The Labor Market Returns to Delaying Pregnancy*

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Abstract

We study the labor market impact of unplanned pregnancy among women using long-acting reversible contraceptives to delay pregnancy. While most women successfully delay, some have unplanned pregnancies, providing quasi-random variation in pregnancy timing. Analyzing linked health and labor market data from Sweden, we find that unplanned pregnancies halt women's career progression, resulting in income losses of 19% five years later. We find similar effects of unplanned births among women using short-acting reversible contraceptives. Using pregnancy as an instrument for birth in a dynamic treatment effect framework, effects of unplanned children are more detrimental for younger women and those enrolled in education.

Keywords: labor market costs of motherhood; fertility; contraceptives; unplanned pregnancy

JEL Codes: J13; J22; J24; J31

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1 Introduction

The effects of children on women's careers are central to understanding gender inequality in the labor market. Having a child is associated with large drops in earnings for women across countries with varying policy environments (Kleven et al., 2019b). In Sweden—the setting of this paper—having a child is associated with an almost 20% drop in earnings. At the same time, women are increasingly delaying motherhood. The average age at first birth for Swedish women rose from 24 to 30 years between 1970 and 2023 (Nordic Statistics database, 2024; Olah and Bernhardt, 2008). This shift in timing suggests that observed earnings losses today may reflect only part of the total cost of children, as women may actively avoid births that would be especially costly to their careers. Unplanned pregnancies offer a unique lens into the career impacts of children typically hidden by planning, but important for understanding the constraints that shape women's childbearing decisions.

In this paper, we use quasi-random variation in the incidence of pregnancy in a setting where women intend to delay having children. We examine the labor market outcomes of Swedish women who become pregnant while using Long-Acting Reversible Contraceptives (LARCs), in particular, intrauterine devices (IUDs) and birth control implants. These increasingly popular contraceptive methods work passively and are effective, but not perfect. About 0.5% of women using a LARC will get pregnant in a year, resulting in a natural experiment in which women who had planned to delay childbirth become pregnant earlier than they desired. We refer to pregnancies shortly following a LARC prescription as unplanned pregnancies.[1] We compare career paths of women who experience an unplanned pregnancy to those who do not, but who receive a LARC in the same year and at the same age.

We find that unplanned pregnancies have substantial, negative, and lasting consequences on the careers of women who have not yet had children. In all the years that we observe after the unplanned pregnancy, earnings are 12-24% lower than if the unplanned pregnancy had not occurred. In the long-term, the earnings effect is driven by occupational sorting rather than by changes in hours or employment: the long-term probability of working in an occupation with higher skill requirements is almost 20 percentage points lower than if the unplanned pregnancy had not occurred. Turning to other welfare measures, we find that unplanned pregnancy increases fertility (beyond the focal birth), but has no significant impact on anti-anxiety or anti-depression prescriptions.

Our reduced form estimates of the impact of unplanned *pregnancy* on labor market outcomes are challenging to compare to existing estimates of the impact of *childbirth* (e.g., from event studies). To isolate the

[1] Our baseline specification defines an unplanned pregnancy as a conception within nine months of receiving a LARC prescription. As we discuss in Section 2.3, most pregnancies within the nine-month time window occur due to device failure or device removal due to discomfort. We provide robustness for shorter windows, where we find pregnancy rates that are similar to what is found in the medical literature.

causal effect of children, we develop a dynamic instrumental variables (IV) strategy that accounts for the fact that women in the control group have children later and some women with unplanned pregnancies have abortions.[2] Our dynamic IV estimates reveal large and persistent career penalties for women experiencing unplanned births. On average, the arrival of an unplanned first child decreases earnings by 25% in years 1–6 after childbirth. Year-by-year earnings remain 20-30% below counterfactual levels, with early declines driven by non-employment. By year four, employment rates converge, but occupational sorting continues to diverge. By year five, mothers with unplanned births are 25 percentage points less likely to work in occupations requiring higher skills.

The heterogeneity in impacts is similar whether we examine the effect of pregnancy or of childbirth: in both cases, we find the largest effects on earnings for young women and women enrolled in education. For women who experience unplanned births between ages 22 and 27, average earnings decline by 32% in the six years after birth, which is more than twice the loss for women who experience unplanned births at age 28 or older.[3] This steep age gradient suggests that the timing of childbirth, rather than fertility per se, plays a central role in shaping career outcomes. A back of the envelope calculation implies that each additional year of delay in childbirth reduces the earnings penalty by roughly 2.5 percentage points. Among women enrolled in an education program before the pregnancy occurs, earnings losses are nearly twice as large as those of women who are not enrolled. These findings suggest that unplanned births disrupt human capital investments and this drives longer-term economic impacts of children. By avoiding or postponing births during these sensitive periods, women may significantly reduce the long-term career costs of motherhood. Our finding that children have the largest impact early in a woman's career is consistent with a large body of literature suggesting that early career decisions and opportunities have long-term effects. In early theoretical work, Neal (1999) provides a model in which individuals discover their talents early in life and then specialize later. In these models, having a child early in the career is particularly costly as it makes it more difficult to search for one's comparative advantage or accumulate skills.[4]

To assess whether our findings generalize beyond the subset of women using LARCs, as these women may be especially motivated to delay pregnancy, we also examine unplanned births among women using Short-Acting Reversible Contraceptives (SARCs)—primarily oral contraceptive pills. We classify a birth as unplanned if the woman filled a new prescription for oral contraceptives shortly after conception, or if she sought an abortion consultation but decided not to terminate the pregnancy. These alternative classifications yield the same conclusion: unplanned births lead to substantial and persistent career impacts with point

[2] We discuss the identifying assumptions in detail in Section 3.2 and provide supporting evidence in Section 4.4.

[3] These age-based differences persist when younger women are re-weighted to match older women's characteristics at similar ages.

[4] Kuka and Shenhav (2024) emphasize the importance of hastening return to work to mitigate these impacts.

estimates similar to the LARC setting. In contrast, the impact is less than half the size of our baseline estimates for *planned* births—estimated among women undergoing In Vitro Fertilization (IVF) treatments—and unplanned births among women who already have children. These planned and higher order births show no discernible impact on occupational sorting.

Our contribution to the literature is both empirical and methodological. Empirically, we provide causal estimates of heterogeneous impacts of unplanned pregnancies and births—particularly how these impacts vary by fertility intentions and the timing of entering motherhood. Methodologically, we propose a new empirical strategy that relies on unplanned pregnancies among LARC users who intend to delay pregnancy. The main strength of this strategy is that most women using LARCs delay pregnancy as intended, while the occurrence of unplanned pregnancy is as-good-as random. We show which assumptions are necessary for identification when using pregnancy as an instrument for birth, and develop an estimator to account for dynamic non-compliance that flexibly accommodates different assumptions on heterogeneous treatment effects.

Our paper extends the large literature utilizing "natural experiments" in fertility to shift the timing of childbirth. Notable papers in this literature include Hotz et al. (2005), Miller (2011), and Bíró et al. (2019), who use miscarriage as a shifter of birth timing. Other notable papers include Rosenzweig and Wolpin (1980), Angrist and Evans (1998), Grogger and Bronars (2001), and Black et al. (2005) who use family composition as an instrument for family size, and Cristia (2008) who focuses on women who seek fertility treatment and Lundborg et al. (2017) and Bensnes et al. (2025) who use initial IVF success to shift the timing of first birth.[5] Combining approaches, Miller (2011) uses survey data to instrument age at first birth with three shifters of birth timing: a miscarriage indicator, an indicator for pregnancy while using contraception, and years between first conception attempt and first birth. To our knowledge, this is the first paper to focus on unplanned pregnancies among LARC users to shift the timing of first birth.[6] Our empirical strategy focuses on the subset of women who intended to *delay* pregnancy, while most past work focuses on those who *planned* to have children but faced obstacles.

Our paper complements the literature studying the impact of unplanned pregnancy through the lens of abortion access. Recent work in this area includes Zohar et al. (2025), Londoño Vélez and Saravia (2025), Miller et al. (2020), and Miller et al. (2023).[7] The average woman exposed to laws that change abortion

[5] Past work on the impact of second children has focused on the effect of twin births compared to singleton births (Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Jacobsen et al., 1999; Grogger and Bronars, 2001; Bronars and Grogger, 1994; Cáceres-Delpiano, 2012). These instruments have become controversial (Bhalotra and Clarke, 2019) and twins entail different parental investments than singletons (Dougan et al., 2025). Our setting offers an alternative strategy of comparing the impact of an additional pregnancy for women who would like to delay having another child.

[6] Compared to Miller (2011), who also uses failed contraception as an instrument, our paper narrows to a form of contraception that works without user action, minimizing selection concerns. We also compare our estimates from the LARC setting to other settings for unplanned and planned births.

[7] A number of additional papers study the impact of abortion access on children's outcomes, finding that children born when

access differs substantially from the LARC users in our sample. Abortions are most common among women in their teens and early 20s. In contrast, LARC users are on average 31 years old, which is also the average age of women without children in our Swedish sample. It is worth noting that in abortion access studies, compliers are women who would like to have an abortion when they get pregnant. While in our setting, compliers are women who decide to have a child when they have an unplanned pregnancy.

We also advance the methodological literature on identification and estimation of dynamic treatment effects in settings with dynamic non-compliance.[8] This setting is relevant not only when instrumenting births among women using contraceptives (e.g., Miller, 2011; and ours), but also for instrumenting births among women getting IVF treatments (e.g., Lundborg et al., 2017; Bensnes et al., 2025; and ours), and using initial assignment in a randomized control trial (RCT) as an instrument for receiving treatment in a setting with dynamic non-compliance, for example, medical treatments (e.g., Angrist et al., 2025) or labor market training programs (e.g., Bloom et al., 1997; van den Berg and Vikström, 2022). We first extend the standard IV assumptions—in particular independence and the exclusion restriction—to a dynamic setting where compliers in the control group may receive treatment in later periods. We then show that identifying dynamic treatment effects beyond the first period requires two additional assumptions about these later-treated individuals: an assumption about their treatment effects, and an assumption ruling out anticipatory effects on pre-treatment outcomes (Proposition 3.1). Cellini et al. (2010) address an analogous dynamic non-compliance problem in a sharp regression discontinuity design, assuming the same treatment effects for the initially-treated and the later-treated. In contemporaneous work, Angrist et al. (2025) make similar assumptions—that average treatment effects are the same for the initially- and later-treated, with no anticipatory effects—in a dynamic instrumental variables setting similar to ours.[9] Under these assumptions, we show that identification follows recursively even with heterogeneous treatment effects (Corollary 3.1.1). We develop an IV-GMM estimator that jointly estimates dynamic local average treatment effects (LATEs) while accommodating different assumptions about both later treatment effects and the anticipatory effects of the later-treated. This flexibility is important in our setting, where later births among LARC users are likely planned and may therefore have different career impacts than unplanned births.

Finally, our paper contributes to the extensive literature on the costs of motherhood which compare the earnings trajectories of mothers before and after they have their first child to fathers and/or women

abortion was available are less likely to live in poverty (Gruber et al., 1999), use controlled-substances (Charles and Stephens, 2006), are less likely to commit crimes (Donohue III and Levitt, 2001), and are less likely to be teen-mothers (Donohue III et al., 2009). Pop-Eleches (2006) studies the removal of abortion access in Romania under Ceausescu and finds that children born in these cohorts had worse outcomes than would be predicted by their mothers' education. Mølland (2016) studies abortion access among teenagers in Norway and finds that abortion access improves educational attainment.

[8] Heckman et al. (2016) and Han (2021) investigate dynamic treatment effects in the context of sequences of choices using exclusion restrictions at each margin and allowing for heterogeneous treatment effects of each choice.

[9] See Ferman and Tecchio (2025) for a partial identification approach that relaxes the assumption of the same average treatment effects for the initially- and later-treated.

who have different first-birth timing using event-studies (Angelov et al., 2016; Kleven et al., 2019a,b; Chung et al., 2017; Eichmeyer and Kent, 2024; Andresen and Nix, 2022). These empirical strategies reveal that motherhood is associated with large and persistent earnings declines and adverse life outcomes. Our novel use of prescription data to identify the *pregnancy intentions* of women yields several insights beyond what is possible with data connecting births to mothers' outcomes. It is precisely the fact that these births are unplanned that allows us to study how much circumstances surrounding birth matter, since observational data includes strategically timed births. In the 1970s, with access to better birth control, women delayed pregnancy, gaining time to find a better partner and to increase their educational attainment and labor market attachment (Goldin and Katz, 2002; Bailey, 2006; Ananat and Hungerman, 2012).[10] Consistent with earlier work showing that women alter their career paths to minimize childbearing impacts (Polachek 1981, Blackburn et al. 1993, Adda et al. 2017, Bronson 2019), we find planned births have smaller career effects than unplanned births.

2 Institutional Setting and Data

2.1 Institutional Setting

Sweden has a variety of "family-friendly" policies intended to help families balance children and careers. In the years we study, parents are allotted a total of 16 months of leave for each child of which two months are earmarked to each of the parents. During 13 of these months, parents that were employed at least 240 days before leave receive higher benefits based on past income. Abortion access is relatively unrestricted with free abortion until the 18th week of pregnancy, and later abortions are only allowed if the fetus is deemed unable to survive. Healthcare is universal and free-of-charge for children. Childcare is highly subsidized and enrollment is 70% for 1-2 year old children and 90% for 3-6 year old children. These institutions are described in more detail in Appendix D. It is important to keep this setting in mind when interpreting the empirical results—the effect sizes may well differ in places like the US, where monetary costs of healthcare and childcare are higher and public family support is lower.

2.2 Swedish Administrative Data

Our sample includes all women who are born in 1965-83 and reside in Sweden. We merge several administrative registers via a unique individual identifier. Labor market data are collected and administered by Statistics Sweden (SCB). The primary source of labor market data is the longitudinal integration database

[10] Using Swedish data, Heckman and Walker (1990) find that rising female wages over the life-cycle delay time to conception, but barely affect childlessness.

for health insurance and labour market studies (LISA) that contains yearly observations during the period 1990-2013 on earnings, public transfers, employment, and occupation. Our primary income measure is earnings (that most closely proxies labor market productivity) plus all transfers related to pregnancy-, parental-, and family-leave benefits.[11]

We also analyze employment and occupation outcomes. Our measure of employment is based on an employee indicator in the last week of November in a given year. We measure occupational skill requirements using the first digit of the Swedish Standard Classification of Occupations (SSYK96). Following the hierarchical structure and skill level criteria established by the International Labor Organization, we classify occupations as high-skill if they fall into one of three major groups: (1) "Legislators, senior officials, and managers," (2) "Professionals," or (3) "Technicians and associate professionals." These occupations require either managerial responsibilities, advanced theoretical competence (typically requiring a university degree), or specialized competence (typically at least a short college degree or substantive on-the-job training and/or previous experience required for competent job performance).[12]

These data also include the level and field of highest completed education, information on enrollment in education, age, civil status, family status, and some information on household composition; including the number of children in various age-groups and the identity of the partner for married couples and for unmarried couples cohabiting with common (biological or adopted) children.

We merge labor market data with health data collected and administered by the National Board of Health and Welfare ("*Socialstyrelsen*"). This includes the Medical Birth Registry (MFR), containing all births between 1973 and 2012; the Prescribed Drug Register (LMED), which includes all prescriptions from July 2005 through 2013; and the National Patient Register (NPR), which contains outpatient doctor visits including day surgery and psychiatric care from both private and public caregivers (2001-2013), but *not* primary care.

2.3 Defining Unplanned (and Planned) Pregnancies

In this section, we describe how we identify unplanned pregnancies among LARC users, our main empirical strategy. We also describe alternate definitions for unplanned and planned pregnancies among women using SARCs, women who attend an abortion meeting but have the child, and women receiving IVF treatments.

[11]See Appendix C.2 for more details on the income measures.

[12]This classification aligns with standard practice in the literature examining occupation-based skill requirements; see Appendix C.3 for details.

2.3.1 Unplanned Pregnancies while using LARCs

LARCs are attractive to women who do not want to get pregnant in the near future. LARCs last at least three years and have low failure rates (Trussell, 2004; Sundaram et al., 2017). Unlike most SARCs, LARCs work passively and do not require women to take any action until they wish to have them removed. The average IUD user is 35 years old, and only about ten percent of the IUDs we observe are purchased by women who have not yet had a child. Implants, on the other hand, are given to younger women (average 28 years old in our data) and about a quarter have not yet had a child. Appendix Figure A1b shows the fraction of women at each age who receive an IUD or an Implant, both among all women and among nulliparous women with no prior childbirth.[13]

We focus our analysis on women who purchase a LARC with a prescription. In Sweden, a woman with a hormonal birth control prescription must physically pick up the prescription at the pharmacy and then take the prescription to a doctor, women's clinic, or midwife to insert it.[14] We observe the date at which a woman paid for and received her LARC device from a pharmacy in the prescription data. Appendix C.1 describes the codes we use to identify LARC prescriptions. We do not rely on outpatient procedure records to identify when women get a LARC inserted and removed as we observe fewer than 10% of these procedures.[15]

Our baseline definition of an unplanned pregnancy is one that occurs within 9 months of purchasing an IUD or implant contraception. We consider conceptions that end in childbirth or in an abortion. Our definition of unplanned pregnancies excludes miscarriages or terminations due to a non-viable pregnancy, for example, an ectopic pregnancy. Women who experience such pregnancies are implicitly assigned to the control group.[16] In the sample of LARC users who have no children, we observe 352 unplanned pregnancies using a nine-month window.[17]

There are three main concerns with our definition of unplanned pregnancy. The first two concerns relate to women changing their mind after purchasing a LARC. First, women may never insert the LARC. Second, women may remove the device early. The third concern is that failures of the device are not as-good-as

[13]Note that this is not the utilization rate. The utilization rate is higher than this, because a LARC lasts for 3-5 (plus) years. If a woman took out a prescription at age 29, she is likely still using the LARC for birth control at ages 30 and 31. We do not present utilization rates because they require assumptions about how long a woman has the LARC inserted. However, if about 5% of women are getting a LARC in a given year and they last on average four years, then a back-of-the-envelope calculation would imply that approximately 20% of women are using a LARC in that year.

