

A Promising Alternative:  
How Making College Free Affects Teens' Risky Behaviors

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## Abstract

Promise-type college scholarships first garnered attention in Kalamazoo, Michigan, with the announcement of the Kalamazoo Promise program in November 2005. Other similar college scholarship programs, in which graduates from local high schools are guaranteed a full-tuition (and fees) scholarship at an in-state, public university or college for up to four years, have been developed across the country. The programs are typically funded by private donors and have few, if any, eligibility criteria beyond graduation from a public high school in the particular geographic area. While there is a small and growing literature on the academic effects of such programs, their impact on adolescent engagement in risky behaviors has yet to be explored. In this paper, we leverage the rollout of several Promise-type college scholarship programs to estimate their impact on juvenile crime and teenage childbearing in the affected county, using a triple-differences framework. We find evidence that program announcements decreased risky behaviors among youth in Promise-adopting counties, observing beneficial changes in arrest rate trends and suggestive evidence of declining teen birth rates over time after announcement. We also consider heterogeneity of effects by race and across the geographies implementing such programs.

JEL Codes: I22, I25, J13, K42

# 1 Introduction

This study explores the impact of the introduction of local college scholarship programs on juvenile crime and teenage childbearing. Promise-type scholarships are community programs that provide full-tuition scholarships for higher education to students graduating from local high schools. Their goals are to facilitate college access for youth from disadvantaged backgrounds and to spur economic development in the affected areas. These scholarship programs are distinct from most other forms of need-based or merit-based aid in that they are generally place-based, universal or near-universal in coverage, generous, and flexible. Kalamazoo Promise was the first program of this kind and was announced in Kalamazoo, Michigan, in 2005. Since then, dozens of similar programs have been launched in cities and counties across the country.

Previous evaluations of the Kalamazoo Promise program find that the dramatic changes in expected tuition costs induced by the program result in fewer suspensions, increased likelihood of earning high school course credits, and—among African-American students—higher grade point averages (Bartik & Lachowska 2012). Despite the evidence that this program improves educational outcomes for teenage recipients, there has been no study of the impact of Promise-type programs on nonacademic outcomes, including engagement in risky behaviors that might derail students' education and career plans.

The current study examines whether an academically-oriented intervention, like Kalamazoo Promise and other local scholarship programs, alters non-academic behavior among teenagers — specifically, crime-committing activity for boys and childbearing decisions for girls. Local college scholarship programs may work against the economic "despair" that spurs teenage childbearing (Kearney & Levine 2011) and engagement in crime by providing hope of economic advancement for poor teens through financial support for higher education. However, expected effects are ambiguous, given that Promise-type scholarships have different income and substitution effects for different students, depending on their prior college-going plans. To address this question, the paper explores changes in county-level juvenile arrest and teen birth rates after the announcement of Promise-type scholarship programs, for eligible cohorts of teenagers. In particular, we measure the impacts of programs in Kalamazoo (announced in November 2005), Lake County (November

2009), and Northport, Michigan (February 2007); El Dorado, Arkansas (January 2007); Pittsburgh, Pennsylvania (December 2006); and Syracuse, New York (December 2008).

To analyze the impact of local college scholarship programs on juvenile arrests and teen birth rates, we leverage difference-in-differences (DiD) and triple-differences (DiDiD) approaches, with county and time fixed effects, controlling for county-specific trends in the outcome measures over time. These approaches net out changes in non-adopting counties in the same states as well as (in the DiDiD specification) changes for slightly older age groups that should not be incentivized by the programs. We find that Promise programs cause a dramatic change in trend in the rate of arrests for teen boys with some evidence of a similar change in the rate of teen births for girls. While the rates of both risky behaviors had been increasing in Promise-adopting counties, they flattened and/or began decreasing once the Promise-type programs were announced. This is consistent with existing evidence on how Promise programs affect teens' academic outcomes, and suggests that Promise-type scholarships may have substantial, beneficial effects on students' outcomes over time.

## 2 Motivation and Literature Review

There are primarily two avenues through which Promise-type college scholarship programs may affect risky behaviors among teens. The first is through a change in the relative costs associated with investments in education and engagement in risky activities, which include unprotected sex and juvenile delinquency. The free or reduced price of college will increase efforts by students to qualify for Promise-type scholarships by meeting the academic requirements to gain admission to college. Furthermore, it will increase the costs of risky activities that have the potential to affect school performance, college admission, or the ability to acquire human capital. Student engagement in risky activities will decrease if the benefits no longer outweigh the increased costs.

The second mechanism that may affect risky behaviors is the increase in income resulting from the Promise programs. Scholarships for college may free up resources in the household, previously allocated to support college-going, or lead to reductions in time spent working to fund education. This increase in income or free time could either increase or decrease engagement in risky behaviors among teens. To the extent that activities that lead to early childbearing or juvenile crime (e.g.,

alcohol consumption) are normal goods, we would expect to see an increase in these behaviors. Little is known about the causal effect of income on fertility, although the evidence tends to point to a positive relationship (see discussion in Lindo 2010). However, previous work does not focus on adolescents. Given that childbearing and crime are more prevalent among youth in families with lower socioeconomic status (Kearney & Levine 2011), this may suggest a negative relationship with income. Recent work has suggested that income inequality and lack of social mobility may contribute to the culture of despair associated with early childbearing and high school dropout (Kearney & Levine 2011, Kearney & Levine 2014). There is also evidence suggesting that one-time income shocks lead to increased involvement in risky behaviors, and that these responses are particularly strong among younger recipients of the income transfers (Evans & Moore 2011, Evans & Moore 2012).

Related to the different mechanisms at work, the impact of the Promise programs will likely vary across students depending on their existing options for financing college. Students who use Promise funds to pay for college directly gain from the program. Some students with alternative sources of aid for college, or those who do not attend college, may experience no direct benefits from the program. Additionally, as pointed out by Bartik and Lachowska (2010), students who would otherwise seek merit-based aid for college may be inclined to exert less effort in school and rely on Promise aid, which is not based on academic achievement. Similarly, there may be a decrease in the short-run costs associated with risky behaviors for these students given that Promise programs do not consider disciplinary and criminal records, nor time taken to complete schooling, in making award decisions. Long-run incentives for achievement remain in place, however, since each Promise program requires admission to a qualifying post-secondary institution, as well as maintenance of a cumulative 2.0 grade point average while there.

Regardless, as discussed below, adolescent decision-making is prone to overweighting short-run over long-run benefits. Finally, all students may experience indirect effects from the scholarship programs regardless of their direct participation. Participation by peers may change the nature of student interactions, while changes in teacher attitudes in schools, as well as improvements in community morale or the local economy, may impact the general environment for all students, both

in and out of school.<sup>1</sup>

Despite these predictions for changes in risk-taking behavior, there are important considerations regarding adolescent decision-making that might suggest little or no effect of the Promise programs. A central feature of risk-taking activities such as unprotected sex and engagement in crime is their short-run benefit and long-run cost (Gruber 2001). In addition, investments in education have long-run payoffs but require short-term efforts by students that have relatively low returns (Levitt et al. 2012). Behavioral economists O’Donoghue and Rabin (2001) argue that the excessive myopia and time inconsistent preferences of teens may lead them to heavily weight present benefits over long-run costs. In addition, projection bias may lead teens to underestimate how their preferences may change as they age, which would also lead to decisions with disproportionate weight on current happiness. The combination of these factors may cause teens to respond less to changes in incentives that do not have an immediate payoff. This is consistent with new experimental evidence that delayed rewards for investments in education have no impact on student performance, in contrast to changes in immediate incentives (Levitt et al. 2012).

Despite these considerations, early evidence of the impact of Promise-type programs indicates that teens are improving their academic performance. Following the announcement of the Kalamazoo Promise program, Bartik and Lachowska (2012) find a reduction in the number of days in suspension among students eligible for the program, as well as evidence of improved GPAs among black students. In addition, Bartik et al. (2010) find evidence suggestive of improvements in standardized test scores in Kalamazoo Public Schools after the start of the program. Finally, Andrews et al. (2010) examine the impact of Kalamazoo Promise on college choice and find that the increased interest in Michigan’s public colleges and universities, with particularly large effects for the state’s most selective colleges. In addition, the authors find evidence that students in more financially constrained families (with incomes less than \$50,000) were more likely to consider higher priced and more selective institutions under the Promise program. Notably, there is a large, existing literature on the effects of financial aid and merit-based scholarships on college going and post-secondary achievement and attainment<sup>2</sup>, but decidedly less emphasis in the literature on the impact of such

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<sup>1</sup>Media articles on the Kalamazoo Promise program discuss improvements in the motivation of teachers under the program (e.g., Fishman 2012).

