

MOTHERHOOD AND CRIMINAL DESISTANCE IN DISADVANTAGED NEIGHBORHOODS*

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Evidence from several qualitative studies has suggested that the transition to motherhood has strong inhibitory effects on the delinquency and drug use trajectories of poor women. Quantitative studies, however, typically have failed to find significant parenthood or motherhood effects. We argue that the latter research typically has not examined motherhood in disadvantaged settings or applied the appropriate statistical method. Focusing on within-individual change, we test the motherhood hypothesis using data from a 10-year longitudinal study of more than 500 women living in disadvantaged Denver communities. We find

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that the transition to motherhood is associated significantly with reductions in delinquency, marijuana, and alcohol behaviors. Moreover, we find that the effect of motherhood is larger than that of marriage for all outcomes. These results support the qualitative findings and suggest that the transition to motherhood—and not marriage—is the primary turning point for disadvantaged women to exit delinquent and drug-using trajectories.

Drawing on the work of Elder and colleagues (1985, 1986; Caspi, Elder, and Herbener, 1990), Sampson and Laub's (1993) age-graded theory of informal social control firmly established the life-course perspective in criminology. The life-course framework emphasizes both behavioral stability and change along with the role of life-course transitions—such as marriage, employment, and military service—on patterns of offending across time. In support of this perspective, research has suggested that transitions to marriage (King, Massoglia, and MacMillan, 2007; Sampson, Laub, and Wimer, 2006), employment (Sampson and Laub, 1993; Uggen, 2000), and military service (Sampson and Laub, 1996) can be key turning points in the lives of individuals, which leads them to desist from crime.

An important theme of life-course theory is that the meaning of a life transition is likely to vary substantially by social context and the timing of the transition (Elder, 1985; Giordano, Cernkovich, and Rudolph, 2002; Matsueda and Heimer, 1997). News of a pregnancy can be an exhilarating and fulfilling event for a married adult woman trying to start a family, or it can be a horrifying occasion for an unmarried teenage girl hoping to graduate from high school. Similarly, joining the military might be a lifelong dream for a young man aspiring to become a career officer, or it might be a last chance for a jobless teen with nothing else to lose. Such differences in meaning likely shape the consequences of the event, which culminates in disparate individual outcomes. For example, an unexpected teen pregnancy might result in one of several outcomes—including a “shotgun” wedding, single motherhood, adoption, or abortion—depending on the mother's social contexts, life circumstances, and personal preferences. Moreover, “having a child may have very different meanings for an impoverished inner-city family than an affluent family” (Matsueda and Heimer, 1997: 179).

Qualitative research provides a window into the contextualized meanings ascribed to life-course events and relates those meanings to individual behavioral outcomes. In this vein, recent ethnographies identify an important pattern of family formation in urban contexts; young women from disadvantaged communities often view motherhood as an essential transition into adulthood but at the same time see marriage as a potentially unattainable, although highly prized, long-term goal (Edin and Kefalas,

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2005). By prioritizing motherhood before marriage, a critical mass of young women foster local support networks for unwed mothers (Anderson, 1999; Stack, 1974). An important implication of these findings is that, for disadvantaged urban women, the transition to parenthood might be a more salient life-course turning point than the transition to marriage. Indeed, in their interviews of 162 poor single mothers living in disadvantaged Philadelphia neighborhoods, Edin and Kefalas (2005) time-and-again documented the positive effects of childbirth on the perceived behavioral trajectories of women. Respondents reported that having a child motivated them to stay in school and work hard for a career, with the eventual hope of attracting a stably employed man and living the “white-picket-fence dream.” Moreover, Edin and Kefalas found a strong link between motherhood and criminal desistance. The disadvantaged mothers in their sample repeatedly stated that childbirth pulled them away from delinquent and drug behaviors and pulled them into conventional adult activities.

This article tests the hypothesis, derived from ethnographic research, that motherhood is accompanied by reductions in delinquency and drug use in disadvantaged inner-city neighborhoods. Rather than relying on the retrospective accounts of mothers, we used prospective longitudinal data on samples of disadvantaged inner-city women residing in Denver. Previous research using samples of broad populations has produced equivocal results on the motherhood–deviance connection. This could be because of treatment group heterogeneity in which motherhood has a transforming effect only in disadvantaged neighborhoods, has no effect in affluent neighborhoods, and therefore, has an illusive effect in broad populations that average across all neighborhoods. Or it could be because of weak research designs and analytical models. We used fixed-effects models of within-individual change to control for unobserved individual characteristics that might affect both childbirth and criminal propensity. We also disentangled the effects of the transition to motherhood from those of subsequent childbirths and from the potentially biasing effects of time-varying covariates, pregnancy, and marriage. Results from this study illuminate the lives of disadvantaged women and extend the burgeoning area of life-course research by focusing on the relatively understudied topic of motherhood and crime (Wakefield and Uggen, 2004).

MOTHERHOOD IN DISADVANTAGED COMMUNITIES: ETHNOGRAPHIC EVIDENCE

In their influential ethnographies, Anderson (1992, 1999) as well as Edin and Kefalas (2005) provided compatible accounts of family life in the

disadvantaged neighborhoods of Philadelphia. Anderson (1992, 1999) described the “mating game” common to poor inner-city areas where the competing expectations of young men and women result in many teenage pregnancies and unwed mothers. Accordingly, pressures from peers and the search for street status encourage young men to “get over” on young women by overcoming women’s defenses and gaining sexual access. At the same time, many young women hold the dream of landing a “decent daddy” and acquiring a middle-class lifestyle with an employed husband and beautiful children. The women, thus, are inclined to believe the “rap” of a young man and attempt to nurture loving and long-term heterosexual relationships. Competing sexual demands between partners who are often young, naïve, and uneducated result in high rates of unprotected sex and teenage pregnancy. Moreover, when a young woman realizes she is pregnant, she often finds that her young man has chosen to continue running with his peer group instead of “playing house” with the expectant mother. With dashed dreams of finding a good husband, provider, and father for her child, the young woman often turns to like-minded pregnant teens and single mothers for social support, which promotes the construction of a new identity as an ideal and doting mother. A child, and not marriage, becomes a symbol of maturity and the key to accessing neighborhood “baby clubs” for young disadvantaged mothers and their children (Anderson, 1999).

Edin and Kefalas (2005) covered similar ground as Anderson (1992, 1999) but extended his work with detailed accounts of the transformative power of motherhood in the lives of poor urban women. By using in-depth interviews of poor single mothers, Edin and Kefalas found that the attitudes of women toward childbirth diverge dramatically by socioeconomic class. With limited opportunities for upward mobility or for meeting the ideal man, poor young women prioritize the fulfillment of motherhood ahead of schooling, career, and marriage. Indeed, many of the interviewed women stated that having a child motivated them to stay in school and work hard for a career with the hope of attracting an ideal mate at some future date.

Edin and Kefalas (2005) documented a strong link between motherhood and criminal desistance in their sample of poor single women. Moreover, they used the words of the mothers to illuminate the potential mechanisms linking childbirth to desistance. Several women stated that having children restructured their daily routines and reduced criminal opportunities (see also Laub and Sampson, 2003).

I used to be *bad*, go around breaking windows and stuff. Now I don’t do *nothing*. I be with *her* all day. I come to school, go home, be with her. (Adlyn, 2005: 180)

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Before I was pregnant with him, I was doing *real* bad in school. I'm a real smart person, but I was messing up in school . . . smoking marijuana, drinking beer. . . . Sometimes I didn't come home for four or five days, didn't even see my mom or nothing. Then I got *him*, and my life change. (Madeline, 2005: 181)

Other women stated that having a child increased their "stakes in conformity" (Briar and Piliavin, 1965) by providing them with a purpose and helping them focus on making better lives for themselves and their children.

Before I got pregnant, I was like smoking a lot of weed. And like drinking and taking pills. And I realized it was getting me nowhere, because I was losing my friends, and I was like losing my self-esteem. . . . [Now] I try to stay focused on what I want in life and for my kids. . . (Denise, 2005: 181)

Before, I didn't have nothing to go home for. Now I have my son to take care of, I have him to go home for. I don't have to go buy weed or drugs with money, I could buy my *son* stuff with my money. I have something to look up to now. (Jen, 2005: 173)

Motherhood also prompted a shift in the identities of the women as well as in their senses of maturity.

Maybe I needed my kids [to keep me safe]. They come first. I've always stayed off of drugs for them, and they helped me grow up. . . . I can't picture myself without them. (Millie, 2005: 170)

Finally, at least one mother mentioned the increased costs of crime that accompany motherhood.