[14]IUD and implant prescriptions cost about US$100 for the women in our sample. There are discounts for younger women outside of the age range in our sample.

[15]Most insertions and removals take place in a primary care or midwife office, which are not included in the outpatient data.

[16]Of the nearly 25,000 women in the control group, we see fewer than 50 instances of miscarriages or ectopic pregnancies within nine months—our baseline window—of a LARC prescription, so it is unlikely that the experiences of this group bias our main estimates. We do not include miscarriages in our main specification because it is unlikely that all miscarriages are recorded—not only are we missing miscarriages reported during visits to primary care doctors, but also women may not visit doctors or be aware that they are miscarrying because both IUDs and implants can cause irregular bleeding. The medical literature suggests IUD pregnancies have a miscarriage rate of 20% (Stenchever, 2001), while our observed rate is 13%.

[17]340 of these women are also observed in the labor market data the year before the LARC prescription. A small fraction of women are not observed in the labor market data, because, for example, they are out of the country.

random. We discuss each of these in turn below. As most of the clinical literature does not condition on nulliparous women, the analyses that follow include all women who get a LARC.

First, we do not believe that failure to insert the LARC is the cause of the unplanned pregnancies that we observe. While we do not observe most insertions, we observe a subset. Pregnancy rates are similar when insertions are observed compared to the whole sample (0.0079 vs. 0.0082), so we do not find any evidence that women are not getting the LARCs inserted in our sample.

Second, we believe that some women are removing the device by nine months after insertion, however, we do not believe that this reflects a change in pregnancy intentions. The pregnancy rate we observe in the population of LARC users at nine months (annualized) is 0.0082.[18] This is higher than what one would expect based on device failure rates reported in clinical trials. However, it is similar to the rate reported in surveys.

Estimates of failure rates in clinical trials range between 0.001 to just under 0.01, as shown in Appendix Figure A2. These estimates may not reflect typical pregnancy rates. In clinical trials, practitioners are likely more careful and, likewise, women are surveyed and asked to check on the status of the device as often as every three months.[19] In order to calculate a comparable pregnancy rate to the clinical-trail setting, we consider the set of women for whom we observe insertions, and for whom removals and expulsions are unlikely by calculating the pregnancy rate within three months of the LARC prescription. In this sample, the pregnancy rate is 0.004, similar to the failure rates found in clinical trials of LARCs.

Our baseline definition (pregnancy within nine months of purchase), however, also includes pregnancies that occur after the device has been removed or expelled. Removal of the device may occur due to side-effects of the LARC, or because women wished to become pregnant. In the former case, we would be concerned that the incidence of side-effects was related to labor market trajectories. We discuss this in detail below. If women remove the LARC within nine months because they wish to become pregnant, this would mean that we misclassify planned pregnancies as unplanned pregnancies. Grunloh et al. (2013) is the only study that we are aware of that surveys less than one year after insertion of the LARC. In this survey, interview subjects report that 7% have removed the LARC at 6 months, but nearly all (98%) of the removals are due either to the side effects of the LARC (pelvic inflammation, hormonal issues, or menstrual issues) or to involuntary expulsion rather than changes in pregnancy intention. Only 0.16% of women have changed their intentions (removed the LARC because they want to have children) by six months. If we extrapolate from this, and assume that around 6 months 0.16% of the sample removes the LARC in order to become pregnant, the

[18] Rates are higher in our sample of nulliparous women, but is completely accounted for by the different age distribution of this population compared to the overall population of LARC users, as documented in Appendix Figure A1. If we reweight the nulliparous sample to have the same age distribution as the full LARC sample, we find a pregnancy rate of 0.0086.

[19] For example, Stoddard et al. (2011) studies typical-use implant failures in Australia and finds that more than a third of the pregnancies are due to doctors failing to insert the implant correctly, a failure mode not reported in the clinical trial literature.

expected number of conceptions by nine months would be 21 (given a 60% chance of conception after three months of trying).

Based on the medical literature and the delay between removing a LARC and achieving conception, we choose a nine-month window as our baseline specification where the pregnancies are unlikely to be planned. We do two robustness exercises to check the potential bias from misclassifications. First, we estimate the effects for shorter windows where planned pregnancies are less likely. We also estimate a model that explicitly accounts for planned pregnancies being misclassified as unplanned in the estimation. Our results are qualitatively unchanged in both exercises.[20]

The third concern is whether unplanned pregnancies among LARC users are as-good-as random. It is likely that pregnancies are more common among women who are more fertile, who have more frequent intercourse, or who have the LARC removed early due to side effects. To overcome the potential challenges this selection poses for identification, we match women based on age and fertility history, focusing only on women without children in our main analysis. We also match on civil status and education in some specifications (in addition to age and prescription year) and do not find that it affects our estimates of the labor market impacts of unplanned pregnancy. Ultimately, our research design rests on the fact that the outcomes of women in the control group captures the counterfactual for women in the treatment group. In Section 4, we present differences between the two groups over time and find that there are no differences in labor market outcomes between these women before the LARC prescription. The strongest evidence we provide for our research design is the fact is that these two groups are very similar in both levels and trends in the seven years leading up to a contraceptive prescription and only diverge after treatment assignment.

2.3.2 Unplanned and Planned Pregnancies while using SARCs

To assess whether the impacts of unplanned pregnancies generalize beyond LARC users, we identify unplanned births among women using SARCs—primarily oral contraceptive pills, used by more than 50% of women in Sweden (see Figure A1a). We use two definitions of unplanned births: (1) births among women who purchased a SARC prescription within two weeks of conception, suggesting they were unaware of the pregnancy; and (2) births among women with an active SARC prescription who had an abortion counseling meeting but ultimately gave birth. We also define planned SARC births to be those where the last SARC prescription was 84–365 days before conception, suggesting deliberate discontinuation with the intention of becoming pregnant. For all definitions, we compare labor market trajectories to women of the same age who get a SARC prescription in the same year.

Identifying pregnancy intentions among SARC users is more challenging than among LARC users because

[20] See Section 4.4.

discontinuing the pill may reflect changing fertility intentions, characteristics of the user (e.g. conscientiousness) that also affect compliance, or some combination of the two. This means that in practice, while we can construct specific scenarios where the resulting birth was likely unplanned (or planned) we cannot construct a control group matched on pregnancy intentions. Because births are likely an endogenously selected subset of pregnancies, causal interpretation of the motherhood effect among SARC users warrants caution. See Appendix C.1 for details of the definitions and additional discussion.

2.3.3 Planned Pregnancies from In Vitro Fertilization (IVF) treatment.

Another way to identify planned pregnancy is among women who undergo fertility treatment. This definition focuses on women who would like to have a child, but face obstacles conceiving. Following Lundborg et al. (2017), we define planned pregnancies as pregnancies resulting from successful first IVF treatment. We restrict our attention to first IVF procedures because after this initial treatment, persistence in seeking IVF may be endogenous to personal characteristics and may also be affected by labor market shocks. Appendix C.1 describes this procedure and definition in detail. Appendix Figure A1c also shows the distribution of women who receive at least one IVF treatment in a year.

The IVF definition above leaves us with a sample of almost 9,000 first IVF treatments. From this, we separate the treatments into successful treatments that lead to a childbirth within 46 weeks (identified from childbirths in the birth registry) and failed treatments that do not lead to a childbirth within this window.[21] If we observe more than one IVF prescription or insertion before a childbirth, then we classify the first treatment as a failure. We identify 2,274 planned IVF births resulting from the first IVF treatment.

2.4 Summary Statistics

Table 1 shows summary statistics of our data and Figure A1 shows the probability that a woman of a given age receives one of these prescriptions or an IVF treatment. Women in our sample are on average 31.2 years old, LARC users are slightly older on average while IVF users are older still on average. Most women are employed, and LARC users without children come closer to the overall population on this margin. Compared to the population overall, LARC users without children are also similar in civil status, but LARC users are somewhat less educated. Among LARC users without children, only 25% work in occupations requiring higher skills. In terms of mental health, SARC users without children are closest to the population overall, while LARC users are more likely to have prescriptions for anti-anxiety medication or anti-depression medication. In contrast, IVF users are less likely to have these prescriptions. This, however, may reflect that these women are avoiding medications when trying to become pregnant.

[21] We chose the 46 week threshold to be sure to include all pregnancies that resulted from the IVF procedure.

We next describe differences in outcomes between these groups, conditional on birth. Appendix Table A1 presents data on the characteristics of women who give birth to a first child in seven categories (columns). In the first column we consider the characteristics of all women having their first child in our sample period, then the characteristics of all women who used LARCs before their first birth, the characteristics of women who had an unplanned pregnancy while using a LARC, the characteristics of all women who underwent an IVF procedure before their first birth, the characteristics of all women who had their first child as a result of a successful initial IVF procedure, and finally, we reweight the last two groups according to the characteristics of women who experience an unplanned pregnancy while using a LARC using the propensity score procedure of DiNardo et al. (1996).[22] Children born to women undergoing IVF and women using LARCs are similarly unhealthy relative to the overall population of births, though mothers using LARCs are more likely to be smokers, to be snuffing, and taking anti-anxiety, anti-depression, and pain-relieving prescription drugs, while women giving birth through IVF are older and more likely to have a planned cesarean section (c-section) delivery. In measures of gestational age, size for gestational age, APGAR, and days in the hospital after birth, babies from LARC unplanned pregnancies are slightly less healthy than babies in the overall population.[23]

2.5 Balance

In this subsection, we test balance on characteristics for LARC users who had an unplanned pregnancy compared to those who did not. Table 2 shows the balance in pre-prescription characteristics, comparing women who get pregnant in the first nine months after taking out a LARC prescription and those who do not. We match women based on their age and year of the LARC prescription. The balance between the two groups is similar except in terms of civil status and on some dimensions of education. Divorced and married women are more likely to have an unplanned pregnancy than single women, possibly due to more sexual activity. Figures in Section 4.1 documents the fact that pre-LARC, differences in labor market outcomes are not present between women who experience an unplanned pregnancy and the control group, both with and without conditioning on pre-LARC education and civil status. In both specifications, there are only small insignificant differences in labor market outcomes between the two groups. We conclude that the likelihood of an unplanned pregnancy is unlikely to be related to labor market outcomes.

Turning to IVF success, a primary concern with treating IVF success as a random event is that women take actions to increase the probability of a success and that this is correlated with labor market paths. In this case, successful treatments are not random and the post-IVF treatment outcomes does not only reflect

[22] More specifically, we predict the probability that a woman in the IVF sample would be in the LARC sample based on her income, education level indicators, married, divorced, employment, indicator for occupation requiring higher skills, private sector indicator, and age in the year of the fertility procedure. We then weight by this probability.

[23] Although children born as a result of unplanned pregnancy while using a LARC are slightly higher birthweight than children born as a result of IVF success, even when the IVF population is re-weighted.

a childbirth, but also differences in labor market behavior. We do find that this is partly the case in the data. In the last three columns of Table 2, we see that women with a successful first IVF treatment also have higher pre-treatment earnings, and that they are slightly more likely to be employed and work in higher-skilled occupations. However, these differences shrink substantially when we also control for education and time since last contraceptive. We discuss these issues in detail in Section 4.4.[24]

Appendix Table A2 also shows balance for women who already have one child and are LARC users. We see that the women who experience unplanned pregnancy look similar to women who do not in this population on all dimensions except our measures of mental health. Women with one child who experience unplanned pregnancies are substantially more likely to have prescriptions for anti-anxiety and anti-depression medication. One reason for this is that some women experience postpartum depression after the first child, and this disproportionately affects their propensity to use the LARC or changes their intentions concerning future children rapidly. When we condition on not having any mental health prescriptions when the first child is an infant, we find that balance in other characteristics is unchanged and that we also have balance in mental health prescriptions (last three columns of Appendix Table A2). We present main effects for all LARC users with one child, and also for the subset of these women who have no mental health prescriptions when the first child is an infant.

3 Empirical Strategy

In this section, we present our empirical strategy. In Section 3.1, we describe a strategy that compares outcomes for women who experience an unplanned pregnancy to those who follow their intended fertility path. This is the comparison of interest for estimating the average return to successfully delaying pregnancy, for assessing the value of policies that affect access to more effective contraceptives, and for studying the timing of childbearing between the two groups. In order to isolate the effect of having an unplanned child, Section 3.2 develops a dynamic IV strategy for estimating the impact of childbirth. This is the comparison of interest for understanding the impact of children on women's careers and yields a parameter which can be compared to what is estimated in the literature, for example from event studies.

3.1 Dynamic Effects of Pregnancy

We first estimate the reduced-form impact of an unplanned pregnancy in the years following a LARC. The baseline specification matches women with an unplanned pregnancy to women who get a LARC at the same

[24] See Groes et al. (2024) for additional discussion of the concerns with the IVF instrument.

age and year. Our primary specification performs the matching using a saturated regression model:

$$\begin{aligned} Y_{is} = & \sum_{t=-7}^{7} \alpha_t^{LARC} \mathbf{1}\left[t = s - year_i\right] UnplannedPregnancy_i \\ & + \sum_{t=-7}^{7} \sum_{y} \sum_{j} \delta_{tyj}^{LARC} \mathbf{1}\left[t = s - year_i\right] \mathbf{1}\left[y = year_i\right] \mathbf{1}\left[j = age_i\right] + \varepsilon_{is} \end{aligned} \tag{1}$$

where Y_{is} is the outcome of interest (e.g., labor market earnings) in calendar year s for woman i. We focus on outcomes $t = -7, ..., 7$ years after woman i got a LARC; i.e., $t = s - year_i$, where $year_i$ is the calendar year and age_i is the age at which woman i received the focal LARC.[25] The $UnplannedPregnancy_i$ indicator is equal to one if woman i had an unplanned pregnancy (see definition in Section 2.3). The second term ensures that we are specifying a saturated model in each year t since LARC, comparing women within each possible value of age and year of getting the LARC. This is equivalent to exact matching with equal weight to all women with the same value of $(t, age_i, year_i)$.[26] This ensures that the counterfactual for women with an unplanned pregnancy are women who get a LARC at the same age and in the same year, but for whom the LARC works as intended. In some specifications we additionally interact the second term with observables measured the year before getting a LARC ($X_{i,year_i-1}$) to corroborate the robustness of our estimates. We estimate the impact of a "planned" pregnancy in a similar way.[27]

The main identifying assumption for the matching estimator is that conditional on age and year of getting a LARC, an unplanned pregnancy is as-good-as randomly assigned. We test the implication of this assumption that there are no pre-existing differences in outcomes between the two groups of women:

[25] Most nulliparous women (75%) in our LARC sample have one, and only one, LARC prescription during our sample period (2005-2012) and 20% have two LARC prescriptions. For the 25% of women with more than one LARC, we randomly select one as the focal LARC.

[26] Performing matching via a saturated regression or a weighted average of contrasts may give different answers as they weigh the contrasts differently. Neither approach gives any weight to the cells with no treated women. In our setting, the probability of treatment is close to independent of age and year of getting the LARC prescription. Consequently, the weights are almost identical. A regression of treatment on age and year of prescription has an F-statistic of 1.4 and $R^2 = 0.009$. The top panel of Appendix Figure A3 shows the number of observations in each of the 142 age × year cells, and the bottom panel singles out the 42 cells with no treated women: most of these cells are for women age 40 or older (43%) and in the two endpoint years: 21% in 2005 for which we only have half a year of data and 26% in 2012 when only women who fill LARC prescriptions in the first three months can be "treated" as we only observe births through the end of 2012.

[27] The specification for planned pregnancies is:

$$\begin{aligned} Y_{is} = & \sum_{t=-7}^{7} \alpha_t^{IVF} \mathbf{1}\left[t = s - year_i\right] PlannedPregnancy_i \\ & + \sum_{t=-7}^{7} \sum_{y} \sum_{j} \sum_{l} \delta_{t,y,j,l}^{IVF} \mathbf{1}\left[t = s - year_i\right] \mathbf{1}\left[y = year_i\right] \mathbf{1}\left[j = age_i\right] \mathbf{1}\left[l = X_{i,year_i-1}\right] + \varepsilon_{is} \end{aligned}$$

where the $PlannedPregnancy_i$ indicator is equal to one if a woman became pregnant with the first IVF treatment. The last term defines the exact matching cells as the interaction between t years since first IVF treatment and all values of $year_i$ and $age_{i,year_i}$ at first IVF treatment, and the last term denotes additionally matching on all values of $X_{i,year_i-1}$ that here includes years since last contraception at time of first IVF treatment ($YearsSinceContra_{i,year_i}$), and college degree obtained by the year before IVF ($CollDegree_{i,year_i-1}$). We match on additional observables as the pre-IVF estimates of $\alpha_{-7}^{IVF}, ..., \alpha_{-1}^{IVF}$ are significantly different from zero otherwise. For more details, see Section 2.3.3 describing the IVF setting.

$\alpha_{-7}^{LARC} = ... = \alpha_{-1}^{LARC} = 0$. The parameters $\alpha_{t\geq 0}^{LARC}$ give the dynamic impact of an unplanned pregnancy on outcomes. α_t^{LARC} is analogous to an intent-to-treat (ITT) estimate of the effect of an unplanned pregnancy, when treatment is an unplanned birth. We next turn to estimating the impact of an unplanned birth.

3.2 Dynamic Effects of Childbirth

In this section, we describe a general instrumental variable strategy to identify dynamic treatment effects when there is dynamic non-compliance. Dynamic non-compliance can appear in many situations, including RCTs of labor market training programs to medical treatments (e.g. vaccines, radiation therapies, and other one-time medical therapies and procedures), where compliers in the control group may endogeneously receive treatment in later periods. In our setting, we observe that some women who do not have an unplanned pregnancy (i.e. our "control" group) go on to have children in the years that follow. In the following, we describe the assumptions required for identification and how we estimate the impact of unplanned birth.

