

The Incidental Fertility Effects of School Condom Distribution Programs

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While the fertility effects of improving teenagers' access to contraception are theoretically ambiguous, most empirical work has shown that access decreases teen fertility. In this paper, we consider the fertility effects of access to condoms—a method of contraception not considered in prior work. We exploit variation across counties and across time in teenagers' exposure to condom distribution programs in schools. These programs began in the early 1990s as hundreds of schools across the United States made condoms available on-site to students in an effort to prevent HIV transmission. We find that access to condoms in schools leads to an increase in teen fertility of about 10 percent, or about 4 extra births per 1,000 teen-age women. These effects are driven by communities where condoms are provided without mandated counseling on their use.

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I. Introduction

The US teen birth rate is far above that of other industrialized countries, and one controversial approach to addressing the issue has been to improve young people's access to contraception. Proponents of this approach argue that improved access will reduce the likelihood of an unwanted pregnancy among sexually active teenagers. However, others argue that providing contraception to teenagers encourages sexual activity and could actually increase teen births if failure rates are sufficiently high or if the method is used inconsistently (Paton 2002; Arcidiacono, Khwaja, and Ouyang, 2012).

The effect of contraceptive access on teen fertility is therefore an empirical question, and a large body of work in the social sciences has considered it. Much of this work has focused on access to oral contraception (the Pill) in the 1960s and 1970s, finding that access not only lowered teen fertility, but also improved long-term career and family outcomes (Goldin and Katz, 2002; Bailey, 2006; Pantano, 2007; Guldi, 2008; Ananat and Hungerman, 2012). Guldi (2008) simultaneously examines the effects of abortion and Pill access in the late 1960s and 1970s, and shows that better access to abortion decreased birth rates for minors. Joyce, Kaestner, and Colman (2006) document a decrease in abortions and an increase in births in response to Texas' parental notification laws for abortions in the late 1990s. Recent work has also examined the effects of access to emergency contraception and found that while sexually-transmitted infections increase—suggesting a behavioral response—there is no effect on teen fertility (Girma and Paton, 2011; Durrance, 2013).

In this paper, we consider the effects of condom access on teen fertility—a contraceptive method that has received much less attention in the literature. Despite the fact that research on other methods consistently suggests that contraceptive access lowers teen fertility (or in some cases has no effects), there are reasons to believe that the effects of condom access programs may be different. First, condoms are a relatively less effective method of birth control—one-year failure rates for condoms are more than double those of the Pill (Trussell, 2011). Second, condom use relies more heavily on the male partner, whereas the methods mentioned above rely more heavily on the female. This may be important given gender differences in the costs of an unintended pregnancy. Third, whereas most of the above studies consider changes in

access to contraception that occurred in the 1960s and 1970s (with the exception of studies on emergency contraception), the change in access to condoms that we study here took place more recently. And fourth, a careful study of the effects of condom distribution has important implications for policy, as both the American Academy of Pediatrics in the United States and the National Institute for Health and Care Excellence in the UK have recently advocated for condom distribution in schools (American Academy of Pediatrics, 2013; National Institute for Health and Care Excellence, 2014).¹ School districts in Boston, Chicago, and elsewhere have consequently reconsidered the role of condoms in their schools (Bidgood, 2013; Chicago Tribune, 2014; Walsh, 2013).

To our knowledge there is no research that provides evidence on how condom access in schools—or condom access more generally—impacts teen fertility.² The goal of this paper is to provide such evidence. To do so, we consider a massive policy intervention that affected millions of teenagers during the early 1990s: the introduction of condoms in schools to prevent HIV transmission. During this period, hundreds of schools across the country provided condoms to their students. We construct a national dataset documenting the introduction of condom access programs across the country. We then match this data to national data on birth rates, allowing us to observe different birth outcomes to women of different ages in different communities. The massive size of this intervention allows us to identify fertility effects that would be missed by using a single school or even a single large school district. We can also explore, parsimoniously, whether the effects of certain types of programs, such as those mandating counseling on condom use, differed from other programs.

We find clear evidence that access to condoms in schools leads to an *increase* in teen fertility. This increase is observed in both rigorous regression specifications and in a basic visual inspection of the data. It does not appear to be driven by differential trends or reverse causation and it is robust to using births to slightly older women as a control. The effects are reasonably large in magnitude: access to condoms leads to about 3 or 4 extra births per 1,000 teenage women. These effects are driven by communities where condom

¹ In November of 2013, the AAP released a policy statement arguing that “schools should be considered appropriate sites for the availability of condoms” and in March of 2014 a NIHCE statement advocated that free condoms should be “readily accessible” at “schools, colleges, and youth clubs.”

² We discuss the literature on the effects of school condom access on sexual behaviors and STIs in the next section.

access was provided without mandated counseling. We find evidence that these effects may have been attenuated, or perhaps even reversed, when counseling was mandated as part of condom provision. This may help reconcile our results with those of Lovenheim, Reback, and Wedenoja (2014), who show that school-based health clinics offering contraceptive services significantly lowered teen fertility. If health clinics can effectively combine contraception access and counseling, this may lead to very different effects than access alone—a conclusion similar to the one drawn by Kirby (2002).

These results suggest that the findings of past work on the impacts of Pill and abortion access on fertility in the 1960s and 1970s may not necessarily match the impacts of other contraceptive methods, and that the circumstances of contraception access may matter a great deal. These results also suggest caution both in interpreting recent policy guidelines for condom provision in schools and in inferring fertility outcomes from small-scale prior studies discussing condom access and sexual behavior.

Our paper proceeds as follows. The next section describes school-based condom access programs and discusses the literature on their effects. Section 3 presents the estimation strategy, and our results are in Section 4. Section 5 concludes.

II. School Condom Distribution Programs

A. Overview of Condom Programs

In the early 1990s, hundreds of schools across the United States began to make condoms available on-site to students.³ Adams County School District 14 in Commerce City, Colorado was the first district to implement a school-based condom program directly on school grounds in 1989.⁴ The largest district in the country, New York City Public Schools, did so in 1991. The activity in New York began after the appointment of Joseph Fernandez as chancellor following the unexpected death of chancellor Richard Green, who died of an asthma attack after only 14 months on the job; during Green's brief tenure little work was done to address concerns about AIDS (Johnson, 1999). The second-largest district, Los Angeles Unified

³ In this section we draw on a wide variety of sources, but particularly noteworthy and extensive discussions can be found Samuels and Smith (1993), Kirby and Brown (1996), and Johnson (1998).

⁴ Kirby and Brown (1996) also provide numbers indicating a few school clinics in the United States made condoms available in the 1980s (see Table 1 of their paper).

School District, followed with a condom program in 1992. By the middle of the decade, dozens of school districts had implemented a policy that allowed students to obtain condoms at school.

