Abortion Costs, Separation and Non-Marital Childbearing*

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Abstract

How do abortion costs affect non-marital childbearing? While greater access to abortion has the first-order effect of reducing childbearing among pregnant women, it could nonetheless lead to unintended consequences via effects on marriage market norms. Single motherhood could rise if lower-cost abortion makes it easier for men to avoid marriage. We identify the effect of abortion costs on separation, cohabitation and marriage following a birth by exploiting the "miscarriage-as-a-natural experiment" methodology in combination with changes in state abortion laws. Recent increases in abortion restrictions appear to have lead to a sizable decrease in a woman's chances of being single and increased the chances of cohabitation. The result underscores the importance of the marriage market search behavior of men and women, and the positive and negative effects of abortion laws on bargaining power for women who abort and give birth respectively.

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How do abortion costs affect non-marital childbearing? Following *Roe vs. Wade* in 1973, both non-marital childbearing and abortion incidence increased significantly in the Unite States. The theory of Akerlof, Yellen and Katz (1996) reconciles these seemingly paradoxical trends by introducing externalities in the marriage market. If lower abortion costs place the onus of the childbearing decision on the woman, thereby making it easier for men to leave their partners, a pregnant woman (who does not avail herself of the lower cost abortion) may instead become a single mother. At issue is whether couples' decision to continue their relationships following a non-marital birth is influenced by abortion costs. This implication of integrating strategic search behavior into a dynamic marriage market model has yet to be tested.[1] Beyond the social sciences, the question of whether abortion laws generate externalities is important for policy makers in light of the large number of children in single-parent households and the dramatic recent changes in cohabitation and marriage patterns in the modern marriage market.

The existing literature on abortion laws has focused on first order effects: we know that the demand for abortions responds to incentives, leaving open the possibility for consequences beyond these first-order effects.[2] More nuanced effects, like those involving male behavior, have been confined to the theoretical realm. Indeed, to support their theory Akerlof, Yellen and Katz (1996) look only at time-series data and descriptive statistics thus failing to grapple with the many unobservables that could be simultaneously driving both abortion access and non-marital childbearing. Formally we cannot test a theory of the post-legalization diminution of norms because we are unlikely to observe as large a cost change as legalization again in the U.S. However, we can test for whether current norms governing relationship

[1]Important contributions have been made integrating a search framework with cohabitation by Brien, Lillard and Stern (2006), and in examining partner substitution in a discrete choice setting by Choo and Siow (2006).

[2]Haas-Wilsom (1996), Levine, Trainor and Zimmerman (1996), Blank, George and London (1996), Bitler and Zavodny (2001), and Levine (2003) all measure the impact of state-level laws on abortion, birth and sexual behavior, but not marriage. Findings tend to be consistent with economic theory: public funding increases abortion; restrictions such as parental consent reduce it. These laws are particularly relevant for minors (Haas-Wilsom (1996), Girma and Paton (2011)). Girma and Paton (2011) exploits the timing of access to emergency birth control (EBC) in northern Britain and shows that increases in EBC lead to increases in sexually transmitted infections, with mixed evidence about the effect on pregnancies.

status following a birth responded to recent changes in access to abortion, allowing us to say whether norms similar to those outlined in Akerlof, Yellen and Katz (1996) are still operative.

The fundamental question we aim to answer is whether, conditional on pregnancy, women giving birth in areas with lower abortion costs see a higher probability of dissolution with the biological father? The comparison is two-fold: comparing women in low-cost areas versus higher cost areas and comparing those who give birth relative to those who do not give birth. The focus is on the interaction between these two in order to determine if giving birth and facing higher abortion costs interact to decrease the chances of dissolution. To estimate this effect we must overcome at least two major sources of endogeneity: unobserved differences in the marriage market across areas with high and low costs, and choosing to give birth or not. Since, abortion access is not independent of marriage market conditions so we exploit within-state variation over seven-year time period in public funding and parental consent laws to shift costs. Since the choice to give birth is conditional a partner's interest in having a child, birth is endogenous with respect to relationship status. To deal with this we employ the recent econometric work of Ashcraft, Fernandez-Val and Lang (Forthcoming) which outlines the conditions under which we can use miscarriage as a natural experiment.[3]

Our results imply that removing public funding actually decreased single motherhood and increased cohabitation among poor and young women. Estimates show around a 13% lower chance of being single following a birth in a state where funding was removed. This policy impact is substantial: if the entire sample were to experience a removal of abortion funding, these estimates would imply that the probability of cohabiting or marrying among low-income mothers would increase by between 12 and 18 percentage points. Among children of low-income mothers, the fraction children living with both biological parents at the time

[3]We mainly follow the insights of Hotz, McElroy and Sanders (2005) and Ashcraft, Fernandez-Val and Lang (Forthcoming), who show that using OLS and IV estimators can deliver bounds on the effects of birth on labor market outcomes for conditionally random miscarriage. Akerlof, Yellen and Katz (1996) and Kane and Staiger (1996) provide models of this information flow, which empirically leads to a simultaneity bias when we condition on birth.

of birth would rise by rouhgly 10% percentage points.

The estimates here suggest a key channel for understanding non-marital childbearing first outlined by Akerlof, Yellen and Katz (1996). Namely we find evidence that changes to abortion costs result in a spillover-effect on the relationship terms of women who give birth. This may represent differential sorting into relationships where births occur, or improved bargaining power within the relationship. Either way the pattern of results highlights the matching behavior of fathers, either before or after a non-marital birth. It seems plausible that for the couples we examine cohabitation is the relevant relationship being bargained over.[4] While we have little to add to a debate over abortion rights per se, it appears that abortion laws have consequences along the broader sequence of choices leading to single motherhood, with negative consequences for women who decide to give birth. As a first step toward understanding how the costs of abortion and relationship terms interact, we review the relevant theoretical work on non-marital childbearing.

1 Abortion Costs and Non-marital Childbearing

Two theoretical contributions to non-marital childbearing and abortion costs guide the discussion here. Both examine how a reduction in the cost of abortion changes decisions made by men versus women, ultimately leading to different predictions about the effects on non-marital childbearing. The Kane and Staiger (1996) model captures the insurance value of abortion. Price changes of differing magnitudes can generate different channels for access to reduce non-marital childbearing. In contrast, Akerlof, Yellen and Katz (1996) model a decrease in "shotgun" marriages following abortion cost declines, which reduces the incentives for male commitment.[5]

[4] In our sample marriage rates are relatively low given the age distribution, nonetheless we still see roughly one-third of these young women marry the biological father following a birth.

