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ORIGINAL PAPER

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 through effects on marriage market norms. Single motherhood could rise if low-cost abortion makes it easier for men to avoid marriage. This study estimated the effect of abortion costs on separation, cohabitation and marriage following a birth by exploiting miscarriage and changes in state abortion laws. There is evidence that norms responded to abortion laws as women who gave birth under abortion restrictions experienced sizable decreases in single motherhood and increased cohabitation rates. The results underscore the importance of norms regulating relationship dynamics in explaining high levels of non-marital childbearing and single motherhood.

Keywords Fertility · Family formation · Non-marital childbearing · Abortion costs

Introduction

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 United States among all age groups. Ventura (2009) showed that between 1980 and 2006 birth rates among unmarried women increased by 59 % among women aged 18–19 and by 95 % among those aged 20–24. While many factors have been offered to explain the increase in non-marital births, for example a rising age at first-marriage and increased non-marital sexual activity, it has been substantially harder for researchers to explain both the increases in births and abortions. The increase in both outcomes represents a simultaneous increase in unwanted pregnancies (ending in abortion), and in wanted non-marital pregnancies (ending in non-marital births). The theory of Akerlof et al. (1996) reconciled these seemingly paradoxical trends by introducing externalities in the marriage market, arguing low abortion costs displaced the norm of "shot-gun" marriage.[1] At issue is whether the decision to continue a relationship following a non-marital birth is influenced by abortion costs themselves, for instance through selection (better matched couples experiencing births) or bargaining (worse outside options for men increasing relationship length). This paper designs an empirical test of whether tightening abortion laws had the consequence of reducing separation among biological parents.

The existing literature on abortion laws has focused on first order effects, the literature showed that the demand for abortions responds to incentives, leaving open the possibility for consequences beyond these first-order effects. Bitler and Zavodny (2001), Blank et al. (1996), Haas-Wilson (1996), Levine (2003), and Levine et al. (1996) all measured the impact of state-level laws on abortion, birth and sexual behavior, but not marriage. Findings tended to be consistent with economic theory, public funding increased abortion; restrictions such as parental consent

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1 "Shot-gun" marriage refers to the marriage of a couple after pregnancy but before the birth of a child conceived outside of marriage. The shot-gun refers to coercion to ensure the man follows through with marriage.

reduced it. These laws are particularly relevant for minors (Haas-Wilson 1996; Girma and Paton 2011). Girma and Paton (2011) exploited the timing of access to emergency birth control (EBC) in northern Britain and showed that increased EBC lead to increases in sexually transmitted infections rates, with mixed evidence about the effect on pregnancies.

More nuanced consequences, like those involving male behavior, have been confined to the theoretical realm. Indeed, to support their theory Akerlof et al. (1996) looked 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. While one cannot test a theory of the post-legalization diminution of norms, one can see whether separation patterns following a birth responded to recent changes in access to abortion, allowing us to say whether there is evidence that spill-over effects similar to those outlined in Akerlof et al. (1996) still operate. This work complements existing efforts which examine the reasons for union formation and dissolution and subsequent single parenthood.[2]

The fundamental question this paper aims to answer is whether women who give birth in areas with rising abortion costs experience a lower probability of dissolution with the biological father? The comparison is twofold, comparing women in low-cost areas versus high-cost areas and comparing those who give birth to those who do not. 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 exploit meaningful variation in abortion costs during a time when the procedure is largely accessible, we must exploit micro-data from the National Longitudinal Study of Adolescent Health, which allows one to directly identify respondents who were minors or lived in poor households. Haas-Wilson (1996) for example, found no significant effects of recent policy changes on overall abortion ratios, but only among those specifically targeted by access policies (for example, parental consent). These women are much more likely to be on a margin where policy changes affect decisions, and the correlation of their choices with policy changes would be difficult to identify in more aggregated measures of birth, marriage and abortion (for example, state-level rates).

To estimate this effect one must overcome 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 we exploited within-state variation in public funding and parental consent laws over a relatively short time interval to shift costs in the same marriage market. The choice to give birth is made in light of many factors, including the male-partner's interest in having a child, so birth is clearly endogenous with respect to relationship status. To deal with this our approach employed the recent econometric frame-work of Ashcraft et al. (2013) who outlined the conditions under which one can use miscarriage as a natural experiment, providing a control group of women who had a pregnancy, faced the same regulatory environment, but who did not have a child.[3]

The results showed that women in states that removed public funding saw decreased single motherhood and increased cohabitation among women giving birth. Estimates showed 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 conditional on giving birth. These estimates mean that among the children of low-income mothers, the fraction of children living with both biological parents at the time of birth would rise by ten percentage points.