We start by adapting the standard potential outcomes notation for a setting with dynamic treatment effects and dynamic non-compliance (see Appendix Figure B1 for a diagram of the dynamic compliance and notation on treatment). We focus on binary treatments that are irreversible or where the first treatment is particularly salient. The analysis is done with respect to period t, which is time relative to treatment assignment or the year that the woman received the LARC in our case. Let $Y_{it}(\tau)$ be the potential outcome for individual i in period t if the individual has been treated for $\tau \in \{0, 1, 2, 3, ...\}$ years. $\tau = 0$ denotes not yet being treated, $\tau = 1$ denotes treatment in the current period, and $\tau = 2$ denotes treatment in the previous period.[28] Furthermore, let T_{it} be equal to one if individual i received treatment in period t. Finally, assume that there is an instrument Z_i that affects treatment assignment in the first period $t = 1$.

In our setting, we define treatment to be the birth of the first child and assume that unplanned pregnancy is a valid instrument conditional on age and year at the time of getting a LARC.[29] Hence, $Y_{it}(\tau = 0)$ is the potential outcome for a woman who has not yet had a child in period t and $Y_{it}(\tau = 1)$ is the potential outcome for a woman who has a newborn in period t ("treated"). In other words, for $\tau > 0$ the age of the firstborn child is $\tau - 1$. In the following discussion, the matching of women by age and year is kept implicit.

We adapt the standard instrumental variable assumptions to the dynamic setting. In what follows, we focus the terminology of compliance on the first period, where we have compliers, never-takers, and always-takers with respect to choices in the first period. We define $\mathcal{C}_i = 1$ if individual i is a complier in the first period (i.e., $\{i : T_{i1}(Z_i = 1) > T_{i1}(Z_i = 0)\}$). The main difference with respect to the static setting is that

[28] Note that the potential outcomes are the same regardless of the number of years before treatment.

[29] While women are observed to have additional children, we are particularly interested in the transition to motherhood. The identification arguments for the second child are similar, except that the sample consists of women who have had one child and we additionally match on the age of the first-born child at the time of getting the LARC.

the independence and exclusion restriction assumptions need to be extended to include both future outcome dynamics and treatment trajectories. We also need to account for possible anticipatory effects for compliers who receive treatment in later periods.

A1 : **Independence:** $Y_{it}(\tau), T_{it}(T_{i1}) \perp Z_i \ \forall t, \tau$

The independence assumption requires that the instrument is as-good-as-randomly assigned in the first period ($t = 1$) with respect to both outcomes and treatment in the first period *and* future values of outcomes and treatment. In our setting, we assume that unplanned pregnancies among LARC users are as-good-as-randomly assigned. See discussion in 3.1.

A2 : **Relevance:** $\text{Cov}(Z_i, T_{i1}) \neq 0$

The relevance assumption requires that the instrument affects treatment assignment in the first period. In our setting, relevance requires that unplanned pregnancies make births in the first period more likely.

A3 : **Exclusion Restriction:** $Y_{it}(\tau, Z_i) = Y_{it}(\tau) \ \forall t, \tau$ and $T_{i,t>1}(T_{i1}, Z_i) = T_{i,t>1}(T_{i1}) \ \forall t$

The exclusion restriction requires that conditional on treatment in the first period, the assignment of the instrument does not affect the future values of outcomes and treatment choices. In our setting, the exclusion restriction assumption states that the instrument does not affect contemporaneous and future labor market outcomes and is also not related to future fertility outcomes except through birth in the first period. For example, when we consider unplanned pregnancy as an instrument for first childbirth, we assume that abortions do not affect the labor market outcomes and fertility trajectories of women who have an abortion following an unplanned pregnancy (i.e. never-takers in period $t = 1$). We discuss this arguably strong assumption in more detail and provide corroborating empirical evidence when presenting the dynamic treatment effects of having the first child in Section 4.4.

A4 : **Monotonicity:** $T_{i1}(Z_i = 1) \geq T_{i1}(Z_i = 0) \ \forall \ i$

In our setting, the monotonicity assumption requires that an unplanned pregnancy shifts each woman to either have a child in the first period or not be affected by the instrument.

A5 : **No anticipatory effects for compliers who are treated in periods $t > 1$:**

$$Y_{it}(\tau = 0) \perp \{T_{i,t+1}, T_{i,t+2}, \ldots\} \quad \text{if} \quad \mathcal{C}_i = 1 \quad \forall t > 1$$

The no anticipation effects assumption requires that pretreatment outcomes do not vary by timing of future treatment within a later-treated individual. This assumption assures that we compare to the untreated counterfactual of having no child and is needed because the instrument only generates variation in the post-treatment period and anticipatory effects are not identified. Note that this does not rule out selection effects, where individuals with better (or worse) outcomes select into future treatment.[30] A5 may seem like a strong or counter-intuitive assumption given that this paper distinguishes between planned and unplanned births. If women make anticipatory changes to their lives before having children—such as changing jobs in order to be closer to future grandparents—A5 means that these changes do not affect earnings *before* the birth of the child. A5 is consistent with these anticipatory changes reducing the impact of childbirth on later earnings, compared to the counterfactual in which women still lived far from their parents. Likewise, high-earning women delaying motherhood for career reasons is a form of self-selection that also does not violate A5. Recall also that this assumption is only required for a particular subgroup of the analysis: the later-treated compliers. As this group is relatively small due to the strong intentions of LARC users, we expect any potential biases to be likewise relatively small. We discuss this assumption in more detail in Section 4.4 and provide an alternative estimation where we allow for anticipation calibrated to event study pre-trends.

We adopt a "forward-engineering" approach to showing identification, similar to Baker et al. (2026) for difference-in-differences and Cellini et al. (2010) for regression discontinuity designs. In the dynamic IV case, we sequentially consider Wald estimators comparing outcomes in each period $t = 1, 2, 3, ...$ This approach makes clear what is being identified under explicitly stated assumptions. The Wald estimator for period t when treatment is assigned in period $t = 1$ is:

$$\text{Wald}_t = \frac{\mathbb{E}[Y_t|Z=1] - \mathbb{E}[Y_t|Z=0]}{P\left[T_1 = 1|Z=1\right] - P\left[T_1 = 1|Z=0\right]}$$

Let $\rho_i(\tau) = Y_{it}(\tau) - Y_{it}(0)$ be the dynamic treatment effect for individual i who has been exposed to treatment for τ periods. That is, the causal effect of having a $\tau - 1$ year old child relative to the counterfactual of not having a child yet.

Proposition 3.1. *Given Assumptions 1-5, the dynamic causal effect for compliers ($\mathcal{C} = 1$) is identified in the first period. Identifying the dynamic effects in subsequent periods requires accounting for bias terms that arise*

[30]Consider the example of a medical treatment. A5 rules out situations in which a patient initially assigned to the control group later finds a way to receive treatment and begins to feel better in anticipation of receiving treatment. Following the same example, A5 does not rule out less healthy patients in the control group making more effort to receive the treatment in later periods.

from compliers in the control group receiving treatment after the first period (dynamic non-compliance):[31]

$$
\begin{aligned}
\underbrace{\mathbb{E}\left[\rho(1)|\mathcal{C}=1\right]}_{LATE_{\tau=1}} &= Wald_1 \\
\underbrace{\mathbb{E}\left[\rho(2)|\mathcal{C}=1\right]}_{LATE_{\tau=2}} &= Wald_2 + \mathbb{E}\left[\rho(\tau=1)|T_2=1,\mathcal{C}=1\right] P[T_2=1|\mathcal{C}=1] \\
&\vdots \\
\underbrace{\mathbb{E}\left[\rho(t)|\mathcal{C}=1\right]}_{LATE_{\tau=t}} &= Wald_t + \sum_{s=2}^{t} \underbrace{\mathbb{E}\left[\rho(\tau=t-s+1)|T_s=1,\mathcal{C}=1\right]}_{LATE\ for\ period\ s\ later\text{-}takers} \ \underbrace{P\left[T_s=1|\mathcal{C}=1\right]}_{Prob.\ of\ later\ treatment}
\end{aligned}
$$

Each bias term is a product of the dynamic causal effect for the later-takers and the probability of treatment in later periods for compliers. Given Assumptions 1-5, the probability of later treatment for compliers can be estimated directly from the data. Identifying the causal effect for $\tau > 1$ requires an additional assumption about the causal effects for the later-treated in the control group. In some settings, such as the IVF setting, it may be reasonable to assume that the average causal effect is the same for initial- and later-treated compliers, and identification of the causal effects follows recursively. This is formalized as Assumption 6:

A6 : **Initial- and later-takers have the same LATE:**

$$\mathbb{E}\left[\rho(t)|T_s=1,\mathcal{C}=1\right] = \mathbb{E}\left[\rho(t)|\mathcal{C}=1\right] \forall s,t$$

Corollary 3.1.1. *Given Assumptions 1-6, the dynamic causal effects are identified in all periods.*[32]

In our LARC setting, the women having children in later periods are likely having planned pregnancies, which may have different causal effects. Assumption 6 is thus a strong assumption in the LARC setting and we instead impose a set of alternative assumptions for the effect of planned pregnancies (later-treated): (1) identifying dynamic effect of planned birth as above using initial IVF success as an instrument for planned birth, (2) imposing Assumption 6 that the effects of initial unplanned and later planned births are the same, (3) assuming that an event study around childbirth estimates the effect of planned birth, and (4) identifying dynamic effect of planned birth as above using initial IVF success as an instrument for planned birth, but re-weighting the women undergoing IVF to have characteristics similar to the unplanned birth population.

[31]The proof is in Appendix Section B.1.

[32]It follows from Proposition 3.1 that the effect in each period can be solved recursively. In other words, the causal effect in the first period can be used to calculate the bias term in the second period, and so on.

Estimation Strategy We develop a dynamic IV-GMM framework that estimates LATE_τ jointly and is flexible to accommodating different assumptions about the effect of the later-treated. We follow the sequential approach in defining the system of moments, where each moment corresponds directly to the identification equation for each period $t \geq 1$ as listed in Proposition 3.1. Let $\widehat{\rho}(\tau)$ denote the estimate of $\text{LATE}_\tau = \mathbb{E}[\rho(\tau)|\mathcal{C} = 1]$. In our baseline specification, we jointly estimate heterogeneous $\widehat{\rho}(\tau)$ using both IVF and LARC settings, where a planned $\widehat{\rho}^p(\tau)$ is identified from the IVF setting and used to estimate the effect of "planned" later-births among LARC users. The unplanned $\widehat{\rho}^u(\tau)$ is identified from the unplanned pregnancies occurring in the first period among LARC users. The joint LARC-IVF IV-GMM estimator is:

$$g_i(\theta) = \begin{bmatrix} Z_i^{IVF}\left(Y_{it} - \sum_{\tau=1}^{T} \rho^p(\tau)\mathbf{1}[\tau - 1 = t - t_i^p]\right) \\ Z_i^{LARC}\left(Y_{it} - \sum_{\tau=1}^{T} \rho^u(\tau)\mathbf{1}[\tau - 1 = t - t_i^u] - \sum_{\tau=1}^{T} \rho^p(\tau)\mathbf{1}[\tau - 1 = t - t_i^p]\right) \end{bmatrix} \tag{2}$$

where the first (second) set of moments use data from the IVF (LARC) setting, t_i^u refers to the year of the first unplanned LARC birth, t_i^p refers to the year of other first births in the sample of LARC users and to IVF births, and all variables are demeaned within age $\times$ year cells. In Appendix Section B.2, we show the specifications for other assumptions about the later-takers.

4 Results

In this section we document the effect of unplanned pregnancy on labor market outcomes among LARC users. To do this, we plot the labor market trajectories of women in the seven years before and after they receive the LARC, comparing those who have a pregnancy soon after receiving this contraceptive to those who do not become pregnant, conditional on age and year of prescription. We compare these estimates to those obtained for women who (1) already have a child, (2) who experience unplanned pregnancies while on other forms of birth control such as the pill, and (3) who have planned children as a result of IVF procedures. We next implement a dynamic IV strategy to obtain estimates of the impact of childbirth on labor market outcomes over time in a setting with dynamic non-compliance. We focus our main results on seven outcome variables: earnings including paid leave, earnings, employment, the propensity to work in occupations requiring higher skills, total number of childbirths, and the propensity to receive prescription drugs to treat anxiety and depression. We present additional results on hours and wages to understand the drivers of the patterns we see in the primary outcomes.

4.1 Effect of Unplanned Pregnancy on Labor Market Outcomes

First, we focus on women with a LARC prescription. For various outcomes, the left-hand side of Figure 1 presents raw means, re-weighting the control group (women who do not experience an unplanned pregnancy) to have the same age and year-of-prescription distribution as the treatment group (women who experience an unplanned pregnancy). In Appendix Figures A4 and A5, we further break the treatment group into those who have an abortion and those who give birth following an unplanned pregnancy. The right hand side of Figure 1 plots the estimated impact of unplanned pregnancy using equation (1), along with 95% confidence intervals. Figure 1a shows the average rate of first births for the two groups and Figure 1b plots the first stage effect: Two years after their LARC prescription, the share of women who have had a child is 70% higher among those who experience an unplanned pregnancy than among those who do not, because about 30% of women have abortions following an unplanned pregnancy. However, seven years after LARC prescription, 40% of women who did not have an unplanned pregnancy (women in the "control" group) have become mothers.[33]

Unplanned pregnancy has a large impact on earnings inclusive of paid leave and is not predicted by pre-LARC earnings conditional on age and date of prescription.[34] Figure 1c shows that earnings diverge immediately following an unplanned pregnancy.[35] The effect of an unplanned pregnancy on earnings shrinks over time but is economically meaningful throughout our window and is equal to about a 19% impact of unplanned pregnancy on income five years after the LARC prescription.

Figure 2 presents the effect of unplanned pregnancy on additional labor market outcomes: earnings (excluding publicly paid leave), employment, and occupation. Employment, depicted in Figure 2d, falls only temporarily with unplanned pregnancy. Although employment effects are not precisely estimated seven years after prescription, point estimates suggest that women who have an unplanned pregnancy are as likely to be employed as in the counterfactual in which they did not experience an unplanned pregnancy. However, an unplanned pregnancy substantially reduces the likelihood that women advance in the career ladder, as

[33] When we extrapolate the probability that these women will ever have a child, we estimate that 60% of the control group will have a child by age 47. We obtain this estimate by giving women of age a who do not have children by period t the observed childbearing probability between period t and $t+1$ equal to the probability of childbirth in the next year for women of age a in the control group 5-7 years after a LARC prescription who have not yet had children in those years. We believe this is a slight underestimate, since the conditional probability of childbirth generally increases with years after LARC even controlling for age.

[34] Our preferred definition of income includes both labor market income and paid leave due to sickness, pregnancy, maternity leave, and parental leave. This is closer to the labor market earnings definition used in Kleven et al. (2019a) and an important source of earnings for women who have recently had children (Adams et al., 2024). We also present the effect of unplanned pregnancy on labor market earnings exclusive of government paid leave—these effects are qualitatively similar, but larger, as in Figures 2a and 2b.

[35] We check if these results are sensitive to dropping outliers as suggested by Broderick et al. (2020). As the "treated" group is quite small relative to the "control" group, we perform an analysis in the spirit of Broderick et al. (2020), where we stratify the sample by unplanned pregnancy. We estimate a constant effect in the post-LARC period and we drop a fraction F of treated observations with the lowest income and a fraction F of control observations with the highest income. We have to drop about 2.9% or 8824 of the most influential observations for the p-value to be greater than 5%. If we consider only the most influential treated (control) observations, then we need to drop the 10% (4.9%) of the most influential observations.

measured by the skill requirements of their job. In Figure 2f we see that by seven years after an unplanned pregnancy, women are 20 percentage points less likely to be in occupations with higher skill requirements, compared to the counterfactual. Our results are not driven by women moving into lower-skilled occupations after having an unplanned pregnancy, but instead they do not advance to higher-skilled occupations at the same rate as the control group (see Figure 2e). Appendix Table A3 shows that 1% (3%) of those who had (did not have) an unplanned pregnancy switched to occupations with lower skill requirements, while 17% (25%) switched to occupations with higher skill requirements.

When we look at effects on hours and wages, we find a consistent pattern where wages, not hours, are lower in the longer term. Appendix Figure A6 plots our measure of wages and hours, as well as an indicator for whether data on wages and hours are not missing. In the short term, decomposing earnings into changes in wages vs. changes in hours poses data challenges, as only a subset of firms are sampled to report wages during a sample week in each year, and women are less likely to be in the wage data around motherhood.[36] If we focus on the years in which representation of women in the data is similar for those with an unplanned pregnancy vs. not (years 5, 6, and 7), the evidence suggests that wage declines and not hours reductions are driving the impact of unplanned pregnancy on earnings. This pattern is also consistent with the fact that women experiencing an unplanned pregnancy stall their progress up the occupational ladder, as discussed above. Our results on employment rates, wages, hours, and occupation are jointly consistent with earnings declines that stem from changes in the type of work women are doing as a result of unplanned pregnancy and not from changes in the amount women work.

Figure 3 turns to non-labor market outcomes: number of children and mental health, as measured by prescriptions for medication to treat depression or anxiety. As depicted in Figure 3a, women who experience an unplanned pregnancy have on average about 1.5 children seven years after an unplanned pregnancy, while women who do not experience an unplanned pregnancy have on average 0.5 children seven years after the initial LARC prescription. This difference persists when we restrict to women who are 35 or older at the time of LARC prescription, who have likely completed their fertility by the end of the study window (see Appendix Figure A7), suggesting the gap reflects completed fertility rather than a shift in fertility timing. This means that there is an impact of unplanned pregnancy on the number of children beyond the unplanned child.[37] This may be because in our setting having two children is generally more common than having a single child (see Appendix Figure A1d and Sobotka and Beaujouan (2014)). We find that there is a slight decline in the propensity to take anti-anxiety and anti-depression medication in the years immediately following an unplanned pregnancy. This may be because women who are pregnant and nursing generally avoid taking

[36] See Appendix C.5 for details concerning the wage data.

