The Impact of Subsidized Birth Control for College Women: Evidence from the Deficit Reduction Act

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Abstract

This paper uses a unique natural experiment to investigate the sensitivity of American college women's contraceptive choice to the price of oral birth control and the importance of its use on educational and health outcomes. With the passage of the Deficit Reduction Act of 2005, Congress inadvertently and unexpectedly increased the effective price of birth control pills ("the Pill") at college health centers more than three-fold, from $5 to $10 a month to between $30 to $50 a month. Using quasi-difference-in-difference and fixed effects methodologies and an intention-to-treat (ITT) design with two different data sets, we find that this policy change reduced use of the Pill by at least 1 to 1.8 percentage points, or 2 to 4 percent, among college women, on average. For college women who lacked health insurance or carried large credit card balances, the decline was two to three times as large. Women who lack insurance and have sex infrequently appear to substitute toward emergency contraception; uninsured women who are frequent sex participants appear to substitute toward non-prescription forms of birth control. Additionally, we find small but significant decreases in frequency of intercourse and the number of sex partners, suggesting that some women may be substituting away from sexual behavior in general.

Introduction

The birth control Pill (the Pill) is by far the most popular form on contraception on college campuses in the United States. Nearly forty percent of college women use the Pill to prevent pregnancy according to the American College Health Association. Of those women using any form of contraception, sixty percent use the Pill. College health centers go to great lengths to make sure that students have access to reasonably priced (or free) contraceptives; they do outreach to students to make sure they are aware of their birth control options, provide thousands of free condoms, free or reduced cost exams for birth control prescriptions, and, until recently, reduced price birth control pills. College students have generally been viewed as a population that is vulnerable to accidental pregnancy and STIs. Pharmaceutical companies, prior to 2007, sold prescription contraceptives to college health clinics at deep discounts in order to attract brand loyalty among young consumers, and everyone benefited; the pharmaceutical companies cultivated a loyal customer base, the students got inexpensive birth control, and the schools made a bit of revenue by adding a small markup on the deeply reduced price to help support other health initiatives around campus (Wasley 2007). This beneficial relationship came to an abrupt halt on January 1, 2007 when the Deficit Reduction Act of 2005 went into effect, eliminating the incentive for pharmaceutical companies to sell drugs below retail price to all but a very small number of organizations, not including college health clinics. As a result, the price of birth control jumped from $5 to $10 a month to $30 to $50 on college campuses around the country. College health professionals worried that the change would lead to fewer women using birth control pills, fewer well-woman visits that could have lasting impacts on health, more use of emergency contraception, and more unintended pregnancies (Chaker 2007, Wasley 2007).

The population of college women is of particular policy interest, as many studies have shown that the availability of effective birth control during college has lasting impacts on women's human capital investments, career choices and wages (Goldin and Katz 2002; Bailey 2006; Bailey, Hershbein and Miller 2010).

This paper uses the unique natural experiment provided by the Deficit Reduction Act of 2005 to examine the effects of a dramatic increase in the price of prescription contraception on

college women's birth control choices and sexual behavior. We find we find that this policy change reduced use of the Pill by at least 1 to 1.8 percentage points, or 2 to 4 percent, among college women, on average. For college women who lacked health insurance or carried large credit card balances, the decline was two to three times as large. Women who lack insurance and have sex infrequently appear to substitute toward emergency contraception; uninsured women who are frequent sex participants appear to substitute toward non-prescription forms of birth control. Additionally, we find small but significant decreases in frequency of intercourse and the number of sex partners, suggesting that some women may be substituting away from sexual behavior in general.

Policy Background

President Bush signed the Deficit Reduction Act of 2005 (DRA) into law on February 8, 2006. The law, which went into effect January 1, 2007, was intended to reduce overall spending on Medicaid by reducing government payments to these agencies and crack down on Medicaid rebate claims fraud. One of the provisions of the law (Title VI, Subtitle A, Chapter 1, section 6001, part (d)) reduced the list of organizations that could receive "nominally" priced drugs from pharmaceutical companies.[1] "Nominal" pricing allows the pharmaceutical companies to provide low cost drugs to clinics and organizations serving low-income populations without decreasing the "Best Price" paid by Medicare and Medicaid[2]. Prior to the DRA, college health clinics were able to purchase contraceptives at the significantly lower nominal price, but since they were not explicitly named as eligible for nominal pricing in the Act, starting in January 2007 they were required to pay the full wholesale price for all drugs. This had the effect of raising the price of contraceptives on college campuses from around $5-$10 for a month's supply to $40 or $50 (Rooney 2007). Most college clinics only heard of the change through an American College Health Association bulletin in

[1] As part of the earlier Omnibus Budget Reconciliation Act, certain organizations including non-profit health centers at college campuses, could buy prescription medications from pharmaceutical companies at a "nominal price," with Medicaid paying the difference.

[2] The "best price" is generally the lowest price offered to any commercial (non-governmental) customer, excluding nominal prices. This price is used to determine the manufacturer rebates owed to state Medicare and Medicaid agencies.

December, 2006, less than a month before the law went into effect. Despite the late notice, many schools were still able to stockpile reduced price contraceptives to continue to sell to their students until supplies ran out. The University of Michigan, for example, was able to purchase enough to last until mid-2008, though other schools ran out much sooner (Rooney 2007). The effect of the act went beyond just birth control prices; the pharmacies at several college campuses, including Duke and Florida State, were forced to close after losing the revenue from their small markup over the nominal price on oral contraceptives (Cho and Reddy 2009).

Importantly, this provision was both accidental and unexpected. Many legislators have said the omission of college health clinics was unintentional and a result of last minute word changes in conference committee (Davey 2007). Organizations that promote the health of college students and access to family planning such as the American College Health Association and Planned Parenthood were caught unaware and began lobbying efforts to reverse the provision only several months *after* it had gone into effect (Rooney 2007, Davey 2007). They were not successful until the "Affordable Birth Control Act" was introduced into the 2009 Omnibus Spending Bill and took effect in March of 2010. Even so, this law only partially undoes the DRA provision in *allowing* but not *requirin*g drug manufacturers to sell their products to student health centers and safety-net clinics at "nominal" prices. Consequently, prices for prescription drugs, including hormonal birth control, at college health centers were elevated for approximately a three-year period from 2007 to 2010.

The Deficit Reduction Act of 2005 provides a nearly ideal natural experiment through which to investigate college women's price sensitivity to the Pill, as it caused a large, exogenous increase in the cost of the Pill at campus health clinics. The magnitude of the shock, a greater than three-fold increase in price, is an important aspect of our research design for two reasons. First, much of the extant literature on the price elasticity of contraception, described below, has tended to find small or zero estimates for moderate price changes, so a large price shock might be necessary to find a sizable effect empirically. Second, because we do not directly observe in our data *where* college women obtain their method of birth control, our empirical design is *intention to treat* in that the price increase is expected to affect only a subset of women in our sample (those that

receive prescription birth control from college health centers). A larger price change increases the power of our research design.