[5] Both models treat marriage as a commitment to maintain the relationship with the partner, presumably forgoing other relationships. In this sense we expect the predictions below to hold for cohabitation as well as marriage.

Kane and Staiger (1996) model information revealed between pregnancy and resolution, interpreted as whether or not the birth would be "legitimated" (i.e. followed by marriage). For women who prefer to remain childless unless married, access to abortion insures against single motherhood. In the model, small decreases in abortion costs are decreases in the price of insurance among the insured. As insurance costs fall, increased risky behavior increases pregnancies. Some pregnancies end in abortion, and others end in in-wedlock births. Non-marital births are perfectly avoided because of abortion services. For large decreases in abortion costs, the channels are different. Large changes can only occur if moving from higher absolute cost levels (like the pre-*Roe vs. Wade* U.S.). With prohibitively high abortion costs, some women would have been forced to have a child out of wedlock. With lower costs, they can now afford to exercise the insurance option of abortion. This change shifts births from outside to within marriage. The first two rows of Table 1 illustrate the effects of small versus large decreases in abortion costs.

In Akerlof, Yellen and Katz (1996) when abortion costs fall, women willing to use abortion are willing to have sex without a pre-commitment.[6] If this fraction of "abortion-using" women is high enough, women who will not use abortion drop their pre-commitment demands as well. This is because the two groups are competing in the marriage market for men who do not want children. Men have better outside options when abortion costs fall, so women who want children lose the bargaining power to insist on a relationship following a pregnancy.

The effects are listed in Row 3 of Table 1, where the relevant difference from the insurance model is the increase in the fraction of non-marital births.[7] In Row 3 those who would have married can now abort, lowering marital childbearing and births, and increasing abortion.

[6] Before abortion was available, a pre-commitment to marriage was the norm for dealing with non-marital pregnancies.

[7] A second model generates similar implications from different assumptions. Men value their partners altruistically, but lower-cost abortions reveal that those who fail to obtain abortions have a lower disutility of being a single parent than those who obtain abortions. The drop in abortion costs lowers the mean disutility of single mothers. A man's probability of marriage is an increasing function of this mean disutility, since he cannot believe revelations by his partner about her disutility. As costs fall, so does the disutility of single mothers and the likelihood of marriage.

Those who do not abort see a higher chance of separation, raising non-marital childbearing.[8] This higher chance of separation is one prediction we test for below.

Both modeling approaches omit something: Kane and Staiger (1996) do not allow strategic choices by men to vary systematically with abortion costs, while Akerlof, Yellen and Katz (1996)ignore the insurance value of abortion. Akerlof, Yellen and Katz (1996) predicts higher non-marital childbearing following abortion cost declines because of the increased likelihood of separation, while Kane and Staiger (1996) predicts decreases in non-marital childbearing and no increases in separation among those who give birth. The magnitude of cost changes can vary substantially with individual characteristics (e.g. being on Medicaid or a minor in states with restrictive laws), so all of these incentives (large and small) may be operating simultaneously. This suggests that given the ambiguous theoretical predictions, establishing which incentives dominate is an empirical question which we now turn toward answering.

2 Empirical Strategy

Our goal is to estimate the effect of giving birth on separation, and in particular the effect of giving birth in places where abortion costs have increased. The baseline estimation equation takes the following form:

$$Separation_i = (\gamma_b^0 + P_i\gamma_b^1 + AG_i\gamma_b^2 + AG_i \cdot P_i\gamma_b^3)'B_i + X_i'\gamma_x + \lambda(Z_i'\delta) + \varepsilon_i. \tag{1}$$

where B_i is birth, P_i is the policy change restricting access to abortion (e.g. removal of funding or imposition of parental consent), AG_i is an indicator for being in the affected group (e.g. poor or a minor), and their interaction captures the change in separation among those who give birth when abortion costs are rising. Here X_i are other controls which can

[8]In addition to altering outcomes for those having sex, the models of Akerlof, Yellen and Katz (1996) generate incentives for some women to stop and start having sex. Some women will no longer risk becoming single mothers, decreasing non-marital births. Others engage in sex as they can now afford an abortion in the event of a pregnancy, with no effect on non-marital births.

include individual, partner, community, attributes and state fixed-effects, along with the level effects of the policy and affected group indicators.

The first step in obtaining credible estimates of γ_b is to exploit policy changes influencing the respondents in Add Health, inducing policy variation in P_i. Here we use whether the state of residence changed legal regimes between Wave I (1995) and Wave III (2001). We also include Wave I state level fixed effects and pregnancy year fixed effects, to ensure variation in P_i comes from changes within-states over time. The second step is to deal with the endogeneity of B_i, for which we use an IV-strategy outlined in Ashcraft, Fernandez-Val and Lang (Forthcoming).

Ashcraft, Fernandez-Val and Lang (Forthcoming) show that one can estimate the causal effect of giving birth on outcomes for pregnant women who would not choose to abort. When outcomes are mean independent with respect to the timing of abortion, the consistent estimator is a linear combination of OLS-estimates on only those who give girth or miscarry and the IV-estimates on the entire sample. For this approach to be valid we need to assume miscarriage is conditionally random.[9]

As Ashcraft, Fernandez-Val and Lang (Forthcoming) show, the major problem using miscarriage is that abortion and miscarriage are competing risks. Assume for the moment that all abortions precede all miscarriages and births (and label this Assumption I). In such a world, miscarriages represent a conditionally random set of women who wanted to give birth. Comparing outcomes between births and miscarriages will identify the effect of birth on outcomes, and OLS is sufficient to pick up the effect since treatment is conditionally-random and not selected through abortion choices. Now suppose the opposite: all miscarriages precede all abortions (we label this Assumption II). In this world miscarriages are a random sample of women, a fraction of whom p_B wanted to give birth, and $1 - p_B$ wanted to have

[9] Conditional refers to a set of behaviors in pregnancy we observe in the data, namely smoking and drinking. Hotz, Mullin and Sanders (1997) allow for bounds on the effect of birth on outcomes when some miscarriages are non-random. We have estimated these bounds and the relevant (upper) bounds have the same sign as the results presented below.

