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Association Between Increased Emergency Contraception Availability and Risky Sexual Practices

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ABSTRACT

Whether to make emergency contraception more easily available is an issue of longstanding policy debate. Emergency contraception (EC) conveys no protection against sexually transmitted infections (STIs), but since it lowers the cost of unprotected sex, many worry that increased availability may increase risky sexual practices - even though economic bargaining models suggest the impact could be positive or negative. We studied whether increased EC availability for women over age 18 was associated with a higher probability of risky sexual practices using data from the National Longitudinal Survey of Youth, 1997 from October 1999 – November 2009. We found that greater access to EC reduced the probability of sexual activity by about -5% for women and reduced the probability of multiple sexual partners for women by -4.4%, but also increased the probability of unprotected sex when women had multiple partners by about +5%. This suggests policies for expanding EC access may need to be paired with education about its inability to control STI risk.

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1. Introduction

The Food and Drug Administration (FDA) approved Plan B, an emergency contraceptive pill (EC), for behind-the-counter (BTC) sale to individuals 18 years and older on August 24, 2006, reversing a position it had taken in 2004 (F.D.A. ; G.A.O. 2005). Prior to this federal ruling, eight states passed legislation allowing EC to be sold BTC[1]. The controversy over offering Plan B BTC continued though the end of 2011, when the Obama administration declined to extend BTC status for Plan B to younger women (Harris 2011; Wood, Drazen et al. 2011). Much of the controversy revolves around the degree to which BTC access to ECP will have unintended consequences for risky sexual behaviors. By reducing the risk of pregnancy, EC lowers the opportunity cost of unplanned, and unprotected sex; when the opportunity cost of anything falls, one expects the amount of that thing to increase. Thus, some opponents of making Plan B available without a prescription are concerned that increasing access may increase sexual activity (especially among youth) and decrease barrier contraceptive (condom) use, which may lead to increases in sexually transmitted infections (STIs) (Wearn and Gill 1999; Grimes 2002). However, an economic bargaining model would also suggest it is possible that increased access to EC may actually reduce women's ability to obtain protected sex (since their partners know pregnancy risk can be controlled *ex post*) and thus lead some women to control STI risk through abstinence or monogamy. These two countervailing forces imply that whether EC access reduces or increases risky sexual behaviors on net is an empirical question.

In this article we provide evidence regarding whether increased EC availability for women over age 18 was associated with a higher probability of risky sexual practices. We do

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[1] These states include: California, New Mexico, Maine, Massachusetts, New Hampshire, Washington, Alaska, and Hawaii. Vermont passed state-level access to all women irrespective of age in 2007, after the FDA policy change which applied to women aged 18 and over.

this by conducting a retrospective analysis of female respondents to the National Longitudinal Survey of Youth, 1997 (NLSY97) using observations from October 1999 – November 2009 (34,030 individual / year observations on 3,786 women). At the state-level, between 1998 and 2006, eight states increased the availability of EC by allowing it to be sold BTC[2] and then in August of 2006, EC became available BTC in all states due to an FDA ruling permitting Plan B to be sold BTC to women over age 18. We estimate separate linear probability regressions with individual fixed effects and random effects longitudinal logit regressions to model the impact of EC access changes on: 1) the probability of any sexual activity, 2) the probability of multiple sexual partners and 3) the probability of any unprotected sex conditional on having multiple sexual partners. Our results suggest that by reducing the pregnancy risk from unplanned sexual encounters, BTC availability of EC both reduced the frequency of sexual activity and the frequency of multiple sexual partners for some women and increased the frequency of unprotected sex (and consequently the risk of STIs) for those women who did have multiple partners. Finally, we discuss what these results suggest about changes to the relative power women have in negotiating safe sex practices with their partners, and ways that FDA and state policies might be adjusted to account for this.

2. History of State and Federal Policy Changes for Emergency Contraception

High dose, or multiple dose, versions of traditional birth control pills have been known to be effective at post-coital pregnancy prevention since the early 1980s. Specially packaged versions of such birth control pills have been marketed as EC in New Zealand and Europe for more than 25 years. However, manufacturers in the United States are not allowed to actively

[2] During this period, some state legislation allowed EC to be purchased BTC by individuals younger than 18; however, we are interested in the effect of these policy changes on young adult women's sexual behaviors, so we do not discuss this aspect of the legislation.

market "off label" (non-approved uses) for their products, and since the FDA had not granted an approval for regular birth control pills to be used as EC, this possibility was often labeled the "best-kept contraceptive secret" in the United States (Cimons 1997). Despite ambivalence from pharmaceutical manufacturers (due primarily to litigation concerns) the FDA announced a new approved use for high or multi-dose birth control pills as EC on February 24, 1997; in an unusual move at the time, the FDA requested that manufacturers re-label and market versions of their birth control pills as EC in order to increase awareness of the option among women (AP 1997; McCullough 1997). However, it was not until September of 1998 that the first such specially-labeled product, *Preven*®, was marketed in the U.S. by Gynetics.

Following the FDA approval, Washington was the first state in which EC was actively marketed and made available without a prescription. Washington was selected as the site of the first pilot study of non-prescription distribution because it had, since 1981, allowed pharmacists to provide some prescription-only medications to patients without an actual prescription, as long as the pharmacist had additional training and was working under the supervision of a physician (AP 1998). The pilot project was initiated in late February 1998 and in its first eight months had more than 500 participating pharmacists and had distributed nearly 5000 doses of EC (Horiuchi 1998). Even though the project was considered a success, and hailed as a break-though by reproductive rights organizations, it would be more than four years before other Western states (California, Alaska, Hawaii and New Mexico) began to create programs to permit non-prescription distribution of *Preven*® and (later) *Plan B*®.

Maine was the first Eastern state to approve non-prescription, "behind the counter," distribution of EC. After significant debate over expanded access between proponents (primarily family planning clinics and women's health advocates) and opponents (primarily pharmacists concerned about liability issues, trial attorneys opposed to bill language that limited pharmacists malpractice exposure, and religious organizations), BTC availability of EC was effective in Maine in July of 2004 (Huang 2004; Betts 2005). As with the experience in Washington, participation in the program grew rapidly. Importantly, the Maine law permitted pharmacists working with physicians to prescribe EC without a prescription to women of any age, not just those over age 18. New Hampshire was the next state to approve BTC provision of EC. As with Maine, the process for approval was contentious. Indeed, a similar bill approving BTC status for EC was vetoed by Governor Craig Benson in 2004. However, in mid-2005 BTC status was again approved by the New Hampshire legislature, and signed into law by Governor John Lynch; it became effective in mid-August of 2005 (Fahey 2005).

