Imprisonment and Infant Mortality

Christopher Wildeman*
University of Michigan

* Direct mail to Christopher Wildeman, University of Michigan, School of Public Health, 3648 SPH Tower, 109 Observatory, Ann Arbor, MI 48109. Direct email to wildeman@umich.edu. This research was supported by a dissertation fellowship from the Harry Frank Guggenheim Foundation and a postdoctoral fellowship from the Robert Wood Johnson Foundation Health & Society Scholars Program. I thank Bruce Western, Sara McLanahan, Devah Pager, Megan Comfort, Jason Beckfield, Jeff Morenoff, Sarah Burgard, Felicia LeClere, Kris Siefert, Michelle Debbink, Jen Lundquist, John Eason, Deirdre Bloome, and Andy Papachristos for helpful comments on earlier versions of this paper. I also thank the CDC PRAMS Working Group members for making the data available and providing valuable comments on multiple drafts of this manuscript and presentations based on it. All errors are my own, and the contents of this paper do not necessarily reflect official views of any funding agencies or the CDC, the CDC PRAMS, or the CDC PRAMS Working Group members.

ABSTRACT

This article extends research on the consequences of mass imprisonment by considering effects of imprisonment on infant mortality, which is generally considered a crucial indicator of population health. The first stage of the analysis uses state-level data from 1990 to 2003 and a series of fixed effects models with a first order autoregressive process to test the relationship between imprisonment and infant mortality, finding evidence that state-level imprisonment rates are positively associated with state-level infant mortality rates. Results are robust to excluding outliers, including data on admissions to drug treatment, including an interaction between imprisonment and crime, and testing for reverse causality and spuriousness. State-level results suggest that had the imprisonment rate remained at the 1990 level, the 2003 infant mortality rate would have been about five percent lower. The second stage of the analysis uses data from the Pregnancy Risk Assessment Monitoring System (PRAMS) from 1998 to 2003 to consider effects of recent parental incarceration on early infant mortality at the individual-level. Results suggest that recent parental incarceration increases early infant mortality risk. They also suggest that effects are concentrated among infants whose mothers experienced numerous other stressors but did not experience abuse before or during the pregnancy. Taken together, results suggest that mass imprisonment has important consequences for population health and should be considered in subsequent analyses assessing variation in health across nations, states, and individuals.

As the American imprisonment rate has increased from a relatively modest 100 per 100,000 to an astounding 509 per 100,000 (West and Sabol 2009), researchers have developed an interest in the consequences of mass imprisonment. Much of this interest has focused on understanding the social patterning of imprisonment (Pettit and Western 2004; Western and Wildeman 2009) and the effects of having ever been imprisoned on adult men's labor market prospects (Kling 2006; Pager 2003; Western 2006), although researchers have also dedicated attention to the effects of mass imprisonment on crime (Western 2006:168-188) and political and economic inequality (Manza and Uggen 2006; Western 2002; Western and Beckett 1999). Potential intergenerational effects of mass imprisonment have also generated substantial interest, leading researchers to consider the social patterning of the risk of parental imprisonment and the consequences of this experience for children. Findings from this literature establish that 25 percent of black children born in 1990 experienced parental imprisonment by age 14 (Wildeman 2009) and that parental incarceration has negative effects on children's wellbeing, development, and risk of social exclusion more broadly (Geller et al. *Forthcoming*; Foster and Hagan 2007; see also the reviews of Hagan and Dinovitzer 1999; Murray and Farrington 2008).

Although researchers have started to consider the consequences of mass imprisonment for American children, no research to date considers the effects of mass imprisonment on child health. This inattention is surprising for three reasons. First, research documents that imprisonment affects not only ever-imprisoned adult men but also their families and children (Braman 2004; Comfort 2007, 2008; Johnson and Raphael *Forthcoming*; see also the reviews of Hagan and Dinovitzer 1999; Murray and Farrington 2008). As such, effects of parental incarceration on the health of children seem plausible. Second, since so many American children are exposed to the penal system relative to children from other developed countries, is seems plausible that mass imprisonment could have effects on health not only at the individual-level, but also at the population-level (Wildeman 2009). One example of these population-level effects can be drawn from the effects of mass imprisonment on foster care. Changes in the female imprisonment rate explain fully 30 percent of the increase in foster care caseloads between 1985 and 2000 (Swann and Sylvester 2006), suggesting that mass imprisonment may contribute to population-level changes in the social experience of childhood. Finally, since childhood health influences subsequent life-chances (Palloni 2007), to the degree that parental imprisonment harms child health, it may exacerbate inequality not only in childhood, but in adulthood as well.

This article extends research on the consequences of mass imprisonment by considering the effects of imprisonment on infant mortality, which is generally considered a crucial indicator of population health—especially of the most disadvantaged portion of the population. In the first stage of the analysis, I use state-level data from 1990 to 2003 and a series of fixed effects models with a first order autoregressive process to test the relationship between imprisonment and infant mortality, finding evidence that state-level imprisonment rates are positively associated with state-level infant mortality rates. Results are robust to excluding outliers from the analysis, including data on admissions to drug treatment, including an interaction between imprisonment and crime, and testing for reverse causality and spuriousness. Results suggest that had the imprisonment rate remained at the 1990 level, the 2003 infant mortality rate would have been about five percent lower. The second stage of the analysis uses data from the Pregnancy Risk Assessment Monitoring System (PRAMS) from 1998 to 2003 to consider effects of recent parental incarceration on early infant mortality at the individual-level. Results suggest that recent parental incarceration increases early infant mortality risk. They also suggest that effects are concentrated among infants whose mothers experienced numerous other stressors but did not experience abuse before or during the pregnancy. Taken together, results suggest that mass imprisonment has important consequences for population health and should be considered in analyses assessing variation in health across nations, states, and individuals. They also suggest that the consequences of mass imprisonment do not extend only to traditionally considered measures such as labor market success and family formation (and dissolution), but also to more severe forms of disadvantage such as infant mortality that are rarely considered by penologists.

SOCIAL FACTORS, THE STATE, AND INFANT MORTALITY

Infant mortality is important not only because it represents a tragic, generally avoidable, event, but also because the infant mortality rate is typically considered one of the best measures of population health—especially the health of the most disadvantaged individuals in a population (Beckfield 2004; Conely and Springer 2001; Wise 2003). Like most countries, the United States has experienced a sharp decline in the infant mortality rate since the middle of the past century (Singh and Kogan 2007:e931). And like most developed democracies, America has an infant mortality rate that is well below 10 per 1,000 live births (OECD 2006). Nonetheless, the American infant mortality rate differs significantly from other developed democracies in at least two ways. First, the American infant mortality rate exceeds the infant mortality rate of comparably developed countries like the Western European nations and Canada (OECD 2006; Table 1). Although raw differences are not always large, the American infant mortality rate continues to be at least 30 percent higher than the infant mortality rate in any Western European nation. Second, declines in the American infant mortality rate—both absolute and relative—have been much smaller than they have been in comparable nations (OECD 2006; Table 1). In fact, the American infant mortality rate even increased between 2001 and 2002, a remarkable event that demonstrated the fragility of recent declines in infant mortality (MacDorman et al. 2005:1).

[Insert Table 1 about here.]

In light of the importance of the infant mortality rate as an indicator of population health and the high American infant mortality rate, researchers have tried to determine what factors most influence macro-level infant mortality rates and micro-level infant mortality risks (Conley and Springer 2001; Wise 2003; see also Beckfield 2004; Beckfield and Krieger *Forthcoming*). Although there is substantial disagreement over the degree to which changing state programs, social conditions, access to new technologies and information, and individual behaviors can affect individual infant mortality risks and population infant mortality rates, there seems to be an emerging consensus that the quickest way to diminish infant mortality rates is by improving maternal health and maternal access to new technologies and information (Wise 2003:356-357).

This is not to say that other predictors of infant mortality (such as low birth weight) are unimportant, but to suggest that changes in maternal health and access to technologies and information may provide the crucial link between social change and changing infant mortality risk. As such, to the degree that a social change is likely consequential for infant mortality, it is best to consider first the degree to which it affects women's health—to what degree it alters physical or mental health by altering social integration, social support, or stress levels, all of which potentially affect the infant mortality rate—and to what degree it influences women's access to technological innovations and new information about how to diminish the risk of infant mortality. One excellent example of how access to information influences inequality in infant mortality comes from the "back to sleep" campaign, which, although successful, exacerbated inequality in SIDS-related mortality because it was more successful among high-SES than low-SES women (Pollack and Frohna 2001). Therefore, to the degree that a social change influences these aspects of women's lives—health and access to new technologies and information—it will alter infant mortality rates. To the degree that it does not, it will likely not alter them.

IMPRISONMENT AND INFANT MORTALITY

Research on infant mortality suggests that anything that alters maternal health or access to new technologies or information may alter the infant mortality rate. Unfortunately, no research considers the consequences of mass imprisonment for infant mortality. This is surprising since both macro-level (Johnson and Raphael *Forthcoming*; Western and Beckett 1999) and micro-level (Binswanger et al. 2007; Braman 2004; Comfort 2007, 2008; Massoglia 2008a, 2008b; Pager 2003) research document that imprisonment effects not only ever-imprisoned adult men but also their families and children. And since so many Americans are exposed to the penal system relative to other developed countries, is seems plausible that mass imprisonment could have effects on infant mortality not only at the individual-level, but also at the population-level (Bonczar 2003; Pettit and Western 2004; Western and Wildeman 2009; Wildeman 2009). In this section, I rely on previous research on the consequences of imprisonment for individuals and their families and a modified version of the weathering hypothesis (Geronimus 1992) to suggest that mass imprisonment increases infant mortality risk by compromising women's physical and mental health, diminishing their social support, and placing them under chronic stress.