[37] This finding is similar to Londoño Vélez and Saravia (2025), who find the effect of an abortion denial on the number of children that is twice as large as the first stage effect on the birth of the focal child.

these medications, or it may reflect improved mental health. In the longer term, we do not see any effect on the propensity to take anxiety or depression medication relative to the control group (as depicted in Figures 3c-3f), but imprecisely estimated.

Turning to other aspects of family life, the presence of a partner may help mitigate career costs for women. Unfortunately, data limitations preclude us from studying the evolution of partner outcomes around an unplanned pregnancy relative to a control group.[38] Nonetheless, we can compare partnership and cohabitation rates for women in our unplanned pregnancy sample relative to other women at the time of their first birth and in the following years. Appendix Table A4 presents the average outcomes following first birth for women who have their first child due to an unplanned pregnancy (column 3), compared to the first birth of past LARC users (presumably these births are not unplanned, column 2), and first births in the population overall (column 1). One striking pattern is that though women whose first child is unplanned are the least likely to be living with the father of that child at birth, the baseline level of cohabitation is high—79 percent. By five years after birth, this falls to 67 percent but still that rate is only five percentage points lower than the rate for past LARC users overall. These patterns suggest that women who experience an unplanned pregnancy generally have a relationship with the father of the child.

Robustness. First, we consider robustness to the window around a LARC prescription that we use to label pregnancies "unplanned." A three month window is our most conservative definition as it is unlikely that pregnancy intentions changed for LARC users so quickly that they decided to get the LARC removed and were able to conceive within three months of initially purchasing the device. The three month window is represented by the gray line in the first panel of Figure 4, (a) and (b): the estimates of the impact of pregnancy on earnings are similar to the nine-month window but they are substantially less precise. The rate of giving birth following an unplanned pregnancy increases with the window. This pattern likely reflects a change in the strength of the intention to avoid pregnancy, which may decline on average as more time passes since the purchase of this very effective form of birth control. Following nine months, there is a small monotonic increase in the pre-LARC difference between treatment and control, possibly due to planned births entering the sample as the window following LARC purchase gets longer. Overall, these figures and the evidence discussed in Section 2.3 from surveys of LARC users lead us to conclude that the 9-month window is a reasonable baseline definition of unplanned pregnancy and that the control group offers a reasonable counterfactual. A three month window would bring us to similar, but substantially noisier, conclusions. Alternative windows for the other outcomes are presented in Appendix Figure A8.

[38] In our data, two unmarried individuals are only recorded as cohabiting if they are cohabiting *and* share common children. This means that we cannot look at how cohabitation evolves over time for women who experience an unplanned pregnancy and a control group who does not because we would not know whether women in the control group are cohabiting as they do not have children.

To assuage concerns about imbalance due to education and civil status, Appendix Figures A9 and A10 present our baseline specification compared to three specifications where we additionally match on civil status, an indicator for higher education, and the interaction between higher education and marital status in the year pre-LARC. We find that this additional matching neither significantly impacts pre-LARC balance nor the estimated longer-term effects.

Appendix Figure A11 shows a variety of additional robustness checks. First, to the extent that IUD births may lead to birth defects, we find our results are similar whether we consider IUDs or Implants. Second, a concern with our identification strategy is that even if women who experience an unplanned pregnancy look similar to those who do not upon receiving the LARC, they may be on unobservably different trends. For example, those who experience an unplanned pregnancy may be more likely to be in a relationship than those who do not, and this relationship may have eventually caused them to step back from the labor market regardless of the unplanned pregnancy. We cannot test this possibility directly, but we can split the sample into those who are married at the time of getting the LARC to those who are not married. When we do this, we see that the results are similar between the two groups—the difference between unmarried treatment and control is similar to the difference between married treatment and control, where we observe the partnership status. This is displayed in Appendix Figure A11b. In Appendix Figure A11c we include additional control variables: the first specification adds the average outcome over the three years prior to LARC purchase, and the last four specifications sequentially add controls for educational attainment, civil status, employment and occupation, and health status.[39] The additional controls do not significantly shift the balance in the pre-LARC period. Finally, given we have an unbalanced panel, we may worry about composition driving some of our results. Appendix Figure A11d displays the main specification but restricting the sample to those who are observed for a certain number of years post-LARC, mostly selecting on calendar year of birth. If anything, the estimates are most negative when we restrict to the women with the earliest pregnancies (e.g. LARCs taken out in 2006 are observed for seven years, in 2007 for six years, and so on).

4.2 Comparing Alternative Methods for Identifying Fertility Intentions

Another concern may be that our sample of 352 unplanned pregnancies resulting from LARC failures is not representative of unplanned pregnancies more broadly, since LARC use may reflect a stronger intention to avoid pregnancy, compared to the typical unplanned pregnancy. We now turn to the pool of SARC users and implement alternative definitions of unplanned pregnancy, as described in Section 2.3.2. We note here that neither SARC strategy allows us to confidently identify all individuals experiencing an unplanned pregnancy, only those who have an unplanned birth, so some caution in interpreting the effect of motherhood among

[39] The note to Appendix Figure A11 lists each control variable.

SARC users causally is warranted. The second panel of Figure 4, (c) and (d), presents our main outcomes for the baseline LARC definition (black line), as well as for births among women with a SARC prescription in which the mother filled a SARC prescription within two weeks of conception (purple line), and births among women using SARCs who have a meeting to discuss abortion but instead give birth (blue line).

We see that the first stages in all of these cases are similar after year two with some mechanical differences in the first years.[40] Labor market earnings also show similar patterns relative to our baseline definition among LARC users. Appendix Figure A12 presents the other outcomes. We see similar paths relative to LARCs, though we see some pre-trends in occupation for one of the SARC definitions and there are worse mental health outcomes in period $t = -1$ associated with SARC unplanned births.[41] This may reflect some correlation in SARC failure with individual behaviors. One advantage of studying births following abortion counseling meetings is that these data are available for a longer period than the prescription data, implying that we can look at outcomes beyond seven years post-prescription. Appendix Figures A15 and A16 present the effects of unplanned birth among SARC users who have an abortion meeting, and the effects among women who have an abortion meeting, matching only on age and year. The effects are similar, and earnings do not recover when we look at the longer panel. The reduced form impact eleven years after an unplanned birth is about ten percent.

While the earnings patterns are remarkably similar across various definitions of unplanned pregnancy, we find rather different patterns when studying other types of pregnancies. In Figure 5, we present three alternative comparisons. The first panel of Figure 5 presents two potential definitions of "planned births." The teal lines compare women whose first IVF procedure is successful vs. those whose first IVF procedure is unsuccessful, as in Lundborg et al. (2017) and Bensnes et al. (2025), and as described in Section 3.1. We find *no impact* of a first successful IVF procedure on earnings including parental leave, despite a strong first stage.[42] Next, we consider SARC users but instead of looking for unplanned pregnancies, we identify new mothers whose last SARC prescription was 365 days to 84 days before the date of conception of their first child, and compare these women's trajectories to those of women who filled SARC prescriptions on the same date and who are the same age, but who presumably continued to use birth control to avoid pregnancy.

[40] The abortion counseling definition, like our baseline LARC definition, allows the conception to occur many months after the prescription, while the other SARC definition has a very narrow window (14 days) post-prescription in which pregnancy can occur.

[41] Appendix Figures A13-A14 present estimates for different windows in which women filled contraceptive prescriptions around conceptions, while Figures A15-A16 present estimates comparing births to women using SARCs who have an abortion meeting to the corresponding estimates for all women.

[42] Because the probability of IVF success is high and our data include almost ten thousand IVF procedures (for nulliparous women), our standard errors allow us to detect even small differences in earnings in the pre-IVF period. In the years before the first IVF procedure, women with a successful first treatment have slightly higher earnings (Figure 5b). Appendix Figure 5 presents other outcomes which also show imbalance. It is a concern that the differences reflect the fact that wealthier women seek out better doctors or hospitals with a higher probability of a success. We present estimates of the impact of IVF success on outcomes both matching only on age, time since contraception, and year of IVF, and also matching additionally on education. We also do this for comparability with other papers in this literature who match on education, such as Bensnes et al. (2025).

In this group also we see a strong first stage but also large pre-trends and, similar to the IVF group, no long-term earnings impact. The strong pre-trends are suggestive that in general, childbirth is not randomly timed. Appendix Figure A17 presents these IVF and SARC-based definitions of planned-pregnancy effects for other outcomes. In the bottom panel of Figure 5 we study *unplanned* pregnancies but among LARC users who have already had a child. Recall that we find imbalance in mental health outcomes for women with postpartum mental health prescriptions and present results for the full sample and the subsample of women who do not get a mental health prescription in the year after the birth of their first child. Figure 5d plots the effect of an unplanned pregnancy among women who already had one child, matching on age, year, and age of first child at the time of getting the LARC. We find similar first-stage effects when we consider this group compared to nulliparous women. Like nulliparous women, women who are already mothers who become pregnant within 9-months of purchasing a LARC look similar to those who do not before getting the LARC. However, the average earnings effect is a persistent earnings reduction of five to ten percent, substantially less than what we see for unplanned pregnancies in nulliparous women. Appendix Figure A18 presents the second unplanned child effects for other outcomes. In contrast to the main results, we see no effect on occupation for women who already have one child. Overall, the data suggest substantially smaller effects of unplanned second children compared to unplanned first children.

Taking the results in Figures 4 and 5 together, we conclude that among non-mothers, unplanned pregnancies have a large impact on women's careers, using a wide range of definitions of unplanned pregnancy. In contrast, planned first pregnancies and unplanned second children have earnings impacts about one-third as large, and seem to have no impact on women's careers. In the next subsection, we further disentangle these effects in light of differences in the probability of having children in the control group over time depending on intentions and circumstances.

4.3 When is Unplanned Pregnancy Most Disruptive?

Our main results describe the impact of unplanned pregnancy on women who were using LARCs to delay (or avoid) pregnancy. This combines two effects: these women have unplanned (rather than planned) first births, and these women are not able to delay birth as intended and thus are younger at the time of their first pregnancy. Below, we test for age-differences in the impact of pregnancy by estimating heterogeneous effects of unplanned pregnancy for younger compared to older women.

An ideal setting to study this question would be an experiment in which women of the same age who wanted to delay pregnancy for the same number of years experienced an unplanned pregnancy at random times. In our setting we are able to study the impact of unplanned pregnancy on women in different circumstances, all of whom were purchasing LARCs to delay pregnancy. These settings are not necessarily the same. Young women who do not experience an unplanned pregnancy but go on to have children soon after the initial birth control prescription mechanically cannot be in the set of older, childless LARC users. However, we consider the birth rate sufficiently small that our setting comes close to the ideal experiment. In Appendix Table A5 we go further and re-weight younger LARC users so that they have similar composition to older LARC users based on observables as measured when the older group was the same age as the younger group.

Figure 6 shows that younger women have earnings losses which are on average more than twenty percentage points larger than those of older women. These differences by age are significant in years 3-4 after prescription, and in those years the compliance rate (relative probability of having a child) is similar. Differences in circumstances seem particularly important when we investigate the importance of human capital accumulation. We find earnings losses of around 40% of counterfactual earnings in years 3-5 after prescription for women who have unplanned pregnancies while enrolled in education, while the impact of unplanned pregnancy on earnings is only about 15% among women who are not enrolled in education at the time of the unplanned pregnancy.

The initial similarity in the probability of a first child suggests that differences in the abortion rate do not drive differences in the impact of unplanned pregnancy by age and education investments. However, turning to the later periods, we see compliance rates diverge slightly as the younger women who initially delay pregnancy are more likely to have a child later than the older women. This may explain some of the convergence in the earnings paths of the two groups. We turn to explicitly accounting for this dynamic non-compliance with treatment assignment (i.e., having children later) in the next section.

4.4 Dynamic Impacts of Unplanned Births

So far, we have focused on how unplanned pregnancies among LARC users affect their labor market outcomes, mental health, and family size over the course of seven years post-LARC. In this section, we present the impacts of an unplanned *birth* using the IV methodology outlined in Section 3.2. These estimates are necessary if we want to compare our results to existing estimates of the impact of children on women's careers in the literature. These estimates are also necessary if we want to formally assess the degree to which age heterogeneity in reduced form estimates of the impact of unplanned pregnancy discussed in the previous paragraph are driven by differences in dynamic compliance by age or differences in the effect of children by age. Essentially, in order to obtain estimates of the dynamic impact of children relative to a counterfactual of not having a child, we need to account for the impact of later children in the control group in our estimates.

To compare the earnings path of women who have a child in period 0 to what their earnings would have been if they had not had a child in periods 0 through t, we assume that unplanned pregnancy is a valid instrument for an unplanned birth. We assume that later births in the control group (women using LARCs who did not experience unplanned pregnancies) are planned, and these births may have a different impact on labor market outcomes because, for example, women are further along in their career.

In our baseline specification, we assume that IVF success is a valid instrument for planned birth. However, we present robustness to several alternative specifications of the impact of planned pregnancy, including event study estimates as in Kleven et al. (2019a). This is because of concerns that the exclusion restriction is violated for the IVF instrument, discussed extensively in Bögl et al. (2024). The exclusion restriction means that IVF failure does not itself affect labor market outcomes of women, which may not be a reasonable assumption if some women in the control group experience a shock—learning they are infertile despite wanting children—which has its own labor market effects.

In the case of unplanned pregnancy, we must assume that experiencing an unplanned pregnancy does not affect labor market outcomes except through its effect on the probability of an unplanned birth. If women who have an abortion experience labor market impacts from their abortion, this would violate this assumption. We present conditional means split by those who have an abortion and those who give birth in Appendix Figures A4 and A5. It is clear from these figures that the never-takers (those who have an abortion) are on lower earnings paths and have worse mental health relative to the compliers. However, in these figures, we do not see discontinuities at the time of abortion, suggesting that abortion itself does not impact labor market outcomes, at least in the short term. This is consistent with the American Psychological Association's (APA) position on abortion and mental health.[43] In a more qualitative account among a somewhat different population—women on the margin of being denied an abortion, Foster (2020) concludes that abortion does not have lasting impacts on women, while children do. Perhaps most compelling, in recent work Janys and Siflinger (2024) study the mental health of Swedish women around the time of an abortion and find a precisely estimated null effect of abortion on all measures of mental health.

Figure 7 plots the TT impact of children on women's earnings, employment, and occupation over six years, estimated using the GMM model in equation (7).[44] We estimate all models in levels, but present earnings effects as a fraction of the counterfactual mean earnings among those initially assigned to the control group: $\frac{\widehat{\rho}(\tau)}{\mathbb{E}[Y_{it}-\widehat{\rho}^{p}(\tau_{it})|Z=0]}$, where τ_{it} is the age of person i's child in year t. Figure 8 plots the TT

[43]The APA position is based on a panel studying the literature on abortion and mental health. They concluded that abortion following an unplanned pregnancy "does not pose a psychological hazard for most women." The APA go on to note that "women who are terminating pregnancies that are *wanted* or who lack support from their partner or parents for the abortion may feel a greater sense of loss, anxiety and distress. For most women, however, the time of greatest distress is likely to be before an abortion; after an abortion, women frequently report feeling 'relief and happiness' " (Cohen, 2006).

[44]We pool the last two years because we have few observations in the seventh period. Appendix Figure A11d shows robustness of the unplanned pregnancy effects to this right censoring.

impact of children on family and mental health outcomes. Each figure presents estimates of the effect of an unplanned birth (ρ_t^u), in black, alongside 95% confidence intervals. We plot the impact of a planned birth (ρ_t^p) in teal. For earnings, we see substantial negative impacts of an unplanned birth. The earnings impact of an unplanned birth is 30% of counterfactual earnings initially and about 25% on average in the years after birth, compared to not having a child. As in the reduced form, we see a drop in employment only in the first years after birth. Women who have an unplanned birth are almost 30% less likely to be in a higher skilled occupation by five years after birth, relative to a counterfactual in which that woman did not have a child. We also see that women are more likely to have subsequent children following an unplanned birth, and this explains some of the earnings effect. The effects for planned births are muted. Earnings exhibit a 10% reduction relative to the counterfactual for planned births. There are no impacts on other outcomes, except a short term reduction in the probability of having a prescription for depression and a short-run lag in additional births compared to what we see among women experiencing unplanned births.[45]

When we account for differences in the dynamic first stage by age and educational enrollment, we see that younger women and women enrolled in education experience the largest impacts of an unplanned birth. In Table 3, we focus on summary measures, rather than displaying the dynamics of the impact of unplanned birth due to the reduced sample size when studying subgroups of the data. The odd columns of Table 3 come from our baseline IV-GMM estimation, while the even columns add controls for the average pretreatment outcome in the three years before LARC prescription in order to increase precision. Point estimates suggest that younger women have earnings that are 32-33% lower than counterfactual earnings, while older women have earnings losses that are only 14-16% lower than counterfactual earnings. These differences by age are significant at the ten percent level. We don't find evidence that this age-wedge is driven by differences in the "type" of women receiving LARCs at various ages: in Appendix Table A5, we reweight the younger group to have similar characteristics as the older group when they were a similar age (those who would not have a child before they are 28 unless they have an unplanned pregnancy). The estimates are not meaningfully affected by this reweighting, consistent with an empirically small fraction of the young LARC users having

[45]In interpreting these differences, it is important to note that women undergoing IVF and those who experience an unplanned pregnancy are quite different from one another in terms of other observable characteristics, such as pre-pregnancy income (see Table 1). Some of these characteristics could be endogenous to planning, but we can explore whether the results are similar when we study the subgroup of IVF users who look most like our LARC population on observables. We use a DiNardo et al. (1996) propensity score re-weighting to compare estimates while holding characteristics fixed. In particular, we predict the probability that a woman in the IVF sample would be in the LARC sample based on her income, education, civil status, employment, occupation, private sector, and age in the year of the fertility procedure and weight the IVF observations by this probability. Appendix Figures A19 and A20 display the weighted versions of the estimates over time, using the IV-GMM estimator as before. When we re-weight women receiving IVF to have characteristics similar to those using LARCs (the "unplanned" sample), we find that planned births are associated with larger earnings declines relative to the estimates without the propensity score re-weighting, but these impacts of reweighted planned births are still two-thirds to half the size of the impact of unplanned birth. We note that these estimates are quite noisy. Occupation differences between the impact of planned and unplanned births remain unchanged after re-weighting. This means that unplanned birth has career consequences not only because unplanned births happen earlier in life or to women in different income groups than planned births, but also potentially because they happen at times which are unobservably sub-optimal from a career perspective.

children before age 28.

