Incarceration and Household Wealth *

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ABSTRACT

A large literature documents the deleterious economic consequences of incarceration. But little is known about the consequences of incarceration for household wealth, a distinct indicator of economic wellbeing that may be especially important to the survival of low-income families, or about the spillover economic consequences of incarceration for romantic partners and communities. In this article, we use individual-level data (from the Fragile Families and Child Wellbeing Study) and state-level panel data (from 1985 to 2005) to examine how incarceration is associated with asset accumulation and asset loss among formerly incarcerated men, their romantic partners, and their communities. Results from the individual-level data, which pay careful attention to the social forces that select individuals into incarceration, document that incarceration is negatively associated with vehicle and bank account ownership among men and that these associations are concentrated among men who previously held such assets. In addition, the economic consequences of incarceration spill over to the current and former romantic partners of these men. Results from the state-level data document that incarceration rates diminish homeownership rates among Blacks and, in doing so, widen Black-white inequalities in homeownership. Taken together, the findings show that the considerable collateral consequences of incarceration—including those that extend beyond the offender—may increase inequality in household wealth.

Incarceration rates in the United States have risen at an unprecedented rate over the past four decades. More than 2.3 million individuals are currently incarcerated in prisons or jails. Ten million individuals, many of whom are still under correctional supervision, are released back to their families and communities annually (Glaze 2011; McLean and Thompson 2007). Incarceration is disproportionately concentrated among already disadvantaged and marginalized populations, including racial minorities, the impoverished, and those with low educational attainment. For example, three-fifths of Black men without a high school diploma are incarcerated by their mid-30s, compared to only 11% of white men with the same educational attainment (Pettit and Western 2004). These men are more likely to be incarcerated than they are to be married or employed and, accordingly, incarceration is a normative and transformative life course experience for some population groups (Western and Pettit 2010).

Given the historically unprecedented rise in incarceration, its unequal distribution across the population, and its wide-reaching consequences for individuals, families, and communities, incarceration may increase social and economic inequality and contribute to the intra- and intergenerational transmission of disadvantage (Alexander 2010; Wacquant 2001; Wakefield and Uggen 2010; Wakefield and Wildeman 2013). A large qualitative, quantitative, and experimental literature documents how incarcerated men have few opportunities to earn income while in prison or jail and, upon release, experience difficulty finding employment, earn low wages, and accumulate legal debt (Apel and Sweeten 2010; Bushway, Stoll, and Weinman 2007; Grogger 1995; Pager 2003; Western 2002, 2006; Western, Kling, Weiman 2001). But despite research documenting the deleterious and accumulating economic effects of incarceration for currently and formerly incarcerated men, no existing research systematically considers the consequences of incarceration for personal wealth among incarcerated men. And despite the fact that most incarcerated men are connected to families and communities, little research considers the spillover economic consequences

for the romantic partners of these men or for their communities (though see Clear 2007; Geller et al. 2009; Geller, Garfinkel, and Western 2011; Schwartz-Soicher, Geller, and Garfinkel 2011).

This lack of attention to the relationship between incarceration and wealth is an especially important oversight given the large racial disparities in wealth, especially between Blacks and whites, and the importance of wealth for intra- and intergenerational mobility (Conley 1999; Oliver and Shapiro 2006; Sherraden 1991). We suspect that sociological inattention to the effects of incarceration on personal wealth may result from the assumption that wealth is not a particularly important dimension of poverty and inequality or from the belief that wealth accumulation is beyond the capacities of the generally disadvantaged individuals affected by incarceration.

However, we first note that though wealth has not featured prominently in sociological accounts of poverty and inequality, the tide is beginning to turn as scholars have recognized that wealth, which for most people includes the ownership of assets (e.g., homes, cars, bank accounts) and small amounts of financial savings, is a distinct and important measure of economic wellbeing (Keister and Moller 2000) that is not simply correlated with earnings or employment (McKernan, Ratcliffe, and Williams 2012; Schneider and Tufano 2007). Second, the common assumption that poverty is incompatible with saving and asset building is wrong. A large body of research has shown that men and women living in poverty often wish to save, try to save, and can build personal wealth when supports are in place and impediments are removed (McKernan et al. 2012; Sherraden 2009). For low-income families, those most affected by incarceration, these assets may be particularly important for survival (Edin 2001) and play an important role in buffering households from financial shocks (McKernan, Ratcliffe, and Vinopal 2009).

In this article, we use both individual- and state-level data to examine how incarceration is associated with asset ownership among formerly incarcerated men and their romantic partners, and how such associations may aggregate and extend through social networks and communities to

generate area-level inequalities. We first use individual-level data from the Fragile Families and Child Wellbeing Study to understand how incarceration is associated with home, vehicle, and bank account ownership among men and their current or former romantic partners. We also examine heterogeneity in the association between incarceration and asset ownership by co-residential status and assets prior to incarceration. Second, given the stark inequalities in both incarceration and asset ownership between Blacks and whites in the United States, we use macro-level data to investigate state-level associations between incarceration rates and the Black-white gap in asset ownership (specifically, homeownership). This analysis examines how the negative effects of incarceration for men, their partners, and others in their communities may aggregate to group-level inequalities. These two sets of analyses, separately and jointly, provide the first examination of incarceration's consequences for household wealth and advance our understanding of the collateral consequences of incarceration for social stratification among families and communities.

BACKGROUND

Asset Ownership and Wellbeing

Wealth is a key aspect of household wellbeing and captures a dimension of advantage that is related to, but distinct from, other elements of socio-economic status such as income and education (Keister and Moller 2000; Schneider and Tufano 2007). Yet, levels of wealth holding are very low for many American households, wealth is more unequally distributed in American society than income, and there are pronounced gaps in wealth by race (Brandolini, Magri, and Smeeding 2010; Neckerman and Torche 2007; Taylor et al. 2011; Wolff 2010). These insufficiencies and inequalities are consequential because wealth affects wellbeing (e.g., Hajat et al. 2011) and also serves as an important intervening mechanism in processes of intergenerational inequality and cumulative disadvantage (Spilerman 2000).

More specifically, financial savings are an important means by which families weather short-term financial emergencies (Lusardi, Schneider, and Tufano 2011; McKernan et al. 2009), and savings improves long-term wellbeing through a number of social processes (Sherraden 1991). For example, financial savings are positively associated with children's educational achievement (Orr 2003; Shanks 2007; Yeung and Conley 2008) and negatively associated with teenage pregnancy (Green and White 1997). Additionally, scholars have focused on the beneficial effects of simply owning assets—such as homes, cars, and bank accounts—apart from the monetary worth of such goods. For instance, owning a vehicle, regardless of its market value, has real effects on labor market outcomes because it facilitates access to work (Raphael and Rice 2002). The simple ownership of assets is also related to entry into marriage, increased marital stability, improved educational outcomes for children, greater residential stability, and better health (Dietz and Haurin 2003; Gibson-Davis, McLanahan, and Edin 2005; Schneider 2011; Shobe 2002).

Assets such as vehicles or bank accounts can also help families manage key tasks that can be costly to accomplish in their absence. For instance, unbanked households often resort to high-cost alternative financial services providers such as check-cashers, pawnshops, and informal lenders (Barr 2004). Apart from the value of the funds in the bank account, the account itself can be an important asset that allows families to avoid the otherwise high costs of managing cash.

Consequences for Asset Ownership among Incarcerated Men

There are good reasons to expect deleterious consequences of incarceration for asset ownership among currently and formerly incarcerated men. Perhaps most obviously, time behind bars impedes men's economic opportunities during incarceration and after release, which may encumber the acquisition and maintenance of assets. The negative, enduring effects of incarceration for men's economic wellbeing begin during incarceration. Men behind bars, most of whom were working prior

to confinement, have few opportunities to earn income and, when they do, these employment opportunities pay, on average, only $0.15 an hour (Cowan and Reich 2013). And in spite of these reduced earnings that accompany confinement, incarcerated men have expenses that do not disappear upon incarceration or that stem directly from the incarceration experience. For example, during incarceration, child support orders often continue to accrue (Pearson 2004) and homeowners are usually required to make their mortgage payments, both despite the fact that the incarcerated earn little income. Further, most incarcerated men incur legal debt resulting from the imposition of fines or fees (Harris, Evans, and Beckett 2010).

Release from prison does not resolve these economic difficulties and may even amplify them. Perhaps most importantly, former inmates have difficulty finding employment. For one, employers may stigmatize men with criminal records (Pager 2003; Pager, Western, Bonikowski 2009). Additionally, the time spent incarcerated reduces human and social capital. Incarcerated men lose work experience, become disconnected from social ties that facilitate legal or illegal employment, and acquire social ties that may impede securing employment, thereby making employment difficult (Hagan 1993). Indeed, research repeatedly shows that formerly incarcerated men are often disconnected from the labor market and, among the employed, earn lower wages (Apel and Sweeten 2010; Bushway et al. 2007; Grogger 1995; Pager 2003; Pettit and Lyons 2007; Waldfogel 1994; Western 2002, 2006; Western et al. 2001). Incarceration is especially detrimental to the employment prospects and earnings of Black men compared to white men (Lyons and Pettit 2011; Pager 2003). To add to their labor market difficulties, formerly incarcerated men experience an accumulation of legal debt across the life course (Harris et al. 2010). Legal debt, of course, reduces access to housing and credit markets, but can also create a disincentive for employment in the formal labor market because payments are often made through wage garnishment (Harris et al. 2010).

Although conviction for fraud is generally grounds to reject an applicant for a bank account (Campbell, Martinez-Jerez, and Tufano 2012), prior incarceration for other offenses is not a legal disqualification for holding a mortgage, taking a vehicle loan, or having a bank account. However, the incapacitation associated with incarceration may directly lead to the loss of assets such as homes, vehicles, and bank accounts. Non-payment of loans may result in foreclosure and repossession, and the failure to meet minimum bank account balance requirements or inadvertent overdrafts may lead to the assessment of fees and involuntary bank account closure (Campbell et al. 2012; Consumer Financial Protection Bureau 2013). These processes may also inhibit future asset re-acquisition if they lead to negative credit reports and to flags in the ChexSystems database that banks use to vet potential account holders (Barr and Blank 2009).

But incarceration may also have deleterious effects on asset ownership for reasons not narrowly economic. For one, formerly incarcerated men may end up residing in particularly disadvantaged neighborhoods following prison release (Massoglia, Firebaugh, and Warner 2013). These neighborhoods are distinguished not only by high levels of poverty and segregation, but also by a paucity of institutional supports for asset building, such as banks and credit unions (Graves 2003; Temkin and Sawyer 2004). The lack of such institutions in disadvantaged neighborhoods may create a barrier to account opening, saving, and obtaining a mortgage (Ergungor 2010). Further, the over-representation of alternative financial service providers in disadvantaged neighborhoods—including check-cashers, payday lenders, and pawnshops—may deplete wealth (Graves 2003; Prager 2009; Temkin and Sawyer 2004).

Relatedly, qualitative research shows that the increase in surveillance and policing in poor neighborhoods, which has risen in tandem with incarceration rates, encourages men to go "on the run" and withdraw from social institutions including banks. In one ethnographic account of a heavily surveilled neighborhood, none of the men in the sample established bank accounts (Goffman

2009:353). Formerly incarcerated men may have warrants out for their arrest, especially if they are not fulfilling child support obligations or making payments to reduce their legal debt (Harris et al. 2010), or may simply avoid banks and other institutions for fear of being imprisoned again.

Consequences for Asset Ownership among Women Connected to Incarcerated Men

The consequences of incarceration for asset ownership likely do not end with its effects on incarcerated men. These marginalized men—prior to confinement, during incarceration, and after release—are members of families. Most prisoners have children and, prior to their incarceration, are in romantic relationships (Mumola 2000). Though the women who share children with these men may shed the stigma of incarceration by distancing themselves from the incarcerated or moving on to a new romantic partner, it is unlikely they can discard the economic consequences of their partners' incarceration. The incarceration of a romantic partner, and the economic consequences of this incarceration, may cause women to both lose assets and have difficulty acquiring assets.