Furthermore, because the price change was exogenous, it can be used as an instrument to assess the impact of Pill use among college women on a variety of social and educational outcomes, including substitution to other, possibly less effective forms of birth control, and changes in sexual behavior.[3]

The price change is clearly evident in the Medical Expenditure Panel Survey (MEPS), which collects detailed information from households about their medical expenses, including prescription birth control. Figure I provides the mean and median prices paid for prescription birth control, excluding emergency contraception, from 2002 to 2008 by student status. Though the prices track very closely until 2007, they diverge sharply in 2008, when the price students pay overtakes that of non-students.

Literature Review

While the leading method of contraception among young women in the United States, especially among women attending college, is the birth control pill[4], few studies have examined the responsiveness of Pill use to changes in price in the U.S. setting. In part, this is because causal identification is typically difficult, with little source of exogenous change. The literature consists mainly of studies conducted in developing countries. Some studies take a non-experimental, cross-sectional approach (Akin and Schwartz 1988, Schwartz et al. 1989) and find use of contraceptives, including the Pill, to be relatively insensitive to small changes in price. Jensen et al. (1994) finds similarly inelastic demand for contraceptives for the overall sample in Indonesia, but substantially more elastic demand among poor households, implying that price elasticities likely vary by income

[3] As Angrist and Imbens (1995) note, we are really estimating a local average treatment effect (LATE), or the average impact of change in Pill use among women whose Pill use is affected by the price change.

[4] According to the 2006-2008 National Survey of Family Growth, 21 percent of American women aged 15 to 24 were currently using the Pill; of women this age who were using any form of contraception, 50 percent used the Pill (Mosher and Jones 2010). In our sample of college women, 40 percent were currently using the Pill or had the last time they had intercourse, and of those using any form of contraception, 61 percent used the Pill.

and credit constraints. All these studies, however, rely on self-reported price estimates and include travel and time costs and so may not reflect the effects of purely monetary price changes.

Others studies use an experimental approach, randomly altering the price of condoms and oral contraceptives (Gadalla 1980, Lewis 1986, Cernada 1982, Ciszewski and Harvey 1995, Harvey 1994, among others), and have generally found contraceptives to be relatively price inelastic. Ciszewski and Harvey (1995), in particular, looked at the effects of an increase in the prices of social marketing condoms and oral contraceptives in Bangladesh and found significant decreases in those contraceptive sales (both condoms and the Pill), which they used to imply that overall demand for contraceptives had decreased. In their review, however, Janowitz and Bratt (1999) found increases in overall demand for both condoms and the Pill in the treatment regions following the price increase, suggesting that people had simply substituted other brands of contraceptives in place of the more expensive social marketing condoms and Pills. Very few studies have attempted to assess the substitution between different contraceptive choices following price changes; this constitutes a major gap in the literature, which this paper begins to address.

A number of other studies have looked at how legal access to the Pill in the late 1960s and early 1970s changed American women's fertility, human capital investment, and labor force decisions (Goldin and Katz 2002, Bailey 2006, Bailey 2010), and have shown large, statistically significant differences in Pill use between states where access was legal and states where it was not. Since changes in the legality of the Pill can be considered changes in the economic cost of use[5], though not necessarily a monetary cost increase, these findings suggest that women who face a higher cost to obtain the Pill are substantially less likely to use it.

In a more recent American context, Kearney and Levine (2009) examine the effect of increasing the income eligibility threshold for Medicaid services, which effectively reduces the price of family planning services for the newly covered women, on contraceptive use. They find that new Medicaid eligibility reduced the probability of not using birth control at last intercourse

[5] Such an economic cost could comprise the psychic cost of breaking the law, stigma for opposing a social norm, or search costs in finding a provider source.

by 5 percentage points among non-teens. However, while the eligibility change did reduce the price of contraceptives, it also reduced the price of health services more generally, making it difficult to disentangle how much of the effect is due to a price change and how much is due to greater access or knowledge about contraception. In contrast, the source of identification in this paper is a price change for a specific form of birth control—prescription-based oral birth control—that is independent of family planning or health services.

Additionally, Levine (2000) provides evidence that American teenagers change both their sexual behavior and birth control choices in response to changes in the price of pregnancy, measured by labor market conditions, AIDS incidence, welfare benefits and abortion restrictions. While changes in the price of contraception are not the same as changes in the price of pregnancy, this suggests that the response of college women to the exogenous change in oral contraceptive prices might be to alter their sexual habits in addition to or in place of changing their contraceptive strategy.

Theoretical Model

To assess the impact of a change in the price of oral contraceptives on women's sexual behavior and contraceptive choice, it is useful to first construct a model. We assume that a woman has utility over a composite consumption good, x, and her risk of pregnancy, P, which is presumed to be a bad. She has control over P by choosing a birth control option $k = 1, 2, 3, \ldots, m \in K$, where K includes both abstinence and no birth control (unprotected sex).[6] Each form of birth control has a price, p_k, with the price of methods such as abstinence, withdrawal, or no birth control having a price of zero. In addition to the price, each birth control method also carries a vector of characteristics, w_k, that directly affects utility; for example, hormonal birth control may cause unpleasant side effects, condoms may interfere with sexual pleasure, and abstinence may have emotional costs.

The woman's problem can be expressed as:

[6] For simplicity, we assume P is non-stochastic, though allowing uncertainty does not qualitatively affect the inferences of the model.

$$Max_k\ U(x, P(k), w_k) \quad s.t. \quad x + p_k \leq M, \tag{1}$$

where *M* represents income. Substituting for *x* using the budget constraint yields

$$Max_k\ U(M - p_k, P(k), w_k), \tag{2}$$

which shows the tradeoff between more effective, and presumably more expensive, birth control and reduced consumption of the consumption good. Since *k* is discrete, a woman chooses birth control option k^* such that

$$U(M - p_{k^*}, P(k^*), w_{k^*}) \geq U(M - p_k, P(k), w_k) \quad \forall k \in K. \tag{3}$$

This model has several characteristics of note. First, if w_k varies across women according to taste and biology, it is possible for women with the same income, facing the same prices to optimally choose different forms of birth control. Second, holding w_k constant, the price sensitivity of the optimal birth control choice depends on the marginal utility of the consumption good and income. In particular, if $U(\cdot)$ is concave in *x*, which is a standard assumption, then higher income women should be less sensitive to price than lower income women.

To see this point, suppose the price of one form of birth control increases; that is, p_k rises for some $k \in K$. How does this affect optimal choice? For women with $k^* \neq k$ nothing happens; a price increase for an inframarginal choice is inconsequential. For women with $k^* = k$, it was originally the case that

$$U\left(M - p_k^1, P(k), w_j\right) \geq U\left(M - p_j, P(j), w_j\right)\ \forall\, j \in K.$$

After the price change, two cases are possible; either

$$U\left(M - p_k^2, P(k), w_j\right) > U\left(M - p_j, P(j), w_j\right)\ \forall\, j \in K, \text{ or}$$

$$U\left(M - p_k^2, P(k), w_j\right) < U\left(M - p_j, P(j), w_j\right) \text{ for some } j \in K.$$

In the first case, *k* is still the utility optimizing choice, but there will be a utility loss from less consumption of *x*. In the second case, the price change induces a change in the optimal choice from option *k* to option *j*. In fact, a woman will switch options if and only if

$$F \equiv U\left(M - p_j, P(j), w_j\right) - U(M - p_k^2, P(k), w_k) > 0 \text{ for some } j \neq k.$$

Differentiating *F* with respect to p_k results in $U_x(\cdot)$, the marginal utility with respect to *x*. Unsurprisingly, a switch is more likely the larger the price change. Taking the cross-partial derivative with respect to *M*, however, yields

$$\frac{\partial^2 F}{\partial p_k \partial M} = U_{xx}.$$

As long as *U* is concave in *x*, then $U_{xx} < 0$ and the likelihood of switching birth control is inversely related to *M*.