I didn't want to get in trouble and then DHS come and take her away from me. (Adlyn, 2005: 180)

The statements of the women represent "testimonials to motherhood's transforming influence, leading [the women] to abandon their 'drinking and drugging,' to trade a wild life for one spent at home, to return to school, pursue employment, reconnect with family, and to find a new sense of hope and purpose" (Edin and Kefalas, 2005: 184). In contrast to the general sense of the public that early childbirth derails the educational and economic potential of poor women, Edin and Kefalas suggested that motherhood represents a beneficial change in the lives of poor young women and pulls them away from delinquent street cultures into conventional roles with added meaning. Lives of hopelessness, loneliness, and instant gratification are replaced with a sense of purpose, self-respect, and optimism toward the future.

Qualitative studies of female gang membership have corroborated Edin and Kefalas's (2005) findings. Using interview data, Hunt, Joe-Laidler, and

MacKenzie (2005) explored the drinking behaviors of 118 female gang members and found that pregnancy and motherhood substantially altered the trajectories of alcohol use in their sample of "homegirls." Prior to pregnancy, most female gang members reported using alcohol at regular intervals, typically at house parties and in other peer contexts. During pregnancy, however, approximately 85 percent of the women completely abstained from alcohol, and "the majority of their time was now spent at home, a dramatic switch from the hustle and excitement of street life" (Hunt, Joe-Laidler, and MacKenzie, 2005: 358). Although many of these women returned to drinking after giving birth, the amount they drank dropped markedly, and they tended to drink alone to relax rather than drinking socially for fun and companionship. Similarly, in their study of 74 gang women, Fleisher and Klienart (2004) found that among the 41 women who became inactive in the gang, two fifths cited pregnancy as the reason for their desistance. The movement from street to home was accompanied by new social networks of friendships, kinship, and community ties, all of which helped insulate the women from their gang ties.

TEENAGE MOTHERHOOD AND LIFE CHANCES

These qualitative results are consistent with a long line of family research examining the economic consequences of single motherhood for disadvantaged teenagers. Sparked by several controversial government reports (Hayes, 1987; Moynihan, 1965) and rising public concerns about urban poverty, the plight of unmarried teenage mothers became a highly politicized issue beginning in the 1970s (Furstenberg, 2003). A wave of research on teenage motherhood followed, as researchers used increasingly complex study designs to separate the effects of poverty and the pre-existing traits of women from the effects of teen childbearing (Hoffman, 1998).

Thirty years of research shows that teenage motherhood is not as destructive for the lives of young women as popularly believed (Booth, Rustenbach, and McHale, 2008; Furstenberg, 2003; Geronimus, 2003; Hoffman, 1998). Teen mothers are more likely than their peers to be economically disadvantaged adults, but this outcome is primarily because of their socioeconomic backgrounds and other individual characteristics, not because of the event of having a child during their teenage years. Women who have children as teenagers likely would be no worse off had they not had children. For example, Furstenberg and colleagues (Furstenberg, 1976, 2003; Furstenberg, Brooks-Gunn, and Morgan, 1987) examined teenage motherhood among a sample of urban Black mothers living in Baltimore and found that when the mothers were in their mid-30s they fared just as well, if not better, than women from similar backgrounds who did not have teenage births. More than three quarters of the teenage mothers were in

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the labor force, and most had graduated from high school or earned their GEDs, “hardly living up to the public stereotype of teenage mothers” (Furstenberg, 2003: 30). Other studies isolated the effect of teen childbearing by comparing sisters who had first births at different ages (Geronimus and Korenman, 1992), comparing teenagers who had given birth to those who had miscarried (Hotz, McElroy, and Sanders, 1997), and comparing teenagers who had given birth to twins to those who had only one child (Grogger and Bronars, 1993). These studies consistently found that teenage childbirth had minimal consequences for socioeconomic outcomes (Geronimus and Korenman, 1992; Grogger and Bronars, 1993) or might even have had economic benefits for poor inner-city women (Hotz, McElroy, and Sanders, 1997).

MOTHERHOOD, DELINQUENCY, AND DRUG USE: RECENT QUANTITATIVE RESEARCH

The proposition that becoming a mother can benefit the lives of disadvantaged women, including reductions in drug use and delinquency, is both provocative and significant. An important question is whether this hypothesis, induced from qualitative research, would receive support when tested using a quantitative design that can address potential threats to validity. For example, some researchers, including most economists, have questioned the veracity of retrospective accounts of behavioral motivations as the basis of scientific explanation. According to this argument, the claims of subjects that having a child transformed their lives and drew them away from lives of drugs and crime might simply be post hoc embellishments that justify decisions to give birth and keep the child.

An empirical test of the motherhood hypothesis should meet at least three conditions. First, the sample should be drawn from disadvantaged communities similar to those observed by urban ethnographers. The causality literature refers to “treatment effect heterogeneity” in which a treatment has heterogeneous effects in different subpopulations, such as advantaged versus disadvantaged neighborhoods (e.g., Angrist, 2004). Because research has suggested heterogeneity in motherhood effects, we restricted our analysis to the specified population (i.e., disadvantaged communities) where motherhood was expected to reduce crime and drug use. Second, the test should examine temporal changes in the effects of childbearing to rule out the possibility of short-term effects or effects that are because of pregnancy and not childbirth. Third, the test should include a comparison group to approximate the counterfactual condition. Some prior research has compared mothers with nonmothers after controlling statistically for stable characteristics that might select for motherhood and illicit behavior. Such research has assumed that nonrandom selection is controlled adequately by including observed time-invariant covariates into

the model. A better strategy would be to compare a random sample of women who became mothers and examine their illicit behavior before and after childbirth using models for within-individual variation. Here, the counterfactual condition is approximated by the comparison group of the same women before they become mothers. Because the comparison group consists of the identical women, all time-stable characteristics that might select for motherhood are controlled.

Previous quantitative studies have addressed some, but not all, of the conditions necessary to test the disadvantaged motherhood hypothesis. Consequently, results from this research have remained equivocal and even contradictory (Siennick and Osgood, 2008). Some studies have found that delinquent behaviors, particularly drug and alcohol use, dropped precipitously during pregnancy but then increased after childbirth to prepregnancy levels. This result has been found when comparing the behaviors of teenage mothers before and after childbirth (Gilchrist et al., 1996; Morrison, Spencer, and Gillmore, 1998) as well as in a national sample of high-school seniors using fixed-effects models (Bachman et al., 1997). Because these studies either have failed to draw a random sample of disadvantaged women and follow the process of becoming a mother (e.g., Gilchrist et al., 1996; Morrison, Spencer, and Gillmore, 1998) or have failed to examine the subgroup of disadvantaged youth (e.g., Bachman et al., 1997), they only indirectly have addressed the disadvantaged motherhood hypothesis.

Research on more general populations also has not found strong inhibitory effects of parenting on offending. In a national sample of Dutch respondents, Blokland and Nieuwebeerta (2005) used retrospective self-report survey data to estimate within-individual effects of parenthood on crime. They found that, net of marital status, parenthood predicted a nonsignificant decrease in delinquency, and single-parenthood was associated with nonsignificant increases in the same outcome. A limitation of the Blokland and Nieuwebeerta (2005) study was that the authors did not report interactions between parenthood and gender, which made it impossible to determine the net effect of *motherhood* on criminal desistance. Recent research that overcame this limitation by sampling disadvantaged women has continued to suggest that motherhood does not necessarily reduce crime. Giordano, Cernkovich, and Rudolph (2002) sampled 127 female prison inmates and found that, net of marriage, motherhood had no significant relationship with criminal desistance. Nevertheless, like Edin and Kafalas (2005), in their qualitative narratives they found evidence that children were mentioned as prominent "hooks for change" in the incarcerated women's lives (27 percent).

In one of the few studies that found support for the motherhood hypothesis, Uggen and Kruttschnitt (1998) used panel data from the

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National Supported Work Demonstration, estimated hazard models of illegal earnings, and found that mothers were less likely than nonmothers to enter periods of illegal earnings (see also Graham and Bowling, 1995). Even these results, however, remain contested. By using the same data set, Wakefield and Uggen (2004) replicated Uggen and Kruttschnitt's (1998) findings but then estimated within-individual effects of motherhood as well as illicit earnings and found that transitioning into motherhood significantly *increased* illegal earnings.

Prior quantitative evidence, thus, provides little, if any, support for the qualitative findings of Edin and Kafalas (2005), Giordano, Cernkovich, and Rudolph (2002), and others (Fleisher and Klienart, 2004; Hunt, Joe-Laidler, and MacKenzie, 2005; Laub and Sampson, 2003). Why the discrepancies? It could be that the qualitative results simply failed to stand up to rigorous quantitative test—either the effects were zero in the population or were too weak to detect—and critics of the qualitative results were correct. Alternatively, it could be that prior studies did not adequately test the specific hypotheses presented by the qualitative researchers. Absent a sample of disadvantaged women and sophisticated quantitative methods, it remains unclear whether a transition to motherhood for young women from disadvantaged areas is associated with desistance from crime and drug use.