To begin with, though they are a generally disadvantaged group, incarcerated men often contribute economically—via legal or illegal earnings—to family life prior to their confinement (Travis, McBride, and Solomon 2005), and the removal of men from households via incarceration can be associated with a large and immediate decrease in household income. These decreases in household income are likely especially consequential when men are living with romantic partners, but the economic shocks may also be felt among separated romantic partners, as many of these fathers are paying formal or informal child support prior to incarceration (Geller et al. 2011). Further, women who share children with incarcerated men may be left with parenting and household responsibilities that force them to leave the paid labor market and, consequentially, impede their ability to maintain the family's economic standard of living (Arditti, Lambert-Shute, and Joest 2003; Braman 2004; Comfort 2008; Lynch and Sabol 2004).

In addition to the reduction in household income that accompanies incarceration, romantic partners of the incarcerated may have additional expenses associated with this incarceration. For one, maintaining relationships with incarcerated men is costly. Prisons are often located far from former residences of the incarcerated and, as such, getting to these prisons is time-consuming and expensive (Grinstead et al. 2001; Hairston 2005). Women may also spend money on care packages and collect calls (Comfort 2008). One study found that poor women spent more than one-quarter of their income maintaining contact with an incarcerated partner (Grinstead et al. 2001).

The economic consequences of incarceration for these women likely persist after the release of their romantic partner. When men return to households after incarceration, as many do (Harding, Morenoff, and Herbert 2013), their difficulty securing employment may strain household finances. Women married to these men are likely especially disadvantaged, as they are often responsible for legal fees and fines. Beckett and colleagues, for example, describe how legal debt can influence family life above and beyond its influence on the offender. They report that, in Washington State, county clerks can seize up to 25% of the earnings of former inmates' spouses. They are also allowed to seize jointly held bank assets, home equity, and tax refunds (Beckett, Harris, and Evans 2008). The negative effects of incarceration for asset ownership among the women connected to these men may also work through relationship dissolution. Incarceration destabilizes romantic relationships (Lopoo and Western 2005; Massoglia, Remster, and King 2011), which can have serious economic consequences for women (Avellar and Smock 2005; Duncan and Hoffman 1985).

Indeed, though the majority of research on the economic consequences of incarceration documents deleterious effects for the incarcerated, some research shows that these deleterious outcomes extend to romantic partners of the incarcerated (Geller et al. 2009; Geller et al. 2011; Hairston 1998; Phillips et al. 2006; Schwartz-Soicher et al. 2011). These economic consequences are in line with a burgeoning literature documenting other spillover effects of incarceration, including

increased housing insecurity (Geller and Franklin 2013), amplified participation in government programs (Sugie 2012), reduced instrumental support (Turney, Schnittker, and Wildeman 2012), and increased mental health problems (Wildeman, Schnittker, and Turney 2012).

Consequences for Black-White Inequality in Asset Ownership

Though it is likely that incarceration impedes asset accumulation and facilitates asset loss for the incarcerated and their romantic partners, it is also possible that incarceration, which is disproportionately experienced by Blacks, contributes to the well-known racial disparities between Blacks and whites in asset ownership in the United States. Recent estimates suggest that Blacks have, on average, only about one-fifth of the net worth of whites (Wolff 2010). Compared to whites, Blacks are less likely to own homes (49% compared to 75%), vehicles (69% compared to 88%), and financial accounts (58% compared to 82%) (Taylor et al. 2011; Wolff 2010). Further, the gap in Black-white median wealth has increased throughout the past two decades (Shapiro, Meschede, and Osoro 2013; Taylor et al. 2011; Wolff 2010).

Incarceration, given its unequal distribution across the population and its attendant and steep economic consequences, especially for Blacks (Lyons and Pettit 2011), may increase racial inequalities in wealth. To begin with, any individual-level effects of incarceration on wealth could translate to aggregate Black-white inequalities. Though incarceration rates have increased for both racial groups throughout the past four decades, incarceration is much more prevalent among Blacks than whites. In 1980, 0.6% of white men aged 20-34 were incarcerated, compared to 1.8% of comparably aged white men in 2008. For Blacks, the proportional increase was smaller, rising from 5.2% in 1980 to 11.4% in 2008, but the share of Black men incarcerated in 2008, more than one in ten, was six times higher than that of their white counterparts and comprised a substantial share of the Black male population (Pettit 2012). Cumulated over the life course, Pettit (2012) estimates that in 2009, 28% percent of

Black men aged 30-34, compared to 5.4% percent of white men, had ever been imprisoned. Simply by virtue of this differential exposure, it is possible that any negative effects of incarceration on asset ownership might exacerbate Black-white wealth inequality.

Additionally, though incarceration could increase aggregate levels of Black-white wealth inequality by reducing asset ownership only among formerly incarcerated men and their partners, we suspect that these effects extend more broadly. Prior research shows that having a poor family member is negatively associated with wealth among middle-class individuals (Chiteji and Hamilton 2005; Heflin and Patillo 2002; O'Brien 2012), suggesting that incarceration may have spillover effects on extended family members. The geographic concentration of incarceration could also broadly undermine the wealth of entire communities by destabilizing these neighborhoods along multiple dimensions (Clear 2007; Pettit 2012). In sum, the racially unequal nature of the incarceration experience—combined with the potential for broad negative effects on former prisoners, their partners, their families, and their communities—likely increases wealth inequality between Blacks and whites.

Threats to Causal Inference

Though existing evidence strongly suggests that incarceration has deleterious consequences for asset ownership and for Black-white inequalities in asset ownership, there are steep threats to causal inference. It is possible that the observed individual-level relationship between incarceration and asset ownership reflects formidable selection forces. Incarceration, as discussed earlier, is most common among the socioeconomically disadvantaged—those with low levels of education, tenuous attachments to the formal labor market, and low earnings—and, as such, these individuals are likely to experience real challenges to asset ownership even absent incarceration (Wakefield and Uggen 2010). There are also other individual-level differences between incarcerated men and their never

incarcerated counterparts. Incarcerated men are less likely to be married, more likely to experience depression, and have less self-control, all of which may make acquiring or maintaining assets difficult (Turney et al. 2012). Relatedly, any observed associations between state-level incarceration and the state-level Black-white gap in homeownership may also result from spurious state-level characteristics. Further, we caution against any mono-causal interpretation of Black-white wealth inequality as, to the extent that incarceration is related to this inequality, it is likely to be one of many causes that include differences in family background characteristics (Conley 1999; though see Killewald 2013), income (Gittleman and Wolff 2004), portfolio allocation (Chiteji and Stafford 1999), and racial discrimination in credit and housing markets (Oliver and Shapiro 2006). Given these substantial threats to causal inference, we carefully account for spuriousness in the individual-level analyses (e.g., by adjusting for an array of covariates and employing propensity score models) and in the state-level analyses (e.g., by adjusting for an array of covariates and including state and year fixed-effects.

DATA AND METHODS

We examine the association between incarceration and asset ownership with two data sets uniquely positioned to answer our research questions. First, to understand the individual-level association between incarceration and asset ownership, we use data from the Fragile Families and Child Wellbeing Study, a longitudinal study of nearly 5,000 new and mostly unmarried parents in 20 large U.S. cities (Reichman et al. 2001). Second, we use state-level panel data (1985 to 2005), compiled from a variety of private-sector and governmental sources, to examine how state-level incarceration rates affect state-level homeownership rates for whites, for Blacks, and for the gap between them. Below, we first detail the Fragile Families data, the key measures, and our analytic strategy. Then we detail our state-level measures and analytic strategy.

Fragile Families and Child Wellbeing Study

Data Source

The Fragile Families and Child Wellbeing Study, which began in 1998, is a longitudinal study of nearly 5,000 new and mostly unmarried parents in 20 large U.S. cities. Mothers and fathers completed a survey almost immediately after the focal child was born, between February 1998 and September 2000, and were interviewed by telephone approximately one, three, five, and nine years later. About 86% of mothers and 78% of fathers in the sampling frame completed the baseline interview. Of these, about 89%, 86%, 85%, and 76% of mothers and 78%, 69%, 67%, 64%, and 59% of fathers completed the one-, three-, five-, and nine-year surveys, respectively.[1] Given that the key explanatory variable, paternal incarceration, is most precisely measured between the three- and five-year surveys, the analyses primarily use data from the first four waves of data collection (with paternal incarceration measured between the three- and five-year surveys and outcomes measured at the five-year survey). Excluding data from the nine-year survey is also advantageous because this wave took place during the Great Recession, which profoundly altered patterns of economic wellbeing and the strength of housing markets in the United States (Pilkauskas, Currie, and Garfinkel 2012).

The Fragile Families data are ideal for considering the association between incarceration and asset ownership. First, because the data include an oversample of unmarried parents, a disproportionately disadvantaged group, they comprise a relatively large number of incarcerated men. The demographic characteristics of these men are comparable to men with young children in local jails, state prisons, and federal prisons (Turney and Wildeman 2013). The data are also advantageous because they include multiple measures of asset ownership (home, vehicle, and bank account) reported by both partners, as well as an array of covariates, both of which facilitate the estimation of causal effects. Additionally, though the data are observational and are, therefore, limited in their ability to provide causal estimates, their longitudinal nature allow for various strategies to reduce unobserved

heterogeneity. Finally, examining asset ownership among parents, specifically, may be especially important because these households are of considerable policy interest, as they include already vulnerable children for whom assets may be especially consequential (Orr 2003; Shanks 2007; Yeung and Conley 2008).

The analyses use two different analytic samples. The first analytic sample estimates fathers' assets (N = 2,703) and the second one estimates mothers' assets (N = 3,831). To construct the first analytic sample, we first deleted the 2,173 observations in which the father did not participate in either the three- or five-year surveys. We then deleted an additional 22 observations missing data on any of the three dependent variables (described below). The second analytic sample excludes the 1,051 observations in which the mother did not participate in the three- or five-year surveys and the additional 16 observations missing data on any of the three dependent variables.[2]

There are several differences between these two analytic samples and the full Fragile Families sample. The analytic samples are generally more advantaged than the full sample, suggesting our estimates are likely conservative. Fathers in the analytic sample, compared to fathers in the full sample, are more likely to be non-Hispanic white and less likely to be Hispanic, less likely to be foreign-born, more likely to be living with the mother at baseline, and have lower educational attainment at baseline. Importantly, at the one-year survey (when comparable measures of asset ownership were first asked), fathers in the analytic sample are more likely to report homeownership (24% compared to 22%) and bank account ownership (57% compared to 54%). With respect to mothers, there are few differences between the analytic and full samples. Mothers in the analytic sample are less likely to be foreign-born and more likely to have education beyond high school at baseline. In both analytic samples, missing covariates values are preserved by producing 20 multiply imputed data sets and averaging results across data sets.

Measures

Asset ownership. The dependent variables include home, vehicle, and bank account ownership, all measured at the five-year survey. First, fathers and mothers were asked about their "current housing situation," and are considered homeowners if they reported owning their own home. Parents who did not move between the three- and five-year surveys were not asked this question at the five-year survey and, for these parents, homeownership is measured by information provided at the three-year survey.[3,4] Second, vehicle ownership is measured affirmatively if fathers and mothers report they or their romantic partner (if they have a partner) report owning a car, truck, or van.[5] Third, bank account ownership is measured affirmatively if fathers and mothers report they have a bank account in their name (and/or their name and their partners' name). Parents reporting that the account is only in their partners' name are considered to not have a bank account. In some analyses, we examine change in asset ownership by adjusting for asset ownership at the three-year survey.