This simple model yields two empirically testable predictions. First, women who were not using a method of birth control that experienced a price increase should not alter their birth control behavior. Second, *some* women who *were* using a form of birth control that experienced a price increase should alter their optimal birth control choice, and the lower the income of these women, the more likely they are to switch.

Data Sources

We use two main sources of data for our analysis, the National College Health Assessment (NCHA) and the National Survey of Family Growth (NSFG). The NCHA is a large-scale survey of college students at participating colleges administered by the American College Health Association. It has been conducted twice a year (the fall and spring semesters) since the spring of 2000 and asks a wide range of questions related to demographic characteristics, health, and risky behavior, including sexual behavior.[7] In particular, the surveys include questions on the method of birth control used at last sex and sexual activity over the last 30 days, as well as on a wide range of medical conditions, including sexually transmitted infections and pregnancy, and health-related academic difficulties over the last year. Between thirty and eighty colleges participate each semester, with many schools participating multiple times.

We restrict the data to include only 4-year colleges, as there are very few two-year colleges in the data and these colleges are much less likely to have full-service campus health clinics. Additionally, since not all institutions participate regularly in the NCHA, we restrict the sample to

[7] We use the spring 2000 through spring 2008 survey waves. Beginning with fall 2008, a change in the survey instrument makes direct comparison with earlier waves difficult.

respondents at institutions that participated both before and after the policy change went into effect to reduce composition bias in our estimates. The institutional self-selection, although the survey is administered to randomly selected students within schools, means the NCHA is not nationally representative of all four-year colleges.

To explore the issue of representativeness, we compare our NCHA sample with four-year colleges from the Integrated Postsecondary Education Data System (IPEDS), a near universe of higher education instititutions in the United States. Table I provides summary statistics from our NCHA sample, as well as the IPEDS sample and the subset of IPEDS restricted to only full-time students. The NCHA sample is younger than the full-time IPEDS sample, and substantially more white. NCHA also contains more schools from the West and fewer from the South and Northeast, as well as more large (over 10,000 students) schools than the IPEDS full-time sample. There are not significant differences in Pill usage by school size or by region, so we do not believe this poses any analytical concerns.

The NSFG is a nationally representative sample of women aged 15 to 44, only a small fraction of whom are currently enrolled in college. We use the 2006-2008 round of the NSFG, which has the unique and beneficial feature of being administered continuously from July of 2006 through December of 2008, thus spanning the policy change. The NSFG asks a wide range of questions related to fertility and family, including a retrospective contraceptive calendar that ascertains the contraceptive methods used each month over the 48 months preceding the interview. We define our treatment population to be women who are currently enrolled in school and whose highest level of education is at least part of one year of college.

We employ both data sets because they complement each other. The NCHA has the advantage of large sample sizes (on the order of 100,000 observations in most cases), providing sufficient power to detect small changes even among subgroups of women. However because every woman in the sample is potentially affected by the policy change, there does not exist a natural control group other than college students before the policy change. The NSFG, on the other hand, allows comparison with similarly-aged women who were not college-goers and thus would not be affected by the policy change, and even allows within-person comparisons through

the monthly contraception method calendar. However, its sample size of roughly 7500 women, less than 20% of whom are attending college, comes with the cost of sharply reduced precision.

It is important to note that since we cannot identify where women obtain their birth control, we are employing an intention to treat (ITT) analysis and our estimates represent a lower bound of the effect of the policy change on Pill usage among women who were obtaining birth control from campus health clinics.[8] This is because some women obtained birth control from other sources before the policy change and thus would not be expected to be affected. Furthermore, even among women who were obtaining birth control from campus health clinics, the option to go elsewhere to obtain their contraception, may mitigate against finding an impact. Planned Parenthood, in particular, is often used by college women, and the majority of their clinics were not affected by the DRA price increase.[9]

Empirical Methodology

Because the two data sets admit different comparison groups, our empirical methodology differs somewhat between them. For the NCHA data, we employ differences and quasi-differences-in-differences designs. For the NSFG, we employ differences-in-differences and fixed effects designs. These specifications are described in detail below.

For all approaches, we define our treatment period to begin in fall (September) 2007. Our decision to begin the treatment period in the fall of 2007 reflects that the timing of the onset of the price increase varied by campus. Many colleges were able to stockpile nominally priced contraceptives in anticipation of the law, so that students were not faced with the retail prices until after those stocks ran out, typically in mid-2007 (Rooney 2007, Chaker 2007). Other college clinics had substantially smaller stocks, so the price increase took effect in early 2007. While this

[8] Information on where students obtain their birth control is surprisingly scant. One article (Cottrell 2007) reported that 10 percent of students at American University obtained prescription birth control at the Student Health Center in 2007. If we assume 40 percent of female AU students use the Pill, in line with the NCHA average, this implies that 25 percent of Pill users obtained their pills on campus. We are currently fielding a survey of University of Michigan students to obtain a more accurate estimate of where college students fill their birth control prescriptions.

[9] In future versions of this paper we hope to be able to quantify the ease of alternative options, using the distance to the nearest Planned Parenthood or Title X clinic.

type of measurement error in the treatment period will tend to bias our results toward zero, we deal with this issue by omitting data from the first half of 2007, when treatment status is uncertain.

NCHA

We estimate the effects of the DRA using one of the following regressions

$$Y_{ijt} = \alpha treat_t \beta + \boldsymbol{X_{ijt}}\lambda + f_j + \kappa t + \varepsilon_{ijt} \quad (4)$$

$$Y_{ijt} = \alpha + (treat_t \times D_i)\gamma + \boldsymbol{X_{ijt}}\lambda + f_j + \kappa t + \varepsilon_{ijt} \quad (5)$$

where Y_{ijt} is an outcome for person *i* at school *j* interviewed at wave/time *t*, $treat_t$ is a dummy for our treatment period (equal to one if *t* is Fall 2007 or Spring 2008 and zero otherwise), D_i is one of a set of demographic and behavioral characteristics used to examine heterogeneous treatment effects, and $\boldsymbol{X_{ijt}}$ is a vector of demographic and behavioral covariates (a superset of D_i). The fixed-effects f_j denote dummies for schools and *t* is a linear time trend. In some specifications we include school-specific linear time trends ($f_j \times t$) to capture school characteristics that evolve over time and may have affected Pill use and other outcomes independently of the price increase, such as attitudes towards the Pill or trends in sexual behavior on campus. In all specifications the spring 2007 wave is dropped from the analysis because of its uncertain treatment status.[10] Standard errors are clustered at the school level. The coefficients of interest are β in the base specification and *γ* in the heterogeneous treatment effects specification. These estimates represent a lower bound on the true treatment effect on the treated, since not all students were receiving their birth control from the college health clinic prior to the price change, and so not all students were subject to the "treatment" of the price change.