THE CURRENT STUDY

In this article, we provide a direct test of the effect of motherhood on the delinquent and drug trajectories of disadvantaged women. With longitudinal data of more than 500 multi-ethnic women residing in disadvantaged Denver neighborhoods, we examined within-individual changes in drug use and delinquent behaviors. To isolate the effect of motherhood, we distinguished the effects of age, pregnancy, sexual activity, contraceptive use, marriage, and motherhood on individual behavioral trajectories.

Our analyses overcame several limitations common to previous studies. First, unlike most prior studies, which used general samples of men and women, we used a multi-ethnic sample of women from impoverished neighborhoods at risk of crime and drug use. Our study, thus, targeted the population and contexts where motherhood was believed to have beneficial effects on the behavioral trajectories of young women.

Second, our study used data collected from an accelerated longitudinal design covering an extended time period (12 years) and a wide age range (10–27 years old). These data provided many individual observations—that is, person-years—that covered the peak periods of delinquency and fertility, which is important to estimate within-individual or fixed-effects models. In addition, data covering adolescence and young adulthood

allowed us to explore potential interactions between age and motherhood. Do motherhood effects on delinquency and drug use differ in the teenage years versus the young-adult years? Little theory was available to guide us here, but the policy implications and public perceptions of teenage childbearing made this an important issue. It is commonly assumed (although generally not supported by research) that “off-time” teenage motherhood derails the life chances of women and pushes them into problematic futures, particularly for disadvantaged women (Furstenberg, 2003). We took advantage of our longitudinal data set to examine this issue and tested whether the motherhood–delinquency relationship varied by age.

Third, we included in our analyses several important time-varying covariates causally prior to motherhood and that might affect drug use and delinquency. Although our fixed-effects models controlled for all omitted time-stable covariates, they did not control for possible omitted time-varying covariates. Therefore, we disentangled the effects of motherhood from the effects of pregnancy, sexual activity, and contraception use. Failing to control for important confounders such as pregnancy might bias estimated motherhood effects and render the motherhood–crime relationship spurious. Introducing sexual activity and contraceptive use into our models controlled for unmeasured situational and cognitive factors that contributed to wanted or unwanted pregnancy and motherhood.

Finally, we predicted the following five outcomes: 1) a general delinquency index, 2) fighting, 3) stealing, 4) marijuana use, and 5) alcohol consumption. This prediction allowed us to compare motherhood effects across related, but distinct, domains. Prior studies typically have examined single outcomes; consequently, some of the inconsistency in the results could have been caused by differences in dependent variables.

DATA

We tested the motherhood hypothesis with data collected from the Denver Youth Survey (DYS). The *DYS* is an accelerated longitudinal survey of youth based on a probability sample of households in high-risk Denver neighborhoods. Using population and housing characteristics, *DYS* researchers identified socially disorganized block groups with official crime rates in the top third of the city. These neighborhoods represented the most disadvantaged areas of Denver. Table 1 compares the demographic characteristics of Denver to Philadelphia—the site of the qualitative studies of Anderson (1992, 1999) and Edin and Kefalas (2005). Philadelphia is more disadvantaged than Denver, with higher poverty

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rates and unemployment as well as lower levels of earnings and postsecondary education. Table 1 also compares DYS neighborhoods with the Philadelphia neighborhoods sampled by Edin and Kefalas (2005) and shows that the DYS neighborhoods were significantly less disadvantaged than those of Edin and Kefalas.¹ To compare our results with theirs, we created an indicator variable for the poorest five (15 percent) census tracts in the DYS sample, which exhibited similar poverty rates and other census characteristics to the neighborhoods covered by Edin and Kefalas.² In our final models, we interacted this indicator with motherhood to determine whether motherhood effects varied between disadvantaged and extremely disadvantaged neighborhoods.

Table 1. Comparison of 2000 Census Characteristics for Denver and Philadelphia Neighborhoods, by Percentage

Census Characteristics	All Denver	DYS Median	Poorest Five DYS Tracts	All Philadelphia	E&K Median
Percent families below poverty line	11	20	37	18	34
Adults over 25 without high school degree	21	33	53	29	43
Male unemployment	6	9	13	11	18
Males earning less than \$10,000 last year	16	20	28	20	24
Female unemployment	6	8	17	11	16
Females earning less than \$10,000 last year	23	30	37	27	34
Children under 18 in female-headed, single parent household	27	36	54	43	60

ABBREVIATIONS: E&K = Edin and Kefalas (2005).

The DYS investigators selected 20,300 of the 48,000 enumerated households in the target neighborhoods, drew a stratified probability sample of households proportional to population size, and used a screening questionnaire to identify appropriately aged respondents (i.e., 7, 9, 11, 13, or 15 years old). This procedure yielded a sample of 1,528 respondents (of which 721 were female) in the first wave, which constituted an 85 percent completion rate of eligible youth (see Esbensen and Huizinga, 1990, and Matsueda, Kreager, and Huizinga, 2006, for more details). Each respondent was interviewed annually from 1988 to 1992 and from 1995 to 1999, thus,

1. Edin and Kefalas (2005) did not report total population values for their sampled neighborhoods, which made it impossible to construct mean values across their eight neighborhoods. We, therefore, reported the median values for their neighborhoods and compared these values with the median values of the DYS tracts.
2. In our sample, 170 females resided in the poorest DYS tracts.

covering ages 7 through 27. For the first five waves of the DYS, respondents aged 9 years and younger completed a child survey, which lacked the adequate measures of delinquency and drug use necessary for this study.³ We, thus, focused on female respondents aged 10 years and older (at a given wave), each of whom was asked relevant questions on delinquency, drug use, motherhood, and pregnancy. The retention rate was impressively high, with more than 90 percent of the sample retained by wave 5, 80 percent retained by wave 8, and 60 percent retained by wave 10 (Huizinga et al., 2003). Because our primary independent variables, motherhood and pregnancy, were not measured accurately prior to wave 6, we included only the 666 women who completed at least one survey in waves 6–10.⁴ In addition, not all reported pregnancies resulted in a birth. Of the 666 women in our sample, 99 (15 percent) had at least one pregnancy end in an abortion or miscarriage. For these cases, we could not determine the precise beginning and end date of the pregnancy. Therefore, to avoid biasing our estimates of pregnancy and motherhood, we removed those women from our sample who experienced an abortion or miscarriage.⁵ Our final sample then included 567 women and 4,177 person-years (less than 5 percent of available person-years were deleted because of missing covariate values). This sample was racially diverse and included 10 percent Whites, 45 percent Latinos, 33 percent Blacks, and 12 percent from other racial/ethnic categories.

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3. Because of funding cuts, surveys were not administered in 1993 and 1994. However, these systematically missing survey years were not problematic for our fixed-effects estimators.
 4. An examination of attrition effects at waves 3, 5, and 8 demonstrated that the underlying distributions of the interviewed sample were not substantially different from the original sample by age, sex, ethnicity, or original levels of drug use or delinquency. As another test of potential attrition effects, we created an indicator variable for respondents who dropped out of the study prior to wave 10. We then interacted this variable with our covariates of interest (motherhood, marriage, and pregnancy). All of these interactions were nonsignificant in our delinquency and marijuana models, which suggested that the covariate effects did not vary by the attrition state of respondents. In our alcohol models, the effects of teen motherhood, marriage, and pregnancy did vary significantly for those respondents who left the study. However, the overall pattern of results remained the same to those reported.
 5. Model results were similar to those reported when women with abortions or miscarriages were included and controlled statistically. To estimate the person-year pregnancy period for induced and spontaneous abortions, we used the average length of pregnancy prior to these events (approximately 8 weeks in both cases) (Strauss et al., 2007).

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MEASURES

Table 2 lists descriptive statistics for our explanatory and dependent variables. Our primary interest was in the effect of motherhood on trajectories of delinquency and drug use when marriage, pregnancy, sexual activity, contraceptive use, and age trends were controlled.

Table 2. Variable Descriptive Statistics

Variable	Person-Years		Proportion of Persons with a Change in Status
	Mean	SD	
Explanatory Variables			
Age	17.27	3.80	
Teen mother	.22	.39	.40
Young-adult mother	.04	.24	.12
Married	.08	.27	.23
Pregnancy	.07	.19	.53
Sexually active	.54	.50	.91
Contraception frequency	1.56	1.83	
Dependent Variables (Counts)			
Delinquency	9.93	74.41	
Fighting	.62	8.99	
Stealing	.95	25.70	
Marijuana	16.07	71.75	
Alcohol	19.10	71.71	
<i>N</i>	4177		567

ABBREVIATIONS: SD = standard deviation.