Table 1 presents the dates for which each state permitting ECP to be provided BTC by pharmacists implemented the law. Note that the implementation dates differ (sometimes substantially) from the dates that legislation was signed. Each of these dates was confirmed by local newspaper accounts, found using a Lexis/Nexis search.

While the process that led the respective states to allow non-prescription distribution of EC by pharmacists was often contentious, the pathway to national BTC approval by the FDA was – even by Washington D.C. standards – remarkably bitter and convoluted. After several years of study and delayed announcements (and an unusual intervention in 2004 by top FDA officials in an independent review panel's report), Lester M. Crawford testified at a Senate hearing on his confirmation as FDA commissioner in March of 2005 that FDA approval for *Plan B* was imminent. On the strength of that testimony, Senators Patty Murray (D-Washington) and Hillary Clinton (D-New York) dropped their objections to his nomination. Soon after Crawford's appointment, the FDA announced on August of 2005 that it was in fact indefinitely postponing a decision to approve *Plan B* for BTC status – a move which "incensed" Senators Clinton and Murray (Kaufman 2005; Kaufman 2005). Commissioner Crawford's tenure at the FDA was rocky and brief, in part due to the Agency's backtracking on *Plan B*. After his resignation, the nomination of his replacement was subsequently held up in the Senate until after the FDA actually approved *Plan B* for BTC sales on August 24, 2006 (Kaufman 2006).

Initially, non-prescription status for *Plan B* was approved by the FDA only for women aged 18 and over. Subsequently, the age restriction was reduced to 17 and over in late July 2009. However, there was continued pressure on the FDA to further extend the age range for which women could receive EC without a prescription. This pressure originated in a 2003 report by the FDA's own independent review panel that recommended that EC be made available BTC for all women of childbearing age. Given increased pressure, the FDA revisited the issue in 2011, at which time an independent panel again recommended that EC be made available BTC to all women who could become pregnant, irrespective of age. The FDA commissioner approved the report and moved to implement the recommendation. However, before the decision could be finalized, U.S. Department of Health and Human Services Secretary Kathleen Sebelius ordered the FDA to deny the extended approval and maintain the 17-year-old age limit. According to published reports, this action by Secretary Sebelius in overruling an announced FDA decision was unprecedented, and underscores the contentiousness of the on-going debate (Adams 2011).

This age restriction is the main difference between the FDA policy permitting BTC access to emergency contraception and the state laws outlined in Table 1. Each of the states permit BTC sales to women of any age, whereas as discussed just above the FDA rules continue to apply to women aged 17 and older. For our purposes, however, this distinction will be moot:

all women in out NLSY sample are over age 18 for all of the years we include. Thus, we will not differentiate between state and FDA policies regarding BTC sales.

3. Literature on ECP Effectiveness

Most research on ECPs' effect on women's sexual behaviors has focused on advance provision of ECPs to groups of women enrolled in research trials. Generally, these studies have not found evidence that providing advance supplies of ECPs results in lower rates of more effective contraception, increases in unprotected sex, increases in STI, or increases in pregnancy rates (Glasier and Baird 1998; Jackson, Schwarz et al. 2003; Gold, Wolford et al. 2004; Lo, Fan et al. 2004; Raine, Harper et al. 2005; Raymond, Stewart et al. 2006; Polis, Schaffer et al. 2007; Ekstrand, Larsson et al. 2008; Trussell and Raymond 2011). Most analyses of advance ECP provision found that having ECPs on hand was not associated with a change in contraceptive use (Jackson, Schwarz et al. 2003; Gold, Wolford et al. 2004; Polis, Schaffer et al. 2007; Ekstrand, Larsson et al. 2008; Trussell and Raymond 2011). A review of 11 studies evaluating the effects of advance provision suggested that advance provision was: 1) not associated with decreased pregnancy rates; and 2) did not result in more frequent unprotected sex, higher rates of STIs, or changes in contraceptive use (Polis, Schaffer et al. 2007; Trussell and Raymond 2011; Wood, Drazen et al. 2011). However one study of a young, high-risk clinic population found that the advance provision group was more likely to use less effective contraception than the control group (Trussell and Raymond 2011; Wood, Drazen et al. 2011). In addition, reanalysis of data from one randomized controlled trial found that women in the advance ECP provision group were more likely to report unprotected sexual activity resulting in pregnancy than women in the control group (Wearn and Gill 1999; Trussell and Raymond 2011). Women receiving advance

ECP provision also were more likely to report using ECPs because they did not want to use condoms or another form of birth control (Weaver, Raymond et al. 2009; Trussell and Raymond 2011). It is important to note that the research on advance ECP provision has been conducted on relatively small samples of women in trials settings; little evidence exists on the effects of advance ECP provision in the community at large.

Several studies have evaluated the impact of providing ECPs without a prescription on women's reproductive behaviors. Pharmacy access to ECPs in France did not increase transitions into sexual activity, decrease age of sexual initiation, or increase risk for unintended pregnancy for young women (Moreau, Bajos et al. 2006; Trussell and Raymond 2011). Unintended pregnancy and STI rates were the same for women (aged 15-24 years old) in four family planning clinics in California who were given either advance ECP provision, pharmacy access, or prescription access (Trussell and Raymond 2011; Wood, Drazen et al. 2011). The first national study of the impact of BTC access to ECPs (from the United Kingdom) indicated that making ECPs available without a prescription did not significantly change women's contraceptive behaviors (Marston, Meltzer et al. 2005; Trussell and Raymond 2011). A more recent study did found that pharmacy provision of ECPs in England was associated with significant increases in STI diagnoses for teens (Girma and Paton 2010).

While there is evidence on the impact of actually giving women ECP in advance to have on-hand, there is very little evidence from the United States regarding how mere ECP availability in the community affects risky sexual behavior. To our knowledge, only three studies have assessed how changes in access to ECPs affected United States populations, and they give conflicting evidence. These found that switching Plan B to BTC status was associated with: a decrease in abortions and an increase in STI rates for women ages 15-29 (Oza 2009); an increase in the lifetime number of sexual partners, frequency of sexual activity, and incidence of STIs (Zuppann 2010); and no change fertility or abortion (Gross, Lafortune et al. 2011).

