The Labor Force Effects of Unplanned Childbearing*

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Abstract

This paper explores the impact of unplanned births on female labor force participation and income. I estimate the causal effect of birth on a sample of unplanned pregnancies, defined as those that happened while the woman was using contraception. Labor supply and the decision to give birth are jointly determined. Women with high labor force attachment may be more likely to use contraception or to have an induced abortion if contraception fails. I use spontaneous fetal losses as a source of exogenous variation in births. Unplanned births significantly reduce labor force participation, by as much as 25%. This effect is remarkably higher than the estimates traditionally reported in the literature, suggesting that family planning plays a key role in the limited magnitude of previous estimates. The negative impact decreases over the sample period, 1973-2005. There are no significant differences in the effect of an unplanned birth by level of education and its impact on income is small.

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1 Introduction

In 2002, 30% of American women 15 to 44 years old had ever experienced an unintended birth (Chandra et al. (2005)). Either unwanted or mistimed, the prevalence of unintended fertility and its persistence over time is a striking fact given the available contraceptive technology. Its occurrence is higher among women whose wealth is below 150% of the poverty level and high school dropouts (over 50% and 60%, respectively, had ever had an unplanned child), but it is not negligible among highly educated women, of whom 18% experienced an unintended birth. Moreover, unplanned childbearing appears not to be a unique event. In the period of 1997-2002, only 69% of all births were intended, but this percentage represented an increase from the 65% of births intended from 1990-1995 (Abma et al. (1997)).

Unintended pregnancies are correlated with delayed access to prenatal care, lower birth weight, and poor child care and development (Kost et al. (1998), Joyce et al. (2000)), but they also affect the woman's ability to participate effectively in the workforce, which in turns impacts her income and poverty status. The prevalence of unplanned pregnancy and its negative consequences induced policy measures, such as the expansion on eligibility for Medicaid coverage of family planning. In 2006, more than nine million women received publicly-funded contraceptive services (Gold et al. (2009)), which cost federal and state governments $1.85 billion. Kearney and Levine (2009) estimate that the expansion on Medicaid family planning eligibility, which occurred at the state level from 1993 to 2008, reduced births on newly eligible women by 9%.

The effect of a birth on maternal labor supply has been profusely studied in economic literature. Goldin (1990) proposes reductions in fertility as the prime mover of the large increase in female labor force participation after WWII. However, the impact of a birth on labor supply is, although significant, of moderate magnitude (Angrist and Evans (1998), Bronars and Grogger (1994), among others), and fails to fully explain the extent of changes in participation that took place during the last century. These estimates take into account the reduction of total parity, but they do not consider the impact of family planning on a woman's ability to postpone and schedule births conveniently. Goldin and Katz (2002) tie the introduction of the birth control pill with increases in female attainment of post-secondary and graduate education, by giving women the faculty for scheduling their pregnancies. Bailey (2006), meanwhile, argues that traditional findings in the literature underestimate the impact of highly effective contraception, since they abstract from the role of birth timing. Miller (2010) and Herr (2007) link postponing one's first birth with better wage growth and career perspectives.

In this paper, I address the impact of unplanned births on female labor force participation and how family planning modulates this causal effect. I select a sample of unplanned pregnancies and compare the results of the estimation with those of a sample of fully intended pregnancies and with results previously reported in the literature. Unplanned pregnancies, defined as those which happened to women actively trying to prevent them through the use of contraception, are trivially mistimed, while planned pregnancies, that happened after the woman stopped contraception in order to become pregnant, are eagerly anticipated. Estimating the effect of unplanned births will shed light on the extend to which family planning drives the modest impact of birth on female labor supply, while providing a consistent estimate of the effect of a surprisingly common event as an unplanned birth.

The estimation of the impact of birth on female labor force participation is usually complicated by the joint determination of fertility and labor supply. Women choose jointly labor supply, fertility, and its timing. When an unplanned pregnancy occurs, however, the fertility choices available for the woman are no longer whether to have a child and when to do so, but only whether to take the pregnancy to term or to terminate through an induced abortion. Taking an unplanned pregnancy to term is endogenous to a woman's unobserved preference for working, and the estimation of the causal effect necessitates an exogenous source of variation in family size. I use spontaneous fetal losses as such a source to achieve unbiased estimates.

Biological fertility shocks have been previously used as exogenous variation in family size. Hotz et al. (2005) and Ashcraft and Lang (2006) use miscarriage as an instrument for birth to estimate the effects of teenage childbearing, while Miller (2010) instruments for age at first birth using age at first conception, occurrence of miscarriage, and contraception use. Similarly, Herr (2007) explores the effect of delayed childbearing on women's wage growth using miscarriage and contraception as instruments for age at first birth. Bronars and Grogger (1994) and Jacobsen et al. (1999) estimate the effect of an additional child by comparing mothers who gave birth to twins with those who gave birth to singletons. In addition, parents' preferences for a mixed-sex set of children make them more likely to have a third child if the first two are of the same sex. Angrist and Evans (1998) use the randomly assigned sex of the two first children to instrument for the presence of a third child and estimated the effect on the labor supply of females who already had two children.

As a preview of the findings, an unplanned birth reduces female labor force participation by as much as 25%. The impact of an unplanned birth is significantly higher than the ones previously reported in the literature, indicating that planning and timing of fertility plays a key role in the effects of childbearing on labor force participation. The negative effect, however, decreases over the sample period. There is no evidence of an unplanned birth affecting differently low- and highly-educated females. This lack of differences suggests that the more negative impact of birth for low educated females previously found might be driven by differences in the planning of the pregnancies.

The paper is organized as follows. Section 2 presents the empirical strategy and argues the exogeneity of miscarriage. Section 3 describes the data used for the analysis. Section 4 presents the results, and section 5 discusses the findings and concludes.

2 Empirical strategy

Childbearing and female labor supply choices are, in general, the product of the household maximization problem, hence a mere correlation among fertility and labor force status is not informative of a causal relation. Women jointly decide their labor supply and their fertility, choosing the number of children and their timing. However, in the case of an unplanned pregnancy, the optimal choices are disrupted. An unplanned pregnancy is defined here as a pregnancy that happened while the woman was using any type of contraception. Although its occurrence is correlated with the contraceptive method chosen and its proper use, an unplanned pregnancy causes the woman to re-optimize her fertility choices, opting either to take the pregnancy to term or to obtain an abortion. This choice is endogenous to unobservables determining labor force participation, and therefore the estimation requires a source of exogenous variation. Spontaneous fetal losses[1], which are conditionally random given pregnancy, are used in this analysis to identify the causal effect of

[1] Here, miscarriage and spontaneous fetal loss will be used indifferently, including miscarriages, tubal pregnancies, and stillbirths.

a birth on labor force participation and other outcomes of interest.

The results obtained in the case of unplanned pregnancies are compared with a sample of planned pregnancies. A pregnancy will be considered as planned if the woman reported having fully stopped all contraceptive use prior to the pregnancy and having done so in order to achieve conception.

The degree of desirability of a pregnancy can be affected by recall bias. Moreover, accurateness of recall can be different depending on the outcome of the pregnancy. Since I use miscarriages as an instrument, I restrict the sample to pregnancies in which a level of planning can be imputed based on factual information. Contraception use, in the case of unplanned pregnancies, and its discontinuation, in the case of planned pregnancies, are used to define the samples, preventing bias on a subjective opinion from affecting the estimation. However, these definitions leave a significant number of pregnancies as neither planned nor unplanned. For instance, pregnancies of women who did not use contraception at all in order to become pregnant will not be considered as planned.

Causal effect of childbearing

The effect of childbearing on the female labor supply is estimated for women who experienced an unplanned pregnancy, and the results are compared with a sample of planned pregnancies. In the case of planned pregnancies, there are virtually no abortions. This feature simplifies the estimation, which is straightforward and is presented first. In both cases, the estimation strategy rely on the conditional randomness of miscarriage, which will be discussed in detail later.