an abortion.[10] Under Assumption II, instrumenting for birth with miscarriage delivers the impact of treatment, assuming that abortion and miscarriage have the same effect on outcomes.[11] Ashcraft, Fernandez-Val and Lang (Forthcoming) show the true effect is a convex combination of the OLS and IV estimates if the outcomes (here separation) is conditionally mean independent with respect to the timing of abortion. For our goal of signing the effect it is sufficient to (1) test for this mean independence, and if it holds (2) estimate the OLS and IV models. [12]

If the assumptions outlined are maintained, we formulate a system of equations to be estimated by instrumental variables. Here we have the following first-stage:

$$\begin{aligned} Birth_i = &\ (\rho^0_{b1} + P_i\rho^1_{b1} + AG_i\rho^2_{b1} + AG_i \cdot P_i\rho^3_{b1})M_i + X_i'\rho_{x1} + \eta_{i1} \\ Birth_i \cdot P_i = &\ (\rho^0_{b2} + P_i\rho^1_{b2} + AG_i\rho^2_{b2} + AG_i \cdot P_i\rho^3_{b2})M_i + X_i'\rho_{x2} + \eta_{i2} \\ Birth_i \cdot AG_i = &\ (\rho^0_{b3} + P_i\rho^1_{b3} + AG_i\rho^2_{b3} + AG_i \cdot P_i\rho^3_{b3})M_i + X_i'\rho_{x3} + \eta_{i3} \\ Birth_i \cdot P_i \cdot AG_i = &\ (\rho^0_{b4} + P_i\rho^1_{b4} + AG_i\rho^2_{b4} + AG_i \cdot P_i\rho^3_{b4})M_i + X_i'\rho_{x4} + \eta_{i4} \end{aligned} \tag{3}$$

where M_i is an indicator of miscarriage. The first equation corresponds to instrumenting for birth with miscarriage, the subsequent equations instrument for the interaction of birth with the policy indicator, affected group indicator, and their interaction respectively, using miscarriage and its corresponding interactions.

[10]This is the assumption put forth in Hotz, McElroy and Sanders (2005).

[11]Since we are interested in separation, we only use miscarriages prior to twenty weeks of gestation in the empirical section. Results were largely insensitive to this cut-off. **?** show a substantially higher risk of separation following a stillbirth (greater than 20 weeks gestation) than a miscarriage.

[12]OLS and IV estimates are sufficient to sign the effect since the average treatment effect takes the following form:

$$ATE = (\alpha\rho_{OLS} + (1-\alpha)\beta\rho_{IV})/(\alpha + (1-\alpha)\beta). \tag{2}$$

To calculate (α, β) requires more moments namely, (1) the fraction of women who would give birth if they did not miscarry, a "latent-birth" type (2) the fraction of women who would have a miscarried had they not aborted, a "latent-miscarriage" type (3) the fraction of women not giving birth (who either miscarry or abort) who abort. Since all three moments are (positive) probabilities, the true ATE must lie between ρ_{OLS} and ρ_{IV}.

3 Data

We use the National Longitudinal Study of Adolescent Health (Add Health), which begin surveying a large sample of teens aged 12-18 in 1995, with follow-ups in 1996, 2002 and 2008.[13] The data used here come from a retrospective history of all relationships between 1995-2001 obtained at Wave III. Partner characteristics were recorded for each relationship, and a detailed survey given about each pregnancy that occurred with that partner.

3.1 Estimation Sample

We first focus on unmarried women experiencing a first pregnancy. The sample restrictions are given as follows: beginning from a sample from female-reported first pregnancies (2728), less married at conception (2389), less stillbirths (2163).[14] Missing probability weights and geographic identifiers, and non-response further limit the final sample size to (1859) pregnancies.[15]

Table 2 gives summary statistics for two samples. The first is consistent with Assumption (I) above, and so includes only those miscarrying or giving birth. The second sample is consistent with Assumption (II), and includes births, abortions and miscarriages. We now outline a number of features of these data. The second sample we can examine abortion reporting. To check for reporting problems, Table 2 allows one to compute the abortion ratio (abortions per 1000 live births) for the estimation sample. In Table 2 the abortion ratio is 309, comparable to age specific administrative data from Centers for Disease Control (CDC (2003)), which show an age-specific abortion ratio for 15 to 24 year-olds of 330.5.[16]

[13] The design was a stratified random sample of U.S. high schools and associated middle schools; Wave I was conducted between 1994 and 1995, Wave II in 1995-1996, Wave III in 2000 and 2001 and Wave IV in 2007.

[14] Stillbirths have been documented to have larger influence on a couples' likelihood of separation. See Gold, Sen and Hayward (2010).

[15] Wherever possible, indicators were included for non-response regarding partner characteristics, which may be particularly relevant. Most non-response problems come from linking the 2001 relationship roster data with early adolescent data on puberty, and from smoking or drinking during pregnancy questions, which is of much less concern than if non-response were related to relationship characteristics. Probability weights are used to correct for unrepresentative over samples in the Add Health survey design.

[16] CDC data are drawn from 2000 and age-specific rates come only from 46 reporting areas in the U.S.

Finer and Henshaw (2006) use data from the Alan Guttmacher Institute, which maintains more accurate data than CDC. From their Table I, the 2001 age-specific ratio of abortion to pregnancies (including fetal losses) is 264.31. This sample has a ratio of 227. These estimates suggest the Add Health data capture between 86 and 93% of abortions that likely occurred to women in our sample, given that we employ probability weights from Wave III to correct for minority over sampling.[17] The percentage of pregnancies ending in miscarriages is similar to other data sources like the NLSY79 and the National Survey of Family Growth: 12%-14%.[18] While less than ideal, these reporting percentages are far better than those from other longitudinal data sources.[19]

In the upper panel of Table 2 separation, marriage and cohabitation are also listed. Separation is measured one year following conception, marriage and cohabitation are indicators for whether either occurred during the relationship.[20] Marriage and cohabitation are less frequent, and separation more frequent, when we include women obtaining an abortion. Around 5% and 1% respectively experienced a change in their state abortion laws between 1995 and 2002. The data show that 2 states in the sample removed abortion funding. The policy changes from for parental consent appear to be the result of migration. For these policy changes to be endogenous would require that minors' parents moving is influenced by their children's relationship and pregnancy outcomes, which seems unlikely. However, given how few individuals experienced a parental-consent change and that it may be related to moving, we view these as a check on the funding results whose variation is driven by more

Calculations come from Table 4 of the CDC report. The age of the Add Health Sample is roughly half 15-19 and half 20-24 in the pregnancy year. 75% of pregnancies happened in 1997-2001.