Although the economic costs of incarceration for ex-offenders have likely received more attention than any other research area (Kling 2006; Pager 2003; Western 2006), researchers have also shown interest in the health consequences of incarceration. A history of incarceration elevates the risk of having severe health limitations and being afflicted with infectious or stress-related diseases (Schnittker and John 2007; Massoglia 2008a, 2008b). As such, it seems likely that ever-incarcerated women will be more likely to suffer from poorer health than similar women not experiencing incarceration. And since many infectious diseases are easily transmitted to intimates, the wives and partners of ever-incarcerated men can also expect to be at elevated risk of being afflicted by some infectious disease. One state-level analysis demonstrates the effect of mass imprisonment on the spread of infectious disease by showing how imprisonment rates are associated with elevated risks of contracting HIV/AIDS for those in the same marriage markets as those imprisoned (Johnson and Raphael *Forthcoming*). As such, it appears that imprisonment has both direct and indirect influences on maternal health. And since having a partner incarcerated increases feelings of hopelessness and depression (Braman 2004; see also Comfort 2008), it seems likely that imprisonment also compromises women's mental health.

Consequences of experiencing imprisonment or having a partner imprisoned for mental and physical health are important. Yet, consequences for social support and integration, which are intimately connected with health and wellbeing, may be even more profound. Although men are likely to provide diminished social support when they are behind bars, research suggests at least two additional ways in which imprisonment diminishes social support. Probably most importantly, having a partner incarcerated causes women to retreat from social situations to avoid the "sticky stigma" that comes with having a partner incarcerated (Comfort 2008; see also Braman 2004:173), leaving them socially isolated in many instances. And since the mothers of incarcerated men suffer from elevated risks of depression, having a partner incarcerated may further weaken the social ties from which women can draw support (Green et al. 2006). In addition to the negative effects of social isolation on women's mental and physical health, socially isolated women may also be less able to gain access to information about the newest techniques used to diminish infant mortality risk—such as putting children to sleep on their backs—thereby elevating their infant's mortality risk relative to other infants.

Consequences of incarceration for physical health, mental health, and social support are important, yet the consequences of incarceration for chronic stress may be even more profound. Since research links chronic stress with poor birth outcomes and elevated infant mortality risk (see the review of Giscombé and Lobel 2005), to the degree that having partner incarcerated (or experiencing incarceration oneself) elevates women's probability of experiencing chronic stress, it likely elevates their infant's mortality risk. In light of the negative consequences of incarceration for men's earnings (Pager 2003; Western 2006) and risk of marital dissolution (Lopoo and Western 2005), partners of ever-incarcerated men can expect to have access to less resources that they would have had access to had their partners not been incarcerated. Diminished financial resources are not the sole pathway through which having a partner incarcerated could contribute to chronic stress, however. Ethnographic research suggests that imprisonment (Nurse 2002:52-54) and contact with the criminal justice system more broadly (Goffman 2009) decreases men's ability to function as partners and fathers—and that it does so in no small part by socializing men to resolve difficult situations with violence (Nurse 2002) and forcing them to cultivate unpredictability in their daily routines (Goffman 2009).

Previous research suggests a host of mechanisms through which having a partner incarcerated—or being incarcerated—compromises women's mental and physical health, diminishes their social support and integration, and contributes to chronic stress. In so doing, mass imprisonment contributes to a type of weathering (Geronimus 1992) in which contact with the criminal justice system compromises maternal well-being and access to information, thereby elevating their children's infant mortality risk. In the sections that follow, I test the hypothesized effects of imprisonment on infant mortality at the state-level and the individual-level.

DATA, MEASURES, AND METHOD

Data

In order to test the relationship between imprisonment and infant mortality, this analysis relies on two datasets. The first dataset is designed to test the relationship between state-level imprisonment rates (male, female, and total) and state-level infant mortality rates. In order to assemble this dataset, annual data from the 50 states and the District of Columbia were pooled over the period 1990 to 2003. These are the years during which the risk of imprisonment for adults and the risk of parental imprisonment for children grew most rapidly (Pettit and Western 2004; Wildeman 2009). It is also when the rate of decline in the infant mortality rate began to diminish, culminating in an increase in the American infant mortality rate between 2001 and 2002 (MacDorman et al. 2005). One difficulty with this period, however, is that the first few years of the analysis also represent the end of the so-called "crack boom." In order to deal with the possibility that it is the "crack boom" rather than rising imprisonment rates that are driving infant mortality rates, models limited to the period after the height of the crack boom rather than during it (1994-2003) were also run. Results from these two periods showed no difference in the effects of imprisonment on infant mortality (see Table 4, Model 3; Table 5, Model 3). Variables considered in the state-level analysis are compiled by state or national governments. As such, there is little missing data. The only missing data come from the District of Columbia, whose prisoners have been included in federal imprisonment rates since the early 2000s. In order to keep the District of Columbia in the sample, the 2001 imprisonment rate is used for subsequent years. (For a list of data sources and descriptive statistics for the state-level sample, see Table 2.)

[Insert Table 2 about here.]

The central hypothesis of this manuscript is that imprisonment increases infant mortality. Analyses considering the predictors of infant mortality have traditionally used data at either the state-level or the national-level (see especially Beckfield 2004; Conley and Springer 2001), in no small part because of how difficult it is to study such a rarely occurring event using traditional survey data. Although analyses tend to focus on macro-level data, if micro-level data could be utilized to test whether there is a link between parental incarceration and infant mortality, then that would provide additional insight into the degree to which state-level analyses can be considered reliable. Unfortunately, individual-level data containing information on both parental incarceration and infant mortality risk are rare. Even surveys that contain information about both parental incarceration and infant mortality, like the Fragile Families and Child Wellbeing Study, do not typically have enough cases of infant death to consider this outcome. As such, data limitations make it difficult to test this hypothesis using traditional survey-based data.

Traditional survey data may not be suitable for considering the consequences of parental imprisonment for infant mortality, but one dataset is well-suited for considering this relationship: the Pregnancy Risk Assessment Monitoring System (PRAMS). Each year since 1988, participating states have sampled between 1,300 and 3,400 women, with an oversample of at-risk births (see Table A1 for a list of states participating in PRAMS between 1990 and 2003). Since the PRAMS data have been collected by the CDC in a number of states for a long period of time and oversample women with infants at elevated risk of mortality, these data provide a unique opportunity for considering the consequences of parental incarceration for infant mortality risk.

Despite the many benefits of the PRAMS data, there are also a number of limitations. First, since most surveys are completed between two and six months after the child's birth, the PRAMS data provide a measure of *early* infant mortality rather than a true measure of infant mortality—defined as the number of infants per 1,000 live births that do not survive their first year. Second, the measure of parental incarceration changes in 1998. The measure used before 1998 is based only on whether the father has been incarcerated in the last year, while the measure used from 1998 to 2003 is based on whether either parent has been incarcerated in the last year. In the interest of providing measures of parental incarceration rather than paternal incarceration, I limit the analytic sample to the later period. A final limitation of the survey is that there are only very limited measures of family income and wealth, making it difficult to know whether it is parental incarceration or poverty that is responsible for any observed association between parental incarceration and infant mortality. In order to deal with this concern, I limit the analytic sample to women who were on WIC at the time of the survey. I also limit the sample to singleton births and children with no known birth defects since the predictors of infant mortality may differ for these high-risk infants. Some analyses split the sample into sub-groups that would or would not be expected to experience large effects in order to strengthen empirical analysis. All analyses using the PRAMS data are also weighted to account for the complex sampling design. (For descriptive statistics for the analytic sample by parental incarceration, see Table 3.)

[Insert Table 3 about here.]

Measures

INFANT MORTALITY. The dependent variable for the state-level analysis is the infant mortality rate (per 1,000) for all 50 states and the District of Columbia between 1990 and 2003. The mean infant mortality rate for the sample is 7.8 per 1,000. For the individual-level analyses, the dependent variable is early infant mortality. This measure is based on maternal reports of whether the infant died before the mother was interviewed or in the first year, whichever comes first. Although this is not a true measure of infant mortality since each child is at risk of mortality for an average of four months (see Table 3), it is better than any other measure of infant death in a large dataset. And since the vast majority of infant deaths occur within the first few months, it should not miss many infant deaths. The mean risk of mortality for children of recently incarcerated parents was .008; for all other children, the mean risk of mortality was .005.

IMPRISONMENT. The explanatory variables for the state-level analysis are male, female, and total imprisonment rates, expressed as the number of individuals in prison in any given state at the end of the year per 1,000. Since imprisonment rates are drawn from year-end prison statistics, the previous year's imprisonment rate is used to predict infant mortality. The mean female imprisonment rate over this period was .2 per 1,000; the mean male imprisonment rate was 3.4 per 1,000; and the mean total imprisonment rate was 1.8 per 1,000. Since the correlation between the male and female imprisonment rate is high (r=.88) and the hypotheses suggest that both the male and female imprisonment rates elevate infant mortality risk, most models consider the total imprisonment rate as the dependent variable. Before moving on, it should be noted that although the imprisonment rate is likely predictive of the number of individuals cycling in and out of the penal system in any year, it drastically underestimates the number of individuals affected by the penal system. As such, each individual included in the imprisonment rate represents a larger number of individuals cycling through the system.