Typically, condoms were provided by an intermediary, and most schools made condoms available through multiple sources.⁵ The most common method of providing condoms was through a school nurse (including either nurses employed by the district or nurses in clinics employed by outside agencies) and teachers. Nearly half of condom programs also made condoms available from counselors, and about a quarter of programs made condoms available through other employees or the school principal. A small number (less than 5%) made condoms available from sources such as vending machines or baskets. Almost all schools made condoms free to obtain, although some suggested or charged a small fee such as 25 cents (Brown et al., 1997). The vast majority of programs were provided in high schools; most programs (about 75%) were located in what Kirby and Brown call “regular academic schools,” but a relatively high proportion were found in alternative schools such as schools for students with children or facing incarceration. About a quarter of all programs were run in conjunction with a school health center that typically provided other services, such as physicals.

Most programs allowed parents to “opt out” on behalf of their children if they wished (although research suggests that very few parents—typically just 2 or 3 percent—did so). Importantly, most programs were implemented at the district level, and the vast majority of programs were adopted by school boards (Leitman, Kramer, and Taylor, 1993).

Several sources agree that whether counseling is part of the district's program is key. Lewin (1991) writes “even those who argue most vociferously that teen-agers need better access to contraception concede that condom programs may not have much effect unless they include counseling and social support;” Martinez-Donate (2004) and Taylor (1991) make similar arguments. As described by Kirby and Brown (1996), “during counseling, students are commonly informed that abstinence is the safest method of protection against STDs; they are also instructed about the proper methods of storing and using condoms.” Fortunately, when collecting information about programs we were able to identify in most cases whether or

⁵ Most of the numbers for this discussion are taken from a national survey of programs conducted by Kirby and Brown (1996).

not counseling was mandated by the district; about half of the programs we use in our study are programs with district-mandated counseling accompanying diffusion.

One might wonder whether students took condoms provided at schools. Kirby and Brown (1996) find that the median school distributed about one condom per student per year, a reasonably large number, although there was large variation with about a fourth of schools distributing less than half a condom per student/year and nearly a sixth of schools distributing more than six condoms per student/year. Alternative schools and smaller schools had higher numbers of condoms distributed.

Of course, taking condoms and using them are separate issues. Moreover, if condom distribution at school merely crowded out obtaining condoms from non-school locations (a possibility discussed by Kirby et al., 1999), then schools could distribute condoms to no effect. Cohen (1999) reviews several studies on the effects of school condom programs on condom use; many (but not all) studies find that the programs led to greater condom use among sexually active students; Kirby and Coyle (1997) present similar survey results and Schuster et al.'s (1998) study of the Los Angeles program suggests that the impacts of the program may be the largest for the least sexually experienced adolescents (such as those initiating intercourse for the first time). Studies typically find little evidence that condom access increases rates of sexual activity. Wretzel et al. (2011) show that a condom availability program lowered sexually transmitted infection rates in one school district.

However, nearly all of this work exploring the effects of condom distribution in schools has suffered from a number of methodological challenges that would prevent a rigorous study of effects on teen fertility. A well-cited survey by Kirby (2002) notes that almost no prior study (1) utilizes both pre- and post-access data, (2) compares students gaining access to other students, and (3) employs large sample sizes. The one noted exception, Kirby et al. (1999), uses data from 10 high schools, a sample that may be too small to study teen fertility. Subsequent work (e.g., Martinez-Donate et al., 2004; Blake et al., 2003) has also faced these challenges.

Furthermore, even if school condom programs led to greater condom use, if condoms are used imperfectly, or if distribution “crowds out” more reliable contraception or creates other changes in behavior,

then this access could actually lead to higher teen fertility rates over time (Stryker, Samuels, and Smith, 1993). In this paper, we provide the first investigation on the effects of school condom distribution on fertility using a rigorous identification strategy and data on condom access programs and teen births from across the country. We describe this data next.

B. Data on Condom Programs

Using Samuels and Smith (1993), Kirby and Brown (1996), and Johnson (1998) as starting points, and supplementing these sources with popular-press coverage, we collected information on condom distribution programs implemented in the 1980s and early 1990s. This gave us a list of districts with programs making condoms available to schools. We included any district where (a) we had documentation of a district condom access program (b) there was information about whether counseling was required by the district (c) there was information about whether the program was district wide, and, if it was not district wide, which schools participated, and (d) there was clear information available about when the program began. Several districts which had condom programs were dropped from our analysis because they lacked some of this information.⁶

Table 1 lists the districts we identify as districts implementing a condom access program during our period of study. The list includes 22 districts in 12 states (including DC) with a total of 484 affected schools (we discuss this number of schools momentarily). The list shows that most programs were implemented in 1992 or 1993. About half of the programs feature mandatory counseling.

Given that we compiled our list well after these programs were introduced, there might be a concern that we have missed a large number of programs. Fortunately, this does not appear to be the case. In 1995—just after the explosion of condom access programs—Kirby and Brown undertook a national survey of school

⁶ Many of the programs we lost were in the state of Massachusetts. In the fall of 1991, the Massachusetts Department of Education suggested that schools consider making condoms available to students. Many of the schools and districts in the state did (Nealon, 1993), but in several instances we were unable to locate clear information on the details of a particular program. Programs where the exact timing of implementation was unclear included the programs in Chelsea (MA), Dade County (FL), Hatfield (MA), and Jackson Public School District (MS). Places where we had clear information about the timing of a program but lacked other information included Martha's Vineyard (MA), Palm Beach (FL), Portsmouth (MA), and Somerville (MA). Places dropped because it was unclear which schools or clinics participated include Dallas (TX), Little Rock (AR), and, perhaps most notably, Chicago Public Schools (IL). The Gadsden County School District (FL) could not be matched to the birth certificate dataset as the county's population was too small for inclusion in the data (as discussed more below). The counties housing these districts were excluded from the analysis, although fortunately this still leaves us with the vast majority of students and counties in the country.

condom-access programs. They identified a total of 421 schools with programs, a number quite comparable to the number we identify.⁷ We also identify about half of all programs as requiring counseling, and this again is very close to the numbers in their survey (see Table 2 in their paper). This gives us confidence that our efforts to collect information have been acceptably thorough. To the extent that we miss programs and thus identify a treatment county as a control county, our estimates will be biased towards zero.

While Table 1 shows that condom access programs were in both large school districts and small school districts all over the country, the table also shows that programs were primarily located in the northeast and the west. Districts introducing condom programs were in counties with slightly higher teen birth rates in 1990 than other districts (6.65 births per 100 females aged 15 to 19 versus 5.97 births), and may have differed in other harder-to-observe ways. One might thus ask why some districts adopted condom access. Discussions of the introduction of these programs at the time overwhelmingly point to concerns with AIDS as the primary driver of condom access adoption (e.g., Banks, 1991; Goldstein and Bates, 1993; Tillman 1992).