<sup>2</sup>See Angrist, Oreopoulos & Williams 2014 for a comprehensive review of merit-based aid studies.

scholarship initiatives on high school outcomes (Pallais 2009).

Given the evidence that students are responding to the introduction of Promise programs with improvements in school performance, it is reasonable to expect that teens may also alter their behaviors on other dimensions. While an extensive literature documents and studies the effect of student aid on educational outcomes (see review in Kane 2006), to our knowledge there has been no study of how financial aid policies influence nonacademic outcomes for teens, including engagement in crime and early childbearing. In fact, there has been little study of the role of education policy more generally on these outcomes. A small and growing literature exploits exogenous variation in school quality and mandatory schooling laws to examine the effects of education on teen pregnancy (Black et al. 2008, McCrary & Royer 2011) and juvenile crime (Deming 2011, Anderson 2014). Mandatory schooling laws, however, are fundamentally different than financial aid policies because they *require* additional time to be spent in school. Evidence of declines in juvenile delinquency or fertility under these policies may result mostly from an incapacitation effect, i.e., less time and opportunity for adolescents to engage in risky behaviors.

This paper contributes to the literature on the effects of financial aid policies by considering their impact on adolescent behaviors with important consequences for adult well-being. Moreover, this work is related to and similar in approach to a small, but growing evidence base on the impact of place-based policies,<sup>3</sup> such as state-run Enterprise Zones (Kolko & Neumark 2010, Ham et al. 2011), federally-funded urban Empowerment Zones (Busso, Gregory & Kline 2013), and the largely unevaluated Promise Neighborhoods such as the Harlem Children’s Zone (Dobbie & Fryer 2011, Comey et al. 2013, U.S. Government Accountability Office 2014). In assessing the impact of place-based scholarship programs, any effects of the introduction of Promise scholarships on teen fertility and criminal activity are an important component of the programs’ costs and benefits. In addition, changes in behavior observed under these programs may also improve our understanding of the factors and determinants driving teenage engagement in risky behaviors, informing policy and programmatic efforts aimed at reducing the negative outcomes associated with risky behaviors.

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<sup>3</sup>See Kline & Moretti 2014 for an overview.

## 3 Data

### 3.1 Promise Programs

In total, approximately 25–30 Promise-type scholarship programs have been implemented across the country since 2005, including ten programs in the Michigan Promise Zone network. In this paper we focus on programs that had generous benefits with few, if any, eligibility requirements beyond high school graduation in a particular geographic area. We elected to include programs with this feature of universality or near-universality because of the aggregate nature of our outcome data, described in further detail in the next sections.

We focus on the Promise-type college scholarship programs that were introduced in Kalamazoo, Lake County, and Northport, Michigan; El Dorado, Arkansas; Syracuse, New York; and Pittsburgh, Pennsylvania. Other similar programs that were introduced in Washington, DC, and several smaller municipalities and counties, are not included in the current analyses because of data limitations or because the program features were sufficiently different from those under investigation.

For the included programs, we use information on the date of program announcement from scholarship program documentation, verified through local media accounts. We focus on this date as it is the earliest possible date of treatment. In considering effects of Promise introduction on teen childbearing, we look at lagged effects—three quarters after announcement, to account for a 9-month pregnancy—to detect effects of the Promise announcement on teen girls’ behavior.

### 3.2 Juvenile Crime

To examine the impact of Promise programs on criminal activity, we use data from the FBI’s National Incident Based Reporting System (NIBRS) for 2004–2012. NIBRS contains detailed data on all reported offenses in a large number of jurisdictions across the United States, but it does not cover all states (or even all areas within states), and substantially fewer jurisdictions are included before 2004. We create a balanced panel of counties that report in all months in all of the sample years. Of the areas with Promise programs of interest, we are able to match four with reported crime data in NIBRS: Kalamazoo, Michigan; Lake County, Michigan; Northport, Michigan; and El Dorado, Arkansas. We coded the corresponding counties as treated during the months after Promise



programs were announced, and include the other counties in Michigan and Arkansas as controls.

As an outcome measure, we consider the number of juvenile arrestees (ages 15 to 19), relative to the local juvenile male population; this gives us the "juvenile arrest rate." (We focus on this age group because of the availability of detailed annual population counts from the Census.) When an arrest was made, the data include information on the age, sex, and race of the arrestee. We focus here on the arrest rate for young men only, as relatively few young women are arrested. We construct juvenile arrest rates for all young men as well as separately by race; rates are per 100,000 individuals in each group (young men, young black men, young white men). We consider four broad categories of crime: all offenses, violent offenses, drug offenses, and property offenses.<sup>4</sup>

For comparison, we also consider arrests for a non-juvenile control group: those ages 20 to 24. We construct a "non-juvenile arrest rate" by dividing the number of non-juvenile arrestees by the local non-juvenile male population, and multiplying by 100,000. The unit of observation is a county-month.

### **3.3 Births to Teenage Mothers**

To construct teenage birth rates at the county level, we use restricted access natality data files with identifiers for all counties for the years 2000 to 2011 from the National Center for Health Statistics (NCHS)<sup>5</sup>. Restricting the sample to births occurring among teen women (ages 15–19), we aggregate individual observations in the natality data by mother's race for each county, quarter, and year. We combine this information with annual county-level bridged-race population estimates for women ages 15–19 from NCHS to construct rates of birth per 100,000 teen women overall and by race group. Because of the availability of natality data for all counties, we are able to consider the impact of all six Promise programs, including Syracuse and Pittsburgh, but also show results of analyses restricted to the four programs for which juvenile crime data are available.

For comparison, we again consider outcomes for a non-teen control group: women ages 20 to 24. We construct a "non-teen birth rate" by dividing the number of births to 20- to 24-year old women

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<sup>4</sup>Note that the "all offenses" category includes additional crimes that are not included in the three subcategories of interest. In other words, it is not the sum of the other categories.

<sup>5</sup>We have requested these data for 2012, to extend the date range to the most recent year included in the arrest analysis.

by the local female population of that age, and multiplying by 100,000. The unit of observation is a county-quarter.

## 4 Empirical Strategy

To assess the impact of Promise program announcements on risky behaviors, we use DiD and DiDiD approaches, controlling for a variety of fixed effects, local characteristics, and county-specific time trends.

Our preferred DiD specification is as follows:

$$y_{ct} = \beta_0 + \beta_1 PostX Promise_{ct} + \beta_2 PostX PromiseX f(time)_{ct} + \beta_3 Post_{ct} + \beta_4 PostX f(time)_{ct} + \gamma_1 D_{ct} + \gamma_2 \delta_c + \gamma_3 \theta_m + \gamma_4 \tau_y + \gamma_5 \delta_c X f(time)_{ct} + \varepsilon_{ct}, \quad (1)$$

where  $c$  indexes counties and  $t$  indexes months or quarters.  $Post$  is an indicator that it is after the date of Promise-program announcement (specified as January 2007 for control counties) and  $Promise$  is an indicator that the county adopted a Promise-type program at some point.  $f(time)$  is a linear function of time (counting months from January 2004 in the arrest rates analysis, or quarters from the first quarter of 2000 in the birth rates analysis).  $D$  is a vector of time-varying local demographic characteristics: the unemployment rate, poverty rate, and racial composition (percents black, white, and Hispanic).  $\delta_c$ ,  $\theta_m$ , and  $\tau_y$  are county, month (for arrest rates) or quarter (for birth rates), and year fixed effects, respectively. Finally, we include county-specific time trends,  $\delta_c X f(time)$ . The outcome variable,  $y_{ct}$ , is the juvenile arrest or birth rate (per 100,000 15-19 year old male or female residents). The teen birth rate is lagged three quarters after the announcement of program availability, to account for nine-month gestation. The coefficients of interest are  $\beta_1$ , which indicates a change in level, and  $\beta_2$ , which indicates a change in slope.