For the individual-level analysis, the dependent variable is whether either parent had been incarcerated in the last year according to maternal reports. Nearly 9 percent of the sample of infants had a parent incarcerated in the last year. In light of the rapidly increasing risk of parental imprisonment for disadvantaged American children over this time period (Wildeman 2009), the fact that incarceration is a more expansive measure of contact with the penal system than imprisonment, and the high-risk nature of the analytic sample considered, it seems plausible that such a high percentage of the sample would have been incarcerated in the last year.

CONTROL VARIABLES. These analyses also include a host of control variables. All of these control variables are included because they are likely associated with the dependent and independent variables. For the state-level analyses, one crucial control is the violent crime rate (per 1,000). This control is especially important for two reasons. First, including this control—and an interaction between it and the imprisonment rate in some models—elucidates the degree to which it is crime or imprisonment that is influencing the infant mortality rate. Second, because no reliable state-level measures of crack-cocaine addiction exist and changes in the violent crime rate correspond with the severity of the crack-cocaine epidemic (Boggess and Bound 1997), including this control has the further advantage of helping indirectly control for the share of the infant mortality rate attributable to crack-cocaine addiction among expectant and new parents.

Another method for dealing with the possibility that it is drug abuse (not imprisonment) that is driving any association is including some measure of drug addiction. Although most measures of addiction cover limited geographic areas, like the Drug Abuse Warning Network (DAWN) data, or limited time periods, like the Treatment Episode Data Set (TEDS), they still provide useful robustness checks. DAWN data track the number of hospital admissions for drug overdose for select urban areas; TEDS data track rates of admissions to drug rehabilitation facilities for most states. Since the TEDS data are available for a more extensive number of areas than are the DAWN data (see Table A1 for a list of states and years), they are utilized for a robustness check (Table 5, Models 3-4). Although the TEDS data are used to consider robustness of effects, these measures are omitted from the main analysis because they cover a limited time period and not all states report in any given year (see again Table A1 for details).

The state-level analysis also controls for state characteristics such as the percent of the population that is foreign-born, that has at least a high school diploma, that is black, that is Hispanic, and that resides in urban areas since these factors may be linked with infant mortality and imprisonment rates (Hummer et al. 2007 provides one good example). The analysis also controls for GDP per capita and the Gini coefficient since research provides some evidence of their association with population health (see the review of Beckfield 2004). The analysis also controls for the number of AFDC/TANF cases per 1,000 and the monthly combined AFDC/TANF and food stamp benefit for a family of three since these measures of welfare state generosity may be associated with imprisonment and infant mortality. Although Conley and Springer (2001) show that state health care spending is a strong predictor of infant mortality across nations, this analysis does not include these measures because they have only been readily available at the state-level since 1997 (Milbank Memorial Fund 1999, 2001, 2003, 2005). In models limited to the years for which these data were available, effects of imprisonment on infant mortality grew stronger when state health care spending was included in the model, suggesting that results are robust to the inclusion of this measure. The analysis also controls for the percentage of births that were nonmarital, the percentage of the population living in poverty, and the unemployment rate because of their potential association with the dependent and independent variables. Finally, the analysis controls for the number of doctors and nurses per 1,000 and a host of variables associated with maternal behaviors, prenatal care, birth outcomes, and infant mortality risk—the percentage of mothers who smoked and who received no prenatal care and the percentage of births that were premature or low birth weight.

For the individual-level analyses, a host of factors associated with the risks of recent parental incarceration and early infant mortality are included as controls. Possibly most importantly, the analysis controls for the number of months between the child's birth and when the mother responded to the interview. Since this variable helps capture time at risk of infant mortality, it is crucial that it be included in the model. The analysis also controls for the following maternal characteristics since research links many of them with infant mortality risk: marital status, race (white, black, Hispanic, other), education (less than high school or more), age, and BMI (underweight, healthy/overweight, obese) (see especially Kramer et al. 2000; Matthews and MacDorman 2007:14; Singh and Yu 1995; Singh and Kogan 2007). The analysis also controls for the number of stressful life experiences in the last year since a growing literature links stress with elevated infant mortality risk (see the somewhat dated but relevant review of Chomitz, Cheung, and Lieberman 1995). This scale ranges from 0 to 3 and is based on whether the mother reported that her partner had lost his job, she had been homeless, and she or someone she was very close to had a bad problem with drugs or alcohol in the last year.

Since previous research shows a strong relationship between past birth outcomes and current birth outcomes, the analysis also controls for the number of previous live births, whether the mother has ever had a low birthweight baby, and whether the mother has ever had a preterm birth. The analysis also controls for whether the birth was male in light of connections between child sex and infant mortality (Matthews and MacDorman 2007:14). Although debates are often contentious about the degree to which maternal smoking, drinking, and drug use influence birth outcomes (see especially Armstrong 2003), the analysis nonetheless controls for whether the mother smoked or drank since research suggests that smoking and drinking while pregnant elevates infant mortality risk (Chomitz, Cheung, and Lieberman 1995 provide but one example). Models also control for the number of prenatal visits and birth outcomes—whether the infant was low birthweight or very low birthweight, preterm, or spent time in the intensive care unit—since research suggests likely connections between these factors, infant mortality risk, and parental behaviors potentially associated with incarceration (Alexander and Kronebrot 1995; Callaghan et al. 2006; see also the relevant discussion in Conley and Springer 2001).

Method

The analytic tool used for testing the hypothesis that state-level imprisonment rates increase state-level infant mortality rates is an OLS fixed effects model with state and year fixed effects and a first order autoregressive process. This model is preferred in a panel dataset in which the entire population is represented and there is serial autocorrelation in the errors (for relevant discussions, see Beckfield 2006; Conley and Springer 2001). Some models use the male or female imprisonment rate as the independent variable (Table 4, Models 1-2 and 4-5), but most models used in this analysis use the total imprisonment rate since it seems likely that both male and female imprisonment have effects on infant mortality and since the high correlation between these variables (r=.88) leads to unstable estimates when they are included in the models together. Robustness checks in which an outlier (the District of Columbia) is excluded from the analysis, data on admissions to treatment for drug or alcohol abuse for a limited number of years are included in the analysis, and an interaction between the imprisonment rate and the violent crime rate is included are also shown. In another robustness check, models test for reverse causality (and spuriousness) by simultaneously including the imprisonment rate in the year before and the year after the predicted infant mortality rate (Conley and Springer 2001 discuss). All models for the state-level analyses use one-sided t-tests since my hypothesis is directional.

For the analysis using the PRAMS data, logistic regression models with state and year fixed effects are used to consider the relationship between recent parental incarceration and early infant mortality in a sample restricted to mothers receiving WIC at the time of the pregnancy. Since the PRAMS data do not contain extensive measures of income or wealth, limiting the sample to women receiving WIC during their most recent pregnancy diminishes heterogeneity is SES. In additional analyses, I limited the sample to women who were and were not abused before or during their pregnancy (Table 6, Models 3 and 4) and who did or did not experience any of the stressful life experiences outlined earlier in the measures section (Table 6, Models 5 and 6). I split the sample by abuse status since it seems likely that incarceration could potentially provide a respite for abused women and because women who had come into contact with the penal system either directly or through their partners were nearly four times more likely to have been abused. I split the sample by stressful experiences for two reasons. First, diminishing the sample to those who had experienced a host of other forms of disadvantage diminishes heterogeneity, thereby strengthening inference. Second, to the degree that consequences of parental incarceration are concentrated among those who are or are not experiencing other disadvantages, we can gain insight into the degree to which incarceration affects inequality. All models for the individual-level analyses use one-sided t-tests since my hypothesis is directional.

RESULTS

Results from State-Level Analyses

The first three models in Table 4 consider the relationship between the female, male, and total imprisonment rate on the infant mortality rate. These models do not control for any time-varying covariates, but they use a first order autoregressive process and control for state and year fixed effects. Results from Model 1, which considers the association between the female imprisonment rate and the infant mortality rate, suggest that the female imprisonment rate is a statistically significant predictor (at the .001 level) of the infant mortality rate. Each one unit increase in the female imprisonment rate is associated with a 4.21 unit increase in the infant mortality rate.

Results from Model 2, which considers the association between the male imprisonment rate and the infant mortality rate, tell a similar story. The male imprisonment rate is a statistically significant predictor (at the .001 level) of the infant mortality rate, and each one unit increase in the male imprisonment rate is associated with a .58 unit increase in the infant mortality rate. Although this would appear to suggest that the female imprisonment rate exerts a stronger effect on the infant mortality rate than the male imprisonment rate does, this is not the case. A one standard deviation change in the male imprisonment rate leads to a larger change in the infant mortality rate (1.30) than does the same change in the female imprisonment rate (1.10).