EXPLANATORY VARIABLES

Motherhood. To capture the transition to motherhood, we relied on the self-reported birthdates of female respondents of their first biological child.⁶ During waves 6–10, each DYS respondent listed the names and birthdates of all biological children born by the survey administration date. From this information, we could identify the survey year that a respondent gave birth to her first child and define that year as the transition from nonparent to mother. Moreover, for the first year of motherhood, we could take the birthdate and determine what percentage of the survey year that a woman was a mother.

Teen motherhood and adult motherhood. To capture age differences in motherhood effects, we created two categories of mothers. Female respondents who bore their first child at aged 19 years or younger were classified

6. Child adoption (either through an adoption agency or the adoption of a partner's children) represents the exception to the pregnancy-motherhood link. Because of data limitations, however, we could not identify adopted children and therefore limited our analyses to biological children.

as teen mothers, whereas females who had their first child at ages older than 19 were classified as young-adult mothers. These categories were, therefore, mutually exclusive, so that a teen mother remains in that category regardless of her age or subsequent child-bearing behavior. For example, a woman who had a child when she was 16 years old and had another child at age 22 would be categorized as a teen mother for the portion of the year when she gave birth to her first child and for all subsequent years.

Pregnancy. An accurate estimate of the effects of motherhood required that we distinguish periods of motherhood from periods of pregnancy. We expected delinquency and drug use to drop during the pregnant months because of the physical limitations imposed by pregnancy and the perceived negative health consequences of prenatal drug use. Collapsing pregnancy and motherhood into a single motherhood construct likely would result in the overestimation of the inhibitory effect of motherhood. To measure periods of pregnancy, we again relied on the respondents to report the birthdates of their children. We classified women as pregnant the 9 months prior to *any* birth. Because these periods were only portions of a year and might have straddled 2 survey years, the pregnancy variable ranged from 0, when a respondent was not pregnant during any portion of the person-year, to .75, when a respondent was pregnant for 9 months of the person-year.

Marriage. Marriage has been a focal concern of life-course research and potentially might explain the relationship between motherhood and crime (Warr, 1998). We operationalized marriage from the wave 1–10 surveys. At each interview, respondents were asked the question “Were you married at any time last year?” Respondents who answered in the affirmative were considered married for the person-year, whereas those that responded “no” were classified as unmarried.

Age. We included linear and quadratic age terms in our models to capture the curvilinear relationship between age and delinquency and to deal with the multi-cohort design of the DYS. The age variable captured the self-reported age of respondents at each person-year and ranged from 10 to 26 years old.

Sexual activity. At each wave, respondents were asked to report their number of opposite-sex sexual partners in the past year. We recoded this item into an indicator for respondents who were sexually active in each wave.

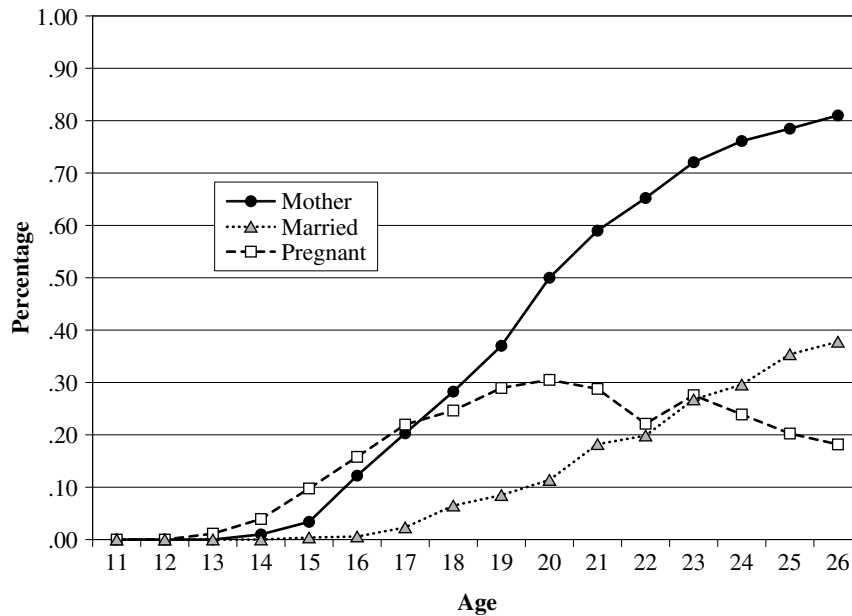
Contraception frequency. Respondents who were sexually active were asked how often they used contraception in the past year. Responses ranged from (1) “almost never” to (4) “always.” We redefined the contraception frequency variable by interacting it with the sexual activity and coding responses for individuals who were not sexually active as zero. In

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addition, contraception use was not asked in waves 1 and 2. To maintain as many cases as possible, we coded missing contraception values for these waves as zero and included a dummy variable for the wave 1 and 2 person-years.

Figure 1 presents the age distributions for rates of motherhood, pregnancy, and marriage in our sample. By age 19 (our cutoff for teenage motherhood), approximately 40 percent of the sample had given birth to a first child. As expected, given our sample of disadvantaged women, this percentage is higher than national averages for teenage motherhood. In 1995, the national rate of first birth for women aged 10–19 years was approximately 45 births per 1,000 women (Hamilton, Sutton, and Ventura, 2003). In our sample, 228 of 567 female respondents gave birth to their first child between the ages of 11 and 19, which resulted in a first birth rate of 400 births per 1000 women.

Figure 1. Observed Motherhood, Marriage, and Pregnancy Percentages, by Age



At the same time, the number of ever-married women in our sample was significantly lower than national averages. In 1990, 38.5 percent of American women aged 20–24 years had ever been married (Norton and Miller, 1992). In our sample, of the 389 female respondents interviewed between the ages of 20 and 24, 27 percent had ever been married. The high

rate of teenage motherhood and the low rate of marriage is consistent with the qualitative evidence presented by Anderson (1999) as well as by Edin and Kefalas (2005).

DEPENDENT VARIABLES

Delinquency. Our primary outcome is an index of 20 delinquent behaviors (see appendix A). Each item was measured at every wave and represents the number of times respondents performed the behavior in the year prior to the survey. An examination of the age-crime curve for each item resulted in the familiar curvilinear relationship, with most behaviors peaking at 15–16 years old and dropping off in young adulthood. We summed the 20 items to create person-year measures at each age. Because the index is highly skewed, we took its cube root as our outcome (see Snijders and Bosker, 1999). Additionally, we created separate indexes for fighting (3 items) and stealing (6 items). Wakefield and Uggen (2004) found that motherhood increased illegal earnings in their high-risk sample and argued that the pressures of caregiving increased property crimes for these women. We reevaluated this hypothesis in our study by examining the link between property crimes and motherhood in a random sample of women in disadvantaged communities.

Marijuana and alcohol use. To examine whether motherhood affected drug behavior differently than it affected other forms of delinquency, we created dependent variables for marijuana and alcohol use. As with the delinquency items, marijuana and alcohol use were measured at each wave and represented behavioral counts across the last year. Marijuana was measured from a single item, “How many times have you used marijuana in the last year?” with values ranging from 0 to 1,000. We created an alcohol measure from the sum of responses to the following three items “How many times have you drunk 1) wine, 2) beer, and 3) hard liquor in the last year?” with values ranging from 0 to 2,000. To address skew, we again took the cube root of each measure.⁷

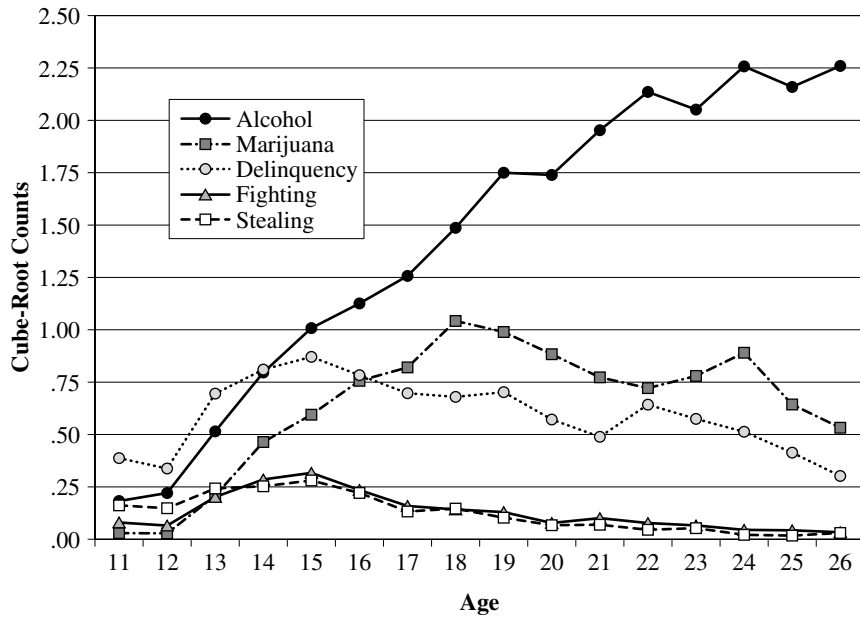
Figure 2 shows the age trends for our dependent variables. Looking first at the delinquency indices, we see the familiar curvilinear pattern, in which delinquency, fighting, and stealing rise in the early teenage years, peak around age 15, and steadily decline thereafter (although a blip occurred in the general delinquency index around the age of 21, perhaps because of increased delinquent opportunities surrounding the legal age of drinking and entering drinking establishments). Marijuana use showed a similar pattern to the delinquency variables but peaks at a somewhat later age,

7. Note that the substance use questions referred to occasions or events, which means that individuals could report using multiple times per day, resulting in counts for each behavior greater than the number of days in a year.