Recent paternal incarceration. The key explanatory variable is recent paternal incarceration, which indicates that the father spent time in prison or jail between the three- and five-year surveys.[6] Both parents were asked about fathers' recent paternal incarceration. Consistent with prior research, which suggests that individuals may under-report incarceration (Geller et al. 2012; Groves 2004), fathers are considered recently incarcerated if either the mother or father reports that the father was incarcerated (or if the father was interviewed in prison or jail). Because incarceration may be associated with a mechanical loss of assets for incarcerated men, in our estimates of fathers' assets, we control for current incarceration (prison or jail at the five-year survey). Estimates of mothers' assets do not include this control, as we do not expect a mechanical loss for them, but supplemental analyses show that results remain robust to this additional control.

Though our measures of current and recent incarceration are quite precise, certain characteristics of the incarceration experience remain unmeasured. For example, we cannot

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distinguish between jail and prison spells. However, others have speculated that most incarcerated

men in these data experience jail and not prison spells (Turney et al. 2012:470). Understanding

relatively short-lived jail spells is important for two reasons. First, jail incarceration is a sufficiently

weak treatment that it quite possibly has no independent influence on asset ownership, as opposed to

prison incarceration, which is often lengthy, and almost certainly diminishes asset ownership. Second,

cycling through jail is a common experience (e.g., Minton 2012:4), which means that our results are

applicable to a much larger population than if we were solely considering prison incarceration. In

addition to our inability to distinguish between jail and prison, these data are also limited because,

though information about incarceration duration and offense type is collected, there exists a relatively

large amount of missing data.[7] We consider variation by duration and offense type in supplemental

analyses described below.

Control variables. Given the steep threats to causal inference in estimating the association

between incarceration and asset ownership, the multivariate analyses adjust for a wide array of

individual-level characteristics associated with both incarceration and asset ownership. Control

variables include the following demographic characteristics: race (non-Hispanic white, non-Hispanic

Black, Hispanic, non-Hispanic other race), foreign-born, age, lived with both parents at age 15,

relationship status with other parent (married, cohabiting, non-residential romantic relationship,

separated), in a relationship with a new partner, and number of children in the household. Analyses

also control for economic characteristics (including employment in the past week, education [less than

high school, high school diploma or GED, post-secondary education], income-to-poverty ratio, and

welfare receipt) and health characteristics (including depression, fair/poor health, and substance

abuse). Finally, analyses control for fathers' and mothers' cognitive ability (measured by the Weschler

Adult Intelligence Scale [WAIS]), fathers' domestic violence (mothers' reports that the father hit,

kicked, or slapped her), fathers' and mothers' impulsivity (Dickman 1990), and fathers' and mothers'

prior incarceration. Descriptive statistics of all variables used in the analyses are presented in Appendix Table A1.

For the most part, estimates of fathers' asset ownership adjust for fathers' characteristics and estimates of mothers' asset ownership adjust for mothers' characteristics. But controls especially associated with selection into incarceration—substance abuse, domestic violence, cognitive ability, impulsivity, and prior incarceration—are included in analyses estimating both fathers' and mothers' outcomes. All control variables, with one exception, are measured at or before the three-year survey and, thus, prior to the measure of recent paternal incarceration. Mothers' impulsivity is measured at the five-year survey, the only survey wave this information is available, but this should not bias the results because impulsivity is considered a stable characteristic (Gottfredson and Hirschi 1990).

Analytic Strategy

The analytic strategy for the Fragile Families analyses proceeds in three stages. In the first stage, presented in Table 1, we use logistic regression models and propensity score models to estimate fathers' and mothers' asset ownership as a function of recent paternal incarceration. In this and subsequent tables, Panel A estimates fathers' assets and Panel B estimates mothers' assets. The models in Table 1 progressively account for selection into incarceration, a substantial threat to causal inference. In Model 1, we adjust for a host of covariates that may render the relationship between incarceration and asset ownership spurious. These include demographic, socioeconomic, health, and selection characteristics. Model 2 examines change in asset ownership by adjusting for a lagged dependent variable.[8] Model 3 restricts the sample to fathers (and mothers who share children with fathers) most at risk of incarceration, those incarcerated prior to the three-year survey. In this model, the reference group is fathers (and mothers attached to fathers) who have experienced incarceration but not in the past two years, which diminishes unobserved heterogeneity (LaLonde 1986; Leamer

1983). Finally, Model 4 presents results from propensity score matching, which allows us to match recently incarcerated fathers and not recently incarcerated fathers on the distribution of their covariates. Within each of the 20 imputed data sets, we generate a propensity score for each individual and ensure the treatment (recently incarcerated fathers) and control (not recently incarcerated fathers) have a similar distribution of covariates. We then use kernel matching (kernel type = epanechnikov, bandwidth = .06) to estimate asset ownership (averaging results across the data sets). Kernel matching compares each treatment observation with all control observations that are weighted according to their distance from treated cases.[9] Because there may remain differences between the treatment and control groups, as balance is sometimes imperfect, we further adjust for all covariates when matching (Shafer and King 2008).

The second and third analytic stages investigate heterogeneity in the relationship between recent paternal incarceration and asset ownership. In the second analytic stage, presented in Table 2, we examine differences in the association by co-residence at the three-year survey. These analyses provide a check on our previous analyses, as we do not expect co-residence to differentially affect the association between recent paternal incarceration and fathers' asset ownership and we do not expect recent paternal incarceration to affect asset ownership among mothers not residing with fathers prior to incarceration. For both fathers and mothers, we estimate subgroup analyses for parents co-residing at the three-year survey and for parents not co-residing at the three-year survey and test for differences across the two groups (Paternoster et al. 1998).[10] We include all covariates and a lagged dependent variable in these models (the equivalent of Model 2 of Table 1).

Finally, in the third analytic stage, presented in Table 3, we examine differences in the association by prior asset ownership. These analyses allow us to better understand whether incarceration results in fathers and their romantic partners losing assets, experiencing impediments to asset acquisition, or some combination of the two. We estimate subgroup analyses of parents who did

and did not report home, vehicle, and bank account ownership at the three-year survey and test for differences across groups, adjusting for all control variables in the equivalent of Model 1 of Table 1. All analyses include robust standard errors to account for the clustering of individuals in cities.

State-Level Data

Data and Measures

Homeownership. Our key outcome measure is the state-level Black-white homeownership gap. We construct this measure by aggregating individual-level reports of homeownership in the annual Current Population Survey (CPS) March supplements to the state level, following the general approach of using CPS data to construct sub-group state-level rates of homeownership that has been used in literature considering the effects of homeownership on child outcomes (Aaronson 2000). The CPS is designed to be representative of states in each year and, for each annual survey between 1977 and 2011, has harmonized data on state of respondent's residence. We begin by pooling all individual-level CPS observations of heads-of-household for the available years—a total of 2,209,453 observations. For each observation, we code homeownership (1 = *owns own home*, 0 = *does not own home*) and race/ethnicity (mutually exclusive dummy variables indicating non-Hispanic white, non-Hispanic Black, and non-Hispanic other). We next collapse the data by year, by state, and by race/ethnicity to construct state-year rates of homeownership for Blacks and for whites, weighting the averages using the CPS sample weights. We then calculate the Black-white homeownership gap as the Black rate as a share of the white rate: (homeownership Black/homeownership white).[11]

However, although the CPS is designed to be representative of states, the annual CPS estimates may be unstable for demographic sub-groups within states because of small sample sizes.[12,13] Consequently, Census recommends that, when making year-to-year comparisons within states, analysts use a two-year average by pooling across waves of the March CPS.[14] We construct this

measure by pooling individual respondents across two consecutive waves of the CPS (year i and year i + 1) and collapse the data as described above. For example, the 1985 Black homeownership rate for a state s is constructed by calculating the share of Black respondents in state s surveyed in the March 1985 and March 1986 CPS who owned their own homes. Combining the two years increases the number of respondents we can use to calculate the homeownership rate for each subgroup. Although our analysis is based on two-year pooled averages, we find that the results presented below are robust to using either single years or three-year pooled averages.

Additionally, we assessed the robustness of our results to using different individual-level data as the source of the homeownership measure: the 1980, 1990, and 2000 Censuses and the 2001-2005 American Community Surveys (ACS). Though these alternative sources cover many fewer state-years than the CPS, they include much larger samples within each state and, therefore, should be less susceptible to measurement error. A detailed description of the construction of these alternative measures, as well as the results we obtain using them, are available in Appendix B. In short, estimates are consistent in significance and direction, though are somewhat reduced in size.

Incarceration. The Bureau of Justice Statistics releases data on the number of prisoners under state or federal jurisdiction in each state at year's end.[15] We take that figure as the numerator, and divide by the total state population, to construct a measure of the state incarceration rate per 1,000 individuals. Pooling across states and years, the incarceration rate averages 3.7 per 1,000. To ensure temporal priority to homeownership and to allow time for incarceration-related home purchase and foreclosure processes to play out, we lag the incarceration rate by three years.

Control variables. We also construct measures of a number of time-varying state-level characteristics that might confound the relationship between incarceration and homeownership, including a set of economic variables (state minimum wage, state unemployment rate, per capita personal income, poverty rate), a set of variables capturing social welfare programs (maximum

AFDC/TANF and Food Stamps/SNAP benefit for a family of four and state EITC rate), a set of

housing market variables (home price index, average contract mortgage interest rate, ratio of

household income to home value), a measure of the violent crime rate, and a set of demographic

variables (percent of the state population that is Black, percent of the state population that is white,

and percent elderly).[16] Detailed descriptions of these measures, including means and standard

deviations, are available in Appendix C and in Appendix Table C1. We also lag these measures by

three years.

Analytic Strategy

Data on homeownership at the state level derived from the CPS micro-data is available from 1977

through 2011 and for all 50 states and Washington, D.C., which totals 1,785 state-year observations.

However, we limit the analytical sample based on two criteria, one temporal and one geographic.

First, the Great Recession was disruptive of homeownership patterns, especially for Blacks

(Bayer, Ferreira, and Ross 2013). To separate these recession dynamics from those related to

incarceration, we exclude state-year observations for the years 2006-2011. We also exclude state-year

observations for the years before 1985. We left truncate because of data limitations and substantive

reasons. With a three-year lag on the predictors, the data on homeownership and incarceration would

permit starting with imprisonment rates in 1974 to predict homeownership in 1977 (the first year of

the CPS data). But, the data on state minimum wage laws, EITC rates, and social welfare generosity is

not available until 1980. Therefore, it is possible to begin with predictors measured in 1980 and

homeownership measured in 1983.

However, there is a substantive argument for starting later. Spelman (2009:35) notes that

incarceration increased markedly from the low point in 1972 through 2005, but that "the 1977 to 2005

period accounts for 92% of the total rate increase, and 93% of the total prisoner increase [and that]

the 1982-2005 period accounts for 80% of the rate increase and 84% of the prisoner increase."

Therefore, we focus on this most concentrated period of mass incarceration from 1982-2005 (that is, incarceration first measured in 1982 estimating homeownership in 1985), which gives us good coverage on the covariates and allows for most of the "prison boom." This creates a somewhat longer window than the 1990-2003 period employed by Wildeman (2012) in his state-level analysis of incarceration and infant mortality, but these years are justified by data availability and context. This exclusion criteria results in an analysis sample of 1,071 state-year observations. The results described below are robust to the use of alternative year ranges with the estimates varying little whether the ranges used are 1985-2005 or 1983-2010 (the earliest start and latest end), 1983-2005 (the earliest start and preferred end), or 1985-2010 (the preferred start and latest end).