Next, we consider a DiDiD specification, using individuals ages 20-24 as an additional control group. They should not be directly affected by the announcement of Promise-type scholarships, as they have likely already made high-school graduation and college enrollment decisions by that time. However, they would be affected by community-level development due to the introduction of

Promise programs, as well as other trends not captured by our fixed effects and time trends. If the free tuition provided by Promise programs is driving changes in teens' behavior, we should only see an effect on juveniles in Promise counties. Our preferred DiDiD specification is as follows:

$$\begin{aligned}
y_{cjt} = & \beta_0 + \beta_1 PostXPromiseXJuvenile_{cjt} + \beta_2 PostXPromiseXJuvenileXf(time)_{cjt} + \\
& \beta_3 Post_{ct} + \beta_4 Juvenile_j + \beta_5 PostXPromise_{ct} + \beta_6 PostXf(time)_{ct} + \beta_7 PromiseXJuvenile_{cj} + \\
& \beta_8 PostXJuvenile_{cjt} + \beta_9 JuvenileXf(time)_{jt} + \beta_{10} PromiseXJuvenileXf(time)_{cjt} + \\
& \beta_{11} PostXJuvenileXf(time)_{cjt} + \beta_{12} PostXPromiseXf(time)_{ct} + \\
& \gamma_1 D_{ct} + \gamma_2 \delta_c + \gamma_3 \theta_m + \gamma_4 \tau_y + \gamma_5 \delta_c \tau_y + \varepsilon_{cjt}, \quad (2)
\end{aligned}$$

where  $c$  indexes counties,  $j$  indexes age group (juvenile or non-juvenile), and  $t$  indexes months or quarters. Because the two age groups give us variation in treatment within a county-year, we replace county-specific linear trends with more flexible county-by-year fixed effects,  $\delta_c \tau_y$ . The coefficients of interest are  $\beta_1$ , which indicates a change in level, and  $\beta_2$ , which indicates a change in slope.

All models are population-weighted. Heteroskedasticity-robust standard errors are clustered on county.

## 5 Results

Summary statistics are presented in Table 1. The table displays 2004 and 2010 arrest and birth rate means for Promise and non-Promise counties (these are pre- and post-announcement years for all programs). It also presents local demographic measures from 2004 and 2010. On many demographic dimensions, Promise-adopting counties do not look decidedly different from non-participating counties in the same states. Rates of unemployment and living in poverty are similar. Promise counties have a higher percentage of white residents, and correspondingly lower composition of Hispanic residents, and notably have higher arrest and birth rates among black teens. The arrest and birth rates for all teens are lower, however, for Promise counties in the pre-period as compared to their non-Promise adopting counterparts.<sup>6</sup>

<sup>6</sup>Appendix Table A-1 shows unweighted versions of these summary statistics. They are more heavily influenced by counties with very small non-white populations.

Figures 1 and 2 contain local linear graphs of arrest and birth rates over time, separately by age group (15–19 and 20–24) and Promise versus non-Promise counties. The data are recentered so that date 0 is the time of the program announcement, lagged by three quarters for teen childbearing data. (Appendix Figures A-1 and A-2 show the same data with fitted linear trends before and after Promise-type program announcements, providing a visual preview of our regression results.)

The most striking features of the arrest rate graphs is that the juvenile arrest rates in Promise counties are steeply increasing during the pre-Promise period. Trends for the other groups (non-juveniles in Promise counties, juveniles in non-Promise counties, and non-juveniles in non-Promise counties) are relatively flat. The other striking feature is that the juvenile Promise-county trend breaks at the time of the Promise program announcements. After that time, juvenile arrest rates flatten considerably or begin falling. There are hints of similar changes for non-juveniles in Promise counties, which helps motivate using them as a control group. The trends for non-Promise counties are essentially flat throughout this period. It is also worth noting that at their peak, juvenile arrest rates in Promise counties are higher than those for all other groups.

Appendix Figures A-3 and A-4 show the local linear graphs for arrest rates separately by race. Here we see some differences that we will highlight in the discussion of the results.

Teen and adult birth rate graphs are shown separately because the scales are quite different—teen birth rates are lower—so the y-axis scales are different on the left-side figures (for teens) and the right-side figures (for adults). These present total birth rates, while Appendix Figures A-5 and A-6 show birth rates for white and black teens separately. Here it is striking that a post-announcement decline in births is evident for all groups with the exception of black adults in Promise counties who experience an increase over the documented period. The pre-announcement trend for teens in Promise counties is steeper, suggesting a greater reversal in trend for this group. But overall the graphs suggest that there were declines in birth rates generally as well, and that the patterns observed in overall birth rates for Promise counties are driven in no small part by changes in trend for white residents.

## 5.1 Juvenile Crime

Results for juvenile arrest rates are shown in Table 2. The first two columns show the results for all male teens in Michigan and Arkansas; columns 3–4 and 5–6 show results separately for white and black teens, respectively. Note that "all teens" include teens that are not white or black, so those estimates are not simple weighted average of the white teen and black teen estimates.

Column 1 shows the estimates from the DiD specification. It indicates a small (but statistically insignificant) drop in juvenile arrest rates overall, after the announcement of Promise-type scholarships. It also indicates a larger, and statistically-significant, decrease in the slope of the arrest rate trend (the coefficient represents the average change each month). As we saw in the graphs, juvenile arrest rates had been trending upward in Promise-adopting counties before the scholarship programs were announced; they begin trending downward after the announcement.

Column 2, using the DiDiD specification, tells essentially the same story (after netting out effects on non-juveniles): the Promise-type scholarship announcements cause a statistically-significant decrease in the trend in juvenile arrest rates, with no significant change in the level of arrests.

Columns 3–6 reveal similar patterns across both racial subgroups (white teens and black teens). The signs of the estimates are all negative. The change in the slope is sometimes statistically-significant, but the change in the level of arrests never is.

This story is more complicated when we consider different subcategories of crime. Table 3 shows results for three specific categories of crime: the first panel considers violent offenses; the second panel considers drug offenses; and the third panel considers property offenses. (Note that Table 2, included some additional types of crime that are not included in these three sub-categories, so those results were not simple weighted averages of the other estimates.) For violent crimes, columns 1 and 2 show an immediate, statistically-significant increase in the level of juvenile arrests at the time that Promise-type programs are announced. This is consistent for both the DiD and the DiDiD specifications. However, the DiDiD specification still shows a statistically-significant downward change in the slope of the arrest rate trend.

Columns 3–4 and 5–6 reveal that the violent arrest story differs by teens' race. For white teens, program announcements result in a negative effect on both the level and trend in arrest rates. In the

DiDiD specification, both effects are statistically significant. For black teens, there is a statistically-significant *increase* in the level of violent arrests after the programs are announced; this is consistent across the DiD and DiDiD specifications. In the DiDiD specification, there is also a statistically significant increase in the trend in violent arrest rates for black teens.

For drug offenses, we see a statistically-significant increase in the level of juvenile arrests and a statistically-significant decrease in the slope of the juvenile arrest function, when we consider all male teens together. This is consistent across the DiD and DiDiD specifications. However, when we consider white and black teens separately, we again see differences: there is a statistically-significant increase in drug offense arrests for white teens, but a negative effect on the slope. For black teens, there appears to be a negative effect on both the level and slope of the arrest rate function, though these effects are statistically significant only in the DiD specification.

For property offenses, we see an immediate and statistically-significant decrease in the level of juvenile arrests for all male teens. This is consistent across the DiD and DiDiD specifications, but differences emerge when we look at white and black teens separately. For white teens, program announcement reduces the level of arrest rates (though the effect is insignificant), as well as the slope of the arrest function (statistically significant in both the DiD and DiDiD specifications). For black teens, there is a large, statistically significant (in the DiD specification) negative effect on the level of the arrest rate. The effect on the slope is also negative across both specifications, though that effect is not significant.