Differences in circumstances seem particularly important when we investigate the importance of human capital accumulation. We find that cumulative earnings losses for women enrolled in education are 37-39% of their counterfactual earnings. In contrast, women who are not accumulating human capital through schooling at the time of an unplanned pregnancy have earnings losses of 18-21% of counterfactual earnings. These differences are significant at the five percent level with controls. We also see that those enrolled in education spend significantly fewer years in occupations requiring medium or high skills. Appendix C.3 discusses patterns of occupation upgrading in this population in more detail. Overall, these occupation patterns paint a compelling picture that women who experience an unplanned pregnancy miss out on valuable occupation upgrading—often catalyzed by enrollment in education—during their early careers when such occupation upgrading is ubiquitous.

Robustness. Appendix Figure A21a presents our estimates of the impact of an unplanned child under different windows for defining unplanned pregnancy relative to LARC purchase. The most conservative window—three months (gray line)—gives similar point estimates as our main estimates (black line). While the survey evidence is clear that women are not changing intentions when removing the LARC in the first six months, we take a conservative approach and assume that all excess pregnancies in the nine month (relative to three month) window—30% of pregnancies—are planned. We model this by weighting each observation in the IV-GMM estimation by the probability that it was unplanned according to the timing of the pregnancy, and using estimates from the weighted IVF sample for the impact of planned births.[46] This gives us the green line in Appendix Figure A21b, which implies the impact of unplanned pregnancy might be larger than our baseline. To the extent that we misclassify some pregnancies as unplanned when they are in fact planned, our baseline estimates are a lower bound for the impact of unplanned birth.

To alleviate concerns about imbalance due to marital status and education, Figure A21c shows that our results hold when we additionally match on marital status and education the year before the prescription. Another assumption underlying our baseline estimates is that the timing of IVF success can be used to identify the impact of planned children on women's labor market outcomes. Figure A21d plots the impact of various assumptions concerning ρ^p, the *planned* birth impact, on our estimates of ρ^u, the *unplanned* birth impact. We find that our estimates of the impact of unplanned birth on earnings including paid leave are virtually unchanged under various potential (and differing) estimates of ρ^p. When we use event study estimates, weighted or unweighted IVF estimates, or assume that the impact of unplanned and planned children are the same, this does not affect our estimate of ρ^u. We also show that if we allow for reasonable

[46] By "weighted IVF sample" we refer to the estimates in Appendix Figure A19.

levels of anticipation by assuming that all of the pre-trend in event study estimates reflects changes in labor market outcomes in anticipation of pregnancy for planned births, we also see that there is not a large impact on the estimated impact of *un*planned birth. The reason these assumptions do not matter much for our conclusions is that in fact, in each period only a small share of the control group has a (presumably planned) child.

5 Discussion

In this section, we discuss the implications of our estimates for the broader understanding of the impact of children on careers. We extrapolate from our estimates to the overall population of women and relate our work to event study estimates using the empirical strategy of Kleven et al. (2019a).

In our data, the raw gender earnings gap is 31.5% in 2005.[47] What portion of this overall gap is driven by the effects of children on women's careers? Following Kleven et al. (2019a), we estimate the association between first childbirth and earnings using the subset of our data that overlaps with available medical records (2005-2013). Appendix Figure A22 presents results from an event study that includes paid parental leave in earnings.[48] On average, childbirth is associated with an 18.3 percent earnings decline over years 1-6. By comparison, our estimates of the causal effect of unplanned births show an average decline of 23.9 percent over the same horizon.

Survey evidence from a study of births in Sweden between March 2008 and August 2010—roughly overlapping with our study period—suggests that 23.2 percent of births result from unplanned pregnancies (Lukasse et al., 2015). Assuming the event study reflects the average impact of all first births, and our estimate identifies the causal impact of unplanned births, we can recover the implied impact of planned births using the equation:

$$\bar{\rho}^{p} = \frac{\bar{\rho}^{event\ study} - r * \bar{\rho}^{u}}{1 - r}$$

where r is the fraction of births resulting from unplanned pregnancies. An estimate of -16.6 percent is twice as large as our estimate of the impact of planned birth using IVF success as an instrument, as presented in Figure 7. As discussed in Bögl et al. (2024) and Martinenghi and Nejad (2025), these IV estimates may understate the effects of children because the experience of an IVF failure and/or childlessness may lead to reduced labor market earnings through channels such as mental health and divorce. Our estimates thus offer an alternative estimate of the impact of planned births on women's careers.

[47] This is calculated as the difference in average labor market earnings of men and women between age 22 and 65 in Sweden (including 0s) as a fraction of average earnings for men. If we focus on the ages of women in our sample (up to age 43) the raw gap is similar: 34.3%.

[48] Estimates of the effect of children are similar in a balanced panel using this same data window, though year of birth must be limited to 2005-2007 to maintain balance.

Our setting allows us to compare women that have the same intentions: all got highly effective birth control in order to delay having children. We see that the arrival of children shapes these women's future, but the impacts of children vary across the life cycle. Our study suggests that there is substantial heterogeneity in the impact of children depending on the circumstances surrounding childbirth. To the extent that women are aware of these costs, and the costs are idiosyncratic, we might expect that women avoid having children at certain times in their lives. Indeed, we see that women are 50% less likely to be enrolled in education when having planned vs. unplanned children. If women strategically time childbirth, it becomes difficult to interpret estimates from event studies causally, as discussed in Bensnes et al. (2025). Unplanned pregnancies provide a natural experiment that reveals career effects of children otherwise hidden by planning.

6 Conclusion

In this paper, we investigate a natural experiment in which women using long-acting reversible contraceptives (IUDs and implants) become pregnant. This setting is ideal for studying the impacts of unplanned pregnancy because LARCs are effective and work passively, so our counterfactual is not confounded with choices (such as not taking the birth control pill regularly) that make identification challenging. We document that there are no empirically significant labor market differences between women who become pregnant shortly after purchasing the LARC and those who do not in the years before they purchased the LARC, conditional only on age and year of prescription. However, after an unplanned pregnancy, a woman's career trajectory changes dramatically. Unplanned pregnancies lead to substantially lower earnings for many years, and women who have unplanned pregnancies are also less likely to advance to more skilled occupations.

Using our IV-GMM strategy to estimate the impact of unplanned children on women's careers, we find longer-term earnings impacts of about 25%, as well as large reductions in the propensity to be in higher-skilled occupations. The effects are substantially larger when women are younger at the time of the unplanned pregnancy or when they are enrolled in education in the year before. Our estimates imply that labor market impacts of children depend significantly on the mother's age and human capital investments, with planning mitigating these career impacts.

Our estimated impacts of children do not capture the effects of anticipated motherhood that manifest *before* treatment assignment. Women may choose education paths (e.g., college majors) with children in mind (Bronson, 2019) regardless of whether they later have unplanned or planned pregnancies. These early life decisions represent a component of the cost of motherhood that neither our estimates nor event-study estimates such as Kleven et al. (2019a,b) capture. A rich structural literature following Polachek (1981) and including work by Weiss and Gronau (1981), Heckman and Walker (1990), Francesconi (2002), Caucutt et al. (2002), Sheran (2007), Keane and Wolpin (2010), and Gayle et al. (2024) get some traction on assessing the dynamic trade-offs that children generate. Most closely related, Adda et al. (2017) model how women's occupation choices depend on their future fertility plans, with prospective mothers choosing occupations featuring less human capital depreciation during work interruptions. The central role of timing and planning in our findings lends strong empirical support to modeling the impact of motherhood dynamically using structural models where women consider when to have children and choose careers with associated child-related costs in mind. Combining such models with our empirical strategy to measure fertility intentions could relax the key strong assumptions about women's knowledge and timing in these models.

To the extent that unplanned pregnancies can alter women's lifetime trajectories and shape future fertility choices beyond the focal birth, unplanned births play an important role in explaining overall fertility rates. Buckles et al. (2022) finds that the recent decline in unplanned births in the US accounts for 57% of the decline in the *total* fertility rate since the Great Recession. Extrapolating our findings on the intensive margin impact of unplanned births to the US setting, as much as 85% of the decline in the fertility rate can be accounted for by the decrease in unplanned pregnancies. The generational shift toward delayed childbearing and reduced unplanned births is consistent with women avoiding pregnancy during periods when it would be particularly disruptive to their career development and economic well-being.

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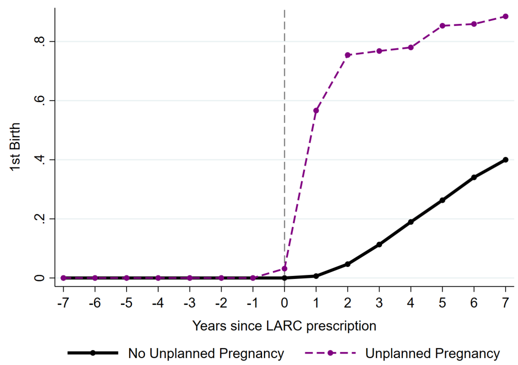
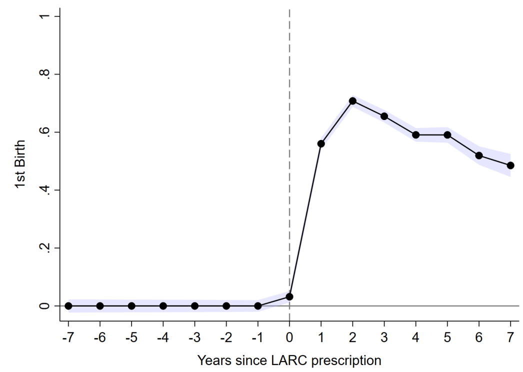
Figures

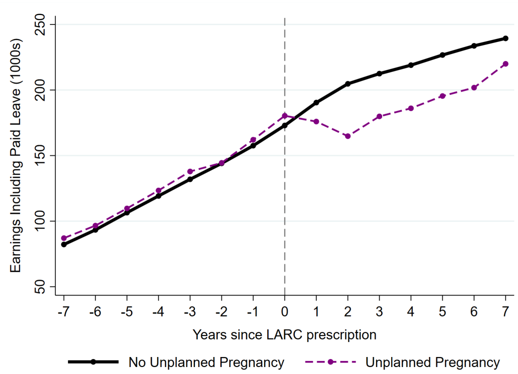
Figure 1: Matching Analysis: Dynamic Effects of Unplanned Pregnancy (LARC) on First Childbirth and Earnings



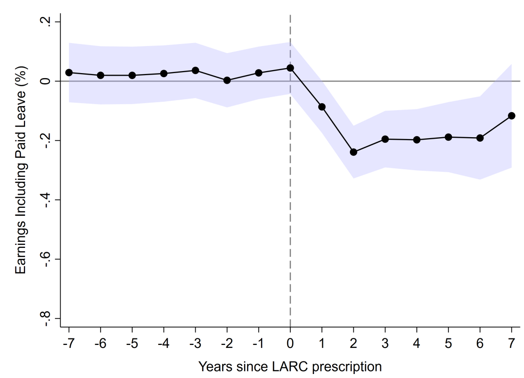
(a) Cond. Means: First Child



(b) ITT: First Child



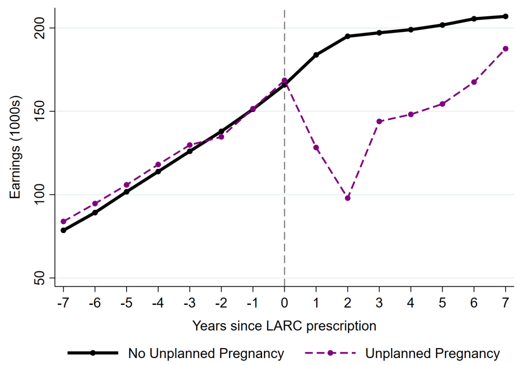
(c) Cond. Means: Earnings including paid leave



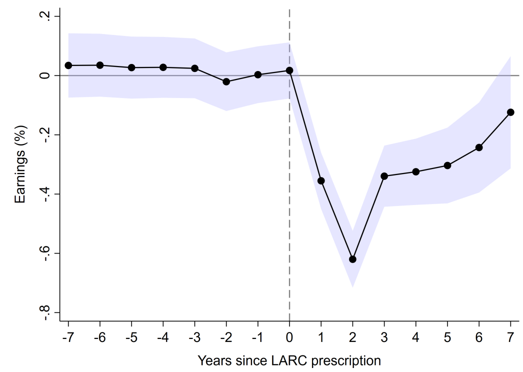
(d) ITT: Earnings including paid leave (%)

Note: This figure displays the impact of unplanned pregnancy on first childbirth and earnings including paid leave (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Earnings including paid leave is measured in thousands real 2010 SEK in (c) and percentage of control group mean (d). Panels (a) and (c) show conditional means separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Panels (b) and (d) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

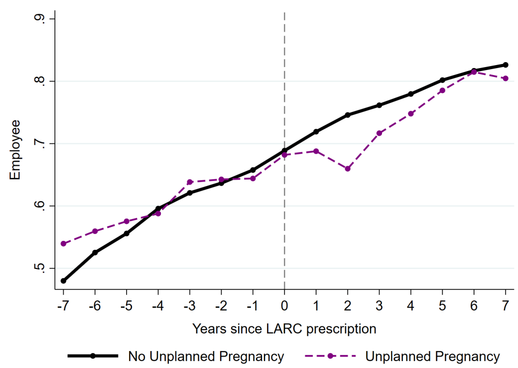
Figure 2: Matching Analysis: Dynamic Effects of Unplanned Pregnancy (LARC) on Earnings, Employment, and Occupation



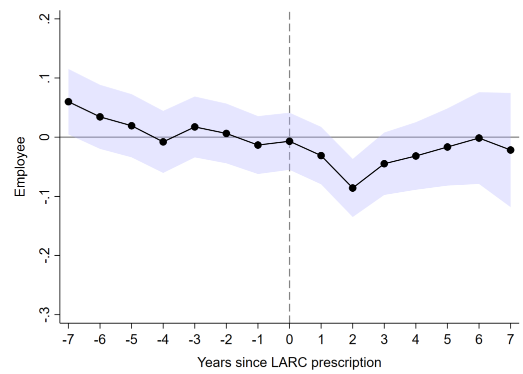
(a) Earnings



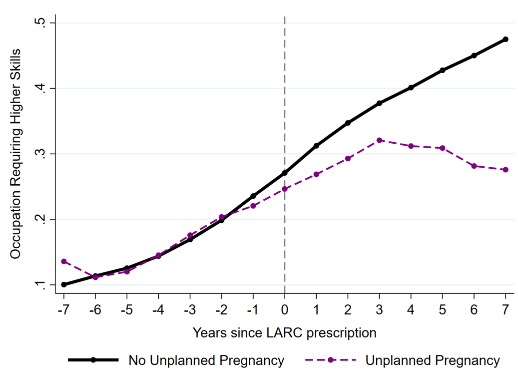
(b) ITT: Earnings



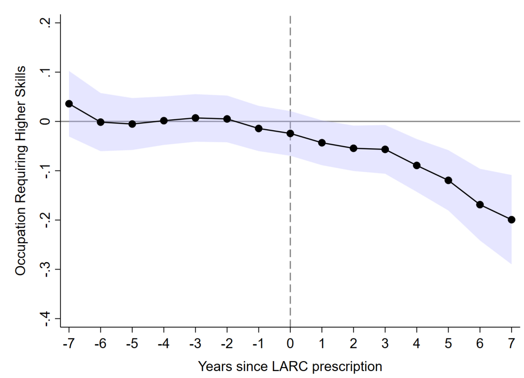
(c) Cond. Means: Employed last week of November



(d) ITT: Employed last week of November



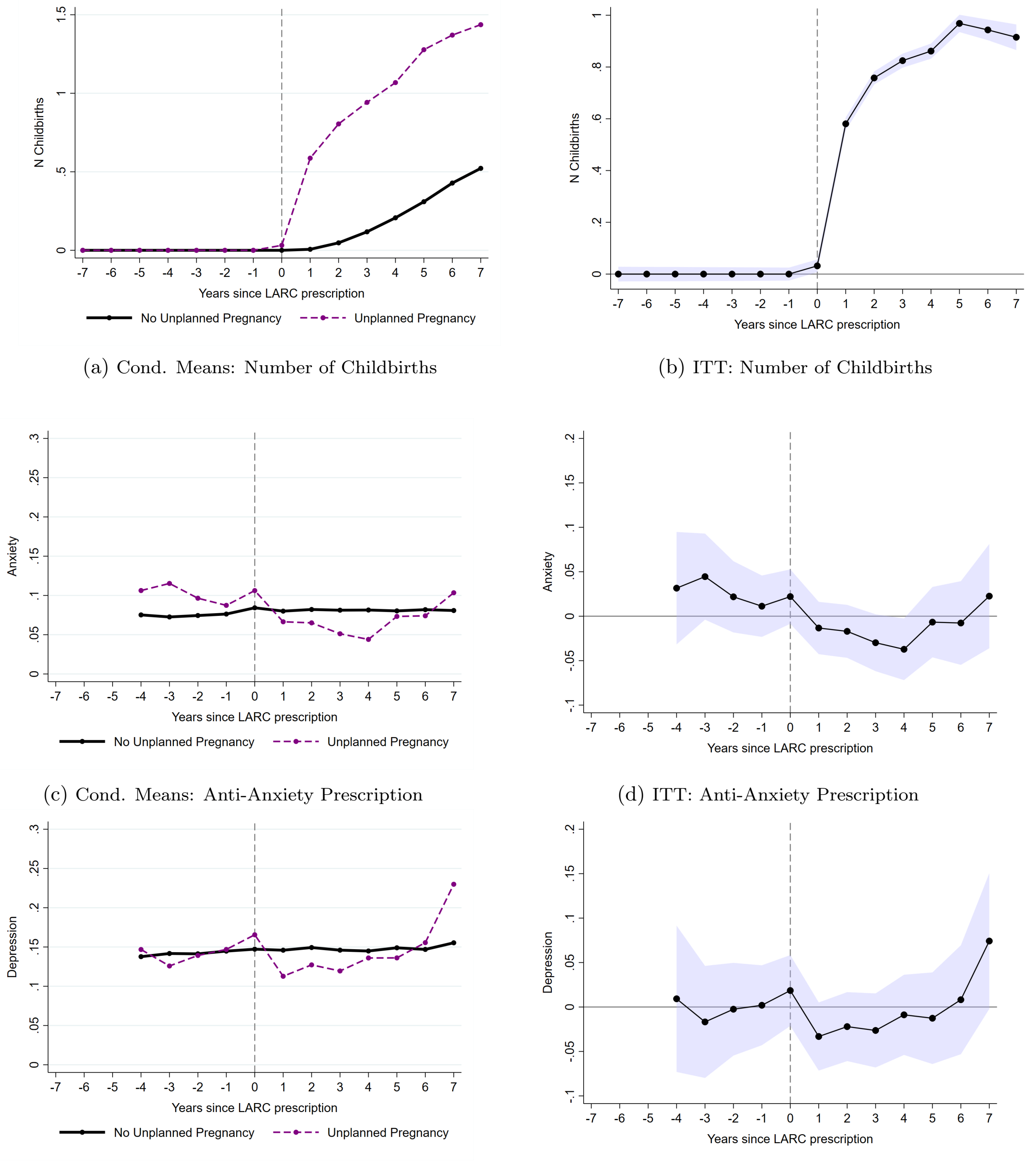
(e) Cond. Means: Occupation with Higher Skill Req.



