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DISCUSSION PAPER SERIES

IZA DP No. 12208

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ISSN: 2365-9793

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ABSTRACT

Effects of Maternal Work Incentives on Adolescent Social Behaviors*

This study investigates the effects of welfare reform in the U.S. in the 1990s, which dramatically limited cash assistance for low-income families, on the next generation as they transition to adulthood. We estimate effects by gender and focus on behaviors that are important for socioeconomic and health trajectories and represent early observable consequences of the reforms for the next generation. Using two nationally-representative datasets, we exploit differences in welfare reform implementation across states and over time in a difference-in-difference-in-differences framework to identify plausibly causal effects of welfare reform on a range of prosocial and antisocial behaviors (volunteering, participating in clubs/teams/activities, skipping school, getting into fights, damaging property, stealing, hurting others, smoking, using alcohol, using marijuana, using other illicit drugs). We explore maternal employment, supervision, and child's employment when not in school as potential mediators. We find that: (1) Welfare reform had no favorable effects on any of the youth behaviors examined. (2) Welfare reform led to a decrease in volunteering among girls. (3) Welfare reform led to increases in skipping school, damaging property, and getting into fights among boys. (4) Welfare reform led to increases in smoking and drug use among both boys and girls, with generally larger effects for boys (e.g., approximately 6% for boys compared to 4% for girls for any substance use). (5) The mediators we are able to consider explain little of the observed effects of welfare reform. Overall, the results from this study suggest that the intergenerational effects of welfare reform on adolescent behaviors were unfavorable, particularly for boys, and do not support longstanding arguments that incentivizing maternal employment by limiting cash assistance leads to responsible behavior in the next generation. As such, the social gains of welfare reform for women found in previous studies may have come at a cost to the next generation, particularly to boys who have been falling behind girls in terms of high school completion for decades.

JEL Classification: H53, I12, I31, I38

Keywords: welfare reform, maternal work, substance use, violence, school, intergenerational, adolescents, risky behaviors

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* Research reported in this publication was supported by the Eunice Kennedy Shriver National Institute of Child Health & Human Development of the National Institutes of Health under Award Number R01HD086223 and by the Robert Wood Johnson Foundation through its support of the Child Health Institute of New Jersey at Robert Wood Johnson Medical School, Rutgers University (grants 67038 and 74260). The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health or the Robert Wood Johnson Foundation. The authors are grateful to Elizabeth Lower-Basch for valuable input; Karen Conway, Jeff DeSimone, Daniel Grossman, Brady Horn, Brad Humphries, Jun Soo Lee, Joseph Sabia, Orgul Ozturk, and Nicholas Anthony Wright for helpful comments, to the staff at the University of Michigan's NADHAP for their data and technical help, and Michael Papotto for excellent research assistance.

The U.S. Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, often referred to as "welfare reform," was a major policy shift in the U.S. that sought to dramatically reduce dependence of single parents on government benefits by promoting work, encouraging marriage, and reducing non-marital childbearing. The legislation represented a convergence of dissatisfaction with the welfare system on both sides of the political spectrum, with welfare participation becoming viewed by many as a cause of dependence rather than a consequence of disadvantage. The key strategy for reducing dependence was to promote employment by imposing work requirements as a condition for receiving benefits in concert with a lifetime limit on receipt of cash assistance. The basic argument was that labor force participation would break a culture of dependence by increasing self-sufficiency and reconnecting members of an increasingly marginalized underclass to the mainstream ideals of a strong work ethic and civic responsibility (Katz, 2001). At the same time, there were concerns that some individuals would be ill equipped to maintain stable employment (e.g., due to low job skills or disability) and that the pro-work regime would marginalize, rather than mainstream, those individuals by contributing to existing hardships (Lichter & Jayakody, 2002). The legislation was targeted to females, who represent the vast majority of welfare recipients in the United States (ACF, 2016).

The reforms have been successful in that welfare caseloads have declined dramatically—e.g., average monthly family welfare caseloads fell from a peak of 5.05 million in 1994 to 1.10 million in 2017, representing a 78% decrease (ACF, 2018). Employment of low-skilled women increased in the aftermath of the reforms and at least some of the increase can be attributed to welfare reform (Fang & Keane, 2004; Ziliak, 2016) and welfare reform also led to declines in women's substance abuse (Corman et al., 2013; Kaestner & Tarlov, 2006) and crime (Corman et

al., 2014) as well as increases in women's civic participation in the form of voting (Corman, Dave & Reichman, 2017). The effects for drug use appeared to operate in part through drug sanctions and strength of state work incentives (Corman et al., 2013), while the effects for voting appeared to operate in part through increases in employment, education, and income (Corman, Dave & Reichman, 2017).

An implicit assumption behind the reforms was that a work-focused regime would not only encourage mainstream behaviors of mothers, which it appeared to do on average for the behaviors that have been examined, but that it would also encourage mainstream behaviors of the next generation and disrupt an intergenerational transmission of welfare dependence through more stable home environments and exposure to work as the key means of financial support. However, research to date on the effects of welfare reform on children has primarily focused on young children who are not old enough to make autonomous decisions; as such, few studies have broadly tested the intergenerational "culture of poverty" argument. The most developed line of research on adolescents includes quasi-experimental studies finding that PRWORA led to decreased high school dropout and likely decreased teen fertility (Dave et al., 2012; Kaestner, Korenman & O'Neill, 2003; Koball, 2007; Lopoo & Deleire, 2006; Miller & Zhang, 2012; Offner, 2005), at least in part through its "minor mother" requirements that mothers under 18 years old participate in education or training activities and live with a parent or legal guardian. Thus, for these outcomes, welfare reform had socially favorable effects on the next generation. Few other adolescent behavioral outcomes have been explored. [1]

The small extant literature on the effects of welfare reform on adolescent behaviors is

[1] We use the term "adolescents, "teens," and "youth" interchangeably when making general points about children who are old enough to make autonomous decisions but later indicate specific age ranges in the context of our analyses.

characterized by another noteworthy gap: Few studies have considered differential effects by gender (exceptions are noted in the Background section). Although welfare reform was primarily targeted to females, the developmental psychology and broader literature suggests that second-generation effects could be substantial for boys. For example, a recent study found that gender gaps in disciplinary problems, achievement test scores, and high school completion (all favoring girls) are larger in disadvantaged families than in more advantaged families (Autor et al., forthcoming), suggesting that changes in families' economic circumstances may affect the behavioral outcomes of boys more than those of girls.

In this study, we investigate the effects of welfare reform, which was implemented in the U.S. in the 1990s but is very much in effect today—on a range of social behaviors of high school-aged youth. We conduct all analyses by gender and consider both socially-desirable behaviors (volunteering and participating in clubs/teams/activities) and socially-undesirable activities (skipping school, getting into serious fights, damaging property, stealing, hurting others, and specific types of substance use). These outcomes can occur within a relatively short time frame and represent early observable consequences of the reforms for the next generation as they transition to adulthood. We explore the contribution of potentially mediating factors, including maternal employment, supervision, and the child's employment when not in school.

We focus on adolescents, as opposed to younger children, based on the literature on identity formation (e.g., Adams & Montemayor, 1983; Marcia, 1980; Wigfield & Wagner, 2005) — the process of integrating experiences and characteristics into a stable identity and making decisions that align with that identity, which begins in childhood but takes hold during adolescence. In addition, theoretical and empirical research has shown that adverse experiences (e.g., economic pressure) lead to negative affect and conflict in the household and suboptimal

parenting, which can compromise adolescents' development of competencies that protect them from those risks and increase their risky behavior (IOM 2011). Finally, youth in this age range are at a critical stage of their lifecourse trajectories. Focusing specifically on high school students captures teens in a particularly important period of autonomy development when their decisions can have long term consequences for their own future economic success and health as well as for society more broadly (Steinberg, 2014).

Background

Welfare reform in the United States

The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, often referred to as welfare reform, ended entitlement to welfare benefits under Aid to Families with Dependent Children (AFDC) and replaced AFDC with Temporary Assistance for Needy Families (TANF) block grants to states. Key features of the legislation were time limits on cash assistance and work requirements as a condition for receiving benefits. States were granted considerable latitude in establishing eligibility and program rules subject to the national guidelines under PRWORA that mandated work requirements and a 5-year lifetime limit on the receipt of cash assistance.

Although welfare reform is often dated to the PRWORA legislation, reforms started taking place in the early 1990s when the Clinton Administration expanded the use and scope of "welfare waivers" to allow states to carry out experimental changes to their AFDC programs. Although not federally mandated, waivers were implemented in the majority of states by the time the federal PRWORA was enacted in 1996. Many policies and features of PRWORA, such as work requirements and time-limited welfare receipt, were integral parts of these earlier programs.

Major statewide waivers—as typically operationalized in research studies and originally defined in a 1997 report by the Council of Economic Advisors (CEA) (CEA 1997) as those that substantially altered the nature of AFDC with respect to work requirements and incentives, time limits, and family caps—were introduced in 29 states over a period of 53 months, and TANF was implemented in all states over a period of 17 months (Appendix Table 1). Considering both waivers and TANF, states implemented any welfare reform over a period of 64 months, from October 1992 through January 1998.

Empirical studies of effects of welfare reform on behaviors in adolescence and beyond

All previous studies of the effects of welfare reform on social behaviors of the next generation of which we are aware have focused on antisocial or delinquent behaviors; that is, none studied prosocial behaviors. Syntheses of pre-PRWORA welfare reform experiments, which included features such as work requirements and time limits that later were included in the PRWORA legislation, did not find consistent evidence of effects of work incentives on adolescent delinquent behaviors such as having trouble with the police or being suspended or expelled from school (Gennetian et al., 2002, 2004; Grogger & Karoly, 2005). However, the welfare experiments were conducted in specific contexts, tended to have small samples of adolescents, and often did not measure adolescent behavioral outcomes.

Aside from the studies focusing on fertility and high school completion discussed earlier, there have been few quasi-experimental studies of effects of welfare reform on adolescent behaviors.[2] Two recent studies exploited differences in the implementation of welfare reform in

[2]There have also been few quasi-experimental studies of effects of maternal employment on adolescent behaviors. An exception is Aughinbaugh and Gittleman (2004), which estimated the effects of maternal work in the past 3 years on teen alcohol, cigarette, and marijuana use with data from the National Longitudinal Survey of Youth 1979 and family fixed effects models and found no significant effects, even for a subsample of unmarried mother households. However, the effects in this study were imprecisely estimated.

the U.S. across states and over time to identify causal effects of welfare reform on youth arrests for drug-related (Corman et al., 2017a) and non-drug-related (Corman et al., 2017b) crime using arrest data from the Federal Bureau of Investigation merged with implementation dates of welfare reform in each state. Corman et al. (2017a) found suggestive evidence that welfare reform led to increases in drug-related arrests for teens ages 15 to 17; the effects appeared to be stronger for boys than for girls, but the estimates were not always statistically significant. Corman et al. (2017b) focused on crimes categorized as serious (violent offenses such as assault, rape, and murder, as well as serious property offenses such as burglary, possession of stolen property and vandalism) and minor (such as disorderly conduct, driving under the influence, and curfew and loitering law violations) and found that welfare reform led to reduced arrests for minor crimes among youth ages 15–17 with similar estimates for boys and girls, but that it did not affect youth arrests for serious crimes.

The estimates from the Corman et al. studies of welfare reform on teen arrests could reflect changes in behavior but could also potentially reflect welfare-reform-induced changes in reporting of teen crime (perhaps as a result of changes in maternal employment or supervision) that changed the probability of arrest. The latter would still be a welfare reform effect but would not reflect adolescents' behavioral responses. In addition, arrest data do not include crimes that do not result in arrests and only capture behaviors that are sufficiently severe and apparent that they result in chargeable criminal arrests. For these reasons, it is important to focus on behavior rather than arrests. In addition, arrest data contain limited information on the characteristics of individuals committing the crimes; although they include sex and age of arrestees, they do not include characteristics such as maternal employment and parental marital status.

Recently, Hartley, Lamarche and Ziliak (2017) estimated the effects of welfare reform on

the intergenerational transmission of welfare participation using data from the Panel Study of Income Dynamics in difference-in-differences models and found that welfare reform attenuated the "intergenerational transmission" of welfare participation by more than 50 percent. A large intergenerational decline in welfare participation is to be expected given the dramatic retrenchment of the cash assistance safety net. As such, much of the effect likely reflects reduced access to welfare in the second generation. However, welfare reform may have increased family resources, led mothers to model socially desirable behavior, led mothers to become more engaged parents, and/or dampened youths' expectations about welfare as a long-term option, which may have favorably affected adolescents' behavior and potentially their longer-term outcomes including self-sufficiency (i.e., non-reliance on welfare). That said, some families may not have achieved greater financial security under the new regime, may have confronted new work/family challenges (such as time available for child supervision), and/or may have experienced increased stress and conflict within the household, which could adversely affect adolescents' behavior and potentially their longer-term outcomes. In the latter case, lack of welfare participation of the next generation would not be a marker of economic success. Thus, without understanding the circumstances behind the overall reduction in welfare participation in the next generation, welfare reform cannot necessarily be considered a success in this regard. It is important to consider more proximal and clearly favorable or unfavorable outcomes in order to identify who was helped and who was harmed by the reforms.