Planned pregnancies This sample is composed of women who were actively seeking a pregnancy. Having adopted any type of contraceptive method before the pregnancy, they discontinued its use in order to conceive. Information on how long took for the women to conceive after stopping all contraceptive methods is not available, but women unsuccessfully trying to conceive for a long period of time are likely to request medical help. A robustness check dropping women who required help to become pregnant or prevent miscarriage does not show any evidence of bias on the estimates.

Given that pregnancies have been intended, the abortion rate is negligible. In absence of abortions, miscarriages are, as it will be discussed below, conditionally random. Let Y be the outcome of interest (labor force status, female earnings, or total family income), Y_{Birth} the realization of the outcome in case of birth, and $Y_{NoBirth}$ in case of absence of birth, such that the observed outcome is $Y_{obs} = Birth * Y_{Birth} + (1 - Birth) * Y_{NoBirth}$, where $Birth$ is a dummy that takes value 1 if the pregnancy ended in birth and 0 otherwise. The effect of interest is given by $E(Y_{NoBirth} - Y_{Birth}|Birth)$, but $Y_{NoBirth}$ is trivially unobservable for women who give birth. Since miscarriages are random within this group, $E(Y_{NoBirth}|Birth) = E(Y_{NoBirth}|Miscarriage)$. It follows that $E(Y_{NoBirth} - Y_{Birth}|Birth) = E(Y_{NoBirth}|Miscarriage) - E(Y_{Birth}|Birth)$, both of which are observed. Therefore, OLS estimator will provide consistent estimates of the effect of childbearing on labor force status or other outcomes for women who planned their pregnancies.

Unplanned pregnancies The main sample used in the analysis includes unplanned pregnancies, which, again, are pregnancies by women who were using contraception but nevertheless conceived. In the absence of abortions, and if contraceptive failures were random, women giving birth in this group would be randomly selected among those who did not plan on having a child. It would therefore be possible to estimate the size of the bias generated by not accounting for joint determination of fertility and labor supply. However, when abortion is an option, the estimate using miscarriage will capture the effect of birth on those who decided not to abort their pregnancy but instead to take it to term (treatment on the treated).

The estimation of the effect of childbearing for this sample is complicated by the presence of abortion, which causes miscarriage to not be random. Whereas in the case of planned pregnancies women always wanted to have a child, in this sample women choose whether to have a child or to terminate the pregnancy. While the woman decides whether to give birth (to be a birth type) or to have an abortion (to be an abortion type), there is a random probability that a miscarriage would occur (the woman will be a miscarriage type with a certain probability independently of her choosing to be a birth type or an abortion type). The following figure illustrates the possible cases and their outcomes:

	No Miscarriage-type	*Miscarriage-type*
Birth-type	Birth	Miscarriage
Abortion-type	Abortion	Miscarriage/Abortion

If the woman chooses to give birth, the outcome observed will be $Y_{obs}^{Birth-type} = Y_{Miscarriage} * MiscType + Y_{Birth} * (1 - MiscType)$, where $MiscType = 1$ if the female is miscarriage type. If the woman chooses to have an abortion, it will be observed if no miscarriage was scheduled to occur (i.e., the woman is an abortion type but not a miscarriage type). However, if the woman is both abortion type and miscarriage type, the observed outcome will depend on the relative timing of abortion and miscarriage. If the miscarriage occurs before the woman obtains an abortion, the miscarriage will be observed, but if the fetal loss was scheduled to occur later in pregnancy, the woman would have aborted the pregnancy before and the observed outcome will be abortion. Therefore, $Y_{obs}^{Abortion-type} = Y_{Abortion} * \{(1 - MiscType) + LateMisc * MiscType\} + Y_{Miscarriage} * MiscType * (1 - LateMisc)$, where $LateMisc$ takes value one when the miscarriage was scheduled to occur "late", i.e., after the abortion was performed, and value 0 when the miscarriage happens before the abortion and is observed.

When a miscarriage is observed, it is not possible to determine whether the observed outcome comes from a woman who would have chosen to give birth or to have an abortion, $Y_{obs} = Y_{obs}^{Birth} * Birth - type + Y_{obs}^{Abortion} * Abortion - type$, since the woman type is not observed. Similarly, when an abortion is observed, it is not possible to know whether a miscarriage was scheduled to occur later on in pregnancy (i.e., whether $MiscType = 0$ or $MiscType = 1$ and $LateMisc = 1$), since the miscarriage-type is not observed either.

Abortions are not a random sample of unintended pregnancies. In the data used in the analysis, women obtaining abortions after an unplanned pregnancy are positively selected among highly educated women. They are more likely to have never been married when the pregnancy occurred and more often white than African-American. Therefore, the miscarriages that will not be observed due to abortions are not a random sample of all miscarriages. In order to provide an accurate estimate of the effect of childbearing, I follow the strategy presented in Ashcraft and Lang (2006) to find bounds on the cost of teenage childbearing.

If pregnancies ending in abortion are dropped from the sample, the remaining miscarriages will not be a random sample of births, since some would have ended in abortion in a miscarriage-free environment. For instance, abortions might be chosen by women with better perspectives in case of absence of birth, s.t. $E(Y_{NoBirth}|Abortion - type) > E(Y_{NoBirth}|Birth - type)$. In this case, $E(Y_{NoBirth}|Miscarriage) > E(Y_{NoBirth}|Birth)$, as miscarriages include some women who would have had an abortion had they not miscarried[2]. Therefore, it would be the case that $E(Y_{NoBirth} - Y_{Birth}|Birth) < E(Y_{NoBirth}|Miscarriage) - E(Y_{Birth}|Birth)$. An OLS estimator of the effect of birth after dropping abortions from the sample will provide an estimate biased towards finding a more negative effect of childbearing.

Instrumental Variables estimation provides the other bound of the effect of childbearing on females trying to avoid pregnancy, where miscarriage is used as an instrument for absence of birth. Once again, the presence of non-random abortion biases this estimation. Abortion type females are overrepresented in the sample not suffering a miscarriage, since some happened before the miscarriage was scheduled to occur ($MiscType = 1$ and $LateMisc = 1$). The Wald estimate of a birth on the outcome is given by $\beta_{IV} = \frac{E(Y_{obs}|Miscarriage) - E(Y_{obs}|NoMiscarriage)}{P(Birth|NoMiscarriage)}$. While birth type females are randomly assigned between women suffering a miscarriage and not suffering a miscarriage, abortion types are overrepresented on the latter group, since abortions prevent some spontaneous fetal losses from happening. If abortion types have a better outcome in case of no birth, $E(Y_{obs}|Miscarriage) - E(Y_{obs}|NoMiscarriage) > E(Y_{obs}|Miscarriage, Birth - type) - E(Y_{obs}|NoMiscarriage, Birth - type)$ because the proportion of abortion types is higher in the group not suffering a miscarriage.

The estimates here correspond to the behavioral response of women rather than the cost, and it might be that the selection of abortion works on the opposite direction. If this is the case, the bounds will be reversed, but the estimation strategy is nevertheless valid.

Women reporting abortions as miscarriages are a concern in the case of unplanned pregnancies. However, the effect of misreporting will be similar to miscarriages happening earlier in pregnancy, when some of the miscarriages observed happen to females who would have aborted otherwise. Miscarriages that occurred before abortions bias the OLS estimation (there are more abortion types in the miscarriage group) and therefore the bias of the OLS will be enhanced. However, the bias of the IV estimates comes from abortions that happened before miscarriages take place.

[2]More formally, $E(Y_{NoBirth}|Miscarriage) = E(Y_{NoBirth}|Birth - type = 1, MiscType = 1) * \frac{P(Birth-type=1,MiscType=1)}{P(Miscarriage)} + E(Y_{NoBirth}|Abort - type = 1, MiscType = 1, LateMisc = 0) * \frac{P(MiscType=1,Abort-type=1,LateMisc=0)}{P(Miscarriage)}$ and $P(MiscType = 1, Abort - type = 1, LateMisc = 0) > 0$

Misreporting of abortions will attenuate the bias, reducing the difference in the proportion of women who would choose abortions between those reporting a miscarriage and those not reporting one[3]. Therefore, the estimation strategy will be valid as far as the misreporting of abortions as miscarriages is limited. Since the rate of miscarriage of unplanned pregnancies lies within the range accepted by the medical literature, this appear to be the case.