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around 18 years old. Marijuana use also rose around ages 23–24, which is a pattern of unclear origin that is perhaps related to the cohort structure of our data. Finally, alcohol use showed a fairly monotonic and increasing trend during our observed ages. As expected, the alcohol slope increased dramatically around the age of 21 when the behavior becomes legal in the United States.

Figure 2. Observed Delinquency, Marijuana, and Alcohol Means, by Age



ANALYSES

Our hypotheses centered on mean behavioral changes related to life-course transitions—such as motherhood, pregnancy, and marriage. To test these longitudinal relationships, we employed fixed-effects estimators of panel data (Allison, 1994). These models controlled for unobserved individual heterogeneity resulting from time-stable characteristics, which might be correlated with included regressors. These models, however, could not provide separate estimates of effects of stable covariates, which was of little consequence to our focus on within-individual change.

Although several regression model variations can estimate such change, which include mixed-effects approaches and regressions on within-individual deviations (Halaby, 2004; Raudenbush and Bryk, 2002; Snijders and Bosker, 1999), the simplest way to do so is by including individual-specific

fixed effects. Moreover, estimates from a model with fixed effects are always consistent, although in the special case when unobserved heterogeneity is orthogonal to included covariates, a model with random effects provides greater efficiency. Here, a Hausman test can be performed to test the null hypothesis that unobserved heterogeneity is uncorrelated with the included covariates (e.g., Wooldridge, 2002).

To define a model with fixed effects more formally, let K be the number of time-varying covariates and N be the total number of individuals. Let y_{it} be a behavioral observation and x_{itk} be an observed value on the k th time-varying covariate, such as age or motherhood status, for person i at time t . We were interested in describing within-individual change, or the association between y_{it} and x_{itk} , within each individual. Assuming that the magnitude of this association was the same for all individuals, a multilevel regression model with individual specific intercepts is expressed as follows:

$$y_{it} = \beta_0 + \sum_{k=1}^K \beta_k x_{itk} + \sum_{n=1}^{N-1} v_n d_{in} + e_{it}, e_{it} \stackrel{\text{i.i.d.}}{\sim} N(0, \sigma^2) \quad (1)$$

where d_{in} are dummy variables such that $d_{in} = 1$ when $i = n$ and 0 otherwise. Coefficient estimates, β_k , represent the within-individual change in an outcome, y , associated with a 1-unit within-individual change in a covariate, x_k .

Estimates from the fixed-effects model in equation (1) are unbiased and consistent under the assumption of strict exogeneity (i.e., $\text{Cov}(x_{it}, e_{it}) = 0$). Standard errors and test statistics are valid under the assumption that the time-varying errors, e_{it} , are homoscedastic and serially uncorrelated. To correct for potentially heteroscedastic and dependent error terms, we estimated robust standard errors using Huber–White sandwich estimators.

RESULTS

Prior to estimating our fixed-effects models, we first decomposed the total variance of our dependent variables into within- and between-individual variance components. For each delinquency and drug outcome, intraclass correlation coefficients ranged between .32 and .42, which implies that approximately 60–70 percent of the variance lies within the individual (across time). It is this within-individual variance that we would like to explain with time-varying covariates in our fixed-effects models.

Before proceeding with the fixed-effects models, we performed Hausman tests, which rejected the null hypothesis of no difference in estimates between the fixed and the random effects. Therefore, we reported the fixed-effects models, which were consistent and efficient.

Tables 3 and 4 present the estimates for our fixed-effects models of delinquency and drug use. For each outcome, we list two models. The first

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includes covariates for teen motherhood, young adult motherhood, marriage, and age. The second adds pregnancy, sexual activity, and contraception frequency to estimate the net effect of the motherhood variables on within-individual changes in female delinquency and drug behaviors.

DELINQUENCY

The first model in table 3 supports the hypothesis, drawn from urban ethnographic research, that motherhood has an inhibiting effect on the delinquent behaviors of women in disadvantaged communities. Both teen and young adult motherhood transitions were associated significantly with within-individual decreases in the general delinquency index. Moreover, the magnitudes of these effects were large. To put this finding in perspective, the expected one-time decline associated with the transition to teen motherhood (-.31) was comparable with the estimated age-related decline between ages 18 and 26 (-.28).⁸ Although the teenage motherhood coefficient was larger than the young-adult mother coefficient, the difference did not reach statistical significance, which suggests that motherhood was related to criminal desistance during both age periods.

Surprisingly, model 1 also shows that marriage is not associated with changes in overall delinquency, controlling for motherhood and the age trend. Contrary to the many studies that found strong relationships between marriage and (particularly male) desistance, our analyses found that, adjusting for motherhood and age, the transition to marriage fails to further inhibit delinquency in a sample of women residing in poor communities. Perhaps this result is not surprising because the infrequency of stable marriages among these women suggests that marriage might not be a salient event in their lives.

Model 2 adds controls for pregnancy, sexual activity, and contraception in the model of delinquency. Analogously to motherhood, pregnancy is associated with a drop in delinquency. Thus, as expected, women reduce their delinquent behavior during pregnancy, perhaps in part because committing crime while pregnant is physically more challenging and in part because women would be transitioning into the mother role during the pregnancy period. As expected, being sexually active increased delinquency in our female sample. This finding was consistent with prior studies that linked an early sexual debut and sexual permissiveness with delinquency and drug use (Armour and Haynie, 2007; Huizinga, Loeber, and Thornberry, 1993). Interestingly, contraception frequency had no similar positive relationship. Moreover, the motherhood estimates did not change

8. Calculation for the decline between ages 18 and 26: $(.211 \times 26) + [-.0056 \times 26^2] - (.211 \times 18) + [-.0056 \times 18^2] = -.28$.

Table 3. Fixed-Effects Models of Delinquency (Cube-Root Counts, N = 567; 4,177 person-years)

Variable	Delinquency Index				Fighting Index				Stealing Index			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE
Intercept	-1.209***	(.324)	-.442	(.359)	-.291*	(.123)	.007	(.149)	.299*	(.145)	.497**	(.175)
Age	.211***	(.038)	.130**	(.040)	.060***	(.014)	.031	(.016)	-.003	(.016)	-.024	(.019)
Age-squared	-.006***	(.001)	-.004***	(.001)	-.002***	(.000)	-.001**	(.000)	.000	(.000)	.000	(.000)
Teen mother	-.306***	(.074)	-.335***	(.075)	-.101***	(.028)	-.112***	(.028)	-.067*	(.030)	-.074*	(.030)
Young-adult mother	-.276**	(.102)	-.256*	(.102)	-.033	(.035)	-.028	(.034)	-.092*	(.042)	-.090*	(.041)
Marriage	-.029	(.082)	-.006	(.083)	.004	(.025)	.010	(.025)	.031	(.025)	.033	(.026)
Pregnancy			-.414***	(.103)			-.127**	(.037)			-.116**	(.036)
Sexually active			.280***	(.066)			.081**	(.025)			.102**	(.033)
Contraception												
frequency			.022	(.015)			.003	(.006)			-.001	(.006)
Missing contraception			-.040	(.061)			-.067*	(.026)			-.002	(.027)
R ² (within)	.02		.03		.02		.03		.02		.03	
F statistic (df1; df2)	12.66 (5; 3636)		10.87 (9; 3606)		15.71 (5; 3636)		10.29 (9; 3606)		17.88 (5; 3636)		11.14 (9; 3606)	

ABBREVIATIONS: SE = standard error.
*p<.05, **p<.01; *** p<.001 (two-tailed significance test).

