PRELIMINARY AND INCOMPLETE,

PLEASE DO NOT CIRCULATE OR CITE WITHOUT PERMISSIOIN

The Incidental Fertility Effects of School Condom Distribution Programs

Kasey S. Buckles, University of Notre Dame and NBER

Daniel M. Hungerman, University of Notre Dame and NBER*

December 2014

Beginning in the early 1990s, hundreds of schools across the United States began to make condoms available on-site to students in an effort to prevent HIV transmission. We examine the fertility effects of these programs. We find that access to condoms in schools leads to an increase in teen fertility; access to condoms increases fertility by about 10 percent, or about 4 extra births per 1,000 teen-age women in a community. These effects are most robust in communities where condoms are provided without mandated counseling.

* We thank Guillermo Roque and Connor Volglewede for excellent research assistance. Email the authors at kbuckles@nd.edu and dhungerm@nd.edu.

I. Introduction

The US teen birthrate is far above the teen birthrate in other industrialized countries, and policy makers have endeavored to find ways of reducing teen fertility. Among different considerations, the distribution of contraception to young people has perhaps been the most contentious. Many studies have explored the benefits of diffusing contraception, and in particular condoms, to young adults, and have concluded that these programs may confer important benefits such as the prevention of sexually transmitted diseases (e.g., Wretzel et. al, 2011; Manlove et. al, 2008). Beyond preventing disease, access to contraception could beneficially impact the longer-term socioeconomic prospects of young adults. Recent work by social scientists on teen oral contraception access in the 1960s and 1970s has suggested that this access may have lowered teen fertility while improving long-term career and family outcomes (Goldin and Katz, 2002; Bailey, 2006; Pantano, 2007; Guldi, 2008; Ananat and Hungerman, 2012).

In turn, this recognition of the potential benefits of contraception access for teens has led to a renewed interest in policies promoting contraception availability, particularly concerning condoms in schools. In November of 2013, the American Academy of Pediatrics released a policy statement arguing that "schools should be considered appropriate sites for the availability of condoms" and the National Institute for Health and Clinical Excellence in the UK has also recently advocated for condom distribution in schools. School districts in Boston, Chicago, and elsewhere have consequently reconsidered the role of condoms in their schools (American Academy of Pediatrics, 2013; Bidgood, 2013; Chicago Tribune, 2014; National Institute for Health and Care Excellence, 2014; Walsh, 2013).

Despite these recent policy events, concerns remain that condoms in schools could have unintended consequences—especially for teen fertility. While prior work shows that pill access lowered teen fertility, some scholars have argued these results are insufficiently identified (Joyce, Tan, and Zhang, 2011) and in any case access to one kind of contraception could have different effects than a different kind of contraception in a different setting. Moreover, work exploring the effects of condom distribution on sexual behavior has been highly inconclusive and has suffered from a number of methodological challenges. A well-cited survey by Kirby (1999) 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.

Even if school condom programs led to greater condom use (as suggested by Furstenberg et al., 1997; and Guttmacher et al., 1997), if school condom access leads to imperfect condom use, "crowds out" more reliable contraception, or creates changes in sexual behavior, then this access could actually lead to higher teen fertility rates over time (Stryker, Samuels, and Smith, 1993). In fact, some studies have suggested that contraception access increases teen fertility (Arcidiacono, Khwaja, and Ouyang, 2012; Paton, 2002). To our knowledge there is no research that provides evidence on how condom access in schools impacts teen fertility. The goal of this paper is to provide such evidence.

To do so, we consider a massive policy intervention which provided condoms to 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; this included the two largest school districts in the United States, NYC Public Schools and Los Angeles Unified School District. We construct a national dataset documenting the introduction of condom access programs across the country. We then match this data to national data on birthrates, 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 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. Unlike the diffusion of oral contraception, the policy event we consider here is too recent to allow us to effectively study long term outcomes such as lifetime fertility. But unlike work on the pill, the contraception diffusion here did not coincide with abortion legalization, was not tied to the age of majority, has a body of work generally attesting to a strong "first stage," and considers a policy intervention that has recently undergone renewed scrutiny.

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 teen-age women. These effects are strongest in communities where condom access was provided *without* mandated counseling. We find suggestive 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). However, our results on diffusion with counseling are somewhat sensitive to specification.

These results are notable given the findings of past work both on pill diffusion and condom diffusion. They suggest that the impacts of pill access on fertility in the 1970s may not necessarily match the impacts of other contraception diffusion, and that the circumstances of contraception access may matter a great deal. These results also suggest extreme 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.

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.[1] 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 that we cannot distinguish.

[1] 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).

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.