Randomness of miscarriage

The principal assumption underlying the estimation strategy is that miscarriages are conditionally random events; that is, the risk of suffering a spontaneous abortion is uncorrelated with unobservables affecting both fertility and labor force choices. Nonetheless, occurrence of miscarriage can be affected by female behavior. This section includes a brief review of the literature on miscarriages, with special attention paid to the causes and characteristics that potentially affect its exogeneity with respect to labor force status.

A miscarriage is defined as the spontaneous end of a pregnancy before the fetus has reached a viable gestational age (Regan and Rai (2000)), but its causes are still subject to debate due to lack of proper epidemiological data. The rate of fetal loss can be as high as 25% of recognized pregnancies. Physiological factors such as chromosomal aberrations, uterine anatomic defects, or certain endocrinopathies have been described as elements that increase the risk of spontaneous fetal loss. However, the most important single predictor is the presence of two or more previous miscarriages (Regan et al. (1989)). Recurrent miscarriages are a well-documented condition suffered by a small percentage of females of reproductive age, but its causes have not been precisely determined. Women whose only pregnancy ended in miscarriage have a 20% risk of suffering another fetal loss, while a never-pregnant woman, or a woman with at least a live birth, present a lower miscarriage hazard, with a miscarriage rate of only 5% in this last case (Regan et al. (1989)). It is unlikely that the occurrence of recurrent miscarriages is correlated with unobservables affecting labor force participation. However, whether a woman suffering this condition continues to attempt childbearing might be. Nevertheless, the results are robust to dropping women who ever asked their doctor

[3] This will be the case if misreporting of abortions is conditionally random. However, if women who misreport are selected closer to the ones who would have a child, the biases described will be attenuated. If, on the other hand, they are selected from the other end of the distribution, the biases will be enhanced.

about how to prevent a miscarriage or how to become pregnant.

The medical literature has not reached a consensus on whether previous induced abortions increase the risk of a miscarriage. While some studies find an increase of almost 50% after an induced abortion, with an exponential increase after the second abortion (Infante-Rivard and Gauthier (1996)), Hogue et al. (1982) report no significant relation between spontaneous and induced abortions. Women opting to terminate their pregnancies through an induced abortion are, in the sample used in the analysis, more educated and therefore might have a stronger attachment to the labor force. The data includes information on every pregnancy a women had and its outcome, allowing to control for previous abortions. Therefore, correlation between previous induced abortions and miscarriages will not bias the estimates. There is no consistent evidence that oral contraception intake affects the risk of a miscarriage (Srisuphan and Bracken (1986)), but the use of IUD has been reported to be correlated with spontaneous fetal losses.

Age at conception has a non-linear effect on the risk of miscarriage. The hazard of fetal loss decreases with age for very young women and it is constant after 16. Once a woman turns 35 years old, the probability of suffering a spontaneous abortion sharply increases: women older than 40 are five times more likely to miscarry than women between 31 and 35 (Smith and Buyalos (1996)). Data on age at conception is consistently recorded, allowing to control for delays in childbearing, which are likely correlated with female unobservable preferences for working.

Miscarriages that happen early in gestation are often asymptomatic. Recognition of an early miscarriage requires the woman to be aware of the pregnancy for it not to be confounded with a late period. Pregnancy recognition is correlated with age at conception and education, while the introduction of the home pregnancy test improved early identification over time (Lang and Nuevo-Chiquero (2010)). Spontaneous fetal losses that happened early in gestation are more likely to be recognized by older, more educated women, who tend to have a stronger attachment to labor force. However, including age at pregnancy and education as covariates appears to control for differences in awareness of pregnancy. The results are robust to restricting the sample to pregnancies lasting long enough for a women to realize her pregnancy under normal circumstances[4].

[4]The robustness check was performed on pregnancies reported to have lasted at least 9 weeks. At 9 weeks of gestation, the woman would have missed already two periods and the miscarriage is likely to need some kind of medical attention.

Herr (2007) finds significant correlation between the probability of miscarriage and observable characteristics for a sample of unmarried females; for married women the correlation was insignificant. Although correlation with female observable characteristics does not invalidate the instrument, the fact that this correlation appears only for unmarried women casts some doubts on the exclusion restriction. If unmarried females are more educated on average, awareness of pregnancies, or a higher incidence of abortion, can drive this result. However, some abortions can be reported as miscarriages; this might happen more frequently with unmarried females, who are potentially less interested in becoming mothers than married females. As mentioned above, the estimation strategy is robust to limited misreporting, which, given the rate of spontaneous fetal losses in the sample, appears to be the case.

Risk factors contingent on a woman's actions during pregnancy are smoking, caffeine, alcohol and drug intake (Garcia-Enguidanos et al. (2002)). Therefore, although female behavior affects the probability of suffering a miscarriage, it does so only through well-known channels. Unfortunately, the data used in the analysis does not include consistent information on drinking, smoking, or drug intake. Previous robustness checks on the validity of miscarriage as an instrument include Hotz et al. (1997), hereafter HMS, also used by Hotz et al. (2005). HMS construct "bounds" for the Wald estimate of the effect of birth on different outcomes assuming there is a proportion of women who would choose to terminate their pregnancy through a non-random miscarriage in the absence of a random fetal loss. Upper and lower bounds are constructed assuming that such women are selected from the top and the bottom of the outcome distribution of women who miscarry. Similar bounds are constructed for the different samples used in the analysis, addressing both concerns that a higher probability of miscarriage occurred to positively (through awareness) or negatively (through engaging in risky activities) selected women who are suffering miscarriages. The degree of selection needed for the bias of miscarriage to significantly affect the estimates is of at least 25% of miscarriage being non-random, higher than the one used in HMS[5], which ensured the absence of a significant bias due to engagement in risky activities.

[5]HMS constructed a measure of the probability of non-random miscarriage using data from risky activities from the National Longitudinal Survey of Youth 1979, and the effect of such activities on the probability of miscarriage from Kline et al. (1989). The percentage of non-random miscarriages was estimated to be below 20% for a population (teenagers) that is more likely to engage in risky activities. In addition, the prevalence in the whole population has decreased over time.

In short, spontaneous fetal losses are influenced by female behavior, either through engaging in risky activities, by awareness of miscarriage, or by reporting an induced abortion as a spontaneous one. The lack of exogeneity of the instrument will cause a bias in the estimate according to its relation with unobservables driving both fertility and labor supply. While low-educated females are less likely to be in the labor force and are more likely to engage in risky activities, highly educated women exhibit a stronger attachment to the labor force and are more likely to recognize an early miscarriage. A robustness check restricting the sample to pregnancies lasting for more than 9 weeks confirms that differences in recognition of early miscarriages does not significantly bias the results. Finally, the HMS procedure shows that lack of exogeneity, if any, does not significantly bias the estimates. Therefore, given pregnancy, miscarriage is an adequate instrument for birth.

3 Data

The data used in the analysis is obtained from waves III to VII of the National Survey of Family Growth (hereafter NSFG), administered by the National Center for Health Statistics in 1982, 1988, 1995, 2002, and 2006-2008. The NSFG collects information on family life, infertility, use of contraception and women's health. The third wave was the first to survey never married females, while waves I and II, in 1973 and 1976, surveyed only women who had ever married.