Overall, there is little past literature directly relevant to our focus on the effects of welfare reform on teenage social behaviors. The two most directly relevant studies focused on arrests, which are an imperfect proxy for delinquent behavior, and those studies were limited in terms of exploring potential heterogeneous treatment effects and underlying mechanisms and had limited

power to detect effects for certain types of crime. In this study, we focus on teenage social behaviors more generally, considering prosocial as well as delinquent behaviors. Delinquent behaviors of adolescents can be precursors to criminal careers (Loeber, Farrington & Petechuk, 2013), and studies have found links between community engagement in adolescence (defined various ways including participation in school clubs or organizations, participation in community service organizations, and volunteering for social causes, depending on the specific study) and civic participation, educational attainment, and lower rates of arrest in emerging adulthood (see Chan, Ou & Reynolds, 2014 for a review).

Expected effects

Employment of low-skilled women increased as a result of welfare reform, with an upper-bound estimate of 27 percent (Fang & Keane, 2004; Ziliak, 2016). This employment effect may have increased household incomes, which could improve teens' behaviors. E.g., Akee et al., (2010) found that a positive income shock led to increases in parental supervision (which Aizer (2004) found was associated with decreases in 10–14-year-old children's alcohol/drug use, skipping school, stealing, and hurting others in a disadvantaged sample) and lower levels of drug dealing and minor crimes among teenage children. However, maternal employment could lead to a net increase in constraints (e.g., if increases in income do not offset transportation and childcare expenses, or by decreasing time available for supervision or parental involvement), which could lead to undesirable effects on teens' behaviors.

Real incomes of unmarried mothers increased by approximately 25% between 1993 and 2002, while real income from public assistance in 2002 was about 20% of its 1993 level (Fang & Keane, 2004). However, unmarried mothers—particularly those who were employed—experienced high rates of financial hardship, poor health, and unreliable child care in the post-

PRWORA era (Teitler et al., 2004). These potentially offsetting effects suggest that the effects of welfare reform on families may vary by household composition.

The effects of welfare reform on marriage, cohabitation, and non-marital fertility have been mixed or weak (Ziliak, 2016), suggesting that if household composition is an important pathway between welfare reform and teen behaviors, it would operate through other aspects of living arrangements. Bitler, Gelbach, and Hoynes (2006), focusing on where children live rather than whether a woman lives with her child (because the Current Population Survey, like most datasets, does not document where a child lives if not with a parent), found that pre-PRWORA waivers were associated with children being less likely to live with unmarried parents, more likely to live with married parents, and more likely to live with neither parent (living instead with a grandparent or other relative). These findings, which pertain to the waivers but not national welfare reform under PRWORA, suggest that child supervision may not have decreased as a result of welfare reform.

The reforms could also affect teen behaviors through channels other than maternal employment, income, and supervision. Welfare reform was a strong intervention that was designed to not only move women from welfare to work, but also to change the normative climate from a culture of dependence to one of personal responsibility and precluded welfare reliance as a long-term option for the next generation by time-limiting cash assistance. As such, it may have led teens to focus more on school and community activities as investments in human and social capital. The reforms led to reductions in high school dropout (Dave et al., 2012; Kaestner, Korenman & O'Neill, 2003; Miller & Zhang, 2012), and thus would have reduced time and opportunities for engaging in delinquent behaviors and possibly decreased the costs of

engaging in school-related activities. [3] Additionally, recent studies found that welfare reform led to decreases in women's crime (Corman, Dave & Reichman, 2014) and substance use (Corman et al., 2013) and increased their civic participation in the form of voting (Corman, Dave & Reichman, 2017), which may have led them to set a positive example for their children and be more engaged parents, which would be expected to improve their children's behaviors. However, welfare reform also could adversely affect parenting quality or quantity, perhaps through increased stress and conflict within household, and ultimately lead to unfavorable teenage behaviors. Notably, Paxson and Waldfogel (2003) found suggestive evidence that early welfare reforms led to increased cases of child maltreatment.

As indicated earlier, previous studies of effects of welfare reform or maternal employment on teen social behaviors have rarely considered differences by gender,[4] but other studies have shown that: (1) Females are less likely than males to commit crime, and factors that affect criminal behavior generally have less of an effect for females than for males (e.g., Levitt & Lochner, 2001), suggesting that the effects would be stronger for boys than girls. (2) Disruptive events have more adverse effects on behaviors of boy teens than girl teens (e.g., Bertrand & Pan, 2013; Kling et al., 2005), suggesting that effects would be stronger for boys, if welfare reform is experienced as a disruptive event. (3) Girls are more likely than boys to take care of younger siblings (East, Weisner & Slonim, 2009) and there is some evidence that this type of caretaking

[3] As indicated earlier, there is evidence suggesting that PRWORA led to decreases in teen fertility (Kaestner, Korenman & O'Neill, 2003; Koball, 2007; Lopoo & Deleire, 2006; Offner 2005), at least in part through its "minor mother" requirements. It is not clear a priori whether or how a reduction in fertility would translate to social behaviors.

[4] As far as we know, the only exceptions were Aughinbaugh and Gittleman (2004), which found no overall or gender-specific effects on teen substance use, sexual activity, and conviction for criminal behavior, but had limited power to detect effects for unmarried-mother families; Gennetian et al. (2002), which revealed no overall gender-specific effects of early welfare experiments on suspension or expulsion from school; Corman et al. (2017a), which found some evidence of stronger effects for boys than girls on drug arrests; and Corman et al. (2017b), which found similar effects for boys and girls on arrests for minor crimes.

increased in the aftermath of welfare reform (Hsueh & Gennetian, 2011), suggesting that girls would have fewer opportunities than boys to engage in school and community behaviors, both anti- and prosocial, as a result of welfare reform. (4) Girls are much more likely than boys to rely on welfare when they are adults—e.g., fewer than 15% of adult TANF recipients in 2015 were male;[5] this reality suggests that effects of welfare reform on behaviors would increase favorable behaviors and reduce unfavorable behaviors of girls, if youth are forward looking in this regard. (5) Girls would be more subject than boys to potential maternal role modeling effects (Bussey & Bandura, 1984; Perry & Bussey, 1979), suggesting that welfare reform would lead to increases in favorable behaviors and reductions in unfavorable behaviors of girls—if welfare reform led to changes in maternal role modeling.

Overall, the patchwork of findings relevant to various potential mechanisms suggests that the effects of welfare reform on teenage behaviors would be the product of potentially competing forces, may differ depending on the specific behavioral outcome, and may differ by gender. The various plausible scenarios make it difficult to anticipate a generalized pattern by gender but point to the potential importance of exploring gender-specific effects.

Data

For our main analyses, we use restricted data from the 10th and 12th grade surveys of Monitoring the Future (MTF), an annual nationally-representative survey of high school students, for the years 1991 through 2006.[6] We use 1991 as the starting point because that was the year the MTF began surveying 10th graders and that year also preceded welfare reform in all states. We use 2006 as the endpoint in order to allow all states to have fully implemented the

[5] In the U.S. in 2015, there were 112,300 adult male TANF recipients (Table 17) compared to 631,957 adult female TANF residents (Table 18). From Characteristics and Financial Circumstances of TANF Recipients, Fiscal Year 2015 (ACF, 2016).
[6] More information about the MTF study design can be found at http://www.monitoringthefuture.org/purpose.html.

reforms and to avoid conflating our results with the effects of the Great Recession that started in the last quarter of 2007. The MTF is administered at over 400 public and private schools, providing representative samples of students in the 8th, 10th, and 12th grades. Between 13,000 and 19,000 students are surveyed each year in each of the three grades. We limit our primary sample to high school students (grades 10 and 12) that are minors (<18 years old), but also consider models that also include the 8th graders, who are in an age range for which many of the behavioral outcomes examined are less salient and gender differences in development are more pronounced compared to high schoolers.

MTF began in 1975 and was designed for the purpose of studying substance use and related attitudes and behaviors among youth, teens, and adults. It is one of two large Federal surveys used to track adolescent drug use in the United States (Substance Abuse and Mental Health Services Administration, 2012).[7] Questionnaires are administered at school rather than at home so that parents are never physically near the teen while he/she is completing the survey. Teens are assured that their responses are confidential and private, and the data collection protocol was designed to minimize underreporting of sensitive information.[8]

The MTF is particularly well-suited for our study, since it allows us to have an observation period that envelops welfare reform, has large sample sizes both overall and within states, and includes high-quality information on many relevant pro-and antisocial behaviors and other relevant factors. Multiple "forms" of the survey are administered such that some questions are asked only of random subsamples of respondents. For the 10th graders, there were two

[7] The other is the National Survey on Drug Use and Health (NSDUH), for which geographic indicators going back to the early 90s are not publicly available. In any case, the MTF is more suitable for our study because the NSDUH is limited in measures of delinquent behaviors, does include any prosocial behaviors, includes a smaller teen sample, and was administered in the household when parents might be present.
[8] Source: http://www.monitoringthefuture.org/purpose.html Accessed 11/29/18.

different forms from 1991 through 1996 and then four different forms from 1997 onward. For the 12th graders, there were six different forms. For this reason, sample sizes vary across behavioral outcomes.

In additional analyses, we use restricted data from the 1979 cohort of the National Longitudinal Survey of Youth 1979 (NLSY79) linked with information on their children from the Child Self-Administered (CS) and Young Adult (YA) Self-Report surveys. The original sample consisted of over 12,000 youth who were ages 14 to 22 years in 1979. The self-administered CS and YA surveys follow the biological children of the women in the original NLSY79 sample. The CS began in 1988 and has been administered biannually (in even-numbered years) to children 10 years and over and includes questions relating to sensitive antisocial behaviors such as hurting others and vandalizing property, as well as prosocial behaviors such as volunteering. Starting in 1994, the NLSY implemented the YA survey, which was administered to children who were 15 years old or more, and only administered the CS (which had much more detailed data on youth behaviors than the YA surveys) to children ages 10–14. In order to consider a sample as comparable as possible to that in our MTF analyses, we created a cohort of teens ages15 to 17 for the years 1990 through 2006.

The MTF is a school-based sample, and thus representative of youth enrolled in and attending school on the survey day. The NLSY, on the other hand, is a representative sample of all youth born to the original NLSY cohort of mothers. The NLSY is also longitudinal, permitting an alternate identification strategy (sibling fixed effects) and an exploration of potential compositional selection, neither of which are possible with pooled cross-sectional data. However, because of the transition to the YA surveys for youth age 15 and older in 1994, which focused more on adult issues than teenage behaviors, the available outcomes in this dataset were

limited. Moreover, much smaller sample sizes in the NLSY than in the MTF provided limited statistical power.

Measures

Outcomes

In our main analyses, which used the MTF data, we consider three different prosocial behaviors and a large set of delinquent behaviors. For convenience, we break down the delinquent behaviors into antisocial behaviors and substance use, recognizing that the use of tobacco and alcohol in minors is illegal (as is the use of marijuana and other illicit drugs) and therefore antisocial. Each of the behavioral outcome measures from the MTF is defined below.

Prosocial (favorable) behaviors

*Volunteering***.** Students were asked how often they participated in community affairs or volunteer work. Categories ranged from "never" to "almost daily." We categorized students as having volunteered if they participated at least once a month.

Participation in school clubs and *participation in school athletics***.** Students were asked about the extent to which they participated in various types of school activities during the school year. Response categories ranged from participated "not at all" to participated to a "great extent." The categories were school athletics, school newspaper or yearbook, music or other performing arts, and other school clubs or activities. Using this information, we created two different behavioral outcome measures. The first, *participation in school clubs*, was coded as a "yes" if the student reported that he/she participated in school newspaper or yearbook, music or other performing arts, or other school clubs or activities (other than athletics) to a "considerable" or "great extent." The second, *participation in school athletics*, was coded as a yes if the respondent reported that he/she participated in school athletics to a considerable or great extent.

Antisocial behaviors

Skipping school. Students were asked how many whole days of school they missed in the past four weeks due to "skipping" or "cutting." We coded them as skipping school if their answer was once or more.

Damaging property. Students were asked how often they had damaged school property on purpose in the last 12 months. We coded them as having damaged property if they answered once or more often.

Involved in fights. Students were asked how often, in the past year, they had gotten into a serious fight in school or at work. Categories ranged from none to 5+ times. We coded this outcome as a "yes" if the student responded that he or she had been in a serious fight at least once.

Stealing. In two separate questions, students were asked how often in the past year they had taken something not belonging to them valued at under $50 or worth over $50, respectively. We coded them as having engaged in stealing if they answered "at least once" to either question.

Hurting someone. Students were asked how often they had hurt someone badly (physically) in the past 12 months. We coded them as having hurt someone if they responded that they had done this once or more often.