4. Conceptual Bases for the Effect of Increased Emergency Contraception Access

In contrast to the clinical literature, economists have reason to be unsure about the net effect of increased ECP access on sexual activity, the incidence of multiple sexual partners and the incidence of unprotected sex with multiple partners. Naturally, since BTC access to ECP lowers the pregnancy risk of unplanned or unprotected sexual activity, BTC access lowers the opportunity cost of these actions. Thus, from a pure cost standpoint, one would certainly expect that greater access to ECPs should increase the frequency of risky behaviors. This is the central thesis of Zuppann (Zuppann 2010).

However, economists have also modeled sexual risk-taking behavior as the outcome of a bargaining process between men and women (Gertler, Shah et al. 2005). While these authors, nor any others of which we are aware, have directly modeled sexual bargaining in the context of EC, we can take the existing models as a starting point, and consider what an economic model of EC bargaining would entail. Consider that when women and men are dating they are implicitly negotiating mutually agreed-upon sexual practices. To explore this, we assume that women are seeking sexual encounters with lower probabilities of pregnancy and reduced probability of STIs via protected (condom) sex, and we assume men are more commonly seeking sexual encounters without a condom, and care less about the pregnancy risk. Women and men then engage in negotiation to determine whether their sexual encounter will take place and whether it will involve protection (a condom). For this negotiation both women and men face the same outside option which can be unilaterally imposed by either – which is no sexual activity.

Whether an encounter occurs and whether it involves a condom will then depend in part on the relative bargaining power of the man and woman engaged in negotiation.

We consider the decision process of the women in the negotiation. Given her preferences she must decide how many individuals to seek as sexual partners – where zero is one option – and then engage in negotiations with each over the terms of the sexual encounter(s) in order to maximize her expected utility (if the outcomes of each negotiation cannot be known *ex ante*). The decision regarding number of partners and acceptable frequency of unprotected sex will depend on her beliefs about the risk of pregnancy (or an STI) from each encounter and her belief about her own relative bargaining power. Her bargaining power in each independent negotiation will depend, in part, on her ability to credibly threaten to impose the outside option of no sex. The greater the objective (or at least bilaterally perceived) risk of pregnancy from an unprotected sexual encounter, the more credible will be the woman's threat of imposing the outside option, and then the greater will be her bargaining power for protected sex with her potential male partner.

In this context, greater availability of emergency contraception may be detrimental to the woman's bargaining power. Consider that when a state, or the FDA, permits Plan B to be sold over the counter, both sides of the sexual negotiation will know that the woman can more easily access effective contraception after an unprotected sexual encounter, so that the pregnancy risk from unprotected sex is reduced. Since the woman's potential partner knows pregnancy may be prevented without a condom then her threat to impose the external (no sex) option because of pregnancy risk is less credible. This implies a lower relative bargaining power in negotiating protected vs. unprotected sex for the woman. This should raise the proportion of the time that unprotected sex obtains as the negotiated outcome. Having rational expectations, the woman will anticipate this compromised bargaining power *ex ante* when ECP becomes available BTC. A rational response to reduced bargaining power would be for the woman to choose to sample fewer potential sexual partners; in the limit, this would lead her to control the risk of pregnancy (and STI) by monogamy or no sexual activity at all. Thus, this bargaining framework would suggest that we may expect to see women choosing less sexual activity or fewer sexual partners in the face of their state, or the FDA, switching emergency contraception to BTC.

What of the women who realize their bargaining power is reduced because of increased access to ECP but who continue to opt for multiple sexual partners? With reduced bargaining power - and given they are negotiating with men who are seeking, everything else equal, unprotected sex - the frequency of unprotected sex should increase for these women.

In summary, women can mitigate the STI risk from sexual activity in three broad ways: abstinence, sexual activity in a bilaterally monogamous relationship, or consistent use of condoms. Opponents worry that expanding ECP access may increase exposure to STIs because of moral hazard from the lower opportunity (pregnancy) cost of risky sexual behaviors: if women who normally use condoms for contraception know they can easily access emergency contraception to protect against pregnancy, they may be less likely to use condoms. However, there is also a theoretical reason to expect that some risky activities might fall when EC is switched to BTC status. Which effect dominates is an empirical question.

5. Methods

5.1 Data

For this study, we used data from the National Longitudinal Study of Youth, 1997 (NLSY97), a nationally representative longitudinal survey of youth. We obtained access to the

NLSY state-level geocode sample and are able to identify respondents by state of residence in each wave of the data. As discussed above, since eight states implemented BTC access to emergency contraception prior to the 2006 FDA policy change, we need state identifiers to correctly measure BTC status for Plan B.[3] Since all women in our sample are over 18 years old in all waves, we measured BTC access as a 1 if the observation is from a state after BTC access is implemented, or anywhere in the U.S. after August of 2006. NLSY97 respondents were contacted approximately annually in waves from 1997 through 2009. The survey collected detailed information on dating, marriage, fertility, and sexual activity, and a rich set of control variables. Table 2 presents the means and standard deviations for our dependent, mediating and control variables.

We restricted our analysis to data from August 1999 to 2009. Since Plan B was approved for prescription distribution in July 1999 (F.D.A.) and Wave 3 of the NLSY97 begins in October 1999, Plan B is available for all time periods in our sample. Also, all but one of the state-level policy changes occurred during this period (the exception being Washington state, which implemented the policy in February 1998). Also, since the determinants of sexual behavior for teenagers may differ from those of older women, we wanted to restrict the sample to women aged 18 or older; after 1999 most women in the NLSY97 were older than 18.