In summary, Promise program announcements appear to reduce arrest rates overall. The magnitude of the effect in the DiDiD specification suggests that, on average, arrest rates were 30% lower one year after program announcement, relative to their 2004 baseline. There are two exceptions to this beneficial effect: we see more arrests of black teens for violent offenses (both the level and trend increase), and more arrests of white teens for drug offenses (at least in the short run – the change in the trend is negative). Effects on property offense arrests are universally negative.

## 5.2 Births to Teenage Mothers

The main findings from the analysis of Promise program impact on teen birth rates are found in Table 4. The columns display results for all teens, white teens, and black teens respectively.

The specifications use a lagged birth rate outcome, three quarters after the announcement of the program's existence, to capture changes in affected teens' behavior at the implementation of the scholarship program. Importantly, this table presents results when looking at all Promise programs; Table 5 shows the results when just looking at the programs included in the arrest analyses, in Michigan and Arkansas. All specifications include county-varying time trends or county-by-year fixed effects, demographic controls, and county, year, and quarter fixed effects.

Column 1 presents the results of the DiD specification for all Promise programs. The pattern for all teens is similar to that observed in the white (column 3) and black (column 5) teen subgroups. While the coefficients on "post-Promise" are all positive, they are not statistically significant. The "post-Promise" indicator interacted with time provides evidence of a decrease in the slope of the birth rate trend which is also present and statistically significant when looking at white teen birth rates.

Column 2 (and columns 4 and 6 for white and black teens respectively) displays the DiDiD results, comparing teen birth rates to those of a non-teen control group. While the coefficients on both the intercepts and slopes are generally negative across the board, with the exception of the time interaction for black teens, they are not statistically significant in these specifications.

In Table 5, columns 1 and 2 present the results of the DiD and DiDiD specifications respectively for all teens in the four Promise programs introduced in Michigan and Arkansas, to match the sample in the arrest rate analysis. Columns 3 and 4 do so for white teens, and columns 5 and 6 for black teens. In the DiD analysis, there are positive effects on the level of teen births that are statistically significant for the total birth rate and among white teens, and the changes in trend are all negatively signed though insignificant. The DiDiD specification suggests negative effects for all the coefficients of interest—again with the exception of the slope coefficient for black teens—with a statistically significant decline in slope among white teens and a statistically significant drop in level among black teens.

The pattern of results in teen childbearing is suggestive that the introduction of Promise programs may have led to more significant declines in birth rates than would have otherwise happened, particularly among white teens. While the results among black teens are more mixed, that is perhaps driven by what is happening to birth rates among black adults in Promise places over this

timeframe, a steady increase that is quite different than what is observed for all other groups' birth rates.

## 6 Robustness

We repeat our analyses restricting control counties to those that are similar to the Promise counties – that is, those with local characteristics (poverty rates, educational attainment, racial composition) in 2005 that are within the same range as the 2005 characteristics of counties that adopted Promise programs. Summary statistics for this subsample are in Appendix Table A-2. Results, presented in Appendix Tables A-3 and A-4, are nearly identical to those described above.

## 7 Discussion

The presence of effects on nonacademic outcomes is an important consideration for Promise program developers and implementers. These outcomes constitute another critical domain in evaluating the effectiveness and overall impact of programs that seek to improve students' long-run outcomes. We find evidence that teens alter their involvement in risky behaviors after Promise programs are introduced, resulting in a dramatic and beneficial change in trends in arrest rates and suggestive evidence of a reversal in birth rate trends among the targeted groups. The effect on arrest rates is robust to controls for effects on teens in non-Promise counties and non-teens in the same county. The effects on birth rates appear similar to those for other groups (which suggests underlying changes in childbearing behavior more broadly), though their magnitudes are larger.

Importantly, we cannot disentangle direct effects on marginal students who adjust their behaviors in response to the Promise scholarships, spillover effects on their peers, and community economic development as a result of the scholarship program (though we think the latter is unlikely to be realized as immediately as individual student responses, and our older control groups should absorb most of the effect). In addition, with arrest rate data, we are unable to differentiate between adolescents' behaving differently and differential enforcement by police as a result of the scholarship's existence. While teen childbearing does not have an enforcement or monitoring aspect in its measurement, it is also a step removed from the risky behavior itself, so in both cases we use



a next-best proxy for behavior change.

The findings have important implications for policy and program development. As the Promise-type program changes expectations about college-going, and in particular the price of higher education, it alters teens' decisions about risky behaviors as it alters their educational choices and practices. Whether and how substantially teens respond is critical to assessing the relative costs and benefits of this type of investment strategy as compared to alternative programs and policies with similar aims. Anticipated effects vary for inframarginal, intramarginal, and marginal teens, depending on their prior college-going expectations, and the sizes of those groups as well as the direction and relative magnitudes of the effects on their risky behaviors are important policy considerations. It is possible that the aggregate effects presented here mask countervailing effects for these subgroups, which presents an important issue for thinking about policy's intended and unintended consequences.

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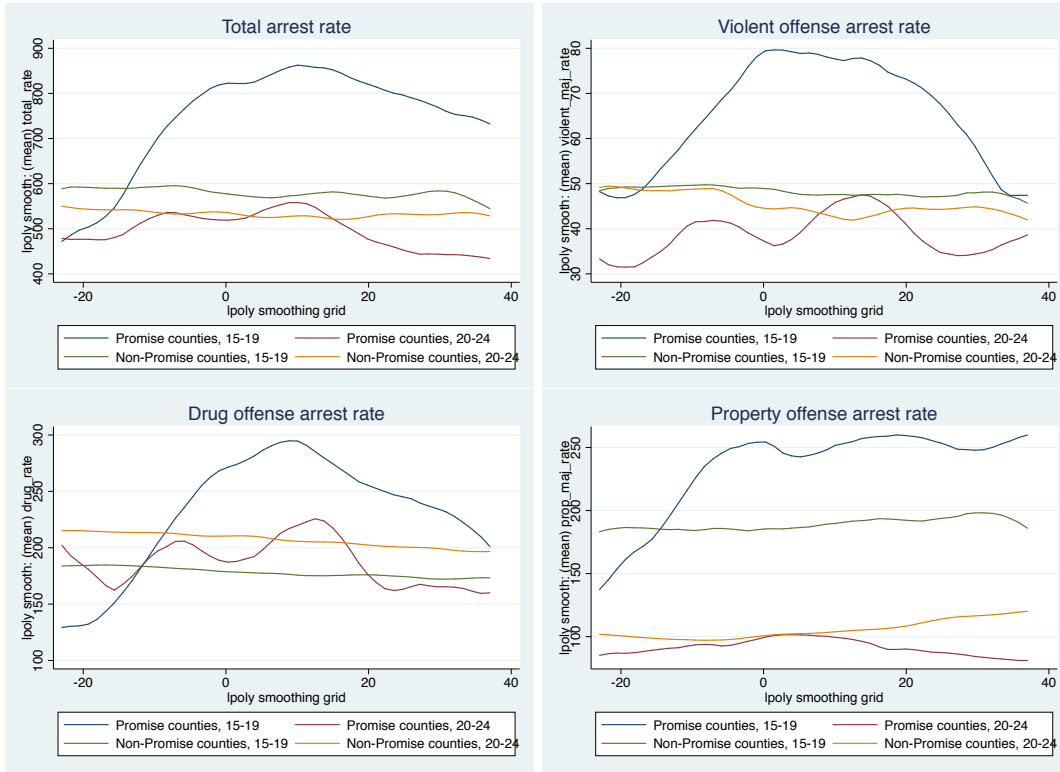
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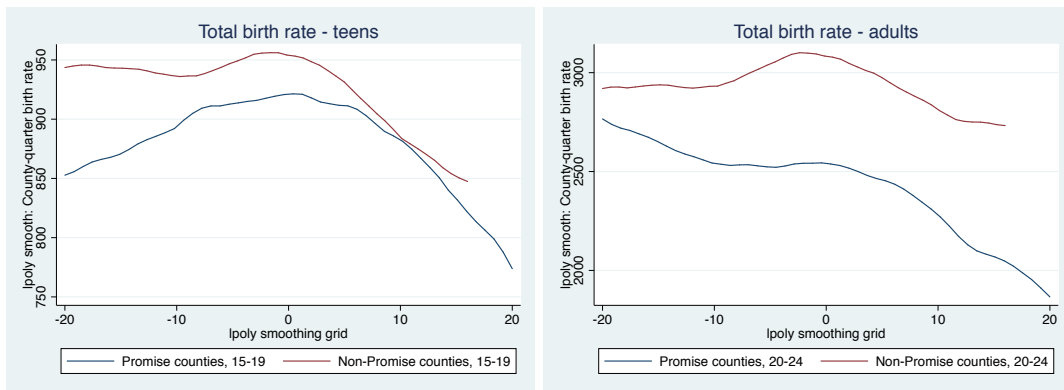
## 8 Figures and Tables

Figure 1: Arrest rates, pre- and post-Promise



Notes: Date 0 is the month of program announcement. Data source: NIBRS 2004-2012.