Substance Use

We consider four categories of substance use: marijuana, alcohol, other illicit drugs, and cigarettes. For marijuana, alcohol, and other illicit drug use, students were asked how many times they had engaged in the activity during the past 30 days. For cigarette smoking, they were asked about the frequency in the past 30 days. For illicit drugs other than marijuana/hashish, students were asked about the following specific types of drugs, which included substances

without a doctor's prescription: LSD, psychedelics other than LSD, crack cocaine, cocaine in any other form, amphetamines, barbiturates,[9] tranquilizers, heroin, narcotics other than heroin, and inhalants. For all four categories of substance use, students were coded as having engaged in the activity if they reported that they had done so (for other illicit drugs, any of the types listed) at least once in the previous month. In addition, we combine all substances into a variable called *any substance use*, which is coded positive if the respondent reported using any of the substances in the past 30 days—alcohol, marijuana, other illicit drugs, or cigarettes. Thus, the substance use variables are *marijuana use, alcohol use, smoking, other illicit drug use,* and *any substance use.*

Outcomes in NLSY analyses

As indicated earlier, a key disadvantage of the NLSY for this study is a paucity of outcomes that are consistently measured for youth ages 15–17. The two relevant outcomes that we were able to consider using the NLSY, which also happened to be similar to outcomes we are investigating in the MTF, were whether the youth had hurt someone and whether the youth had used cigarettes, alcohol, or marijuana.[10]

Hurting someone. In the CS survey in 1990 and 1992, teens were asked, "In the last year, about how many times have you hurt someone badly enough to need bandages or a doctor?" We coded any frequency greater than "never" as a yes. In 1994, 1996, and 1998, the YA survey asked: "In the last year, have you ever hurt someone badly enough to need bandages or a doctor?" This question was not asked in 2000. In 2002, 2004, and 2006, the YA survey asked how often (in the past year) the teen had hurt someone badly enough to need bandages or a doctor. We coded any frequency greater than "never" as a yes.

Any substance use. The NLSY consistently asked teens whether they had smoked

[9] In 2004, this category was changed to barbiturates/sedatives.
[10] Information on the use of other illicit drugs is very limited in the NLSY and thus cannot be used for this analysis.

cigarettes, whether they had consumed alcohol, and whether they had used marijuana in the past three months. We combined these into a measure of whether the teen had used any of these substances in the past three months.

Welfare reform

Following the convention in the welfare reform literature (Blank, 2002), we exploit differences in the timing of both AFDC waivers and TANF implementation across states. For waivers, we consider whether, in a given year and month, a given state had a statewide AFDC waiver in place that substantially altered the nature of AFDC with regard to time limits, work requirements, earnings disregards, sanctions, and/or family caps, based on the CEA classification (1997). For TANF, we consider whether, in a given year and month, the state had implemented TANF post-PRWORA. In most specifications, we include a single indicator for any welfare reform (AFDC waiver or TANF). In supplementary models, we use separate indicators for AFDC and TANF.

We matched the timing of each phase of welfare reform to the teens' records in the MTF and NLSY based on maternal state of residence[11] and year and month of interview. A teen is considered exposed to welfare reform if the mother resided in a state in which welfare reform was implemented and had been in effect for at least 12 months—i.e., welfare reform was implemented in that state at least 12 months prior to the year and month of interview. The one-year lag addresses the retrospective nature of the youth outcomes, many of them capturing participation over the past month, past 3 months, or past year, and also allows for a lag between maternal exposure to welfare reform, maternal responses to welfare reform, and children's behavioral responses. We explore lagged effects more formally in supplementary analyses

[11] State indicators are available in restricted MTF and restricted NLSY data.

described later.

Covariates and sample selection variables

Individual-level covariates in all analyses include the child's age, grade in school, and race/ethnicity (white, black, or other, with the last category including Hispanics). Mothers' marital status and education are used to define target and comparison groups for our analyses and education is controlled for as relevant (e.g., when comparing groups with mothers with less than or equal to a high school education, we control for maternal high school completion). State/year covariates include unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number of Medicaid beneficiaries, numbers of National School Breakfast and Lunch Program beneficiaries, and population.

Mediators

In our analyses using the MTF data, we consider the following variables as potential mediators of effects of welfare reform on adolescent behaviors: whether the teen engaged in paid work when out of school, whether the teen was home alone for 4+ hours after school, and whether the mother was employed. We also considered continuous variables for the numbers of hours the teen worked for pay and was home alone after school.

Methods

Baseline difference-in-difference-in-differences

We employ a quasi-experimental difference-in-difference-in-differences (DDD) research design, which exploits variation in the timing of welfare reform implementation across states in conjunction with comparisons across treatment and comparison groups and is standard in the economics literature on evaluating the effects of welfare reform. The following reduced-form baseline DDD specification directly relates changes in children's behaviors to their exposure to

welfare reform, for the target group relative to a comparison group:

$$(1) \quad D_{imst} = \alpha + (Welfare_{st-12} * Target_{mst})\,\Pi_1 + Welfare_{st-12}\,\Pi_2 + X_{imst}\,\beta + V_{mst}\,\lambda + Z_{st}\,\beta + State_s\,\Omega + Time_t\,\Psi + \varepsilon_{imst}$$

A given behavior (*D*), for the i^{th} child born to mother *m* residing in state *s* and observed at time *t*, is a function of welfare policy (*Welfare*), measured here by an indicator for whether a given state had in place a major AFDC waiver (prior to enacting TANF) or had implemented TANF for at least 12 months. As indicated earlier, we build in the 12-month lag ($Welfare_{st-12}$) because some of the behavioral measures reference past year participation (see Table 1) and to allow time for the implementation of welfare reform to affect mothers (e.g., their employment) and youth behaviors.[12] In alternate specifications, we use separate indicators for the two phases of welfare reform that capture whether the state had an AFDC waiver in place for at least 12 months and whether the state had implemented TANF for at least 12 months. We control for a vector of child characteristics (*X*) and a vector of maternal characteristics (*V*).

Models are estimated separately by the child's gender because, as discussed earlier, there are many reasons to expect that boys and girls would respond differently to the policy shift. We estimate all models using Ordinary Least Squares (linear probability models) and report standard errors that are adjusted for arbitrary correlation in the error term (ε) across and within individuals in a given state, and hence clustered at the state level.[13]

To account for other potentially confounding policy shifts, we include a rich set of time-varying state factors (*Z*), detailed earlier and in table notes. Models further include *State* and *Time* (month/year of interview) fixed effects, which control for time-invariant state heterogeneity, national trends, and seasonal variations in youth outcomes.

[12] We explore the timing of the effects more formally in an event study framework.
[13] Our estimates are not sensitive to estimation via logit or probit regression.

The population of interest is children born to *all* women at risk of relying on public assistance, not just children born to current or former welfare recipients. Traditionally, the welfare caseload has consisted primarily of low-educated unmarried mothers (Bitler & Hoynes, 2010). This at-risk population is the target group of women for whom welfare policy would be expected to have the largest effects on employment, income, and other household conditions and potentially the largest behavioral effects, if any exist, on their children. While we control for a large set of time-varying state-level factors, the possibility of omitted variables remains. The DDD framework addresses this issue by considering a comparison group – individuals similar in many ways to the target group but unlikely to participate in public assistance programs and therefore not expected to be affected by welfare reform policies. In the above specification, *Target* represents a dichotomous indicator equal to one if the child is in the target group (has a low-educated unmarried mother and is thus at risk of being on welfare) and zero if in the child is in the comparison group (has a mother who is not at risk of being on welfare; defined below). The parameter vector of interest is Π_1, the coefficients of the interaction terms between the policy measures (*Welfare*) and the *Target* group indicator, which represent the reduced-form effects of exposure to welfare reform on the children's behaviors operating through any and all reinforcing and/or offsetting channels.

In these models, the error terms (ε) for a given individual *i* are likely to be correlated across the various outcomes since behaviors tend to be clustered together within individuals. Accounting for this cross-equation correlation across outcomes can increase statistical power and improve the efficiency of estimates. In alternative specifications, we transform the outcome variables into a consistent scale, standard normal deviates [(Y – mean)/standard deviation] and estimate the effects of welfare reform on youth behaviors using a seemingly unrelated regression

(SUR) framework, allowing us to test joint hypotheses across equations. These estimates are evaluated as changes in standard normal deviations of the dependent variable, and we test whether the average estimate across models is statistically different from zero. This approach has the added advantages of bypassing issues related to multiple comparisons and testing and yielding a consistently-defined average magnitude across outcomes, making it straightforward to compare effects across gender and model specification.

Assessing validity of DDD

The key assumption necessary for the DDD estimate to represent an unbiased causal effect is that in the absence of welfare reform, unobserved time-varying state factors would affect the target and comparison groups similarly. We follow the literature by utilizing a target group of unmarried mothers with at most a high school education and a comparison group of similarly low-educated married mothers. As marriage generally precludes eligibility for cash assistance, low-educated mothers who are married are at much lower risk of welfare receipt than those who are unmarried and thus much less likely to be affected by welfare reform.[14] Appendix Table 2 descriptively shows that the outcome means at baseline (in survey years 1991 and 1992, before any welfare reform was implemented) were generally quite similar in the target and comparison groups.[15] In supplementary models described later, we consider an alternative comparison group.

The validity of this approach depends on: (1) welfare reform being unrelated to unobserved state-level factors that are associated with the outcomes, and (2) similar trends in l

[14] Data from the 2014 NSDUH indicate that about 14% of unmarried mothers with at most a high school education received welfare, compared with only about 2% of similarly-educated married mothers. The NSDUH does not differentiate welfare/cash assistance from child care and job placement assistance; as such, a portion of the 2% of low-educated married mothers receiving assistance likely captures child care or job placement assistance.
[15] The MTF Surveys are fielded in school during the spring of each year. Thus, the 1992 MTF predates the first implementation of welfare reform by any state (see Appendix Table 1).

outcomes for the target and comparison groups before the implementation of welfare reform ("parallel trends" assumption). We assess the importance of unobserved time-varying state-specific factors and pre-welfare reform trends by adding 1-, 2-, and 3-year lags of the state's unemployment rate, poverty rate, personal income per capita, and welfare caseloads to the baseline model.[16] This specification addresses the possibility that the timing of welfare reform implementation was dependent on, and thus endogenous to, the state's economic conditions and welfare history. We further include state-specific linear trends, which allow all states (including early- and late-reform states) to have differential systematic trends over the entire sample period. We also expand the baseline DDD specification into an event study framework, which decomposes the policy effects into leads and lags and allows us to more formally identify significant differential trends between the target and comparison groups prior to policy implementation.

Alternate identification and fixed effects

We assess the robustness of the estimated effects of welfare reform to alternate comparison groups and by exploiting the MTF and NLSY data to control for fixed effects at narrower levels than the state of residence. First, with the MTF, we use an alternative comparison group that also has been established in the welfare reform literature—higher-educated unmarried mothers (specifically, unmarried mothers with more than a high school education). Participation in public assistance is associated with low maternal education (Bitler & Hoynes, 2010); as such, it is a valid exercise to compare outcomes for otherwise similar mothers

[16] The lagged economic indicators are in addition to the contemporaneous measure of the state's economy included in the vector Z. However, we do not control for contemporaneous welfare caseloads as these would be endogenous controls that are a function of welfare reform. The lagged measures of welfare caseloads can help account for the possibility that the state-specific timing of welfare reform implementation was a function of the state's previous levels and trends in caseloads. However, given the high degree of correlation in a state's welfare population over time, the estimates from these models are only suggestive of the sensitivity of our estimates to additional selection on observed state-specific factors.

who have higher levels of education and should be less affected by welfare reform. Second, with the NLSY, we exploit variation in policy exposure across siblings by including household (mother) fixed effects. This approach provides an alternate comparison group and identification strategy while controlling for all non-time varying heterogeneity across mothers. In these specifications, the thought experiment involves comparing changes in outcomes across two (or more) siblings, one who is exposed to welfare reform and the other who is not exposed to welfare reform but is otherwise identical.[17] These models essentially add a fourth "D," by comparing siblings who were and were not exposed to welfare reform when they were of a similar age. Third, with the MTF being a school-based survey, in alternate specifications we control for time-invariant school-specific unobserved characteristics by adding school fixed effects.[18] Since the policy variation is at the state level, we do not expect school-specific unobserved characteristics to introduce any bias. However, accounting for heterogeneity across schools can reduce sampling variance and affect standard errors. Robustness of the estimates to adding school fixed effects also provides an indirect check on any systematic compositional selection issues with the school-based sampling in the MTF.

Additional analyses

The DDD effect, based on Equation 1, captures the average treatment effect over the post-policy period. However, there may be stronger effects among teens exposed to the policy shift for longer periods of time. To explore the extent to which the effects of welfare reform on

[17] For example, we are comparing changes in outcomes across two siblings, one of whom was exposed to welfare reform at a given age and the other of whom had not yet been exposed at that same age. Since this comparison is within a given state and both siblings reside in the same state, the variation in exposure comes from timing of exposure across different cohorts. The year fixed effects and the third "D" (comparison of children born to low-educated unmarried vs. married mothers) account for general trends and period effects; that is, they address the concern that the difference in outcomes across siblings is not due to the passage of time.

[18] The school fixed effects are in lieu of state fixed effects, as the nesting of school districts within states automatically controls for unobserved factors at higher levels of geography. Controlling for county fixed effects in lieu of school fixed effects had no material effects on our estimates.

youth behaviors are persistent or cumulative, we estimate models of the effects of welfare reform by duration of exposure. This alternative specification also serves as a dose-response check. Additionally, we present estimates from a more flexible event study specification, which decomposes the timing of the policy effects into leads and lags and allows us to gauge (and partly adjust for) differential pre-policy trends between the treatment and comparison groups and assess lagged policy responses in the behavioral outcomes.