5.2. Basic Empirical Model

We modeled three binary outcome variables: whether or not the respondent reported any sexual activity; whether or not the respondent reported sexual activity with more than one

[3] Note that since Washington state had BTC access for ECP for the entire time frame of our data, we excluded all observations from that state. Further, Alaska and Hawaii are unusual states compared to the "Lower 48" (indeed, in many parts of Alaska "morning after" access to a pharmacy may not be feasible), and so we also exclude observations from those states. Our empirical findings are extremely robust to the inclusion or exclusion of these three states.

partner; and whether or not the respondent reported any unprotected sex conditional on having multiple sexual partners. All three are measured since the date of last interview. We estimate our models three times: once for the full sample of women; once for the sub-sample of married or cohabitating women; and once for the sub-sample of single women. Overall, 84.7% of respondents report sexual activity since the date of last interview, with rates of 94.8% for married or cohabitating and 78.7% for single women. Given the extremely high rate of any sexual activity among cohabitating and married women, we will model the sexual activity choice only for single women. Approximately 38.6% of the sexually active respondents reported multiple sexual partners, and 47.8% of the responses with multiple sexual partners reported at least one unprotected sexual encounter (sex without a condom). (See Table 3).

As a preliminary analysis, we compared average rates of any sexual activity, multiple sexual partners, and unprotected sex with multiple partners using simple t-tests comparing mean rates one year before the FDA Plan B BTC change (2005) to those one year after the FDA change (2007). These t-tests ignore the effects of the individual state-level changes but do give an idea of the aggregate effect of the FDA policy change in 2006 on women's sexual behaviors. Bivariate analyses obviously ignore many confounding factors (e.g., respondent age), and also ignore the contribution of state BTC policies to the sexual decision-making of women in those states. This latter issue will bias any measured differences toward zero, since some women in the "before" (2005) period will actually have BTC access to Plan B from their state policies.

Thus, we also modeled the probability that a woman reported any of these behaviors using linear probability models with individual fixed effects[4] and random effects logistic regression. The general specification for this set of regressions is:

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[4] The non-linear logit estimator with fixed effects had difficulty with convergence – and often remained lodged in regions of non-concavity of the hessian (covariance) matrix. Given the well-known sensitivity of

$$B_{it} = \boldsymbol{P}_{st}\alpha + \boldsymbol{X}_{it}\beta + \delta_{st} + \mu_{it} + \varepsilon_{it} \qquad (1)$$

where B_{it} represents the behavior (either sexual activity, multiple partners, or unprotected sex conditional on multiple partners) of the i^{th} respondent at time t, $\boldsymbol{P}_{st}$ represents the policy in state s at time t (although in 2006 with the federal change this indicator variable switches on for all states), $\boldsymbol{X}_{it}$ is a vector of individual characteristics in time t, δ_{st} are state-specific time trends, μ_{it} signifies either the individual fixed or random effect, and ε_{it} is an i.i.d. error term.

Again, we estimated these behaviors two ways: first using a linear probability model with individual fixed effects and second using random effects logistic regression. We used individual fixed effects regression to account for the potential endogeneity bias that could be introduced due to individual-level and time-invariant unobservables. The random effects model also accounts for individual unobserved heterogeneity by controlling for unobserved time varying individual characteristics, so long as the unobserved individual heterogeneity is uncorrelated with the observed regressors in the model. We will compare the parameters from these two approaches to both assess the degree to which individual unobservables drive the findings and to determine upper and lower bounds for the effect of the policy changes on behaviors. For ease of comparison, the random effects logit results are presented as marginal effects. We estimate the fixed and random effects models for three groups of women: 1) the full sample of women 2) a sub-sample of married or cohabitating women 3) and a sub-sample of single women.

Marginal effects, which reflect the impact of a one-unit change in the independent variable on the probability of a positive outcome, are one meaningful way to presents results. For

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logit models to fixed effects (and indeed, the lack of a closed-form solution to the standard probit likelihood function with fixed effects), we opted for a linear probability model for our fixed effects specification.

example, the marginal effect of the Plan B going BTC for the multiple partner logit would reflect the percentage change in the probability of a woman reporting having multiple sexual partners after Plan B went BTC. The coefficients from our fixed effect linear probability model coefficients are directly interpretable as marginal effects. Coefficients from longitudinal logit models are not directly interpretable in this way. For ease of comparison and interpretation we presented our random effects longitudinal logits results as marginal effects calculated at the means of the data (or sub-sample). All models were estimated using either the 'xtreg' or the 'xtlogit' command and all marginal effects were calculated using the 'margins' command, in Stata 12.

5.3. Sensitivity Analyses

Although not reported, we also estimated instrumental variables (two-stage residual inclusion (2SRI)) versions of all models to further account for the potential endogeneity of two of our predictor variables (health insurance and employment). One might worry about these variables in particular. Both could be argued to represent choices on the part of the respondent (though whether those choices are correlated with sexual behaviors is arguable). If health insurance and employment are choices correlated with the error in (1) it is probably because of the unobservable individual risk preferences. More risk averse women are more likely to purchase health insurance and simultaneously less likely to engage in risky sexual behaviors. Employment decisions may also be related to risk preferences.

Once response to the potential endogeneity would be to exclude these predictors from our models; however, they are statistically significant predictors of women's sexual behaviors. Alternatively, we could implement IV. We estimated IV versions of the models using county-specific all-cause death rates as an instrument for health insurance (women in counties with greater death rates should have a higher demand for insurance, ceteris paribus) and using average annual county level unemployment rates and labor force size as instruments for employment. These variables passed the usual tests for weak instruments (they were individually statistically significant in the first stage, and the partial F-statistics were high); however only a few of the first-stage predicted residuals were statistically significant in the second stage, which indicated that endogeneity is not a systematic problem. Additionally, the sign and significance patterns on the policy variables of interest did not change when the IV estimators were used (though the precision with many other variables were estimated fell). Thus, there is little evidence that assuming exogeneity with regard to health insurance and employment status introduces bias; further, it is unlikely that the federal and state emergency contraception variable is correlated with individual health insurance and employments statuses. Finally, as long as risk preferences are relatively stable within individuals over time, any residual insurance or employment variable endogeneity from omitting risk preference measures should be alleviated by the fixed and random effects.

Finally, we conducted sensitivity analyses to explore whether the emergency contraception BTC policy variable parameters might be biased through the inclusion of the state-level measures of policy adoption. This might occur if states adopted these polices due to some state-level unobservables that are not captured in our regressions but which are correlated with the average sexual practices of women in the state; examples include states with higher than average abortion rates or unintended pregnancy rates. If this is the case, then including the state-level policy changes in our definition of EC BTC status might lead to bias in our estimated policy marginal effects. On the other hand, the federal-level policy change is arguably exogenous to behaviors in any individual state. To assess the degree to which this is a problem, we estimated our models excluding observations from the eight states that adopted their own BTC EC laws. We did not find any meaningful differences between the parameters with the policy-changing states included or excluded, so we conclude that the potential for bias associated with including the state-level policies in our measure of EC policy change is quite low. The complete set of sensitivity analyses are available upon request.