Second, we exclude an additional 189 state-year observations for the nine states (ID, ME, MT, NH, ND, SD, UT, VT, WY) where less than 2% of the population was Black in 2010. Besides having small numbers of Black CPS respondents, these states are less substantively important than other states for examining the Black-white homeownership gap. We are also missing data on the imprisonment rate for two state-year observations, which leaves a final analysis sample of 880 state-year observations covering 42 states over 21 years. There is no other missing data.

We pool the 880 state-year observations and estimate OLS regression models of the relationship between incarceration and (1) the state-level rates of homeownership for whites, (2) the state-level rates of homeownership for Blacks, and (3) the Black-white homeownership gap, weighting by average state population between 1985 and 2005. In addition to the time-varying controls discussed above, the models also include state- and year-fixed effects. The results were also robust to including a state linear time trend (calculated as year × state). Tests suggest that the errors follow a first order autoregressive process (AR(1)). We present models that adjust for the AR(1) serial correlation, though the results were robust to using a basic fixed-effects specification.[17] Our analytic

approach is similar to that of Wildeman's (2012) analysis of state-level incarceration and infant mortality.

RESULTS

Our results section proceeds in two stages. In the first stage, we document our findings from the individual-level Fragile Families data, which allows us to estimate the relationship between incarceration and asset ownership. We first describe the average effect of incarceration on home, car, and bank account ownership among men and their romantic partners, and discuss a series of related supplemental analyses. We then show how the association between incarceration and asset ownership may vary by co-residential status of the parents and by prior asset ownership. In the second stage, we describe our findings from the state-level panel data documenting the association between state-level incarceration and state-level Black-white inequality in homeownership.

Fragile Families and Child Wellbeing Study

Descriptive Statistics

Descriptive statistics for all variables used in the analysis are presented in Appendix Table A1. These descriptive statistics show that 30% of fathers and 24% of mothers reported homeownership at the five-year survey. On average, 75% of fathers and 66% of mothers reported vehicle ownership, and 59% of both fathers and mothers reported bank account ownership. Incarceration in the past two years was common. About 15% of fathers in the fathers' analytic sample were recently incarcerated, and 18% of mothers in the mothers' analytic sample shared children with recently incarcerated fathers.

Demographic and socioeconomic characteristics suggest that the sample is relatively disadvantaged. About half (48% of fathers and 49% of mothers) of parents are non-Hispanic Black

and about one-quarter (25% of fathers and 26% of mothers) are Hispanic. On average, fathers and

mothers were 31 and 28 years old, respectively, at the three-year survey. Nearly three-fifths of fathers

(57%) and more than half of mothers (53%) did not have education beyond high school. About 38%

of fathers and 6% of mothers reported incarceration prior to the three-year survey.

Differences in Fathers' and Mothers' Asset Ownership, by Recent Paternal Incarceration

Figure 1 shows striking differences in fathers' and mothers' asset ownership by recent paternal

incarceration. We turn first to fathers' assets. Only 6% of recently incarcerated fathers reported

homeownership at the five-year survey, compared to 34% of fathers not recently incarcerated.

Recently incarcerated fathers are about half as likely to report vehicle ownership (45% compared to

80%) and about three times less likely to report bank account ownership (22% compared to 65%).

The differences in mothers' assets, by fathers' incarceration, are equally striking. Mothers who share

children with recently incarcerated fathers, compared to their counterparts, are less likely to report

homeownership (5% compared to 29%), vehicle ownership (50% compared to 70%), and bank

account ownership (44% compared to 62%). All differences are statistically significant ($p < .001$).

[Figure 1 about here.]

Estimating Fathers' and Mothers Asset Ownership

These descriptive differences in asset ownership by recent paternal incarceration are striking and,

beginning in Table 1, we present a series of logistic regression and propensity score models that take

into account possible threats to causal inference. In this and subsequent tables, we present

parsimonious tables that include only the coefficients of interest, but full models are available upon

request. We turn first to Panel A, which estimates asset ownership among fathers. In Model 1, which

adjusts for a wide array of demographic, socioeconomic, and behavioral characteristics, recent

incarceration is associated with a lower likelihood of home (b= -0.583, $p < .05$), vehicle (b = -0.517, $p < .001$), and bank account (b = -0.652, $p < .001$) ownership.

[Table 1 about here.]

The association between incarceration and homeownership, though still moderate in magnitude, falls from statistical significance when adjusting for prior homeownership (Model 2) and remains statistically insignificant when restricting the sample to previously incarcerated fathers (Model 3).[18] But in Models 2 and 3, incarceration remains significantly associated with vehicle and bank account ownership. The most conservative logistic regression model, which adjusts for prior assets and limits the sample to previously incarcerated fathers, shows that recently incarcerated fathers have about half the odds of vehicle ownership (b = -0.603, $p < .01$; OR = 0.55) and three-fifths the odds of bank account ownership (b = -0.525, $p < .05$; OR = 0.59) than those without recent incarceration. Finally, in Model 4, we present results from propensity score models. These estimates show that incarcerated men, compared to their matched counterparts, are less likely to own a vehicle and a bank account ($p < .001$). Taken together, the findings in Panel A suggest that, among fathers, recent incarceration has deleterious consequences for vehicle and bank account ownership but that the association between incarceration and homeownership results from social selection forces.

Panel B estimates asset ownership among mothers. Model 1, which adjusts for an extended set of covariates, shows that mothers who share children with recently incarcerated men, compared to their counterparts, have a lower likelihood of home (b = -0.847, $p < .001$) and vehicle ownership (b = -0.289, $p < .01$). There is no statistically significant association between recent paternal incarceration and mothers' bank account ownership (b = -0.140, n.s.). Recent paternal incarceration continues to be associated with lower odds of home and vehicle ownership in the following two models. In Model 3, the most conservative model, those experiencing recent incarceration have only half the odds of homeownership (b = -0.689, $p < .05$; OR = 0.50) and less than three-quarters the odds of vehicle

ownership (b = -0.369, p < .01; OR = 0.69) as those not recently incarcerated. The propensity score

estimates presented in Model 4 are of a similar magnitude and significance. See Figures 3 and 4 for

predicted probabilities of home, vehicle, and bank account ownership for mothers and fathers.

[Figure 3 about here.]

[Figure 4 about here.]

Supplemental analyses. The analyses in Table 1 show that incarceration is robustly associated with

asset ownership among both recently incarcerated men and the women with whom they share

children. In supplemental analyses, we further unpack this relationship by considering incarceration

duration and offense type. First, we consider the potentially differential consequences of incarceration

duration for fathers' and mothers' assets, with duration measured as a series of mutually exclusive

dummy variables: less than three months (5% in the fathers' analytic sample), three months or longer

(7%), duration missing (3%), not recently incarcerated (85%). We find no evidence that incarceration

lasting less than three months, compared to incarceration lasting three months or longer, is any less

consequential for fathers' home or vehicle ownership, as the coefficients are not statistically different

from one another (in the equivalent of Model 2 of Table 1, p = .274 for homeownership and p = .509

for vehicle ownership). There is some evidence, though, that incarceration lasting three months or

longer is more negatively associated with fathers' bank account ownership than incarceration lasting

less than three months (p = .006). With respect to mothers' assets, we find no statistically significant

differences between fathers' shorter and longer incarcerations (p = .801 for homeownership, p = .849

for vehicle ownership, and p = .860 for bank account ownership, in the equivalent of Model 2 of

Table 1).

Second, we consider the potentially differential consequences of offense type, also measured

as a series of mutually exclusive dummy variables: violent offense (3% in the fathers' analytic sample),

non-violent offense (6%), offense type missing (5%), and no recent paternal incarceration (85%).

Across all models and outcomes, we find no evidence that incarceration for violent and non-violent offenses are differentially associated with fathers' (p = .396, .322, and .603, for home, vehicle, and bank account ownership, in the equivalent of Model 2 of Table 1) or mothers' (p = .916, .663, and .223, respectively) assets. These lacking differential effects provide some evidence against threats to causal inference, as we expect unmeasured characteristics are correlated with offense severity.

Estimating Fathers' and Mothers' Asset Ownership, by Co-residence

Table 2 estimates fathers' and mothers' asset ownership separately by parents' residential status at the three-year survey (prior to the measure of recent incarceration). Turning first to Panel A, which estimates fathers' assets, we find that incarceration is similarly consequential for fathers co-residing and not co-residing with mothers at the three-year survey. Consistent with the results presented in Table 1, recent incarceration is associated with a lower likelihood of vehicle and bank account ownership but is not significantly associated with homeownership. These similar associations between incarceration and fathers' assets, regardless of co-residence, are further supported by z-scores that test for statistically significant differences between the two coefficients.

[Table 2 about here.]

Panel B shows striking differences for mothers' asset ownership by co-residential status, with associations between recent paternal incarceration and asset ownership concentrated among mothers living with fathers at the three-year survey. Among mothers living with fathers at the three-year survey, incarceration is associated with a lower likelihood of homeownership (b = -1.468, p < .001), vehicle ownership (b = -0.780, p < .001), and bank account ownership (b = -0.269, p < .10). There are no statistically significant associations between incarceration and asset ownership among mothers not living with fathers prior to incarceration. Z-scores confirm the subgroup differences for home and bank account ownership are statistically different from one another.

Estimating Fathers' and Mothers' Asset Ownership, by Prior Asset Ownership

Finally, in Table 3, we examine how the association between incarceration and asset ownership varies by prior asset ownership. Estimates of fathers' outcomes, presented in Panel A, show that, for all three outcomes considered, the size of the incarceration coefficient is larger for fathers who reported home, vehicle, and bank account ownership at the three-year survey. Z-scores indicate that, with respect to vehicle ownership, incarceration is more strongly associated with a loss in assets than an inability to acquire new assets. Panel B, which estimates mothers' outcomes, also shows that the magnitude of the incarceration coefficients are larger for mothers with prior assets, but these z-scores show these coefficients are not statistically different from one another, suggesting that mothers experience both asset loss and challenges to acquiring new assets.

[Table 3 about here.]

State-Level Data

Descriptive Statistics

We now turn to the state-level data to examine the association between incarceration rates and Black-white inequality in homeownership. Figure 2 presents a box plot of the Black-white homeownership gap by year, using the simple difference as a measure of the homeownership gap. The dark horizontal band marks the median for each year. The top and bottom edges of each rectangle mark out the 75th and 25th percentiles. Pooling across all state-year observations in the analytic sample, an average of 42% of Blacks and 71% of whites own their own homes, producing a mean Black-white homeownership gap of 29 percentage points and a median of 28 percentage points.

[Figure 2 about here.]

Multivariate Analyses

Results from the multivariate analysis are presented in Table 4. Model 1 presents results from the OLS

models with state- and year-fixed effects, the full set of controls, and the AR(1) correction. This

model estimates the effect of incarceration on the Black-white homeownership gap, now calculated

using our preferred measure (Black homeownership rate / white homeownership rate).[19] This model

shows a negative relationship between the state incarceration rate and the Black homeownership rate

as a share of the white homeownership rate (b = -0.023; p < .01). That is, higher incarceration rates

are associated with larger homeownership gaps between whites and Blacks. Predicted values from

Model 1 suggest that the Black homeownership rate is 62% of the white rate when the incarceration

rate is 1/1,000 (the 10th percentile of the distribution of incarceration rates) to 51% the white rate

when the incarceration rate is 6/1,000 (the 90th percentile of the distribution of incarceration rates), a

range of approximately 10 percentage points representing 17% of the mean Black-white gap in

homeownership or two-thirds of a standard deviation.