NSFG

The NSFG data provides two avenues for examining the effects of the DRA using the panel sample from the contraceptive calendar. In this framework an observation is now a person-month rather

[10] Relaxing this restriction does not substantively change the point estimates, and generally increases the magnitude, but also increases the standard errors.

than a person, and the time index is not the interview month, but the month referenced in the contraceptive calendar. The definition of $college_{it}$ is now such that it equals one only for the months during or which the woman is enrolled in college.[11] Because there are now multiple observations for each individual, we can employ fixed effect models:

$$Y_{it} = \alpha + treat_t\beta + college_{it}\delta + (treat_t \times college_{it})\rho + \boldsymbol{X_{it}}\lambda + \varepsilon_{it} \quad (6)$$

$$Y_{it} = \alpha + treat_t\beta + college_{it}\delta + (treat_t \times college_{it})\rho + \mu_i + \varepsilon_{it}, \quad (7)$$

where Y_{it} is an outcome for person *i* interviewed in month *t*, $treat_t$ is a dummy for treatment status (equal to one if *t* is on or after September 2007 and zero otherwise, and all other variables are as previously defined. In (6) and (7), the coefficient of interest is the difference-in-difference estimate, ρ, on the interaction of treatment and college-going, which represents the change in the difference of the outcome (e.g. Pill use) for women enrolled in college, relative to non-college-going women. Because this data set includes multiple observations for each individual, we can also employ fixed effect models in (7), where individual fixed effects, μ_i, replace the controls $\boldsymbol{X_{it}}$ from before. For heterogeneous effects, we run (6) and (7) on different subsamples of the data rather than impose additional interactions for ease of interpretation. For all NSFG models, standard errors are clustered at the PSU level.

Although all regressions with binary outcomes are run as linear probability models (using OLS), the results presented below are robust to specifying probit models and calculating average partial effects, as well.

Results

Table II presents the β coefficients on the treatment dummy from (4) with Pill use (either at last sex or currently) as the dependent variable, along with mean Pill use for reference. We run regression (4) separately for three sets of women: all women, sexually active women, and women who have had sex in the last 30 days. For each of these groups, we run regression (4) without any individual-level controls, with demographic controls (dummies for age, race, class, health

[11] We determined that a woman was enrolled in college in month *t* if she was enrolled as of the interview month and month *t* was after the interview month minus twelve times the number of years of college she had attained.

insurance status, full-time student status, residence type, greek status, and credit card debt), and with full controls (all demographic controls plus dummies for hours worked and volunteered per week and relationship status). We restrict the sample to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007 to reduce composition bias caused by colleges that participated only once or twice. Additionally, we omit the Spring 2007 wave because of its uncertain treatment status.

We find consistently negative and statistically significant estimates of the change in Pill usage following the price change. These estimates imply that overall women reduced their use of the Pill by 1 to 1.8 percentage points, or 2 to 4 percent, in response to the price increase. The effects are highly significant only for the panel of all women, but this is not surprising as the precision of the other panels is reduced due to the smaller sample sizes. Although they are less precise, the point estimates are generally of slightly smaller magnitude for sexually active women and again for women who have had sex recently. This is consistent with the idea that women who are having sex, or having more sex, are deriving more benefit from the Pill and would thus be willing to pay a higher price to continue using it. Conditioning on relationship status, with the full set of controls, does weaken the estimates, particularly among the set of all women, but this is not surprising if birth control method is endogenous to relationship status. For this reason, our preferred specification includes only the demographic controls. Finally, we note that including school-specific linear time trends does not greatly change the point estimates.

As mentioned earlier, however, not all college women are likely to have been equally affected by the price change. In particular, women with health insurance are unlikely to have been fully affected by the price increase. Nearly all insurance plans cover birth control pills, and while it may be less convenient to get birth control through private insurance, and there is an additional risk of parental disapproval if the claim goes on parental policies, which may dissuade some women from utilizing their insurance coverage (Chaker 2007, Cottrell 2007), it remains an option for women with insurance. As a result, though the price of birth control is likely to have gone up for them (few insurance policies cover prescription birth control with a $5 co-pay), the price change was probably less severe for women with insurance (Frey 2007). Women without health insurance, on the other hand, are more likely to face a binding price increase due to their much

more limited set of low-cost birth control providers outside of the campus health center, primarily Planned Parenthood or Title X clinics, which may or may not be available in the area.[12] Thus, for these reasons, and consistent with the simple model presented earlier, we would expect a more pronounced impact among women lacking health insurance. The results of regression (5) with Pill use as the dependent variable and insurance status as the heterogeneous treatment variable, D_i, are reported in Table III.[13]

We find that the reduction in Pill usage is at least twice as large among women without health insurance relative to women with health insurance (2.0 to 3.6 percentage points, compared with 0.9 to 1.2 percentage points) and the larger negative effect for uninsured women is robust to the inclusion of school-specific linear time trends. Unsurprisingly, women with health insurance have significantly higher rates of Pill usage overall. These effects for insured women represent a roughly 1.5 to 2 percent decrease in the fraction of women using the Pill, while the effects for uninsured women represent a 3.5 to 8 percent decrease in the fraction of women using the Pill. Assuming women without insurance faced a larger price increase, this is consistent with our model that the larger the price change the more likely a woman is to change her optimal birth control choice.

To further explore heterogeneous treatment effects, Table IV presents the results of regression (5) with Pill use as the dependent variable and credit card debt as the heterogeneous treatment variable. Credit card debt can be thought of as a proxy for financial constraints; women with large credit card balances are more likely to be both lower income and more credit constrained and price sensitive than their peers without balances. We find that Pill use decreased among women without any credit card debt by around one percentage point, and this magnitude increases monotonically in the amount of debt. Women with credit card balances less than $2,000 reduced their use of the Pill by 1.5 to 2.3 percentage points, and women with over $2,000 in debt reduced their use of the Pill by 3.5 to 4.6 percentage points, a nearly ten percent decrease in the fraction of women using the Pill. This too is consistent with our model; if women with large credit

[12] Proximity to alternative clinics is something we hope to examine in future versions of this paper.
[13] We classify women who report they don't know whether they have health insurance, a relatively small group, as not having health insurance.

card balances have less disposable income, either because they are lower income to begin with or because a larger fraction of their income goes to pay down debt, then our model predicts that they would be more likely to switch birth control choices, and that the likelihood is increasing in the amount of debt, which we find.