Our paper proceeds as follows. The next section discusses condom access programs. Section 3 discusses estimation, section 4 our results, and 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.[2] Commerce City, Colorado, is often regarded as the first district to implement a school-based condom program directly on school grounds; the district did so in 1989.[3] The largest district in the country, New York City (NYC) Public Schools, did so in 1991. The activity in NYC in turn 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, LAUSD, 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. Many districts followed the lead of NYC in adopting condom programs in the early 1990s.

Typically, condoms were provided by an intermediary, and most schools made condoms available through multiple sources.[4] 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

---

[2] 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).

[3] 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); several of these school clinics will be exploited in the analysis below.

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

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 program were provided in high schools, although the data used here include several junior-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 children out if they wished (although research suggests that very few parents–typically just 2 or 3%–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 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 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 1 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 6 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). Wretzel et. al (2011) show that a condom availability program lowered sexually transmitted infection rates in one school district. These studies focus on outcomes different from fertility, however, and essentially all prior work here considers a condom program in a particular school (or a small number of schools) where the population exposed to diffusion is too small to allow a rigorous investigation of fertility. To provide a suitably large-sampled study, we collected data on condom access programs across the country; we describe this 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.[5]

[5] 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), Hatifeld (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

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 1991, 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 condom-access programs. They identified a total of 421 schools with programs, a number quite comparable to the number we identify.[6] 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 has 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 birthrates 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

---

Gadsden County School District (FL) could not be matched to the NIH dataset as the county's population was too small for inclusion in the NIH data (as discussed more below). The counties housing these districts were excluded from the analysis, although fortunately this still leaves us with the vast marjority of students and counties in the country.

[6] 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.

communities and other communities. That is, 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 birthrates 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 choose to fight AIDS in a variety of other ways. But one would likely expect such coincidental changes to work *against* our conclusion that these programs increased birthrates. Additionally, we can investigate this concern by testing for changes in the birthrate just before a community adopts condom access in schools. That is, 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. But consistent evidence that the changes in outcomes coincide with the years of condom diffusion would be harder to reconcile with this story. Finally, in some specifications we control for the birthrate among women age 20-24; these women would likely have been subject to many of the same public health efforts to address the AIDS epidemic.

III. Estimation

In order to identify the effect of school condom-access programs on teen fertility, we employ a differences-in-differences 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.[7] Our estimated number of schools affected–484–is based off of the 1990 enrollment data.

We estimate equations of the form:

$$lbr_{ct} = \delta\, Condom_{ct} + \beta\, X_{ct} + \theta_c + \theta_t + T_t + \varepsilon_{zt} \qquad (1)$$

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 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_t represents a set of county-specific time trends.

Annual county-level birthrates 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.[8] 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 NIH birth

[7] 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.

[8] For 50% sampling states, each observation is doubled.

data. Our estimates include 396 counties in total.

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 birthrate 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 deccenial census data; doing so produces results that are very close to those shown here. We can also constuct (a) the birthrate to women 20 to 24 years old and (b) age-specific birthrates for young women; we show results below using these specifications as well.[9]

The key coefficient in equation (1) is δ, which could be interpreted as showing the proportional change in the birthrate 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 birthrate 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.[10]

---

[9] 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 birthrate (as we find) but also in abortions, then the results below would represent underestimates of the effect of diffusion on unwanted pregnancies.

[10] 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.

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 striking 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 7 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 quite 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 coeficient 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 birthrate 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 birthrates (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 birthrates 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 generilazation of a regression that used, in the spirit of Figure 1, a ratio of birthrates 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 ommited in Figure 1), the implied effect of 0.075 is close to what the simple increase in Figure 1 suggests. The results of Table 2 thus indicate that condom access programs increase teen births, and that full diffusion would lead to 7 to 9 percent more births to women ages 15 to 19, or a little over 3 additional births per 1000 women off a mean birthrate of 54 per 1000 ($0.07 \times 54 = 3.78$), and that these estimates are not driven by pre-existing trends.[11]

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. As a very rough back-of-the envelope take on whether this channel alone might plausibly be large enough to account for these results, suppose that condom use crowded out use of oral contraception. Based on a pregnancy risk for condoms of 21% 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 lower than that for the pill), then if 3 or 4 teenage girls out of 100 substitute the condom for the pill, that would be consistent with the increase in births we see here. (The risk rates are from Trussell, 2011). This calculation is intentionally simple and ignores many 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

---

[11] One could also explore estimates using birthrates 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. We compare our results using logs and levels more below.

potentially consistent with condoms having negligible changes on sexual activity overall, and that even in that circumstance a modest crowd out effect would be sufficient to produce the effects 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 birthrate, but is unrelated to changes in the birthrate for slightly older women; a Wald-test rejects that the coefficients are the same for the two groups of women ($F(1,395)=4.19$, $p=0.0413$). The next column adds quadratic trends; the results are qualitatively the same as before.