(f) ITT: Occupation with Higher Skill Requirements

Note: This figure displays the impact of unplanned pregnancy on earnings, employment, and occupation (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Earnings are measured in thousands real 2010 SEK in (a) and as a percentage of control group earnings in (b). Employment is measured in the last week of November. Occupation is an indicator for being in an occupation requiring higher skills. Panels (a), (c), and (e) show conditional means separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Panels (b), (d), and (f) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

Figure 3: Matching Analysis: Dynamic Effects of Unplanned Pregnancy (LARC) on Number of Children and Mental Health.



(a) Cond. Means: Number of Childbirths

(b) ITT: Number of Childbirths

(c) Cond. Means: Anti-Anxiety Prescription

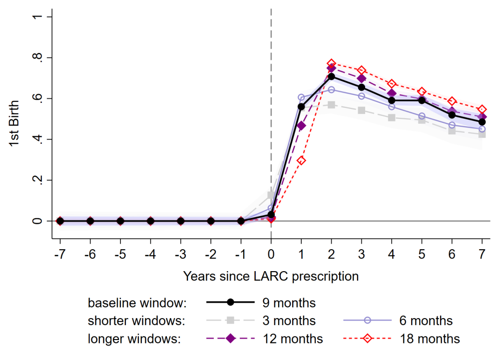
(d) ITT: Anti-Anxiety Prescription

(e) Cond. Means: Anti-Depression Prescription

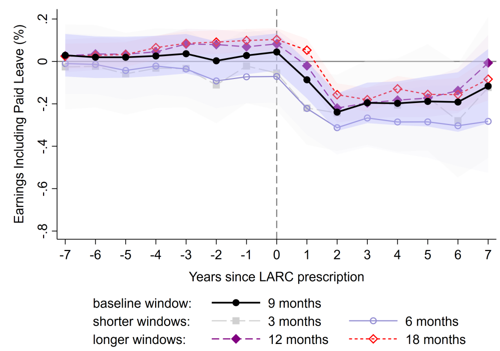
(f) ITT: Anti-Depression Prescription

Note: This figure displays the impact of unplanned pregnancy on number of children born to date, prescription for anti-anxiety medication and prescription for anti-depression medication (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Prescriptions for Anti-Anxiety or Anti-Depression medication are indicators for whether an individual filled any such prescription in the year. Panels (a), (c), and (e) show conditional means separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Panels (b), (d), and (f) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

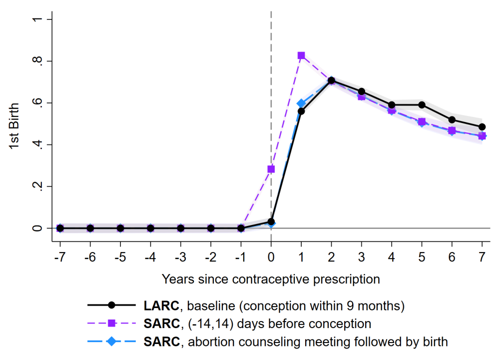
Figure 4: Matching Analysis: Dynamic Effects of Unplanned Pregnancies Using Alternative Definitions



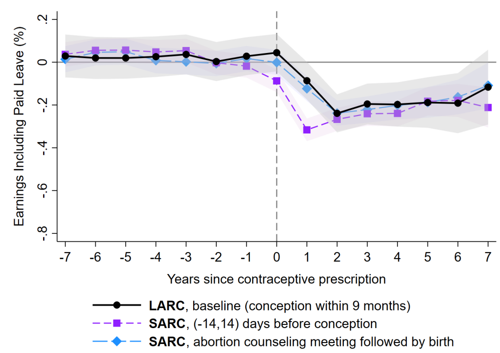
(a) First Childbirth



(b) Earnings including paid leave



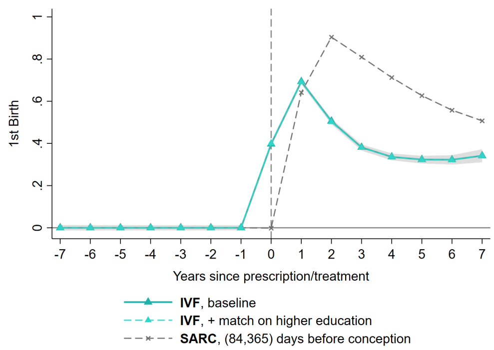
(c) First Childbirth



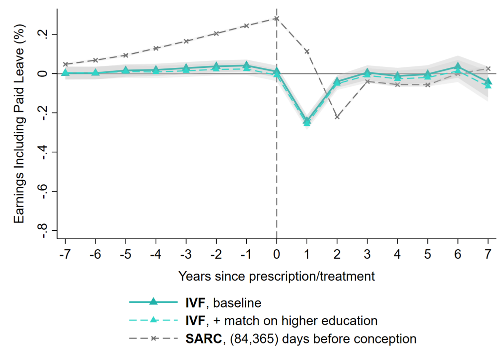
(d) Earnings including paid leave

Note: This figure displays the impact of unplanned pregnancy on labor market and birth outcomes (y-axis) by time since contraceptive prescription (x-axis). The vertical dashed line marks the year of prescription ($t = 0$). Figures (a) and (c) show the dynamic effects and 95% confidence intervals for giving birth to a child (i.e., the "first stage"), and figures (b) and (d) show earnings including paid leave as a percentage of control group earnings. Baseline control variables include a saturated model with indicators for age and year of prescription. Our baseline definition of unplanned LARC pregnancy is compared to various windows since LARC for defining unplanned pregnancies in (a) and (b) and compared to two alternative definitions based on SARCs in (c) and (d); see Section 2.3.2 for definitions and Appendix Figures A13 and A15 for comparison of SARC definitions to alternatives. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC or SARC prescription.

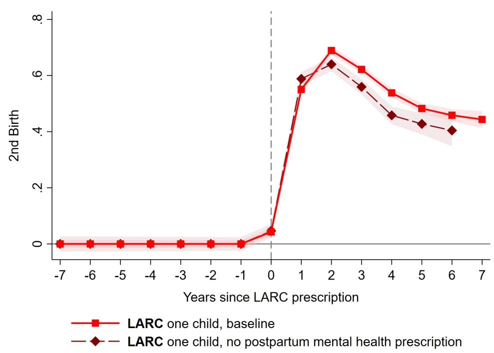
Figure 5: Matching Analysis: Dynamic Effects of Planned Pregnancies and 2nd Children



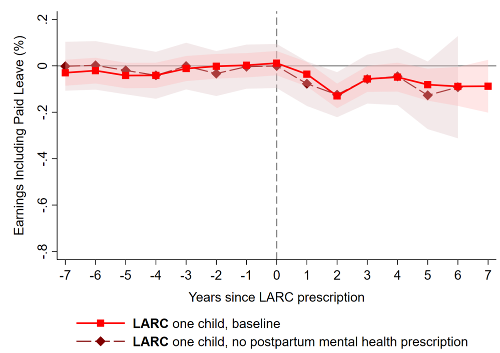
(a) Planned: First Childbirth



(b) Planned: Earnings including paid leave



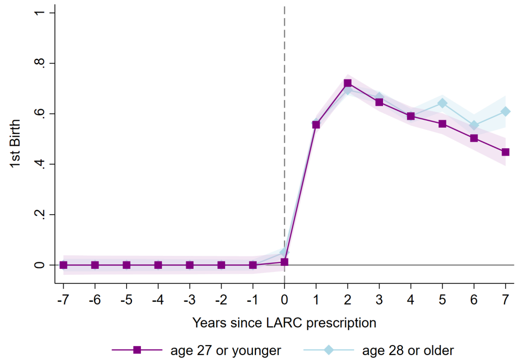
(c) 2nd Child: Additional Childbirth



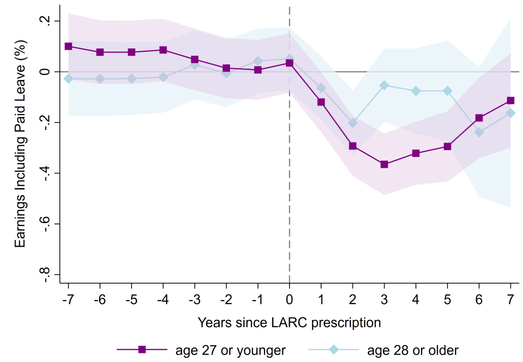
(d) 2nd Child: Earnings including paid leave

Note: This figure displays the impact of pregnancy on labor market and birth outcomes (y-axis) by time since LARC or SARC prescription or IVF treatment (x-axis). The vertical dashed line marks the year of LARC or SARC prescription or IVF treatment ($t = 0$). These figures show the dynamic effects and 95% confidence intervals for an additional childbirth (i.e., the "first stage") in (a) and (c), and for earnings including paid leave relative to the control group mean in (b) and (d). Baseline control variables include a saturated model with indicators for age and year of LARC or SARC prescription, as well as years since first childbirth for the LARC specifications. The IVF sample additionally interacts age and year of IVF treatment controls with time since last contraceptive and the "+ match on higher education" specification adds an additional interaction with an indicator for completing higher education. Sample: Women born in 1965-83 with no prior childbirths at the time of the SARC prescription or IVF treatment (top panel) or with one prior childbirth at the time of LARC (bottom panel). We also present results for a LARC subsample of those who do not take out a prescription for any anti-depression or anti-anxiety medication in the year their first child is born ("no postpartum mental health prescription").

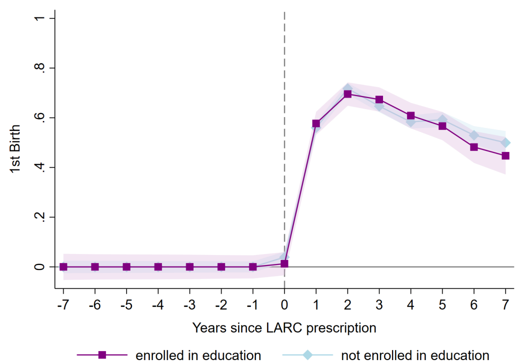
Figure 6: Matching Analysis: Heterogeneity by Age and Enrollment



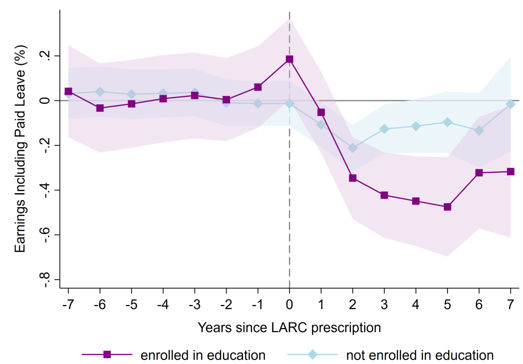
(a) First Childbirth: by Age



(b) Earnings incl. paid leave: by Age



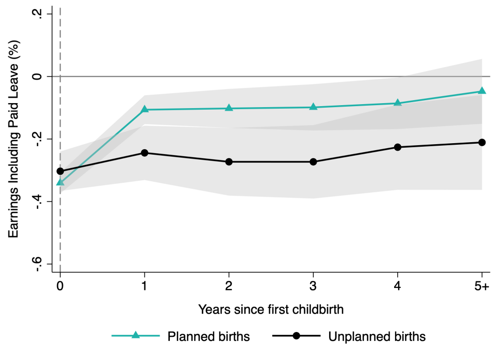
(c) First Childbirth: by Education Enrollment



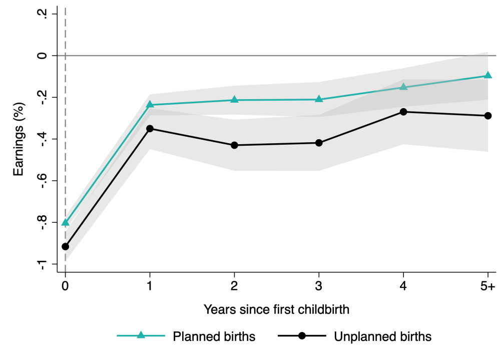
(d) Earnings incl. paid leave: by Education Enrollment

Note: This figure displays the impact of an unplanned pregnancy by time since LARC by age in panel (a) and (b) and by enrollment in education the year before prescription in panel (c) and (d). Panels (a) and (c) show the dynamic effects and 95% confidence intervals for giving birth to a child (i.e., the “first stage”), and figures (b) and (d) show earnings including paid leave as a percentage of control group earnings. The vertical dashed line marks the year of LARC prescription ($t = 0$). Controls include a saturated model with indicators for year and age at LARC prescription. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

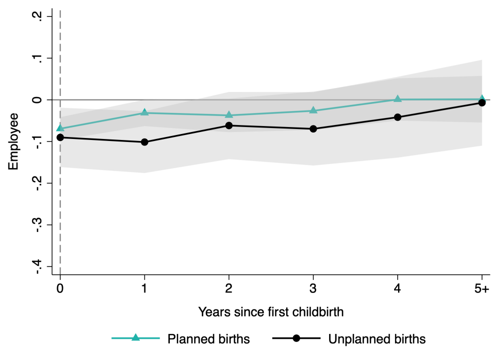
Figure 7: LATE Analysis: Dynamic Effects of Unplanned Births Compared to Planned (Labor Market Outcomes)



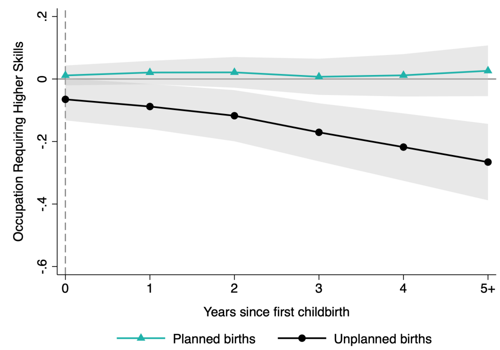
(a) Earnings including paid leave (%)



(b) Earnings (%)



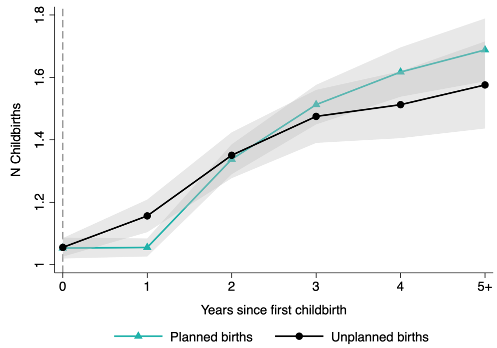
(c) Employed in the last week of November



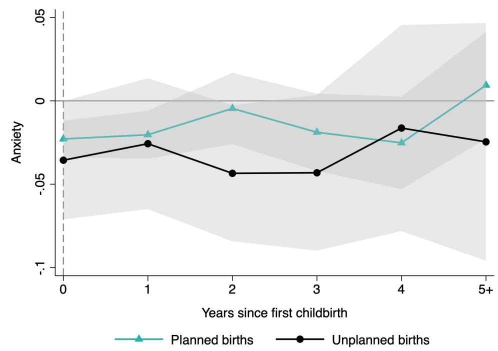
(d) Occupation with Higher Skill Requirements

Note: This figure displays the impact of first child and 95% confidence interval (y-axis) by time since birth (x-axis). Two estimates of the impact of first child are displayed: the effects estimated in a subsample of women who wanted and planned for children (women undergoing IVF), in teal; the effects estimated in a subsample of women who wanted to delay children (LARC users), in black. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

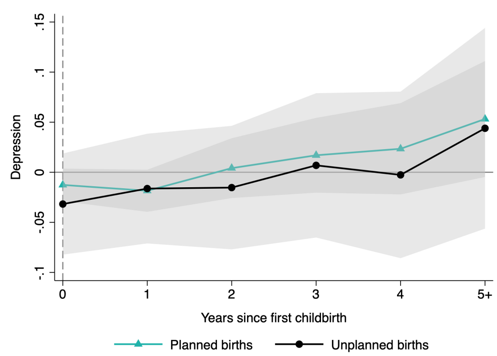
Figure 8: LATE Analysis: Dynamic Effects of Unplanned Births Compared to Planned (Family and Mental Health Outcomes)