The main study variable of interest in our analysis is the absorbing indicator variable for whether the observation was in a state and a period on or after either the state-level policy change[5] or the period on or after the date that the FDA allowed Plan B to be sold behind-the-counter (August 2006 and after). We also included an indicator variable to control for the shock of Preven (another FDA approved prescription-only emergency contraceptive pill) being withdrawn from the market in May of 2004. To capture this shock, we included an indicator variable for the first three months after Preven's discontinuation. Other control variables included in each regression were: respondent age, pregnancy history, self-reported health status, health insurance status, household income, marital or cohabitating status, self-assessed overweight status, race/ethnicity, urbanicity, education, and state-specific time trends.

6. Results

Table 4 presents the simple "before/after" t-tests on changing rates of sexual behaviors surrounding the FDA Plan B BTC switch. Single women exhibited a drop in the frequency of any sexual activity of -11.8% (p<0.01). Married or cohabitating and single women saw a drop in

[5] This applies to the following states which implemented a behind-the-counter policy for EC prior to the federal ruling: California, New Mexico, Maine, Massachusetts, and New Hampshire. We exclude Washington from our analysis because the policy is enacted for all periods of our data. We also exclude Alaska and Hawaii.

the rate of multiple sexual partners of -3.4% ($p<0.05$) and -10.3% ($p<0.01$), respectively. Finally, the simple difference in means test suggested that comparing 2005 to 2007, single women experienced an increased rate of unprotected sex conditional on multiple partners, of +15.9% ($p<0.01$). These unprotected sex differences were illustrated in Figure 1 (which also included data for 2003 and 2009, which were not part of the t-test).

The marginal effects of the Plan B BTC switch from the fixed effects linear probability models and the random effects longitudinal logits are presented in Table 5. With regard to the fixed effects linear probability model results, we found that switching Plan B to BTC status decreased the likelihood of any sexual activity by -5.2% ($p<0.01$) for the full sample and -11% ($p<0.01$) for single women. For the random effects longitudinal logit results, we found that switching Plan B to BTC status decreased the likelihood of any sexual activity by -4.7% ($p<0.01$) for the full sample and -9.1% ($p<0.01$) for single women. Recall that we did not estimate these models for the married or cohabitating sub-sample since nearly 95% of these women are sexually active. There was no effect of the Preven discontinuation shock on the probability of sexual activity for any of these specifications.

The results for the models predicting the probability of multiple partners, conditional on sexual activity are presented in Table 6. For the fixed effects linear probability models, we found that the Plan B BTC switch lowered the probability of multiple partners by -3.5% ($p<0.05$) for single women. Using the random effects longitudinal logit models, we found that the Plan B policy change lowered the probability of multiple partners for the full sample by -4.4% ($p<0.01$) and by -8% ($p<0.01$) for the single sub-sample. Again, we found no evidence of an effect of the Preven discontinuation on sexual behaviors.

Finally, the results for the models predicting the probability of unprotected sex (no condom), conditional on multiple partnerships are reported in Table 7. The fixed effects linear probability model results indicated that switching Plan B BTC increased the probability of unprotected sex by +4.7% ($p<0.01$) for the full sample and +6.1% ($p<0.01$) for single women. The findings from the random effects models indicated that the Plan B BTC switch increased the likelihood of unprotected sex for women with multiple partners by +9.8% ($p<0.01$) for the full sample, by +9.8% ($p<0.10$) for married or cohabitating women, and by +11% ($p<0.01$) for single women. In the fixed effects linear probability model, we did find evidence that the Preven discontinuation was associated with a +14.4% ($p<0.05$) increase in the probability of unprotected sex conditional on multiple partners for the married or cohabitating sub-sample. These results were consistent with the concern that increased access to emergency contraception may have been associated with higher likelihood that some women engaged in unprotected sex, which implied unintended negative consequences for these women by putting them at increased risk for contracting STIs.

7. Discussion

We found that the state policy changes prior to the August 2006 FDA decision and the FDA policy change to switch Plan B to BTC status was associated with an increased probability of sex without a condom for single women with multiple partners. This suggests that increased access to emergency contraception introduced moral hazard by discouraging some women in this group from taking preventive actions by using a condom. Increased moral hazard increased the risk of STIs. However, we also found that increased access to ECPs was associated with a decreased probability that single women had any sexual activity and had multiple sexual partners. Further, note that our estimates of the impact of the Plan B BTC switch on the probability of multiple partners ranged from -3.5% to -8% (depending on the specification) for the 3,567 single women in our sample, while it increased the frequency of unprotected sex between 6.1% and 11% (depending on the specification) for only 2,921 single women who actually did have multiple partners; thus, the increased unprotected sex effect impacted a much smaller population than the reduced multiple partner effect did.

Why might increased ECP access lead to lower rates of any sexual activity and multiple sexual partners? After all, as Zuppann argues, greater access to ECPs should lower the opportunity cost of sex outside of monogamous relationships and so increase the frequency of these behaviors (Zuppann 2010). However, as we discussed above, it is also the case that women and men when dating are implicitly negotiating what their sexual practices will be. If women are less able to negotiate protected sex – because their partners know that pregnancy can be avoided even without a condom (in the event of an unplanned encounter when a woman is not using daily birth control) – then many women will have to choose between unprotected sex, monogamous sex, or no sex; it is plausible that many of these women may opt for monogamy or "no sex." Our results are consistent with the "lost negotiating power" effect dominating the "lower opportunity cost" effect: when ECPs became more available, more women chose to abstain (for single women) and enter monogamous relationships. Further, the women who continued to report multiple sexual partners with easier ECP access also show evidence of decreased bargaining power: they were more likely to have unprotected sex.

This study includes several limitations. First, because of the wording of the survey questions, we cannot know if women who report multiple partners in one year are in non-monogamous relationships or are moving from one monogamous relationship to another.