Our aggregated NIH data includes births in two age groups—ages 15 to 19 and ages 20 to 24—but it also allows for births at individuals ages from age 15 to age 21. The last two columns in Table 3 present results by individual age at birth. 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 birthrate 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 birthrate increases with age the implied effect on births-per-100-women peaks for 16- and 17-year old births and falls thereafter).[12] Further, the estimates become insignificant (and wrong signed) as women age out of high school; providing clear 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 birthrate 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 results 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 histrogram 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 counties housing any condom program where counseling was mandated, and a variable for diffusion when this was not the case. Importantly, a number of counties fall into each group.[13] The first column in Table 4

---

[12] For both the first pair of regressions and the last pair in Table 3, results using birthrates 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.

[13] Twelve counties here housed programs with counseling: Adams, Alexandria, Baltimore City, Middlesex Multnomah, New Haven, Norfolk, Philadelphia, Rockingham, San Francisco, Washington, and Worcester. Nine counties are non-counseling: Barnstable, Bronx, Hampshire, King, Kings, Los Angeles, New York, Queens, and Richmond. We code Los Angeles as non-counseling as its two non-counseling dristricts (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

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. (The results in the table use quadratic trends; linear trends produce similar estimates.) The results in column 1 indicate that the teen birthrate 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) the the baseline effects shown in Table 2 earlier.

The next column includes both the 15-to-19 year-old birthrate and the 20-to-24 year old birthrate in each county, so that the effect of condom programs can be identified across each age group and each type of program separately. The results add age-by-county dummies (which could not be included in column 1). As before, the effect in counties without counseling shows an increase in the teen birthrate and there is no effect for the 20-24-year-old birthrate. Looking at counties with counseling, however, the effect on the teen birthrate is insignificant. Further, the coefficient for the 20/24 group *is* negative and significant, so that the implied "triple difference" estimate in the counseling case actually becomes positive. The last column presents stronger specifications where each coefficient is estimated in a separate regression. In the first row, the coefficient comes from a regression using only the teen birthrate from (a) control (i.e. no-condom) counties and (b) treatment counties without counseling. The third row includes control counties and only treatment counties with counseling. Rows 2 and 4 match the specifications of the other two rows but use women ages 20 to 24.[14]

The first coefficient in column 4 of Table 4 once more indicates that counties with condom programs lacking mandated counseling saw an increase in teen fertility. The second row once again confirms that this increase is unique to teen births and not births from other young women in the same counties and at the same time. Turning to the counseling counties, the coefficient indicates that programs with counseling

---

Worcester from the sample yields similar results to those here. Results using levels rather than logs are also qualitatively similar.

[14] One could also estimate these specifications on the earlier regressions in Table 3 that compared the overall effects of diffusion between 15-19 year olds to 20-24 year olds. Doing so produces results similar to those in Table 3, and indeed estimates in the spirit of column 4 here were presented (for teens) in Table 2. Redoing the baseline estimates from Table 2 using 20-24 year olds instead produces coefficients of 0.025 [0.041] with linear trends and -0.012 [0.038] with quadratic trends.

saw a decrease in teen fertility, as was the result in the baseline regression. This result is mirrored in the 20-24 year old group, however. Thus, a comparison of the 15/19 and 20/24 coefficients here suggests that condom access with counseling has no effect on teen births relative to the births of young women.

Overall, Table 4, clearly shows that increases in teen fertility observed when condoms are provided without counseling. Further, across all specifications there is *no* significant change in the birthrate for older women in non-counseling counties, so that the result in the non-counseling case *is* robust to using 20 to 24 year olds as a control group. Next, the strongest specification in column 4 suggests that programs with counseling may have seen zero change (relative to births for slightly older women) or perhaps a decline in teen fertility. Either of these outcomes would stand in contrast to the results without counseling. However, this result appears less robust and should be interpreted as suggestive. Despite the lack of robustness, two important takeaways from Table 4 are (a) condom diffusion without counseling coincided with increased teen pregnancy and (b) the impacts of condom diffusion could potentially vary significantly depending upon the circumstances under which condoms are accessed.

Conclusions

In this paper, we show that the introduction of condom access programs in schools coincided with a large increase in teenage fertility. This result is clearest in schools that provided condoms without mandating counseling. The effects are reasonably large in magnitude and contrast with the implications of prior work on condom access and oral contraception's diffusion in the 1970s.

In addition to being useful for policy, our work is also noteworthy in its failure to provide a significant explanatory force for the decline in teenage childbearing during the 1990s. The decline in teenage childbearing in the 1990s is well-known (cf. Buckles and Hungerman, 2008) and several explanations for this decline have been proposed, including incarceration (Mechoulan, 2011), welfare reform (Lopoo and DeLeire, 2006), or the improving economy (Colen, Geronimus, Phipps, 2006; Arkes, and Klerman, 2009). While all of these factors may have played a role, it is important to note that teenage fertility trends changed very early in the 1990s, before the economy improved, incarceration rates changed, or welfare reform was implemented.