In addition to examining separation, we also estimated policy effects on marriage and cohabitation. As a large literature has documented, there are a number of barriers to marriage among younger and low income populations. Edin and Reed (2005) reviewed the literature and identify a number of factors, including financial standards for marriage, concern about divorce, partner quality and multi-partner fertility.[4] One problem with examining relationships with more commitment is that marriage and cohabitation incentives vary with geography. For example welfare benefit generosity creates incentives for cohabitation. While shot-gun marriage rates were still substantial in our data (roughly 30 %), many individuals may not be prepared to enter marriage. Indeed Myers (2013) showed that among low-income, unmarried fathers, the definition of responsible fatherhood did not include primary caregiving and

[2] See Weiss and Willis (1997) who examined patterns in the US; see Weiss (1993) for a review. Brien et al. (1999) showed family childbearing and separation hazards share common unobservables, which is addressed below.

[3] Throughout the study we mainly followed the insights of Ashcraft et al. (2013) and Hotz et al. (2005), who showed that using OLS and IV estimators can deliver bounds on the effects of birth on labor market outcomes for conditionally random miscarriage. Akerlof et al. (1996) and Kane and Staiger (1996) provided models of this information flow, which empirically leads to a simultaneity bias when one conditions on birth.

[4] Horner (2014) showed women with children see lower levels of happiness when divorce barriers are reduced.

breadwinning. For these reasons we examined marriage separately and also combined marriage and cohabitation as one outcome. Combining outcomes and including state fixed effects helped to control for differences in marital incentives across states. Our estimates revealed that among those induced to change behavior by the policy change, cohabitation did indeed seem like the relevant relationship alternative, as cohabitation increased though the estimates are less precise than those for separation.

The analysis suggests a key channel for understanding non-marital childbearing is the one first outlined by Akerlof et al. (1996). Namely there is evidence that changes to abortion access result in a spillover-effect on the relationship terms of women who give birth. This may represent differential sorting into relationships where birth occurs, or improved bargaining power within the relationship. Either way the pattern of results highlights the matching behavior of fathers surrounding a non-marital birth, who often decide whether to continue co-residence and child support in light of future marriage market concerns.[5] It seems plausible that for the young couples in our data that cohabitation is the relevant relationship bargained over. It appears that abortion laws have consequences along the broader sequence of choices leading to single motherhood, with negative spillovers for some women who decide to give birth.

Two theoretical contributions to non-marital childbearing and abortion costs guided our thinking. Both examined, among other things, how a reduction in the cost of abortion changed fertility and relationship decisions. Kane and Staiger (1996) modeled the insurance value of abortion. In a key comparative static, they showed large cost reductions, such as moving from pre- to post-*Roe vs Wade*, would decrease non-marital childbearing. This is akin to moving a large group of women from the no insurance state to being insured against non-marital births (the source of risk being insured against is the father not being willing to marry). In contrast, Akerlof et al. (1996) argued abortion cost reductions decreased incentives for shot-gun marriage, male commitment and therefore increased non-marital childbearing. They outlined two different models in which the fact that some women adopt abortion in the event of a pregnancy forces all women to bear the risk of single motherhood in order to maintain a relationship. Their conclusion rests on some men being uninterested in having a relationship with children, and the fraction of women using abortion being high enough so as to make it attractive to a man to re-enter the marriage market rather than continue a relationship following a birth. Thus a decrease in abortion costs benefits women who use abortion, but women who want children (or will not use abortion) see higher chances of being single. Given these two plausible scenarios for large changes in abortion costs, the question of which dominates is an empirical one. The analysis focuses on groups likely to experience binding cost changes, minors and those who cannot otherwise afford health care, in an effort to isolate women who faced a large change in abortion costs.

Empirical Strategy

Our empirical strategy focused on testing whether relationship continuation was positively related to birth following an abortion cost change. Thus our goal was to estimate the effect of giving birth on separation, but to allow that effect to be heterogeneous with respect to whether abortion costs had increased. The baseline estimation equation took the following form:

$$Separation_{ist} = (\gamma_b^0 + \gamma_b^1 P_{st} + \gamma_b^2 AG_{is} + \gamma_b^3 AG_{is} \cdot P_{st})' B_{ist} + X'_{ist}\gamma_x + \varepsilon_{ist}. \quad (1)$$

where B_{ist} is a birth to a woman i, in year t, living in state s, P_{st} is the policy change restricting access to abortion in state s (for example, removal of funding or imposition of parental consent), AG_{is} is an indicator for being in the affected group (for example, poor or a minor); since the focus is on first pregnancies this characteristic does not vary over time. The triple interaction captures the change in separation among those who give birth when abortion costs have gone up. Here X_{ist} are other controls which can include individual, partner, and community attributes and state fixed-effects; it also included the direct effects of abortion laws and affected group status on separation rates, along with year fixed effects.

Assuming for the moment one can find plausibly exogenous variation in P_{st} and B_{ist}, Eq. (1) is a reduced form for models of relationship continuation and fertility. The parameters γ_b capture the effect of giving birth on separation. These effects may combine multiple channels, such as selection into birth, direct costs of abortion, and the influence of relationship bargaining, into one estimate. If γ_b^3 is positive it may be because increasing abortion costs (1) changes the mix of those conceiving towards couples who want to have a child and stay together, (2) shifts women at the margin away from abortion and these women are more likely to continue relationships with their partners following a birth, or (3) shifts bargaining power toward women who prefer to give birth.