(a) Number of Childbirths



(b) Anti-Anxiety Prescription



(c) Anti-Depression Prescription

Note: This figure displays the impact of first child and 95% confidence interval (y-axis) by time since birth (x-axis). Two estimates of the impact of first child are displayed: the effects estimated in a subsample of women who wanted and planned for children (women undergoing IVF), in teal; the effects estimated in a subsample of women who wanted to delay children (LARC users), in black. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

Tables

Table 1: Summary Statistics: LARC, SARC, and IVF Samples Compared to All Women

		LARC		IVF	SARC
	All	Nulliparous	1 Child	Nulliparous	Nulliparous
Age	31.209	30.263	33.261	32.253	28.499
Earnings (1000s)	189.665	157.549	189.119	256.431	187.255
Earnings Including Paid Leave (1000s)	193.831	164.638	214.002	260.169	190.727
Monthly Wage, FT-equivalent (1000s)	24.483	23.076	24.577	25.590	22.861
Fraction of Full-Time Employment	88.875	83.471	87.232	92.082	85.794
Employed	0.707	0.662	0.787	0.860	0.742
Occupation Requiring Higher Skills	0.364	0.247	0.378	0.508	0.353
Anti-Depression Prescription	0.050	0.080	0.054	0.035	0.046
Anti-Anxiety Prescription	0.095	0.153	0.113	0.059	0.094
Enrolled in Education	0.193	0.262	0.127	0.161	0.288
High School	0.323	0.417	0.448	0.313	0.344
College Degree or Higher	0.481	0.347	0.374	0.573	0.487
Married	0.199	0.183	0.386	0.476	0.092
Divorced	0.045	0.060	0.089	0.034	0.029
Observations	398,954	21,744	32,446	8,691	222,206

Note: This table displays average characteristics of all nulliparous women in 2008, nulliparous LARC users, LARC users with one child when they take out their LARC prescription, nulliparous women who undergo IVF in the year before they start the treatment ($t = -1$), and nulliparous SARC users in the year they take out their SARC prescription. We focus on 2008 for the overall population because this is the year before midpoint of our prescription data. For LARC and SARC users, we randomly select a focal prescription year by choosing a random prescription between 2005 and 2012 if there is more than one. Yearly earnings are measured in thousands of 2010 SEK and do not include any leave payments, whereas our second income measure includes all sickness and leave payments related to pregnancy and caring for children. Employment status is measured in the last week of November of a given year. Work hours are measured as a percent of a full-time equivalent worker. Wages and work hours are measured for the $\approx 50\%$ sub-sample (undersampling smaller firms) observed in the wage statistics, which means that the last four columns have 8132, 14146, 4910, and 97104 observations, respectively, for these two variables.

Table 2: Balance: LARC and IVF Setting

	LARC: Nulliparous Women			IVF: Nulliparous Women		
	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff	First IVF Successful	First IVF Unsuccessful	p-value diff
Earnings (1000s)	151.568	151.229	0.952	259.159	255.471	0.002
Earnings Including Paid Leave (1000s)	162.084	157.561	0.535	263.012	259.169	0.002
Monthly Wage, FT-equivalent (1000s)	21.531	22.098	0.908	25.132	25.764	0.174
Fraction of Full-Time Employment	82.827	82.236	0.677	91.926	92.084	0.766
Observed in Wage Statistics	0.365	0.365	0.980	0.607	0.550	0.000
Employed	0.644	0.658	0.592	0.880	0.853	0.001
Occupations Requiring Higher Skills	0.221	0.236	0.541	0.526	0.502	0.000
Anti-Depression Prescription	0.147	0.145	0.932	0.051	0.062	0.140
Anti-Anxiety Prescription	0.087	0.076	0.523	0.030	0.037	0.211
Enrolled in Education	0.232	0.287	0.014	0.162	0.160	0.974
High School	0.435	0.409	0.313	0.306	0.315	0.011
College Degree or Higher	0.318	0.358	0.100	0.587	0.568	0.000
Married	0.224	0.132	0.000	0.487	0.472	0.625
Divorced	0.068	0.044	0.047	0.023	0.038	0.040
Observations	340	21,404		2,263	6,428	

Note: This table presents average labor market, education, and civil status variables for the "treated" and "untreated" in the year before fertility intentions are measured ($t = -1$): LARC purchase for the first three columns and IVF treatment for the last three columns, as well as the p-value of the difference. The untreated are re-weighted to have the same mean age and year of prescription as the treated. Yearly earnings are measured in thousands of 2010 SEK. Employment status is measured in the last week of November of a given year. Work hours are measured as a percent of a full-time equivalent worker. Wages and work hours are measured for the $\approx 50\%$ sub-sample (undersampling smaller firms) observed in the wage statistics. Note that a small number of women are not observed in the year before LARC purchase; for example, if they are out of the country, and these women are not included in these calculations. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

Table 3: Long-Term Effect Heterogeneity: LARC/Unplanned

			Age				Enrolled in Education			
	All Unplanned		27 and Younger		28 and Older		Yes		No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Earnings Inc. Paid Leave (Years 1-6)	-60.224	-60.275	-79.082	-76.316	-37.899	-32.245	-97.122	-92.494	-47.081	-40.770
	(11.997)	(9.557)	(14.317)	(11.713)	(20.288)	(15.723)	(17.730)	(16.849)	(15.059)	(10.969)
...As % of Average Counterfactual Earnings	-0.254	-0.254	-0.332	-0.321	-0.159	-0.136	-0.385	-0.368	-0.205	-0.178
	(0.051)	(0.040)	(0.060)	(0.049)	(0.085)	(0.067)	(0.070)	(0.067)	(0.066)	(0.048)
Aggregate Years Employed	-0.371	-0.181	-0.621	-0.610	-0.153	0.255	-0.574	-0.504	-0.321	-0.170
	(0.187)	(0.156)	(0.244)	(0.204)	(0.302)	(0.274)	(0.311)	(0.279)	(0.230)	(0.180)
Aggregate Years in Occupations Requiring Higher Skills	-0.925	-0.827	-1.291	-1.233	-0.428	-0.324	-1.755	-1.790	-0.479	-0.340
	(0.226)	(0.184)	(0.275)	(0.243)	(0.392)	(0.287)	(0.437)	(0.408)	(0.265)	(0.196)
Aggregate Years with Anti-Anxiety Prescription	-0.189	-0.283	-0.149	0.026	-0.278	-0.487	-0.186	-0.336	-0.222	-0.269
	(0.089)	(0.150)	(0.104)	(0.237)	(0.166)	(0.201)	(0.150)	(0.097)	(0.107)	(0.193)
Aggregate Years with Anti-Depression Prescription	-0.015	-0.420	0.026	-0.379	-0.048	-0.754	-0.085	-0.407	-0.090	-0.551
	(0.158)	(0.202)	(0.190)	(0.228)	(0.280)	(0.332)	(0.291)	(0.278)	(0.184)	(0.252)
Aggregate Number of Children	1.576		1.565		1.570		1.826		1.487	
	(0.071)		(0.086)		(0.131)		(0.126)		(0.083)	
Observations	30,846	30,293	9,607	9,505	21,239	20,788	6,983	6,983	22,972	22,972
Controls	N	Y	N	Y	N	Y	N	Y	N	Y

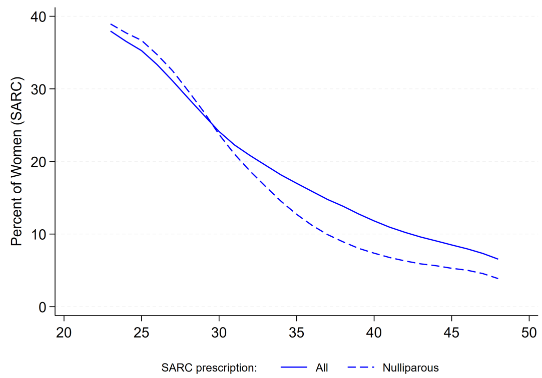
Note: This table presents IV-GMM estimates of the dynamic effects of unplanned birth. Odd columns display estimates without additional controls, while even columns add a control for average pre-treatment outcome in the three years before the LARC prescription to increase precision. Average earnings including parental leave refers to the estimates of ρ_τ for $\tau = 1, ..., 6$ where we impose that these are constant across years 1 through 6 in the estimation. Counterfactual earnings including parental leave are given by the predicted earnings among omen initially assigned to the control group when all child age indicators are set to zero. Years employed, years in occupations requiring higher skills, and years with anti-anxiety/depression prescriptions cumulate the year-by-year estimates of ρ_τ for all years after birth. Observation counts reflect the number of observations in $t = 1$ period including both the LARC and IVF sample. Observation counts are lower for columns (7)-(10) relative to the rest of the table because enrollment is missing for some observations and (unlike age) cannot be perfectly imputed when missing. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

Appendix for:

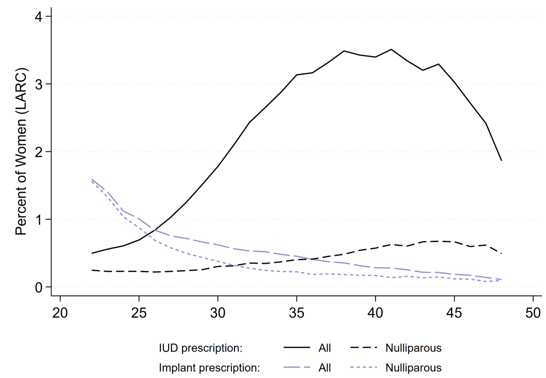
The Labor Market Returns to Delaying Pregnancy

A Appendix Figures and Tables

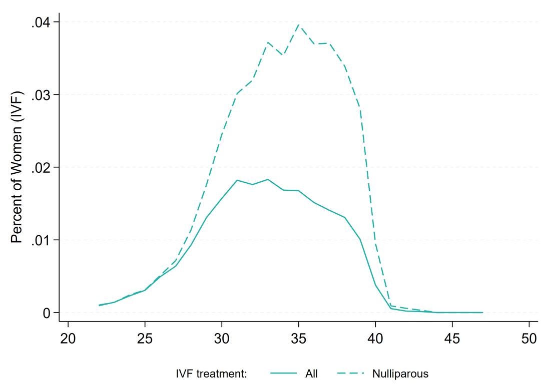
Figure A1: Contraceptive Prescriptions, Fertility Treatments, and Parity by Age



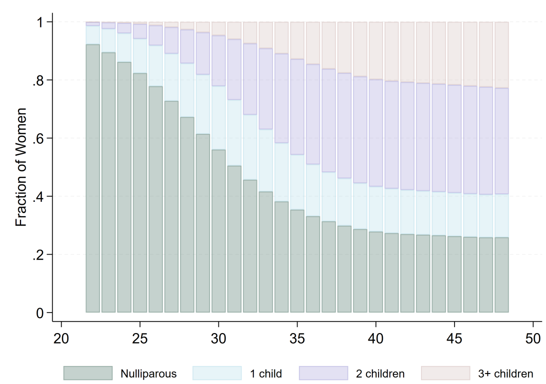
(a) SARC Prescriptions



(b) LARC Prescriptions



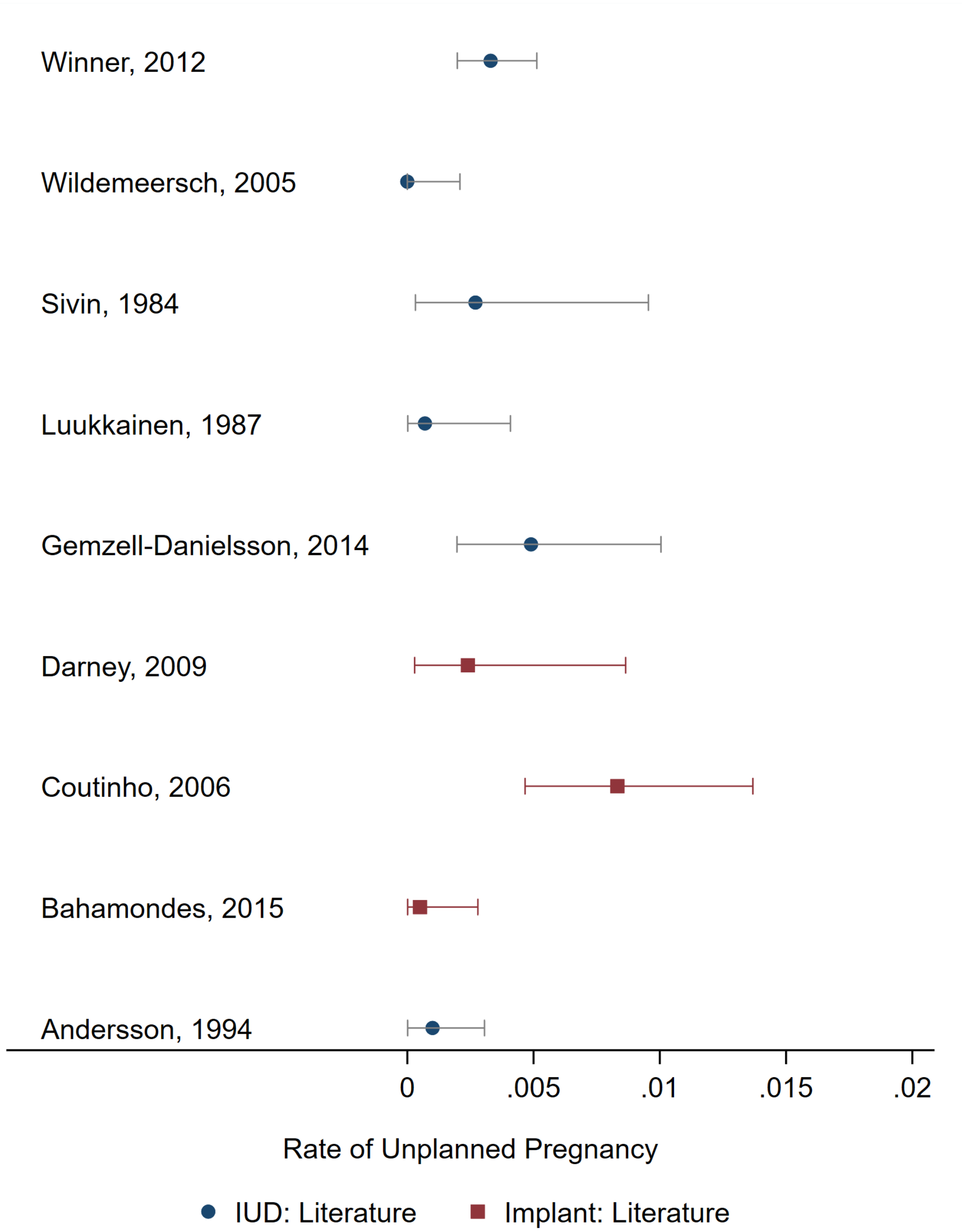
(c) IVF Treatments



(d) Parity Distribution

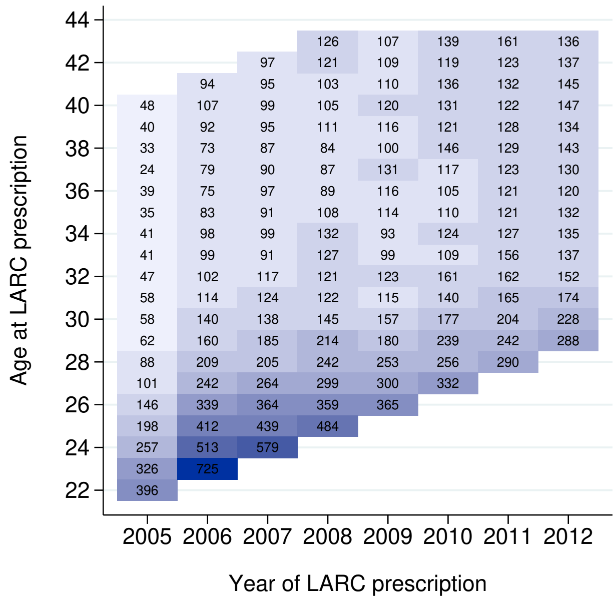
Note: This figure describes contraceptive prescriptions, fertility treatments, and parity distribution by age. Panels (a), (b), and (c) show the percent of women who receive at least one: (a) SARC prescription (oral birth-control pill, ring, etc.), (b) LARC prescription (IUD or birth-control implant), and (c) IVF treatment at each age. In panels (a) and (c), solid lines show the percent of all women and dashed lines show the percent of nulliparous women only. In panel (b), IUD prescriptions follow this same convention, while implant prescriptions use long-dashed lines for all women and short-dashed lines for nulliparous women. Note that the percent of women in panel (b) refers to the age of receiving a LARC, which typically lasts 3-5 years after insertion; thus, utilization at each age is likely around four times these prescription numbers. Panel (d) shows the parity distribution at each age: the fraction of women who are nulliparous (i.e., have not had any childbirth) or have had 1, 2, or 3+ children. Sample: Prescriptions taken out between 2005 and 2013 for women born in 1965-83.

Figure A2: LARC Failure Rates

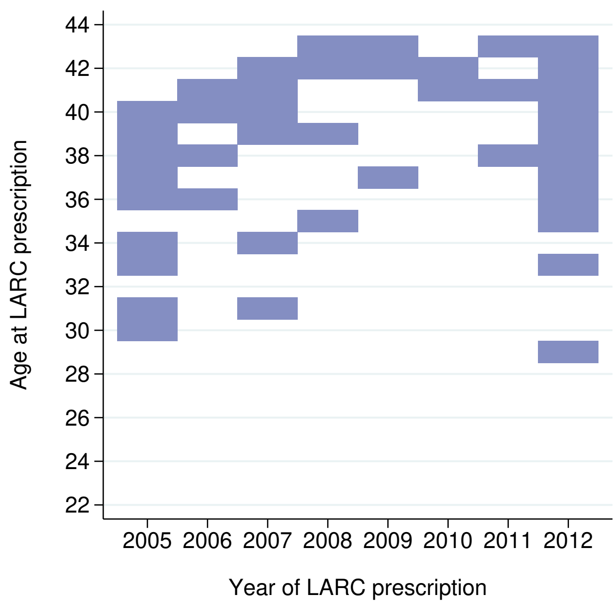


Note: This figure displays estimates of IUD and Implant failure rates from clinical trials, annualized to 1 year.