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.

References

American Academy of Pediatrics. 2013. "Condom Use by Adolescents," *Pediatrics* 132(5), 973-981.

Ananat, Elizabeth, and Daniel Hungerman. 2012. "The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics." *Review of Economics and Statistics* 94: 37-51.

Arcidiacono, Peter, Ahmed Khwaja and Lijing Ouyang. Forthcoming. "Habit Persistence and Teen Sex: Could Increased Access to Contraception have Unintended Consequences for Teen Pregnancies?" *Journal of Business and Economic Statistics.*

Arkes, Jeremy, and Jacob Klerman. 2009. "Understanding the Link Between the Economy and Teenage Sexual Behavior and Fertility Outcomes." *Journal of Population Economics* 22, 517-536.

Ashcraft, Adam and Kevin Lang. 2006. "The Consequences of Teenage Childbearing." NBER working paper 12485.

Asimov, Nanette. (1992, Apr. 8). Condom program starts May 4. San Francisco Chronicle.

Bailey, Martha J. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *Quarterly Journal of Economics* 121, 289–320.

Banks, Sandy. 1991. "School Officials back Plan to Make Condoms Available," Los Angeles Times, September 27.

Berg, Mary Helen. 1994. "Clinic Considered at Middle School." *Los Angeles Times*, January 9.

Blake, Susan, Rebecca Ledsky, Carol Goodenow, Richard Sawyer, David Lohrmann, and Richard Windsor. 2003. "Condom Availability Programs in Massachusetts High Schools: Relationships with Condom Use and Sexual Behavior." *American Journal of Public Health* 93, 955-962.

Brown, Nancy, Michelle Pennylegion, Pamela Hillard. 1997. "A Process Evaluation of Condom Availability in the Seattle, Washington Public Schools." *Journal of School Health* 67, 336-340.

Buckles, Kasey, and Daniel Hungerman. 2008. "Season of Birth and Later Outcomes: Old Questions, New Answers." NBER working paper 14573.

Chu, Henry. 1993. "Free Condoms Now Just a Fact of Life at High Schools." *Los Angeles Times*, July 6.

Colen, Cynthia G. Arline Geronimus, Maureen Phipps. 2006. "Getting a Piece of the Pie? The Economic Boom of the 1990s and Declining Teen Birth Rates in the United States." *Social Science & Medicine* 63, 1531-1545.

Furstenberg, Frank. 1976. "The Social Consequences of Teenage Parenthood." *Family Planning Perspectives* 8(4), 148-151, 151-164.

Furstenberg, Frank, Lynne Geitz, Julien Teitler and Christopher Weiss. 1997. "Does Condom Availability Make a Difference? An Evaluation of Philadelphia's Health Resource Centers." *Family Planning Perspectives* 29, 123-127.

Geronimus, Arline and Sanders Korenman. 1992. "The Socioeconomic Consequences of Teen Childbearing Reconsidered." *The Quarterly Journal of Economics*, 107(4): 1187-1214.

Goldin, Claudia, and Lawrence F. Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions," *Journal of Political Economy* 110, 730–770.

Goldstein, Amy, and Steve Bates. 1993. "Alexandrea to Fight AIDS with Condoms in School; Grades 9-12 First to be Targeted in VA," The Washington Post, June 19.

Guldi, Melanie. 2008. "Fertility Effects of Abortion and Birth Control Pill Access for Minors," *Demography* 45, 817–827.

Guttmacher Institute. 2010. U.S. Teenage Pregnancies, Births and Abortions: National and State Trends and Trends by Race and Ethnicity.

Guttmacher, Sally, Lisa Lieberman, David Ward, Nick Freudenberg, Alice Radosh, and Don Des Jarlais. 1997. "Condom Availability in New York City Public High Schools: Relationships to Condom Use and Sexual Behavior." *American Journal of Public Health* 87, 1427-1433.

Hoffman, Saul D. 1998. "Teenage Childbearing Is Not So Bad After All…Or Is It? A Review of the New Literature." *Family Planning Perspectives* 30(5), 236-239+243.

Johson, Johan. 1998. A Battle of Values: The Condom Controversy in New York City Public Schools. PhD Dissertation.

Jones, Charisse. 1992. "Distribution of Condoms OKd in L.A. Schools." *Los Angeles Times*, January 22.

Joyce, Ted, Ruoding Tan and Yuxiu Zhan. 2011. "Changes in Birth Rates of Young Women Following Access to the Pill and Abortion in the Early 1970s." Working paper.

Kearny, Melissa, and Phillip Levine. 2012. "Whay is the Teen Birth Rate in the United States so High and Why Does it Matter?" *Journal of Economic Perspectives* 26(2), 141-166.