Figure 2: Birth rates, pre- and post-Promise



Notes: Date 0 is three quarters after program announcement. Data source: NCHS 2000-2011.

Table 1: Summary Statistics

	2004		2010	
	Non-Promise Mean (SD)	Promise Mean (SD)	Non-Promise Mean (SD)	Promise Mean (SD)
<b>Average Monthly Arrest Rate</b>				
All Teens	554.4 (555.6)	499.8 (156.7)	538.9 (446.6)	769.3 (282.4)
<i>N (unweighted)</i>	<i>1,524</i>	<i>48</i>	<i>1,524</i>	<i>48</i>
White Teens	178.2 (312.3)	236.1 (174.0)	248.3 (352.5)	591.5 (299.6)
<i>N (unweighted)</i>	<i>1,524</i>	<i>48</i>	<i>1,524</i>	<i>48</i>
Black Teens	792.7 (1315.0)	1136.5 (542.0)	962.8 (1234.0)	2643.0 (1135.0)
<i>N (unweighted)</i>	<i>1,512</i>	<i>48</i>	<i>1,476</i>	<i>48</i>
<b>Average Quarterly Birth Rate</b>				
All Teens	778.2 (391.6)	677.6 (203.6)	693.9 (347.7)	625.9 (204.8)
<i>N (unweighted)</i>	<i>1,124</i>	<i>24</i>	<i>1,124</i>	<i>24</i>
White Teens	530.2 (349.8)	389.1 (189.3)	483.7 (335.2)	379.9 (142.9)
<i>N (unweighted)</i>	<i>1,124</i>	<i>24</i>	<i>1,124</i>	<i>24</i>
Black Teens	1348.4 (648.5)	1917.3 (366.4)	1179.9 (598.8)	1565.9 (491.5)
<i>N (unweighted)</i>	<i>1,088</i>	<i>24</i>	<i>1,120</i>	<i>24</i>
<b>Local Demographics</b>				
Percent White	72.38 (23.09)	82.90 (2.93)	69.74 (23.07)	80.91 (2.91)
Percent Black	13.90 (13.51)	12.35 (3.54)	14.17 (12.91)	13.10 (3.30)
Percent Hispanic	9.30 (10.72)	1.99 (0.98)	10.94 (11.38)	2.64 (1.23)
Unemployment Rate	6.10 (1.48)	5.38 (0.50)	9.62 (2.39)	8.16 (1.09)
Poverty Rate	13.32 (5.66)	12.07 (1.40)	15.53 (6.34)	13.81 (3.10)
<i>N (unweighted)</i>	<i>1,124</i>	<i>24</i>	<i>1,124</i>	<i>24</i>

Arrest and birth rates are for local adolescents, per 100,000 adolescent males or females, respectively.

All statistics are population-weighted. Observations are county-months or county-quarters.

Arrest rate statistics include counties in Michigan and Arkansas; birth rate statistics and local demographics include counties in Michigan, Arkansas, Pennsylvania, and New York.

Table 2: Effect of Promise Programs on Arrest Rates for Men Ages 15–19

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>All Offenses</b>						
Post-Promise	-4.309		-16.37		-127.4	
	(44.93)		(40.82)		(102.3)	
Post-Promise X Time	-9.813***		-4.096		-18.38***	
	(2.389)		(2.900)		(5.313)	
Post-Promise X Juvenile		16.13		-3.599		-81.29
		(38.48)		(33.71)		(278.2)
Post-Promise X Juvenile X Time		-13.72***		-7.907**		-21.86
		(2.562)		(3.352)		(26.25)
<i>2004 Promise-county mean</i>	499.8	499.8	236.1	236.1	1137	1137
Observations	7990	15980	7990	15980	7990	15716
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X

Robust standard errors are clustered by county, and shown in parentheses. Data source: NIBRS 2004-2012.

Outcome measure: arrest rate (arrests of juvenile men, divided by the juvenile male population).

Non-juvenile control group in triple-difference specification is arrestee rate for adults age 20-24.

All specifications include county, year, and month-of-year fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$



Table 3: Effect of Promise Programs on Arrest Rates for Men Ages 15–19

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Violent Offenses</b>						
Post-Promise	34.45***		-9.848		211.7**	
	(10.14)		(6.974)		(103.1)	
Post-Promise X Time	-1.181		-1.506*		1.193	
	(1.048)		(0.804)		(6.848)	
Post-Promise X Juvenile		24.02**		-16.21*		231.8***
		(11.51)		(9.760)		(30.21)
Post-Promise X Juvenile X Time		-1.089**		-1.760***		2.511**
		(0.438)		(0.365)		(1.235)
<i>2004 Promise-county mean</i>	40.30	40.30	25.89	25.89	117.3	117.3
<b>Drug Offenses</b>						
Post-Promise	31.66**		47.61***		-54.59**	
	(13.89)		(17.11)		(21.28)	
Post-Promise X Time	-6.058**		-4.173***		-15.63***	
	(2.390)		(1.422)		(4.740)	
Post-Promise X Juvenile		51.14***		73.10***		-74.17
		(15.43)		(29.77)		(110.6)
Post-Promise X Juvenile X Time		-7.465***		-5.301***		-12.93
		(0.901)		(1.422)		(10.85)
<i>2004 Promise-county mean</i>	127.5	127.5	89.10	89.10	330.0	330.0
<b>Property Offenses</b>						
Post-Promise	-66.05**		-9.889		-299.0***	
	(28.38)		(33.76)		(106.4)	
Post-Promise X Time	-3.822		-4.686**		-3.699	
	(2.973)		(1.786)		(9.757)	
Post-Promise X Juvenile		-63.53**		-39.04		-242.2
		(26.10)		(36.14)		(148.0)
Post-Promise X Juvenile X Time		-4.732**		-3.120***		-13.04
		(1.955)		(1.173)		(12.50)
<i>2004 Promise-county mean</i>	176.8	176.8	143.9	143.9	366.6	366.6
Observations	7990	15980	7990	15980	7990	15716
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X

Robust standard errors are clustered by county, and shown in parentheses. Data source: NIBRS 2004-2012.

Outcome measure: arrest rate (arrests of juvenile men, divided by the juvenile male population).

Non-juvenile control group in triple-difference specification is arrestee rate for adults age 20-24.

All specifications include county, year, and month-of-year fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Table 4: Effect of Promise Programs on Lagged Birth Rates for Women Ages 15–19

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Promise	26.98		16.67		51.02	
	(25.32)		(16.40)		(91.11)	
Post-Promise X Time	-3.408***		-2.434***		-2.124	
	(1.146)		(0.7408)		(3.428)	
Post-Promise X Teen		-41.20		-6.47		-174.6
		(46.22)		(27.65)		(121.5)
Post-Promise X Teen X Time		-5.503		-3.513		15.53
		(6.447)		(6.866)		(10.45)
<i>2004 Promise-county mean</i>	677.6	677.6	389.1	389.1	1917	1917
Observations	13776	27552	13776	27552	13440	26668
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X

Robust standard errors are clustered by county, and shown in parentheses.

Data source: NCHS 2000-2011.

Outcome measure: birth rate (births to teen women per 100,000 teenage female residents).

Non-teen control group in triple-difference specification is birth rate for women ages 20-24.