The NSFG interviewed a nationally representative sample of non-institutionalized women aged 15 to 44 at the time of the interview. Retrospective information was collected on every pregnancy experienced, along with personal characteristics such as education attainment, marital status, race, religion, or parental education. Labor force status at the time of the interview was consistently recorded in all waves, as well as total family income[6] in the 12 months prior to the survey. In addition, waves III to V collected data on female income if working and husband's or partner's income if married or cohabiting, both during the year preceding the interview. Female usual hours of work are recorded until 1995, but waves VI and VII only included information on whether the woman was working full-time or part-time. Nonetheless, the different waves of NSFG are generally

[6]Total family income includes all income received by the family in the 12 months prior to the interview. It is reported on bins, for which the mean value is imputed. Some females did not report this variable, but there are no zeros: women reporting no income are assigned to the lowest bin, for which a non-zero value is imputed.

comparable, although the exact wording of the questions may vary. In 1995, an extended self-reported section was included, allowing the female to impute by herself information on sensitive areas such as abortion or sexual partners. Therefore, misreporting of abortions might be more frequent in early surveys.

Women were asked about each of their pregnancies, recording the outcome (birth, abortion, miscarriage[7]), the year in which it occurred and her contraception status at the time of conception, which will be used to determine the level of planning of the pregnancy. Although having twins has been used as an exogenous source of variation on family size, the number of multiple births in the data is small and prevents precisely estimating a differential effect on labor supply and income. Therefore, multiple births are dropped from the sample. Additionally, given that the sample is comprised of pregnancies, I drop females who adopted or became guardian of a child. These cases were limited and did not affect the estimates.

Since pregnancies ending in birth last longer than pregnancies ending in miscarriage or abortion, in the closest period to each survey there will be a disproportionately large amount of miscarriages and abortions, while some of the births will still be current pregnancies. In order to avoid interfering with the estimation, the last year prior to the interview for each survey is dropped from the data. Additionally, contraception status was not recorded in the 1982 NSFG for the period 1979-1982, and therefore no pregnancies occurring in this period and reported in wave III are included. Nevertheless, the 1988 NSFG includes observations regarding this time period.

Sample composition

The NSFG includes information about women's contraceptive behavior before each pregnancy. For first pregnancies, questions referred to any point before the pregnancy, and for second and subsequent pregnancies, to the interval from the previous pregnancy. If any contraceptive use[8] was reported, women were asked whether or not they fully stopped it prior to the pregnancy. If

[7]The NSFG differentiates between tubal pregnancy, miscarriage, and stillbirth. However, given that tubal pregnancies and stillbirths are also exogenous variations in family size, they will be included as miscarriages in this analysis. The proportion of tubal pregnancies and stillbirth included is, in any case, small, and never above 3%.

[8]For a short period before each survey, the NSFG collected monthly information on which contraceptive method was in use. Since month of conception is available in the data, it is possible to impute the contraceptive method being used when the pregnancy occurred. Unfortunately, the sample is reduced to 320 observations, and therefore the analysis is performed on all unplanned pregnancies independently of the contraceptive method being used.

contraception was still in use at the time of conception, the pregnancy is considered unplanned. If a woman reports having stopped all use of contraception before the pregnancy, she was asked whether the objective was to conceive. Only pregnancies that happened in this circumstances will be considered planned. These definitions prevent concerns regarding recall bias or misreporting depending on the outcome of the pregnancy. There will be, however, a substantial number of pregnancies that will be neither planned nor unplanned.

Ideally, using all unplanned pregnancies that happened any number of years before the survey will allow to estimate more precisely the effect of birth on labor force and its variation as the child ages. However, an unplanned birth not only affects labor force decisions but also choices regarding marriage and fertility. If the unplanned birth happens to a woman who did not wish to have any other children, the potential birth only changes total fertility and the interpretation of the effect is straightforward. In most cases, however, unplanned pregnancies are not unwanted, but only mistimed: the woman did plan to have a child in the future, and the birth affects not only labor force choices but also posterior fertility choices, changing the desired timing of fertility.

In order to be able to obtain an estimate of the effect of an unplanned birth on labor force participation and other outcomes of interest, most of the analysis will be performed on a sample of last, unplanned pregnancies (i.e., if the last pregnancy a woman reports in the survey is unplanned, she will be included in the sample). After controlling for previous fertility, the estimate will measure the direct impact of the unplanned birth. This restriction only partially solves the endogeneity of future fertility choices: the outcome of a pregnancy might determine the existence of a posterior one. If an unplanned pregnancy ends in birth, it is less likely that the woman chooses to have another child, making the unplanned pregnancy her last pregnancy. Women with a mistimed pregnancy who miscarry will attain another pregnancy, which may not be unplanned, and will drop from the sample. Therefore, the sample will include more than proportionally women who achieve a birth or who had a miscarriage but did not want a child in the future. In the data used in the analysis, the probability that a last, unplanned pregnancy ended in miscarriage presents a strong upward trend in the years since it happened, after controlling for female characteristics. Given that most unplanned pregnancies are only mistimed, this result suggests that the composition of the sample is affected by selection of last pregnancies.

Women who did not want a child at any time in the future and did not obtain an abortion will remain in the sample independently of the outcome of the pregnancy. When considering pregnancies that occurred many years before the survey, most of the miscarriages will have happened to women who did not want another child, since women who did want a child will have become pregnant again. If both types of women are not significantly different in unobservables affecting labor force attachment, the restriction of the sample to last pregnancies will not present problems other than the loss of power implied by the small number of miscarriages (i.e., the comparison group will be smaller but still a random sample of pregnancies ending in birth). If this is not the case, the results will be biased. If career-oriented women, who present a strong attachment to labor force, were more likely to choose to remain childless or to postpone fertility enough to not report any subsequent pregnancy, the impact of a would that happened many years before the survey might be biased towards a negative effect of birth. In order to reduce the significance of the bias, the sample is restricted to pregnancies happened, at most, 10 years before the survey. The results, as it will be shown, are similar to the ones obtained when the sample is further restricted to only a 5-year lapse before the interview. The bias due to selection of last pregnancies, if it exits, is limited.

The restriction to pregnancies that happened only up to 10 years prior to the survey ensures that age composition of women is fairly constant. The NSFG surveyed women aged 15 to 44 at the time of the interview, who will be 5 to 34 on the later year included in the sample. The sample is restricted to pregnancies that occurred after the woman turned 18. Therefore, the sample will be composed of women aged 18 to 34 for the first year in the sample, 18 to 35 for the second year, etc. The sample is therefore not fully consistent by age, but controls for age of pregnancy and age at interview are included. Dropping pregnancies that happened after 34 years of age from the sample does not affect the results. Finally, 1972 is dropped from the sample in order to ensure that all women had access to abortion at the time of the pregnancy[9].

Table 1 presents the descriptive statistics for the different samples that will be used in the analysis, in all cases referring to pregnancies that happened up to 10 years before the survey in which they were reported. For comparative purposes, column (1) includes the descriptive statistics of a sample composed of all pregnancies, planned, unplanned, or neither. Columns (2) to (4) include

[9] The Supreme Court decision on *Roe v. Wade* in 1973 guaranteed the access to abortion to all women in the U.S.

data on unplanned pregnancies, including all unplanned pregnancies, those that were the woman's last pregnancy, and those that were the first, respectively. Finally, the last column includes data on all planned pregnancies.

After imposing the restrictions described above, there are 32,137 pregnancies in the full sample, of which 9,447 were unplanned (i.e., happened when a woman was using contraception) and 8,757 were planned (i.e., happened after a women who was taking contraception stopped doing so). For 2,405 women, their first pregnancy was unplanned, while for 4,149 it was their last pregnancy. Note that females with only one, unplanned pregnancy will be included in both samples. 63% of all pregnancies happened to white women, but only 61% of the unplanned conceptions happened to this group. However, this difference is reversed when only first pregnancies are considered, indicating that recurrent unplanned pregnancies appears to be a more frequent phenomenon among African-American than whites. Also, white women experienced a higher proportion of planned pregnancies. There are no significant differences by religion in which the woman was raised. Females suffering an unplanned pregnancy are slightly less educated, while those planning their pregnancy have significantly higher education.