[17]We note that the surveys selection mechanism likely generates a sample which is not nationally representative of women obtaining abortions.

[18]The National Survey of Family Growth is one of the few reliable sources for miscarriage estimates. The total miscarriage rate rose slightly through the 1980s and early 1990s. Ventura, Taffel, Mosher, Wilson and Henshaw (1995) attribute this to an aging population. For the age group here they show 12-14% as well. The Add Health data still suffer from underreporting problems, but do have better reporting than the NLSY79.

[19]Lundberg and Plotnick (1995) document severe reporting problems in the NLSY79, which sees reporting rates around 60% for whites, and even lower for minorities.

[20]Separation results were nearly identical when using 9-24 months as cutoffs. Respondents were asked to combine all periods of on-again off-again sexual intercourse with the partner so that separation measures the end of all sexual contact between the (former) partners.

plausibly exogenous legal changes within each state over-time.[21] In the lower panel we see for both samples that partner characteristics include the standard two-year age gap between male and female partners. Although our sample is young, they are not solely women experiencing a teen pregnancy, a point underscored by the fact that roughly one-third marry the biological father following a non-marital birth. The partners of women who experienced a non-marital-first pregnancy, are more frequently minority men. Finally, the educational attainment at the time of pregnancy is concentrated at or below twelve years of schooling.

3.2 State-Level Policy Changes and Policy Effects

The Add Health data contain observations on state-level funding, parental consent, and waiting period laws in both 1995 and 2002. We cannot pin down the exact time of policy enactment because we do not observe the state where the pregnancy occurred. We can, however, identify whether the state of residence had different policies in 1995 and 2002. A small amount of the variation in policies evident in Table 2 results from individuals' moving states.[22] Given that state funding and parental consent laws have been shown to induce sizable cost changes for the affected demographic groups, we focus on those who were minors at the time the sexual relationship started, and those with a Wave I family income below the median.[23]

The effects of removing public funding and imposing parental consent laws on the likelihood of separation one year following the pregnancy are presented in Table 3. The estimates, from a linear probability model, show dramatic differences in the likelihood of separation among women giving who experienced a binding increase in abortion costs, with the likeli-

[21]Due to the Add Health data security requirements we do not know state identifiers, and cannot link state identifiers across waves. Policy and moving information are both drawn from questions specifically asked to individuals in each cross-section.

[22]Only 9% of the pregnant sample moved to another state between Wave I and Wave III. Controlling for moving-state indicator had no effect on the policy impacts estimated below. Dropping movers and examining only public funding showed very similar results.

[23]Results below strengthen when the income threshold is reduced, and the median is admittedly arbitrary. See Medoff (2007) for a review of how these restrictions reduced abortion demand.

hood of separation falling.[24] Importantly no significant effects show up for those who should not have been affected by the policies. These results persist when including state and year fixed effects, along with a large set of individual control variables outlined below.

The lower panel of Table 3 presents LPM estimates for giving birth among all pregnancies (those who gave birth, aborted, or miscarried). These estimates also so large policy change effects on the likelihood of birth, though sign for removing public fundings is counter-intuitive. These estimates suggest that removing public funding actually reduced the likelihood of birth among the low income group. This is the same result which Kane and Staiger (1996) obtained, which they argued was consistent with an endogenous pregnancy model. For the imposition of parental consent laws, we see an increase in the probability of birth among minors, which is also consistent with the prior literature (see Haas-Wilsom (1996)). The results point toward two facts: the policies did influence pregnancy outcomes, and also appeared to influence dissolution, although we cannot separate selection into pregnancy or birth from bargaining effects conditional on pregnancy or birth. While these estimates are suggestive evidence that abortion costs change the underlying household bargaining process, they suffer from the fact that birth is not an exogenous conditioning variable. We now turn to using miscarriage to deal with this problem.

3.3 Validity of Miscarriage

Table 4 divides the timing of abortion decisions into four categories and tests for differences in mean separation rates. While the fraction of couples separating increases slightly with the length of the pregnancy, we cannot reject the null of mean independence across groups. Additionally the t-statistic from a linear regression of length of pregnancy on separation was also well below one both with and without controls.[25]. We view this as evidence that the strategy outlined by Ashcraft, Fernandez-Val and Lang (Forthcoming) for identifying the

[24]Estimates of the policies' association with birth, available upon request from the author, looked similar those from Kane and Staiger (1996), with increase in abortion costs reducing the probability of births.

[25]Using different lengths of time following pregnancy we were unable to reject the null of mean independence

effect of birth on outcomes under mean independence is a reasonable way forward.

Table 5 shows conditional means for the two estimation samples by the pregnancy outcomes. Under Assumption (I), miscarriages and births show no significant differences for many characteristics with the exception of drinking and smoking during pregnancy, and test scores and maternal education. Under Assumption (II), where we include births and abortions in the non-miscarriage group, these differences disappear except fir smoking (and drinking is still significant at the 10% level). This suggests that the miscarriage group is a sample mixing some women who would have given birth, and some who would have aborted, had they not miscarried. If this is true the OLS estimates are because abortion and miscarriage are competing risks. The lack of significant differences shows miscarriage is not correlated with these characteristics across the two groups, evidence in favor of the idea that conditional on drinking and smoking, miscarriages are random with respect to many characteristics.[26] Under Assumption (II) miscarriages preempt abortion/birth choices, so miscarriages are randomly drawn from the population of all women who became pregnant. Lower test scores and maternal education are correlated with the underlying desire to give birth.[27] consistent with much of the debate regarding the impact of teenage childbearings which shows women who give birth have lower opportunity costs (see Ashcraft, Fernandez-Val and Lang (Forthcoming) and Hotz, Mullin and Sanders (1997)). We expect higher scores from the miscarriage group if it includes some women who would not have given birth. This can be seen under Assumption (II) when the size and significance of the gap shrinks following the addition of (more) women who obtained an abortion. The same pattern holds for maternal education. The pattern reinforces the notion that miscarriage is a sample of those who became pregnant, but that some rather than all miscarriages are preempted by abortion.

[26] Ashcraft, Fernandez-Val and Lang (Forthcoming) show similar results using evidence from a different data source, the National Survey of Family Growth.

[27] AHPVT is an abbreviated Peabody Picture Vocabulary test, measuring vocabulary and verbal cognition.