Kempner, Martha. "Pediatricians Group Advocates for Greater Cndom Access for Teens,"

Kirby, Douglas. 1999. "Reflections on two decades of research on teen sexual behavior and pregnancy." *Journal of School Health*, *69*(3), 89-94.

Kirby, Douglas. 2002. "The Impacts of Schools and School Programs Upon Adolescent Sexual Behavior," in *The Journal of Sex Research* 39(1), 27-33.

Kirby, Douglas, Nancy Brener, Nancy Brown, Nancy Peterfreund, Pamela Hillard, and Ron Harrist.

1999. "The Impact of Condom Distribution in Seattle Schools on Sexual Behavior and Condom Use." *American Journal of Public Health* 89, 182-187.

Kirby, Douglas, and Nancy Brown. 1996. "Condom Availability Programs in U.S. Schools." *Family Planning Perspectives* 28, 196-202.

Leitman, Robert, Erich Kramer, and Humphrey Taylor. 1993. "A Survey of Condom Programs," in: Sarah Samuels an dMark Smith (eds.) Condoms in the Schools. Menlo Park CA: Kaiser Family Foundation.

Lewin, Tamar. 1991. "Studies on Teen-Age Sex Cloud Condom Debate," The New York Times, February 8.

Lopoo, Leanard M., and Thomas DeLeire. 2006. "Did Welfare Reform Influence the Fertility of Young Teens?" *Journal of Policy Analysis and Management* 25, 275-298.

Lovenheim, Michael, Randall Reback, and Leigh Wedenoja. 2014. "How Does Access to Health Care Affect Health and Education? Evidence from School-based Health Center Openings," Working paper.

Manlove, Jennifer, Erum Ikramullah, and Elizabeth Terry-Humen. 2008. "Condom Use and Consistency Among Male Adolescents in the United States." *Journal of Adolescent Health* 43, 325-333.

Martinez-Donate, Melbourne Hovell, Jennifer Zellner, Carol Sipan, Elaine Blumberg, and Claudia Carrizosa. 2004. "Evaluation of Two School Based HIV Prevention Interventions in the Border City of Tijuana, Mexico." *The Journal of Sex Research* 41, 267-278.

Mechoulan, Stéphane. 2011. "The External Effects of Black Male Incarceration on Black Females." *Journal of Labor Economics* 29, 1-35.

National Institute for Health and Care Excellence. 2014. " Contraceptive Services with a Focus on Young People up to the Age of 25," NICE public health guidance 51, accessed on 12/12/2014 at guidance.nice.org.uk/ph51 .

Nealon, Patricia. 1993. "Condoms: A Fact of Life in Many Mass. Schools," Boston Globe, March 5.

Pantano, Juan. 2007. "Unwanted Fertility, Contraceptive Technology, and Crime: Exploiting a Natural Experiment in Access to the Pill." California Center for Population Research working paper no. 028-07

Paton, David. 2002. "The Economics of Family Planning and Underage Conceptions." *Journal of Health Economics* 21(2), 207-225.

Samuels, Sarah and Mark Smith, eds. 1993. Condoms in the Schools. Menlo Park CA: Kaiser Family Foundation.

Santelli, John and Andrea Melnikas. 2010. "Teen Fertility in Transition: Recent and Historic Trends in the United States." *Annnual Review of Public Health* 31, 371-383.

Schuster, Mark, Robert Bell, Sandra Berry, and David Kanouse. 1998. "Impact of a High School Condom Availability Program on Sexual Attitudes and Behaviors," Family Planning Perspectives 30(2), 67-88.

Stryker, Jeff, Sarah Samuels, and Mark Smith. 1993. "Executive Summary." in Samuels and Smith. eds., Condoms in the Schools. Menlo Park CA: Kaiser Family Foundation.

Taylor, Paul. 1991. "High School Clinic: Case Study in Sexual risk; In Rural Florida Town, Impact of Offering Free Condoms to Teenagers Remains Unclear," The Washington Post, March 10.

Tillman, Leeroy. 1992. "Washington, D.C., to Give Condoms in Schools to Fight AIDS," Associated Press Report, May 12.

Trussell, James. 1976. "Economic Consequences of Teenage Childbearing." Family Planning Perspectives, 8(4), 184-190.

Trussel, James, Anjana Lalla, Quan Doan, Eileen Reyes, Lionel Pinto, and Joseph Gricar. 2009. "Cost Effectiveness of Contraceptives in the United States," *Contraception* 79(1), 5-14.

Trussell, James. 2011. "Contraceptive Efficacy." In Hatcher, Trussell, Nelson, Cates, Kowal, Policar (eds) *Contraceptive Technology: Twentieth Revised Edition*. New York NY: Ardent Media.

Wretzel, Sharon, Paul Visintainer, and Laura Pinkston Koenigs. 2011. "Condom Availability Program in an Inner City Public School: Effect on the Rates of Gonorrhea and Chlamydia Infection," Journal of Adolescent Health 49, 324-326.