Marital status at the time of the interview differs by level of planning of the pregnancy. Females whose first pregnancy was unplanned are more likely to be single than when all first pregnancies are considered. However, women with planned pregnancies are more likely to be married and less likely to be divorced. Women with unplanned pregnancies tend to have less children, but gave birth for the first time at an early age. Unplanned pregnancies happen more often to never married females than all pregnancies, which is especially significant in the case of first pregnancies. On the other hand, more than 90% of all planned pregnancies happened to females who were ever married at the time of the pregnancy.

Regarding the outcome of the pregnancy, unplanned pregnancies are less likely to end in birth than all or planned pregnancies. Only 66% of them ended in birth, while 75% of all pregnancies and 87% of planned pregnancies did so. On the other hand, unplanned pregnancies are more likely to end in abortion, up to 16% or 20% when only first pregnancies are considered. Planned pregnancies, as expected, are very unlikely to be aborted, with only 0.6% of them reporting this outcome. Assuming that they are therapeutic abortions and treating them as miscarriages does not significantly change the estimates. However, it is surprising that miscarriage is a more frequent outcome for all and unplanned pregnancies than for planned pregnancies, where there are no abortion preventing miscarriages. This might indicate misreporting of abortion as miscarriages, but, as explained above, the rate of miscarriage lies within the accepted range and the estimation strategy is robust to limited misreporting.

Table 1 goes here

4 Results

This section presents the impact of an unplanned birth on (i) labor force participation, (ii) full-time status if working, (iii) usual hours if working, (iv) total family income, (v) female earnings if working, and (vi) husband's income if married or cohabiting. All income variables are measured for the 12 months prior to the interview.

As explained in detail above, the equation

$$Y = \alpha + \beta Birth + Birth X_1' \lambda + X' \gamma + \varepsilon$$

is estimated by OLS after dropping abortions and by IV using all observations. *Birth* is a dummy variable that takes value 1 if the pregnancy ended in birth and 0 otherwise. The OLS estimation without abortions provides one of the bounds on the effect of birth for females using contraception, while the other bound is given by the IV estimation. The first stage of the IV estimation regress *Birth* on a dummy for the absence of miscarriage ($Birth = \delta_0 + \delta_1 NoMiscarriage + NoMiscarriage X_1' \Lambda + X' \Gamma + \epsilon$). Given that most pregnancies not ending in miscarriage end in birth (around 80%), the first stage is trivially very strong.

The impact of birth on the outcome of interest is allowed to vary by a small subset of characteristics, X_1, namely time since pregnancy, year in which it occurred, and number of children present in the household before the pregnancy. Age of child modulates the effect of fertility stepwise due to schooling. Therefore, giving birth is interacted with having a child less than 6 years old, (i.e., the pregnancy happening less than 6 years ago), while the omitted category is the child being between 6 and 10, the oldest in the sample. The presence of previous children in the household can affect female labor supply or income, as well as modulate the way a new offspring affects the outcome. Therefore, I include controls for the number of children before the pregnancy and age of the youngest child, and allow the effect of birth to vary with the number of previous children. Given that the sample covers a long period of time (1973-2005), exogenous changes in the labor force over the period might impact the response a woman gives to childbearing. Therefore, birth is interacted with a time trend.

Effect of an unplanned birth on total and subsequent fertility Unplanned pregnancies affect female labor supply choices, but they impact subsequent choices of total fertility as well. Given that most unplanned pregnancies are not unwanted but rather mistimed, the outcome can affect decisions about the timing of posterior births. For instance, it might be the case a woman who had an unplanned birth at 22 years of age would have chosen to have a planned birth at 26 in case of miscarriage. Therefore, a comparison at age 30 will be between a woman with an unplanned 8 year old and an equivalent woman with a planned 4 year old, rather than between a woman an unplanned 8 year old and a childless woman, or between a woman with an unplanned 8 year old and a planned 4 year old and a woman with only a planned 4 year old, which will allow a straightforward interpretation of the effect of the unplanned birth.

Table 2 presents evidence of the effect of a first, unplanned pregnancy on the total number of children and their timing. In the sample of first pregnancies that were unplanned, having a birth significantly increases the total number of children. However, the increase in parity is smaller than one for women whose pregnancy happened at least 6 years ago, except on the very beginning of the sample period. A childless woman who had an unplanned birth is more likely to have a higher parity but the increase is less than one, indicating that there is some substitution of future children. This result also holds if the sample is restricted to women whose total parity is higher than 2 children (not shown), and therefore it is not driven by females who did not wish to have a child at any point. Additionally, women who have already given birth wait longer until their second pregnancy.

There is, therefore, evidence that having an unplanned birth affect female choices on subsequent fertility. Using all unplanned pregnancies in the estimation will confound the effect of the unplanned birth on labor force participation and its effects on subsequent fertility, which will also affect labor supply. In order to obtain a straightforward interpretation of the estimates, the main estimation will be performed on the sample of last, unplanned pregnancies. Since whether a pregnancy is the last one a woman has is also affected by its outcome, the sample is restricted to 10 years prior to the survey.

Table 2 goes here

An unplanned birth might affect the marriage perspectives of the female, either married or unmarried when the unplanned birth happened. However, after controlling for subsequent births and their year of occurrence, there is no evidence of a first, unplanned birth affecting marital status at the time of the interview. When the last pregnancy is considered, the same result holds.

Effect of an unplanned birth in labor supply Table 3 presents the effect of an unplanned birth on the probability of being in the labor force at the time of the interview. Columns (1) and (2) include OLS without abortions and IV estimates for the sample of last, unplanned pregnancies that happened up to 10 years before the interview took place. Since restricting the sample to last pregnancies more than proportionally drops miscarriages (women who had a birth postpone their next pregnancy), columns (3) and (4) present the equivalent results when the sample is further restricted to pregnancies that occurred up to 5 years before the survey.

Having an unplanned birth reduces female labor force participation by as much as 25%-30% in comparison with a similar woman who had a miscarriage if it happened less than 6 years ago and about 15% if it happened 6 to 10 years ago. OLS and IV estimates are close to each other and never significantly different, although the OLS estimation has more predictive power. Restricting the sample to last pregnancies that happened up to 5 years before the survey reduces the power of the estimation, especially in the IV estimation. In these specifications, all pregnancies happened less than 6 years ago, and therefore only this parameter is estimated. In order to establish the size of the bias due to the use of last pregnancies, I compare the effect of birth on a mother whose child was 0 to 5 years old, estimated with the more restricted sample with the effect on mothers whose child was below 6 years old 5 years earlier (controlling for year of pregnancy), estimated with the baseline sample. The OLS estimates of the total effect of a childless woman who had a child in 1975, for instance, is -0.271 (0.087) while the child was below 6, when estimated with a sample using unplanned pregnancies that happened up to 10 years before the interview. When using the sample including only pregnancies up to 5 years before, a similar women whose child was born in 1980, after accounting for year of pregnancy, would suffer an effect of -0.175 (0.093). These estimates are not significantly different from each other. In the IV estimation, the estimates are less precise. The extended, up to 10 years before the interview sample, reports an estimate of -0.238 (0.124) while for the restricted sample is -0.074 (0.142). Therefore, the bias coming from restricting the sample to last, unplanned pregnancies is not significant. The rest of the analysis is therefore performed using pregnancies happened up to 10 years before the survey.

Even though some literature has focused on the impact of postponing the first birth on female career perspectives, I found no evidence of the number of children already present in the household modulating the effect of an unplanned birth. The sample is composed by last pregnancies only, but it does nevertheless include 1,497 pregnancies by childless women. However, there is evidence of the effect of an unwanted birth on labor force participation being reduced over the sample period, by around 0.8 percentage points per year.

The magnitude of the effect of an unplanned birth on female labor force participation is significantly higher than the ones traditionally found in the literature. Angrist and Evans (1998) report estimates rarely above 10%, using the sex of the first two children as an instrument for a third child. Therefore, most compliers are likely to be planned pregnancies. On the other hand, Bronars and Grogger (1994) estimate the effect of childbearing using a sample of unmarried mothers, who are less likely to plan their pregnancy, finding relatively small effects of a twin birth compared with the birth of a singleton. Nevertheless, the additional, exogenous child that comes with multiple birth happens at the same time as the previous child, and therefore its effects might not be representative of the impact of an unplanned birth of a singleton[10].