Having concurrent sexual relationships without condom use puts women at higher risk for contracting an STI than serial monogamy without condom use. We also cannot know whether women who are married or cohabitating and report multiple partners and unprotected sexual activity are always using protection outside their main relationship and limiting unprotected sexual activity to their main relationship; this would not put them at as much risk for STIs as engaging in unprotected sex outside the main relationship.

When considering the unintended consequences of making Plan B available BTC on STI risk for women, policy-makers may focus on women with multiple partners. If women in monogamous relationships substituted away from condoms once access to Plan B was increased, there are few public health issues. However, if women with multiple partners substituted away from condoms after Plan B was BTC, the consequences for STI transmission are more substantial. Emergency contraception may decrease unintended pregnancy and abortion rates by offering women an option for contraception after another method fails or following unplanned unprotected sex; for public health policy, these benefits must be weighed against the potential STI exposure associated with increased risky sexual practices in some groups of women.

Thus, it would be a mistake to conclude that our results suggest that the shift toward greater access to emergency contraception from reclassifying Plan B as an BTC drug was harmful. Some women were less exposed to STI risk after the BTC switch, and some women (likely fewer) were more exposed. However, there would appear to be gains that could still be made from education. Public service messages could continue to educate women about the multiple risks from unprotected sex; even if emergency contraception can mitigate one risk, it is ineffective against STIs. In addition, the FDA could mandate that the *Plan B One Step* package display a prominent warning on the front – like the boxed statements on some prescription drugs and on cigarette packages – reminding women that the product conveys no protection against STIs. (It currently does not have this warning.) These types of policies have been effective in other public health areas in the past, and could fruitfully compliment the public health goal of reducing unintended pregnancy through greater access to emergency contraception.

Figure 1:

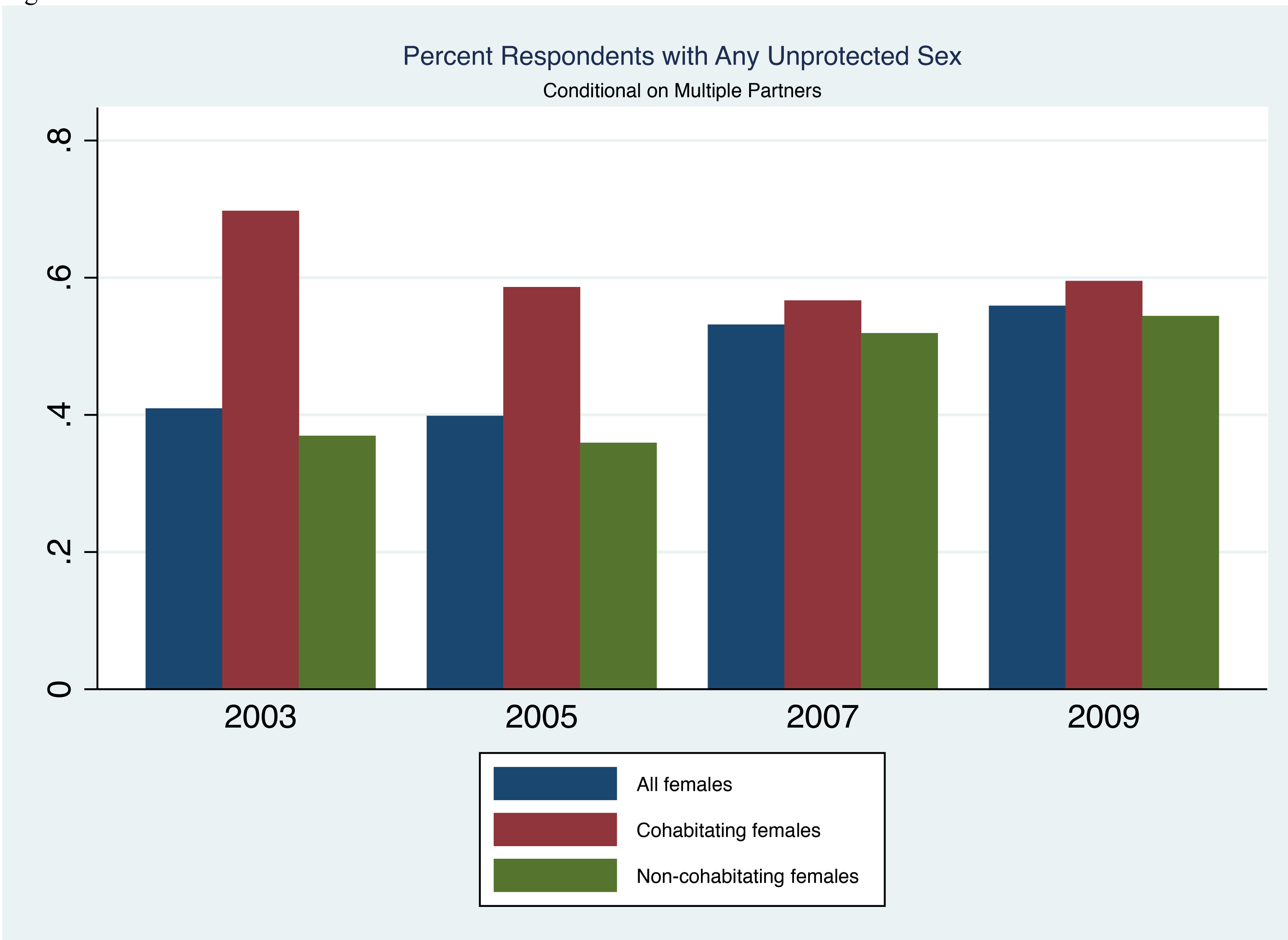
Percent Respondents with Any Unprotected Sex
Conditional on Multiple Partners
0
.2
.4
.6
.8
2003
2005
2007
2009
All females
Cohabitating females
Non-cohabitating females

Table 1: Dates of State-Level Behind the Counter Access Implementation

State	Date Law Implemented
Washington	2/1/1998
California	1/1/2002
Alaska	12/18/2002
Hawaii	7/11/2003
New Mexico	10/30/2003
Maine	7/30/2004
Massachusetts	12/14/2005
New Hampshire	8/15/2005
Vermont	9/30/2007