Specifications are population-weighted and include county, year, and quarter fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Table 5: Effect of Promise Programs on Lagged Birth Rates for Women Ages 15–19, MI & AR only

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Promise	94.18*** (23.85)		76.02*** (20.11)		114.92 (81.60)	
Post-Promise X Time	-2.455 (2.029)		-1.999 (2.169)		-5.125 (7.987)	
Post-Promise X Teen		-238.1 (188.9)		-184.5 (154.5)		-532.1*** (183.9)
Post-Promise X Teen X Time		-15.38 (16.16)		-26.32*** (9.847)		38.87 (37.61)
<i>2004 Promise-county mean</i>	868.0	868.0	520.9	520.9	2374	2374
Observations	7584	15168	7584	15168	7296	14368
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X

Robust standard errors are clustered by county, and shown in parentheses.

Data source: NCHS 2000-2011.

Outcome measure: birth rate (births to teen women per 100,000 teenage female residents).

Non-teen control group in triple-difference specification is birth rate for women ages 20-24.

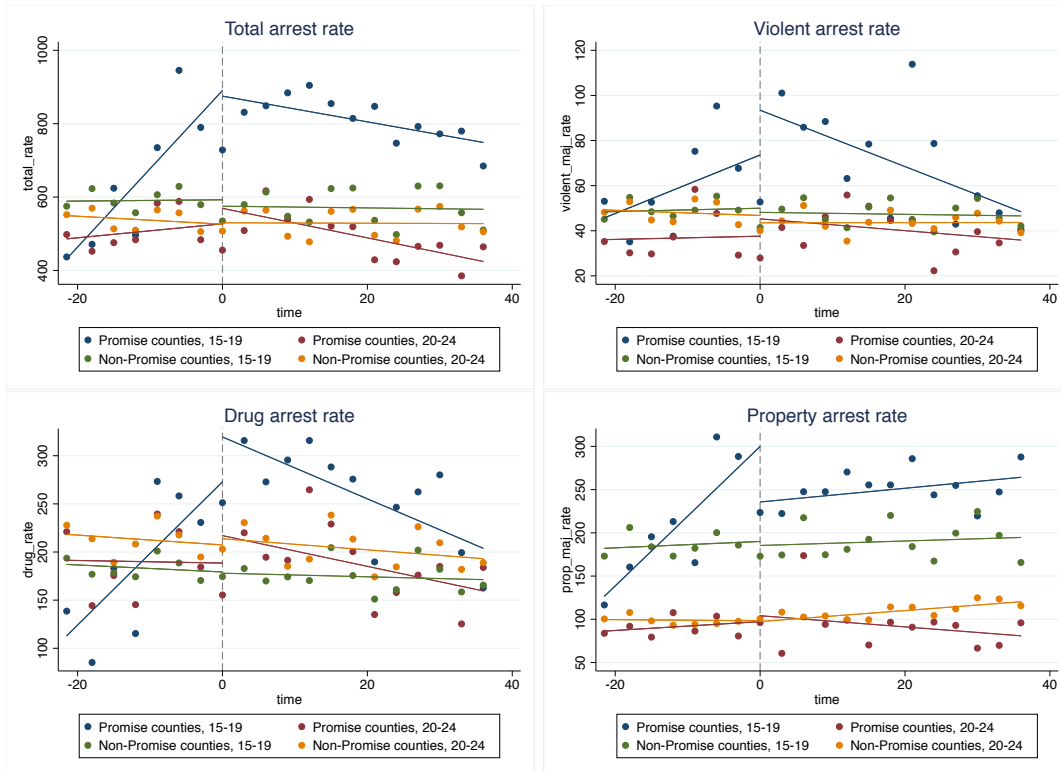
Specifications are population-weighted and include county, year, and quarter fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

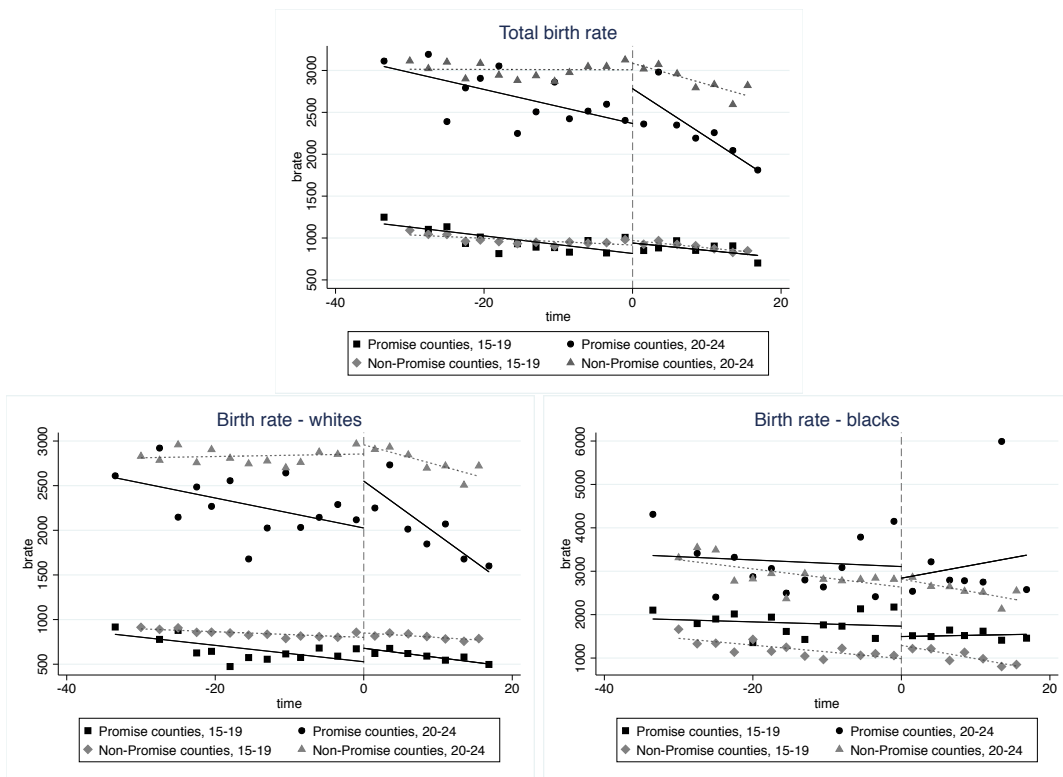
# A Appendix Figures and Tables

Figure A-1: Arrest rates, pre- and post-Promise



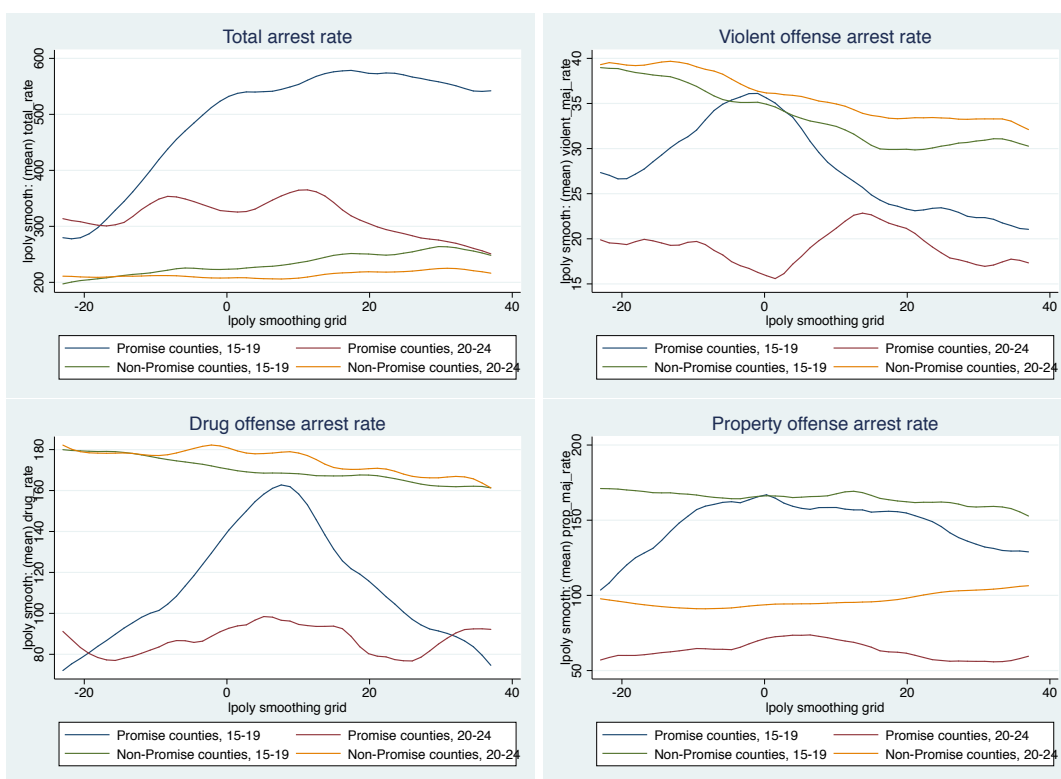
Notes: Date 0 is the month of program announcement. Data source: NIBRS 2004-2012.