Table 3 goes here

[10] Angrist and Evans (1998) found significantly smaller effects of an additional child using twins as an instrument for a third birth, than using sex of thefirst two children for the same dataset.

In addition to changes in labor force participation, women can adjust their labor supply through changes in usual working hours or by switching from full-time to part-time employments. Table 4 includes the estimates for the intensive margin of labor supply for women who were in the labor force at the time of the interview. Full-time status is included in all waves of the survey used in the analysis, while usual number of hours were only included in the III to V waves, therefore the smaller number of observations. There is weak evidence of an unplanned birth reducing the probability of working full-time. Since this variable is only observed for working women, it is not possible to determine whether this change is due to full-time workers dropping from the labor force more frequently than part-time workers in the event of an unplanned birth or whether it is driven by actual transitions from full-time to part-time employment. Although having an unplanned child younger than 6 years of age makes the woman less likely to be a full-time worker than a women with an older unplanned child, the sum of both coefficients fails to be significantly different from zero. As in the case of labor force participation, there is evidence of a reduction of the impact of an unplanned birth in labor force participation as the sample period progresses. The results for hours of work are imprecise but follow a similar pattern.

Table 4 goes here

Women with different levels of education face different perspectives in the labor force and therefore the impact of an unplanned birth might differ as well. Unfortunately, the sample size and the number of miscarriages do not allow for a full set of interactions by level of education. Hence, women are divided between “low education”, those with 12 or less years of education (high school graduates or less), and “high education”, those with more than 12 years of education. Low-educated women account for 45% of the sample. The effects of birth, the child being less than 6, year of pregnancy and previous children if birth are interacted with the level of education. While the effect of the rest of the covariates might differ as well by education, the available data does not allow for a separate estimation for high and low education women.

Table 5 includes the estimates for labor force participation, full-time and hours of work if working by education. The impact of an unplanned child on labor force status appears to be slightly higher for highly educated women when the child is young and for low-educated females when the child is older than 6, but the coefficients are never significantly different by level of education. There is no evidence of differences by level of education on the effect of birth on full-time status if working. However, the presence of previous children in the household significantly increases the negative impact of an unplanned birth on the number of hours worked, but does so only for low-educated women. Highly educated women present no evidence of willingness or ability to change the number of hours usually worked.

Table 5 goes here

The lack of differences in the response to an unplanned birth in terms of labor force participation by level of education appears to contradict previous findings in the literature, which claims that that highly-educated women are less likely to drop from the labor force after a birth than low-educated women. Nevertheless, this result relies on pregnancy being planned, and highly-educated females might be more likely to do so than females with low education.

Cost of unplanned childbearing An unplanned birth affects female labor force participation but can also have a negative effect on the total family income, as well as on female earning capabilities. Table 6 presents the estimates of the impact of an unplanned birth on different measures of income. When the effect is not allowed to vary by level of education, the coefficients of birth and its interactions are never significant and therefore are not shown. Contrary to the case of labor supply, the impact of an unplanned birth on income does vary by education. Although the coefficient is only marginally significant, there is a sizable effect of having an unplanned birth for low-educated women when the child is young with respect to when the child is older than 6, although the results are not estimated with enough precision to assert a decrease in income with respect to females having a miscarriage.

Female earnings during the last 12 months if in the labor force, reported for waves III to V of the NSFG, present a significantly different pattern by level of education. While there is no evidence of a birth damaging the earning capabilities of high school dropouts and high school graduates, women with at least some college education see their earnings significantly reduced when an unplanned birth happens and they choose to remain in the labor force. However, this negative effect attenuates over the sample period.

If the female was married or cohabiting at the time of the interview, there is no evidence of an unplanned birth affecting her husband's earnings.

Sample selection and impact of unplanned births on female earnings While an unplanned birth significantly reduces labor force participation, it shows little impact on female earnings, affecting only the income of highly educated women while the child is young. Wages are only observed for women who are working, while there is no information on potential earnings for those who choose not to participate in the labor force. Given that birth causes withdrawals from the labor force, the impact on observed wages would only reflect the impact on the true earning capabilities if females who drop are selected at random from the wage distribution. If this is the case, an unplanned pregnancy does not have a negative impact on low-educated women, while significantly damaging the earning capabilities of those who are highly educated. However, if only women with higher earning capabilities are able to stop working because they are married to well-compensated husbands, their withdrawal from the labor force implies a decrease in average potential earnings of mothers in the labor force. For women who had a miscarriage and whose labor force participation remains the same, and there will be no change in potential earnings. In this case, selection would imply a decrease in average female income, even if having a child does not affect earning capabilities. Only if females drop randomly form the wage distribution will the distribution of potential earnings of mothers and non-mothers in labor force be unchanged, and therefore a regression in observed wages will provide unbiased estimates of the causal effect of birth on earnings[11].

In order to address this concern, I use imputation techniques to assign wages for women who were not in the labor force at the time of the interview. I impute wages with respect to the median of the distribution, following Johnson et al. (2000) and Olivetti and Petrongolo (2008). However, given the limited labor market information available in the data, estimating the probability of an

[11] More formally, if $F(w|birth) = F(w|birth, LF = 1)Pr(LF = 1|birth) + F(w|birth, LF = 0)Pr(LF = 0|birth)$, where $F(.)$ is the distribution of wages for females who gave birth $F(w|birth = 1)$ or who did not $F(w|birth = 0)$ and LF is an indicator variable that takes value one if the woman is working and 0 otherwise. Given that birth causes withdrawals from the labor force, it is expectable that $Pr(LF = 1|birth = 1) \neq Pr(LF = 1|birth = 0)$. If this difference depends on unobserved female characteristics correlated with earning, changes in probabilities will be confounded with changes in the wage distribution.

observation being above or below the mean is based on limited information about the husband and therefore only possible for married females. In order to be able to present a more complete analysis, I first assume that women are more likely to drop from the labor force if their earning ability is low and impute a wage below the median for all missing values. Then, I assume that all females drop from the higher end of the distribution and impute them a wage above the sample median. Finally, I predict the probability of being below the median wage based on her husband's earnings and education, given that it usually exists positive matching on characteristics between spouses. Using this predicted probability, I impute a third wage as follows $w^I = P_L * w^L + (1 - P_L) * w^H$, where P_L is the probability of having a wage below median, w^L is the 5th percentile of the wage distribution for the corresponding sample, and w^H is the 95th percentile. I estimate a median regression with bootstrapped errors after dropping abortions on the original sample on the three resulting samples from the different imputation techniques.

Table 7 includes the results for the median regressions without abortion for women reporting earnings. Median regression, as in the case of OLS, would be biased towards finding a more negative effect of birth if the outcome in case of absence of birth is more beneficial for females seeking abortions. The effects of birth for low-educated females are significantly more negative when a quantile regression is estimated. Although the coefficients for birth are not significantly different, the effect of a young child is. However, the positive trend over time appears to compensate for this impact. When imputing below the median (column (2) of table 7) or above the median (column (3)), the estimates behave as expected, becoming more negative in the first case and less in the second. However, the changes in birth trends suggest that it might be a change in sample composition over the period. Nonetheless, when the value of earnings for non-working women is estimated using the husbands' characteristics, the results are close to the ones found using the reported income. This suggests that sample selection is limited, although the estimates found are imprecise.

Table 7 goes here

Planned pregnancies

Finally, the effect of birth is estimated on a sample of planned pregnancies. The bias caused by the selection of last pregnancies is even more problematic in the case of planned pregnancies, since women seeking a birth are highly likely to continue trying to achieve it in the event of a miscarriage. Therefore, the results presented here use a sample including all planned pregnancies and control for the occurrence of other births and their year. Table 8 includes the results for all planned pregnancies up to 10 and up to 5 years before the survey. In this case, absence of abortions guarantees that the OLS estimation provides an unbiased estimate of the causal effect of birth. There is no evidence of planned pregnancies following a similar pattern to unplanned pregnancies, although the results are estimated with similar precision.