We also find significant differences in treatment effects by age, with "older" women being substantially more likely to decrease their Pill usage than "younger" women.[14] Although these results are not reported in the tables, college women aged 18 to 22 reduced their Pill usage by 1.0 percentage point, and this effect was marginally significant at 10 percent. For women aged 23 and older, Pill use fell by 4.4 percentage points, and this effect was significant at 1 percent. Given that the modal age to graduate college is 22, women over 23 are more likely to be graduate students or non-traditional students who are less likely to have parental support and more likely to be financially constrained.

These results point to a pattern of women at risk of financially disadvantage, who were the women most likely to benefit from the subsidies at campus health centers, having strong and statistically significant reductions in their usage of the Pill in response to the price increase, while more advantaged women were more likely to absorb the price increase. For women without insurance and women with large amounts of debt, for whom the price increase is more likely to bind, we estimate the change in demand for the Pill to between 5 and 10 percent, in response to a price increase of at least 300 percent. This implies a small price elasticity for the Pill[15], consistent with the previous literature, though it is clear the price elasticity is greater for vulnerable populations.

Of course, if college women are using the Pill less, how else is their birth control choice or sexual behavior changing? Results for regressions using other birth control methods and outcomes as dependent variables are reported in Table V. The coefficients reported are those on the treatment dummy in (4) with use of the method at last sex (or current use) as the dependent

[14] Since our sample is of college students, "older" here means age 23 and above, while "younger" means ages 18 through 22.

[15] We should be cautious in estimating an actual elasticity, as we don't what fraction of college women received their birth control pills at college health centers before the policy change. Even if this fraction is as small as 20 percent among the financially disadvantaged, the implied elasticity is no larger than 0.17 ((0.1/0.2)/3).

variable. We find a small insignificant decrease in condom usage overall, but a significant increase (2.7 percentage points) among uninsured women who have had sex in the last 30 days. We find small increases in no use of a method at all, but these are significant only for infrequent sex participants. Taken together this suggests that the substitution towards unprotected sex is concentrated among poorer women who do not have sex frequently. Grouping the non-prescription methods together, we do find substantive increases, so it appears as though women who have sex frequently did substitute some towards non-prescription methods following the price change. Table VI reports estimates of non-prescription birth control usage—including condoms, withdrawal, sponge and rhythm—at last sex as well as emergency contraception in the last 12 months by health insurance and credit card debt. Among sexually active and recent sex women, we see sharp increases in the usage of non-prescription methods among women without insurance and women with credit card debt, suggesting that roughly half of the women in these groups who stopped using the Pill switched to non-prescription methods. We also see a significant increase in emergency contraception (EC) usage among women without insurance, as well as a modest, though statistically insignificant, increase overall. The largest increase in EC use occurs among women who have sex infrequently and do not have insurance. This is consistent with the idea that if you are only at risk of pregnancy infrequently, purchasing EC on those occasions might be less expensive than purchasing continuous birth control every month.

If women were substituting toward unprotected sex, we might expect to find positive effects on the incidence of sexually transmitted infections (STIs) or accidental pregnancy. Most of the point estimates for STI diagnosis, however, including those that are significant, are negative. Table VII reports coefficients from regressions with STI diagnosis and accidental pregnancy as the dependent variable. It does not appear as though women without insurance or with debt had differential rates of STI infection after the policy change. The same is true for accidental pregnancy: though most of the point estimates are positive, they are very small and insignificantly different from zero with the exception of the most credit-constrained women.

Perhaps suggesting that some women substitute not toward other contraceptive methods or toward unprotected sex but toward less sex, the treatment coefficient on the number of sexual partners in the last 30 days in Table V is negative and highly significant (between -0.06 and -0.08,

or a 6 to 7 percent decrease in the average number of partners), as is the coefficient on the binary variable for vaginal sex in the last 30 days (between -0.009 and -0.015, a 1.8 to 3 percent decrease in the proportion of women who had vaginal sex in the last 30 days). Table VIII breaks down the results for number of sex partners and vaginal sex in the last 30 days by health insurance and debt status. The change in the number of sex partners seems to be roughly stable by insurance status; in terms of debt status, the effects on both the number of sex partners and recent vaginal sex are greatest for women with moderate amounts of debt.[16]

Overall, these results suggest that college women did reduce their use of the Pill in response to the price increase, and that the reductions were larger among lower income, more credit constrained women and women without health insurance. Additionally, we find that the alternatives employed by women differed based on their frequency of sex and their debt and insurance status. Women without insurance were more likely to increase their usage of EC and non-prescription methods of birth control, while women with moderate to high amounts of debt increased use of EC, but we notice increases in non-prescription birth control among only moderate debt women. Infrequent sex participants appear to have been more likely to switch to no method, while frequent sex participants appear to have been more likely to reduce their number of sex partners. Although we find no evidence of increases in STI infections or accidental pregnancy for most affected women, we cannot rule out that the most credit-constrained women did suffer an increase in the risk of unintended pregnancy.

NSFG

The NSFG results are, in general, imprecise due to the significantly smaller sample size (1,172 college students). We report them mainly as a robustness check of our previous results, and they do not always agree. Demographic controls include dummies for age, race, educational attainment, race, insurance status and poverty status. Full controls add dummies for household type and relationship status to the set of demographic controls. For these regressions, we restrict

[16] Note that this is consistent with the marginally significant findings of an increase in accidental pregnancy among the most credit-constrained women: they show a sharp reduction in pill use, little substitution toward condoms, and insignificant reductions in number of sexual partners or sexual frequency.

the sample to women who were interviewed during or after September, 2007 so that we have information on their contraceptive use before and after the price change. Because this data tracks individual women over time before and after the price change, we believe that these results are more representative of the true effect and less susceptible to composition bias that would be found using a cross-sectional approach. The differences-in-differences approach shows that college students reduced their use of the Pill by three percentage points more than non-students, which is slightly higher than the NCHA estimates. To better compare with the NCHA results, we also restricted the panel sample to only women who were enrolled in college and ran regression (4). The β coefficients from that regression show that college students reduced their Pill use by 1.5 to 1.8 percentage points, which is very similar to the estimates we obtained from the NCHA. This suggests that measuring the change in Pill usage among only college students might understate the true effect if non-students were also reducing their use of the Pill. Adding further confidence to our results, the fixed effects specifications from regression (7) provide very similar point estimates. None of the panel estimates are significant, but that is not surprising given the reduced sample size of the NSFG.

When we look at the difference in Pill usage by insurance status, we find very similar patterns in the NSFG. It is worth noting that the question about insurance is slightly different. In the NCHA the insurance question is "Do you currently have health insurance?"; in the NSFG the question is "Have you had health insurance continuously for the last 12 months?" While the responses are likely to be highly correlated, they are not completely comparable. Despite the differences in question wording we do find support for larger effects for uninsured women across all panel specifications in the NSFG. These estimates suggest that college women with insurance either slightly increased their Pill use or decreased their Pill use by around 2 percentage points, while college women without continuous insurance decreased their Pill use by 7 to 9 percentage points. These results are again consistent with uninsured women facing a larger price increase and so being more likely to switch their optimal birth control choice as predicted by our model.