Models 1 and 2 considered the effects of the female and male imprisonment rates on the infant mortality rate separately. Model 3, on the other hand, considers the effect of the total imprisonment rate on the infant mortality rate in a simple fixed effects model with state and year fixed effects and using a first order autoregressive process that does not include any controls for time-varying covariates. Results indicate that the total imprisonment rate, like the male and female imprisonment rates, is a statistically significant predictor (at the .001 level) of the infant mortality rate. Each additional one prisoner (per 1,000 population) increase is associated with a 1.07 (per 1,000) increase in the infant mortality rate. A one standard deviation increase in imprisonment is associated with a 1.25 (per 1,000) increase in the infant mortality rate.

[Insert Table 4 about here.]

Results from Models 1, 2, and 3 support the hypothesis that male, female, and total imprisonment rates are all positively associated with infant mortality rates. Since these models do not adjust for other variables likely associated with both the imprisonment rate and the infant mortality rate, however, they can only be considered extremely preliminary evidence. In Models 4, 5, and 6, a host of time-varying covariates likely associated with both imprisonment and infant mortality rates are introduced. Results from Model 4, which considers the effects of the female imprisonment rate on the infant mortality in a fixed effects model with state and year fixed effects, a first order autoregressive process, and a host of time-varying covariates, suggest that the female imprisonment rate is positively and significantly (at the .001 level) associated with the infant mortality rate. Each one unit increase in the female imprisonment rate (per 1,000) is associated with a 2.08 increase (per 1,000) in the infant mortality rate.

Results from models considering the effects of the male imprisonment rate (Model 5) and the total imprisonment rate (Model 6) on the infant mortality rate after adjusting for time-varying covariates suggest that the male and total imprisonment rates are both positively and significantly (at the .001 level) associated with the infant mortality rate. Although the coefficient for the effects of the female imprisonment rate (2.08) on the infant mortality rate is larger than the coefficients for the male (.27) and total (.50) imprisonment rates, results suggest that the consequences of a one standard deviation change in the female imprisonment rate on the infant mortality rate (.33) are much smaller than the consequences of a one standard deviation change in the male (.60) or total (.59) imprisonment rates. As such, it appears that the male and total imprisonment rates are stronger predictors of infant mortality than is female imprisonment.

Results from Table 4 suggest that the female, male, and total imprisonment rates are all positively and significantly (at the .001 level) associated with the infant mortality rate in a series of models that adjust for state and year fixed effects, adjust for serial autocorrelation, and occasionally include time-varying controls for factors likely associated with both imprisonment rates and infant mortality rates. Although results provided in these analyses tentatively suggest a positive association between imprisonment and infant mortality, additional robustness checks are also shown in Table 5 to demonstrate how estimates change when an outlier is excluded, data on drug admissions treatment are included, and an interaction between the imprisonment rate and the infant mortality rate is included. The final two models also test for spuriousness and reverse causality by simultaneously including the imprisonment rate at time *t-1* and *t+1* in the models. For all of these models, the total imprisonment rate is used as the independent variable since the theoretical framework advanced earlier in this manuscript suggests that both male and female imprisonment rates likely contribute to elevated infant mortality rates.

Models 1 and 2 in Table 5 test the relationship between imprisonment and infant mortality after excluding the District of Columbia, which is an outlier on a number of variables, from the analysis. Results from Model 1, which includes state and year fixed effects and a first order autoregressive process but does not control for time-varying covariates, suggest that the total imprisonment rate is positively and significantly (at the .01 level) associated with the infant mortality rate when the District of Columbia is excluded from the analysis. Results from Model 2 suggest that the imprisonment rate is positively and significantly (at the .01 level) associated with the infant mortality rate after removing the Districting of Columbia from the analysis and adjusting for time-varying covariates. Although the relationship continues to be statistically significant after removing the District of Columbia from the analysis, the relationship is less statistically significant than it was in models including the District of Columbia—as comparing Model 6 in Table 4 to Model 2 in Table 5 illustrates—and the magnitude of effects is also diminished. While a one standard deviation change in the imprisonment rate in a comparable model including the District of Columbia was associated with a .59 increase in the infant mortality rate, a one standard deviation change in the imprisonment rate in a model excluding this outlier (Model 2, Table 5) is associated with only a .40 increase in the infant mortality rate. Thus, although results are robust to the exclusion of the District of Columbia from the analysis in that they remain statistically significant predictors of the infant mortality rate after removing this outlier, effects are dampened both in terms of statistical significance and magnitude of effects.

One major concern for this analysis was that outliers were not exerting undue influence on the results. Another concern for this analysis is the fact that controls for drug abuse were not included in results shown in Table 4. Models 3 and 4 in Table 5 limit the sample to the states for which TEDS data are available (for a list of states and years contributing to the TEDS data, see Table A1). Model 3 in Table 5 considers the relationship between imprisonment and infant mortality in this more restricted sample. Results from Model 3, which does not include controls for time-varying covariates, suggest that the relationship between imprisonment and infant mortality is positive and statistically significant (at the .001 level) for the years the TEDS data are available. Interestingly, the coefficient in this model (1.08) and the comparison model from Table 4 (Model 3: 1.07) are virtually identical, as are the changes in infant mortality associated with a one standard deviation change in the imprisonment rate (1.25 and 1.21). This suggests that merely limiting the analytic sample to the years in which the TEDS are available does not change results much—at least before controls are included in the model.

Model 4 in Table 5 extends the analysis to consider the relationship between the imprisonment rate and the infant mortality rate controlling for the admission to drug treatment rate, which is likely associated with both dependent and independent variables. Results from this model suggest that including data on admissions to drug treatment neither renders the association between imprisonment and infant mortality nonsignificant—the relationship is statistically significant at the .001 level in this model—nor leads to a diminished association between the imprisonment rate and the infant mortality rate. In fact, the effect of a one standard deviation change in the imprisonment rate on the infant mortality rate in this analytic sample is virtually identical whether controls for admissions to drug treatment are included (1.04) or not (1.05).

[Insert Table 5 about here.]

Thus far, results have been robust to excluding an outlier from the analysis and including data on treatment to drug admissions—although results did become weaker when the District of Columbia was excluded. Models 5 and 6 test for an interaction between the imprisonment rate and the violent crime rate to make sure that these two highly correlated outcomes (r=.72) have independent effects. Results from Model 5, which includes an interaction between the imprisonment rate and the crime rate but does not include controls for any time-varying covariates, suggests that the effects of the imprisonment rate and the violent crime rate on infant mortality may not be independent, as the coefficient for the interaction term is significant (at the

.01 level). Nonetheless, the coefficient for the imprisonment rate is also statistically significant (at the .01 level) in this model, although the coefficient for the violent crime rate is now statistically significant at only the .10 level. Results from Model 5 suggest that although the effects of imprisonment and violent crime on infant mortality may not be independent, the imprisonment rate remains a statistically significant predictor when the interaction is included in the model. Results from Model 6, which includes the interaction term and adjusts for other time-varying covariates, suggest that the effects of the imprisonment rate and violent crime rate are independent. The interaction term does not attain statistical significance, and the coefficients for the imprisonment rate and violent crime rate are comparable to results from models adjusting for covariates but not including an interaction (Table 4, Model 6). This suggests that the effects of imprisonment on infant mortality are independent from effects of violent crime.

Two additional concerns with this analysis are that the relationship between the imprisonment rate and the infant mortality rate may be spurious or that there may be reverse causality—although the second of these seems implausible. In order to deal with these concerns, results from models that simultaneously include the imprisonment rate at time *t-1* and *t+1* are presented. (Results were comparable using the imprisonment rate at time *t*.) Results from Model 7, which includes state and year fixed effects, a first order autoregressive process, and measures of the imprisonment rate at two different times (but no additional controls), suggest that the imprisonment rate at times *t-1* and *t+1* are both positive, statistically significant predictors of the infant mortality rate—although the coefficient for the imprisonment rate at time *t-1* is nearly twice as large and more statistically significant (.001 level versus .05 level). This provides some evidence that findings are robust to modeling concerns about reverse causality or spuriousness. Results are even more convincing when models also adjusting for covariates are considered. In this model (Model 8), the coefficient for the imprisonment rate at time *t-1* is statistically significant (at the .01 level) and comparable in size to previous models (see Table 4, Model 6), while the coefficient for the imprisonment rate at time *t+1* is small and not statistically significant. This suggests that results are likely not spurious or due to reverse causality.

Results from Tables 3 and 4 provide support for the hypothesis that imprisonment rates are positively associated with infant mortality rates. Although some discussion of the magnitude of the effects has been provided throughout this section—focusing especially on the relative magnitude of effects of the imprisonment rate on the infant mortality rate in various models—there has been little discussion to this point of how different the American infant mortality rate would be if the imprisonment rate had not grown to the 2003 level. In order to give the reader some idea of the magnitude of the effects of the imprisonment rate on the infant mortality rate, estimates of the infant mortality rate based on three levels of imprisonment are presented here: (1) the mean American imprisonment rate in 2003; (2) the mean American imprisonment rate in 1990; and (3) the mean imprisonment rate in the United Kingdom in 1990. Estimates for these three scenarios are based on point estimates from Model 6 in Table 4, holding all covariates other than the imprisonment rate at their means. Results suggest the following: (1) the infant mortality rate based on the 2003 American imprisonment rate (2.20 per 1,000) is 7.95 per 1,000; (2) the infant mortality rate based on the 1990 American imprisonment rate (1.37 per 1,000) is 7.53 per 1,000; and (3) the infant mortality rate based on the 1990 imprisonment rate in the United Kingdom (.89 per 1,000) is 7.29 per 1,000. These estimates suggest that the 2003 American infant mortality rate would have been 5.3 percent lower had the imprisonment remained at the 1990 level—and 8.3 percent lower if it were to drop to the 1990 level in the United Kingdom. This suggests that imprisonment rates exert effects on infant mortality rates that are not only statistically significant, but also substantial. It also suggests, however, that the effects detected in these models are not so large as to be considered implausible.