Figure A-2: Birth rates, pre- and post-Promise



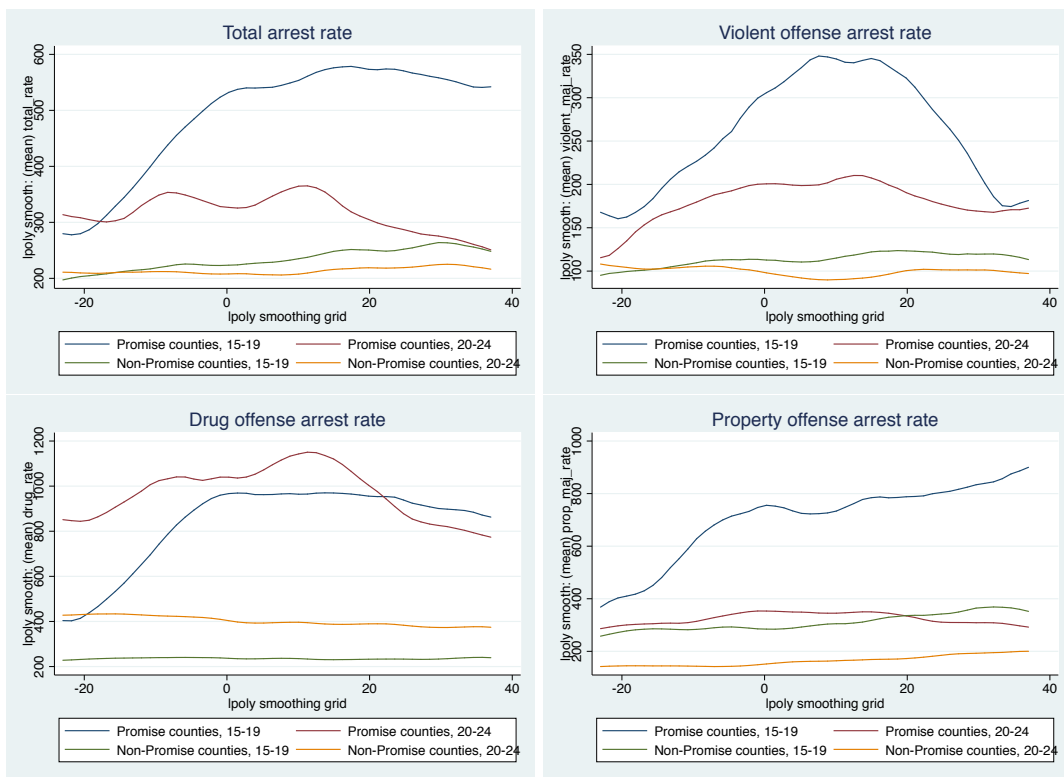
Notes: Date 0 is three quarters after program announcement. Data source: NCHS 2000-2011.

Figure A-3: Arrest rates for white residents, pre- and post-Promise



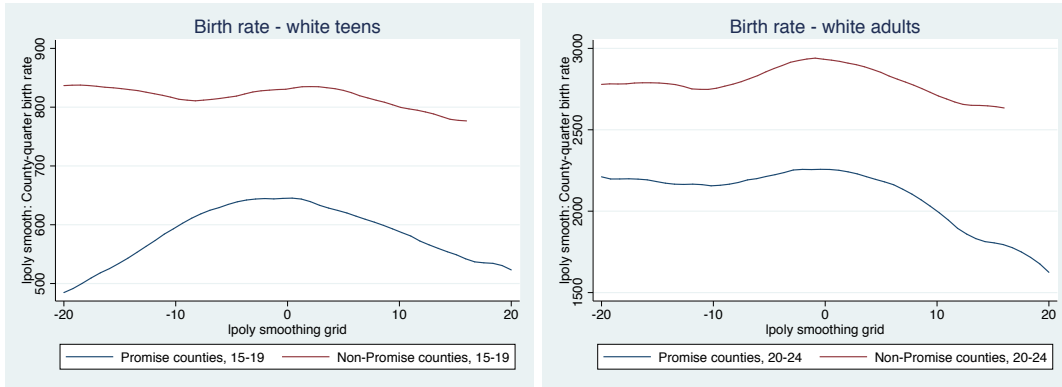
Notes: Date 0 is the month of program announcement. Data source: NIBRS 2004-2012.

Figure A-4: Arrest rates for black residents, pre- and post-Promise



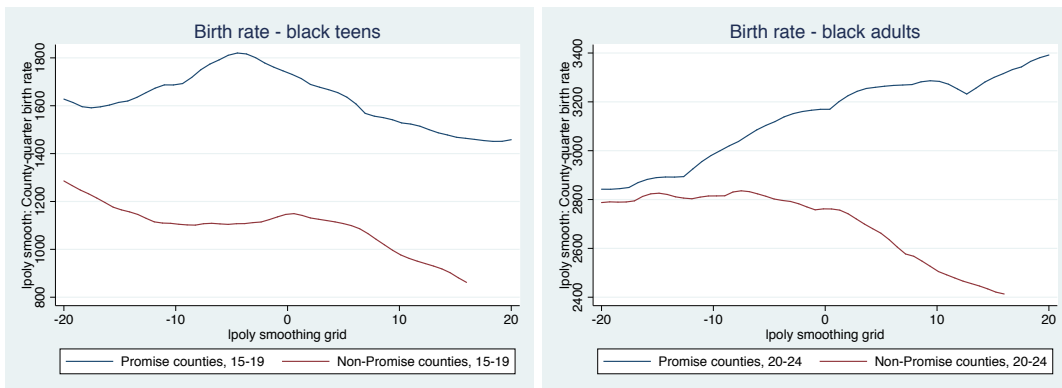
Notes: Date 0 is the month of program announcement. Data source: NIBRS 2004-2012.

Figure A-5: Birth rates for white residents, pre- and post-Promise



Notes: Date 0 is three quarters after program announcement. Data source: NCHS 2000-2011.

Figure A-6: Birth rates for black residents, pre- and post-Promise



Notes: Date 0 is three quarters after program announcement. Data source: NCHS 2000-2011.



Table A-1: Summary Statistics – Unweighted

	2004		2010	
	Non-Promise Mean (SD)	Promise Mean (SD)	Non-Promise Mean (SD)	Promise Mean (SD)
<b>Average Monthly Arrest Rate, Unweighted</b>				
All Teens	766.0 (1824)	426.3 (251.0)	655.2 (1245)	490.8 (447.4)
<i>N</i>	1,524	48	1,524	48
White Teens	253.0 (1056)	276.0 (303.4)	278.3 (909.2)	423.6 (502.0)
<i>N</i>	1,524	48	1,524	48
Black Teens	1738.5 (9074)	683.2 (645.5)	1916 (8729)	1222 (1433)
<i>N</i>	1,512	48	1,476	48
<b>Average Quarterly Birth Rate, Unweighted</b>				
All Teens	947.0 (578.3)	1074.4 (720.1)	875.8 (540.4)	916.6 (565.9)
<i>N</i>	1,124	24	1,124	24
White Teens	834.6 (555.2)	795.5 (762.5)	787.5 (547.9)	602.8 (401.3)
<i>N</i>	1,124	24	1,124	24
Black Teens	1136.2 (2328.7)	1819.2 (1204.3)	1076.9 (3858.4)	1872.6 (2470.6)
<i>N</i>	1,088	24	1,120	24
<b>Local Demographics, Unweighted</b>				
Percent White	87.18 (14.20)	82.04 (8.70)	85.73 (14.65)	80.66 (9.36)
Percent Black	7.40 (11.65)	13.02 (9.93)	7.74 (11.72)	13.48 (10.06)
Percent Hispanic	3.55 (4.88)	2.49 (0.89)	4.38 (5.55)	3.16 (0.98)
Unemployment Rate	6.49 (1.68)	6.58 (2.02)	10.05 (2.82)	10.42 (2.79)
Poverty Rate	13.30 (4.11)	13.67 (4.21)	16.48 (5.18)	17.02 (5.77)
<i>N</i>	1,124	24	1,124	24

Arrest and birth rates are for local adolescents, per 100,000 adolescent males or females respectively.