But, of course, communities with especially strong concerns about AIDS might have different populations of students than other districts. We have several responses to this observation. First, the nature of our identification strategy involves comparing changes over time in fertility between condom-adopting communities and other communities so that persistent differences across communities should not confound the analysis. Next, it is possible that divergent *trends* in outcomes between communities could lead to changes in fertility between communities even absent the adoption of condom programs. For example, it could be that changes in teen birth rates lead to condom programs being introduced, rather than the other way around. Fortunately, all of the national results below include controls for such trends and our findings are robust (as we show) to either parsimonious or more aggressive trend controls; and we present both simple and more sophisticated evidence indicating that reverse causality is not driving the results.

Next, one might wonder whether communities fighting AIDS through condom programs might

⁷ We would like to have used their original data for our study. We contacted these authors, and we appreciated their cooperation with us, but unfortunately (albeit understandably) it appears that the original data they collected cannot be located.

choose to fight AIDS in a variety of other ways. But such coincidental changes would be expected to work *against* our conclusion that school condom distribution programs increased birth rates. Indeed, of the anti-HIV procedures considered by schools in the early 1990s, condoms were uniquely singled out for their potential to unintentionally raise fertility rates (Rafferty and Radosh, 2000; Blake et al., 2003). However, we can investigate this concern in several ways. First, we test for changes in the birth rate just before a community adopts condom access in schools. Clear drops in teen pregnancy in treatment communities observed a year or two before condom access would be a signal that these communities may be aggressively fighting AIDS in a variety of ways—for example, with state-recommended or required AIDS education programs. This is a plausible scenario (which, again would likely work against our finding below) as at least some anti-AIDS programs could have predated the early-1990s rise of condom use (Kirby and Coyle, 1997). But consistent evidence that the changes in fertility—and especially increases in fertility—coincide with the years of condom diffusion would be difficult to reconcile with this story. Additionally, in some specifications we control for the birth rate among women age 20-24. These women would have been subject to many of the same public health efforts to address the AIDS epidemic, but should not have been as affected by condom distribution in schools.

III. Estimation

In order to identify the effect of school condom access programs on teen fertility, we employ a framework that exploits within- and across-county variation in teens' exposure to these programs. While the condom-program information is available at the school or district level, the national birth outcome data we will use in our analysis is available at the county level. We therefore calculate the fraction of public-high-school (9th through 12th grade) students in each county in a district implementing a condom program. In those districts where program implementation varied across schools and we can identify the schools gaining a condom program, we code the fraction of students in the county who attended those schools. To avoid concerns about student migration into or out of a public school district in response to a program's

introduction, we use enrollment data from the 1990 Common Core of Education.⁸ Our estimated number of schools affected—484—is based off of the 1990 enrollment data.

We estimate equations of the form:

$$lbr_{ct} = \delta Condom_{ct} + \beta X_{ct} + \theta_c + \theta_t + T_c + \varepsilon_{ct} \quad (1)$$

where lbr_{ct} is the log of total live births to women ages 15 to 19 in county c that were *conceived* in year t , over the population of women 15 to 19 in county c in 1990 (discussed in more detail below). The variable $Condom_{ct}$ measures the fraction of students in a county attending a school with a district condom access program; this will exploit variation in the relative magnitude of diffusion across affected counties as compared to simply using a dummy variable for whether *any* student in a county gains condom access. The matrix X_{ct} includes controls for per-capita county income, per-capita Medicaid payments, and per-capita state unemployment insurance compensation (all from the BEA), total county population in levels and logs, the fraction of the county population that is Hispanic, the fraction white, the fraction black, the fraction under 18, the fraction poor and under 18, and the fraction over age 65 (from decennial censuses and linearly interpolated across years). The terms θ_c and θ_t represent county and year dummies, respectively, and T_c represents a set of county-specific time trends.

Annual county-level birth rates are available from the National Center for Health Statistics' Natality Detail Files, from 1982 to 2000. The data provide records for all births in the 51 U.S. states for every year (including the District of Columbia), with the exception of a few states that report 50% of births prior to 1985.⁹ Each record contains detailed information on the mother, father, and baby. Data used in this study include the mother's age, race, and county of residence. County of residence is only available for counties with populations greater than 100,000, but this covers about 98% of all births in the data. Only one district condom program, in Gadsden county (FL), was dropped because it could not be matched to our birth data. Our estimates include 396 counties in total.

⁸ Seattle is a noteworthy district where several schools began distributing condoms in 1993 and more followed in 1995. In this case we adjust the number of students affected over time. Thus, in the year 1995 we increase the estimated fraction of students in Seattle given condom access using the enrollment information from 1990. There are two other counties (Middlesex, MA; and Worcester, MA), where we make similar adjustments as the number of districts in the county with condom access changed over time.

⁹ For 50% sampling states, each observation is doubled.

Our main interest will be in the births to women ages 15 to 19 that were conceived in a given county and year. The birth certificate data contains information on both month of birth and gestation, which allows us to estimate the month of conception. To estimate the population of women in each county, each year, ages 15 to 19, we use 1990 census population count data. The use of 1990 data allows us to avoid concerns of endogenous migration between counties in response to condom-program adoption. The mean birth rate to women 15 to 19 in our sample is 5.4 births per 100 women. An alternate approach would be to interpolate population counts using decennial census data; doing so produces results that are very close to those shown here. We can also construct (a) the birth rate to women 20 to 24 years old and (b) age-specific birth rates for young women; we show results below using these specifications as well.¹⁰

The key coefficient in equation (1) is δ , which could be interpreted as showing the proportional change in the birth rate from a county going from no students in a county having condom access in schools to all students having condom access in schools. Some specifications will alter (1) to include births to women ages 20 to 24.

One potential concern about estimation strategy is that because our independent variable of interest is the number of students in a county attending a school with condom access, we might be unable to identify any impact of a condom access program if affected districts make up only a small fraction of students in their respective counties. For example, if just one school in a large county adopted a condom access program, the effects of this program might be swamped by the overall population birth rate in the county even if the program had a very large effect. Fortunately, access appears to be suitably widespread in the affected counties. In the 21 counties that saw a policy adopted, the average fraction of students covered was 49.9 percent, and by 2000 there were over 860,000 women aged 15 to 19 in these counties.¹¹

¹⁰ Our focus is on live births given the important (and earlier-mentioned) policy concerns related to teen childbearing. But one might wonder if condom access could impact other outcomes, such as abortion. We know of no reliable data on the universe of abortions to women by age group and county across years. However, if diffusion caused unwanted pregnancies and this led to a rise not only in the birth rate (as we find) but also in abortions, then the results below would represent underestimates of the effect of diffusion on unwanted pregnancies.

¹¹ There were 22 affected districts in 21 counties. New York City Public School District covers five counties, while eight Massachusetts districts are housed within five counties and three school districts are housed within Los Angeles County.