Results from Individual-Level Analyses

Results from state-level analyses suggest that the imprisonment rate is positively and statistically significantly associated with the infant mortality rate. Nonetheless, macro-level analyses such as these are always subject to the ecological inference problem, making it important to know if infants whose parents have been incarcerated in the last year are at elevated early mortality risk relative to other infants. Individual-level analyses also allow me to split the sample into groups of children that could be expected to experience larger or smaller effects of parental incarceration for one reason or another. As such, they allow an additional opportunity to decipher the degree to which the effects uncovered here seem plausible or implausible.

Table 6 presents results from a series of logistic regression models including state and year fixed effects that consider the effects of recent parental incarceration on early infant mortality risk using the PRAMS data. Model 1 demonstrates a descriptive relationship between parental incarceration and infant mortality risk after including state and year fixed effects. Results from this model suggest that parental incarceration significantly increases infant mortality risk (at the .01 level). Being born to a recently incarcerated parent is associated with an increase of about 55 percent in the odds of experiencing mortality in the first year. Results from Model 2, which includes all controls, provide further evidence that recent parental incarceration elevates early infant mortality risk. The coefficient for recent parental incarceration is significant at the .05 level and increases the odds of early infant mortality by about 31 percent. Furthermore, the size of the coefficient (.27) is comparable to the protective effects of marriage on infant mortality (-.26) and the additional risks associated with being abused (.29). As such, it appears that the effects of recent parental incarceration are relatively large even after including controls.

Results from Models 1 and 2 in Table 6 indicate that parental incarceration elevates infant mortality risk. Yet, since odds-ratios only offer insight into relative effects, it is difficult to know what the absolute magnitude of the effects of parental incarceration on infant mortality risk is. In order to provide an estimate of the effects of parental incarceration on infant mortality risk, predicted probabilities of early infant mortality are generated for those experiencing and not experiencing parental incarceration based on results from Model 2 in Table 6 and with all other covariates set to their means. According to these estimates, children of recently incarcerated parents had a .54 percent chance of dying, while all other children had a .42 chance of dying. Thus, parental incarceration increases the probability of infant death by about 29 percent. This again suggests that mass incarceration may have substantial effects on infant mortality risk.

[Insert Table 6 about here.]

Results from logistic regression models in a sample of only women who were on WIC while pregnant showed an association between recent parental incarceration and early infant mortality, even after including a host of controls and state and year fixed effects in Model 2. In Models 3 and 4, I further interrogate the relationship between recent parental incarceration and early infant mortality risk by restricting the sample to women who had (Model 3) and had not (Model 4) been abused in the time leading up to the pregnancy and during the pregnancy. Since it seems implausible that having an abusive partner removed from the household would elevate infant mortality risk, splitting the sample in this way allows me to see if parental incarceration increases early infant mortality risk in situations that it seems plausible that it would—when there is no abuse in the household—and has no effect on early infant mortality risk in situations that it does not seem plausible that it would—in households where there was abuse. Results from Models 3 and 4 in Table 6 present additional evidence that parental incarceration increases infant mortality risk—although only for those who were not exposed to domestic violence. Parental incarceration had a relatively small (.13) and nonsignificant effect on early infant mortality risk in households in which there was abuse and much larger (.37) and statistically significant effect on infant mortality risk in households where abuse was not reported. As such, results from these two models suggest that parental incarceration does increase infant mortality risk, even if effects are restricted to households in which the mother did not document abuse of any kind.

In the final two models in Table 6, I present estimates of the effects of recent parental incarceration on early infant mortality in two samples: Parents who also experienced at least one of the stressors described earlier in the paper (Model 5) and those who did not experience any stressors (Model 6). I split the sample in this way for two reasons. First, limiting the sample to families in which risks of crime, incarceration, and infant mortality are elevated because of a host of other stressors diminishes heterogeneity and provides a more rigorous empirical test. Second, splitting the sample by level of disadvantage also provides insight into the implications of mass imprisonment for inequality in infant mortality risk. Results from Model 5, which is limited to households that experienced at least one stressful experience, suggest that parental incarceration has substantial (.51) and significant (at the .001 level) effects on early infant mortality. Interestingly, the coefficient presented in this model, which includes all controls, is even larger than the coefficient from Model 1 in Table 6, which does not include controls and includes the full sample. Results from Model 6, which is limited to households experiencing no stressful life events, show small, negative, nonsignificant effects of parental incarceration on early infant mortality. Taken together, results from these two models suggest not only that parental incarceration influences infant mortality risk in a sample in which heterogeneity is diminished, but also that mass imprisonment may increase inequality in infant mortality risk.

DISCUSSION AND CONCLUSION

This article has suggested that imprisonment is positively associated with infant mortality risk at the state-level and the individual-level. Results from a series of fixed-effects models with an adjustment for serial autocorrelation provided consistent evidence that increases in the imprisonment rate are associated with increases in the infant mortality rate. They also suggest that results are robust to excluding an outlier, including data on drug treatment admissions and an interaction between imprisonment and crime, and testing for spuriousness. Results further suggest that had the American imprisonment rate remained at the 1990 level, the American infant mortality rate would have been 5.3 percent lower. The primary thrust of this article was to test state-level hypotheses, but I also present estimates of the effects of recent parental incarceration on early infant mortality risk since macro-level effects seem implausible in the absence of micro-level effects. Results from logistic regression models with state and year fixed effects suggest that parental incarceration elevates early infant mortality risk. Furthermore, effects are substantial; infants of recently incarcerated parents had a 29 percent higher chance of dying than comparable infants. Results also indicate that effects are concentrated among infants whose mothers had not been abused and infants whose mothers experienced some other stressful event, suggesting that effects of imprisonment on family life are likely concentrated among those already at risk because of a host of other disadvantages but not involved in domestic violence.

Taken together, results suggest that the American experiment in mass imprisonment may be partially responsible for the distinctively high American infant mortality rate.

These results are provocative, but this research still has a number of limitations. First, it is possible that some omitted (or poorly measured) variable correlated with both imprisonment and infant mortality is driving any observed association between these two variables. Although this is a possibility, the models used throughout this analysis—especially the more rigorous state-level models—diminish this possibility by controlling all selection bias due to stable characteristics. Lack of information about cause of death and the fact that the measure of infant death does not include a full year of exposure to risk in the PRAMS data are also limitations, although not huge ones. Endogeneity bias is also a concern. Although some have estimated incarceration effects using exogenous shocks in imprisonment (Levitt 1996), these analyses have generally relied on exogenous shocks in release rather than admission (but see Johnson and Raphael 2006). Unfortunately, there are a host of reasons to think that it matters less for infant mortality—and other outcomes—*when* an individual is released from prison than *whether* they had ever been imprisoned. As such, the ideal exogenous shock would be in admission rather than release. A final concern is that some unmeasured type of crime (rather than incarceration) is driving the results at the individual-level. Although I was able to include a control for the violent crime rate in the state-level analyses, I was not able to do so for the individual-level analyses. Thus, it remains possible that crime rather than incarceration was driving the observed association.

Despite these limitations, these findings have a number of important implications. First, they suggest that the imprisonment rate may be an important predictor of population health and should be considered in analyses comparing nations, states, and individuals. Although some have suggested that the penal state may have important implications for population health (see

Beckfield and Krieger *Forthcoming*), this is the first study to simultaneously provide evidence at the macro- and micro-levels that demonstrates an association between the penal state and a health outcome. Second, these findings fall in line with a sparse literature that suggests that growth in the American penal system may be one reason why America has diverged from Europe in a host of ways since the 1970s (Western and Beckett 1999). This is not to say that America and Europe would be exactly the same if the American penal state disappeared, of course, but merely to suggest that the penal state is a distinctive feature of American life that creates further differentiation between the United States and Europe (Western and Beckett 1999). Finally, these findings, along with other studies showing that imprisonment may elevate mortality risk (Binswanger et al. 2007), suggest that those considering the costs and benefits of mass imprisonment should include another variable in their analysis: Loss of life for the imprisoned and their family members, including children. This is not to say that mass imprisonment could not save lives as well by removing dangerous individuals from the streets but to provide a reminder that there may be both mortality costs and benefits of the penal state.

Future research in this area should do no less than four things. First, research must try to decipher how long these consequences of the penal state for population health will last. One way to do this is to distinguish between the consequences of the imprisonment rate, which can rise and fall rapidly in response to public policy, and the percentage of the population that has ever been imprisoned, which can only change slowly as ever-imprisoned individuals die off and are replaced by those who have never been imprisoned (and never will be). Future research should also follow in the footsteps of others considering the consequences of mass imprisonment for population health by considering inequality in population health more explicitly (Johnson and Raphael *Forthcoming*; Massoglia 2008b). Future research should also spend more time trying to find out what the mechanisms are through which imprisonment elevates infant mortality risk since knowing this will allow us to minimize the consequences of the penal state for population health, even in the absence of changes in the imprisonment rate. Finally, research should explicitly test the hypothesis that imprisonment has an effect on population health at the national level by considering both of the most recognized measures of population health—the infant mortality rate and life expectancy at birth—in models considering cross-national variation. Whatever the results of these analyses, they will provide valuable insight into the importance of the penal state for population health—both within and between countries.