Observations are county-months or county-quarters.

Arrest rate statistics include counties in Michigan and Arkansas; birth rate statistics and local demographics include counties in Michigan, Arkansas, Pennsylvania, and New York.

Table A-2: Summary Statistics – Similar counties only

	2004		2010	
	Non-Promise Mean (SD)	Promise Mean (SD)	Non-Promise Mean (SD)	Promise Mean (SD)
<b>Average Monthly Arrest Rate</b>				
All Teens	663.9 (871.0)	499.8 (156.7)	631.7 (602.1)	769.3 (282.4)
White Teens	311.3 (478.9)	236.1 (174.0)	382.6 (422.4)	591.5 (299.6)
Black Teens	1439.6 (1541.3)	1136.5 (542.0)	1500.6 (1530.3)	2643.0 (1135.0)
<i>N (unweighted)</i>	180	48	180	48
<b>Average Quarterly Birth Rate</b>				
All Teens	879.0 (302.7)	677.6 (203.6)	810.1 (275.4)	625.9 (204.8)
White Teens	586.1 (293.5)	389.1 (189.3)	539.8 (306.5)	379.9 (142.9)
Black Teens	1859.7 (627.7)	1917.3 (366.4)	1569.9 (525.4)	1565.9 (491.5)
<i>N (unweighted)</i>	132	24	132	24
<b>Local Demographics</b>				
Percent White	79.81 (4.53)	82.90 (2.93)	77.14 (4.61)	80.91 (2.91)
Percent Black	11.48 (5.39)	12.35 (3.54)	12.06 (5.33)	13.10 (3.30)
Percent Hispanic	6.13 (3.22)	1.99 (0.98)	7.81 (4.58)	2.64 (1.23)
Unemployment Rate	6.20 (1.08)	5.38 (0.50)	9.78 (1.92)	8.16 (1.09)
Poverty Rate	13.13 (1.92)	12.07 (1.40)	16.35 (2.78)	13.81 (3.10)
<i>N (unweighted)</i>	132	24	132	24

Arrest and birth rates are for local adolescents, per 100,000 adolescent males or females, respectively.

All statistics are population-weighted. Observations are county-months or county-quarters.

Arrest rate statistics include counties in Michigan and Arkansas; birth rate statistics and local demographics include counties in Michigan, Arkansas, Pennsylvania, and New York.

Table A-3: Effect of Promise Programs on Arrest Rates for Men Ages 15–19

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>All Offenses</b>						
Post-Promise	59.54		63.42		138.0	
	(50.26)		(51.97)		(177.3)	
Post-Promise X Time	-15.55***		-8.209		-31.42*	
	(4.576)		(5.180)		(15.08)	
Post-Promise X Juvenile		8.606		15.51		-52.01
		(56.75)		(47.40)		(332.9)
Post-Promise X Juvenile X Time		-13.00***		-9.024**		-25.71
		(3.628)		(4.108)		(27.53)
<i>2004 Promise-county mean</i>	499.8	499.8	236.1	236.1	1137	1137
<b>Violent Offenses</b>						
Post-Promise	29.74**		-13.59		217.0**	
	(11.62)		(10.03)		(69.10)	
Post-Promise X Time	-2.274**		-1.649		-3.058	
	(1.071)		(1.214)		(6.480)	
Post-Promise X Juvenile		16.63		-20.73*		218.3***
		(13.94)		(11.84)		(49.46)
Post-Promise X Juvenile X Time		-1.030		-1.317**		1.235
		(0.696)		(0.538)		(3.325)
<i>2004 Promise-county mean</i>	40.30	40.30	25.89	25.89	117.3	117.3
<b>Drug Offenses</b>						
Post-Promise	58.94***		65.07***		10.28	
	(16.57)		(17.55)		(45.93)	
Post-Promise X Time	-7.677**		-6.653**		-13.79	
	(3.110)		(2.746)		(8.640)	
Post-Promise X Juvenile		59.08*		82.70**		-87.97
		(30.23)		(31.62)		(138.1)
Post-Promise X Juvenile X Time		-6.315***		-4.560***		-10.71
		(1.098)		(1.242)		(11.02)
<i>2004 Promise-county mean</i>	127.5	127.5	89.10	89.10	330.0	330.0
<b>Property Offenses</b>						
Post-Promise	-36.13		-22.05		-108.9	
	(30.37)		(27.76)		(68.61)	
Post-Promise X Time	-6.312*		-7.579**		-8.252	
	(3.224)		(2.798)		(6.620)	
Post-Promise X Juvenile		-52.28*		-36.91		-177.1
		(29.07)		(36.91)		(159.3)
Post-Promise X Juvenile X Time		-5.331*		-3.415*		-14.98
		(2.542)		(1.924)		(13.14)
<i>2004 Promise-county mean</i>	176.8	176.8	143.9	143.9	366.6	366.6
Observations	1158	2316	1158	2316	1146	2292
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X
Controls restricted to similar counties	X	X	X	X	X	X

Robust standard errors are clustered by county, and shown in parentheses. Data source: NIBRS 2004-2012.

Outcome measure: arrest rate (arrests of juvenile men, divided by the juvenile male population).

Non-juvenile control group in triple-difference specification is arrestee rate for adults age 20-24.

All specifications include county, year, and month-of-year fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Table A-4: Effect of Promise Programs on Lagged Birth Rates for Women Ages 15–19

	All Teens		White Teens		Black Teens	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>All Promise Programs</b>						
Post-Promise	14.88		18.92		-2.540	
	(29.77)		(26.58)		(103.8)	
Post-Promise X Time	-2.539		-1.931		-5.134	
	(1.947)		(1.321)		(5.310)	
Post-Promise X Teen		-57.55		-34.02		-143.1
		(56.94)		(43.07)		(174.7)
Post-Promise X Teen X Time		-1.239		-2.415		8.065
		(7.279)		(7.338)		(12.69)
<i>2004 Promise-county mean</i>	677.6	677.6	389.1	389.1	1917	1917
Observations	1872	3744	1872	3744	1872	3744
<b>MI &amp; AR Promise Programs Only</b>						
Post-Promise	86.26*		89.30*		114.21	
	(45.23)		(45.58)		(99.96)	
Post-Promise X Time	-5.659		-3.882		-20.24*	
	(3.381)		(3.888)		(11.21)	
Post-Promise X Teen		-270.8		-241.07		-562.80**
		(197.3)		(167.9)		(203.63)
Post-Promise X Teen X Time		-14.56		-24.88**		30.20
		(17.13)		(10.74)		(39.95)
<i>2004 Promise-county mean</i>	868.0	868.0	520.9	520.9	2374	2374
Observations	1200	2400	1200	2400	1200	2400
Demographic Controls	X	X	X	X	X	X
County Trends	X		X		X	
Triple-Difference		X		X		X
County*Year FEs		X		X		X
Controls restricted to similar counties	X	X	X	X	X	X

Robust standard errors are clustered by county, and shown in parentheses.

Data source: NCHS 2000-2011.

Outcome measure: birth rate (births to teen women per 100,000 teenage female residents).

Non-teen control group in triple-difference specification is birth rate for women ages 20-24.

Specifications are population-weighted and include county, year, and quarter fixed effects.

Triple difference includes county-by-year fixed effects.

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$