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substantially when controlling for pregnancy, sexual activity, and contraception, which increased our confidence in the robustness of the motherhood effects. We found similar results for the models of fighting and stealing. In both models, teenage motherhood had a significant inhibitory effect, and this effect was net of other time-varying covariates.⁹ The finding for stealing ran counter to the findings of Wakefield and Uggen (2004), who suggested that early motherhood increases property crime because of strain.

MARIJUANA AND ALCOHOL

With respect to drug use, we again found that the transition to motherhood was associated with significant reductions in marijuana smoking and alcohol use, controlling for age and marital status (see models 1 for marijuana and alcohol use in table 4). Consistent with statements from the mothers interviewed by Edin and Kefalas (2005), our findings suggested that the transition to motherhood accompanied reductions in drug behaviors.

In contrast to the results for delinquency, we found that marriage also was associated with reductions in drug use, controlling for other time-varying covariates. Teen motherhood, marriage, and young-adult motherhood were associated with comparable reductions in marijuana and alcohol use. It was unclear why marriage would have an effect on female drug use but not an effect on delinquency. Perhaps the married women were less inclined to attend parties or nightclubs for the purpose of meeting men, thus, reducing their opportunities for drug use. At the same time, the married women of our disadvantaged sample may have been more likely to have a criminal spouse than married women in more advantaged settings, which led to relatively stable criminality before and after marriage. Future research is needed to identify the mechanisms underlying our results.

As expected, pregnancy was associated with significant reductions in alcohol and marijuana use. The drop in alcohol use because of pregnancy was more than twice the size of the drop because of teen motherhood, a difference that was statistically significant (t statistic = 3.43, $p < .001$). This result perhaps reflects the strong social messages and informal sanctions aimed at preventing fetal damage from prenatal alcohol consumption (Bachman et al., 1997). For both marijuana and alcohol, adding pregnancy, sexual activity, and contraception use into the model attenuated little of the motherhood effect.

We also examined the robustness of our use of cube-root transformations on the dependent variables. We logged the dependent variable, a

9. The motherhood effect remained significant when the teenage and young-adult indicators were collapsed into a single motherhood variable.

Table 4. Fixed-Effects Models of Drug Use (Cube-Root Counts, N = 567; 4,177 Person-Years)

Variable	Marijuana				Alcohol Index			
	Model 1		Model 2		Model 1		Model 2	
	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE
Intercept	-3.513***	(.381)	-2.605***	(.435)	-3.192***	(.385)	-2.447***	(.426)
Age	.396***	(.045)	.289***	(.049)	.329***	(.046)	.249***	(.049)
Age-squared	-.008***	(.001)	-.006***	(.001)	-.004**	(.001)	-.002	(.001)
Teen mother	-.369***	(.085)	-.397***	(.085)	-.341***	(.085)	-.392***	(.085)
Young-adult mother	-.398**	(.154)	-.386*	(.153)	-.640***	(.151)	-.560***	(.151)
Marriage	-.308***	(.085)	-.291**	(.087)	-.327**	(.097)	-.263**	(.097)
Pregnancy			-.441**	(.129)			-1.007***	(.124)
Sexually active			.438***	(.075)			.415***	(.071)
Contraception frequency			.032	(.018)			.043*	(.017)
Missing contraception			.145*	(.064)			.059	(.065)
R ² (within)	.06		.09		.20		.23	
F statistic (df1; df2)	48.58 (5; 3636)		32.97 (9; 3606)		173.86 (5; 3636)		112.33 (9; 3606)	

ABBRVIATIONS: SE = standard error.

*p<.05; **p<.01; *** p<.001 (two-tailed significance test).

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more dramatic transformation than the cube root, and found results similar to those reported. We also estimated a negative binomial model and found evidence of overdispersion and similar results. Our results, thus, seem robust to alternative dependent variable transformations and analytical strategies.

CESSATION OR REDUCTION?

Desistance can be defined as a social process, whereby criminal behaviors decline with time, or as the transition from a state of offending to nonoffending (Mulvey et al., 2004). Operationalizing crime and drug use with continuous measures captures the social process of desistance, whereby life-course events result in reductions in behaviors without necessarily leading to cessation. An alternative strategy is to define crime and drug use with binary measures, such that individuals are classified as offending or nonoffending in each person-year. We then can apply a fixed-effect logistic model to the prediction of behavioral cessation.

Table 5 presents the results of fixed-effect logistic regressions of behavioral cessation of delinquency and drug use, controlling for pregnancy, sexual activity, and contraception use. Overall, the pattern of coefficients was similar to those presented in the continuous fixed-effects models. Teen motherhood, in particular, was a strong predictor of delinquency and marijuana cessation, with marriage having smaller effects and being nonsignificant in the case of delinquency. A discrepancy between the linear and nonlinear models was that teen motherhood and marriage did not predict the termination of alcohol consumption. This result was perhaps not surprising because the legality and normative quality of alcohol use make it unlikely that life transitions would result in complete cessation from this behavior. The perceived risks of fetal alcohol syndrome, however, likely explain the strong effect of pregnancy on temporary alcohol cessation.

DOES THE TEENAGE MOTHERHOOD EFFECT DECAY WITH TIME?

Our findings to this point suggest that motherhood resulted in declining delinquency and substance use among women from disadvantaged neighborhoods. However, these analyses assumed that the transition to motherhood produced a constant downward shift in the overall level of offending or drug use that was similar among all teenage or young-adult mothers. It is possible that motherhood effects decay with time, perhaps to the point that mothers return to prepregnancy levels of delinquency and drug use (Bachman et al., 1997; Gilchrist et al., 1996; Morrison, Spencer, and Gillmore, 1998).

By focusing on teenage motherhood, we examined the decay effects by introducing dummy variables for each year since teenage birth into our

Table 5. Fixed-Effects Logistic Models of Delinquency and Drug Use

Variable	Delinquency Index		Marijuana		Alcohol Index	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Age	.193	(.110)	1.149***	(.158)	.925***	(.120)
Age-squared	-.007*	(.003)	-.026***	(.004)	-.019***	(.003)
Teen mother	-.819***	(.179)	-1.284***	(.222)	-.302	(.182)
Young-adult mother	-.402	(.294)	-.864*	(.341)	-.638*	(.282)
Marriage	-.387	(.237)	-.726**	(.272)	-.367	(.213)
Pregnancy	-.914**	(.263)	-1.176***	(.308)	-1.768***	(.245)
Sexually active	.417**	(.139)	.774***	(.167)	.586***	(.142)
Contraception frequency	.052	(.034)	.026	(.040)	.045	(.034)
Missing contraception	-.094	(.150)	.476*	(.213)	.391*	(.156)
Log-likelihood	-1210.56		-797.39		-1173.25	
Chi-square (df)	106.25 (9)		307.24 (9)		600.95 (9)	
N persons; person-years ^a	405; 3079		316; 2373		444; 3419	

ABBREVIATIONS: SE = standard error.

^a Persons without variation in dependent variable are excluded from analyses.

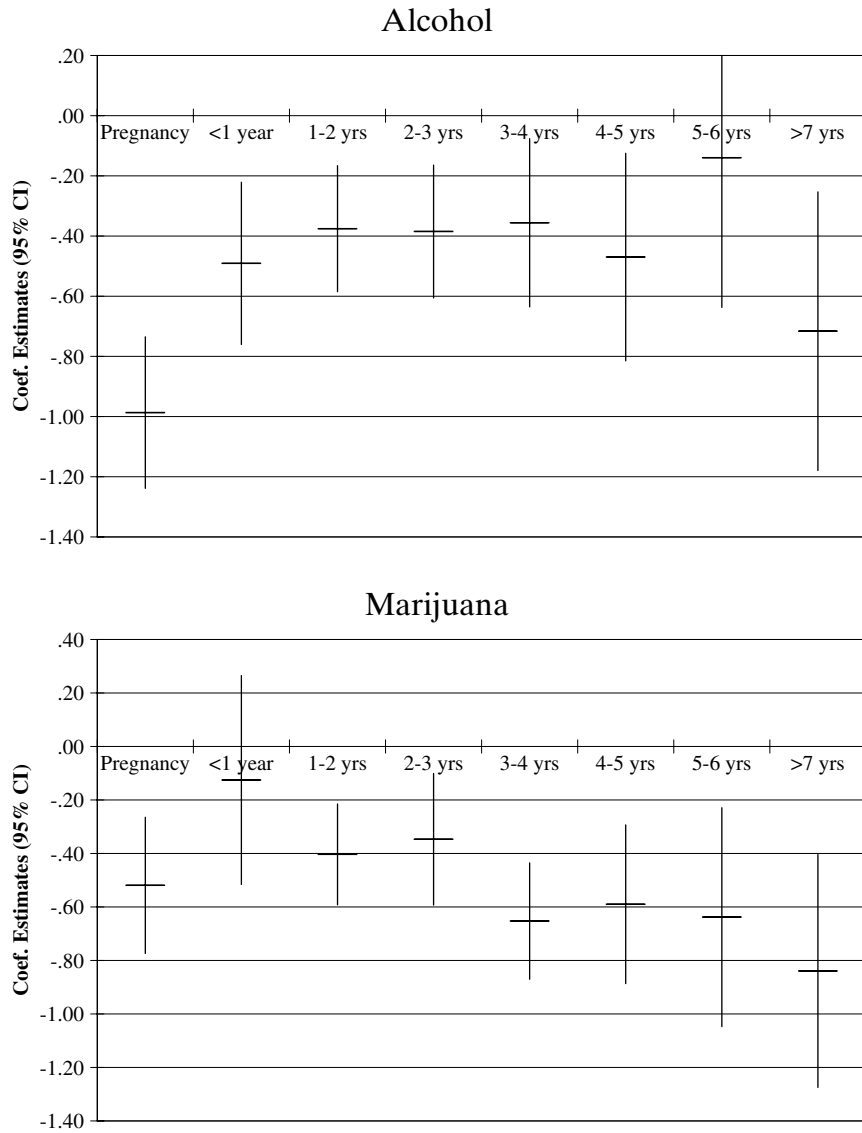
*p<.05; **p<.01; *** p<.001 (two-tailed significance test).