<center>[Table 4 about here.]</center>

Models 2 and 3 estimate the Black homeownership rate and the white homeownership rate,

respectively, as a function of incarceration. There is a significant negative effect on the Black

homeownership rate (b = -0.014; p < .05), but no significant effect on the white homeownership rate,

indicating that the effect of incarceration on the Black-white homeownership gap arises primarily by

depressing rates of Black homeownership.[20]

Robustness Checks

We previously noted that these results are robust to alternative constructions of state-level

homeownership from the CPS micro-data, to the use of Census/ACS data in place of CPS data, and

to an alternative measure of the Black-white homeownership gap. We also noted that the choice of

analysis years does not substantively affect the results nor does the implementation of the AR(1) correction or the inclusion of a state linear time trend.

We also conduct additional robustness checks. First, we substitute our independent variable with the log of the imprisonment rate per thousand, which produces a similarly positive and significant effect on the Black-white homeownership gap (standardized b = -0.374, p < .01) as our preferred measure (standardized b = -0.227, p < .01). We next exclude the 52 state-year observations with Black-white homeownership gaps in the bottom 3% (Black-homeownership rates less than 25% of white rates) and the top 3% (Black homeownership rates greater than 85% of white rates) of the distribution. This analysis, though essentially selecting on the dependent variable, provides a useful check on the robustness of the results to the exclusion of outliers. The results show that these observations do not substantively affect the results (b = -0.021; p < .01). Third, we test the robustness of the results to using the male incarceration rate—the number of men incarcerated at year's end divided by the number of men in the state. The latter number, the denominator, is derived from intercensal estimates of state population. The results are again substantively similar, with the standardized coefficient on the total incarceration rate equal to -0.227 (p < .01) and the standardized coefficient on the male incarceration equal to -0.243 (p < .05). Finally, including four, three, and two year lagged measures of the incarceration rate all together in the model and testing for joint significance also shows the expected negative and significant result.

These analyses represent a fairly conservative test of the effect of incarceration on home-ownership. Though it is possible that unobservable characteristics could render the relationship spurious, we show the results are not explained by a set of reasonable confounders, withstand adjustment to time invariant state-level characteristics and to year-specific characteristics common across states, and are robust to a large set of alternative modeling choices. To subject our results to further rigor, we also conduct a falsification test that assesses if incarceration, measured *after* the

homeownership gap, has an effect on the dependent variable. Here, we expect to find no relationship and the presence of one might indicate reverse causality or spuriousness. In support of our results, we find no relationship between incarceration measured at $t + 3$ (or $t + 2$) and the homeownership gap at time t or between incarceration measured at $t + 3$ (or $t + 2$) entered along with incarceration measured at $t - 3$ on the homeownership gap at time t.

DISCUSSION

The rising incarceration rate in the United States, as well as its unequal distribution across the population, may widen inequalities among individuals and families (Pettit and Western 2004; Wakefield and Uggen 2010). Indeed, in recent years, scholars have become increasingly attuned to how prison and jail experiences disadvantage the confined men, their family members, and their communities. Research documents profound negative effects of incarceration on measures of economic wellbeing including employment (Pager 2003), wages (Lyons and Pettit 2011), legal debt (Harris et al. 2010), and material hardship (Schwartz-Soicher et al. 2011). But, despite attention given to incarceration's economic consequences, as well as increasing sociological attention to processes related to household wealth inequality, prior research has neglected how incarceration affects household wealth and the implications of this for household wealth inequality.

In this manuscript, we use micro- and macro-level data to provide the first examination of the association between incarceration and household wealth. First, using rich longitudinal data from the Fragile Families and Child Wellbeing Study, we show that incarceration is significantly and detrimentally associated with asset ownership among men and the women who share children with them. Recently incarcerated men, compared to their counterparts, are less likely to own vehicles and hold back accounts, and these associations persist after adjusting for a wide array of covariates, a lagged dependent variable, and a sample restricted to men at risk of incarceration. These

disadvantages proliferate to the women connected to them, especially women living with them just prior to their incarceration, as they are less likely than their counterparts to own homes and vehicles.

Second, analysis of state-level panel data, spanning two decades, shows that incarceration rates depress rates of Black homeownership and, in doing so, widen the Black-white homeownership gap. Given the sheer scale of incarceration among Black men, the effects of incarceration on the Black-white homeownership gap may be brought about simply through the direct negative effects of incarceration on homeownership for men and their romantic partners. But we suspect that this ecological analysis also detects effects of incarceration on others connected to recently incarcerated men and on the communities in which these men reside. Indeed, the individual-level results suggest that there is only a homeownership penalty for romantic partners of incarcerated men and not for the incarcerated men themselves. This makes sense to the extent that homeownership, compared to vehicle or bank account ownership, is more difficult to achieve and, therefore, is already effectively out of reach for many formerly incarcerated men. But it also implies that the effects of incarceration reach a wider demographic than simply the formerly incarcerated, a result in accord with prior research on asset accumulation (O'Brien 2012).

Incarceration might reduce asset ownership and widen Black-white inequalities in household wealth for a number of reasons. Incarcerated men experience confinement that facilitates asset loss, as they may be unable to make mortgage or car payments and may have bank accounts involuntarily closed, and that has negative consequences for asset re-acquisition upon release. Further, once released, these formerly incarcerated men face challenging economic circumstances (Western 2006), avoid social institutions such as banks (Goffman 2009), and settle in disadvantaged neighborhoods that are lacking institutional supports (Massoglia et al. 2013), all of which may make asset accumulation difficult and asset loss effortless. Though the economic consequences of incarceration for romantic partners is less systematically documented (though see Schwartz-Soicher et al. 2011), it is

equally plausible that the mechanisms leading to asset reduction among men also lead to asset reduction among their romantic partners.

But any research on the effects of incarceration must consider the possibility that selection into prison or jail leads to spurious associations between incarceration and outcomes. Indeed, it is easy to imagine the numerous ways incarcerated men are more disadvantaged than their counterparts, and that these disadvantages—and not incarceration—result in a decline in asset ownership. We take extensive measures to guard against this possibility in our analysis. In our individual-level analysis, we first leverage the rich individual characteristics included in the Fragile Families data to control for attributes that might both increase incarceration and reduce asset ownership (including, but not limited to, substance use, domestic violence, cognitive ability, impulsivity, and prior incarceration). We also include a lagged indicator of asset ownership to estimate change in asset ownership, limit some estimates to previously incarcerated men, and consider propensity score models that assure closer comparisons between incarcerated men and their counterparts. Additional analyses provide some assurance that our results are not driven by selection. For example, the negative effects of incarceration on men do not vary by their pre-incarceration co-residence with the focal child's mother, as we would expect, and the effects for women are specific to those living with romantic partners prior to incarceration. Also, the effects of incarceration do not vary based on offense severity, which we expect is correlated with unobserved characteristics, furthering strengthening causal inference.

In our state-level analysis, we adjust for a full set of control variables that might confound the relationship between state-level incarceration and state-level homeownership including the generosity of state social welfare programs, housing market conditions and economic conditions, demographic composition, and crime. In addition, we include state fixed-effects to net out unobserved but potentially confounding state characteristics that are stable over time and year fixed-effects to net out

unobserved characteristics that vary over time but are found across states. Our results are robust to a wide range of modeling choices including selecting alternative year ranges, including a state-specific time trend, and calculating our dependent variable using Census/ACS data instead of CPS data.

Therefore, through an array of modeling strategies, our analysis homes in on the effects of incarceration on asset ownership. We are more limited in our ability to assess the precise mechanisms by which incarceration reduces asset ownership, but our analysis does contain some clues. In the individual-level data, the negative effects of men's incarceration are most pronounced for those who previously held assets. Incarceration is associated with a loss of assets—likely through repossession, foreclosure, account closure, or asset sale—but does not slow asset accumulation for those who have not recently owned a home, vehicle, or bank account. This result affirms the relevancy of processes of asset accumulation and loss even for this highly disadvantaged population. Relatedly, though the main state-level analysis is unable to differentiate between processes of asset loss and slowed accumulation, as we cannot break down foreclosure by the mortgage holder's race, supplemental analyses provide some supporting evidence. The relationship between state-level incarceration and overall foreclosure is positive, though not significant, pointing in the direction of asset loss. Future research should continue to investigate the consequences of incarceration for wealth and, to the extent possible, the mechanisms (e.g., legal debt, stigma and discrimination, neighborhood conditions) underlying this association.

Limitations

Several important limitations exist. The Fragile Families data is comprised of a sample of parents in urban areas who gave birth between 1998 and 2000. The focus on parents is both a virtue and a limitation of these data. Parents, especially the unmarried parents included in the data, are a population of key policy interest as any effects of incarceration on parents may have spillover effects

on their children (Wakefield and Wildeman 2013). Indeed, research on the collateral consequences of incarceration for children speculate that economic insecurity following incarceration may be one mechanism through which paternal incarceration exerts deleterious effects on children (Geller et al. 2012), and a separate body of research suggests that household wealth is a strong predictor of children's outcomes (Orr 2003). However, these data do not allow us to consider if the effects of incarceration on household wealth extend to non-parents. But our state-level analysis includes both parents and non-parents and, in those analyses, we do find average negative effects on homeownership—effects that were absent, in the individual-level analyses, for the recently incarcerated men themselves.

Few nationally representative longitudinal surveys contain reliable and complete data on incarceration. The Fragile Families study is one data source that includes detailed information on incarceration. But the measures of incarceration are limited. For one, it is impossible to distinguish between jail and prison incarceration. Prison stays, which are usually lengthier and located farther from prisoner's families than jail stays, may be more detrimental to household wealth. But we expect that jail is also quite disruptive of asset holding and acquisition, and our comparison of shorter and longer confinements indirectly supports that jail (as proxied by short stays) is harmful. Additionally, though incarceration is measured precisely at the five-year survey, capturing any incarceration that occurred since the prior survey, which is why our key explanatory variable measures incarceration during this time, incarceration is not measured precisely at all survey waves. Work to date has documented the consequences of incarceration using data sources not designed to study the effects of incarceration, but researchers must develop surveys—potentially linked up to administrative data— that are specifically designed to study the effects of incarceration.

Finally, our two-part empirical approach allows us to estimate the individual-level effects of men's incarceration on their own and their romantic partners' assets and to estimate how such

dynamics aggregate to produce racial wealth inequality in homeownership in the United States. However, the correspondence between these two analyses is imperfect. The Fragile Families data includes measures of incarceration that make it impossible to distinguish between prison and jail, as described above, and the state-level data, by and large, considers only imprisonment. Further, the individual-level analysis allows us to consider the spillover effects of incarceration on romantic partners, but not spillover effects on extended family members or communities. In contrast, the incarceration effects found in the state-level analysis could be produced by effects on the formerly incarcerated, their family members, and their communities more broadly. Lastly, although we examine home, vehicle, and bank account ownership in the Fragile Families data, the CPS data, which we use in our state-level analysis, only includes information on homeownership. Further, the data on asset ownership in the Fragile Families is not ideal in that it imprecisely measures the value of all assets and debts and asks only asks about the ownership of a few assets. Surveys such as the Survey of Consumer Finances and the Health and Retirement Study contain far more detailed data on personal wealth that allows for the calculation of total net worth, of liquid financial savings, and of ownership of a large set of assets and debts. However, such surveys contain no data on incarceration.

Conclusion

By considering the link between incarceration and household wealth, our analysis adds to two growing and distinct literatures, one on the collateral consequences of incarceration and another on the social processes associated with wealth. We document that incarceration has substantial effects on household wealth for formerly incarcerated men and their romantic partners, and that these individual-level effects add up to racial inequality in homeownership at the state level. Given that both the individual- and state-level effects remain robust across a wide array of analytic strategies and attention to social selection processes, and that the individual-level effects are apparent regardless of

offense type and even for relatively short incarceration experiences, brings the substantial costs of

mass incarceration into sharp focus. These effects are likely to feed cycles of cumulative disadvantage,

whereas incarceration reduces wealth among already disadvantaged young men and their families and

this wealth poverty, in turn, further disadvantages the children of formerly incarcerated men.