Conclusion

In this paper, we examine the effects of a large exogenous shock to the price of the birth control Pill on college campuses caused by the Deficit Reduction Act of 2005. Using two different data sources, the NCHA and the 2006-2008 NSFG, we find that the three- to ten-fold increase in the price of the Pill reduced the use of oral contraception by 1 to 1.8 percentage points, on average, or 2 to 4 percent, among college women. These findings are consistent with previous literature that documents small price elasticities for contraception in other contexts. We also find evidence that the reduction in the use of the Pill was significantly stronger for women without health insurance, women with credit card debt, and older women. We find some evidence of substitution towards non-prescription birth control methods and emergency contraception among those same women, as well as a reduction in the number of sexual partners, particularly among frequent sex participants. We find minimal evidence of changes in STI infection or accidental pregnancy. The analysis of the NSFG data returns broadly consistent estimates, increasing our confidence in our NCHA results.

Our results suggest that the enactment of the Deficit Reduction Act and the consequent increase in the price of the Pill on college campuses did have an effect on the contraceptive choices of college women, primarily by reducing Pill usage in populations that are likely to be financially constrained, but did not increase the rates of unintended pregnancy or sexually transmitted infections for most women.

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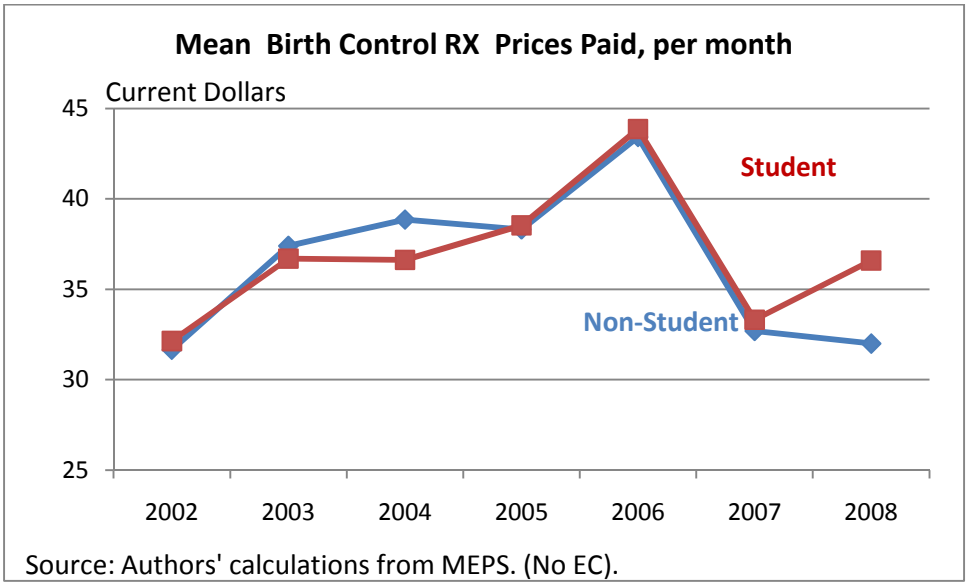
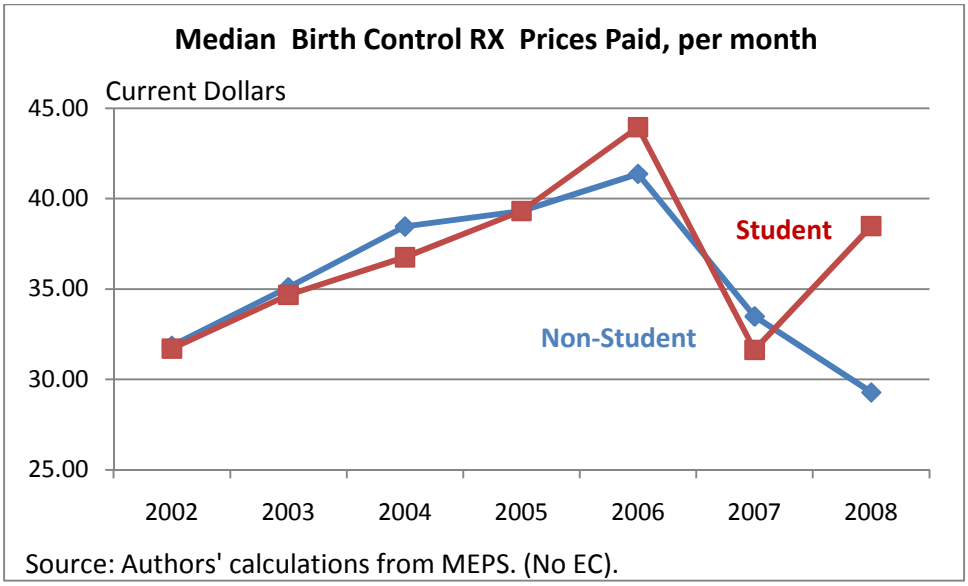
Figure I

MEPS Expenditures on Prescription Birth Control



Source: Authors' calculations from MEPS. (No EC).



Source: Authors' calculations from MEPS. (No EC).

Outcome variable is price paid for prescription birth control, excluding emergency contraception, separated by student status. Data from the Medical Expenditure Panel Survey (MEPS).

Table I

Comparison of NCHA sample to IPEDS and IPEDS full-time

	NCHA	IPEDS	IPEDS Full Time
Age			
17-19	0.362	0.218	0.278
20-21	0.348	0.203	0.262
22-24	0.168	0.154	0.163
25-29	0.068	0.120	0.093
30+	0.045	0.202	0.110
Missing	0.010	0.104	0.093
Race/Ethnicity			
White	0.743	0.614	0.621
Black	0.056	0.122	0.116
Hispanic	0.054	0.093	0.091
Asian	0.096	0.054	0.059
International	0.038	0.036	0.040
Level			
Undergraduate	0.882	0.773	0.846
Graduate/Professional	0.086	0.227	0.154
Full-time	0.952	0.717	1.000
Region			
Northeast	0.148	0.209	0.207
Midwest	0.230	0.244	0.240
South	0.237	0.330	0.327
West	0.319	0.196	0.205
Public	0.700	0.619	0.615
Carnegie			
Baccalaureate	0.076	0.114	0.120
Masters	0.243	0.364	0.338
Doctoral	0.659	0.405	0.432
Size			
< 2500	0.046	0.133	0.140
2500-5000	0.038	0.125	0.122
5000-9999	0.144	0.173	0.163
10000-19999	0.380	0.235	0.229
20000+	0.393	0.335	0.345
Missing	0.000	0.000	0.000

Table II

NCHA – Pill Use at Last Sex

β Coefficients from regression (4)

Panel A: All Women	-0.0167**	-0.0171***	-0.0108*	-0.0182**	-0.0146***	-0.00900*
mean = 0.418	[0.00781]	[0.00605]	[0.00580]	[0.00745]	[0.00533]	[0.00525]
n = 95,886						
Panel B: Sexually active women	-0.00936	-0.0145*	-0.0119	-0.00916	-0.0116	-0.00971
mean = 0.586	[0.00846]	[0.00774]	[0.00781]	[0.00919]	[0.00743]	[0.00753]
n = 68,053						
Panel C: Sex in last 30 days	-0.00805	-0.0134*	-0.0117	-0.0109	-0.0139*	-0.0129*
mean = 0.626	[0.00890]	[0.00751]	[0.00751]	[0.0103]	[0.00751]	[0.00762]
n = 46,547						
Demographic Controls		X	X		X	X
Full controls			X			X
School-specific linear time trend				X	X	X

Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex. Each cell is a separate regression. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status. Full controls include all demographic controls plus dummies for hours worked and volunteered per week, and relationship status.