Identifying the Parameter of Interest

The first step in obtaining credible estimates of γ_b is to exploit changes in the regulatory environment facing the

[5] Recent work by Beauchamp et al. (2015) showed men are more less likely to pay child support once they match with another partner.

respondents in Add Health, inducing variation in P_{st}. Here we used whether the state of residence changed legal regimes between Wave I (1995) and Wave III (2001). Also included are Wave I state-level fixed effects and pregnancy year fixed effects, to ensure variation in P_{st} came from changes within-states over time. The second step is to deal with the endogeneity of B_{ist}, which was instrumented using miscarriage as outlined by Ashcraft et al. (2013).

Ashcraft et al. (2013) showed 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 birth or miscarry and the IV-estimates on the entire sample (those who give birth, abort, or miscarry). For this approach to be valid one needs to assume miscarriage is conditionally random. Conditional refers to a set of behaviors during pregnancy observed in the Add Health data, namely smoking and drinking. Hotz et al. (1997) allowed for bounds on the effect of birth on outcomes when some miscarriages are non-random. We estimated these bounds and the relevant (upper) bounds were the same sign as the results presented below.

As Ashcraft et al. (2013) showed, the major problem using miscarriage is that abortion and miscarriage are competing risks, however, they also showed how to bound the effect of giving birth on outcomes. 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 across those who gave birth and miscarried will identify the effect of birth on outcomes; OLS is sufficient to pick up the effect since treatment is conditionally-random and not selected through abortion choices. Now suppose the opposite, that all miscarriages precede all abortions (labeled 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 an abortion.[6] Under Assumption II, instrumenting for birth with miscarriage delivers the impact of treatment, assuming that abortion and miscarriage have the same effect on outcomes (a point discussed in more detail below).[7] Ashcraft et al. (2013) showed the true effect is a convex combination of the OLS and IV estimates if the outcome 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. Under mean independence of abortion timing one can interpret the OLS and IV estimates as bounds for the true effect of giving birth on separation.[8]

If the assumptions outlined are maintained, one can formulate a system of equations to be estimated by instrumental variables. The four endogenous variables related to birth in Eq. (1) can be projected onto their four counter-parts:

$$Y^j_{ist} = \left(\rho^0_{bj} + \rho^1_{bj} P_{st} + AG_{is}\rho^2_{bj} + AG_{is}\cdot P_{st}\rho^3_{bj}\right) M_{ist} + X'_{ist}\rho_{xj} + \eta_{istj} \quad (3)$$

where Y^j_{ist} denotes the j-th endogenous variable (birth or its interaction) and M_{ist} 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. Estimating these specifications on a set of women who either miscarried, gave birth, or aborted, means miscarriage will not be perfectly predictive in the first stage.

Data

We used the National Longitudinal Study of Adolescent Health (Add Health), which began surveying a large sample of teens aged 12–18 in 1995, with follow-ups in 1996, 2002 and 2008. The design was a stratified random sample of US high schools and associated middle schools; Wave I was conducted between 1994 and 1995, Wave II in 1995 and 1996, Wave III in 2000 and 2001, and Wave IV in 2007. The data used here come from a retrospective history of all relationships between 1995 and 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. These detailed data were required to determine the relevance of restrictions to abortion based on age.

[6] This is the assumption put forth in Hotz et al. (2005).

[7] Since the focus is on separation, the data only include miscarriages prior to twenty weeks of gestation in the empirical section. Results were largely insensitive to this cut-off. Gold et al. (2010) show a substantially higher risk of separation following a stillbirth (>20 weeks gestation) than a miscarriage.

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

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

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

Estimation Sample

We first focused on unmarried women experiencing their 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).[9] Missing probability weights and geographic identifiers, and non-response further limit the final sample size to (1859) pregnancies. Wherever possible, indicators were included for non-response regarding partner characteristics, which may be particularly relevant. Most non-response problems came from linking the 2001 relationship roster data with early adolescent data on puberty, and from smoking or drinking during pregnancy questions, which was much less of a concern than if non-response were related to relationship characteristics. Probability weights were used to correct for unrepresentative over samples in the Add Health survey design.