Table 1: Districts with Condom Diffusion Programs

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

Districts dropped because we could not 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: Diffusion Condom Programs and Teen Fertility

	Linear Trends (1)	Quadratic Trends (2)	Cubic Trends (3)	Placebo Control (4)	With 20-24 Birthrate (5)
Diffusion	0.09	0.063	0.081	0.076	0.075
	[0.035]	[0.032]	[0.027]	[0.040]	[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	Yes	Yes	Yes	Yes	Yes
Quad Trends	No	Yes	Yes	No	No
3^{rd} Order Trends	No	No	Yes	No	No
Obs	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 birthrate among women ages 15 to19. 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. Time of conception is estimated by XXXXX. Right-hand side controls include per-capital income, percapita Medicaid transfers, state unemployment insurance compensation per capita, total population in levels and logs, the fraction of the population Hispanic, fraction white, fraction under18, 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 birthrate 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 Birthrate	5.41	0.179	0.146	-	-
		[0.069]	[0.071]		
Diffusion × 20-to-24 Birthrate	10.38	-0.05	-0.082	-	-
		[0.061]	[0.059]		
Diffusion × 15-Year-old-Birthrate	1.65	-	-	0.254	0.223
				[0.088]	[0.092]
Diffusion × 16-Year-old-Birthrate	3.47	-	-	0.158	0.127
				[0.067]	[0.069]
Diffusion × 17-Year-old-Birthrate	5.5	-	-	0.105	0.074
				[0.048]	[0.049]
Diffusion × 18-Year-old-Birthrate	7.26	-	-	0.061	0.03
				[0.035]	[0.034]
Diffusion × 19-Year-old-Birthrate	8.18	-	-	0.045	0.015
				[0.063]	[0.060]
Diffusion × 20-Year-old-Birthrate	9.07	-	-	-0.017	-0.047
				[0.076]	[0.071]
Diffusion × 21-Year-old-Birthrate	10	-	-	-0.038	-0.069
				[0.094]	[0.090]
Obs		14,996	14,996	52,464	52,464
R-squared		0.935	0.938	0.925	0.926

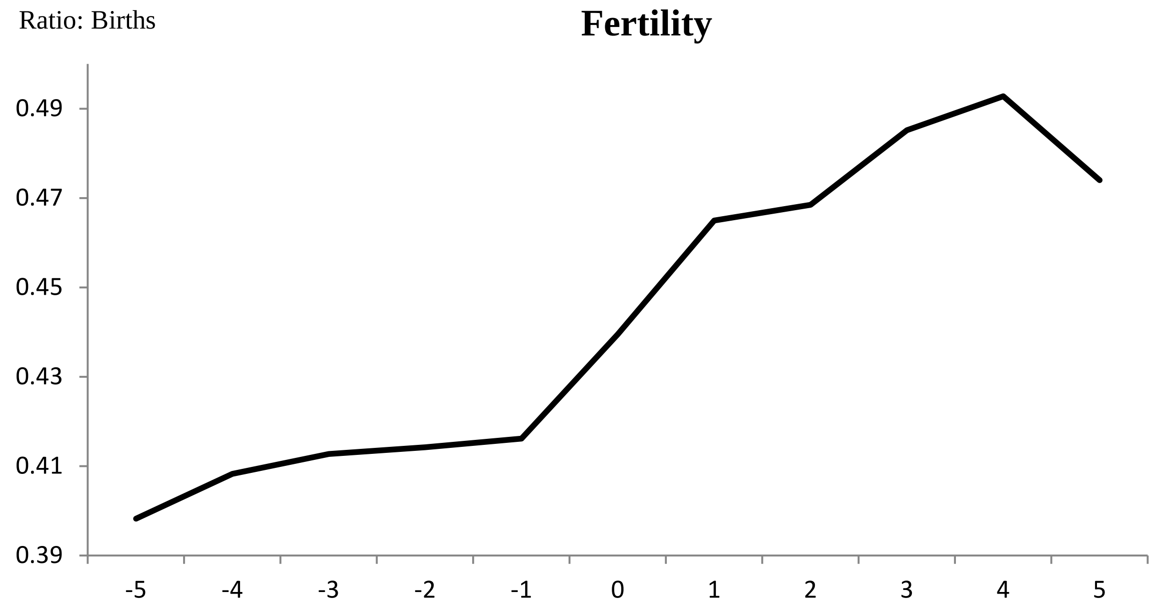
Columns 1through 5 show coefficients from separate regressions. Column 1 shows the mean birthrate (in levels) for the relevant age group across the 7,498 county-by-year observations in the sample. In columns 2 through 5, the dependent variable is the logged birthrate for women of a particular age. Each 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 (1)	Age-Group-By-County FEs (2)	Separate Regressions (3)
Diffusion without Counseling × 15-to-19-Year-old-Birthrate	0.095 [0.030]	0.093 [0.061]	0.09 [0.025]
Diffusion with Counseling × 15-to-19-Year-old-Birthrate	-0.122 [0.057]	-0.026 [0.059]	-0.117 [0.051]
Diffusion without Counseling × 20-to-24-Year-old-Birthrate	-	0.008 [0.027]	0.007 [0.044]
Diffusion with Counseling × 20-to-24-Year-old-Birthrate	-	-0.205 [0.069]	-0.107 [0.051]
RHS Controls	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Age-by-County FEs	No	Yes	NA
County FEs	Yes	Yes	Yes
County Trends	Yes	Yes	Yes
Obs	7,498	14,996	14,996
R-squared	0.988	0.983	0.936