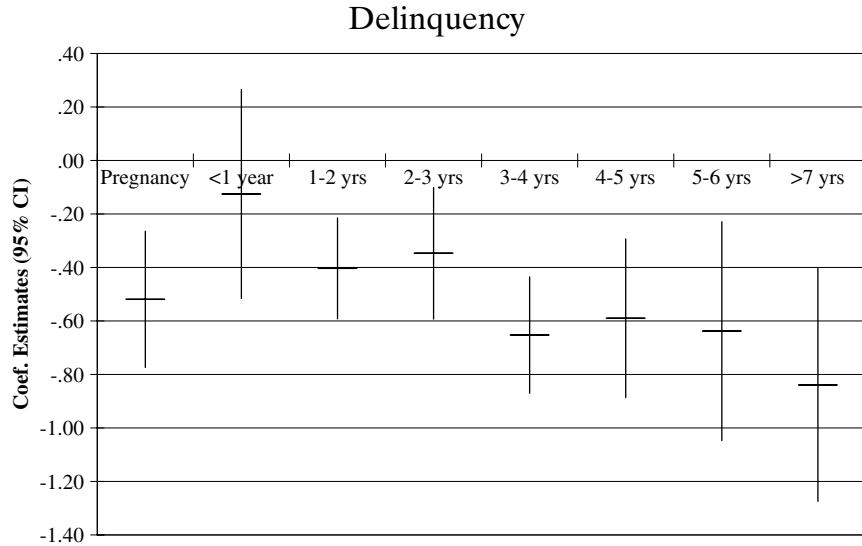
equations, with nonmother as the reference category. The coefficient estimates and 95 percent confidence intervals for each subsequent year of teenage motherhood (along with pregnancy) are listed in Figure 3.¹⁰ Across the three behaviors, teenage motherhood effects tended to persist across time, with most year-since-birth estimates remaining negative and significant. However, the trends did differ somewhat by outcome. For delinquency, the effect of teenage motherhood generally became weaker with years since childbirth, becoming nonsignificant at 4–6 years before again reaching significance for women who gave birth as teenagers and have had a child for 7 or more years. So although it seems that some evidence of decay in the teenage motherhood effect is present, this decay did not occur until years after childbirth, and longtime mothers continued to have lower delinquency than women without children. This pattern contrasts with that observed for marijuana. Here, the estimated effect of teen motherhood was no different from zero for the year after childbirth but became more negative for all subsequent years, which suggests an amplification of the motherhood effect across time. Finally, for alcohol use, teenage motherhood seemed to hold a fairly constant negative relationship for each year since birth.

10. We also estimated years-since-motherhood for young-adult mothers as well as the fighting and stealing outcomes and found virtually identical patterns to those reported. However, the reduced variance in the fighting and stealing outcomes increased the error for each motherhood-year estimate, particularly at later years.

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Figure 3. Coefficient Estimates and Confidence Intervals for Years Since Teenage Mother





DOES THE TEENAGE MOTHERHOOD EFFECT CHANGE WITH ADDITIONAL CHILDREN?

Our teenage motherhood effects might be the result of multiple child-births rather than the transition from nonmother to mother. To examine the effects of subsequent children, we controlled for the second and third births of teenage mothers in our models. Sixty-nine (30 percent) of the teenage mothers had a second child in the interviewed years, and 15 (7 percent) of these mothers had three children. Table 6 shows that having an additional child had no effect on the delinquency or alcohol use of teenage mothers, net of the first child and other covariates. For marijuana use, however, a second child did have an added inhibitory effect, but even here, the first child remained the larger and more important transition. These findings suggest that the transition to motherhood, more than the cumulative effects of having multiple children, inhibited crime and drug use among young urban women. These findings are consistent with the life-course conception of turning points and within-individual behavioral change.

MODERATION BY RACE AND EXTREME COMMUNITY POVERTY

The research of Edin and Kefalas (2005) implied that motherhood improves the lives of women in poor communities regardless of race, ethnicity, or local contexts. It is possible, however, that the effect of motherhood differs by race-ethnicity because of the structural positions of African Americans and Hispanics in American society. It also might be that

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Table 6. Fixed-Effects Models of Delinquency and Drug Use (Cube-Root Counts, *N* = 567; 4,128 Person-Years)

Variable	Delinquency Index		Marijuana		Alcohol Index	
	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE
Intercept	-.443	(.359)	-2.612***	(.435)	-2.449***	(.426)
Age	.132**	(.040)	.289***	(.049)	.248***	(.049)
Age-squared	-.004***	(.001)	-.006***	(.001)	-.002	(.001)
Teen mother of first child	-.343***	(.076)	-.361***	(.089)	-.376***	(.087)
Teen mother of second child	.011	(.122)	-.228*	(.112)	-.082	(.138)
Teen mother of third child	.420	(.279)	.375	(.272)	-.037	(.373)
Young adult mother	-.241*	(.102)	-.392*	(.154)	-.568***	(.151)
Marriage	-.012	(.083)	-.295**	(.087)	-.262**	(.097)
Pregnancy	-.389***	(.104)	-.444**	(.129)	-1.018***	(.126)
Sexually active	.288***	(.064)	.439***	(.074)	.412***	(.070)
Contraception frequency	.022	(.015)	.031	(.018)	.043*	(.017)
Missing contraception	-.039	(.061)	.146*	(.064)	.059	(.066)
<i>R</i> ² (within)	.03		.09		.23	
<i>F</i> statistic (df1; df2)	9.34 (11; 3604)		27.04 (11; 3604)		93.38 (11; 3604)	

ABBREVIATIONS: SE = standard error.
 p*<.05; *p*<.01; *** *p*<.001 (two-tailed significance test).

the effect of motherhood varies by the degree of neighborhood poverty. We explored these possibilities by estimating interactions between motherhood and the time-stable characteristics of race and high neighborhood poverty.

In other analyses (not shown), we introduced interaction terms between teen motherhood and Black as well as between teen motherhood and Hispanic (with White/other as the reference category) into our models. For delinquency and alcohol, the motherhood interactions were not significant and we could not reject the null hypothesis of identical effects of motherhood by race and ethnicity. For marijuana, however, the motherhood-Black interaction was positive and significant, and the motherhood-Hispanic interaction was negative and significant, which suggests that motherhood had the strongest inhibitory effect for Hispanic women and the lowest inhibitory effect for Black women.

To address extreme community poverty, we included an interaction term between teenage motherhood and the most impoverished neighborhoods of Denver, which corresponded most closely to the poverty levels of Edin and Kefalas's (2005) Philadelphia neighborhoods. For each outcome, the interaction term was not significant and we could not reject the null hypothesis of identical motherhood effects for extremely impoverished neighborhoods and other disadvantaged DYS areas (analyses not shown).

DISCUSSION

This study found that motherhood was associated with significant reductions in delinquency and drug use among a sample of disadvantaged young women. These reductions persisted when controlling for pregnancy, marriage, sexual activity, and contraceptive use and varied little by extreme neighborhood disadvantage or race/ethnicity. Thus, our results provide strong evidence for the hypothesis, drawn from ethnographic research, that motherhood represents an important shift in the lives of women residing in poor communities and helps pull them away from high-risk behaviors. In the language of life-course criminology, we found that motherhood, and not marriage, represents the primary “turning point” in the delinquent trajectories of women in disadvantaged settings.