IV. Results

Before presenting estimates of equation (1), we first consider a simplified investigation of our data. For each of our “treatment” counties implementing a school condom program, we construct a birth ratio equal to the births for women ages 15-19 conceived in year t over births to women ages 20-24 conceived in t . Figure 1 shows the average value of this ratio across counties beginning 5 years before condom access occurs in a county through 5 years after access occurs (year zero is thus the year a program was implemented). The figure shows a prominent and persistent break from trend exactly at the time a condom access program is introduced. Going from one period before a condom program to one period after, the figure indicates a county will on average see about 5 additional children born to teenagers for every 100 children born to women ages 20 to 24. The figure shows no evidence of a pre-existing change in relative fertility between these two groups in the years immediately preceding diffusion.

While simple and striking, Figure 1 fails to account for changes in teen births in other communities, and its magnitude is somewhat hard to interpret. Table 2 presents regression results of equation (1) that address these issues. The regressions are weighted by the number of women ages 15 to 19 in a county in 1990. Standard errors are clustered by county. The year a program is introduced is dropped from the regression. The table shows estimates of δ , the coefficient for the variable measuring the fraction of students in a county exposed to a district condom access program.

The baseline result in column 1 indicates that in-school-condom access increases teen births—the coefficient suggests that if a county went from zero access to full access, the birth rate among women 15 to 19 would increase by 9 percent (we discuss the magnitude of this effect more below). The next two columns present strong controls for differential trends across communities, with quadratic (column 2) and cubic (column 3) county-specific trends added as controls. The estimates are robust to these alternatives and the results do not appear to be driven by pre-existing, or differential, trends. Column 4 presents another investigation of this possibility by including a variable measuring diffusion two years in the future as a control. If the inclusion of this control dramatically weakened the “true” diffusion variable, this would indicate that communities adopted condom programs following changes in birth rates (rather than the other way around).

But the main result is comparable to before; the future-diffusion coefficient is small and insignificant, and the regression along with Figure 1 indicate that preemptive increases in birth rates are not driving the estimates.

The last column revisits Figure 1 by including logged births to women 20-24 as a control on the right hand side; the specification is thus a generalization of a regression using, in the spirit of Figure 1, a ratio of birth rates for women 15-19 over women 20-24 (where here the specification allows the coefficient on births to women 20 to 24 to vary, rather than forcing it to be unity). But despite this generalization, and the different nature of identification here (including control communities that were omitted in Figure 1), the implied effect of 0.075 is close to before. The results of Table 2 thus indicate that condom access programs increase teen births, and that these estimates are not driven by pre-existing trends.¹² The coefficients imply that full diffusion would lead to 7 to 9 percent more births to women ages 15 to 19, or a little over four additional births per 1000 women off a mean birth rate of 54 per 1000 ($0.08 \times 54 = 4.32$). Since the average program covered about half of the teenage women in the county, the typical program led to an additional two births per 1,000 women.

Given that some earlier papers have argued that condom access does not increase sexual activity, one might wonder what could drive this result absent an increase in teen sexual behavior. One potential explanation would involve condoms “crowding out” other contraception. This might happen as teens who had been using a method like the Pill switch to the now-freely available condom, or as women who have never used any method (perhaps initiating intercourse for the first time) adopt the condom rather than another method. Ott et al. (2002) document that teenage women rarely combine condoms with other methods of contraception, but instead “trade off between hormonal contraceptives and condoms according to partner type and perceived risk.” As a back-of-the envelope take on whether this channel could plausibly account for these results, suppose that marginal condom use partially crowded out use of oral contraception. Based on a pregnancy risk for condoms of 18% within the first year of typical use (for all women) versus 9% for the Pill, and assuming continued use for a year (although the percent of women continuing condom use is

¹² One could also explore estimates using birth rates in levels, rather than logs. Using levels typically gives qualitatively similar results to those shown here. The baseline estimate in Table 2 in levels yields a coefficient of 0.36 [se = 0.22], implying a proportional effect of about 7 percent, which is comparable to the effects in Table 2.

lower than that for the Pill), then 3 or 4 teenage girls out of 100 substituting the condom for the pill could be sufficient to explain the increase in births we see here. (The risk rates are from Trussell, 2011). This calculation is intentionally simple and ignores other potential channels such as extensive-margin changes in contraception use, couples moving into or out of risky behavior, changes in individuals initiating sexual behavior, crowd out of other non-Pill contraception, or changes in consistency of contraception use over time. But this calculation indicates that our results are potentially consistent with condoms having negligible changes on sexual activity overall and that a modest crowd-out effect could be sufficient to produce the findings here.

Table 3 presents results where births for older women are now modeled not as a control but rather as a dependent variable. The sample used in the first regression in the table includes two observations for each county and year: one for women ages 15 to 19 and one for ages 20 to 24. The regression then includes (a) an interaction of the variable for condom diffusion with a dummy for whether the relevant observation is for the 15-19 year old group, and (b) the interaction of the diffusion variable with a dummy for the 20 to 24 year old group. (A non-interacted group dummy for the 20 to 24 year old group is included in the regression, while a non-interacted diffusion variable would be subsumed by the two new interacted variables.) The results indicate that condom access leads to significant increases in the teen birth rate, but is unrelated to changes in the birth rate for slightly older women; a Wald-test rejects that the coefficients are the same for the two groups of women ($F(1,395)=4.19$, $p=0.0413$). Indeed, for older women the coefficient is of the opposite sign. The next column adds quadratic trends; the results are qualitatively the same as before.

Our data also allow us to aggregate births for individual years of age. The last two columns in Table 3 present results by individual age at birth, for ages 15 to 21. Here, each county now has 7 observations—one for women age 15, one for women age 16, and so on through age 21. The regression includes an interaction of the diffusion variable with each age group and a set of dummies for each age group. As before diffusion measures condom access at the estimated *time of conception*, while the groups are identified by *age at birth*, so that the diffusion coefficient for (e.g.) 19 year olds illustrates how the birth rate for 19-year-old women (who are generally too old to attend high school) changes depending on whether these women had school-condom

access at the time of conception (which would typically be when these women were 18 years old, and potentially still in school.) The results in column 4 show strong and significant effects for the youngest age groups, with the results decreasing with age (although these are proportional effects, and as the average birth rate increases with age the implied effect on births-per-100-women peaks for 16- and 17-year old births and falls thereafter).¹³ Further, the estimates become insignificant (and wrong signed) as women age out of high school; providing evidence that the impacts of school diffusion are limited to women of high-school-going age. The last column shows that these results are similar with quadratic trends (or cubic, although we omit cubic results for brevity).

Given the striking result shown earlier in Figure 1 and the results here, one might wonder how Figure 1 would look using the birth rate for women of a particular age in the numerator of the birth ratio. Appendix Figure 1 in the appendix shows this result; the figure indicates that condom diffusion coincides with clear increases in relative births for women across several age groups, with the most visually striking increases coming to women ages 16 and 17, consistent with the implied levels effects in Table 3.