Table 2: Variable Means and Standard Deviations

	Mean	Standard Deviation
Sexually active	0.847	0.360
Respondent had multiple sex partners	0.386	0.487
Respondent had any unprotected sexual encounters conditional on multiple partners	0.478	0.500
ECP available BTC	0.402	0.490
Preven withdrawn from market	0.002	0.042
Age	22.56	2.934
Respondent has had pregnancy in past	0.440	0.496
Respondent reports health good or better	0.605	0.489
Respondent has health insurance	0.730	0.453
Respondent is employed	0.603	0.489
Household income (in $10K)	5.294	5.144
Self-assessed as overweight	0.506	0.500
African-American	0.294	0.456
Hispanic	0.215	0.411
Other race	0.034	0.181
Living in urban setting	0.775	0.417
Has associates degree	0.042	0.200
Has undergraduate degree	0.120	0.325
Has graduate degree	0.012	0.108
Year	2004.8	2.783
Observations	34030	

All NSLY97 women over 18 at time of survey round. Frequency and standard deviation for unprotected sexual encounters are calculated on the sub-sample of 11,118 person/year observations who reported multiple partners. Categorical variables are coded 1=Yes, 0=No; means are the fraction of respondents who report “Yes.

Table 3: Average Rates of Sexual Activity, Multiple Partners, Unconditional and Conditional Unprotected Sex

	(1) All Females	(2) Married or Cohabitating Sample	(3) Single Sample
	Mean [Number in Sub-Sample]	Mean [Number in Sub-Sample]	Mean [Number in Sub-Sample]
Sexually active	0.847	0.948	0.787
	[N=34030]	[N=12616]	[N=21414]
Respondent had multiple sex partners	0.386	0.164	0.543
	[N=28821]	[N=11965]	[N=16856]
Respondent had any unprotected sexual encounters and multiple partners	0.478	0.622	0.447
	[N=11118]	[N=1965]	[N=9153]
Observations	34030	12616	21414

All NSLY97 women over 18 at time of survey round.

Table 4: T-Tests for Changes in Rates of Sexual Activity, Multiple Partners, Conditional Unprotected Sex, from 2005 to 2007

	(1) All Females	(2) Married or Cohabitating Sample	(3) Single Sample
	Mean (T-Test on Difference) [Mean for 2005] [Mean for 2007]	Mean (T-Test on Difference) [Mean for 2005] [Mean for 2007]	Mean (T-Test on Difference) [Mean for 2005] [Mean for 2007]
Sexually active	-.052*** (-6.96) [.847] [.899]	.002 (.224) [.949] [.948]	-.118*** (-10.17) [.750] [.868]
Respondent had multiple sex partners	-.123*** (-10.56) [.309] [.432]	-.034** (-2.60) [.148] [.182]	-.103*** (-6.16) [.502] [.604]
Respondent had any unprotected sexual encounters and multiple partners	.133*** (6.68) [.532] [.399]	-.016 (-.368) [.569] [.585]	.159*** (7.09) [.519] [.360]
Observations	7814	3415	4399

All NSLY97 women over 18 at time of survey round.
* p<0.10, ** p<0.05, *** p<0.01

Table 5: Marginal Effects for Models for Probability of Any Sexual Activity

	All Females - FE LPM	All Females - RE Logit	Single Sub-Sample – FE LPM	Single Sub-Sample – RE Logit
ECP available OTC	-0.052***	-0.047***	-0.11***	-0.091***
	(-7.70)	(-8.76)	(-10.39)	(-10.23)
Preven withdrawn from market	0.037	0.067	0.028	0.13
	(0.94)	(1.45)	(0.47)	(1.45)
Age	-0.017***	-0.0040***	-0.020***	-0.0058**
	(-3.54)	(-2.68)	(-2.72)	(-2.40)
Respondent has had pregnancy in past	0.052***	0.033***	0.11***	0.055***
	(6.52)	(7.21)	(7.96)	(7.24)
Respondent reports health good or better	0.00053	0.0048	-0.0014	0.0037
	(0.11)	(1.50)	(-0.18)	(0.70)
Respondent has health insurance	0.017***	0.016***	0.022***	0.022***
	(3.54)	(4.77)	(3.03)	(4.25)
Household income (in $10K)	0.0012***	0.0017***	0.00078	0.0018***
	(2.88)	(5.00)	(1.26)	(3.62)
Currently married and living together	0.11***	0.10***		
	(14.61)	(14.88)		
Cohabitating without marriage	0.077***	0.075***		
	(12.66)	(13.33)		
Self-assessed as overweight	0.0021	0.0029	0.0076	0.0051
	(0.38)	(0.84)	(0.86)	(0.92)
African-American		-0.043***		-0.055***
		(-7.05)		(-5.78)
Hispanic		-0.034***		-0.033***
		(-4.99)		(-2.98)
Other race		-0.0061		-0.0096
		(-0.47)		(-0.47)
Living in urban setting	-0.0034	0.0014	-0.0062	-0.00099
	(-0.52)	(0.31)	(-0.54)	(-0.13)
Has associates degree	-0.021	0.0064	-0.024	-0.0032
	(-1.53)	(0.75)	(-1.16)	(-0.23)
Has undergraduate degree	-0.036***	-0.0028	-0.036***	-0.013
	(-4.32)	(-0.55)	(-2.98)	(-1.56)

Has graduate degree	-0.011	0.026*	-0.0014	0.016
	(-0.53)	(1.78)	(-0.04)	(0.69)
Constant	-17.6*		-24.0*	
	(-1.89)		(-1.67)	
State specific time trends	Yes	Yes	Yes	Yes
Observations	27734	27734	16309	16309

Estimated for the NLSY97 female population over 18 at time of survey.
* p<0.10, ** p<0.05, *** p<0.01