The dependent variable in column 1 is the log of the birthrate among women ages 15 to19; both coefficients are from the same regression. In columns 2 and 3, the logged birth rate for two groups (ages 15-19 and ages 20-24) is included for each county each year; column 3 reports allows county fixed effects to vary by age group. For the final column, each coefficient comes from a separate regression. The first coefficient is from a regression using only 15-19 year olds, all control counties, and only treatment counties without counseling. The next coefficient uses only treatment counties without counseling, and the final two coefficients are from regressions on 20-24 year olds. All regressions include the righthand 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).

Figure 1:
Years Pre/Post Diffusion and the Relative Rate of Teen Fertility



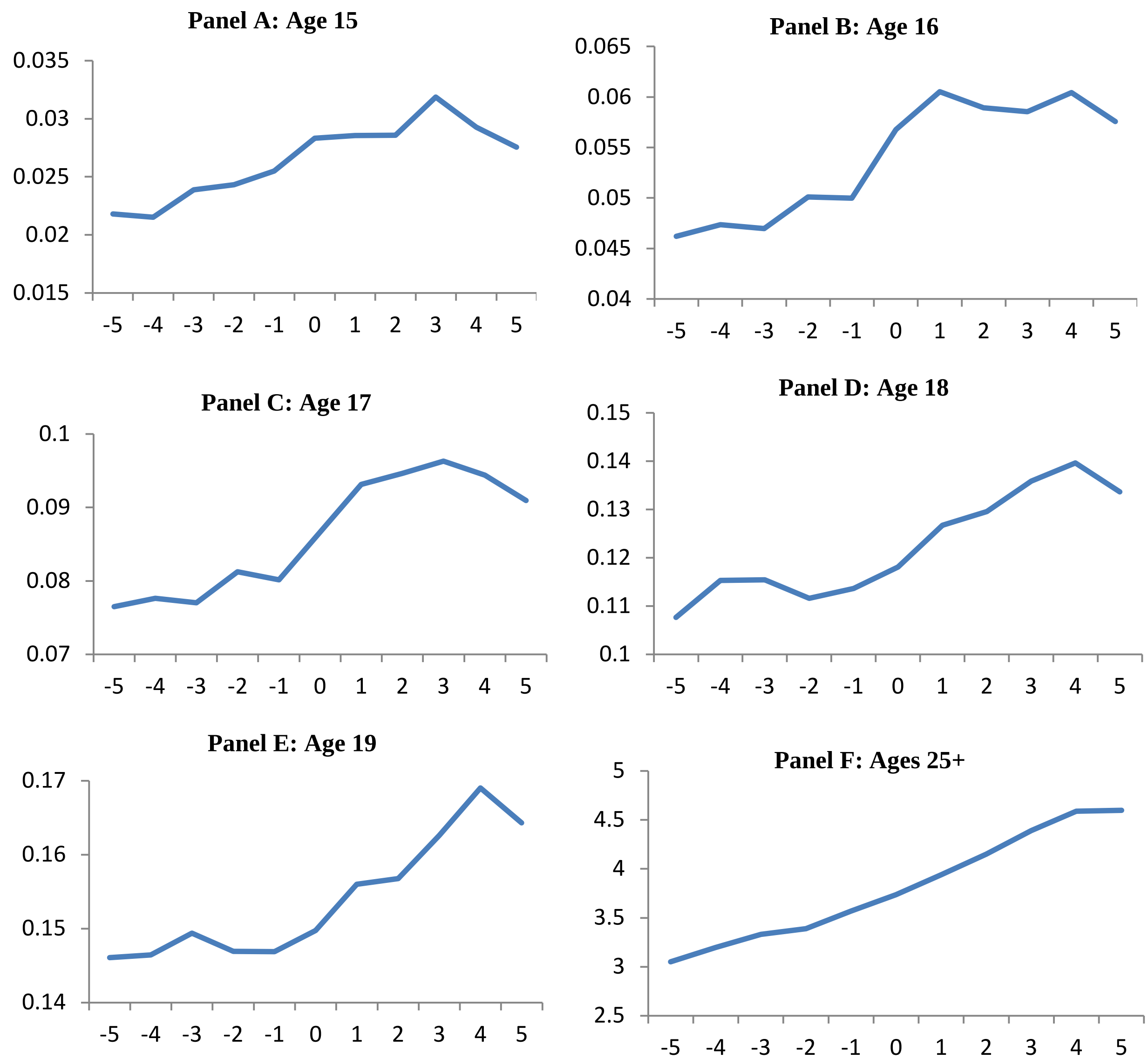
The figure shows relative teen fertiliy for the counties implementing a school condom program. The figure uses a birth ratio of teen fertilty which is, for each county and each year, the total number of births to women ages 15 to 19 over the total number of biths to women ages 20 to 24. This ratio is