Table 1 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. Using the second sample abortion reporting was examined. To check for reporting problems, Table 1 allows one to compute the abortion ratio (abortions per 1000 live births) for the estimation sample. In Table 1 the abortion ratio was 309, comparable to age specific administrative data from Centers for Disease Control (CDC 2003), which showed an age-specific abortion ratio for 15–24 year-olds of 330.5.[10] Finer and Henshaw (2006) used data from the Alan Guttmacher Institute, which maintains more accurate data than CDC. From their Table 1, the 2001 age-specific ratio of abortion to pregnancies (including fetal losses) was 264.31. This sample had a ratio of 227. These estimates suggest the Add Health data captured between 86 and 93 % of abortions that likely occurred to women in our sample. This is conditional on the probability weights from Wave III which are employed to correct for minority over-sampling and the fact that the surveys' selection mechanism likely generates a sample which is not nationally representative of women obtaining abortions. The percentage of pregnancies ending in miscarriages is similar to other data sources like the National Longitudinal Survey of Youth (NLSY79) and the National Survey of Family Growth which both show 12–14 %. The National Survey of Family Growth is one of the few reliable sources for miscarriage estimates. The total

Table 1 Estimation sample

	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
Mean age	18.85	21.92	18.75	21.66
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
<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

Sample includes only female-reported first pregnancies. Sample I includes only women who miscarried and gave birth. Sample II includes both of these groups and those who obtained an abortion. Figures are percentages unless otherwise noted

miscarriage rate rose slightly through the 1980s and early 1990s. Ventura et al. (1995) attributed 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. While less than ideal, these reporting percentages are far better than those from other longitudinal data sources. Lundberg and Plotnick (1995) documented severe reporting problems in the NLSY79, which sees reporting rates around 60 % for Whites, and even lower for minorities.

In the upper panel of Table 1 separation, marriage and cohabitation are also listed. Separation was measured 1 year from conception; marriage and cohabitation are indicators for whether either ever occurred following conception.[11] Marriage and cohabitation are less frequent, and

[9] Stillbirths have been documented to have larger influence on a couples' likelihood of separation. See Gold et al. (2010).

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

[11] Separation results were nearly identical when using 9–24 months from conception 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.

separation more frequent, after women obtaining an abortion were included. Between 1995 and 2002, 5 % of the sample lost public funding. The data showed that two states in the sample period removed abortion funding, motivating the use of standard errors clustered at the state-level in the estimations below. The changes in effective policy for parental consent are the result of migration across states between 1995 and 2002, and only 1 % of the sample experienced a change of this policy. For minor restrictions to be endogenous would require that minors' parents moving behavior is influenced by their child'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, these only serve as a check on the funding results whose variation was driven by more plausibly exogenous legal changes across states over-time. In Table 10 below the specification was re-estimated on the sample of women residing in the same state in Waves I and III.

In the lower panel of Table 1 one can see that in both samples partner characteristics include the standard 2-year age gap between male and female partners. Although our sample is young, they were not solely women experiencing a teen pregnancy, a point underscored by the fact that roughly one-third married the biological father following a non-marital birth. The partners of women who experienced a non-marital-first pregnancy, were more frequently minority men. Finally, the educational attainment at the time of pregnancy was concentrated at or below 12 years of schooling.

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. Thus one can identify whether the state of residence had different policies in 1995 and 2002.[12] Given that state funding and parental consent laws have been shown to induce sizable cost changes for the affected demographic groups, the analysis focused on those who were minors at the time the sexual relationship and those with a Wave I family income below the median.[13] This approach was taken because abortion restrictions can usually be compensated for with travel, but this compensation is considerably more difficult for these constrained groups. Beauchamp (2015) and Joyce et al. (2012) addressed declining geographic access to abortion. Add Health data prevent one from observing access in adjoining counties to respondents' residences, precluding using travel time as a meaningful cost-shifter. We also preformed robustness checks to verify results are not driven by individual migration.

The effects of removing public funding and imposing parental consent laws on the likelihood of separation 1 year following the pregnancy are presented in Table 2. The estimates, from a linear probability model, showed dramatic differences in the likelihood of separation among women giving birth who experienced a binding increase in abortion costs, with the likelihood of separation falling.[14] Importantly no significant effects showed up for those who should not have been affected by the policies. These results persisted when including state and year fixed effects, along with a large set of individual control variables outlined in the Appendix.

The lower panel of Table 2 presents LPM estimates for giving birth among all pregnancies (those who gave birth, aborted, or miscarried).[15] These estimates also showed large policy change effects on the likelihood of birth, though the sign for removing public funding is counterintuitive. The estimates suggest that removing public funding actually reduced the likelihood of birth among the low income group. Kane and Staiger (1996) obtained a similar result, which they argued was consistent with an endogenous pregnancy model. For the imposition of parental consent laws, there was an increase in the probability of birth among minors, which is also consistent with the prior abortion-policy literature (Haas-Wilson 1996). The results suggest two points, firstly the policies did influence pregnancy outcomes, and secondly also appeared to influence dissolution, although one cannot separate selection into pregnancy or birth from bargaining effects following pregnancy or birth. While these estimates are suggestive evidence that abortion costs changed 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.

Validity of Miscarriage

Table 3 divides the timing of abortion decisions into four categories and tested for differences in mean separation rates. While the fraction of couples separating increased

12 One cannot pin down the exact time of policy enactment because the data do not contain the state where the pregnancy occurred if a respondent moved states, a point returned to in the robustness exercise.