Table III

NCHA – Pill Use by Insurance Status

 Coefficients from regression (5)

	Health Insurance	No/DK Insurance	Health Insurance	No/DK Insurance
Panel A: All Women				
Mean	0.428	0.354	0.428	0.354
Treatment	-0.0127**	-0.0363***	-0.0116**	-0.0327***
(n = 95,886)	[0.00587]	[0.00859]	[0.00531]	[0.00853]
Panel B: Sexually active women				
Mean	0.605	0.494	0.605	0.494
Treatment	-0.0104	-0.0264***	-0.00911	-0.0237**
(n = 68,053)	[0.00769]	[0.00970]	[0.00764]	[0.00985]
Panel C: Sex in last 30 days				
Mean	0.641	0.534	0.641	0.534
Treatment	-0.00925	-0.0203	-0.0112	-0.0194
(n = 46,547)	[0.00735]	[0.0137]	[0.00770]	[0.0139]
Demographic Controls	X	X	X	X
School-specific linear time trend			X	X

Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex. Each group of cells in a row is a separate regression. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table IV

NCHA – Pill Use by Credit Card Debt

 Coefficients from regression (5)

	No debt	< $2000 in debt	> $2000 in debt	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.391	0.486	0.491	0.391	0.486	0.491
Treatment	-0.0123*	-0.0230**	-0.0469***	-0.0103*	-0.0191**	-0.0407***
(n = 95,886)	[0.00615]	[0.00868]	[0.0137]	[0.00549]	[0.00902]	[0.0128]
Panel B: Sexually active women						
Mean	0.603	0.587	0.532	0.603	0.587	0.532
Treatment	-0.00779	-0.0233**	-0.0397***	-0.00522	-0.0197*	-0.0354**
(n = 68,053)	[0.00769]	[0.0103]	[0.0148]	[0.00722]	[0.0113]	[0.0140]
Panel C: Sex in last 30 days						
Mean	0.647	0.620	0.546	0.647	0.620	0.546
Treatment	-0.00860	-0.0154	-0.0410***	-0.00968	-0.0144	-0.0403***
(n = 46,547)	[0.00784]	[0.0115]	[0.0146]	[0.00797]	[0.0119]	[0.0136]
Demographic Controls	X	X	X	X	X	X
School-specific linear time trend				X	X	X

Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex. Each group of cells in a row is a separate regression. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table V

NCHA – Other Contraceptive Methods and Outcomes

β Coefficients from regression (4)

Dependent Variable	All Women	Sexually Active Women	Sex in last 30 days
Condom use	-0.0106	-0.00196	0.00516
	[0.00639]	[0.00796]	[0.00866]
Emergency Contraception Use	0.00392	0.00900	0.00839
	[0.00369]	[0.00560]	[0.00693]
Rhythm Method Use	0.000471	0.00172	0.00231
	[0.00182]	[0.00267]	[0.00358]
No Method Use	0.00300	0.00544*	0.00152
	[0.00206]	[0.00272]	[0.00364]
Non-Prescription Method Use	0.00196	0.0116	0.0141*
	[0.00554]	[0.00700]	[0.00788]
STI diganosis	-0.00498**	-0.00550*	-0.00523
	[0.00216]	[0.00307]	[0.00339]
Accidental Pregnancy	0.000980	0.00157	0.000788
	[0.00120]	[0.00163]	[0.00250]
Number of Sex Partners	-0.0758***	-0.0625**	-0.0882***
	[0.0217]	[0.0266]	[0.0328]
Vaginal Sex in Last 30 Days	-0.0150***	-0.00963*	-
	[0.00540]	[0.00518]	-
Demographic Controls	X	X	X

This table reports the coefficients on the treatment dummy from various regressions with the given dependent variable. Standard errors, clustered at the school level, are in brackets. Each cell is a separate regression. All birth control method variables are use at last sex, STI and pregnancy variables are in the last 12 months, and number of sex partners is in the last 30 days. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table VI

NCHA – Non-prescription Birth Control and Emergency Contraception Usage by Health Insurance and Debt Status

 Coefficients from regression (5)

Non-Prescription Methods	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.219	0.210	0.275	0.204	0.252	0.263
Treatment	0.00196	0.000251	0.00317	-0.00228	0.0111	0.0128
(n = 95,886)	[0.00554]	[0.00572]	[0.00878]	[0.00575]	[0.00788]	[0.0104]
Panel B: Sexually active women						
Mean	0.308	0.297	0.383	0.314	0.305	0.284
Treatment	0.0116	0.00754	0.0251**	0.00475	0.0207**	0.0285**
(n = 68,053)	[0.00700]	[0.00704]	[0.0119]	[0.00758]	[0.00914]	[0.0121]
Panel C: Sex in last 30 days						
Mean	0.278	0.267	0.350	0.282	0.275	0.264
Treatment	0.0141*	0.00922	0.0237	0.00733	0.0235**	0.0299**
(n = 46,547)	[0.00788]	[0.00760]	[0.0148]	[0.00876]	[0.0110]	[0.0128]
Emergency Contraception	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.0919	0.0916	0.0928	0.0865	0.109	0.0935
Treatment	0.00392	0.00235	0.0157*	-5.66e-05	0.0149**	0.00852
(n = 95,886)	[0.00369]	[0.00366]	[0.00862]	[0.00365]	[0.00580]	[0.00743]
Panel B: Sexually active women						
Mean	0.128	0.128	0.128	0.131	0.130	0.101
Treatment	0.00900	0.00605	0.0305**	0.00706	0.0140*	0.00470
(n = 68,053)	[0.00560]	[0.00568]	[0.0123]	[0.00578]	[0.00730]	[0.00887]
Panel C: Sex in last 30 days						
Mean	0.138	0.137	0.138	0.143	0.139	0.106
Treatment	0.00839	0.00553	0.0285**	0.00732	0.00927	0.00375
(n = 46,547)	[0.00693]	[0.00710]	[0.0134]	[0.00695]	[0.00912]	[0.0107]
Demographic Controls	X	X	X	X	X	X

This table reports the coefficients on the treatment dummy from various regressions with the given dependent variable. Standard errors, clustered at the school level, are in brackets. Each group of cells in a row is a separate regression. Non-prescription methods variable is use at last sex, emergency contraception variable is use in the last 12 months. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table VII

NCHA – STI Diagnosis and Accidental Pregnancy Rates by Health Insurance and Debt Status

 Coefficients from regression (5)