Earlier, we described a number of causal channels that may underlie reduced-form DDD effects of welfare reform on youth behavioral outcomes, many of which we are not able to explore with our data. However, we are able to take a broad look at the extent to which changes in youth behaviors attributed to welfare reform appear to be mediated by maternal employment, child supervision, and the child's after-school work for pay. To accomplish this, we re-estimate Equation 1 adding measures of these potential mediators and evaluate the extent to which the estimated effects of welfare reform can be explained by these factors. We do not control for these factors in our main models, as they represent more proximal potential effects of welfare reform that may lie along the pathway from welfare reform to youth behaviors.

Given that the target and comparison groups are defined according to characteristics (maternal marital status and education) that are observed in the cross section over a number of survey years, potential bias due to compositional selection is a concern as these characteristics may have been affected by welfare form. In particular, there may be selective composition with respect to classifying an individual as low- or high-educated, as previous research found that adult women's educational attainment was affected by welfare reform (Dave, Corman & Reichman 2012). We are less concerned about selection bias with respect to marital status, as prior research has generally found weak to no effects of welfare reform on marriage. In auxiliary

analyses using the MTF, we confirm that the both of these characteristics, which are used to define the target and comparison groups, have not changed significantly or systematically over the sample period. We also use the NLSY to more directly address the sensitivity of the estimates to potential compositional selection by exploiting the longitudinal nature of the data. Specifically, we compare estimates from models that define the target and comparison groups based on maternal characteristics at the time of the survey when the youth outcomes were measured (as in the MTF analyses) to those from models that redefine the target and comparison groups using baseline maternal characteristics, with baseline being the first year that the child entered the NLSY (i.e., a mother is assigned to the target or comparison group during the first survey year when we observe the child and that assignment is fixed over the sample period, regardless of any changes in the mother's marital status or educational attainment). The comparison of these estimates provides a sense of the extent to which our estimates from the MTF are biased by systematic changes in sample composition.

Results

Table 1 presents means of the behavioral outcomes and sample characteristics for our target and comparison groups by gender. Between 1/5 and 1/2 of teens engaged in prosocial activities; boys in the target group participated at approximately the same rate as boys in the comparison group, but the same was not true for girls. Girls were more likely than boys to volunteer and participate in school clubs, and less likely to participate in school athletics. Boys were more likely than girls to damage property, fight, steal, or hurt someone. Teens in the target group were more likely than those in the comparison group to engage in all of the antisocial behaviors (Table 1). Substance use was fairly prevalent among youth in both the target and comparison groups, with participation ranging from 10–12% (illicit drugs) to ~45% (alcohol)

among boys and ~10% (illicit drugs) to ~40% (alcohol) among girls. Overall, differences in substance use between the target and comparison groups and by gender were minimal.

We present baseline DDD estimates from Equation 1 in Tables 2–4 for prosocial behaviors, antisocial behaviors, and substance use, respectively, by gender. Table 2 presents the estimated effects of welfare reform on participation in volunteering, school clubs, and school athletics. Overall, there is little evidence that welfare reform led to any meaningful change in these activities for either gender, except for volunteering among girls, for which we find a significant reduction of about 1.5 percentage points (5.9% relative to the baseline mean).[19] Estimates also suggest a 1–2 percentage point reduction in participation in school clubs and athletics for boys, but the effects are not statistically significant at conventional levels.

Table 3 presents the estimates for skipping school, damaging property, fighting, stealing, and hurting someone. Here we find more consistent evidence that welfare reform was associated with an increase in these behaviors, with larger effects for boys than girls. Boys exposed to welfare reform were more likely to skip school (2.1 percentage points), damage property (4.1 percentage points), and be involved in a fight (6 percentage points). These effect sizes translate into 7–21% increases relative to the baseline means for the target group. We also find non-negligible effect magnitudes for stealing and hurting someone (8–11%) in the same direction, although the estimates are imprecise. For girls, we find a marginally significant increase only for skipping school (1.7 percentage points or 7%) and changes in other antisocial behaviors are close to zero in magnitude and have a negative coefficient in two of the four outcomes.

[19] In additional analyses not shown, we found no effects of welfare reform on time spent doing homework or working at a job for pay after school, suggesting that the reduction in volunteering attributed to welfare reform was not offset by an increase in these other productive uses of time. However, the MTF did not ask about other non-school-based athletics or clubs or caring for younger siblings; as such, we cannot rule out that welfare reform led to substitution from volunteering to other types of prosocial behavior.

The estimated effects of welfare reform on youth substance use are presented in Table 4. The DDD estimates in Table 4 suggest that welfare reform led to an increase in substance use for both boys and girls. The patterns mirror those for antisocial behaviors, with generally larger effects for boys than for girls (e.g., 3.3 vs. 2.3 percentage points, which translates to 5.8 vs. 4.3%, for any substance use). The increase in any substance use for boys is driven by smoking, marijuana, and other illicit drugs; girls were also more likely to smoke and use marijuana as a result of welfare reform, but not to use other illicit drugs. We do not find any significant effects of welfare reform on alcohol use for either gender.

For the results presented so far, we used a single indicator of any welfare reform, both for ease of comparison and to maximize variation and precision. Appendix Table 3 reports estimates that decompose the composite effect of welfare reform into separate effects of the AFDC Waivers and TANF. The patterns are consistent with those that used the single indicator of any welfare reform; there is some loss of precision in this specification, but the coefficient magnitudes and overlapping confidence intervals generally suggest similar effects across the two phases of welfare reform. This finding is not altogether surprising given that many of the early state reforms of AFDC were later incorporated into the federal reforms under TANF.

The results discussed thus far are robust to several model extensions, both in terms of patterns (across outcomes and gender) and effect sizes. Specifications reported in Appendix Table 4 control for lagged state-level economic conditions and welfare caseloads to address the potential endogeneity of the timing of welfare reform, which may reflect the state's recent economy and welfare caseloads. In Appendix Table 5, we control for state-specific trends and assess the sensitivity of the estimates to systematically (linearly) time-varying state unobserved characteristics and differential trends across states. Appendix Table 6 presents estimates that

adjust for time-invariant school-specific heterogeneity by including school fixed effects. Finally, we present DDD estimates based on an alternate comparison group (unmarried mothers with more than a high school education) in Appendix Table 7. As shown in Appendix Table 2, baseline pre-policy means for youth in the target group are closer to those of youth in this alternate comparison group for some outcomes. Both comparison groups have been employed in the welfare reform literature (for example, Corman et al., 2013; Corman, Dave & Reichman 2017). We find that our results are insensitive to the use of the alternative comparison group.

Estimates from Table 2–4 provide some evidence that welfare reform is associated with increases in antisocial behaviors and substance use of youth, with somewhat stronger effects for boys than girls. Table 5 presents the SUR results that account for cross-equation correlation across outcomes. For convenience, we focus on the DDD estimates derived from our baseline model that includes state covariates and state and time fixed effects, though we note here that the results are robust to the various alternative model specifications. Columns 1–4 present joint estimates across sets of outcomes for boys and Columns 5–8 present corresponding estimates for girls. In Columns 1 and 5, we consider joint effects across the three prosocial behaviors – volunteering, participation in school clubs, and participation in school athletics. From the first set of figures in these columns, we can see that welfare reform has no statistically significant effect across these outcomes. The figures in the following rows present the average estimated effects of welfare reform across the set of transformed outcomes and associated test statistics. For example, the average effect for boys can be interpreted as follows: Welfare reform is associated with a 0.03 standard deviation decrease in prosocial behaviors for boys and this estimate is on the margin of statistical significance ($p = 0.102$). For girls, there is a corresponding 0.02 standard deviation decrease in prosocial behaviors and the estimate is not statistically significant.

In Columns 2 and 6, we report cross-equation estimates for the antisocial behaviors. While the effects of welfare reform were not statistically significant for some of these outcomes, we can categorically reject the null hypothesis of no effect across all five outcomes at the one-percent level for boys and at the five percent level for girls. When looking at the average effect, here too we are able to categorically reject the null that the average effect is zero for boys; the combined DDD effect suggests that welfare reform is associated with a 0.082 standard deviation increase in antisocial behaviors. For girls, the effect is smaller (0.011 standard deviation) and not significant. For marijuana, alcohol, smoking, and other illicit drugs (Columns 3 and 7), there is strong evidence that the average DDD effect is not zero for either gender; the combined estimate points to a welfare reform-induced increase in substance use for both boys and girls, with a stronger response for boys (0.06 standard deviation vs. 0.04 standard deviation for girls).

In Columns 4 and 8, we fully maximize statistical power and account for correlated errors across all outcomes (prosocial, antisocial, and substance use) by grouping all outcomes together in the SUR framework. In order to implement this, it was necessary to redefine all of the outcomes in a consistent direction; thus we defined all outcomes in a socially favorable direction (e.g., from hurting someone to not hurting someone, from smoking to not smoking, etc.). We find consistent evidence that the joint effect of welfare reform across outcomes is statistically significant for both boys and girls, that welfare reform led to significantly worse behaviors, and that the overall effect is substantially more negative for boys (0.06 standard deviation decrease) than for girls (0.02 standard deviation decrease).

Our DDD specifications using an indicator for welfare reform implementation capture the net average effect of the policy change over the entire post-policy window – a period of roughly 8 years. In Panel A of Tables 6 and 7 for boys and girls, respectively, we investigate the extent to

which welfare reform had persistent or cumulative effects based on the duration of exposure to the new policy regime. For these models, we re-specified our policy measure as the number of years that the child had been exposed to welfare reform based on their age at interview, state of residence, and age when welfare reform was implemented in their state. The coefficient of interest is the interaction between the target group and number of years exposed, which measures the linear effect of an increase in exposure duration by one year for children in the target group relative to those in the comparison group. Here too, it is convenient to utilize the SUR framework in order to bring key patterns into focus and bypass issues related to comparisons across multiple outcomes. The results in Tables 6 and 7 are consistent with our main findings thus far. Greater exposure to welfare reform had detrimental effects on behaviors of boys; most generally, being exposed to welfare reform for an additional year led to an average unfavorable change in behaviors of about 0.006 standard deviations for boys (Model 4, Panel A, Table 6). When multiplying the exposure effects by 8 (average number of years of exposure among children in the target group), the effect sizes are similar to the average DDD estimates reported in Table 5. Greater exposure to welfare reform also had detrimental effects on behaviors of girls, and those were of substantially smaller magnitude than for boys as in our main models. These patterns suggest cumulative effects with respect to exposure to welfare reform for both genders.[20]

[20] Since duration of exposure is a function of age at initial exposure and current age, these effects may also capture age of initial exposure. E.g., consider two youths who are both 15 years old. One had been exposed to welfare reform for 7 years, and the other had been exposed for 3 years. If we find differences in behaviors across these two youths based on their duration of exposure conditional on age at interview, these differences may also reflect that the first youth was initially exposed when he was 8 years old and the second was exposed when he was 12. We do not have sufficient statistical power to disentangle age-at-exposure from duration effects. However, by imposing some parametric restrictions on one of the two effects, some evidence can be gleaned. We find crude evidence that youth who had been exposed to welfare reform between the ages of 11 and 13 tended to be most adversely affected. Controlling for the age at initial exposure, we continue to find significant duration effects, suggesting that the estimated in Tables 6 and 7 do not completely reflect variation in age at initial exposure. We do not present these results as they are highly imprecise and lack statistical power.

In Panel B of Tables 6 and 7, we further parse out the timing of the effects of welfare reform by estimating an event study. Specifically, we define windows of time with respect to welfare reform implementation (year and month of implementation in each state) capturing pre-policy leads (5+ years prior to implementation, 4 years prior to implementation, 3 years prior to implementation, and 2 years prior to implementation) and post-policy lags (within 1 year of implementation, 2 years post-implementation, and 3 years post-implementation, and 4+ years post-implementation), with 1 year prior to implementation as the reference category. For brevity, only the DDD interactions between these timing indicators and the target group are reported.

For boys, virtually all of lead effects are statistically insignificant (Panel B, Table 6), suggesting no differential trends between the target and comparison groups prior to policy implementation, and 14 of the 16 lead indicators are statistically insignificant. The two significant lead estimates are for open-ended lead category (5+ years pre-welfare reform), which essentially captures a sub-sample of states that implemented welfare reform in 1996 or later (generally, states that had not implemented pre-TANF waivers).[21] Consistently significant estimates that indicate welfare reform associated increases in unfavorable behaviors are confined to periods after welfare reform was already in effect, and the strongest effects appear after 2- or 3-year lag post implementation. Given that some of the hypothesized effect mechanisms on youth are indirect, driven first through changes in maternal outcomes (i.e., maternal work, household structure, supervision), a lag in the policy response is plausible.

For girls, the lead effects are generally statistically significant and there is less consistent evidence of lagged effects (Panel B, Table 7). We find a significant adverse effect only with

[21] While such timing analyses have been standard in the applied DD literature, these should be viewed as suggestive given recent evidence indicating that lags and leads in event study-type models with heterogeneous treatment effects may spuriously reflect treatment effects from other periods (Goodman-Bacon, 2018; Abraham & Sun, 2018).

respect to substance use, and, similar to the pattern for boys, there is some evidence of a lagged policy response, with stronger effects materializing about 2–3 years post welfare reform. Overall, the event study results indicate: (1) little systematic evidence of differential pre-trends for boys or girls; (2) more consistent evidence of increases in antisocial behaviors post-welfare reform for boys than for girls; (3) evidence of a welfare reform-associated increase in substance use for both genders, with larger responses for boys; and (4) when effects materialize, there is some evidence of a lagged policy-response, with strongest effects taking place 2–3 years post-reform.