13 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.

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

15 Results, available upon request, showed very similar estimates from probit models; linear models were used throughout for consistency with the IV results below.

Table 2 Policy effects on separation and birth

	P (separation ∣ birth)			
	(i)	(ii)	(iii)	(iv)
Funding lost × <median income	−0.284*** (0.071)	−0.280*** (0.057)	–	–
<Median income	0.010 (0.035)	−0.027 (0.036)	–	–
Consent imposed × minor	–	–	−0.448* (0.197)	−0.516*** (0.138)
Minor	–	–	0.034 (0.038)	0.016 (0.050)
Parental consent imposed	0.109 (0.078)	0.081 (0.108)	0.254 (0.213)	0.248 (0.175)
Public funding lost	0.061 (0.051)	0.063 (0.044)	−0.051 (0.053)	−0.046 (0.055)
	P (birth)			
	(i)	(ii)	(iii)	(iv)
Funding lost × <median income	−0.209* (0.081)	−0.217** (0.068)	–	–
<Median income	0.067 (0.031)	0.046 (0.026)	–	–
Consent imposed × minor	–	–	0.452*** (0.089)	0.374*** (0.121)
Minor	–	–	−0.124* (0.052)	0.014 (0.064)
Parental consent imposed	−0.091 (0.060)	−0.117[a] (0.060)	−0.176[a] (0.102)	−0.204** (0.082)
Public funding lost	0.034 (0.112)	0.030 (0.074)	0.095 (0.113)	0.046 (0.087)
Individual and partner information	No	Yes	No	Yes
County level covariates	No	Yes	No	Yes

Coefficients are from a linear probability model. Separation is measured 1 year following pregnancy. Controls are listed in the "Appendix" section. All columns include abortion policy changes, state and year fixed effects. N for the specifications is 1227 and 1859 respectively

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

Table 3 Test of mean independence in abortion timing

Time of abortion	Mean 1-year separation	Tests of significant differences			N
		1st month	2nd month	3rd month	
1st month	0.272 (0.066)	–	–	–	75
2nd month	0.353 (0.049)	0.081 (0.082)	–	–	182
3rd month	0.399 (0.063)	0.127 (0.091)	0.046 (0.080)	–	117
2nd trimester	0.385 (0.098)	0.113 (0.118)	0.032 (0.110)	−0.014 (0.116)	40

Means and tests are weighted. Separation is measured 1 year from the beginning of pregnancy

slightly with the length of the pregnancy, one 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. Also, using different lengths of time following pregnancy we were unable to reject the null of mean independence. This is evidence that the strategy outlined by Ashcraft et al. (2013) for identifying the effect of birth on outcomes under mean independence is a reasonable way forward. It is also the case that if miscarriage were correlated with separation through a channel other than birth, the major channel is likely emotional trauma following pregnancy loss. This has been shown to be more acute at later gestational ages (see Gold et al. 2010), yet here there was no significant evidence of separation rates rising with gestational age.

Table 4 shows conditional means for the two estimation samples by the pregnancy outcomes. Under Assumption (I), miscarriages and births showed 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 births and abortions were included in the non-miscarriage group, these differences disappeared except for smoking (and drinking is still significant at the 10 % level). This tells us the miscarriage group is a sample mixing some women who would have given birth, and some who would have aborted, had they not miscarried. The OLS estimates

Table 4 Mean characteristics by pregnancy outcomes

Characteristic	Assumption (I)		Assumption (II)	
	Birth	Miscarriage	Abortion or birth	Miscarriage
Age	18.89 (0.111)	18.60 (0.274)	18.76 (0.100)	18.60 (0.274)
Black	0.259 (0.036)	0.198 (0.053)	0.240 (0.033)	0.198 (0.053)
Hispanic	0.120 (0.018)	0.111 (0.026)	0.125 (0.018)	0.111 (0.026)
HS grad	0.438 (0.023)	0.502 (0.057)	0.419 (0.020)	0.502 (0.057)
Public fund lost	0.056 (0.021)	0.056 (0.024)	0.057 (0.019)	0.056 (0.024)
Parental consent imposed	0.009 (0.004)	0.029 (0.016)	0.011 (0.003)	0.029 (0.016)
Smoke during pregnancy	0.247 (0.020)	0.362* (0.049)	0.264 (0.017)	0.36* (0.049)
Drink during pregnancy	0.002 (0.001)	0.041* (0.021)	0.029 (0.005)	0.041 (0.021)
AHPVT score	97.07 (0.711)	99.54* (1.336)	98.28 (0.664)	99.54 (1.336)
Mother col. grad	0.140 (0.013)	0.230* (0.031)	0.180 (0.015)	0.230 (0.031)
1-Year separation	0.191 (0.033)	0.180 (0.015)	0.220 (0.014)	0.191 (0.033)

* Denotes miscarriage mean is significantly different at the 5 % level. Standard errors are in parentheses. Separation is measured 1 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

are biased because abortion and miscarriage are competing risks. The lack of significant differences showed 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. Ashcraft et al. (2013) showed similar results using evidence from a different data source, the National Survey of Family Growth. 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 were negatively correlated with the underlying desire to give birth.[16] This is consistent with work on the impact of teenage childbearing which showed women who give birth have lower opportunity costs (Ashcraft et al. 2013; Hotz et al. 1997). One would 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 shrank following the addition of (more) women who obtained an abortion. The same pattern held for maternal education.