Appendix Table 1--Dropping Each County

County Excluded:	Trends	County Excluded:	Trends
All Counties	0.09	No Worcester	0.09
	[0.035]		[0.035]
No Los Angeles	0.092	No Rockingham	0.089
	[0.043]		[0.035]
No San Francisco	0.096	No Bronx	0.096
	[0.035]		[0.038]
No Adams	0.089	No Kings	0.088
	[0.035]		[0.046]
No New Haven	0.088	No New York	0.074
	[0.035]		[0.037]
No Washington	0.107	No Queens	0.068
	[0.029]		[0.036]
No Baltimore City	0.107	No Richmond	0.09
	[0.029]		[0.036]
No Barnstable	0.09	No Multnomah	0.089
	[0.035]		[0.035]
No Hampshire	0.089	No Philadelphia	0.094
	[0.035]		[0.034]
No Middlesex	0.09	No Alexandria	0.091
	[0.035]		[0.035]
No Norfolk	0.09	No King	0.09
	[0.035]		[0.035]

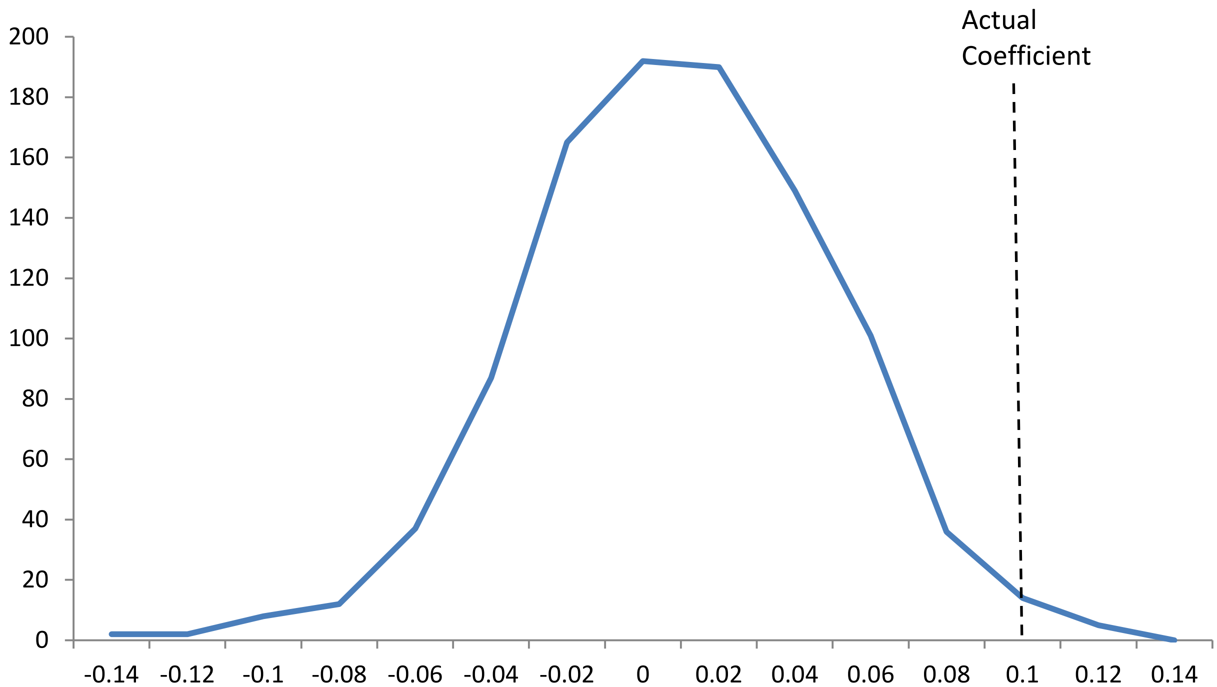
Each Coefficient is from a separate regression and shows the diffusion coefficent estimates from the specification in column 1, Table 1, which a particular treatment county ommitted from the sample. The original baseline estimate is given in the first row on the left.

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 distrution 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.