The DDD estimates presented to this point are consistent with a causal interpretation, particularly for boys, given the numerous validity checks conducted and the robustness across specifications. As discussed earlier, these effects are plausibly driven by a number of different mechanisms, many of which are not observed or are imperfectly measured in the MTF, which was a school-based sample focusing on the students and included few questions about their parents. Nevertheless, we can explore the extent to which the effects we identified can be explained by some of the hypothesized pathways. These analyses are presented in Table 8. We rely on the SUR framework, grouping outcomes to conserve statistical power. For each group of outcomes, we present the average DDD effect (within the group), both excluding the potential mediators we can observe in the MTF and then including those potential mediators. For the mediators, we consider measures related to maternal employment, unsupervised time that the youth is at home after school, and youth employment. The top row of estimates in Table 8 reports the average DDD effect (from Table 5) without controlling for any of the mediators. The next row adds broad measures related to whether the teen engages in any paid work after school, whether the teen is home alone for 4+ hours each day after school, and whether the teen's mother is employed. The final row of estimates incorporates more refined measures of working after

school and being home alone after school—the number of hours for each.

Comparing the average effects when excluding and including the mediators, we find that about 14% of the overall effect for boys across the four substance use outcomes can be explained by the mediating factors considered, but that the mediators explain little of the observed effects for the other sets of outcomes. For girls, the overall effects of welfare reform on behaviors were smaller than those of boys, but the observed mediators are more predictive. Comparing the average effects when excluding and including the mediators, we find that the mediators can explain about 20–60% of the observed DDD effects for girls—59% for prosocial behaviors, 50% for antisocial behaviors, and 21% for substance use. We note that Angrist and Pischke (2008) caution against including mediators directly in models this way, as these factors are endogenous; as such, the estimates in Table 8 are exploratory. That said, these results are consistent with the patterns we would expect to emerge if welfare reform causally impacted youth behaviors through some of these observed mediating factors.

Next, we consider the effects of welfare reform on the children of married college-educated mothers as a placebo check, as we would not expect any statistically or substantively significant effects of welfare reform for this group. In fact, significant effects for children for this group would point to spurious time-varying state trends. Table 9 presents these estimates, which are DD estimates since no comparison group is utilized. It is validating that across all sets of outcomes, the joint and the average effects of welfare reform are statistically insignificant for this group and the size of the average DD effect is small and close to zero.

Table 10 presents estimates from the NLSY for adolescents ages 15–17 (an age range close to that of the 10^{th} and 12^{th} graders in our MTF sample), for hurting someone and any substance use. In Panel A, we present estimates for each outcome from the standard DDD model

with state and time fixed effects as well as from a model that incorporates household fixed effects. The latter exploits variation across siblings within households and relies on variation across discordantly exposed siblings within the household. As such, identifying variation is limited in these models, and this is reflected in the inflation of the standard errors. Both the standard DDD and the sibling fixed effects estimates suggest that welfare reform led to an increase in any substance use and the likelihood of hurting someone among boys. Effect magnitudes across both models and identification strategies are consistent. Furthermore, effects for girls, while positive, are statistically insignificant and smaller than those of boys, mirroring the pattern of results from the MTF. Finally, the bottom panel in Table 10 presents a check for compositional selection bias by redefining the target and comparison groups in the NLSY based on maternal characteristics at the time of the child's first interview.[22] We see little difference in the estimates when comparing Panel B to Panel A, suggesting that compositional selection is not confounding our main results. Given the data limitations of the NLSY, we consider these estimates as a secondary robustness check. That said, it is validating that the patterns of results are similar to those from the MTF when using an alternate (non-school based) dataset, incorporating household fixed effects, and addressing compositional selection.

Finally, estimates from models that included the 8th graders in addition to the 10th and

[22] In auxiliary analyses not shown, we used the MTF to test whether inclusion in the target or comparison groups was significantly associated with welfare reform, after controlling for state and period fixed effects. First, we defined an indicator for whether an observation was either in the target or comparison group vs. outside our analysis sample and regressed this on an indicator for welfare reform implementation (contemporaneous, 1-month lag, 3-month lag, and 1-year lag in alternate specifications) along with year and month of interview and state fixed effects. In these models, the coefficient on the welfare reform indicator ranged from 0.0030 to 0.0050 (p-values ranged from 0.73 to 0.87). Second, we limited the specification to the analysis sample, and regressed an indicator for being in the target group on the welfare reform indicator and included the requisite fixed effects. In these models, the coefficient on the welfare reform indicator ranged from -0.0011 to -0.0024 (p-values ranged from 0.77 to 0.94). Hence, there is no indication that welfare reform was systematically associated with any shift in the characteristics (marital status and educational attainment) used to define the target and comparison groups.

12th graders are attached in Appendix Tables 8–10. For the prosocial behaviors, the estimates are very similar (in terms of both magnitudes and significance) to those in our main models, although there is now also a marginally significant negative effect of welfare reform on school athletics for girls. For the antisocial behaviors, the estimates are generally smaller in magnitude than those in our main models, but only those for damaging property and fighting remain statistically significant for boys, and skipping school is no longer significant for girls. For the substance use behaviors, the estimated effects are smaller than those in the main models, and for both boys and girls, only marijuana is significantly associated with welfare reform.

Conclusion

As pointed out by Moffitt (2015), there have been surprisingly few studies of the effects on children of the substantial (and un-reversed) reduction of the cash assistance safety net for poor families that took place in the 1990s. This is particularly true for adolescents, who represent the next generation in a presumed transmission of welfare dependence. This study addressed this gap while testing the larger and fundamental "culture of poverty" argument that welfare leads to an intergenerational cycle of irresponsible behavior. Specifically, we investigated the effects of welfare reform on adolescent social behaviors, which are not only important for children's socioeconomic trajectories and societal well-being, but also can occur within a relatively short time frame and represent early observable consequences of the reforms for the next generation as they transition to adulthood.

When grouping behaviors into three categories (prosocial behaviors, antisocial behaviors, and substance use), we found no significant effects of welfare reform on youth prosocial behaviors, for either gender. For boys, welfare reform led to a significant increase in antisocial behaviors (skipping school, damaging property, fighting, stealing, hurting others), with an

average DDD effect of .08 standard deviations. For girls, there were no systematic effects of welfare reform on these behaviors other than a small welfare reform-associated increase in skipping school. For both boys and girls, welfare reform increased substance use (marijuana, cigarettes, other illicit drugs), although the average DDD effect was larger for boys than girls (about .06 standard deviations versus about .04 standard deviations). As median age of initiation is between 16 and 17 years for-ever users of cigarettes and marijuana and 18 years for ever-users of other illicit drugs (calculations from 2014 National Survey of Drug Use and Health), our effects on substance use are likely capturing this initiation margin.

Overall, we found no evidence of favorable effects of welfare reform on youth behaviors, but considerable evidence of unfavorable effects that were generally much larger for boys than girls. Of the various hypothesized (and potentially competing) mechanisms laid out earlier, that most consistent with this finding of stronger effects for boys than girls is that of disruptive events taking more of a toll on boys, perhaps through differential responses to welfare reform-associated maternal stress/anxiety, conflict between parents and children, or parental disengagement.[23] This potential scenario is consistent with previous findings of gender differences in the non-cognitive returns to parental inputs such as time, attention, and emotional connectedness during middle childhood and early adolescence (Bertand & Pan, 2013). Specifically, Bertrand and Pan found that boys' likelihood of "acting out" was greatly reduced when they received higher quality and quantity of parental inputs, while the relationship between parental inputs and acting out was much weaker for girls. Recent findings from experimental interventions that reduced youth crime and increased school engagement of disadvantaged boys in Chicago by shaping their decision making (specifically, getting them to take time and consider

[23] We found no evidence from our mediation analysis that the effects operated through adult supervision.

whether their impulses were appropriate) underscore that vulnerable boys can be responsive to structure and guidance (Heller et al., 2017). The stronger effects of welfare reform for boys than girls also could reflect differential levels of maturity or impulsivity of boys and girls at the same ages or, possibly, differential effects of welfare-reform-induced changes in contexts—e.g., household composition, place of residence, and/or peer groups.

It is also possible that other hypothesized mechanisms buffered the effects for girls (e.g., having assumed more responsibility for housework and childcare at home, having internalized the new reality that welfare is no longer a long-term option, or having responded to positive role modeling on the part of their mothers). Unfortunately, we were able to explore only a limited set of mechanisms with our data and we were only able to investigate those in a crude way.

There are few relevant previous studies with which to reconcile our findings. No previous studies of which we are aware have explored any of the outcomes investigated in this study. The most relevant studies are Corman et al. (2017a) and Corman et al. (2017b). Our results for marijuana and other illicit drug use are consistent with those of Corman et al. (2017a), which found suggestive evidence that welfare reform led to increases in drug-related arrests for teens ages 15 to 17 and that the effects appeared to be stronger for boys than for girls. Although arrests are not the same as illegal behavior, arrests are often used as proxies for criminal behavior in research studies. Corman et al. (2017b) estimated effects of welfare reform on youth arrests for non-drug related crimes, some of which are related to damaging property and hurting others. However, these types of crimes were not disaggregated from other types of crime, making it impossible to reconcile our result with that study.

Overall, the findings from this study suggest that while welfare reform may have had favorable effects on social behaviors of mothers (at least in terms of reduced crime and increased

civic participation in the form of voting, the only social outcomes previously studied in this context), the intergenerational effects on social behavior were not favorable, particularly for boys, and may have hindered the affected youths' socioeconomic trajectories. The results from this study do not support culture of poverty arguments that requiring poor mothers to work would make the next generation more responsible and suggest that the social gains of welfare reform for women have come at a cost to the next generation, particularly to boys who have been falling behind girls in terms of high school completion for decades (Murnane 2013). More generally, the results from this study underscore the importance of fully exploring the effects of policy changes, particularly those that implemented on a large scale and currently in effect, on the next generation and considering potential differential effects by gender.

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Table 1
Outcomes and Sample Characteristics
Across Target and Comparison Groups by Gender
Monitoring the Future (MTF) Surveys 1991–2006
Children in Grades 10 & 12 and ≤ 17 y

Variable	**Boys**		**Girls**	
Sample	Target	Comparison	Target	Comparison
	Mothers ≤ High School Graduate		Mothers ≤ High School Graduate	
	Unmarried	Married	Unmarried	Married
Prosocial behaviors				
Volunteering (≥ once a month)	0.198	0.207	0.245	0.282
Participating in school clubs	0.295	0.300	0.445	0.511
Participating in school athletics	0.412	0.438	0.261	0.334
Antisocial behaviors				
Skipping school (past 4 weeks)	0.286	0.222	0.260	0.216
Damaging property (past year)	0.229	0.210	0.106	0.096
Fighting (past year)	0.289	0.238	0.178	0.143
Stealing (past year)	0.375	0.340	0.231	0.215
Hurting someone (past year)	0.245	0.201	0.095	0.062
Substance use				
Marijuana use (past month)	0.251	0.179	0.190	0.143
Alcohol consumption (past month)	0.459	0.445	0.412	0.401
Cigarette smoking (past month)	0.273	0.250	0.271	0.263
Other illicit drug use (past month)	0.119	0.103	0.114	0.104
Any substance use (past month)	0.572	0.538	0.535	0.509
Sample characteristics				
Age (years)	16.07	16.03	16.05	16.01
Grade 10	0.746	0.727	0.694	0.686
Grade 12	0.255	0.273	0.306	0.314
White	0.506	0.709	0.494	0.701
Black	0.253	0.064	0.280	0.075
Other race/ethnicity (includes Hispanics)	0.241	0.227	0.226	0.224
Mother: Married	0.0	1.0	0.0	1.0
Mother: Less than high school graduate	0.239	0.178	0.289	0.211
Mother: High school graduate	0.700	0.766	0.641	0.718
Observations	11,136	44,129	16,415	53,160

Notes: Weighted means, based on the MTF sampling weights, are reported as column percentages. Reported number of observations is the maximum sample size; sample sizes are smaller for some variables owing to missing data.