4 Results

4.1 IV Estimates

The first stage estimates of (3) are presented in Tables 6 and 7, for the loss of public funding and the imposition of parental consent laws respectively. Four first stage regressions are presented, one in each column, where the dependent variable is birth, birth interacted with the demographic group, birth interacted with the policy change, and the triple interaction of birth. We set abortion as the excluded group and include miscarriage and its three interaction terms. Although not presented in the table, a list of included instruments is provided in the appendix. In column four, the birth equation, we can see the counter-intuitive policy effect on minors outlined above, with removing public funding reducing the probability of birth among poor women. Looking at the diagonal elements in the last four rows, one can see that miscarriage or its corresponding interaction term is providing the identifying power for the first stage. The R^2 indicates miscarriage is indeed highly correlated with being a "birth-type". The AP F-tests suggest miscarriage is not a weak instrument. In Table 7 we see a similar pattern, though the policy changes' influence on birth is no longer significant once we control for miscarriage. Also we note that the KP-F statistic for parental consent changes is low, 3.22 versus the rule of thumb of 10. This is likely because we are clustering at the state level and the parental consent variation in the data is small. We therefore view these as a robustness check on the funding results.

Estimates using changes in abortion funding policies as a cost shifter are presented in Table 8. Separation is measured at one year following pregnancy, the OLS sample uses only those giving birth or having a miscarriage whereas IV uses all pregnancies. Sequentially adding controls across the three specifications presented in the table, beginning by including only cost shifters. The triple interaction shows large negative and significant effects for low-income women giving birth in areas that increased the cost of abortion between Waves I and III. The negative impact of giving birth on separation persists as we add controls.

In specification (ii) we add own and partner characteristics like age, race, and education. Finally adding county level controls for income, density, religiosity, political measures, and even including state fixed effects does not appreciably change the estimates on the triple interaction. Other coefficients in the table suggest the following: women who did not give birth but lost public funding saw a slight decrease in separation rates (-0.163 in the baseline OLS), suggestive of different selection into relationships.[28] Combining the relevant coefficients for low-income mothers who experienced the removal of public funding experienced a decrease in their likelihood of separation of between 25 and 32%, relative to those not giving birth.

Table 9 shows the results for a similar estimation where the variation in abortion costs comes from the imposing of parental consent laws. The triple interaction for women who gave birth in areas where abortion costs increased is again negative for the relevant group, minors. Again, adding controls at the individual, county and state level does not change the essential range of estimates on the triple interaction. Again parental consent laws appear to changed separation behavior among those not giving birth as well, suggesting changes in partner selection. Combining the interaction terms for women below age 18 who experienced a legal change and gave birth, the likelihood of separation decreased between 13 and 28% relative to those women who did not give birth.

4.2 Marriage and Cohabitation

Given the results above, a question of interest is whether the formal implication of Akerlof, Yellen and Katz (1996) holds today: do rising abortion costs increase marriage? The results on this point are mixed, and suggest a more nuanced theory than the original work of Akerlof, Yellen and Katz (1996).

We change the dependent variable in Equation (1) to an indicator of whether the bio-

[28]This last note is speculative: the clustering at the state level may be responsible for interpreting the coefficient as significant. Using the robust standard error calculation increases standard errors so that only the triple-interaction appears significant.

logical couple ever married following the pregnancy, and also an indicator of whether they ever married or cohabited following the pregnancy.[29] Estimates from these specifications are presented in Table 10. The upper panel presents results for funding changes, and the lower panel for parental consent changes. In both panels, using both OLS and IV, we find no impacts on marriage. However, examining cohabitation we do see positive and significant impacts for the triple interaction terms. This suggests the decrease in separation likelihood was related to an increase in cohabitation. These results are consistent with a number of explanations. Firstly, in a certain sense the theory of Akerlof, Yellen and Katz (1996) could be either wrong or outdated. Their theory was meant to address the norms of the pre-1970s U.S. marriage market, where cohabitation was very rare. It could also be that minors and poor women, those subject to increasing abortion costs, are simply much less likely to marry today. The relevant form of "commitment" or "marriage" more broadly defined may be cohabitation for these women. Also, it may be that the sample is too young to consider marriage as a behavioral response to changes in the matching market. Finally, although we confirm that higher abortion costs make separation less likely, men in the affected relationships may substitute toward cohabitation rather than marriage. The reduced availability of childless partners in the marriage market may provide an incentive for men to stay with, but not marry, their current partner. This strategy would preserve a man's option value of more easily leaving in the future, and rationalize our findings of an increased likelihood of staying with the partner, but no effects on marriage.

4.3 Robustness Checks

The work of Fletcher and Wolfe (2009) casts doubt on whether miscarriages are in fact random by pointing out that unobserved community level factors can influence both miscarriage and pregnancy choices (birth and abortion). The main strategy of Fletcher and Wolfe (2009) is to use the community level controls in Add Health, and we do the same. Some controls

[29] The final age we observe women is between age 20-24, with a mean of 22. Even at this young age roughly one-third of non-marital births are followed by a marriage between the biological parents.

such as the 1995 abortion regulations at the state level are included directly in the above specifications. The state policy variation allows us to conduct a further robustness check. In Table 11 we re-estimate the impact of funding changes including school fixed effects. The effects are still identified because some schools in the original Add Health sample draw their enrollment from across state boundaries (7.7% of the schools used in this sample). The impact of the relevant policy changes on separation for minors and those below the median income is significant, with the same pattern of results above, and we again see increases in cohabitation. [30]

Including school fixed effects removes the impact of the school level likelihood to abort or give birth, and deals with unobserved neighborhood characteristics as discussed in Fletcher and Wolfe (2009). The fact that the results do not change is likely due to the extensive list of controls at the state, county and individual level already included. Together these results suggest that miscarriage provides a valid source of conditionally-random variation, and the coefficients above represent real reductions in the likelihood of separation resulting from abortion regulation tightening.

Finally, the use of miscarriage as a natural experiment raises questions about the power of the test. To address this we re-estimated the above specifications on an expanded sample of pregnancies. By ignoring variables measured at Wave I, we can use male reporting of pregnancies, which adds approximately 300 observations to the IV-estimation sample, with results reported in Table 11. Next, we add multiple pregnancies to the estimation sample to increase the number of miscarriages, which, combined with the male sample adds over 800 observations. Again the results for funding changes are nearly identical, while the impact of consent law changes are not significant. Despite the small number of miscarriages in the original sample, with the expanded sample the results indicate a robust difference in separation and cohabitation based on whether one gives birth when abortion costs are rising.