The appendix also shows two additional results. First, Appendix Table 1 gives results dropping each treatment county one-by-one, showing that the main result is not driven by any particular county. Next, Appendix Figure 2 provides a robustness test where we take the diffusion profile for each of our 22 treatment counties, and, without replacement, randomly assign the treatment profile to another of the 396 counties in the data. We then estimate the diffusion coefficient generated from this random assignment exercise 1,000 times using the baseline specification in Table 2, and provide a histogram of the resulting distribution in the figure. As expected, the distribution is centered around zero and symmetric, and the true coefficient value falls in the 99th percentile of the distribution.

As mentioned earlier, many observers have noted that beyond *whether* condoms were provided, a key issue might be *how* they were provided, with the possibility of counseling often mentioned as a crucial aspect of condom provision. In Table 4 we consider this possibility, including one variable for diffusion among

¹³ For both the first pair of regressions and the last pair in Table 3, results using birth rates give effects similar to (and sometimes slightly larger than) the level effects suggested here, although the estimates are somewhat less precise when quadratic or cubic trends are used.

counties housing any condom program where counseling was mandated, and a variable for diffusion when this was not the case. A number of counties fall into each group.¹⁴ The first column in Table 4 redoes the baseline estimate from Table 2, but interacts the treatment effect with a pair of dummy variables for whether a county housed a condom program with, or without, a counseling program. Column 1 uses linear trends, column 2 adds quadratic trends. The results in the two columns indicate that the teen birth rate significantly increased when condoms were introduced without mandated counseling, but *decreased* when counseling was mandated. The effects are large in magnitude—indeed, somewhat larger (in absolute value) than the baseline effects shown in Table 2 earlier. The bottom of the table includes *F* statistics from Wald tests that the two coefficients on condom access are equal; the hypothesis that the coefficients are equal (that is, the hypothesis that condom access has the same fertility impact in both counseling and non-counseling communities) is unsurprisingly rejected at the 99 percent level. The results indicate that the increase in fertility observed earlier was driven by counties where counseling was not mandated.

The next two columns include the 20-to-24 year old birth rate in each county as a control, so that the effect of condom programs can be identified from variation between each age group and each type of program. As before, counties without counseling see an increase in the teen birth rate (the apparent similarity of the estimates in columns 2 and 3 is an artifact of rounding). Looking at counties with counseling, however, the effect on the teen birth rate is notably smaller than before and is now statistically insignificant. However, despite the lack of precision, the coefficients continue to suggest an economically significant difference in the effects of condom access across the two types of counties, and again a Wald test of the hypothesis of equality of the coefficients is rejected in both column 3 and column 4 at the 98- and 99-percent levels, respectively.

These regressions impose the restriction that the right-hand side controls in the regressions impact teen fertility in similar ways regardless of whether a county offers condoms or not. The last two columns

¹⁴ See Table 1 for how counties are classified. We code Los Angeles as non-counseling as its two non-counseling districts (Los Angeles Unified and Santa Monica Unified) are vastly larger than its counseling district (Culver City Unified). Also, two other counties housing counseling programs also housed programs without counseling (Middlesex and Worcester); simply dropping Los Angeles, Middlesex, and Worcester from the sample yields similar results to those here. Results using levels rather than logs are also qualitatively similar.

investigate the importance of this effect by redoing the estimates in columns 3 and 4, but here, the right-hand-side covariates in X_{ct} from equation (1) are interacted with a dummy for whether a county ever houses a condom-access program. Changes in either coefficient would be evidence that the differential behavior of a particular type of county could in part stem from differential responses to the covariates. In fact, that is not the case. The precision of the estimates (and the Wald statistics) is somewhat lower, which is unsurprising as the set of covariates now includes additional regressors that are mechanically correlated with the treatment variables.¹⁵ But this notwithstanding, the estimates are qualitatively close to before, and again the equality of the coefficients can be rejected in each column (the p value for the test in column 5 is 0.074).¹⁶

Overall, Table 4 clearly shows that the increase in teen fertility is driven by counties where condoms are provided without counseling and that the fertility response is significantly different in these counties compared to counties that offered counseling. In fact, the results suggest that programs with counseling may have seen zero change or perhaps a decline in teen fertility.

V. Conclusions

The effects of improving access to contraception to teenagers on their fertility are theoretically ambiguous. In this paper, we show that the introduction of condom access programs in schools led to an increase in teenage fertility. This result is driven by schools that provided condoms without mandating counseling. The effects are reasonably large in magnitude and contrast with the implications of prior work on access to abortion and oral contraception in the 1960s and 1970s.

These results have implications for recent policy efforts to increase the availability of condoms in schools. One important caveat is that these programs are not solely intended to reduce teenage pregnancy—the main objective is often a reduction in sexually transmitted infections, and there is some evidence that the

¹⁵ One might wonder about whether the additional interacted covariates are themselves typically significant. Of the 22 additional covariates in columns 5 and 6 in the Table (11 in each regression), none are significant in both regressions and in fact only two are statistically significant at the 10 percent level (that is, $p < 0.10$), and none are significant at the 5 percent level ($p < 0.05$) or better.

¹⁶ One could also apply this robustness test to the baseline regressions earlier that do not disaggregate counties by counseling. Doing so produces qualitatively similar results; for example adding these interaction terms to the estimate in column 2 of Table 2 produces a coefficient of 0.091 [se = 0.053]

programs have had success along that dimension (Wretzel et al. 2011).

In addition to being useful for policy, our work informs the discussion about the causes of the notable decline in teenage childbearing during the 1990s. Several explanations for this decline have been proposed, including incarceration (Mechoulan, 2011), welfare reform (Lopoo and DeLeire, 2006), pregnancy prevention messaging campaigns (Martin et al., 2012) or the improving economy (Colen, Geronimus, Phipps, 2006; Arkes, and Klerman, 2009). Some observers, especially Santelli and Melnikas (2010) have noted that the decline in teen fertility coincides with the rise of condom access (and that the more recent rise in teen fertility coincides with a fall in contraceptive use), but their observation goes no further than a discussion of overall trends. Our work suggests that, in fact, condom access did not play a role in the decline in teen fertility in the 1990s. Continued study on this topic we leave for future work.

Finally, we note that a large literature regards teen pregnancy as an injurious outcome, in which case our findings suggest that the normative impacts of condom access programs could be complicated by incidental fertility effects.¹⁷ However, Kearny and Levine (2012) argue that in many cases the detrimental effects of teen pregnancy might be surprisingly small, as when forward-looking teenagers facing limited career and family options decide to have children early. Our positive fertility finding is consistent with several stories; however, if our results are driven by (for example) contraception failure by teens facing good future prospects who are hoping to avoid a teenage pregnancy, then the normative implications of our study could be large even if one accepts recent arguments that teen pregnancy is itself often limited in its detrimental effects.