REFERENCES

Alexander, Greg R., and Carl C. Korenbrot. 1995. "The Role of Prenatal Care in Preventing Low Birth Weight." *The Future of Children* 5:103-120.

Armstrong, Elizabeth M. 2003. *Conceiving Risk, Bearing Responsibility: Fetal Alcohol Syndrome and the Diagnosis of Moral Disorder*. Baltimore, MD: Johns Hopkins University Press.

Beckfield, Jason. 2004. "Does Income Inequality Harm Health? New Cross-National Evidence." *Journal of Health and Social Behavior* 45:231-248.

-----. 2006. "European Integration and Income Inequality." *American Sociological Review* 2006:964-985.

Beckfield, Jason, and Nancy Krieger. Forthcoming. "Epi + demos + cracy: A Critical Review of Empirical Research Linking Political Systems and Priorities to the Magnitude of Health Inequities." *Epidemiologic Reviews*.

Binswanger, Ingrid A., Marc F. Stern, Richard A. Deyo, Patrick Heagerty, Allen Cheadle, Joann

G. Elmore, and Thomas D. Koepsell. 2007. “Release from Prison—A High Risk of Death for Former Inmates.” *New England Journal of Medicine* 356: 157-165.

Boggess, Scott, and John Bound. 1997. “Did Criminal Activity Increase During the 1980s? Comparisons Across Data Sources.” *Social Science Quarterly* 78:725-729.

Bonczar, Thomas. 2003. *The Prevalence of Imprisonment in the U.S. Population, 1974-2001*. Washington, DC: U.S. Department of Justice.

Braman, Donald. 2004. *Doing Time on the Outside: Incarceration and Family Life in Urban America*. Ann Arbor, MI: University of Michigan Press.

Callaghan, William M., Marian F. MacDorman, Sonja A. Rasmussen, Cheng Qin, and Eve M. Lackritz. 2006. “The Contribution of Preterm Birth to Infant Mortality Rates in the United States.” *Pediatrics* 118:1566-1573.

Chomitz, Virginia Rall, Lilian W.Y. Cheung, and Ellice Lieberman. 1995. “The Role of Lifestyle in Preventing Low Birth Weight.” *The Future of Children* 5:121-138.

Comfort, Megan. 2007. “Punishment Beyond the Legal Offender.” *Annual Review of Law and Social Science* 3:271-296.

-----. 2008. *Doing Time Together: Love and Family in the Shadow of the Prison*. Chicago, IL: University of Chicago Press.

Conley, Dalton, and Kristen W. Springer. 2001. “Welfare State and Infant Mortality.” *The American Journal of Sociology* 107:768-807.

Foster, Holly, and John Hagan. 2007. “Incarceration and Intergenerational Social Exclusion.” *Social Problems* 54:399-433.

Geller, Amanda, Irwin Garfinkel, Carey Cooper, and Ronald Mincy. Forthcoming. “Parental Incarceration and Child Wellbeing: Implications for Urban Families.” *Social Science*

Quarterly.

Geronimus, Arline T. 1992. "The Weathering Hypothesis and the Health of African American Women and Infants: Evidence and Speculations." *Ethnicity and Disease* 2:207-221.

Giscombé, Cheryl L., and Marci Lobel. 2005. "Explaining Disproportionately High Rates of Adverse Birth Outcomes among African Americans: The Impact of Stress, Racism, and Related Factors in Pregnancy." *Psychological Bulletin* 131:662-683.

Goffman, Alice. 2009. "On the Run: Wanted Men in a Philadelphia Ghetto." *American Sociological Review* 74:339-357.

Green, Kerry, Margaret Ensminger, Judith Robertson, and Hee-Soon Juon. 2006. "Impact of Adult Sons' Incarceration on African American Mothers' Psychological Distress." *Journal of Marriage and the Family* 68:430-441.

Hagan, John, and Ronit Dinovitzer. 1999. "Collateral Consequences of Imprisonment for Children, Communities, and Prisoners." *Crime and Justice* 26:121-162.

Hummer, Robert A., Daniel A. Powers, Starling G. Pullum, Ginger L. Gossman, and W. Parker Frisbie. 2007. "Paradox Found (Again): Infant Mortality among the Mexican-Origin Population in the United States." *Demography* 44:441-457.

Johnson, Rucker, and Raphael, Steven. 2006. "How Much Crime Reduction Does the Marginal Prisoner Buy?" Unpublished Manuscript.

-----. Forthcoming. "The Effects of Male Incarceration Dynamics on AIDS Infection Rates among African-American Women and Men." *Journal of Law and Economics*.

Kling, Jeffrey. 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review* 96:863-876.

Kramer, Michael S., Louis Séguin, John Lydon, and Lise Goulet. 2000. "Socio-economic

Disparities in Pregnancy Outcome: Why Do the Poor Fare So Poorly?" *Paediatric and Perinatal Epidemiology* 14:194-201.

Levitt, Steven. 1996. "The Effect of Prison Population Size on Crime: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics* 111:319-351.

Lopoo, Leonard, and Bruce Western. 2005. "Incarceration and the Formation and Stability of Marital Unions." *Journal of Marriage and the Family* 67:721-734.

Manza, Jeff, and Christopher Uggen. 2006. *Locked Out: Felon Disenfranchisement and American Democracy*. New York, NY: Oxford University Press.

Marian F. MacDorman, Joyce Martin, T.J. Mathews, Donna Hoyert, and Stephanie Ventura. 2005. "Explaining the 2001-02 Infant Mortality Increase: Data from the Linked Birth/Infant Death Data Set." *National Vital Statistics Reports* 53:12. Hyatsville, MD: National Center for Health Statistics.

Massoglia, Michael. 2008a. "Incarceration as Exposure: The Prison, Infectious Disease, and Other Stress-Related Illnesses." *Journal of Health and Social Behavior* 49:56-71.

-----. 2008b. "Incarceration, Health, and Racial Disparities in Health." *Law & Society Review* 42:275-306.

Mathews, T.J., and Marian F. MacDorman. 2007. "Infant Mortality Statistics from the 2004 Period Linked Birth/Infant Death Data Set." *National Vital Statistics Reports* 55:14. Hyatsville, MD: National Center for Health Statistics.

Milbank Memorial Fund. 1999. *1997 State Health Care Expenditure Report*. New York, NY: Milbank Memorial Fund.

-----. 2001. *1998-1999 State Health Care Expenditure Report*. New York, NY: Milbank Memorial Fund.

-----. 2003. *2000-2001 State Health Care Expenditure Report*. New York, NY: Milbank Memorial Fund.

-----. 2005. *2002-2003 State Health Care Expenditure Report*. New York, NY: Milbank Memorial Fund.

Murray, Joseph, and David P. Farrington. 2008. "Effects of Parental Imprisonment on Children." *Crime and Punishment* 37:133-206.

Nurse, Anne. 2002. *Fatherhood Arrested: Parenting from Within the Juvenile Justice System*. Nashville, TN: Vanderbilt University Press.

OECD. 2006. *OECD Health Data, 2006*. OECD, Paris.

Pager, Devah. 2003. "The Mark of a Criminal Record." *American Journal of Sociology* 108:937-975.

Palloni, Alberto. 2007. "Childhood Health and the Reproduction of Inequalities." *Demography* 43:587-615.

Pettit, Becky, and Bruce Western. 2004. "Mass Imprisonment and the Life Course: Race and Class Inequality in U.S. Incarceration." *American Sociological Review* 69:151-169.

Pollack, Harold A., and John G. Frohna. 2001. "Infant Sleep Placement after the Back to Sleep Campaign." *Pediatrics* 109:608-614.

Schnittker, Jason, and Andrea John. 2007. "Enduring Stigma: The Long-Term Effects of Incarceration on Health." *Journal of Health and Social Behavior* 48:115-130.

Singh, Gopal K., and Michael D. Kogan. 2007. "Persistent Socioeconomic Disparities in Infant, Neonatal, and Postneonatal Mortality Rates in the United States, 1969-2001." *Pediatrics* 119:2929-e939.

Singh, Gopal K., and Stell M. Yu. 1995. "Infant Mortality in the United States: Trends, Differentials, and Projections, 1950-2010." *American Journal of Public Health* 85:957-964.

Swann, Christopher, and Michelle Sheran Sylvester. 2006. "The Foster Care Crisis: What Caused Caseloads to Grow?" *Demography* 43:309-335.

West, Heather C., and William J. Sabol. 2009. *Prison Inmates at Midyear 2008 – Statistical Tables*. NCJ 225619. Washington, DC: Bureau of Justice Statistics.

Western, Bruce. 2002. "The Impact of Incarceration on Wage Mobility and Inequality." *American Sociological Review* 67:477-498.

-----. 2006. *Punishment and Inequality in America*. New York, NY: Russell Sage Foundation.