[30] This strategy is sensible only for funding, because poor women are usally linked their home-state address, even if they travel out-of-state for abortions, through Medicaid funding.

5 Conclusion

Did falling abortion costs contribute to the high rates of non-marital childbearing in the United States? We cannot go back in time and test this theory formally. We can, however, use miscarriage as a natural experiment to identify the effect of birth on outcomes. We do this in the presence of policy changes which induced plausibly exogenous increases in abortion costs for poor women. We test whether the basic incentives of Akerlof, Yellen and Katz (1996) are present in the modern market marriage market: namely as abortion costs rise do we see a strengthening of the bargianing position of women who want both a child and a continued relationship? Although we cannot confirm that raising abortion costs increased marriage, there is evidence that it decreased separation rates and increased cohabitation among those women who gave birth.

Our findings suggest firstly that understanding the strategic nature of matching and marriage choices is important in explaining non-marital childbearing in the United States. Parents are making strategic decisions with an eye on costs, particularly marriage market costs. Secondly, there appear to be important consequences of abortion law beyond those intended by policy makers.

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Appendix

All regressions include all the following sets of controls in the matrix X discussed above.

Individual and Partner Characteristics: Female and partner age at pregnancy resolution; education level at pregnancy: less than a high school diploma, high school diploma, some college, bachelors degree or more and indicator of partner currently enrolled at time of pregnancy; in male and female religious attendance in year of pregnancy (six values: 1=never-6=more than once per week) and its square; indicators for no religious attendance for men and women and unknown partner religious information; indicators of Black non-Hispanic, Hispanic, and Other; Welfare Recipient in year of pregnancy, and year prior to pregnancy; Work status (majority of time in pregnancy year) part time or full time; Total years work experience before pregnancy; cohabitation during pregnancy; indicators drinking alcohol daily during pregnancy; indicator for smoking one pack per day or more during pregnancy; exercise intensity at Wave I (none, moderate, intensive), age at first intercourse and its square, weight at Wave I and its square.

County Level Controls: Income, 1990 Census county per capita and median income; Population: 1990 Census population level, density, census designated percent urban; Religiosity, county percent adherents, percent adherents and percent population in conservative and liberal denominations, and proportion Catholic, from Churches and Church Membership 1990 data; Voting data, county percent voting Republican and Democrat in 1992 presidential election, Census fraction of males never married, county level.

Fixed Effects: We include state fixed effects for thirty states and year indicators for six years between 1995 and 2002.

Tables

Table 1: Outlining Implications of Lower Abortion Costs

Model-Group	(1) Pregnancies	(2) Birth	(3) Abortion	(4) MB	(5) NMB	(6) $Frac_{NMB}$[a]
(1) KS-Small	$\uparrow$	$\uparrow$	$\uparrow$	$\uparrow$	-	$\downarrow$
(2) KS-Large	-	$\downarrow$	$\uparrow$	-	$\downarrow$	$\downarrow$
(3) AYK	-	$\downarrow$	$\uparrow$	$\downarrow$	$\uparrow$	$\uparrow$

[a] MB=Marital births, NMB=non-marital births, $Frac_{NMB}$, fraction of non-marital births is *NMB/(NMB+MB)*

Table 2: Estimation Sample[a]

	Sample with:			
	Assumption (I)		Assumption (II)	
N	1438		1859	
Pregnancy Outcome				
Birth	85.72		67.77	
Abortion	0		20.94	
Miscarriage	14.28		11.28	
Separated by:				
1 Year	18.07		21.61	
Married Partner	29.35		24.54	
Married or Cohabited	71.04		64.12	
Abortion Funding Removed	5.55		5.64	
Consent Law Imposed	1.17		1.28	
< Median Family Income	25.10		26.53	
Minor at Pregnancy	39.92		37.00	
	Female	Partner	Female	Partner
Age (Years)				
Mean	18.85	21.92	18.75	21.66
Race				
White	60.53	42.59	60.16	53.08
Black	25.10	31.67	23.65	27.33
Hispanic	11.82	16.31	12.33	12.17
Other	2.55	9.34	3.86	7.42
Education				
<HS Diploma.	49.24	35.09	49.32	34.64
HS Diploma.	44.69	43.74	42.90	42.46
Some College	5.64	14.52	6.98	15.91
Bachelors Deg.	0.43	2.85	0.80	3.28
Unknown	-	3.80	-	3.71

[a]Sample includes only female-reported first pregnancies. Sample I drops women who do no report their pregnancies to partners prior to resolving them, and drops women whose partner left before the date of pregnancy resolution, Sample II includes both of these groups. Figures are percentages unless otherwise noted.

Table 3: Policy Effects On Separation and Birth

	Separation|Birth[a]			
	(i)	(ii)	(iii)	(iv)
Funding Lost × < Median Income	-0.284	-0.280	-	-
	(0.071)	(0.057)		
< Median Income	0.010	-0.027	-	-
	(0.035)	(0.036)		
Consent Imposed × Minor	-	-	-0.448	-0.516
			(0.197)	(0.138)
Minor	-	-	0.034	0.016
			(0.038)	(0.050)
Parental Consent Imposed	0.109	0.081	0.254	0.248
	(0.078)	(0.108)	(0.213)	(0.175)
Public Funding Lost	0.061	0.063	-0.051	-0.046
	(0.051)	(0.044)	(0.053)	(0.055)
N	1227	1227	1227	1227
R^2	0.037	0.223	0.036	0.219

	Birth[b]			
	(i)	(ii)	(iii)	(iv)
Funding Lost × < Median Income	-0.209	-0.217	-	-
	(0.081)	(0.068)		
< Median Income	0.067	0.046	-	-
	(0.031)	(0.026)		
Consent Imposed × Minor	-	-	0.452	0.374
			(0.089)	(0.121)
Minor	-	-	-0.124	0.014
			(0.052)	(0.064)
Parental Consent Imposed	-0.091	-0.117	-0.176	-0.204
	(0.060)	(0.060)	(0.102)	(0.082)
Public Funding Lost	0.034	0.030	0.095	0.046
	(0.112)	(0.074)	(0.113)	(0.087)
N	1859	1859	1859	1859
R^2	0.086	0.292	0.097	0.294
Abortion Cost Shifters	yes	yes	yes	yes
State and Year FE	yes	yes	yes	yes
Individual and Parter Information	no	yes	no	yes
Selection Correction	no	yes	no	yes
County Level Covariates	no	yes	no	yes

[a]Coefficients are from a linear probability model. Separation is measured 1 year following pregnancy. Controls in columns (ii) and (iv) are listed in the Appendix. Below medain income ($32,000) is drawn from family income at Wave I. Minor is measured from the age the woman became pregnant.