¹⁷ Research has long found that teenage motherhood is associated with adverse outcomes for women and their children including an increased likelihood of poverty, lower educational attainment, and poor infant health (Furstenberg, 1976; Trussell, 1976). These findings persist (although sometimes appear smaller in magnitude) even when using advanced methodologies to control for differences in background characteristics between teenage mothers and other young women (Ashcraft and Lang 2006; Hoffman, 1998; Geronimus and Korenman, 1992). The fact that women born to teen mothers are more likely to have a teenage birth themselves means that these consequences are transmitted across generations (Kahn and Anderson, 1992).

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Table 1: School Districts with Condom Diffusion Programs

District	County	State	Year	Counseling
Adams County School District 14	Adams	Colorado	1989	Yes
Baltimore City Public School System	Baltimore City	Maryland	1990	Yes
Cambridge Public School District	Middlesex	Massachusetts	1990	Yes
New York City Public Schools	Bronx, Kings, New York, Queens, Richmond	New York	1991	No
Brookline Public School District	Norfolk	Massachusetts	1992	Yes
Culver City Unified School District	Los Angeles	California	1992	Yes
District of Columbia Public Schools	Washington	D.C.	1992	Yes
Falmouth School District	Barnstable	Massachusetts	1992	No
LA-Unified School District	Los Angeles	California	1992	No
Lincoln-Sudbury School District	Middlesex	Massachusetts	1992	No
Newton Public Schools	Middlesex	Massachusetts	1992	Yes
Portland Public Schools	Multnomah	Oregon	1992	Yes
Portsmouth School District	Rockingham	New Hampshire	1992	Yes
Public schools of Northborough and Southborough	Worcester	Massachusetts	1992	Yes
San Francisco Unified School District	San Francisco	California	1992*	Yes
Santa Monica-Malibu Unified School District	Los Angeles	California	1992	No
School District of Philadelphia	Philadelphia	Pennsylvania	1992 [‡]	Yes
Alexandria City Public Schools	Alexandria	Virginia	1993	Yes
Amherst-Pelham Regional Public School District	Hampshire	Massachusetts	1993	No
New Haven Public Schools	New Haven	Connecticut	1993	Yes
Seattle Public School District	King	Washington	1993 [†]	No
Wachusett Regional School District	Worcester	Massachusetts	1993	No

Districts dropped because we could not verify program details (often on which schools adopted programs or had access to clinics with condoms): Chelsea (MA), Dade County (FL), Hatifeld (MA), Jackson Public School District (MS), Martha's Vineyard (MA), Palm Beach (FL), Portsmouth (MA), Somerville (MA), Dallas (TX), Little Rock (AR), and Chicago Public Schools (IL). The last column in the table denotes whether a district mandated counseling when condoms were distributed.

[‡]There was a pilot program with several schools in December of 1991. *1991 for Balboa High, 1992 for others. [†]Condoms were diffused over time to schools in Seattle from 1993 to 1995.

Table 2: Condom Diffusion Programs and Teen Fertility

	Linear Trends (1)	Quadratic Trends (2)	Cubic Trends (3)	Placebo Control (4)	With 20-24 Birth rate (5)
Diffusion	0.09 [0.035]	0.063 [0.032]	0.081 [0.027]	0.076 [0.040]	0.075 [0.030]
Lead of Diffusion	-	-	-	0.005 [0.022]	-
RHS Controls	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
County Trends	Linear	Quadratic	Cubic	Linear	Linear
Observations	7,498	7,498	7,498	6,706	7,498
R-squared	0.982	0.988	0.991	0.983	0.986

The dependent variable is the log of the birth rate among women ages 15 to 19. Diffusion measures the fraction of high-school students in a county exposed to a district-wide condom access program. The mean of the dependent variable (in levels) is 5.4. Among counties with a condom program, the mean level of diffusion is 0.55 and the standard deviation is 0.34. Standard errors are clustered by county. The regressions include 396 counties covering conceptions from 1982 through 2000. Month of conception is estimated by subtracting the reported gestation period from the birth month. Right-hand side controls include per-capita income, per capita Medicaid transfers, state unemployment insurance compensation per capita, total population in levels and logs, the fraction of the population Hispanic, fraction white, fraction under 18, the fraction poor and under 18, and the fraction over age 65. Regressions are weighted by the population of women ages 15 to 19 as of 1990. The year that a condom program is adopted in a county is dropped from the sample. The last column adds the birth rate for women ages 20 to 24, in logs, as a control variable.

Table 3: Results Across Age Groups

	Mean Birth Rate (Levels) (1)	Trends (2)	Quadratic Trends (3)	Trends (4)	Quadratic Trends (5)
Diffusion × 15-to-19 Birth Rate	5.41	0.179 [0.069]	0.146 [0.071]	-	-
Diffusion × 20-to-24 Birth Rate	10.38	-0.05 [0.061]	-0.082 [0.059]	-	-
Diffusion × 15-Year-old-Birth Rate	1.65	-	-	0.254 [0.088]	0.223 [0.092]
Diffusion × 16-Year-old-Birth Rate	3.47	-	-	0.158 [0.067]	0.127 [0.069]
Diffusion × 17-Year-old-Birth Rate	5.50	-	-	0.105 [0.048]	0.074 [0.049]
Diffusion × 18-Year-old-Birth Rate	7.26	-	-	0.061 [0.035]	0.03 [0.034]
Diffusion × 19-Year-old-Birth Rate	8.18	-	-	0.045 [0.063]	0.015 [0.060]
Diffusion × 20-Year-old-Birth Rate	9.07	-	-	-0.017 [0.076]	-0.047 [0.071]
Diffusion × 21-Year-old-Birth Rate	10.00	-	-	-0.038 [0.094]	-0.069 [0.090]
Observations		14,996	14,996	52,464	52,464
R-squared		0.935	0.938	0.925	0.926