Importantly for the approach taken here we also tested whether separation rates were different between those who intended to give birth and those who did not. There were no significant differences between separation rates under Assumptions I or II, and separation rates were actually lower conditional on miscarriage. Thus if miscarriage failed to satisfy the exogeneity assumption, these conditional means suggest it is negatively correlated with separation and thus using it as an instrument biases our IV estimates towards zero.

Results

OLS and IV Estimates

The first stage estimates of (3) are presented in Tables 5 and 6, 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. Abortion was set as the excluded group and miscarriage and its three interaction terms were included. Although not presented in the table, a list of included instruments is provided in the Appendix. In Column 4, the birth equation, one can see the counter-intuitive policy effect on minors outlined above, the removal of public funding reduced 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 provided the identifying power for the first stage. The R^2 indicates miscarriage was indeed highly correlated with being a birth-type. The AP F tests showed miscarriage is not a weak instrument. Table 6 presents a similar pattern, though the policy changes' influence on birth is no longer significant once miscarriage was controlled for. Also the KP- F statistic for parental consent changes was 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. Therefore

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

Table 5 First stage estimates

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

All estimations include abortion cost shifters and state and year fixed effects. Standard errors in parenthesis. Columns are separate regressions. Standard errors are clustered at the state level. The KP-F test statistic for the instruments is 22.1

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

Table 6 First stage estimates

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

All estimations include abortion cost shifters and state and year fixed effects. Standard errors in parenthesis. Columns are separate regressions. Standard errors are clustered at the state level. The KP-F test is 3.22

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

these should be viewed as a robustness check on the funding results.

Estimates using changes in abortion funding policies as a cost shifter are presented in Table 7. Separation is measured at 1 year following pregnancy and the OLS sample uses only those giving birth or having a miscarriage whereas IV includes women who had abortions. Controls were added sequentially across the three specifications presented in the table, beginning by including only cost shifters. The triple interaction showed 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 persisted as controls were added. In specification (ii) own and partner characteristics like age, race, and education

Table 7 OLS and IV separation estimates-changes in abortion funding

Cost interactions	Specification					
	(i)		(ii)		(iii)	
	OLS	IV	OLS	IV	OLS	IV
Birth × funding lost × <median income	−0.867* (0.355)	−0.979* (0.476)	−0.783** (0.286)	−0.911* (0.398)	−0.736** (0.276)	−0.864* (0.394)
Birth × <median income	0.064 (0.071)	0.051 (0.082)	0.079 (0.085)	0.079 (0.097)	0.111 (0.074)	0.126 (0.087)
Birth	−0.025 (0.056)	0.047 (0.072)	−0.020 (0.061)	0.059 (0.082)	−0.024 (0.053)	0.026 (0.069)
Public funding lost	−0.163** (0.064)	−0.182** (0.054)	−0.249** (0.074)	−0.281** (0.079)	−0.312** (0.101)	−0.321** (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		Yes	
State and year FE & county level covariates	No		No		Yes	

Standard errors in parenthesis. Columns are separate regressions. Individual and county level observables are listed in the "Appendix" section. Abortion costs include state level indicators for abortion policy (funding, consent, and waiting period). Standard errors are clustered at the state level

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

were added. Finally adding county level controls for income, density, religiosity, political measures, and even including state fixed effects did not appreciably change the estimates on the triple interaction. Other coefficients in the table suggested that 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.[17] Combining the relevant coefficients low-income mothers who experienced the removal of public funding saw a decrease in their likelihood of separation of between 25 and 32 %, relative to those not giving birth. One reason for such large effects may be that the margin of women affected by legal change are particularly sensitive in terms of match quality (for example, very low quality matches dissolve, and high quality matches continue).

Table 8 shows the results for a similar estimation where the variation in abortion costs came from the imposing of parental consent laws. The triple interaction for women who gave birth in areas where abortion costs increased was again negative for the relevant group, minors. Adding controls at the individual, county and state level did not change the essential range of estimates on the triple interaction. Again parental consent laws appeared to change 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.

Marriage and Cohabitation

Given the results above, a question of interest is whether the formal implication of Akerlof et al. (1996) holds today, namely do rising abortion costs increase marriage following pregnancy? The results on this point are mixed and suggest a more nuanced theory of post-birth relationship dynamics than the original work of Akerlof et al. (1996).