Table 2
Effects of Welfare Reform on Youth Prosocial Behaviors
MTF 1991–2006

Sample	**Boys Grades 10 & 12 and ≤ 17 y**			**Girls Grades 10 & 12 and ≤ 17 y**		
Outcome	**Volunteering**	**Participating in School Clubs**	**Participating in School Athletics**	**Volunteering**	**Participating in School Clubs**	**Participating in School Athletics**
Model	**1**	**2**	**3**	**4**	**5**	**6**
Target	**Children of Unmarried Mothers With High School Education or Less**					
Comparison	**Children of Married Mothers With High School or Less**					
Welfare Reform	0.0088	-0.0007	-0.0083	0.0054	0.0266*	0.0061
	(0.0112)	(0.0162)	(0.0142)	(0.0104)	(0.0140)	(0.0090)
Welfare Reform*Target	-0.0064	-0.0132	-0.0195	-0.0149**	-0.0005	-0.0116
	(0.0087)	(0.0157)	(0.0150)	(0.0069)	(0.0150)	(0.0134)
Observations	44,939	21,515	21,830	54,964	27,719	27,904

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Table 3
Effects of Welfare Reform on Youth Antisocial Behaviors
MTF 1991–2006

Sample	**Boys Grades 10 & 12 and ≤ 17 y**					**Girls Grades 10 & 12 and ≤ 17 y**				
Outcome	**Skipping School**	**Damaging Property**	**Fighting**	**Stealing**	**Hurting Someone**	**Skipping School**	**Damaging Property**	**Fighting**	**Stealing**	**Hurting Someone**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Target	**Children of Unmarried Mothers With High School Education or Less**									
Comparison	**Children of Married Mothers With High School Education or Less**									
Welfare Reform	-0.0010	-0.0201*	-0.0216	-0.0538***	-0.0079	-0.0165*	0.0019	-0.0112	-0.0082	-0.0094
	(0.0135)	(0.0106)	(0.0147)	(0.0174)	(0.0167)	(0.0087)	(0.0085)	(0.0102)	(0.0122)	(0.0064)
Welfare Reform*Target	0.0207**	0.0414***	0.0601***	0.0304	0.0228	0.0169*	0.0075	0.0134	-0.0084	-0.0068
	(0.0095)	(0.0108)	(0.0203)	(0.0207)	(0.0175)	(0.0093)	(0.0091)	(0.0150)	(0.0100)	(0.0086)
Observations	52,359	20,800	17,477	20,906	17,411	66,398	26,481	22,348	26,570	22,295

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Table 4
Effects of Welfare Reform on Youth Substance Use
MTF 1991–2006

Sample	**Boys Grades 10 & 12 and ≤ 17 y**					**Girls Grades 10 & 12 and ≤ 17 y**				
Outcome	**Marijuana**	**Alcohol**	**Smoking**	**Other Illicit Drugs**	**Any Substance Use**	**Marijuana**	**Alcohol**	**Smoking**	**Other Illicit Drugs**	**Any Substance Use**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Target	**Children of Unmarried Mothers With High School Education or Less**									
Comparison	**Children of Married Mothers With High School Education or Less**									
Welfare Reform	0.0005	0.0093	-0.0070	-0.0120**	0.0056	0.0064	0.0107	-0.0085	0.0057	0.0097
	(0.0145)	(0.0115)	(0.0107)	(0.0058)	(0.0103)	(0.0095)	(0.0180)	(0.0155)	(0.0050)	(0.0194)
Welfare Reform*Target	0.0321***	0.0168	0.0229**	0.0190**	0.0329***	0.0250***	0.0132	0.0288***	0.0020	0.0226**
	(0.0064)	(0.0116)	(0.0097)	(0.0082)	(0.0079)	(0.0063)	(0.0091)	(0.0093)	(0.0063)	(0.0102)
Observations	54,225	52,473	54,458	55,265	52,835	68,596	66,285	68,632	69,575	66,719

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Table 5
Cross–equation Estimates of the Average Effect of Welfare Reform on Youth Behaviors
MTF 1991–2006

Target Group	**Children of Unmarried Mothers With High School Education or Less**							
Comparison Group	**Children of Married Mothers With High School Education or Less**							
Sample	**Boys Grades 10 & 12 and ≤ 17 y**				**Girls Grades 10 & 12 and ≤ 17 y**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Outcomes	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**
	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs
Joint DDD Effect = 0	[0.420]	[0.000]	[0.000]	[0.000]	[0.194]	[0.0229]	[0.000]	[0.000]
Average DDD Effect: Welfare Reform*Target	-0.0281 (0.0172) [0.102]	0.0818*** (0.0254) [0.001]	0.0574*** (0.0143) [0.000]	-0.0602*** (0.0129) [0.000]	-0.0199 (0.0183) [0.277]	0.0112 (0.0179) [0.531]	0.0419*** (0.0110) [0.000]	-0.0236** (0.0102) [0.021]

Notes: Models are jointly estimated using a seemingly unrelated regression (SUR) framework. All outcomes are redefined as standard normal deviates (see text). Two sets of results are presented for each model: (1) P-values from joint significance tests across all noted outcomes are reported in the first results row in square brackets; (2) Estimates of the average effect of welfare reform (average DDD effect) across all noted outcomes, with state-clustered standard errors reported in parentheses and the p-value reported in square brackets. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10.All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Table 6
Cross–equation Estimates of the Average Effect of Welfare Reform on Youth Behaviors
Duration of Exposure and Event Study
MTF 1991–2006
Boys, Grades 10 & 12 and ≤ 17 y

Target Group	**Children of Unmarried Mothers With High School Education or Less**			
Comparison Group	**Children of Married Mothers With High School Education or Less**			
Model	**1**	**2**	**3**	**4**
Outcomes	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**
	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteer School clubs School athletics Skipping school Damaging property Fighting Stealing Hurt someone Marijuana Alcohol Smoking Other illicit drugs
Panel A	**Effects of Duration of Exposure to Welfare Reform (# of Years)**			
# Years Exposed to WR	0.0087	0.0016	-0.0082	0.0042
	(0.0067)	(0.0059)	(0.0064)	(0.0042)
Average DDD Effect # Years Exposed to WR*Target	-0.0020	0.0091***	0.0055***	-0.0061***
	(0.0016)	(0.0025)	(0.0019)	(0.0014)
Panel B	**Event Study: Lead and Lag Effects of Welfare Reform**			
Average DDD Effect				
Pre WR 5+ Years*Target	0.0060	0.0833	0.0897**	-0.0631**
	(0.0573)	(0.0562)	(0.0398)	(0.0294)
Pre WR 4 Years*Target	-0.0341	0.00004	0.0544	-0.0267
	(0.0513)	(0.0638)	(0.0370)	(0.0294)
Pre WR 3 Years*Target	-0.0013	-0.0110	0.0457	-0.0110
	(0.0588)	(0.0561)	(0.0414)	(0.0330)
Pre WR 2 Years*Target	0.0637	0.0603	0.0191	-0.0156
	(0.0601)	(0.0512)	(0.0356)	(0.0289)
Pre WR 1 Year*Target	–	–	–	–
Post WR 1 Year*Target	-0.0013	0.1243*	0.0331	-0.0632
	(0.0475)	(0.0669)	(0.0413)	(0.0389)
Post WR 2 Years*Target	-0.0144	0.1213	0.0972**	-0.0866*
	(0.0578)	(0.0863)	(0.0464)	(0.0445)
Post WR 3 Years*Target	-0.0729	0.1512*	0.0886**	-0.1107**
	(0.0564)	(0.0814)	(0.0386)	(0.0447)
Post WR 4+ Years*Target	-0.0109	0.1276***	0.0991***	-0.0890***
	(0.0369)	(0.0439)	(0.0264)	(0.0202)

Notes: Models are jointly estimated using a seemingly unrelated regression (SUR) framework. All outcomes are redefined as standard normal deviates (see text). Each column in each panel presents estimates from a separate model. Estimates of the average effect of welfare reform across all noted outcomes are reported, with state-clustered standard errors reported in parentheses. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10.

Table 7
Cross-equation Estimates of the Average Effect of Welfare Reform on Youth Behaviors
Duration of Exposure and Event Study
MTF 1991–2006
Girls, Grades 10 & 12 and ≤ 17 y

Target Group	**Children of Unmarried Mothers With High School Education or Less**			
Comparison Group	**Children of Married Mothers With High School Education or Less**			
Model	**1**	**2**	**3**	**4**
Outcomes	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**
	Volunteer School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs
Panel A	**Effects of Duration of Exposure to Welfare Reform (# of Years)**			
# Years Exposed to WR	0.0083	-0.0114	-0.0058	0.0088***
	(0.0058)	(0.0038)	(0.0042)	(0.0033)
Average DDD Effect				
# Years Exposed to WR*Target	-0.0012	0.0021	0.0051**	-0.0029**
	(0.0022)	(0.0025)	(0.0013)	(0.0013)
Panel B	**Event Study: Lead and Lag Effects of Welfare Reform**			
Average DDD Effect				
Pre WR 5+ Years*Target	-0.0100	-0.0551	-0.0123	0.0246
	(0.0444)	(0.0392)	(0.0335)	(0.0293)
Pre WR 4 Years*Target	0.0109	-0.0239	-0.0101	0.0161
	(0.0623)	(0.0339)	(0.0369)	(0.0288)
Pre WR 3 Years*Target	-0.0001	-0.0042	-0.0180	0.0077
	(0.0541)	(0.0552)	(0.0437)	(0.0399)
Pre WR 2 Years*Target	-0.0902*	0.0058	0.0410	-0.0386
	(0.0505)	(0.0427)	(0.0332)	(0.0260)
Pre WR 1 Year*Target	_	_	_	_
Post WR 1 Year*Target	-0.0258	-0.0526*	-0.0050	0.0171
	(0.0382)	(0.0309)	(0.0289)	(0.0195)
Post WR 2 Years*Target	-0.0331	0.0068	0.0369	-0.0234
	(0.0461)	(0.0477)	(0.0387)	(0.0324)
Post WR 3+ Years*Target	-0.0804	-0.0227	0.0448	-0.0256
	(0.0504)	(0.0518)	(0.0371)	(0.0292)
Post WR 4+ Years*Target	-0.0388	-0.0132	0.0431*	-0.0186
	(0.0393)	(0.0353)	(0.0263)	(0.0224)

Notes: Models are jointly estimated using a seemingly unrelated regression (SUR) framework. All outcomes are redefined as standard normal deviates (see text). Each column in each panel presents estimates from a separate model. Estimates of the average effect of welfare reform across all noted outcomes are reported, with state-clustered standard errors reported in parentheses. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10.

Table 8
Cross-equation Estimates of the Average Effect of Welfare Reform on Youth Behaviors Controlling for Potential Mediators
MTF 1991–2006

Target Group	**Children of Unmarried Mothers with High School Education or Less**							
Comparison Group	**Children of Married Mothers with High School Education or Less**							
Sample	**Boys, Grades 10 & 12 and ≤ 17 y**				**Girls, Grades 10 & 12 and ≤ 17 y**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	8
Outcomes	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**
	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs
Average DDD Effect:	-0.0281	0.0818***	0.0574***	-0.0602***	-0.0199	0.0112	0.0419***	-0.0236**
Welfare Reform*Target	(0.0172)	(0.0254)	(0.0143)	(0.0129)	(0.0183)	(0.0179)	(0.0110)	(0.0102)
(From Table 5)	[0.102]	[0.001]	[0.000]	[0.000]	[0.277]	[0.531]	[0.000]	[0.021]
Adding Mediators (A)								
Average DDD Effect:	-0.0415**	0.0836***	0.0491***	-0.0616***	-0.0081	0.0055	0.0331**	-0.0154
Welfare Reform*Target	(0.0198)	(0.0300)	(0.0180)	(0.0147)	(0.0250)	(0.0211)	(0.0135)	(0.0125)
	[0.036]	[0.005]	[0.006]	[0.000]	[0.746]	[0.795]	[0.014]	[0.220]
Adding Mediators (B)								
Average DDD Effect:	-0.0423**	0.0843***	0.0498***	-0.0623***	-0.0098	0.0079	0.0365***	-0.0179
Welfare Reform*Target	(0.0200)	(0.0284)	(0.0177)	(0.0139)	(0.0250)	(0.0212)	(0.0129)	(0.0122)
	[0.034]	[0.003]	[0.005]	[0.000]	[0.695]	[0.709]	[0.005]	[0.142]

Notes: Models are jointly estimated using a seemingly unrelated regression framework. All outcomes are redefined as standard normal deviates (see text). Estimate of the average effect of welfare reform (average DDD effect) across all noted outcomes is reported, with state-clustered standard errors reported in parentheses and the p-value reported in square brackets. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Mediators (A) include: any work after school, youth is home alone 4+ hours each day, and mother is employed. Mediators (B) further adds hours of work after school and hours youth is home alone. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10.

Table 9
Cross-equation Estimates of the Average Effect of Welfare Reform on Youth Behaviors
Placebo Group: Children of Married Mothers with a College Degree or More
MTF 1991–2006

Sample	**Boys, Grades 10 & 12 and ≤ 17 y**				**Girls, Grades 10 & 12 and ≤ 17 y**			
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Outcomes	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**	**Prosocial Behaviors**	**Antisocial Behaviors**	**Substance Use**	**All Behaviors Measured in Favorable Direction**
	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics	Skipping school Damaging property Fighting Stealing Hurting someone	Marijuana Alcohol Smoking Other illicit drugs	Volunteering School clubs School athletics Skipping school Damaging property Fighting Stealing Hurting someone Marijuana Alcohol Smoking Other illicit drugs
Joint DD Effect = 0	[0.216]	[0.566]	[0.509]	[0.161]	[0.530]	[0.762]	[0.563]	[0.401]
Average DD Effect: Welfare Reform	0.0284 (0.0267) [0.287]	-0.0124 (0.0229) [0.588]	-0.0259 (0.0210) [0.218]	0.0209 (0.0168) [0.214]	0.0294 (0.0266) [0.269]	-0.0223 (0.0174) [0.199]	-0.0008 (0.0225) [0.973]	0.0169 (0.0182) [0.354]

Notes: Models are jointly estimated using a seemingly unrelated regression framework. All outcomes are redefined as standard normal deviates (see text). Two sets of results are presented: 1) p-value from a joint significance test across all noted outcomes is reported in the first results row in square brackets; 2) estimate of the average effect of welfare reform (average DDD effect) across all noted outcomes is reported, with state-clustered standard errors reported in parentheses and the p-value reported in square brackets. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10.