[b]Coefficients are from a linear probability model estimated on those who gave birth, aborted, or miscarried.

Table 4: Test of Mean Independence in Abortion Timing

		Tests of Significant Differences[a]			
Time Of Abortion:	Mean 1-Year Separation	1st Month	2nd Month	3rd Month	N
1st Month	0.272	-	-	-	75
	(0.066)				
2nd Month	0.353	0.081	-	-	182
	(0.049)	(0.082)			
3rd Month	0.399	0.127	0.046	-	117
	(0.063)	(0.091)	(0.080)		
2nd Trimester	0.385	0.113	0.032	-0.014	40
	(0.098)	(0.118)	(0.110)	(0.116)	

[a]Means and tests are weighted. Months come from length of pregnancy in week divided by four and rounded. Separation is measured one year from the beginning of pregnancy.

Table 5: Mean Characteristics by Pregnancy Outcomes[a]

	Assumption (I)		Assumption (II)	
Characteristic	Birth	Miscarriage	Abortion or Birth	Miscarriage
Age	18.89	18.60	18.76	18.60
	(0.111)	(0.274)	(0.100)	(0.274)
Black	0.259	0.198	0.240	0.198
	(0.036)	(0.053)	(0.033)	(0.053)
Hispanic	0.120	0.111	0.125	0.111
	(0.018)	(0.026)	(0.018)	(0.026)
HS Grad	0.438	0.502	0.419	0.502
	(0.023)	(0.057)	(0.020)	(0.057)
Public Fund Lost	0.056	0.056	0.057	0.056
	(0.021)	(0.024)	(0.019)	(0.024)
Parental Consent Imposed	0.009	0.029	0.011	0.029
	(0.004)	(0.016)	(0.003)	(0.016)
Smoke during Pregnancy	0.247	0.362*	0.264	0.362*
	(0.020)	(0.049)	(0.017)	(0.049)
Drink during Pregnancy	0.002	0.041*	0.029	0.041
	(0.001)	(0.021)	(0.005)	(0.021)
AHPVT Score	97.07	99.54*	98.28	99.54
	(0.711)	(1.336)	(0.664)	(1.336)
Mother Col. Grad	0.140	0.230*	0.180	0.230
	(0.013)	(0.031)	(0.015)	(0.031)
1-Year Separation	0.191	0.180	0.220	0.191
	(0.033)	(0.015)	(0.014)	(0.033)

[a] * denotes miscarriage mean is significantly different. Standard errors are in parentheses. Separation is measured one year from the pregnancy occurring. Smoking is an indicator for any cigarette smoking during pregnancy, and drinking is an indicator for any drinking during pregnancy.

Table 6: First Stage Estimates[a]

	Endogenous Covariate			
Regressors	Birth × Funding Lost × < Median Income	Birth × < Median Income	Birth × Funding Lost	Birth
Funding Lost × < Median Income	0.684	-0.111	-0.071	-0.152
	(0.039)	(0.043)	(0.066)	(0.069)
Funding Lost	-0.012	0.019	0.678	0.042
	(0.036)	(0.034)	(0.089)	(0.117)
< Median Income	0.001	0.821	0.000	0.079
	(0.001)	(0.028)	(0.002)	(0.025)
Miscarriage × Funding Lost × < Median Income	-0.691	0.083	0.051	0.073
	(0.026)	(0.043)	(0.073)	(0.083)
Miscarriage × < Median Income	-0.003	-0.826	-0.003	-0.100
	(0.002)	(0.030)	(0.003)	(0.031)
Miscarriage × Funding Lost	0.008	0.034	-0.72	0.07
	(0.018)	(0.023)	(0.089)	(0.102)
Miscarriage	0.001	-0.001	0.002	-0.733
	(0.001)	(0.006)	(0.002)	(0.029)
N	1859	1859	1859	1859
R^2	0.684	0.769	0.758	0.346
AP F-stat (Miscarraige and interactions)	645.2	730.8	66.7	627.1

[a] All estimations include abortion cost shifters and state and year fixed effects. Standard errors in parenthesis. *,** indicate significance at 10% and 5% level respectively. Columns are separate regressions. Standard errors are clustered at the state level. The KP-F test statistic for the instruments is 22.1.

Table 7: First Stage Estimates[a]

	Endogenous Covariate			
Regressors	Birth × Minor × Consent Imposed ×	Birth × Minor	Birth × Consent Imposed	Birth
Consent Imposed × Minor	0.701	0.004	0.128	0.212
	(0.075)	(0.071)	(0.176)	(0.157)
Minor	0.009	0.041	0.589	-0.165
	(0.007)	(0.035)	(0.142)	(0.158)
Parental Consent Imposed	0.002	0.681	0.003	-0.124
	(0.002)	(0.041)	(0.002)	(0.046)
Miscarriage × Consent Imposed × Minor	-0.682	0.045	-0.071	-0.115
	(0.070)	(0.081)	(0.198)	(0.195)
Miscarriage × Minor	0.001	-0.689	0.003	0.118
	(0.001)	(0.042)	(0.002)	(0.060)
Miscarriage × Consent Imposed	0.0003	-0.008	-0.576	0.151
	0.004	0.032	0.153	0.162
Miscarriage	0.000	0.001	-0.001	-0.799
	(0.001)	(0.005)	(0.001)	(0.035)
N	1859	1859	1859	1859
R^2	0.708	0.658	0.646	0.352
AP F-stat (Miscarraige and interactions)	12.9	439	12	1550

[a] All estimations include abortion cost shifters and state and year fixed effects. Standard errors in parenthesis. *,** indicate significance at 10% and 5% level respectively. Columns are separate regressions. Standard errors are clustered at the state level. The KP-F test is 3.22.