Western, Bruce, and Katherine Beckett. 1999. "How Unregulated is the U.S. Labor Market? The Penal System as a Labor Market Institution." *The American Journal of Sociology* 104:1030-1060.

Western, Bruce, and Christopher Wildeman. 2009. "The Black Family and Mass Incarceration." *Annals of the American Academy of Political and Social Science* 621:221-242.

Wildeman, Christopher. 2009. "Parental Imprisonment, the Prison Boom, and the Concentration of Childhood Disadvantage." *Demography* 46:265-280.

Wise, Paul H. 2003. "The Anatomy of a Disparity in Infant Mortality." *Annual Review of Public Health* 24:341-362.

Table 1

1990 AND 2003 INFANT MORTALITY RATES AND ABSOLUTE AND RELATIVE CHANGE IN INFANT MORTALITY RATES BETWEEN 1990 AND 2003

Country	IMR (1990)	IMR (2003)	Absolute Change (%)	Relative Change (%)
Australia	8.2	4.8	-3.4	-41.6
Austria	7.8	4.5	-3.3	-42.3
Belgium	6.5	4.3	-2.2	-33.8
Canada	6.8	5.3	-1.5	-22.1
Czech Republic	10.8	3.9	-6.9	-63.9
Denmark	7.5	4.4	-3.1	-41.3
Finland	5.6	3.1	-2.5	-44.6
France	7.3	4.0	-3.3	-45.2
Germany	7.0	4.2	-2.8	-40.0
Greece	9.7	4.0	-5.7	-58.8
Hungary	14.8	7.3	-7.5	-50.7
Iceland	5.8	2.4	-3.4	-58.6
Ireland	8.2	5.3	-2.9	-35.4
Italy	8.2	3.9	-4.3	-52.4
Japan	4.6	3.0	-1.6	-34.8
Korea	10.0	5.3	-4.7	-47.0
Luxembourg	7.3	4.9	-2.4	-32.9
Mexico	36.2	20.5	-15.7	-43.4
Netherlands	7.1	4.8	-2.3	-32.4
New Zealand	8.4	4.9	-3.5	-41.7
Norway	6.9	3.4	-3.5	-50.7
Poland	19.3	7.0	-12.3	-63.7
Portugal	11.0	4.1	-6.9	-62.7
Slovak Republic	12.0	7.9	-4.1	-34.2
Spain	7.6	3.9	-3.7	-48.7
Sweden	6.0	3.1	-2.9	-48.3
Switzerland	6.8	4.3	-2.5	-36.8
Turkey	55.4	28.7	-26.7	-48.2
United Kingdom	7.9	5.3	-2.6	-32.9
United States	9.2	6.9	-2.3	-25.0
Entire OECD	11.0	6.0	-5.0	-45.5

SOURCE: OECD (2006).

NOTES: All infant mortality rates are expressed per 1,000 live births. Since Korea did not report an infant mortality rate in 2003, I rely on the 2002 infant mortality rate for Korea.

Table 2

DESCRIPTIVE STATISTICS AND SOURCES FOR VARIABLES USED IN STATE-LEVEL ANALYSES (N=714), 1990-2003

Variable	M	(SD)	Source
Infant Mortality Rate (per 1,000)	7.8	(1.9)	National/Monthly Vital Statistics Reports
Female Imprisonment Rate (per 1,000)	0.2	(0.2)	Bureau of Justice Statistics
Male Imprisonment Rate (per 1,000)	3.4	(2.2)	Bureau of Justice Statistics
Imprisonment Rate (per 1,000)	1.8	(1.2)	Bureau of Justice Statistics
Violent Crime Rate (per 1,000)	5.2	(3.5)	Uniform Crime Reports
Percent Foreign-Born	6.5	(5.3)	Statistical Abstracts of the U.S.
Percent with High School Diploma Plus	81.9	(5.6)	Statistical Abstracts of the U.S.
Percent Black	11.0	(11.8)	Statistical Abstracts of the U.S.
Percent Hispanic	7.0	(8.3)	Statistical Abstracts of the U.S.
Percent Residing in Urban Areas	71.9	(15.0)	Statistical Abstracts of the U.S.
GDP per Capita (in $1,000s; 2000 dollars)	31.4	(11.4)	U.S. Bureau of Economic Analysis
GINI	.4	(0.0)	Census/American Community Survey
AFDC/TANF Cases (per 1,000)	11.9	(6.7)	U.S. Department of Health/Human Services
AFDC/TANF + Food Stamp (per month in $100s; 2000 dollars)	7.7	(1.7)	U.S. House of Representative *Green Books*
Percent Nonmarital Births	31.5	(7.5)	National/Monthly Vital Statistical Reports
Percent of the Population in Poverty	12.7	(3.9)	Statistical Abstracts of the United States
Unemployment Rate	5.2	(1.5)	Bureau of Labor Statistics
Doctors (per 1,000)	2.3	(0.8)	Statistical Abstracts of the U.S.
Nurses (per 1,000)	8.0	(2.0)	Statistical Abstracts of the U.S.
Percent Whose Mothers Smoked	15.4	(4.9)	National/Monthly Vital Statistics Reports
Percent with No Prenatal Care	4.1	(2.0)	National/Monthly Vital Statistics Reports
Percent of Births Premature	11.3	(2.0)	National/Monthly Vital Statistics Reports
Percent of Births Low Birth Weight	7.5	(1.5)	National/Monthly Vital Statistics Reports
Drug Treatment Admissions (per 1,000)	8.4	(4.4)	Treatment Episode Data (TEDS)

NOTE: Due to variation in reporting rates by state and year, Drug Treatment Admissions are not available for all state-years. For states and years reporting, see Table A1.

Table 3

DESCRIPTIVE STATISTICS FOR VARIABLES USED IN SUBSEQUENT ANALYSES BY PARENTAL INCARCERATION STATUS USING PRAMS DATA, 1998-2003

	Parental Incarceration		No Parental Incarceration	
	M	*(SD)*	*M*	*(SD)*
Dependent Variables				
Early Infant Mortality (%)	0.8	(0.9)	0.5	(7.1)
Controls				
Months between Birth and Interview (0-19)	4.0	(1.2)	4.1	(1.3)
Mother Married (%)	24.1	(42.8)	44.9	(49.7)
Maternal Race (%)				
White	51.0	(50.0)	47.7	(49.9)
Black	32.0	(46.7)	26.5	(44.1)
Hispanic	12.3	(32.8)	21.1	(40.9)
Other	4.7	(21.2)	4.7	(21.1)
Maternal Education (%)				
< HS	40.8	(49.1)	32.9	(47.0)
HS +	59.2	(49.1)	67.1	(47.0)
Maternal Age	22.9	(5.2)	24.4	(5.7)
Maternal BMI (%)				
Underweight	18.1	(38.5)	14.6	(35.3)
Healthy or Overweight	59.7	(49.0)	63.8	(48.1)
Obese	22.1	(41.5)	21.6	(41.2)
Total Stressful Experiences (0-3)	1.0	(0.9)	0.4	(0.6)
Previous Births (0-18)	1.1	(1.3)	1.0	(1.3)
Previous Low Birthweight Birth (%)	8.0	(27.1)	7.6	(26.5)
Previous Preterm Birth (%)	9.2	(28.9)	7.6	(26.5)
Boy (%)	51.6	(50.0)	51.3	(50.0)
Mother Smoked (%)	30.1	(45.9)	16.9	(37.5)
Mother Drank (%)	1.6	(12.4)	0.7	(8.2)
Number of Prenatal Visits (0-81)	10.5	(4.5)	11.0	(4.3)
This Birth Low Birthweight (%)	8.4	(27.8)	7.2	(25.9)
This Birth Very Low Birthweight (%)	1.2	(10.9)	1.2	(11.0)
This Birth Preterm (%)	10.2	(30.2)	8.6	(28.1)
Child in Intensive Care (%)	11.7	(32.1)	11.2	(31.6)
Mother Reported Abuse (%)	40.0	(49.0)	11.2	(32.2)
N	8,443	---	86,114	---

NOTE: All descriptive statistics are weighted. The sample is limited to women who were on WIC at the time of the child's birth.

SOURCE: Pregnancy Risk Assessment Monitoring System, 1998-2003.