ENDNOTES

[1] Though not all mothers and, especially, fathers participate in all survey waves, the attrition levels of

these data are lower than the attrition levels among cohabiting women and their partners in other

nationally representative samples (e.g., Sassler and McNally 2003).

[2] Another strategy would be to use the same analytic sample for estimates of fathers' and mothers'

assets. In supplemental analyses, we estimated mothers' outcomes with the first analytic sample and

found that results were substantively similar to those using the second analytic sample. We use two

different analytic samples to maximize the number of observations used.

[3] Parents who reported homeownership were also asked to report whose name is on the mortgage. In

the majority of cases (78% of mothers and 84% of fathers at the five-year survey), respondents who

reported homeownership also reported that their name is on the mortgage. But in some cases, the

respondents reported homeownership and that the mortgage was instead in solely their partner's

name or another family member's name. As mortgage information was only asked of respondents in

18 of the 20 cities, we do not consider this additional information in the analyses presented. But, in

supplemental analyses not presented, we consider two auxiliary measures of homeownership: (1)

respondent reports homeownership and reports his/her name is on the mortgage and (2) respondent

reports homeownership and reports his/her name is on the mortgage (if unmarried) or his/her name

or spouse's name is on the mortgage (if married). These two alternative measurement strategies

produced substantively similar results.

[4] Parents were also asked to report how much money they could sell their house for at the time of the interview. We do not use this information in the analyses presented, as there is likely substantial reporting error and a large amount of missing data. In supplemental analyses, though, we estimated home value (log) and vehicle value (log) for respondents who reported owning these assets. The coefficient for recent paternal incarceration falls to statistical insignificance in models adjusting for covariates, but the relationship is in the expected direction.

[5] For respondents with romantic partners, it is impossible to distinguish whether they, their partner, or both individuals own the car, truck, or van.

[6] Very few mothers were incarcerated between the three- and five-year surveys, making it not possible to conduct comparable or rigorous analyses of the relationship between maternal incarceration and asset ownership.

[7] In the fathers' analytic sample, 18% are missing data on duration and 35% are missing data on offense type. Fathers convicted for both non-violent and violent offenses are considered violent offenders.

[8] Fixed-effects models would be an ideal method to estimate how change in incarceration is associated with change in asset ownership, as these models diminish unobserved heterogeneity by accounting for all time-invariant individual-level characteristics. Unfortunately, because few parents experience changes in both incarceration and asset ownership between waves, it is not possible to precisely estimate these models.

[9] We use probit regression models, and all covariates from Model 2, to generate a propensity score for each observation.

[10] It is inadvisable to compare across logistic regression models (Mood 2010). Therefore, in supplemental analyses, we used linear probability models to estimate the subgroup analyses presented in Tables 2 and 3. These analyses produced coefficients that were comparable to the average marginal

effects from the logistic regression models. We also tested for statistically significant group differences with the linear probability models, which also produced similar findings.

[11] The results are also substantively similar if we instead simply calculate the gap as the difference between the white and Black homeownership rates (homeownership white - homeownership Black).

[12] See http://www.census.gov/cps/about/faq.html.

[13] For states in our analysis sample (described below), there were an average of about 250 Black respondents in the paired years between 1985 and 2000 and about 450 Black respondents in the paired years from 2001-2005 (after the CPS increased their sample size). The number of white respondents is much larger, averaging approximately 1,500 per paired state-years before 2001 and 2,200 after 2001.

[14] See http://www.census.gov/hhes/www/poverty/about/datasources/description.html.

[15] This figure includes the relatively small fraction of prisoners held in private facilities (6.5% in 2003). In Alaska, Connecticut, Delaware, Hawaii, Rhode Island, and Vermont, this figure includes both prison and jail inmates. Prisoners under the legal authority or jurisdiction of one state but who are housed in another state's prisons are attributed to the state of jurisdiction.

[16] Measures of the punitiveness of the state criminal justice system, such as the presence of "three strikes" or "truth in sentencing" laws, are also available. However, adjusting for these measures risks over-controlling in that the presence of such laws is likely one pathway by which variation in imprisonment rates is generated. Measures of the partisan identification of state governors and legislatures may capture some of these effects indirectly as well as other potentially confounding processes. We tested the robustness of our models to include a dummy indicator for the state governor being democratic and continuous measures of the percent of the upper and lower bodies of the legislature that were democratic. The results are unaffected by the inclusion of these measures, but including them necessitates dropping observations for Washington, DC (which has no governor) and

for Nebraska (which is unicameral and non-partisan). Consequently, to preserve sample size, we do not include these political variables in the main analysis.

[17] We also tested whether the series on the Black-white homeownership gap and the incarceration rate were stationary in the 42 state panels using Fisher type unit root tests based on augmented Dickey-Fuller tests. The tests reject the null hypothesis that all panels contain a unit root. Further, estimates from a first differences model, commonly employed when dealing with non-stationary series, with state and year fixed-effects returns very similar results to the preferred model.

[18] Given that homeownership becomes more attainable with age, it is possible that these null effects result from the relatively young age of these fathers (who are, on average, 31 years old at the three-year survey). These null results persist in supplemental analyses where we restrict the sample to fathers older than 30 (n = 1,268) and fathers older than 35 (n = 682).

[19] The table shows 838 observations because the AR(1) corrected models drop the first year's worth of observations (n = 42).

[20] This effect could be attributable to fewer purchases, more foreclosures, or both. We attempted to gain insight into these mechanisms by directly examining the relationship between incarceration and foreclosure. We constructed a state-year measure of foreclosure starts based on data from the Mortgage Bankers Association (MBA) National Delinquency Survey on the percentage of residential mortgages that began the foreclosure process during a given year. On average, 1.3% of mortgages began the foreclosure process each year. We then estimated the state- and year-fixed effects models with the same controls as above and with the AR(1) correction, weighting by state population, and lagging the incarceration rate and the covariates by three years. The coefficient is in the expected direction, but the incarceration rate is not significantly associated with the foreclosure start rate ($p = .442$). In the period 1990-2005, the coefficients are also in the expected direction and here the relationship with the male incarceration rate is marginally significant ($p < .10$).

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TABLES AND FIGURES

Table 1. Estimating Fathers' and Mothers' Assets as a Function of Recent Paternal Incarceration

	Logistic regression models			Propensity score models
	Model 1	Model 2	Model 3	Model 4
	+ extended controls	*+ lagged DV*	*previously incarcerated*	*kernel matching*
Panel A. Fathers' Assets				
Homeownership	-0.583 *	-0.411	-0.545	-0.417
	(0.271)	(0.487)	(0.554)	(0.344)
Vehicle ownership	-0.517 ***	-0.799 ***	-0.603 **	-0.765 ***
	(0.140)	(0.175)	(0.209)	(0.165)
Bank account	-0.652 **	-0.662 **	-0.525 *	-0.880 ***
	(0.197)	(0.230)	(0.226)	(0.171)
Observations	2,703	2,703	1,014	2,703
Panel B. Mothers' Assets				
Homeownership	-0.847 ***	-0.865 ***	-0.689 *	-0.843 **
	(0.187)	(0.189)	(0.330)	(0.298)
Vehicle ownership	-0.289 **	-0.319 **	-0.369 **	-0.797 ***
	(0.100)	(0.115)	(0.125)	(0.165)
Bank account	-0.140	-0.022	-0.038	-0.068
	(0.108)	(0.123)	(0.127)	(0.120)
Observations	3,831	3,831	1,606	3,831

Note: Unless otherwise noted, estimates of fathers' assets control for fathers' characteristics and estimates of mothers' assets control for mothers' characteristics. All models estimating fathers' assets controls for current incarceration. Model 1 controls for race, mother and father are a mixed-race couple, immigrant status, age, lived with both parents at age 15, relationship status with child's other parent, relationship with a new partner, employment, education, income-to-poverty ratio, depression, fair/poor health, substance use, mother incarcercation, mother impulsivity, cognitive ability, father substance abuse, father domestic violence, father impulsivity, father prior incarceration. Model 2 includes all variables from Model 1 and a lagged dependent variable. Model 3 includes all variables from Model 2 and restricts the sample to previously incarcerated fathers (and mothers who share children with previously incarcerated fathers). Robust standard errors are in parentheses. Propensity score models for fathers include 395 treated cases and 2,308 untreated cases; for mothers, they include 676 treated cases and 3,155 untreated cases. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 2. Estimating Fathers' and Mothers' Assets as a Function of Recent Paternal Incarceration, by Co-residence

| | Model 1 | Model 2 | |
	Co-residential at 3-year survey	Not co-residential at 3-year survey	z-score
Panel A. Fathers' Assets			
Homeownership	-0.759	0.156	-1.553
	(0.532)	(0.639)	
Vehicle ownership	-0.648 **	-0.935 ***	0.818
	(0.243)	(0.252)	
Bank account	-0.589	-0.700 *	0.229
	(0.393)	(0.285)	
Observations	1,708	995	
Panel B. Mothers' Assets			
Homeownership	-1.468 ***	-0.403	-1.947 ^
	(0.392)	(0.381)	
Vehicle ownership	-0.780 ***	-0.146	-2.546 *
	(0.204)	(0.142)	
Bank account	-0.269 ^	-0.039	-1.144
	(0.137)	(0.157)	
Observations	1,975	1,856	

Note: All models control for race, mother and father are a mixed-race couple, immigrant status, age, lived with both parents at age 15, relationship status with child's other parent, relationship with a new partner, employment, education, income-to-poverty ratio, depression, fair/poor health, substance use, mother incarceration, mother impulsivity, cognitive ability, father substance abuse, father domestic violence, father impulsivity, father prior incarceration, and a lagged dependent varible. All analyses include robust standard errors (in parentheses) to account for the clustering of individuals in cities. In supplemental analyses, we used linear probability models to estimate the subgroup analyses, which produced coefficients that were comparable to the average marginal effects from the logistic regression models. The z-score represents the difference between the two groups. ^ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 3. Estimating Fathers' and Mothers' Assets as a Function of Recent Paternal Incarceration, by Prior Assets

|	Model 1 Assets at 3-year survey		Model 2 No assets at 3-year survey		z-score	
Panel A. Fathers' Assets						
Homeownership	-1.125	^	-0.144		-1.292	
	(0.635)		(0.416)			
Vehicle ownership	-1.200	***	-0.018		-4.007	***
	(0.170)		(0.241)			
Bank account	-0.987	**	-0.473		-1.179	
	(0.325)		(0.289)			
Observations	Varies		Varies			
Panel B. Mothers' Assets						
Homeownership	-1.266	**	-0.724		-1.044	
	(0.443)		(0.271)			
Vehicle ownership	-0.381	*	-0.298		-0.318	
	(0.168)		(0.201)			
Bank account	-0.072		0.015		-0.295	
	(0.171)		(0.179)			
Observations	Varies		Varies			

Note: All models control for race, mother and father are a mixed-race couple, immigrant status, age, lived with both parents at age 15, relationship status with child's other parent, relationship with a new partner, employment, education, income-to-poverty ratio, depression, fair/poor health, substance use, mother incarceration, mother impulsivity, cognitive ability, father substance abuse, father domestic violence, father impulsivity, and father prior incarceration. All analyses include robust standard errors (in parentheses) to account for the clustering of individuals in cities. For fathers with assets at the 3-year survey, N = 736 for homeownership, 1,956 for car ownership, and 1,544 for bank account ownership. For fathers without assets at the 3-year survey, N = 1,964 for homeownership, 734 for car ownership, and 1,137 for bank account ownership. For mothers with assets at the 3-year survey, N = 823 for homeownership, 2,424 for car ownership, and 2,069 for bank account ownership. For mothers without assets at the 3-year survey, N = 3,006 for homeownership, 1,396 for car ownership, and 1,744 for bank account ownership. In supplemental analyses, we used linear probability models to estimate the subgroup analyses, which produced coefficients that were comparable to the average marginal effects from the logistic regression models. The z-score represents the difference between the two groups. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 4. Black-White Homeownership Gap, Black Homeownership Rate, and White Homeownership Rate as a Function of State Incarceration

	Model 1	Model 2	Model 3
	Black-white gap	*Black rate*	*White rate*
Prisoners per thousand (lagged three years)	-0.023 **	-0.014 *	0.002
	(0.009)	(0.006)	(0.002)
Constant	0.706	0.624	0.877
Observations	838	838	838
Year fixed-effects	Yes	Yes	Yes
State fixed-effects	Yes	Yes	Yes
State controls	Yes	Yes	Yes
AR(1) correction	Yes	Yes	Yes

Note: In addition to the year and state fixed-effects, each model includes controls for the state minimum wage, the state unemployment rate, per capita personal income, the povety rate, the maximum combined AFDC/TANF Food Stamps benefit for a family of four, the state EITC rate, the home price index value, the average contract mortgage interest rate, the ratio of median household income to home value, the violent crime rate, the percent black, the percent white, and the percent elderly. * $p < .05$, ** $p < .01$.