STI Diagnosis	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.0462	0.0464	0.0456	0.0362	0.0641	0.0877
Treatment	-0.00498**	-0.00427*	-0.00533	-0.00667***	4.92e-06	0.00187
(n = 95,886)	[0.00216]	[0.00236]	[0.00429]	[0.00212]	[0.00404]	[0.00813]
Panel B: Sexually active women						
Mean	0.0630	0.0636	0.0604	0.0529	0.0755	0.0947
Treatment	-0.00550*	-0.00470	-0.00329	-0.00804**	0.000763	0.00104
(n = 68,053)	[0.00307]	[0.00338]	[0.00604]	[0.00315]	[0.00513]	[0.00904]
Panel C: Sex in last 30 days						
Mean	0.0673	0.0679	0.0640	0.0568	0.0777	0.0999
Treatment	-0.00523	-0.00396	-0.00337	-0.00777**	5.76e-05	0.000438
(n = 46,547)	[0.00339]	[0.00353]	[0.00732]	[0.00357]	[0.00636]	[0.0107]
Accidental Pregnancy	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.0158	0.0151	0.0216	0.0113	0.0236	0.0330
Treatment	0.000980	0.000804	0.00194	0.00130	-0.00293	0.00756
(n = 95,886)	[0.00120]	[0.00111]	[0.00379]	[0.00129]	[0.00253]	[0.00467]
Panel B: Sexually active women						
Mean	0.0221	0.0211	0.0298	0.0172	0.0284	0.0356
Treatment	0.00157	0.00119	0.00387	0.00174	-0.00257	0.0103**
(n = 68,053)	[0.00163]	[0.00154]	[0.00524]	[0.00177]	[0.00316]	[0.00508]
Panel C: Sex in last 30 days						
Mean	0.0239	0.0225	0.0335	0.0188	0.0289	0.0385
Treatment	0.000788	0.000475	0.00222	0.00107	-0.00427	0.00961
(n = 46,547)	[0.00250]	[0.00249]	[0.00676]	[0.00263]	[0.00452]	[0.00656]
Demographic Controls	X	X	X	X	X	X

This table reports the coefficients on the treatment dummy from various regressions with the given dependent variable. Standard errors, clustered at the school level, are in brackets. Each group of cells in a row is a separate regression. STI diagnosis and pregnancy variables are in the last 12 months. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table VIII

NCHA – Number of Sex Partners and Vaginal Sex in Last 30 Days
by Health Insurance and Debt Status
 Coefficients from regression (5)

Number of Sex Partners	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	1.256	1.257	1.273	1.171	1.432	1.550
Treatment	-0.0758***	-0.0768***	-0.0983**	-0.0699***	-0.103***	-0.0567
(n = 95,886)	[0.0217]	[0.0210]	[0.0452]	[0.0220]	[0.0326]	[0.0637]
Panel B: Sexually active women						
Mean	1.678	1.683	1.697	1.682	1.682	1.661
Treatment	-0.0625**	-0.0706***	-0.0541	-0.0577**	-0.0978**	-0.0263
(n = 68,053)	[0.0266]	[0.0253]	[0.0606]	[0.0281]	[0.0375]	[0.0693]
Panel C: Sex in last 30 days						
Mean	1.768	1.770	1.802	1.769	1.769	1.757
Treatment	-0.0882***	-0.0947***	-0.0863	-0.0862**	-0.119**	-0.0528
(n = 46,547)	[0.0328]	[0.0323]	[0.0729]	[0.0347]	[0.0470]	[0.0871]
Vaginal Sex in Last 30 Days	All	Health Insurance	No/DK Insurance	No debt	< $2000 in debt	> $2000 in debt
Panel A: All Women						
Mean	0.507	0.506	0.511	0.452	0.612	0.715
Treatment	-0.0150***	-0.0137**	-0.0291***	-0.0127**	-0.0223***	-0.0210
(n = 95,886)	[0.00540]	[0.00558]	[0.0106]	[0.00569]	[0.00680]	[0.0138]
Panel B: Sexually active women						
Mean	0.720	0.720	0.720	0.702	0.743	0.775
Treatment	-0.00963*	-0.0110*	-0.00696	-0.00836	-0.0192**	-0.00155
(n = 68,053)	[0.00518]	[0.00575]	[0.00974]	[0.00591]	[0.00725]	[0.0123]
Panel C: Sex in last 30 days						
Mean	1	1	1	1	1	1
Treatment	-	-	-	-	-	-
(n = 46,547)	-	-	-	-	-	-
Demographic Controls	X	X	X	X	X	X

This table reports the coefficients on the treatment dummy from various regressions with the given dependent variable. Standard errors, clustered at the school level, are in brackets. Each group of cells in a row is a separate regression. Number of sex partners is in the last 30 days and vaginal sex is yes/no in the last 30 days. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Fall 2007. Spring 2007 is omitted. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status.

Table IX

NSFG – Pill Use at Last Sex in Cross-Sectional and Panel Data

Panel	Treat*College		Treat	
A: Diff-in-Diff	-0.0368	-0.0329		
mean = 0.1840	[0.0276]	[0.0266]		
n = 159,675				
B: Students only			-0.0177	-0.0148
mean = 0.2706			[0.0276]	[0.0262]
n = 14,637				
C: Fixed Effects	-0.0130			
mean = 0.1840	[0.0204]			
n = 159,675				
Demographic Controls	X	X	X	X
Full Controls		X		X

Dependent variable is Pill use at last sex within 12 months. Standard errors cluster at the PSU level are reported in brackets. All regressions include population weights. Each cell is a separate regression. The Panel sample uses the retrospective contraceptive calendar and restricts the sample to women who were interviewed during or after September, 2007. Demographic controls include age, education, race, insurance and poverty status. Full controls include all demographic controls plus household type and relationship status.

Table X

NSFG – Pill Use at Last Sex by Insurance Status in Cross-Sectional and Panel Data

	Yes, Continuous Insurance				No Continuous Insurance			
Panel	Treat*College		Treat		Treat*College		Treat	
A: Diff-in-Diff								
Mean	0.2042				0.1393			
Treatment	-0.0293	-0.0286			-0.07	-0.0668		
n = 159,675	[0.0312]	[0.0285]			[0.0513]	[0.0493]		
B: Students only								
Mean			0.2958				0.1802	
Treatment			0.00367	0.00221			-0.0910**	-0.0838**
n = 11,238			[0.0314]	[0.0272]			[0.0377]	[0.0365]
C: Fixed Effects								
Mean	0.2042				0.1393			
Treatment	0.00914				-0.0865**			
n = 159,675	[0.0249]				[0.0395]			
Demographic Controls	X	X	X	X	X	X	X	X
Full Controls		X		X		X		X

Dependent variable is Pill use at last sex within 12 months. Standard errors cluster at the PSU level are reported in brackets. All regressions include population weights. Each cell is a separate regression. The Panel sample uses the retrospective contraceptive calendar and restricts the sample to women who were interviewed during or after September, 2007. Panel regressions are run separately for each insurance condition. Demographic controls include age, education, race, insurance and poverty status. Full controls include all demographic controls plus household type and relationship status. Insurance status is determined by whether the respondent had continuous health insurance coverage over the last 12 months.