Table 10
Effects of Welfare Reform on Hurting Someone and Substance Use (past 3 months)
NLSY 1990–2006

Sample	**Boys, Ages 15–17**				**Girls, Ages 15–17**			
Outcome	**Hurting Someone**		**Substance Use**		**Hurting Someone**		**Substance Use**	
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Panel A	**Target and Comparison groups defined based on current year maternal characteristics**							
Target	**Children of Unmarried Mothers With High School Education or Less**							
Comparison	**Children of Married Mothers With High School Education or Less**							
Welfare Reform	-0.0592	-0.0071	-0.1065*	-0.1301	0.0218	-0.0105	0.0563	0.0312
	(0.0402)	(0.0745)	(0.0542)	(0.0855)	(0.0264)	(0.0526)	(0.0609)	(0.1012)
Welfare Reform*Target	0.0669*	0.0716	0.1328***	0.1455	0.0215	0.0156	0.0479	0.1032
	(0.0368)	(0.0910)	(0.0417)	(0.1148)	(0.0401)	(0.1200)	(0.0541)	(0.1044)
Observations	1,899	1,899	2,325	2,325	1,804	1,804	2,164	2,164
Panel A	**Target and Comparison groups defined based on baseline (first entry year of child in survey) maternal characteristics**							
Target	**Children of Unmarried Mothers With High School Education or Less**							
Comparison	**Children of Married Mothers With High School Education or Less**							
Welfare Reform	-0.0395	-0.0085	-0.1243**	-0.1231	0.0158	-0.0393	0.0644	0.0188
	(0.0363)	(0.0743)	(0.0544)	(0.0772)	(0.0241)	(0.0532)	(0.0587)	(0.0892)
Welfare Reform*Target	0.0692**	0.0874	0.1270***	0.1421	0.0397	0.0197	0.0568	0.1211
	(0.0286)	(0.0847)	(0.0428)	(0.1066)	(0.0360)	(0.1082)	(0.0483)	(0.0957)
Observations	1,964	1,964	2,406	2,406	1,903	1,903	2,286	2,286
Fixed Effects	State Year, Month	Household Year, Month	State Year, Month	Household Year, Month	State Year, Month	Household Year, Month	State Year, Month	Household Year, Month

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Appendix Table 1: Implementation Dates of Welfare Reform by State, U.S.

	10/92 to 2/97	9/96 to 1/98	10/92 to 1/98		10/92 to 2/97	9/96 to 1/98	10/92 to 1/98
	AFDC Waiver	**TANF**	**Any Welfare Reform**		**AFDC Waiver**	**TANF**	**Any Welfare Reform**
Alabama		Nov-96	Nov-96	Montana	Feb-96	Feb-97	Feb-96
Alaska		Jul-97	Jul-97	Nebraska	Oct-95	Dec-96	Oct-95
Arizona	Nov-95	Oct-96	Nov-95	Nevada		Dec-96	Dec-96
Arkansas	Jul-94	Jul-97	Jul-94	New Hampshire		Oct-96	Oct-96
California	Dec-92	Jan-98	Dec-92	New Jersey	Oct-92	Jul-97	Oct-92
Colorado		Jul-97	Jul-97	New Mexico		Jul-97	Jul-97
Connecticut	Jan-96	Oct-96	Jan-96	New York		Nov-97	Nov-97
DC		Mar-97	Mar-97	North Carolina	Jul-96	Jan-97	Jul-96
Delaware	Oct-95	Mar-97	Oct-95	North Dakota		Jul-97	Jul-97
Florida			Oct-96	Ohio	Jul-96	Oct-96	Jul-96
Georgia	Jan-94	Jan-97	Jan-94	Oklahoma		Oct-96	Oct-96
Hawaii	Feb-97	Jul-97	Feb-97	Oregon	Feb-93	Oct-96	Feb-93
Idaho		Jul-97	Jul-97	Pennsylvania		Mar-97	Mar-97
Illinois	Nov-93	Jul-97	Nov-93	Rhode Island		May-97	May-97
Indiana	May-95	Oct-96	May-95	South Carolina		Oct-96	Oct-96
Iowa	Oct-93	Jan-97	Oct-93	South Dakota	Jun-94	Dec-96	Jun-94
Kansas		Oct-96	Oct-96	Tennessee	Sep-96	Oct-96	Sep-96
Kentucky		Oct-96	Oct-96	Texas	Jun-96	Nov-96	Jun-96
Louisiana		Jan-97	Jan-97	Utah	Jan-93	Oct-96	Jan-93
Maine		Nov-96	Nov-96	Vermont	Jul-94	Sep-96	Jul-94
Maryland	Mar-96	Dec-96	Mar-96	Virginia	Jul-95	Feb-97	Jul-95
Massachusetts	Nov-95	Sep-96	Nov-95	Washington	Jan-96	Jan-97	Jan-96
Michigan	Oct-92	Sep-96	Oct-92	West Virginia		Jan-97	Jan-97
Minnesota		Jul-97	Jul-97	Wisconsin	Jan-96	Sep-97	Jan-96
Mississippi	Oct-95	Jul-97	Oct-95	Wyoming		Jan-97	Jan-97
Missouri	Jun-95	Dec-96	Jun-95				

Source: U.S. Department of Health and Human Services (1999).

Appendix Table 2
Behavioral Outcomes across Target and Comparison Groups and by Gender
Monitoring the Future (MTF) Surveys, 1991–1992
Grades 10 & 12 and ≤ 17 y

Variable	Boys			Girls		
Sample	**Target:** Children with Unmarried Mothers with ≤ High School	**Comparison A:** Children with Married Mothers with ≤ High School	**Comparison B:** Children with Unmarried Mothers with >= High School Education	**Target:** Children with Unmarried Mothers with ≤ High School	**Comparison A:** Children with Married Mothers with ≤ High School	**Comparison B:** Children with Unmarried Mothers with >= High School Education
Volunteering (≥ once a month)	0.191	0.196	0.214	0.255	0.261	0.303
Participating in school clubs	0.316	0.310	0.374	0.441	0.498	0.547
Participating in school athletics	0.477	0.438	0.485	0.232	0.318	0.343
Skipping school (past 4 weeks)	0.279	0.224	0.273	0.242	0.210	0.268
Damaging property (past year)	0.193	0.224	0.234	0.102	0.093	0.101
Fighting (past year)	0.278	0.268	0.289	0.174	0.160	0.167
Stealing (past year)	0.357	0.380	0.409	0.195	0.196	0.241
Hurting someone (past year)	0.210	0.199	0.252	0.087	0.051	0.063
Any substance use (past month)	0.567	0.556	0.582	0.510	0.512	0.524
Marijuana (past month)	0.137	0.102	0.150	0.102	0.079	0.109
Alcohol (past month)	0.485	0.478	0.512	0.411	0.418	0.443
Cigarette smoking (past month)	0.252	0.244	0.227	0.233	0.248	0.245
Other illicit drugs (past month)	0.088	0.081	0.094	0.089	0.096	0.099

Note: Weighted means for 1991 and 1992, based on the MTF sampling weights, are shown as adjusted proportions.

Appendix Table 3
Effects of Welfare Reform on Youth Behaviors
Separate Effects of AFDC Waiver & TANF
MTF 1991–2006

Outcome	Volunteer	School Clubs	School Athletics	Skipping School	Damage Property	Fight	Steal	Hurt Someone	Substance Use	Marijuana	Alcohol	Smoking	Other Illicit Drugs
Target	**Children of Unmarried Mothers With High School Education or Less**												
Comparison	**Children of Married Mothers With High School Education or Less**												
Sample	**Boys Grades 10 & 12 and ≤ 17 y**												
AFDC Waiver	0.0073	-0.0050	-0.0211	-0.0055	-0.0202*	-0.0288**	-0.0561***	-0.0160	0.0005	-0.0032	0.0096	-0.0107	-0.0137**
	(0.0122)	(0.0165)	(0.0145)	(0.0137)	(0.0118)	(0.0131)	(0.0182)	(0.0156)	(0.0114)	(0.0160)	(0.0126)	(0.0121)	(0.0064)
TANF	0.0030	-0.0184	0.0500	0.0181	-0.0092	0.0116	-0.0578**	0.0028	0.0370**	0.0295	0.0137	0.0159	-0.0067
	(0.0205)	(0.0245)	(0.0444)	(0.0244)	(0.0228)	(0.0321)	(0.0285)	(0.0317)	(0.0171)	(0.0201)	(0.0165)	(0.0176)	(0.0125)
AFDC Waiver	0.0065	0.0244	0.0177	0.0341**	0.0354	0.0880*	0.0473	0.0695**	0.0404***	0.0332**	0.0117	0.0280**	0.0246**
*Target	(0.0126)	(0.0219)	(0.0328)	(0.0147)	(0.0218)	(0.0437)	(0.0288)	(0.0335)	(0.0120)	(0.0126)	(0.0166)	(0.0134)	(0.0119)
TANF*Target	-0.0091	-0.0207	-0.0276	0.0179*	0.0427***	0.0529***	0.0268	0.0108	0.0312***	0.0318***	0.0179	0.0217**	0.0178*
	(0.0093)	(0.0150)	(0.0173)	(0.0098)	(0.0113)	(0.0192)	(0.0215)	(0.0168)	(0.0083)	(0.0068)	(0.0117)	(0.0103)	(0.0091)
Observations	44,939	21,515	21,830	52,359	20,800	17,477	20,906	17,411	52,835	54,225	52,473	54,458	55,265
Sample	**Girls Grades 10 & 12 and ≤ 17 y**												
AFDC Waiver	-0.0037	0.0417***	0.0103	-0.0239**	0.0019	-0.0130	-0.0176	-0.0065	0.0175	0.0086	0.0162	-0.0053	0.0095*
	(0.0101)	(0.0142)	(0.0094)	(0.0102)	(0.0079)	(0.0110)	(0.0127)	(0.0064)	(0.0186)	(0.0098)	(0.0178)	(0.0158)	(0.0051)
TANF	0.0434***	-0.0192	-0.0070	0.0273	0.0132	-0.0005	0.0218	-0.0330	-0.0214	-0.0047	-0.0190	-0.0249	-0.0128
	(0.0147)	(0.0274)	(0.0289)	(0.0195)	(0.0211)	(0.0246)	(0.0254)	(0.0206)	(0.0263)	(0.0156)	(0.0243)	(0.0187)	(0.0093)
AFDC Waiver	0.0041	-0.0424	-0.0230	0.0191	0.0014	0.0174	0.0208	-0.0096	0.0089	0.0233**	0.0091	0.0260**	-0.0018
*Target	(0.0099)	(0.0259)	(0.0228)	(0.0216)	(0.0117)	(0.0170)	(0.0192)	(0.0106)	(0.0150)	(0.0106)	(0.0146)	(0.0106)	(0.0071)
TANF*Target	-0.0189**	0.0078	-0.0093	0.0164**	0.0089	0.0123	-0.0150	-0.0060	0.0255**	0.0254***	0.0141	0.0294***	0.0029
	(0.0075)	(0.0169)	(0.0138)	(0.0080)	(0.0099)	(0.0163)	(0.0096)	(0.0101)	(0.0101)	(0.0067)	(0.0090)	(0.0097)	(0.0068)
Observations	54,694	27,719	27,904	66,398	26,481	22,348	26,570	22,295	66,719	68,596	66,285	68,632	69,575

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications include the following covariates: child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Appendix Table 4
Effects of Welfare Reform on Youth Behaviors
Controlling for Lagged Economic and Welfare Conditions
MTF 1991–2006

Outcome	Volunteer	School Clubs	School Athletics	Skipping School	Damage Property	Fight	Steal	Hurt Someone	Substance Use	Marijuana	Alcohol	Smoking	Other Illicit Drugs
Target	**Children of Unmarried Mothers With High School Education or Less**												
Comparison	**Children of Married Mothers With High School Education or Less**												
Sample	**Boys Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0045	-0.0005	-0.0083	-0.0016	-0.0245**	-0.0258*	-0.0482***	-0.0119	-0.0054	0.0315	-0.0012	-0.0114	-0.0120*
	(0.0098)	(0.0169)	(0.0142)	(0.0132)	(0.0114)	(0.0141)	(0.0163)	(0.0177)	(0.0095)	(0.0201)	(0.0112)	(0.0111)	(0.0063)
Welfare Reform *Target	-0.0062	-0.0137	-0.0195	0.0207**	0.0420***	0.0602***	0.0300	0.0229	0.0331***	0.0279***	0.0170	0.0232**	0.0193**
	(0.0087)	(0.0155)	(0.0150)	(0.0094)	(0.0107)	(0.0203)	(0.0207)	(0.0175)	(0.0078)	(0.0071)	(0.0116)	(0.0097)	(0.0082)
Observations	44,939	21,515	21,830	52,359	20,800	17,477	20,906	17,411	52,835	54,225	52,473	54,458	55,265
Sample	**Girls Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	-0.0022	0.0270	0.0072	-0.0191**	0.0038	-0.0114	-0.0071	-0.0025	0.0097	0.0104	0.0058	-0.0074	0.0054
	(0.0110)	(0.0179)	(0.0111)	(0.0094)	(0.0094)	(0.0115)	(0.0135)	(0.0073)	(0.0203)	(0.0094)	(0.0192)	(0.0153)	(0.0054)
Welfare Reform *Target	-0.0148**	-0.0014	-0.0117	0.0167*	0.0072	0.0138	-0.0090	-0.0069	0.0224**	0.0244***	0.0131	0.0284***	0.0019
	(0.0068)	(0.0151)	(0.0134)	(0.0094)	(0.0091)	(0.0150)	(0.0100)	(0.0085)	(0.0101)	(0.0065)	(0.0089)	(0.0093)	(0.0063)
Observations	54,694	27,719	27,904	66,398	26,481	22,348	26,570	22,295	66,719	68,596	66,285	68,632	69,575

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications include the following covariates: child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Models also control for lagged economic /welfare conditions including one, two, and three-year lags of the unemployment rate, poverty rate, state personal income per capita, and welfare caseloads.