Figure 1. Fathers' and Mothers' Assets, by Recent Paternal Incarceration
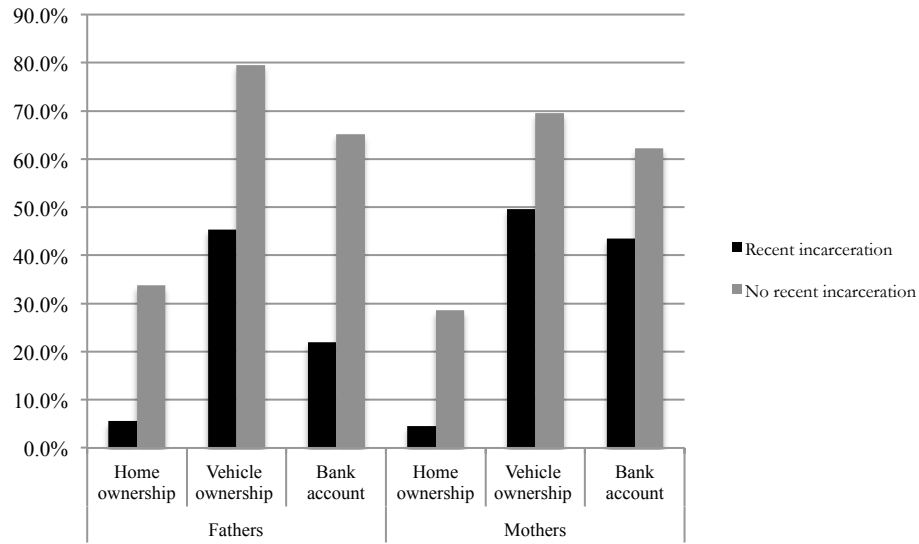
Note: Data from Fragile Families and Child Wellbeing Study. Among fathers, 395 fathers were recently incarcerated and 2,308 fathers were not recently incarcerated. Among mothers, 676 share children with a recently incarcerated father and 3,155 share children with a not recently incarcerated father. All differences between recent incarceration and no recent incarceration are statistically significant ($p < .001$).

Figure 2. Box Plot of Black-White Homeownership Gap (1985-2005)

excludes outside values
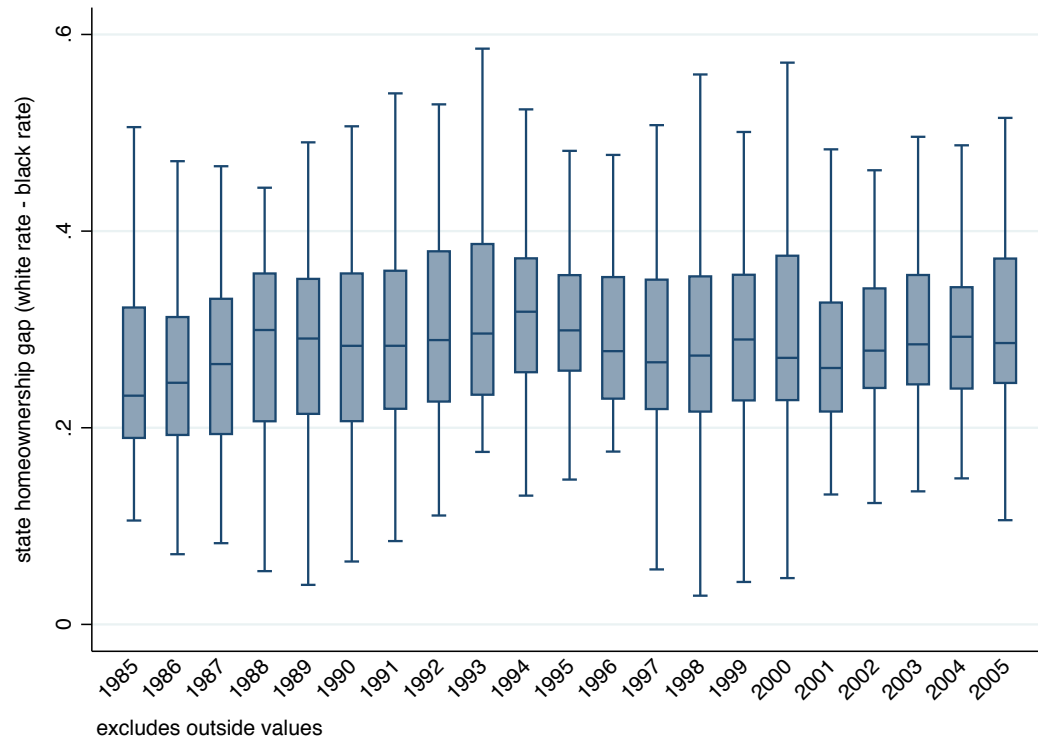
Figure 3. Predicted Probabilities of Fathers' Asset Ownership
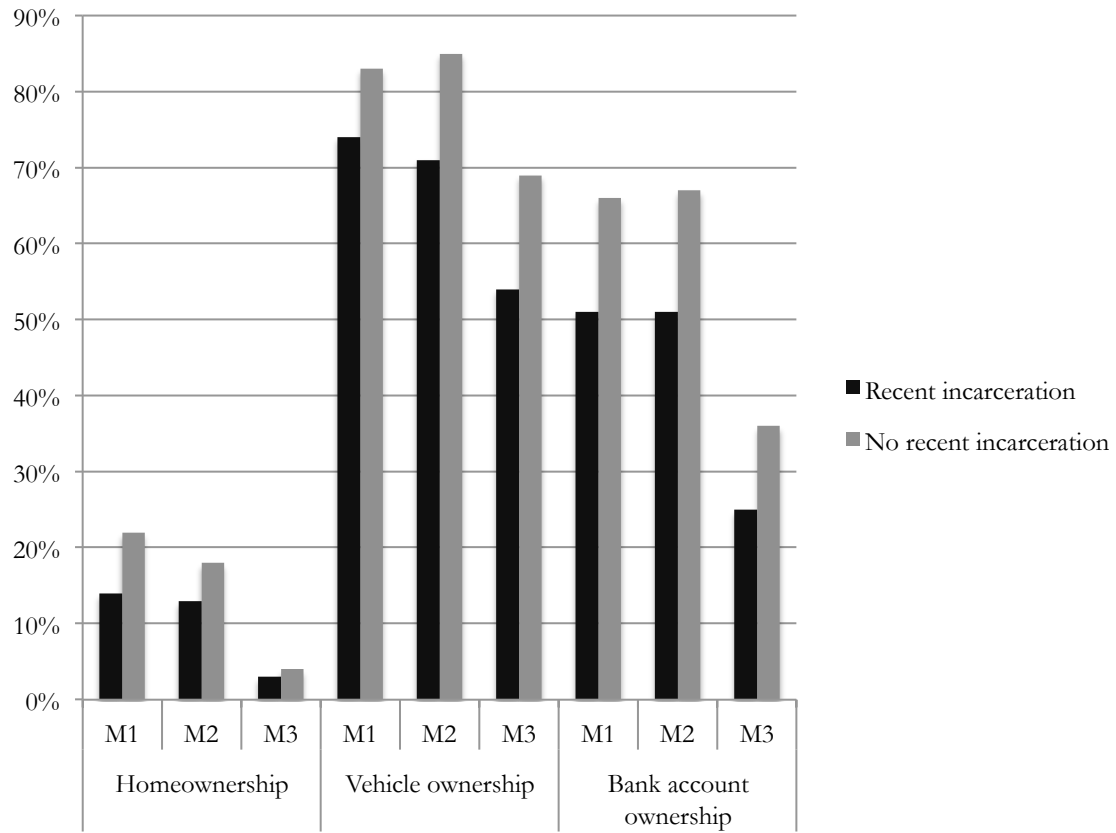
Note: Predicted probabilities based on the first imputed data set. M1, M2, and M3 correspond to Models 1, 2, and 3 of Table 1, respectively. All values held constant at their mean.
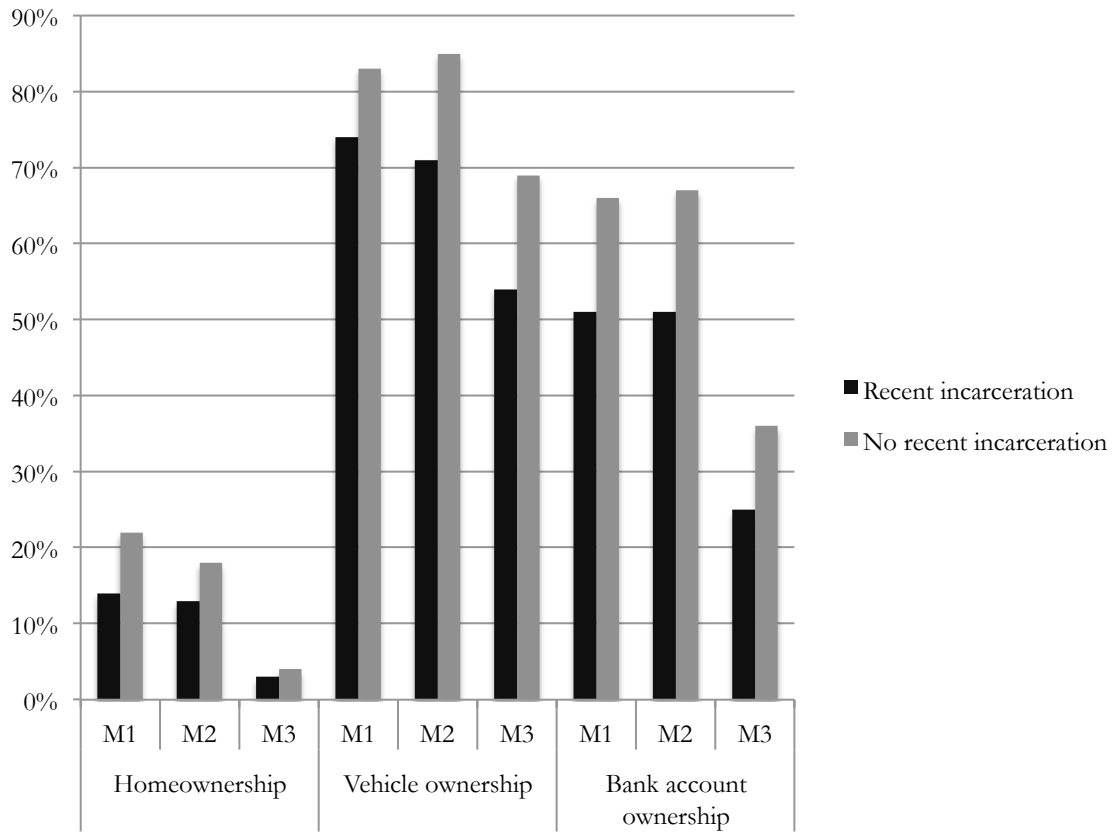
Figure 4. Predicted Probabilities of Mothers' Asset Ownership

Note: Predicted probabilities based on the first imputed data set. M1, M2, and M3 correspond to Models 1, 2, and 3 of Table 1, respectively. All values held constant at their mean.