Table 6: Marginal Effects for Models for Probability of Multiple Sexual Partners

	All Females - FE LPM	Married or Cohabitating Sample - FE LPM	Single Sample - FE LPM	All Females – RE Logit	Married or Cohabitating Sample – RE Logit	Single Sample – RE Logit
ECP available OTC	-0.016	0.010	-0.035**	-0.044***	-0.0054	-0.080***
	(-1.51)	(0.80)	(-2.23)	(-3.80)	(-0.46)	(-4.19)
Preven withdrawn from market	-0.0028	0.047	0.025	-0.020	0.0064	-0.018
	(-0.05)	(0.53)	(0.31)	(-0.34)	(0.08)	(-0.19)
Age	-0.040***	-0.034***	-0.044***	-0.014***	-0.0093***	-0.017***
	(-5.56)	(-3.55)	(-4.04)	(-4.33)	(-3.20)	(-3.19)
Respondent has had pregnancy in past	-0.040***	-0.039**	-0.059***	-0.063***	-0.012	-0.11***
	(-3.37)	(-2.48)	(-2.90)	(-6.82)	(-1.34)	(-6.84)
Respondent reports health good or better	-0.015**	-0.013	-0.0091	-0.027***	-0.028***	-0.028**
	(-2.16)	(-1.41)	(-0.83)	(-3.73)	(-3.89)	(-2.31)
Respondent has health insurance	-0.039***	-0.041***	-0.046***	-0.051***	-0.042***	-0.068***
	(-5.25)	(-4.11)	(-4.13)	(-6.75)	(-5.42)	(-5.44)
Household income (in $10K)	-0.000048	0.00016	0.000081	-0.00011	-0.00077	-0.00025
	(-0.08)	(0.16)	(0.09)	(-0.18)	(-0.95)	(-0.25)
Currently married and living together	-0.32***			-0.42***		
	(-28.75)			(-41.96)		
Cohabitating without marriage	-0.27***			-0.30***		
	(-30.29)			(-36.72)		
Self-assessed as overweight	-0.027***	-0.033***	-0.025*	-0.025***	-0.033***	-0.014
	(-3.20)	(-2.95)	(-1.91)	(-3.21)	(-4.35)	(-1.13)
African-American				-0.0039	0.056***	-0.056***
				(-0.32)	(4.87)	(-2.84)
Hispanic				-0.042***	-0.010	-0.082***
				(-3.05)	(-0.84)	(-3.50)
Other race				-0.0011	0.022	-0.023
				(-0.04)	(0.88)	(-0.54)
Living in urban setting	0.0052	-0.018	0.049***	0.014	0.0038	0.034**
	(0.53)	(-1.40)	(2.87)	(1.48)	(0.42)	(2.02)
Has associates degree	-0.025	0.046	-0.040	-0.037**	-0.021	-0.056*
	(-1.26)	(1.54)	(-1.31)	(-2.01)	(-1.24)	(-1.82)
Has undergraduate degree	-0.055***	0.054**	-0.070***	-0.066***	-0.056***	-0.078***
	(-4.33)	(2.17)	(-3.85)	(-5.58)	(-4.18)	(-4.13)

Has graduate degree	-0.047	0.055	-0.026	-0.062*	-0.084**	-0.0051
	(-1.57)	(1.19)	(-0.49)	(-1.92)	(-2.53)	(-0.09)
Constant	-62.6***	-50.9***	-71.9***			
	(-4.46)	(-2.69)	(-3.38)			
Observations	24317	10811	13506	24317	10811	13506

Estimated for the NLSY97 female population over 18 at time of survey.
p<0.10, ** p<0.05, *** p<0.01

Table 7: Marginal Effects for Models for Probability of Unprotected Sex and Multiple Partners

	All Females - FE LPM	Married or Cohabitating Sample - FE LPM	Single Sample - FE LPM	All Females – RE Logit	Married or Cohabitating Sample – RE Logit	Single Sample – RE Logit
ECP available OTC	0.047***	0.050	0.061***	0.098***	0.098*	0.11***
	(2.66)	(0.94)	(3.09)	(4.16)	(1.77)	(4.12)
Preven withdrawn from market	0.077	1.44**	-0.027	0.073	3.75	-0.037
	(0.88)	(2.53)	(-0.28)	(0.64)	(0.00)	(-0.30)
Age	0.020	0.049	0.018	0.012*	-0.0096	0.012*
	(1.61)	(1.30)	(1.36)	(1.86)	(-0.74)	(1.72)
Respondent has had pregnancy in past	0.030	0.038	0.036	0.21***	0.071*	0.23***
	(1.29)	(0.59)	(1.27)	(11.17)	(1.79)	(11.27)
Respondent reports health good or better	-0.0030	0.036	-0.0074	-0.025*	0.025	-0.031*
	(-0.24)	(0.95)	(-0.53)	(-1.70)	(0.76)	(-1.90)
Respondent has health insurance	-0.00081	-0.00034	-0.011	-0.0045	0.057	-0.015
	(-0.07)	(-0.01)	(-0.79)	(-0.30)	(1.61)	(-0.90)
Household income (in $10K)	0.0017	0.0070*	0.0015	0.0034***	0.010**	0.0030**
	(1.63)	(1.74)	(1.31)	(2.69)	(2.56)	(2.24)
Currently married and living together	0.055*			0.081***		
	(1.95)			(2.74)		
Cohabitating without marriage	0.051***			0.14***		
	(2.78)			(6.35)		
Self-assessed as overweight	0.063***	0.15***	0.062***	0.078***	0.16***	0.063***
	(4.22)	(3.29)	(3.72)	(4.94)	(4.69)	(3.64)
African-American				-0.23***	-0.22***	-0.22***
				(-9.50)	(-4.53)	(-8.54)
Hispanic				-0.18***	-0.21***	-0.16***
				(-6.21)	(-4.07)	(-5.11)
Other race				-0.16***	-0.100	-0.17***
				(-2.99)	(-0.91)	(-2.96)
Living in urban setting	0.011	-0.045	0.0089	0.034*	0.011	0.034
	(0.63)	(-0.76)	(0.41)	(1.65)	(0.25)	(1.49)
Has associates degree	0.049	0.023	0.062	0.038	-0.066	0.058
	(1.30)	(0.14)	(1.48)	(0.96)	(-0.80)	(1.32)
Has undergraduate degree	0.044*	0.11	0.043*	0.11***	-0.066	0.13***
	(1.96)	(0.70)	(1.80)	(4.47)	(-0.99)	(4.81)

Has graduate degree	0.084	0.070	0.11	0.23***	0.21	0.25***
	(1.32)	(0.22)	(1.55)	(3.02)	(1.08)	(3.15)
Constant	38.9	137.5*	38.5			
	(1.64)	(1.87)	(1.46)			
Observations	9258	1720	7538	9258	1720	7538

Estimated for the NLSY97 female population over 18 at time of survey. Conditional on multiple partners
* p<0.10, ** p<0.05, *** p<0.01

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