The dependent variable was changed from Eq. (1) to an indicator of whether the biological couple ever married following the pregnancy, and also an indicator of whether they ever married or cohabited following the pregnancy. The final age women were observed was between 20 and 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. Estimates from these specifications are presented in Table 9. The upper panel presents results for funding changes, and the lower panel for parental consent changes. In both panels, using both OLS and IV, there were no significant impacts on marriage.

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

Table 8 OLS and IV separation estimates-changes in parental consent

Cost interactions	Specification					
	(i)		(ii)		(iii)	
	OLS	IV	OLS	IV	OLS	IV
Birth × consent imposed × minor	−1.397*** (0.169)	−1.492*** (0.163)	−1.428*** (0.140)	−1.406*** (0.130)	−1.421*** (0.137)	−1.418*** (0.158)
Birth × minor	0.066 (0.084)	0.070 (0.116)	0.067 (0.091)	0.121 (0.124)	0.101 (0.083)	0.110 (0.126)
Birth	−0.037 (0.039)	0.011 (0.051)	−0.020 (0.039)	0.029 (0.052)	−0.019 (0.037)	0.027 (0.049)
Parental consent imposed	−0.179** (0.065)	−0.183** (0.057)	−0.190** (0.059)	−0.192** (0.058)	−0.219** (0.073)	−0.219** (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 partner information	No		Yes		Yes	
State and year FE & county level covariates	No		No		Yes	

Standard errors in parenthesis. Columns are separate regressions. Individual and county level observables are listed in the "Appendix" section. Abortion cost include state level indicators for abortion policy (funding, consent, and waiting period). Standard errors are clustered at the state level

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

However, examining cohabitation there were positive and significant impacts for the triple interaction terms. This suggests the decrease in separation likelihood was linked to increased cohabitation. These results are consistent with a number of explanations. Firstly, in a certain sense the theory of Akerlof et al. (1996) could be either wrong or outdated. Their theory was meant to address the norms of the pre-1970s US marriage market, where cohabitation was very rare and the typical age at marriage considerably lower than today. It could be that minors and poor women, those subject to exogenously increasing abortion costs, are simply much less likely to marry today. The relevant form of commitment more broadly defined may be cohabitation. 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 partners. 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 finding no effects on marriage. This would also be consistent with the high levels of delinquent fatherhood among low income men. Beauchamp et al. (2015) showed nearly 70 % of men not living with biological children gave no financial support, implying the financial benefits of more casual unions are relatively large.

Robustness Checks

The work of Fletcher and Wolfe (2009) cast doubt on whether miscarriages are in fact random by pointing out that unobserved community level factors influenced both miscarriage and pregnancy choices (birth and abortion). The main strategy of Fletcher and Wolfe (2009) was to use the community level controls in Add Health, and we do the same. Some controls they used, such as the 1995 abortion regulations at the state level, are included directly in the above specifications already. The state policy variation allows us to conduct a further robustness check. In Table 10, Eq. (1) was re-estimated for the impact of funding changes but 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 was significant, with the same pattern of results as above, with increased cohabitation.[18]

Including school fixed effects removed the impact of the school level likelihood to abort or give birth, and dealt with unobserved neighborhood characteristics as discussed in Fletcher and Wolfe (2009). The fact that the results did not change is likely due to the extensive list of controls at the state, county and individual level already included.

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

Table 9 OLS and IV marriage and cohabitation estimates

Low income effects	Ever married		Married or cohabit	
	OLS	IV	OLS	IV
Specification				
Birth × funding lost × <median income	0.074 (0.228)	−0.075 (0.316)	0.406[a] (0.209)	0.433 (0.294)
Birth × <median income	−0.030 (0.069)	0.021 (0.089)	−0.192* (0.083)	−0.184[a] (0.113)
Birth × funding lost	−0.037 (0.151)	0.106 (0.213)	−0.056 (0.274)	−0.015 (0.325)
Birth	0.112 (0.038)	0.045 (0.066)	0.132 (0.056)	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[a] (0.216)	−0.127 (0.263)	0.649* (0.230)	0.986** (0.333)
Birth × minor	−0.131* (0.075)	−0.185* (0.114)	−0.057 (0.081)	−0.168 (0.117)
Birth × consent imposed	0.234 (0.171)	0.263 (0.186)	0.204* (0.101)	0.216* (0.100)
Birth	−0.185 (0.188)	−0.196 (0.188)	−0.702*** (0.115)	−0.726*** (0.111)
N	1438	1859	1438	1859
R^2	0.295	0.271	0.289	0.240
Abortion cost shifters	Yes		Yes	
Individual and partner information	Yes		Yes	
State and year FE & county level covariates	Yes		Yes	

Standard errors in parenthesis, clustered at the state level. Columns are separate regressions

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level. Cohabitation refers to any non-marital co-residence with the relevant partner in the past