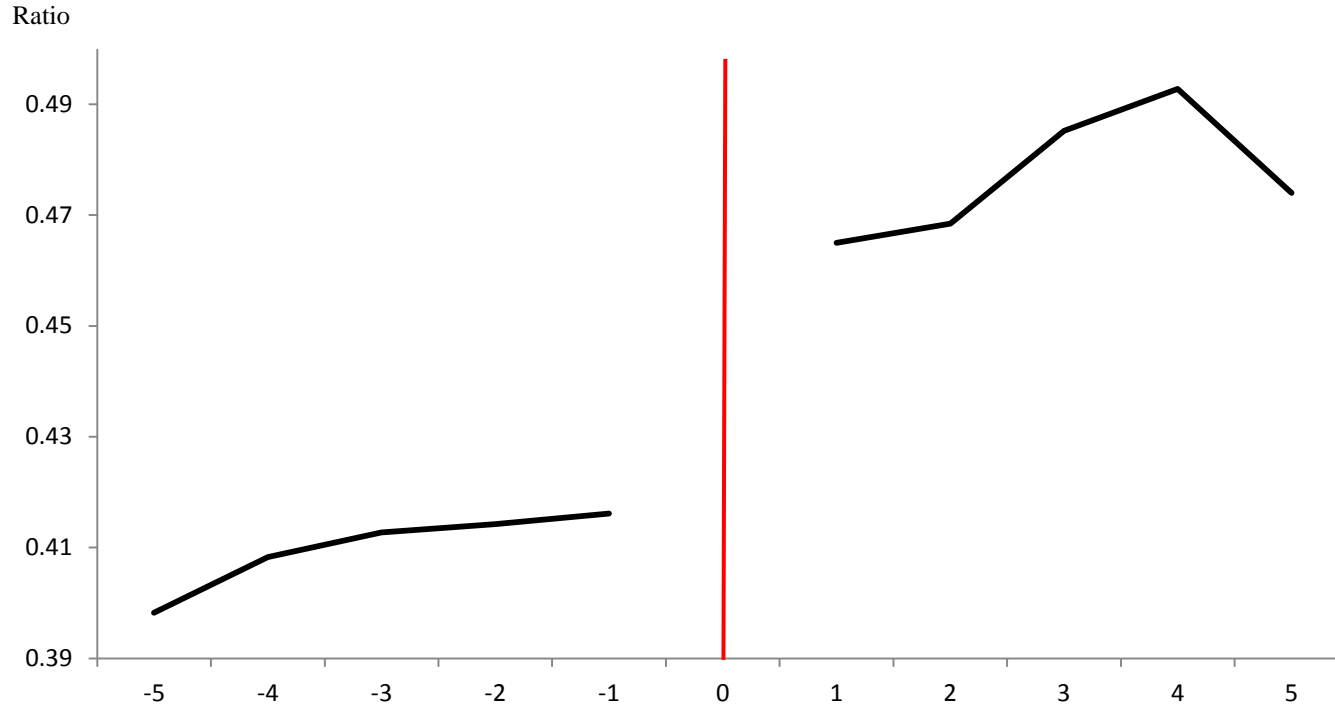
Columns 1 through 5 show coefficients from separate regressions. Column 1 shows the mean birth rate (in levels) for the relevant age group across the 7,498 county-by-year observations in the sample. In columns 2 through 5, the dependent variable is the logged birth rate for women of a particular age. Each regression includes a dummy variable for mother's age. All regressions include the right-hand side controls from earlier tables, along with county fixed effects and year fixed effects. Standard errors clustered by county are in brackets.

Table 4: Counseling and Non-Counseling Diffusion

	Baseline		With 20-24 Birth Rate		With 20-24 Birth Rate & RHS Interactions	
	(1)	(2)	(3)	(4)	(5)	(6)
Diffusion without Counseling	0.128 [0.029]	0.095 [0.030]	0.095 [0.030]	0.091 [0.023]	0.094 [0.042]	0.142 [0.042]
Diffusion with Counseling	-0.160 [0.069]	-0.122 [0.057]	-0.055 [0.056]	-0.063 [0.053]	-0.04 [0.068]	-0.035 [0.063]
RHS Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County Trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Test of Equality $F(1, 395)$	16.19	12.30	5.99	7.46	3.201	7.685
Observations	7,498	7,498	7,498	7,498	7,498	7,498
R-squared	0.982	0.988	0.986	0.989	0.986	0.99

The dependent variable is the log of the birth rate among women ages 15 to 19; in each column both coefficients are from the same regression. All regressions include the right-hand side controls described under Table 2. There are 12 counties with districts implementing counseling programs and there are 9 counties with programs that do not mandate counseling (see text). The F-statistic is from a Wald test of equality of the diffusion effect for the counties with counseling and the counties without counseling; the 99-percent critical value of the $F(1, 395)$ distribution is 6.70 and the 90-percent critical value is 2.72. In columns 3 and 4, the logged birth rate for ages 20-24 is included as a control. In the last two columns, a dummy for being a treatment county is interacted with the right-hand side controls listed in the text under equation (1) and below Table 2.

**Figure 1:
Ratio of Teen Fertility to Fertility of Women 20-24,
in Years Before and After School Distribution Program**



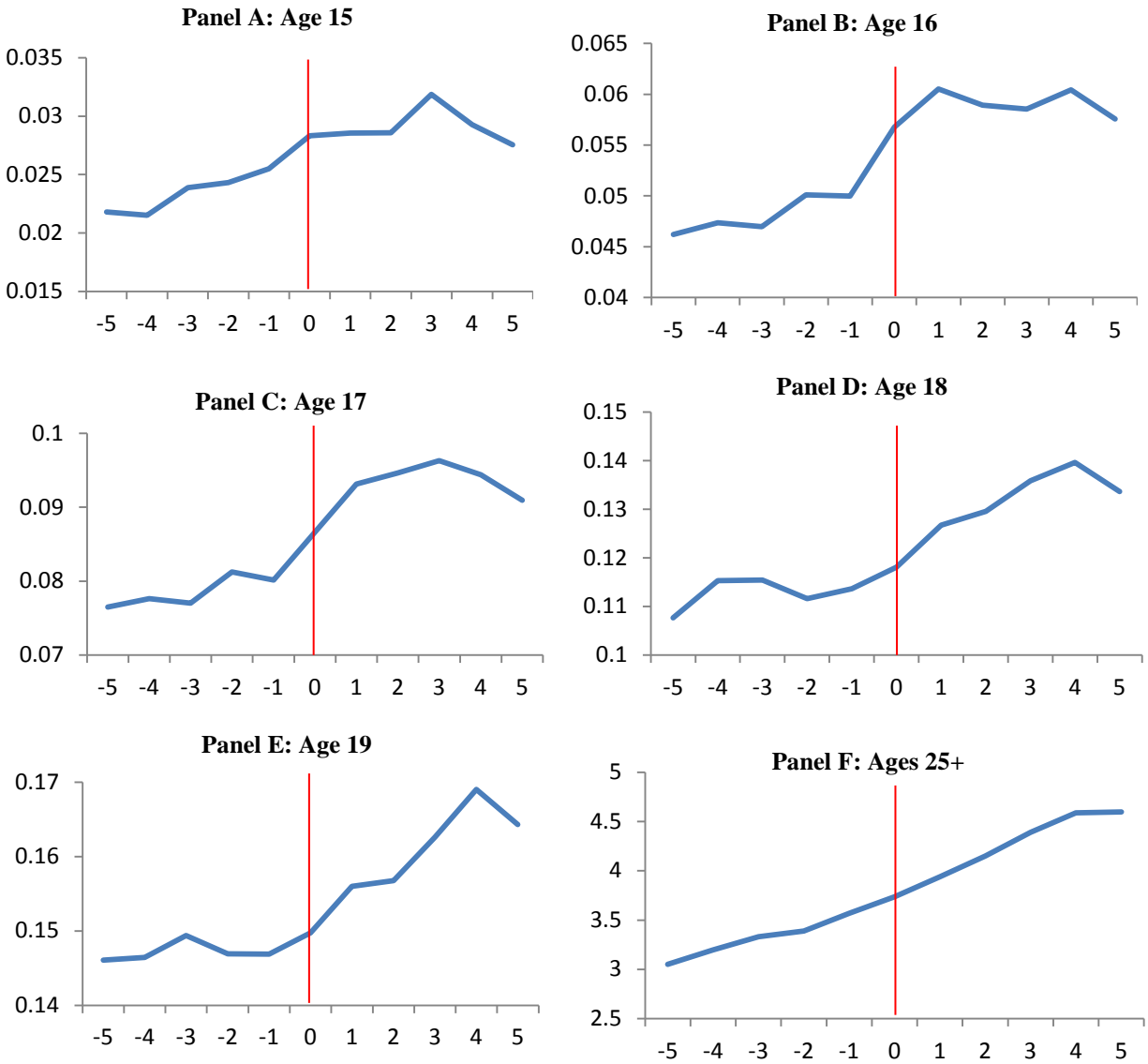
The figure shows relative teen fertility for the counties implementing a school condom program. The ratio is defined as the total number of births conceived by women ages 15 to 19 in the county and year, divided by the total number of births conceived by women ages 20 to 24. This ratio is averaged across the counties annually starting 5 years before diffusion through 5 years after diffusion (year zero is the year a program was implemented).