Table 8 goes here

5 Conclusions

Modern contraception allows women to control and time their pregnancies. However, introduction of "the pill" did not lead to a sharp decrease in fertility, and childbearing has been shown to have only limited effect on female labor force participation. Therefore, changes in fertility fail to account for the large increase in female labor force participation occurred during the last century. On the other hand, highly effective contraception gave women the opportunity to decide the most convenient time of their pregnancies. The ability to choose the right time for each pregnancy appears to play a key role on the limited magnitude of the effect of childbearing on labor force participation. In this paper, I use contraceptive failures and spontaneous fetal losses to evaluate the relevance of planning of a birth on its effect on labor force participation. Considering only unplanned pregnancies, a birth reduces the probability of participating in the labor force by as much as 25%. For a similar period, the estimates previously reported are rarely above 10% . The small impact found in the literature, therefore, appears to be driven by females' ability to plan pregnancies.

Although the negative impact of an unplanned birth on labor force participation reduces over the sample period, its magnitude justifies the current debate on publicly-funded family planning. More than 30% of women of reproductive age experience unplanned births which damage their labor market perspectives and that have been shown to decrease by measures such as expansions on Medicaid coverage of family planning.

Also in contrast with previous results, I found no differences in the impact of an unplanned birth, by level of education. If highly educated women are more able to time fertility through proper contraception use, this might be driving the disparities, since pregnancies for low-educated females are more likely to be unplanned and therefore have a more negative impact. Unfortunately, my data does not allow checking whether highly-educated females choose more effective contraceptive methods or whether the failure rate is different by level of education.

Total family income is not affected by the occurrence of an unplanned birth, especially in the case of highly educated females, while the effect for low educated females appears to be substantial. However, the data used in the analysis has low predictive power in this case, and the estimates are imprecise. Nevertheless, an unplanned birth damages the earning capabilities of highly-educated women more than it does low-educated women.

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Table 1: Descriptive statistics

	Full sample	Unplanned pregnancies			All planned
		All	Last pregnancies	First pregnancies	pregnancies
White	0.631	0.604	0.631	0.655	0.731
	(0.483)	(0.489)	(0.483)	(0.476)	(0.443)
African-American	0.145	0.175	0.158	0.157	0.067
	(0.352)	(0.380)	(0.365)	(0.364)	(0.250)
Hispanic	0.166	0.155	0.143	0.133	0.146
	(0.372)	(0.362)	(0.350)	(0.339)	(0.353)
Age at interview	32.510	31.412	32.387	28.399	34.244
	(5.624)	(5.655)	(5.831)	(4.601)	(5.001)
Protest	0.517	0.521	0.525	0.547	0.501
	(0.500)	(0.500)	(0.499)	(0.498)	(0.500)
Catholic	0.351	0.347	0.330	0.325	0.368
	(0.477)	(0.476)	(0.470)	(0.469)	(0.482)
Married	0.694	0.624	0.602	0.586	0.843
	(0.461)	(0.484)	(0.490)	(0.493)	(0.364)
Divorced	0.147	0.164	0.170	0.124	0.102
	(0.355)	(0.370)	(0.376)	(0.330)	(0.302)
Education	13.062	12.988	13.222	13.363	13.788
	(2.715)	(2.520)	(2.533)	(2.340)	(2.764)
Age at first birth	23.198	22.273	22.389	23.304	24.753
	(5.063)	(4.653)	(4.452)	(4.248)	(4.963)
Parity	2.417	2.368	2.072	1.419	2.433
	(1.333)	(1.405)	(1.359)	(1.002)	(1.115)
Pregnancy outcome					
Birth	0.744	0.659	0.734	0.645	0.865
	(0.437)	(0.474)	(0.442)	(0.479)	(0.342)
Abort	0.094	0.161	0.153	0.194	0.006
	(0.292)	(0.367)	(0.360)	(0.396)	(0.076)
Miscarriage	0.163	0.180	0.113	0.161	0.130
	(0.369)	(0.384)	(0.316)	(0.368)	(0.336)
Age at pregnancy	26.586	25.869	27.363	22.664	27.706
	(5.331)	(5.437)	(5.467)	(4.102)	(4.735)
Year of pregnancy	1992.21	1992.88	1993.30	1992.47	1991.94
	(9.248)	(8.827)	(8.849)	(8.781)	(9.287)
Ever married before pregnancy	0.740	0.644	0.688	0.457	0.911
	(0.439)	(0.479)	(0.463)	(0.498)	(0.284)
Unplanned pregnancy	0.278	1	1	1	0
	(0.448)	(0)	(0)	(0)	(0)
Planned pregnancy	0.334	0	0	0	1
	(0.472)	(0)	(0)	(0)	(0)
1982 NSFG	0.130	0.100	0.112	0.102	0.098
	(0.337)	(0.300)	(0.316)	(0.303)	(0.298)
1988 NSFG	0.156	0.166	0.153	0.180	0.187
	(0.363)	(0.372)	(0.360)	(0.385)	(0.390)
1995 NSFG	0.155	0.168	0.171	0.156	0.155
	(0.362)	(0.374)	(0.377)	(0.363)	(0.362)
2002 NSFG	0.151	0.169	0.165	0.182	0.131
	(0.358)	(0.375)	(0.372)	(0.386)	(0.338)
2006-2008 NSFG					
2006	0.146	0.152	0.165	0.142	0.138
	(0.353)	(0.359)	(0.371)	(0.349)	(0.345)
2007	0.146	0.154	0.141	0.142	0.146
	(0.353)	(0.361)	(0.348)	(0.349)	(0.353)
2008	0.116	0.091	0.092	0.097	0.144
	(0.320)	(0.288)	(0.289)	(0.295)	(0.351)
N	32137	9447	4142	2405	8757

Table 2: Effect of an unplanned birth at first pregnancy on total and subsequent fertility (up to 10 years before the interview)

	Total parity		Years to second pregnancy	
	OLS	IV	OLS	IV
Birth	0.849***	0.617***	0.404	0.880**
	(0.171)	(0.234)	(0.287)	(0.377)
Birth***1**(child<6)	0.380**	0.476**	-0.125	-0.187
	(0.167)	(0.222)	(0.210)	(0.260)
Birth*(year of pregnancy-1970)	-0.016*	-0.014	0.012	-0.003
	(0.009)	(0.013)	(0.013)	(0.016)
F (shown)	55.85***	22.90***	13.33***	12.79***
F (all)	20.880	22.156	15.333	16.258
N	1943	2405	942	1119
Drop abortions	YES	NO	YES	NO
NSFG waves	III to VII	III to VII	III to VII	III to VII

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status at pregnancy and at interview, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A dummy for up to 4 births and the year in which they happened are included as controls.

Table 3: Effect of an unplanned birth: labor force participation (last pregnancy, up to 10 and 5 years before the interview).

	Up to 10 years		Up to 5 years	
	OLS	IV	OLS	IV
Birth	-0.169*	-0.142		
	(0.099)	(0.120)		
Birth***1**(child<6)	-0.141**	-0.137*	-0.240*	-0.128
	(0.059)	(0.072)	(0.134)	(0.181)
Birth*(year of pregnancy-1970)	0.008**	0.008**	0.006	0.005
	(0.004)	(0.004)	(0.005)	(0.006)
Birth*previous children	0.015	0.002	-0.020	-0.055
	(0.035)	(0.049)	(0.039)	(0.059)
F (shown)	3.52***	1.95*	2.87**	1.64
F (all)	12.627	14.315	11.073	10.460
N	3483	4142	2223	2663
Drop abortions	YES	NO	YES	NO
NSFG waves	III to VII	III to VII	III to VII	III to VII

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status at pregnancy and at interview, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls.

Table 4: Effect of an unplanned birth: full time work and usual hours *if working* (last pregnancy, up to 10 years before the interview).