APPENDICES

Appendix A1. Means and Standard Deviations of All Variables in Fragile Families Analysis

	Fathers (N = 2,703)		Mothers (N = 3,831)	
	Mean	S.D.	Mean	S.D.
Home ownership (y5)	29.7%		24.4%	
Vehicle ownership (y5)	74.5%		66.0%	
Bank account (y5)	58.9%		58.9%	
Current incarceration	6.0%		---	
Recent paternal incarceration	14.6%		---	
Race/ethnicity (b)				
Non-Hispanic White	22.1%		21.6%	
Non-Hispanic Black	48.1%		49.0%	
Hispanic	25.4%		25.9%	
Non-Hispanic other race	4.4%		3.5%	
Mixed-race couple (b)	14.0%		14.0%	
Immigrant (b)	15.5%		14.4%	
Age (y3)	31.162	(7.312)	28.159	(6.047)
Lived with both parents at age 15 (b)	46.5%		42.9%	
Relationship status with other parent (y3)				
Married	40.1%		32.1%	
Cohabiting	23.5%		19.6%	
Non-residential romantic	6.0%		5.7%	
Separated	30.4%		42.7%	
Repartnered (y3)	13.4%		18.5%	
Number of children in household (y3)	1.580	(1.421)	2.309	(1.340)
Employed (y3)	79.6%		56.6%	
Education (y3)				
Less than high school diploma	27.5%		28.1%	
High school degree or GED	29.8%		24.8%	
Post-secondary education	42.7%		47.1%	
Income-to-poverty ratio (y3)	2.834	(3.405)	1.953	(2.492)
Depression (y3)	14.2%		20.8%	
Fair or poor health (y3)	8.2%		13.3%	
Mother substance abuse (y3)	---		9.8%	
Mother ever incarcerated (b, y1, y3)	---		6.3%	
Impulsivity (y1 for fathers, y5 for mothers)	1.999	(0.674)	1.526	(0.483)
Father abused substances (b, y1, y3)	15.8%		---	
Father engaged in domestic violence (b, y1, y3)	11.4%		---	
Father prior incarceration (b, y1, y3)	37.5%		---	
Cognitive ability (y3)	6.639	(2.726)	6.765	(2.644)
Home ownership (y3)	27.3%		21.5%	
Vehicle ownership (y3)	72.6%		63.4%	
Bank account (y3)	57.3%		54.2%	

Notes: b = measured at baseline interview, y1 = measured at one-year telephone interview, y3 = measured at three-year telephone interview, y5 = measured at five-year telephone interview.

Appendix B: Alternative Estimates from the Census/ACS

We constructed an alternative measure of Black and white homeownership and of the Black-white gap using data from the 1980, 1990, and 2000 Census and from the 2001 – 2005 American Community Surveys (ACS). The Census and the ACS are large sample surveys designed to be representative of the states and for sub-groups within the states. Whereas the average number of Black respondents in the CPS sample for each state paired year was 250 (through 2001) and 450 (after 2001), the average number of Black respondents in each state in the analysis sample is nearly 10,000 in Census years and 1,000 in ACS years.

We employed these data to construct parallel measures of homeownership to those we created from the CPS. Appendix Table B1 compares the year-specific means and standard deviations of the Black-white homeownership gap (Black homeownership rate / white homeownership rate) for the years in which there is both CPS data and Census/ACS data. The standard deviations are general smaller with the Census/ACS data than the CPS data, the means are quite close across both measures, suggesting the measures from the two different sources are quite similar.

We next merged the Census/ACS measures of the Black-white homeownership gap with the state-level data on incarceration. We estimated the relationship between the total incarceration rate (lagged three years) and the Black-white homeownership gap (measured as Black/white), adjusting for both state- and year-fixed effects. For comparability with the CPS models, we use data from the years 1980, 1990, 2000, and 2001-2005.

Appendix Table B2, below, replicates the analysis of CPS data presented in Table 4. The key differences from those earlier models is (1) the use of Census/ACS vs. CPS, (2) use of the years 1980, 1990, 2000-2005 vs. 1985-2005, and (3) omission of the control for the contract interest rate since it is not available until 1978 and so cannot be lagged three-years from the first (1980)

observation of the dependent variable. The models show evidence of effects of the incarceration rate on the Black-white home-ownership gap. Smaller coefficients signal a larger gap. Compared with the models presented in Table 4, the coefficients are somewhat reduced in magnitude but are still negative, significant, and substantial. For example, comparing the preferred model (Model 1) in Table 4 and in Appendix Table B2, below, the Beta is reduced from -0.022 to -0.012.

Appendix B1. Black-White Homeownership Gap in CPS and Census/ACS

| | Black/White | | | | White-Black | | | |
| | CPS | | Census/ACS | | CPS | | Census/ACS | |
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
1980	0.65	(0.17)	0.65	(0.11)	0.24	(0.11)	0.24	(0.06)
1990	0.58	(0.18)	0.60	(0.11)	0.29	(0.11)	0.27	(0.07)
2000	0.61	(0.14)	0.59	(0.10)	0.28	(0.10)	0.29	(0.06)
2001	0.61	(0.15)	0.59	(0.10)	0.28	(0.10)	0.29	(0.06)
2002	0.61	(0.14)	0.60	(0.10)	0.29	(0.10)	0.29	(0.06)
2003	0.61	(0.14)	0.59	(0.10)	0.29	(0.11)	0.29	(0.07)
2004	0.60	(0.14)	0.59	(0.10)	0.30	(0.09)	0.30	(0.07)
2005	0.60	(0.13)	0.58	(0.11)	0.30	(0.09)	0.31	(0.07)

Appendix B2. Black-White Homeownership Gap, Black Homeownership Rate, and White Homeownership Rate as a Function of State Incarceration (with Census/ACS data)

	Model 1		Model 2	Model 3
	Black-White gap		*Black rate*	*White rate*
Prisoners per thousand (lagged three years)	-0.012	*	-0.006	0.001
	(0.005)		(0.004)	(0.002)
Constant	0.471		0.285	0.624
Observations	292		292	292
Year fixed-effects	Yes		Yes	Yes
State fixed-effects	Yes		Yes	Yes
State controls	Yes		Yes	Yes
AR(1) correction	Yes		Yes	Yes

Note: In addition to the year and state-fixed effects, each model includes controls for the state unemployment rate, per capita personal income, the poverty rate, the home price index value, the ratio of median household income to home value, the violent crime rate, the percent black, the percent white, and the percent elderly. Data on the state minimum wage, the contract interest rate, the maximum combined AFDC/TANF Food Stamps benefit for a family of four, and the state EITC rate are not available as of 1977 and so are excluded from the models to preserve the state-year observations derived from the 1980 Census. * $p < .05$.

Appendix C: State-Level Control Variables

1. *Unemployment rates (1970-2013).* We accessed annual unemployment rates for the states for the period 1976-2013 from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) database. We then added unemployment data for the years 1970-1971 from the 1972 Statistical Abstract and for the years 1972-1975 from the 1976 Statistical Abstract.

2. *Home Price Index (1975-2012).* We accessed data from the Federal Housing Finance Agency's (FHFA). The Home Price Index (HPI) is a measure of single-family house prices that is constructed on a quarterly and annual basis for each of the 50 states and DC based on repeat transactions for properties involving mortgages securitized or purchased by Fannie Mae or Freddie Mac. It is estimated based on sale price data and on appraisal data and is normalized to be 100 in the first quarter of 1980. Since the HPI is updated each quarter, the values of all prior observations can changed. We downloaded data ending in the fourth quarter of 2012.

3. *Per Capita Personal Income (1970-2012).* We accessed data from the Bureau of Economic Analysis (BEA) on per capita income, which is calculated as total personal income (from all sources) divided by mid-year state population. We used the CPI-U to put this in constant 1970 dollars.

4. *Contract Interest Rate (1978-2011).* We accessed data from the FHFA on the average contract interest rate on conventional single-family mortgages by state.

5. *Poverty Rate (1977-2012).* We calculated state poverty rates using the individual CPS data in the March supplements.

6. *Percent Black (1970-2010).* We combined information from Census's intercensal state- population estimates for the years 1970-1979, 1981-1989, 1990-1999, and 2000-2010 to construct a measure of the percent of the state population that was Black in each year for each state. Information on Hispanic ethnicity was not available for all years, so this is a measure of the percent Black non-Hispanic and Hispanic.

7. *Percent White (1970-2010).* We combined information from Census's intercensal state-population estimates for the years 1970-1979, 1981-1989, 1990-1999, and 2000-2010 to construct a measure of the percent of the state population that was Black in each year for each state. Information on Hispanic ethnicity was not available for all years, so this is a measure of the percent white non-Hispanic and Hispanic.

8. *Percent Elderly (1970-2010).* We combined information from Census's intercensal state-population estimates for the years 1970-1979, 1981-1989, 1990-1999, and 2000-2010 to construct a measure of the percent of the state population that was over the age of 65.

9. *Violent Crime Rate (1970-2010).* We accessed data from the FBI Uniform Crime Reports to construct a measure of the violent crime rate where violent crime includes murder, forcible rape, robbery, and aggravated assault.

10. *State Minimum Wage (*1980-2011). University of Kentucky's Center for Poverty Research. This variable records the state minimum wage if higher than the Federal and the Federal minimum wage otherwise. Details are available at http://www.ukcpr.org/AvailableData.aspx.

11. *State EITC Rate (1980-2011).* We accessed data from the University of Kentucky's Center for Poverty Research. Details are available at http://www.ukcpr.org/AvailableData.aspx.

12. *State Maximum AFDC/TANF and Food Stamps Benefit for family of four (1980-2011).* We accessed data from the University of Kentucky's Center for Poverty Research. Details are available at http://www.ukcpr.org/AvailableData.aspx.

13. *Median Income to Home Value (1975 to 2005).* We accessed data on the median income of four person families (in current dollars) by state from the US Census Bureau's archive of tabulations used by HHS for LIHEAP. We divided that figure by the average home price (in current dollars) by state, available from the Lincoln Institute of Land Policy (LILP), averaging across the quarterly values to generate annual measures. These average home price values are estimated by

LILP using data from the Decennial Census of Housing and the FHFA HPI. Detailed information is available the following: Davis, Morris A. and Jonathan Heathcote. 2007. "The Price and Quantity of Residential Land in the United States" *Journal of Monetary Economics* 54: 2595-2620.

Appendix C1. Means and Standard Deviations of Variables in the State-Level Analysis (Pooled Across States and Years)

	State Years (880)	
	Mean	S.D.
Black homeownership rate	43.0	(12.0)
White homeownership rate	71.0	(8.0)
Black/white homeownership	50.0	(16.0)
Imprisonment rate	3.5	(2.3)
Unemployment rate	6.0	(2.9)
Home price index	192.0	(82.0)
Median income/average home value ratio	42.0	(12.0)
Violent crime rate	565.0	(317.0)
Poverty rate	13.8	(4.3)
Contract interest rate	8.5	(2.2)
Percent elderly	12.4	(2.1)
Percent Black	11.0	(10.9)
Percent white	67.6	(22.0)
Per capita income ($)	5877.0	(1135.0)
EITC rate	2.9	(8.8)
Minimum wage ($/hr)	4.2	(1.1)
Maximum AFDC/TANF and food stamps (family of four) ($)	772.0	(202.0)