These results contrast sharply with previous quantitative research on motherhood and crime, which tended to find null or increased effects of motherhood on the criminal trajectories of women. Those studies, however, did not examine the precise population of young women in disadvantaged neighborhoods for which qualitative research has found inhibitory motherhood effects. Some used samples of the general population, whereas others failed to separate men from women.

The two studies that used samples of extremely disadvantaged individuals and separated women from men were Wakefield and Uggen’s (2004) analysis of data from the National Supported Work Demonstration, which found that motherhood was associated with greater future illegal earnings, and Giordano, Cernkovich, and Rudolph’s (2002) analysis of 127 institutionalized women, which found no effect of attachment to children on the crime of women. At least two possible reasons—each having to do with the populations sampled—help explain why these two studies yielded different findings from ours. First, both studies used samples of young adults with few teenage mothers. The average age for Wakefield and Uggen’s (2004) sample was 25 years, and that of Giordano, Cernkovich, and Rudolph (2002) was 29 years. If women who had a child during adolescence reduced their offending *and reduced the risk of arrest in young adulthood*, then they would have been unlikely to enter the sample of arrestees captured by Wakefield and Uggen as well as Giordano, Cernkovich, and Rudolph.

Second, both studies sampled special populations with severe problems. The Supported Work data were from a jobs program that targeted chronically unemployed individuals in several major U.S. cities, which included offenders who had been incarcerated, addicts who had undergone drug treatment, and high-school dropouts—half of whom were arrested (see Hollister, Kemper, and Maynard, 1984; Piliavin et al., 1986). This population of individuals is special; they were not only disadvantaged but also

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had severe problems of unemployment, drug addiction, and criminality that brought them to the attention of correctional and therapeutic institutions. Similarly, Giordano, Cernkovich, and Rudolph's (2002) data derived from a sample of incarcerated men and women. Indeed, Giordano, Cernkovich, and Rudolph (2002: 1038) recognized that their sample likely explained their null motherhood results: "The difference between our findings and those [that find a motherhood effect] likely stems from our focus on a sample of early-starting delinquents with a significant history of conduct problems." In contrast, the DYS randomly sampled a population of disadvantaged youth, most of whom have not experienced contact with the criminal justice system. It might be that the inhibitory effect of motherhood on crime would not apply to individuals at severe risk of institutionalization. For such individuals, the etiology of drug use and criminality might be beyond the effects of a life-course transition, such as becoming a mother. This speculation is consistent with the overall findings of the National Supported Work Demonstration, which found, using a randomized experiment, that providing another life-course transition—namely, jobs—did not significantly reduce crime or drug use for the sample as a whole.

This discussion underscores the importance of interpreting our results within the specific social context within which members of our sample were embedded. Given previous research findings, it could be that the motherhood effect identified here among disadvantaged women applies neither to general populations of more affluent women nor to extremely disadvantaged women beset with problems so severe to be at high risk of institutionalization. Thus, the motherhood effect could follow an inverted U-shaped function across the class structure—absent among the affluent and the severely impaired but present among most residents of disadvantaged neighborhoods. Such a functional form, which implies potential heterogeneity in disadvantaged populations, warrants future research.

In interpreting our results, it is instructive to consider broader research on populations of disadvantaged inner-city women. Such research suggests that structural forces play a critical role in shaping the life chances, opportunities, and possibilities of disadvantaged women. The trap of poverty that crosses generations, in part resulting from fewer family resources (including inheritance, social capital, and cultural capital) passed down from parents, puts many women at a disadvantage early in life. The lack of marriageable men—particularly for African American women, who are represented disproportionately among the disadvantaged—reduces the probability of achieving the ideal stable married life with children to which so many women aspire, regardless of class. The abundance of illicit role models, incentives, and opportunities in inner-city neighborhoods provides ways of obtaining a semblance of meaning for disadvantaged women. In

short, within a context of bleak conventional prospects and ample illicit alternatives, having a child in the absence of the father can provide meaning and fulfillment for an otherwise empty and hollow life. Such meaning amounts to a pragmatic adaptation to a difficult circumstance—one generated in large part by broader structural forces. We should emphasize that our results have not compared explicitly advantaged versus disadvantaged social contexts. Rather, we have made implicit comparisons between our findings for disadvantaged neighborhoods and previous research using general samples of more advantaged populations. Future research should sample both advantaged and disadvantaged neighborhoods and compare the effects of motherhood across samples.

It also should be noted that we do not compare motherhood with fatherhood in our study. Again, we were interested in testing the specific hypothesis that motherhood inhibits criminal involvement for disadvantaged women. The meaning of parenthood is likely to differ dramatically for men, particularly men living in disadvantaged communities. Indeed, the definition of “father” is likely to have much greater variation than the definition of “mother.” Men might father biological children without their knowledge, have children with multiple partners without the assumption of caregiving responsibilities, assume the father role for the children of a partner from a previous relationship, or be the sole provider for their own biological children. The complexity of fatherhood makes the comparison with motherhood challenging and worthy of its own research design.

Our finding that motherhood was related strongly to criminal desistance might be compromised by two threats to validity. First, our measures of crime and drug use are for an entire year. Therefore, in the year in which mothers gave birth, we could not determine whether crime preceded childbirth, childbirth preceded crime, or both. For this duration, we could not determine causal order. To address this issue, we created an indicator variable for the person-year in which respondents became mothers and included this into our models (not shown). In no case was this variable significant, which suggests that motherhood effects persist regardless of our inability to identify the exact temporal ordering of crime and motherhood in the year of childbirth.

Second, although our fixed-effects models sweep out all time-invariant confounder variables, and we control for the time-varying effects of age, sex, contraception, and pregnancy, it remains possible that omitted time-varying covariates could render the motherhood–crime relationship spurious. But because the effect of motherhood on crime is negative, such a time-varying covariate would have to affect motherhood and delinquency with opposite signs. Consider a simplified version of our model in which $y_{it} = -\beta x_{it} + e_{it}^*$, where y is delinquency and x is motherhood, but the correct model is $y_{it} = -\beta x_{it} + \gamma z_{it} + e_{it}$, where z is an omitted time-varying covariate

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correlated with x according to $x_{it} = \delta z_{it} + u_{it}$. Then the motherhood effect is completely spurious when $-\beta^* = \beta\delta$. More generally, for $-\beta^*$ to be biased toward zero, the product $\beta\delta$ would have to be negative. If that product is positive—as most substantive examples, such as parent monitoring, would suggest—then our models would underestimate the effect of motherhood. In other words, they would be conservative estimates.

One scenario that could render our results spurious would be if some transient event, such as partying and enjoying the night life, *temporarily* increased drug use and the risk of pregnancy. If the cessation of partying *fortuitously coincided with childbirth*—rather than being *caused* by childbirth—then the delinquency and drug use of the mother might subsequently regress to her person-specific (lower) mean. This outcome would produce a spurious negative effect of motherhood on deviance.¹¹ Absent a randomized controlled experiment, we are reluctant to treat the effect of motherhood as causal.

Finally, our analyses have focused on the association between the motherhood transition and changes in delinquency and drug use among disadvantaged women. We have not examined empirically the specific causal mechanisms by which motherhood transforms the lives of women. Here, the ethnographic research has provided several possible explanations, which include 1) a shift in priorities, whereby the identity of being a mother and caring for a child become paramount; 2) a change in the value placed on the risk of arrest, which is greater given the potential consequences for the child; and 3) alterations in routine activities such as refraining from the nightlife, which are linked to the identity of being a good mother (e.g., Edin and Kefalas, 2005; Giordano, Cernkovich, and Rudolph, 2002). Identifying the mechanisms that connect motherhood to criminal desistance would have implications for life-course theories of crime, including Sampson and Laub's (1993) theory of informal social control. Future research is needed to test whether the bonds of disadvantaged mothers with their children, their changed daily activities, or their perceptions of the future are responsible for “knifing off” criminal lines of action and desistance from crime and substance use.

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11. We thank an anonymous reviewer for pointing out this possibility.

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Appendix A. Delinquency Index

Loud/unruly in public
Theft of \$5 or less^a
Obscene phone calls
Avoid paying for a service
Sell marijuana
Vandalize
Sell something fraudulently
Theft \$50-\$100^a
Theft \$10-\$50^a
Concealed weapon
Receive/possess/sell stolen property^a
Burglary/theft from vehicle^a
Simple assault^b
Theft of \$100 or more^a
Gang fighting^b
Grand theft auto
Burglary
Robbery
Aggravated assault^b
Sell hard drugs

^a Theft index.

^b Violence index.