	Full time		Usual hours	
	OLS	IV	OLS	IV
Birth	-0.017	0.024	0.706	3.900
	(0.110)	(0.163)	(2.898)	(4.311)
Birth***1**(child<6)	-0.207**	-0.270***	-3.976*	-4.251
	(0.080)	(0.102)	(2.252)	(3.147)
Birth*(year of pregnancy-1970)	0.009**	0.011*	0.183	0.091
	(0.004)	(0.006)	(0.182)	(0.256)
Birth*previous children	-0.015	-0.037	-1.206	-1.652
	(0.043)	(0.062)	(1.322)	(1.869)
F (shown)	2.23*	2.30*	0.84	0.75
F (all)	3.577	3.973	3.629	4.826
N	2019	2477	1314	1641
Drop abortions	YES	NO	YES	NO
NSFG waves	III to VII	III to VII	III to V	III to V

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls.

Table 5: Effect of an unplanned birth by level of education: labor supply (last pregnancy up to 10 years before the interview)

	Labor force status		Full time *if working*		Usual hours *if working*	
	OLS	IV	OLS	IV	OLS	IV
Low education						
Birth	-0.187*	-0.161	-0.038	0.034	1.505	3.542
	(0.102)	(0.120)	(0.120)	(0.165)	(3.112)	(4.947)
Birth***1**(child<6)	-0.116	-0.122	-0.282**	-0.189	-3.478	-1.386
	(0.098)	(0.125)	(0.110)	(0.172)	(3.750)	(5.938)
Birth*(year of pregnancy-1970)	0.009*	0.010*	0.013**	0.010	0.071	-0.096
	(0.005)	(0.005)	(0.005)	(0.008)	(0.258)	(0.428)
Birth*previous children	0.021	0.005	-0.057	-0.094*	-2.355**	-2.693*
	(0.042)	(0.054)	(0.044)	(0.056)	(1.022)	(1.465)
High education						
Birth	-0.131	-0.142	-0.025	-0.026	-2.039	4.575
	(0.110)	(0.144)	(0.122)	(0.193)	(3.591)	(6.010)
Birth***1**(child<6)	-0.170***	-0.165*	-0.167	-0.321**	-4.368	-6.847
	(0.064)	(0.097)	(0.103)	(0.144)	(2.773)	(4.867)
Birth*(year of pregnancy-1970)	0.006*	0.007	0.006	0.012*	0.335	0.262
	(0.003)	(0.005)	(0.005)	(0.007)	(0.230)	(0.334)
Birth*previous children	-0.009	-0.003	0.041	0.046	0.835	-0.360
	(0.053)	(0.064)	(0.064)	(0.094)	(1.994)	(2.855)
F (shown)	2.25**	1.25	1.67	1.62	1.87*	1.54
F (all)	11.249	12.393	3.244	3.382	3.446	4.200
N	3483	4142	2019	2477	1314	1641
Drop abortions	YES	NO	YES	NO	YES	NO
NSFG waves	III to VII	III to VII	III to V	III to V	III to V	III to V

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls.

Table 6: Effect of an unplanned birth by level of education: income (last pregnancy up to 10 years before the interview)

	Total family income		Female earnings *if working*		Husband's earnings *if married*	
	OLS	IV	OLS	IV	OLS	IV
Low education						
Birth	0.184	0.185	-0.253	0.054	-0.072	0.133
	(0.157)	(0.211)	(0.272)	(0.391)	(0.183)	(0.242)
Birth***1**(child<6)	-0.590*	-0.608*	0.057	0.161	0.209	0.231
	(0.308)	(0.369)	(0.344)	(0.510)	(0.225)	(0.300)
Birth*(year of pregnancy-1970)	0.015	0.017	0.004	0.003	-0.008	-0.010
	(0.012)	(0.014)	(0.023)	(0.036)	(0.015)	(0.020)
Birth*previous children	-0.006	-0.014	-0.021	-0.166	-0.000	-0.066
	(0.068)	(0.086)	(0.086)	(0.120)	(0.082)	(0.102)
High education						
Birth	0.003	0.113	-0.278	0.344	-0.058	0.213
	(0.164)	(0.241)	(0.295)	(0.448)	(0.181)	(0.255)
Birth***1**(child<6)	-0.010	-0.053	-0.403**	-0.613*	0.219	0.309
	(0.125)	(0.169)	(0.189)	(0.356)	(0.151)	(0.232)
Birth*(year of pregnancy-1970)	0.005	0.003	0.037**	0.042*	0.008	0.006
	(0.006)	(0.008)	(0.016)	(0.024)	(0.011)	(0.014)
Birth*previous children	-0.022	-0.020	0.046	-0.219	-0.034	-0.164*
	(0.078)	(0.111)	(0.133)	(0.205)	(0.063)	(0.096)
F (shown)	0.85	0.66	1.48	1.42	1.02	1.06
F (all)	42.990	45.270	10.051	11.641	23.008	22.191
N	3212	3795	1284	1601	1353	1533
Drop abortions	YES	NO	YES	NO	YES	NO
NSFG waves	III to VII	III to VII	III to V	III to V	III to V	III to V

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status at pregnancy and at interview, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls.

Table 7: Effect of an unplanned birth on female earnings, controlling for selection into labor force (median regression without abortions)

	Reported income	Imputation below median	Imputation above median	Probabilistic imputation
Low education				
Birth	-0.400	-0.809**	-0.166	-0.301
	(0.290)	(0.406)	(0.388)	(0.225)
Birth***1**(child<6)	-0.362*	-0.432	0.480**	-0.040
	(0.203)	(0.321)	(0.209)	(0.154)
Birth*(year of pregnancy-1970)	0.036**	0.048**	-0.014	0.014
	(0.014)	(0.021)	(0.024)	(0.012)
Birth*previous children	0.019	0.017	0.112	0.084
	(0.103)	(0.114)	(0.121)	(0.071)
High education				
Birth	0.159	0.080	-0.013	-0.022
	(0.285)	(0.501)	(0.345)	(0.218)
Birth***1**(child<6)	-0.175	-0.443	0.188	-0.172
	(0.199)	(0.333)	(0.197)	(0.146)
Birth*(year of pregnancy-1970)	0.005	0.025	-0.004	0.013
	(0.015)	(0.026)	(0.020)	(0.012)
Birth*previous children	-0.005	-0.211	0.097	0.066
	(0.109)	(0.154)	(0.126)	(0.073)
N	1284	2235	2235	1796
Drop abortions	YES	YES	YES	YES
NSFG waves	III to V	III to V	III to V	III to V

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status at pregnancy and at interview, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls. Sample size in column (1) reflects last, unplanned pregnancies who did not end in abortion if earnings were reported. Columns (2) and (3) imputed earnings for all females whose last pregnancy was unplanned and did not end in abortion if not working, while column (4) does so only for those who reported a husband or domestic partner at interview.

Table 8: Effect of a *planned* pregnancy on labor force status

	Pregnancies happened up to	
	10 years before survey	5 years before survey
Birth	0.012	
	(0.049)	
Birth*$\mathbf{1}$(child<6)	0.042	0.081
	(0.047)	(0.120)
Birth*(year of pregnancy-1970)	-0.001	-0.000
	(0.003)	(0.006)
Birth*previous children	0.008	-0.086
	(0.036)	(0.066)
F (shown)	0.27	1.15
F (all)	13.341	9.814
N	8757	3083
Drop abortions	YES	YES
NSFG waves	III to VII	III to VII

Robust standard errors are reported in parentheses. *** denotes significance at 1%, ** at 5% and * at 10%. All specifications control for year of pregnancy, age at pregnancy, race, religion, education, marital status at pregnancy, previous abortions, number of children already in the household, age of youngest child and year of interview. Both year of pregnancy and year of birth are coded as 0 in 1970 and age at pregnancy is coded as 0 at age 18, the youngest in the sample. A series of dummies for years since pregnancy are included as controls.