Appendix Table 5
Effects of Welfare Reform on Youth Behaviors
Controlling for State Linear Trends
MTF 1991–2006

Outcome	Volunteer	School Clubs	School Athletics	Skipping School	Damage Property	Fight	Steal	Hurt Someone	Substance Use	Marijuana	Alcohol	Smoking	Other Illicit Drugs
Target	**Children of Unmarried Mothers With High School Education or Less**												
Comparison	**Children of Married Mothers With High School Education or Less**												
Sample	**Boys Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0036	0.0012	-0.0112	0.0001	-0.0215*	-0.0231	-0.0468***	-0.0100	-0.0020	-0.0010	-0.0034	-0.0101	-0.0090
	(0.0115)	(0.0173)	(0.0155)	(0.0130)	(0.0116)	(0.0150)	(0.0173)	(0.0203)	(0.0099)	(0.0139)	(0.0116)	(0.0108)	(0.0061)
Welfare Reform *Target	-0.0049	-0.0110	-0.0178	0.0195*	0.0416***	0.0594***	0.0277	0.0222	0.0335***	0.0310***	0.0190	0.0224**	0.0183**
	(0.0089)	(0.0158)	(0.0150)	(0.0097)	(0.0105)	(0.0206)	(0.0208)	(0.0179)	(0.0081)	(0.0065)	(0.0117)	(0.0100)	(0.0083)
Observations	44,939	21,515	21,830	52,359	20,800	17,477	20,906	17,411	52,835	54,225	52,473	54,458	55,265
Sample	**Girls Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0030	0.0366**	0.0193	-0.0154	-0.0028	-0.0123	-0.0151	-0.0085	0.0112	0.0114	0.0071	-0.0070	0.0077
	(0.0114)	(0.0159)	(0.0118)	(0.0099)	(0.0109)	(0.0117)	(0.0138)	(0.0080)	(0.0178)	(0.0086)	(0.0161)	(0.0129)	(0.0056)
Welfare Reform *Target	-0.0119*	-0.0007	-0.0117	0.0175*	0.0071	0.0142	-0.0106	-0.0065	0.0217**	0.0245***	0.0127	0.0286***	0.0019
	(0.0065)	(0.0152)	(0.0138)	(0.0092)	(0.0093)	(0.0153)	(0.0100)	(0.0085)	(0.0102)	(0.0065)	(0.0089)	(0.0093)	(0.0064)
Observations	54,694	27,719	27,904	66,398	26,481	22,348	26,570	22,295	66,719	68,596	66,285	68,632	69,575
Lagged Economic & Welfare Conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10 All specifications include the following covariates: child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Models also control for state-specific linear trends (state dummies interacted with a linear time trend) and lagged economic / welfare conditions including one, two, and three-year lags of the unemployment rate, poverty rate, state personal income per capita, and welfare caseloads.

Appendix Table 6
Effects of Welfare Reform on Youth Behaviors
Controlling for School Fixed Effects
MTF 1991–2006

Outcome	Volunteer	School Clubs	School Athletics	Skipping School	Damage Property	Fight	Steal	Hurt Someone	Substance Use	Marijuana	Alcohol	Smoking	Other Illicit Drugs
Target	**Children of Unmarried Mothers With High School Education or Less**												
Comparison	**Children of Married Mothers With High School Education or Less**												
Sample	**Boys Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0016	-0.0648**	-0.0129	0.0151	0.0153	-0.0210	0.0002	0.0034	0.0328*	0.0315	0.0169	0.0262	-0.0048
	(0.0249)	(0.0303)	(0.0382)	(0.0128)	(0.0259)	(0.0324)	(0.0378)	(0.0337)	(0.0195)	(0.0201)	(0.0208)	(0.0158)	(0.0149)
Welfare Reform *Target	-0.0045	-0.0117	-0.0256	0.0202**	0.0428***	0.0503**	0.0258	0.0200	0.0259***	0.0279***	0.0124	0.0171	0.0222**
	(0.0082)	(0.0173)	(0.0164)	(0.0096)	(0.0126)	(0.0245)	(0.0242)	(0.0225)	(0.0084)	(0.0071)	(0.0102)	(0.0110)	(0.0092)
Observations	44,939	21,515	21,830	52,359	20,800	17,477	20,906	17,411	52,835	54,225	52,473	54,458	55,265
Sample	**Girls Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0119	0.0022	-0.0177	-0.0153	0.0082	-0.0196	-0.0141	-0.0026	0.0253	0.0279**	0.0200	0.0069	0.0097
	(0.0199)	(0.0275)	(0.0214)	(0.0153)	(0.0189)	(0.0221)	(0.0220)	(0.0187)	(0.0164)	(0.0135)	(0.0177)	(0.0140)	(0.0081)
Welfare Reform *Target	-0.0131**	0.0033	-0.0116	0.0180*	0.0076	0.0066	-0.0116	-0.0039	0.0185*	0.0251***	0.0116	0.0287***	0.0043
	(0.0058)	(0.0146)	(0.0139)	(0.0097)	(0.0097)	(0.0173)	(0.0115)	(0.0094)	(0.0097)	(0.0062)	(0.0076)	(0.0103)	(0.0061)
Observations	54,694	27,719	27,904	66,398	26,481	22,348	26,570	22,295	66,719	68,596	66,285	68,632	69,575
Lagged Economic & Welfare Conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications include the following covariates: child's age, grade, and race/ethnicity; the mother's education; and annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for year, month, and school. Lagged economic / welfare conditions include one, two, and three-year lags of the unemployment rate, poverty rate, state personal income per capita, and welfare caseloads.

Appendix Table 7
Effects of Welfare Reform on Youth Behaviors
Alternate Comparison Group
MTF 1991–2006

Outcome	Volunteer	School Clubs	School Athletics	Skipping School	Damage Property	Fight	Steal	Hurt Someone	Substance Use	Marijuana	Alcohol	Smoking	Other Illicit Drugs
Target	**Children of Unmarried Mothers With High School Education or Less**												
Comparison	**Children of Unmarried Mothers With More than High School Education**												
Sample	**Boys Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	-0.0225	0.0314	-0.0282	0.0024	-0.0440	-0.0438	-0.1093***	-0.0174	-0.0118	0.0232*	-0.0206	-0.0076	0.0043
	(0.0149)	(0.0231)	(0.0234)	(0.0196)	(0.0272)	(0.0270)	(0.0271)	(0.0216)	(0.0134)	(0.0129)	(0.0191)	(0.0113)	(0.0084)
Welfare Reform *Target	-0.0102	-0.0325	-0.0238	0.0267**	0.0503***	0.0540**	0.0536**	0.0339	0.0501***	0.0330***	0.0493***	0.0242**	0.0185**
	(0.0104)	(0.0225)	(0.0186)	(0.0112)	(0.0120)	(0.0220)	(0.0232)	(0.0223)	(0.0092)	(0.0087)	(0.0116)	(0.0097)	(0.0075)
Observations	19,379	9,087	9,241	22,441	8,650	7,150	8,701	7,116	22,656	23,315	22,517	23,507	23,955
Sample	**Girls Grades 10 & 12 and ≤ 17 y**												
Welfare Reform	0.0085	0.0054	-0.0191	-0.0299	0.0122	-0.0039	0.0129	-0.0045	-0.0170	0.0075	-0.0153	-0.0321**	-0.0077
	(0.0198)	(0.0246)	(0.0207)	(0.0187)	(0.0137)	(0.0168)	(0.0242)	(0.0124)	(0.0133)	(0.0132)	(0.0149)	(0.0134)	(0.0094)
Welfare Reform *Target	-0.0054	-0.0017	-0.0011	0.0223*	0.0011	-0.0067	0.0011	-0.0127	0.0313***	0.0193**	0.0272***	0.0339***	0.0072
	(0.0120)	(0.0162)	(0.0179)	(0.0123)	(0.0112)	(0.0179)	(0.0200)	(0.0083)	(0.0091)	(0.0087)	(0.0101)	(0.0123)	(0.0059)
Observations	25,564	12,901	12,989	31,448	12,081	10,035	12,117	10,020	31,689	32,665	31,554	32,708	33,259
Lagged Economic & Welfare Conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications include the following covariates: child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month. Models also control for lagged economic / welfare conditions including one, two, and three-year lags of the unemployment rate, poverty rate, state personal income per capita, and welfare caseloads.

Appendix Table 8
Effects of Welfare Reform on Youth Prosocial Behaviors
MTF 1991–2006

Sample	**Boys Grades 8, 10 & 12 and ≤ 17 y**			**Girls Grades 8, 10 & 12 and ≤ 17 y**		
Outcome	**Volunteering**	**Participating in School Clubs**	**Participating in School Athletics**	**Volunteering**	**Participating in School Clubs**	**Participating in School Athletics**
Model	**1**	**2**	**3**	**4**	**5**	**6**
Target	**Children of Unmarried Mothers With High School Education or Less**					
Comparison	**Children of Married Mothers With High School or Less**					
Welfare Reform	0.0019	-0.0077	-0.0160	0.0051	0.0082	0.0153*
	(0.0065)	(0.0125)	(0.0122)	(0.0075)	(0.0129)	(0.0082)
Welfare Reform*Target	-0.0074	-0.0078	-0.0078	-0.0100**	0.0160	-0.0162*
	(0.0045)	(0.0130)	(0.0144)	(0.0045)	(0.0118)	(0.0092)
Observations	85,541	37,134	37,716	104,766	47,475	47,886

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Appendix Table 9
Effects of Welfare Reform on Youth Antisocial Behaviors
MTF 1991–2006

Sample	**Boys Grades 8, 10 & 12 and ≤ 17 y**					**Girls Grades 8, 10 & 12 and ≤ 17 y**				
Outcome	**Skipping School**	**Damaging Property**	**Fighting**	**Stealing**	**Hurting Someone**	**Skipping School**	**Damaging Property**	**Fighting**	**Stealing**	**Hurting Someone**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Target	**Children of Unmarried Mothers With High School Education or Less**									
Comparison	**Children of Married Mothers With High School Education or Less**									
Welfare Reform	-0.0009	-0.0153**	-0.0105	-0.0286***	-0.0037	-0.0072	0.0003	-0.0105	-0.0089	-0.0112**
	(0.0081)	(0.0069)	(0.0129)	(0.0089)	(0.0117)	(0.0076)	(0.0073)	(0.0086)	(0.0094)	(0.0051)
Welfare Reform*Target	0.0043	0.0298***	0.0373**	0.0202	0.0200	-0.0000	0.0075	0.0064	0.0011	-0.0095
	(0.0080)	(0.0103)	(0.0143)	(0.0151)	(0.0144)	(0.0059)	(0.0064)	(0.0103)	(0.0083)	(0.0080)
Observations	90,245	36,061	29,985	36,326	29,849	113,882	46,278	38,417	46,505	38,272

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.

Appendix Table 10
Effects of Welfare Reform on Youth Substance Use
MTF 1991–2006

Sample	**Boys Grades 10 & 12 and ≤ 17 y**					**Girls Grades 10 & 12 and ≤ 17 y**				
Outcome	**Marijuana**	**Alcohol**	**Smoking**	**Other Illicit Drugs**	**Any Substance Use**	**Marijuana**	**Alcohol**	**Smoking**	**Other Illicit Drugs**	**Any Substance Use**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Target	**Children of Unmarried Mothers With High School Education or Less**									
Comparison	**Children of Married Mothers With High School Education or Less**									
Welfare Reform	0.0041	0.0040	-0.0030	-0.0085**	0.0016	0.0094	0.0117	0.0032	0.0039	0.0128
	(0.0079)	(0.0081)	(0.0073)	(0.0037)	(0.0079)	(0.0071)	(0.0110)	(0.0090)	(0.0036)	(0.0108)
Welfare Reform*Target	0.0206***	0.0022	0.0069	0.0100	0.0145*	0.0189***	-0.0030	0.0105	0.0032	0.0083
	(0.0042)	(0.0099)	(0.0076)	(0.0062)	(0.0079)	(0.0052)	(0.0089)	(0.0078)	(0.0055)	(0.0093)
Observations	94,190	90,102	94,511	96,207	90,664	118,045	113,043	118,016	119,963	113,822

Notes: Coefficients from OLS models are reported. Standard errors are clustered at the state-level and reported in parentheses. Asterisks denote significance as follows: *** p-value ≤ 0.01, ** 0.01 < p-value ≤ 0.05, * 0.05 < p-value ≤ 0.10. All specifications control for the child's age, grade, and race/ethnicity; the mother's education; annual state-level measures (unemployment rate, poverty rate, personal income per capita, Earned Income Tax Credit (EITC) rate, refundable EITC, minimum wage, number Medicaid beneficiaries, numbers of National School Lunch and School Breakfast Program participants, and population); and indicators for state, year and month.