Table 10 Robustness checks

	Separated		Married or cohabit	
	OLS	IV	OLS	IV
With school fixed effects				
Birth × funding lost × <median income	−0.926** (0.277)	−0.925* (0.472)	0.407** (0.185)	0.308 (0.271)
N	1438	1859	1438	1859
Including male reporting				
Birth × funding lost × <median income	−0.673* (0.307)	−0.813* (0.382)	0.511** (0.192)	0.640* (0.275)
Birth × consent imposed × minor	−1.343*** (0.141)	−1.389*** (0.143)	0.695*** (0.248)	0.965*** (0.382)
N	1645	2147	1645	2147
Including multiple pregnancies and men				
Birth × funding lost × <median income	−0.591[a] (0.321)	−0.722[a] (0.393)	0.520* (0.231)	0.635* (0.296)
Birth × consent imposed × minor	−0.405 (0.102)	−0.212 (0.224)	−0.442 (0.301)	−0.565 (0.460)
N	2053	2690	2053	2690
Same-state sample				
Birth × funding lost × <median income	−1.068*** (0.166)	−1.119*** (0.171)	0.194*** (0.094)	0.164 (0.146)
N	1321	1683	1321	1683

Standard errors in parenthesis, clustered at the state-level. 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

*, ** and *** denote significance at the 5 and 1 and 0.1 % levels respectively

[a] Represents significance at the 10 % level

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 tightening abortion regulation.

Finally, the use of miscarriage as a natural experiment raises questions about the power of the test. To address this the sample was expanded and the above specifications re-estimated. By ignoring variables measured at Wave I, one can use male reporting of pregnancies, which adds approximately 300 observations to the IV-estimation sample, with results reported in Table 10. Also included in the estimation sample were multiple pregnancies, which when combined with the male sample added over 800 observations. Again the results for funding changes were nearly identical, while the impact of consent law changes were not significant. Despite the small number of miscarriages in the original sample, with the expanded sample the results indicated a robust difference in separation and cohabitation rates based on whether one gives birth when abortion costs are rising.

Another problem with the sample used above is that moving across state-lines is responsible for some policy variation in public funding laws. In the event that moving is correlated with legal changes, the results could be written off as saying women who move are more likely to separate. The final panel in Table 10 re-runs the analysis above on the sub-sample of women residing in the same state in Waves I and III. Given the age of the sample it is highly unlikely that these women experienced a cross-state move, which would require three moves before age twenty-five. As the results show, the effects found are concentrated among a group of poor women who did not move, but experienced the removal of public funding.[19] The basic finding holds up, low-income women who experienced a birth saw a substantial reduction in their likelihood of separation relative to those who miscarried or aborted.

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. However one can exploit the presence of policy changes which induced plausibly exogenous increases in abortion costs for poor women, using micro data to identify groups of women for whom a policy change was binding. OLS estimates from these policy changes showed that separation following a birth was substantially less likely when abortion costs were increased. This result was further strengthened by using miscarriage as a natural experiment to bound the effect of birth on outcomes, providing a check on the OLS estimates. Following this approach we tested whether evidence of the basic incentives of Akerlof et al. (1996) are present in the modern market marriage market, as abortion costs rise there is an increase in the likelihood of relationships between the biological parents? Our results did not confirm that raising abortion costs increased marriage rates, however there was evidence that it substantially decreased separation rates and increased cohabitation following a birth.

Easier access to abortion may lead to more pregnancies (see Levine and Staiger 2004) or more relationships with partners who do not make acceptable fathers. Another possibility is that as abortion costs fall, men find it easier to meet and match with women who do not want children. While our evidence cannot distinguish between these scenarios, it nonetheless makes understanding how relationship dynamics respond to fertility control an important issue, because abortion costs spillover onto the relationship terms and sexual norms of those who give birth. What is perhaps most striking about the results here is that we detected changes in these norms in the opposite direction (for example, increased cohabitation) from only public funding changes.

The findings here are relevant for policies related to family structure, reproductive health and child poverty. Our estimates mean that promoting access to technologies (for example, lower cost abortion) without attention to the behavioral responses in the marriage market can prove counter productive. Take the case of child-support enforcement laws, which have been strengthened dramatically since the early 1970s, while child poverty rates have remained stubbornly high. Our estimates show that some "child-support" is provided voluntarily by co-residing fathers in our data when abortion laws were tightened. This suggests that easy access to abortion may have undercut the gains which would have accrued to improved child-support enforcement. Finally this means that, similar to Levine and Staiger (2004), there may be an optimal level of abortion costs which minimizes these negative consequences of access.

Appendix

The following sets of controls are 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

[19] The size of the coefficients on the triple interaction are larger than one, but the effect is given by adding this to other (positive) interaction terms.

pregnancy; 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: county percent voting Republican and Democrat in 1992 presidential election; Marriage: census fraction of males never married, county level.

Fixed Effects

Fixed effects for thirty states and years from 1995 to 2002 are included.

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