Table 8: OLS and IV Separation Estimates-Changes in Abortion Funding[a]

	Specification					
	(i)		(ii)		(iii)	
Cost Interactions	OLS	IV	OLS	IV	OLS	IV
OLS	IV					
Birth × Funding Lost × < Median Income	-0.867	-0.979	-0.783	-0.911	-0.736	-0.864
	(0.355)	(0.476)	(0.286)	(0.398)	(0.276)	(0.394)
Birth × < Median Income	0.064	0.051	0.079	0.079	0.111	0.126
	(0.071)	(0.082)	(0.085)	(0.097)	(0.074)	(0.087)
Birth	-0.025	0.047	-0.020	0.059	-0.024	0.026
	(0.056)	(0.072)	(0.061)	(0.082)	(0.053)	(0.069)
Public Funding Lost	-0.163	-0.182	-0.249	-0.281	-0.312	-0.321
	(0.064)	(0.054)	(0.074)	(0.079)	(0.101)	(0.087)
N	1438	1859	1438	1859	1438	1859
R^2	0.026	0.005	0.154	0.100	0.207	0.145
Abortion Cost Shifters	Yes		Yes		Yes	
Individual and Parter Information	No		Yes			
State FE & County Level Covariates	No		No		Yes	

[a]Standard errors in parenthesis. *,** indicate significance at 10% and 5% level respectively. Columns are separate regressions. Individual and county level observables are listed in the Appendix. Abortion costs include state level indicators for abortion policy (funding, consent, and waiting period). Standard errors are clustered at the state level.

Table 9: OLS and IV Separation Estimates-Changes in Parental Consent[a]

	Specification					
	(i)		(ii)		(iii)	
Cost Interactions:	OLS	IV	OLS	IV	OLS	IV
Birth × Consent Imposed × Minor	-1.397	-1.492	-1.428	-1.406	-1.421	-1.418
	(0.169)	(0.163)	(0.140)	(0.130)	(0.137)	(0.158)
Birth × Minor	0.066	0.070	0.067	0.121	0.101	0.110
	(0.084)	(0.116)	(0.091)	(0.124)	(0.083)	(0.126)
Birth	-0.037	0.011	-0.020	0.029	-0.019	0.027
	(0.039)	(0.051)	(0.039)	(0.052)	(0.037)	(0.049)
Parental Consent Imposed	-0.179	-0.183	-0.190	-0.192	-0.219	-0.219
	(0.065)	(0.057)	(0.059)	(0.058)	(0.073)	(0.069)
N	1438	1859	1438	1859	1438	1859
R^2	0.026	0.011	0.141	0.096	0.200	0.136
Abortion Cost Shifters	Yes		Yes		Yes	
Individual and Parter Information	No		Yes		Yes	
State FE & County Level Covariates	No		No		Yes	

[a]Standard errors in parenthesis. *,** indicate significance at 10% and 5% level respectively. Columns are separate regressions. Individual and county level observables are listed in the Appendix. Abortion cost include state level indicators for abortion policy (funding, consent, and waiting period). Standard errors are clustered at the state level.

Table 10: OLS and IV Marriage and Cohabitation Estimates[a]

	Specification			
	Ever Married		Married or Cohabit	
Low Income Effects	OLS	IV	OLS	IV
Birth × Funding Lost × < Median Income	0.074	-0.075	0.406	0.433
	(0.228)	(0.316)	(0.209)	(0.294)
Birth × < Median Income	-0.030	0.021	-0.192	-0.184
	(0.069)	(0.089)	(0.083)	(0.113)
Birth × Funding Lost	-0.037	0.106	-0.056	-0.015
	(0.151)	(0.213)	(0.274)	(0.325)
Birth	0.112	0.045	0.132	0.056
	(0.038)	(0.066)	(0.056)	(0.084)
N	1438	1859	1438	1859
R^2	0.341	0.308	0.332	0.307
Minor Effects				
Birth × Consent Imposed × Minor	-0.365	-0.127	0.649	0.986
	(0.216)	(0.263)	(0.230)	(0.333)
Birth × Minor	-0.131	-0.185	-0.057	-0.168
	(0.075)	(0.114)	(0.081)	(0.117)
Birth × Consent Imposed	0.234	0.263	0.204	0.216
	(0.171)	(0.186)	(0.101)	(0.100)
Birth	-0.185	-0.196	-0.702	-0.726
	(0.188)	(0.188)	(0.115)	(0.111)
$\lambda(Z'\delta)$	0.062	0.055	-0.154	-0.147
	(0.117)	(0.088)	(0.079)	(0.090)
N	1438	1859	1438	1859
R^2	0.295	0.271	0.289	0.240
Abortion Cost Shifters	Yes		Yes	
Individual and Parter Information	Yes		Yes	
State FE & County Level Covariates	Yes		Yes	

[a]Standard errors in parenthesis. *,** indicate significance at 10% and 5% level respectively. Columns are separate regressions. All estimations include individual and county level observables listed in the Appendix, along with state level indicators for abortion policy (funding, consent, and waiting period). Standard errors are clustered at the state level.

Table 11: Robustness Checks[a]

With School Fixed Effects:	Separated OLS	Separated IV	Married or Cohabit OLS	Married or Cohabit IV
Birth × Funding Lost × < Median Income	-0.926	-0.925	0.407	0.308
	(0.277)	(0.472)	(0.185)	(0.271)
N	1438	1859	1438	1859

Including Male Reporting	Separated OLS	Separated IV	Married or Cohabit OLS	Married or Cohabit IV
Birth × Funding Lost × < Median Income	-0.673	-0.813	0.511	0.640
	(0.307)	(0.382)	(0.192)	(0.275)
Birth × Consent Imposed × Minor	-1.343	-1.389	0.695	0.965
	(0.141)	(0.143)	(0.248)	(0.382)
N	1645	2147	1645	2147

Including Multiple Pregnancies and Men	Separated OLS	Separated IV	Married or Cohabit OLS	Married or Cohabit IV
Birth × Funding Lost × < Median Income	-0.591	-0.722	0.520	0.635
	(0.321)	(0.393)	(0.231)	(0.296)
Birth × Consent Imposed × Minor	-0.405	-0.212	-0.442	-0.565
	(0.102)	(0.224)	(0.301)	(0.460)
N	2053	2690	2053	2690

[a]Standard errors in parenthesis. Each coefficient comes from a separate regression. All estimations include the individual and county level characteristics. School fixed effect regressions do not include state fixed effects, male reporting and multiple pregnancy regressions include state fixed effects but no selection correction terms. Standard errors are clustered at the state level.