Table 4

RESULTS FROM OLS FIXED EFFECTS MODELS WITH STATE AND YEAR FIXED EFFECTS AND AN ADJUSTMENT FOR SERIAL AUTOCORRELATION PREDICTING INFANT MORTALITY RATES BY IMPRISONMENT RATES, 1990-2003

	M1	M2	M3	M4	M5	M6
Female Imprisonment	4.21***	---	---	2.08***	---	---
Male Imprisonment	---	.58***	---	---	.27***	---
Total Imprisonment	---	---	1.07***	---	---	.50***
Violent Crime Rate	---	---	---	.10*	.15**	.14**
Percent Foreign-Born	---	---	---	-.18	-.14	-.14
Percent with HS Plus	---	---	---	.02	.01	.01
Percent Black	---	---	---	.29***	.28*	.28*
Percent Hispanic	---	---	---	.08	.08	.08
Percent Urban	---	---	---	-.04	-.05	-.04
GDP	---	---	---	-.07#	-.08*	-.08#
GDP * GDP	---	---	---	.00	.00	.00
GINI	---	---	---	-4.35	-10.07	-9.42
AFDC/TANF Cases	---	---	---	-.02	-.01	-.01
AFDC/TANF Benefit	---	---	---	-.02	-.07	-.06
Percent Nonmarital	---	---	---	.01	.00	.00
Percent in Poverty	---	---	---	-.02	-.02	-.02
Unemployment Rate	---	---	---	.01	.00	.01
Doctors	---	---	---	.03	.29	.29
Nurses	---	---	---	-.33*	-.31*	-.31*
Mother Smoked	---	---	---	-.01	.01	.01
Percent No Prenatal	---	---	---	.09#	.09#	.09#
Percent Premature	---	---	---	.20**	.21**	.21**
Percent Low Bw	---	---	---	.03	.02	.02
State/Year FE	YES	YES	YES	YES	YES	YES
AR(1) Adjustment	YES	YES	YES	YES	YES	YES
Intercept	8.20**	7.24***	7.29***	10.08	13.00#	12.67#
R-Squared	.44	.58	.57	.69	.71	.71
N	663	663	663	663	663	663

NOTE: All t-tests are one-sided. Standard errors are omitted to conserve space.
SOURCES: Various.
# $p < .10$
* $p < .05$
** $p < .01$
*** $p < .001$

Table 5

RESULTS FROM OLS FIXED EFFECTS MODELS WITH STATE AND YEAR FIXED EFFECTS AND AN ADJUSTMENT FOR SERIAL AUTOCORRELATION PREDICTING INFANT MORTALITY RATES BY IMPRISONMENT RATES, 1990-2003

	M1	M2	M3	M4	M5	M6	M7	M8
Imprisonment Rate (t-1)	.40**	.38*	1.08***	.92***	.48**	.45**	.81***	.49**
Imprisonment Rate (t+1)	---	---	---	---	---	---	.41*	.03
Violent Crime Rate	---	.10*	---	.19**	.10#	.12*	---	.14**
Imprisonment * Crime	---	---	---	---	.02**	.01	---	---
Drug Admissions	---	---	---	-.03	---	---	---	---
Includes State/Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Includes AR(1) Adjustment	YES	YES	YES	YES	YES	YES	YES	YES
Includes All Controls	NO	YES	NO	YES	NO	YES	NO	YES
Omits D.C.	YES	YES	NO	NO	NO	NO	NO	NO
Drug Admission Years Only	NO	NO	YES	YES	NO	NO	NO	NO
Intercept	8.18**	15.31*	4.58***	8.92*	7.39***	12.55#	7.04***	12.53#
R-Squared	.32	.38	.46	.64	.58	.72	.58	.71
N	650	650	490	490	663	663	663	663

NOTE: All t-tests are one-sided. Standard errors are omitted to conserve space. Models include the same controls as those included in Table 3.

SOURCES: Various.

# $p < .10$

* $p < .05$

** $p < .01$

*** $p < .001$

Table 6

RESULTS FROM LOGISTIC REGRESSION MODELS WITH STATE AND YEAR FIXED EFFECTS PREDICTING EARLY INFANT DEATH, 1998-2003

	M1	M2	M3	M4	M5	M6
Parental Incarceration	.44***	.27*	.13	.37*	.51***	-.18
Months	---	.02#	-.04	.04	.15*	-.05
Mother Married	---	-.26*	-.42	-.22#	-.33#	-.22
Maternal Race						
Black	---	-.04	.15	-.09	-.05	-.00
Hispanic	---	-.29#	-.99*	-.15	-.48#	-.18
Other	---	.05	-.15	.12	.19	-.11
Maternal Ed. < HS	---	-.31**	-.14	-.36**	-.44*	-.21
Maternal Age	---	-.02*	.01	-.03*	-.03#	-.01
Maternal BMI						
Underweight	---	.11	.50#	-.01	.82***	-.51*
Obese	---	.14	.26	.10	.38*	-.00
Stressful Experiences	---	.04	-.23	.12	---	---
Previous Births	---	.08*	.09	.08	.14*	.03
Previous Low	---	-.31#	-.42	-.22	-.62*	-.05
Previous Preterm	---	.33*	.47	.25	.58*	.11
Boy	---	.03	-.09	.05	-.05	.09
Mother Smoked	---	.04	.07	.04	-.05	.05
Mother Drank	---	-.65	-.75	-.60	-.93	-.37
Prenatal Visits	---	-.07***	-.14***	-.05***	-.06***	-.07***
This Birth Low	---	1.25***	1.19***	1.26***	1.08***	1.33***
This Birth Very Low	---	3.41***	3.31***	3.47***	3.50***	3.43***
This Birth Preterm	---	.31#	-.14	.44*	-.05	.52*
Intensive Care	---	-.86***	-.71*	-.91***	-.40	-1.10***
Abuse	---	.29*	---	---	.23	.31#
Intercept	-5.43***	-5.39***	-3.30***	-5.94***	-5.71***	-5.26***
-2 Log Likelihood	6237	4744	871	3806	1840	2823
N	94557	94557	13460	81021	34847	59625

NOTE: All t-tests are one-sided. Standard errors are omitted to conserve space. All models include state and year fixed effects. All analyses are weighted and limited to women who were on WIC at the time of the child's birth. Models 3 and 5 lacked sufficient variation in Montana, resulting in that state being dropped from the analysis. Models 4 and 6 lacked sufficient variation in North Dakota, causing that state to be dropped from the analysis. After weighting, dropping these two states from the analysis results in a loss of only .18 percent of the total cases.

SOURCES: Pregnancy Risk Assessment Monitoring System.

\# $p < .10$
* $p < .05$
** $p < .01$
*** $p < .001$

Table A1

AVAILABILITY OF PREGNANCY RISK ASSESSMENT MONITORING SYSTEM (PRAMS) DATA AND TREATMENT EPISODE (TEDS) DATA BY STATE AND YEAR (* = PRAMS; # = TEDS)

Year	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
Alabama				* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
Alaska		*	*	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
Arizona									#	#	#	#	#	#
Arkansas				#	#	#	#	* #	* #	* #	* #	* #	* #	* #
California				#	#	#	#	#	#	#	#	#	#	#
Colorado				#	#	#	#	#	* #	* #	* #	* #	* #	* #
Connecticut				#	#	#	#	#	#	#	#	#	#	#
Delaware				#	#	#	#	#	#	#	#	#	#	#
D.C.					#	#	#	#	#	#	#	#	#	#
Florida				* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
Georgia				* #	* #	* #	* #	* #	#	#	#	#	#	#
Hawaii				#	#	#	#	#	#	#	* #	* #	* #	* #
Idaho				#	#	#	#	#	#	#	#	#	#	#
Illinois				#	#	#	#	* #	* #	* #	* #	* #	* #	* #
Indiana				#	#	#	#		#	#	#	#	#	#
Iowa				#	#	#	#	#	#	#	#	#	#	#
Kansas				#	#	#	#	#	#	#	#	#	#	#
Kentucky								#	#	#	#	#	#	#
Louisiana				#	#	#	#	#	* #	* #	* #	* #	* #	* #
Maine	*	*	*	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
Maryland				#	#	#	#	#	#	#	#	#	#	#
Massachusetts				#	#	#	#	#	#	#	#	#	#	#
Michigan				* #	* #	* #	* #	#	#	#	#	* #	* #	* #
Minnesota				#	#	#	#	#	#	#	#	#	* #	* #
Mississippi						#	#	#	#	#	#	#	#	* #
Missouri				#	#	#	#	#	#	#	#	#	#	#
Montana				#	#	#	#	#	#	#	#	#	#	#
Nebraska				#	#	#	#	#	#	#	#	#	* #	* #
Nevada				#	#	#	#	#	#	#	#	#	#	#
New Hampshire				#	#	#	#	#	#	#	#	#	#	#
New Jersey				#	#	#	#	#	#	#	#	#	* #	* #

New Mexico				#	#	#	#	#	* #	* #	* #	* #	* #	* #
New York				* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
North Carolina				#	#	#	#	* #	* #	* #	* #	* #	* #	* #
North Dakota				#	#	#	#	#	#	#	#	#	* #	#
Ohio				#	#	#	#	#	#	* #	* #	* #	* #	* #
Oklahoma		*	*	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
Oregon				#	#	#	#	#	#	#	#	#	#	* #
Pennsylvania				#	#	#	#	#	#	#	#	#	#	#
Rhode Island				#	#	#	#	#	#	#	#	#	* #	* #
South Carolina				* #	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
South Dakota				#	#	#	#	#	#	#	#	#	#	#
Tennessee				#	#	#	#	#	#	#	#	#	#	#
Texas				#	#	#	#	#	#	#	#	#	#	#
Utah				#	#	#	#	#	#	* #	* #	* #	* #	* #
Vermont				#	#	#	#	#	#	#	#	* #	* #	* #
Virginia				#	#	#	#	#	#	#	#	#	#	#
Washington				#	* #	* #	* #	* #	* #	* #	* #	* #	* #	* #
West Virginia	*	*	*	* #	*	* #	* #	*	*	* #	*	* #	* #	*
Wisconsin				#	#	#	#	#	#	#	#	#	#	#
Wyoming				#	#			#	#	#	#	#	#	#

NOTE: Although PRAMS data were also available for Maryland for some years, data from Maryland are not included because the state did not approve use of the data for this project. The analyses presented in this manuscript are limited to the 1998-2003 period.