Appendix Table 1--Dropping Each County

County Excluded:	Trends	County Excluded:	Trends
All Counties (baseline)	0.090 [0.035]	Worcester	0.090 [0.035]
Los Angeles	0.092 [0.043]	Rockingham	0.089 [0.035]
San Francisco	0.096 [0.035]	Bronx	0.096 [0.038]
Adams	0.089 [0.035]	Kings	0.088 [0.046]
New Haven	0.088 [0.035]	New York	0.074 [0.037]
Washington	0.107 [0.029]	Queens	0.068 [0.036]
Baltimore City	0.107 [0.029]	Richmond	0.090 [0.036]
Barnstable	0.09 [0.035]	Multnomah	0.089 [0.035]
Hampshire	0.089 [0.035]	Philadelphia	0.094 [0.034]
Middlesex	0.090 [0.035]	Alexandria	0.091 [0.035]
Norfolk	0.090 [0.035]	King	0.090 [0.035]

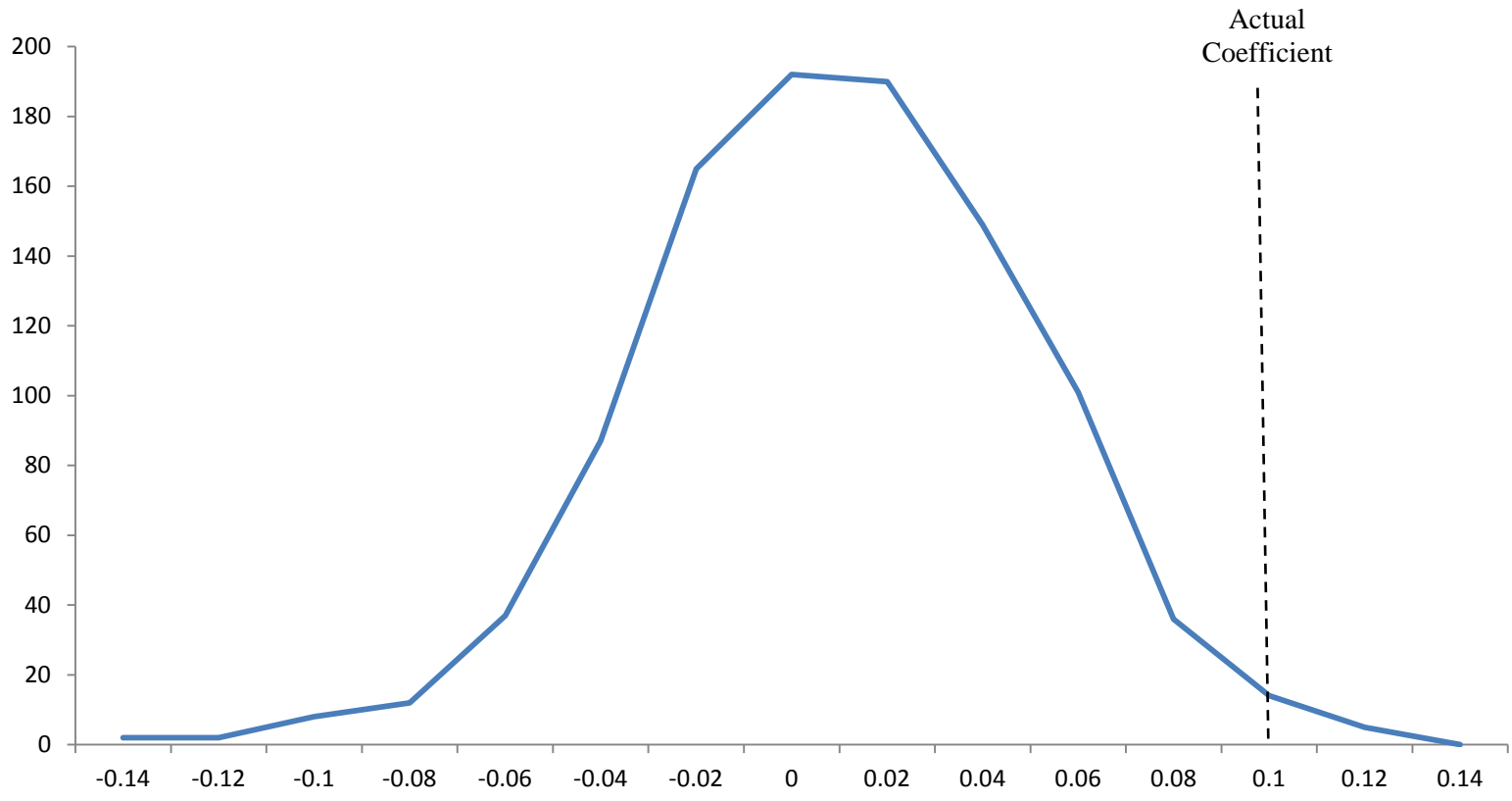
Each Coefficient is from a separate regression and shows the diffusion coefficient estimates from the specification in column 1, Table 2, which a particular treatment county omitted from the sample. The original baseline estimate is given in the first row on the left.

Appendix Figure 1: Birth Ratios by Age



Each panel shows a ratio of total births for a particular age group over births to women ages 20 to 24. Using each county where a condom program was implemented, the average ratio is calculated starting 5 years before implementation through 5 years after implementation. Note the axis range is different for each panel.

**Appendix Figure 2:
Distribution of Betas from Permutation Test**



The picture shows the frequency distribution of 1,000 "placebo" regressions. In each regression, the diffusion profile from 1982 to 2000 for each treatment county was randomly assigned, without replacement, to another county in the sample, the baseline regression was then repeated where the real diffusion variable was replaced by this randomly generated variable. The coefficient from the actual diffusion is in 99th percentile of the above distribution.