have fluctuated pre-Wave I, which is particularly relevant for incarcerated men (see Western 2018). Finally, I am also limited by the fact that my outcome variable may not be fully comprehensive in terms of its coverage of future-oriented constructs (but see Table A2 in the online supplement, which demonstrates consistent results across constructs).

In spite of these limitations, my study markedly advances our understanding of the sources of the intergenerational transmission of educational and socioeconomic disadvantage by showing—with the strongest tests in this area to date—how mass incarceration, a novel and extensive social experiment unique to the contemporary United States, has contributed to enduring disparities through persistently high rates of paternal incarceration.

## Notes

- 1 The data underlying this article is restricted-use Add Health data, which is housed at the Carolina Population Center at UNC-Chapel Hill. See their website for guidance on obtaining restricted-use data: <https://data.cpc.unc.edu/projects/2/view>. Also see Add Health's website for additional technical information: <https://addhealth.cpc.unc.edu/>. While these data cannot be shared publicly, a complete code replication package for all analyses in this paper will be made publicly available.
- 2 CBPS and MICE were conducted using R packages: 1) Greifer, N (2021). *cobalt: Covariate Balance Tables and Plots*. R package version 4.4.1. <https://CRAN.R-project.org/package=cobalt>. 2) Pishgar F, Greifer N, Leyrat C, Stuart E (2021). "MatchThem:: Matching and Weighting after Multiple Imputation." *The R Journal*. <https://journal.r-project.org/archive/2021/RJ-2021-073/>. 3) van Buuren S, Groothuis-Oudshoorn K (2011). "mice: Multivariate Imputation by Chained Equations in R." *Journal of Statistical Software*, 45(3), 1-67. doi:10.18637/jss.v045.i03.
- 3 To clarify, a respondent is excluded from the analysis if they indicated their biological father or father figure experienced more than one incarceration stint, or if they indicated their biological father and father figure both experienced exactly one incarceration. I restrict Futures as well as Pasts because it is possible that a respondent's father was incarcerated a single time due to a longer sentence following a more severe crime, and

this could bias results if only Pasts were restricted to one incarceration (though in practice, the results are essentially identical if I restrict only Pasts to one incarceration).

4 For added context: in the education literature, an effect size greater than 0.20 standard deviations is considered “large” (see Kraft 2020).

5 Note that this particular result, using CBPS comparing those who do experience paternal incarceration to those who do not, would be the most analogous (from a modeling/methodological standpoint) to those from previous parent incarceration studies.

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