Figure A3: Age×Year Cells in the LARC Matching Analysis



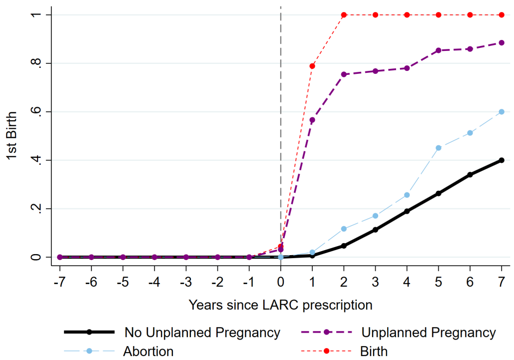
(a) N nulliparous LARC women



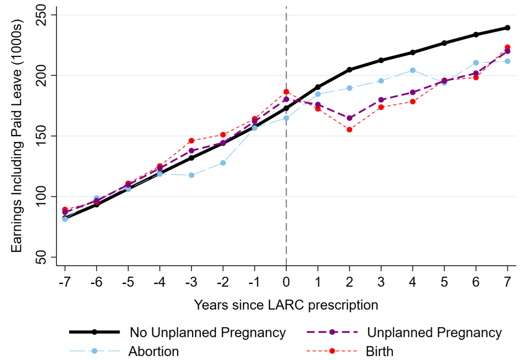
(b) Age×year cells with no unplanned pregnancy

Note: The top panel (a) of this figure shows the number of women in each of the 142 age×year cells, while the bottom panel (b) marks the 42 cells in which we do not observe any unplanned pregnancy within 9-months of LARC purchase. That is, we only observe women assigned to the "control" group and no "treated" women in these cells. Note that most of the cells with no unplanned pregnancy are for women age 40 or older (43%) and in the two endpoint years: 21% in 2005 for which we only have half a year of data and 26% in 2012 when only women who fill LARC prescriptions in the first three months can be "treated" as we only observe births through the end of 2012. Sample: Nulliparous women born in 1965-83 who at age 43 or younger fill a LARC prescription between July 2005 and 2012.

Figure A4: Conditional Means for Unplanned Pregnancy (LARC)
Separating Abortion and Birth



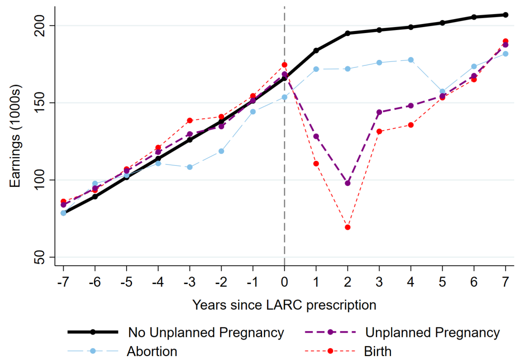
(a) Cond. Means: First Child



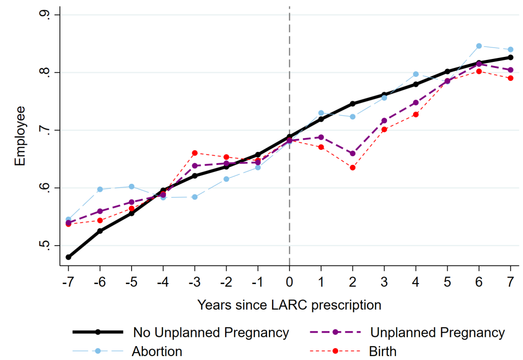
(b) Cond. Means: Earnings including paid leave

Note: This figure displays means of (a) indicator for whether a woman has a child, (b) earnings including paid leave (in 1000 SEK). The vertical dashed line marks the year of LARC prescription ($t = 0$). Means are shown separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Conditional means are also shown separately for the "treatment group compliers" who give birth to their first child following the unplanned pregnancy (red dotted line) and the "treatment group non-compliers" who have an abortion following the unplanned pregnancy (light blue long dashed line). Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

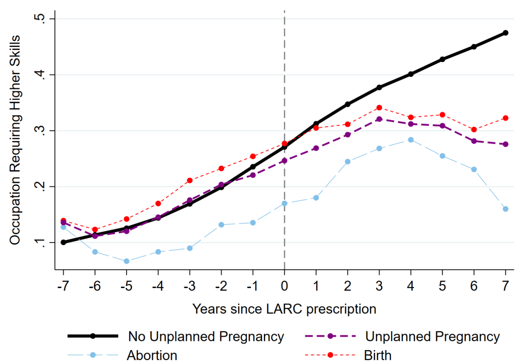
Figure A5: Conditional Means for Unplanned Pregnancy (LARC)
Separating Abortion and Birth (cont.)



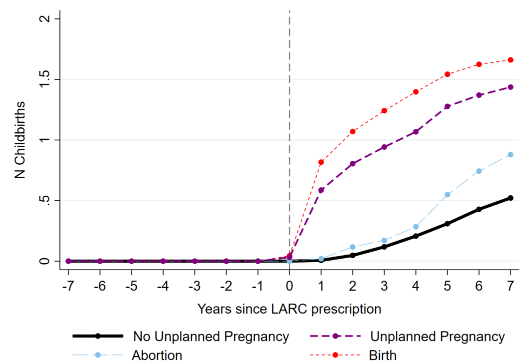
(a) Earnings



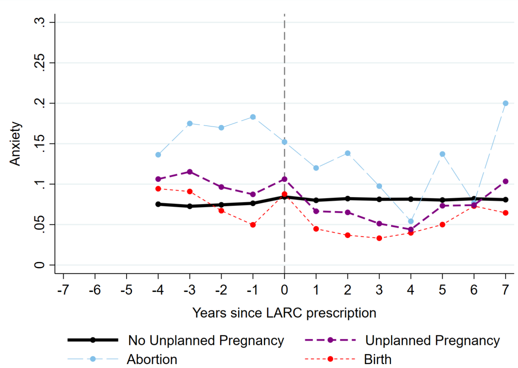
(b) Cond. Means: Employed last week of November



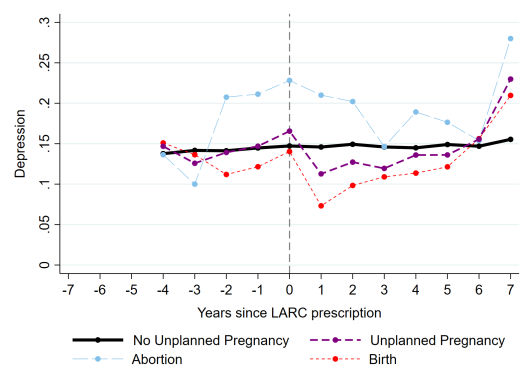
(c) Cond. Means: Occupation with Higher Skill Req.



(d) Cond. Means: Number of Childbirths



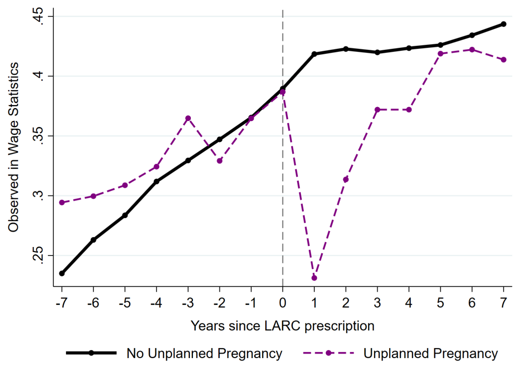
(e) Cond. Means: Anti-Anxiety Prescription



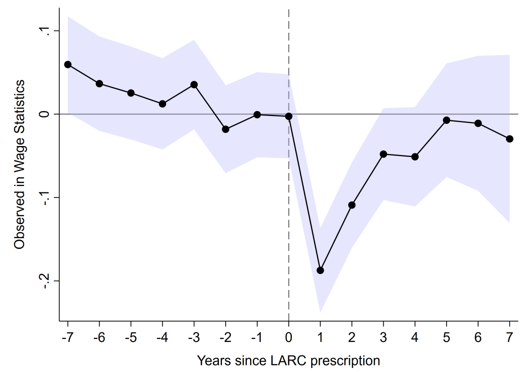
(f) Cond. Means: Anti-Depression Prescription

Note: This figure displays means of (a) earnings in 1000 SEK, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. The vertical dashed line marks the year of LARC prescription ($t = 0$). Prescriptions for Anti-Anxiety or Anti-Depression medication are indicators for whether an individual filled any such prescription in the year. Means are shown separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Conditional means are also shown separately for the "treatment group compliers" who give birth to their first child following the unplanned pregnancy (red dotted line) and the "treatment group non-compliers" who have an abortion following the unplanned pregnancy (light blue long dashed line). Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

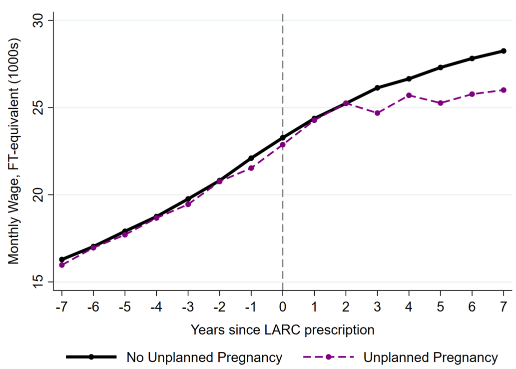
Figure A6: Matching Analysis: Dynamics Effects of Unplanned Pregnancy (LARC) on Wages, Hours, and Presence in Wage Data.



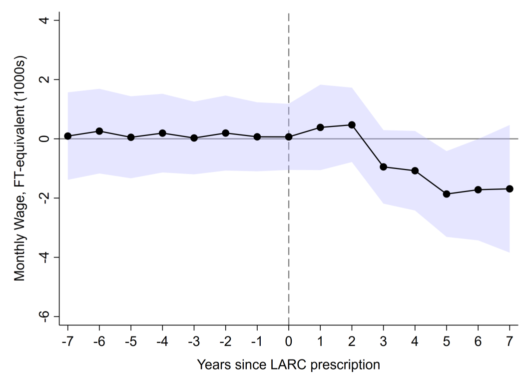
(a) In wage data



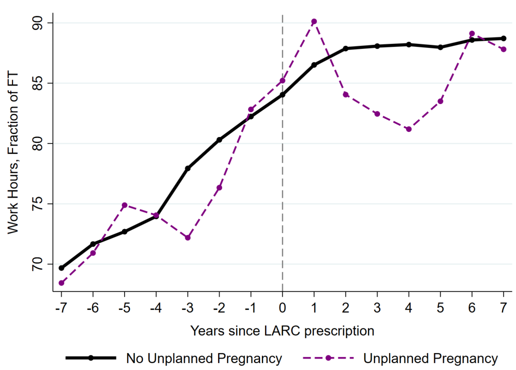
(b) In wage data



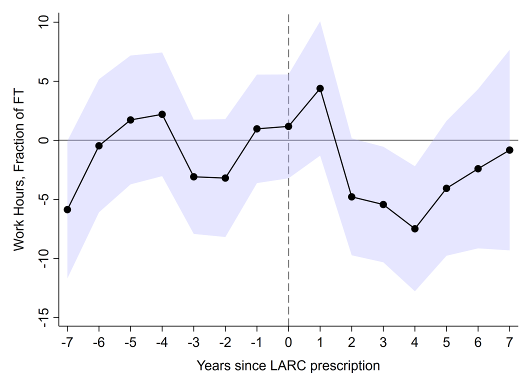
(c) Wages



(d) Wages



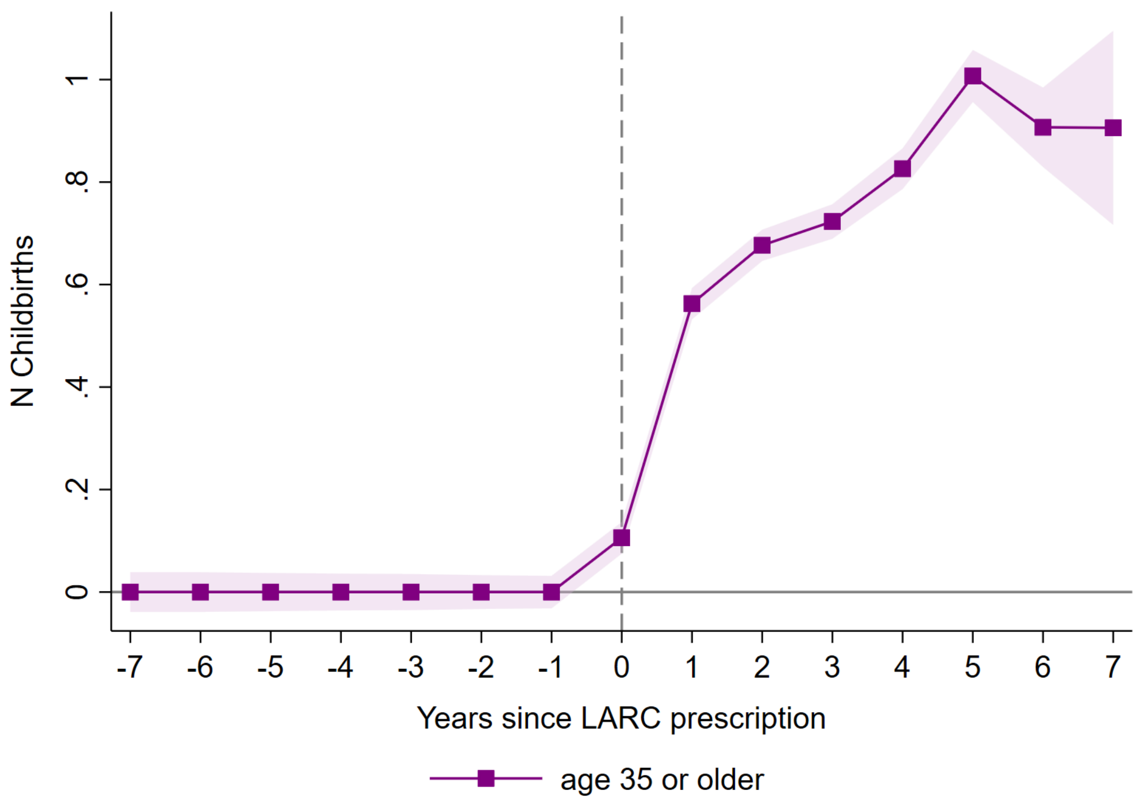
(e) Hours



(f) Hours

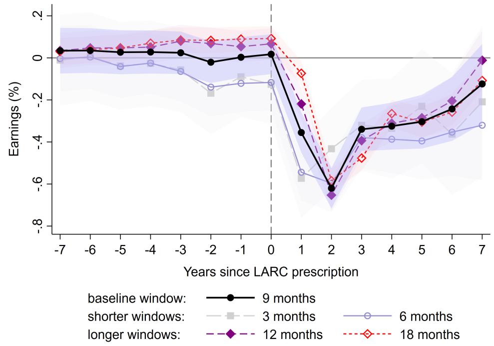
Note: This figure displays the impact of unplanned pregnancy on first childbirth and earnings including paid leave (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Wages and work hours are measured for the $\approx 50\%$ sub-sample (undersampling smaller firms) observed in the wage statistics. "In wage data" is an indicator if the individual was in the survey that year where individuals on leave are less likely to be surveyed in a given year. The monthly wage is measured thousands real 2010 SEK scaled to a full-time equivalent position. Work hours are measured as a percent of a full-time equivalent worker. See Appendix Section C.5 for more information about the survey data. Panels (a), (b), and (c) show conditional means separately for the "control group" of women who do not conceive within nine months of LARC prescription (black solid line) and the "treatment group" of women who conceive within nine months of LARC prescription (purple dashed line). Panels (b), (d), and (f) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

Figure A7: Number of Childbirths Among Women 35 and Older

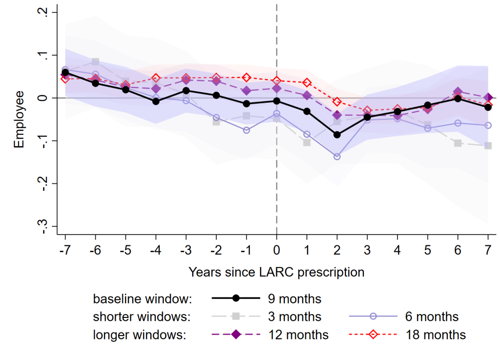


Note: This figure displays the impact of unplanned pregnancy on number of childbirths (y-axis) by time since LARC prescription (x-axis) among women age 35 and older at the time of the LARC prescription. The vertical dashed line marks the year of LARC prescription (t = 0). These figures show how the dynamic effects of unplanned pregnancy and 95% confidence intervals. Sample: Women born in 1965-83.

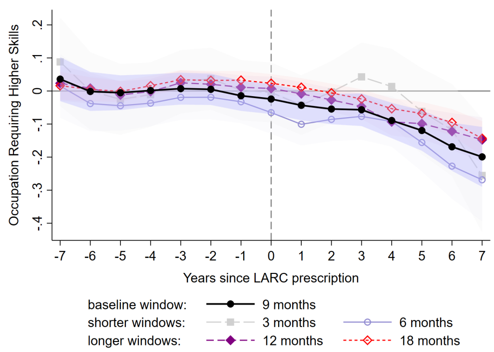
Figure A8: Matching Analysis: Robustness to Unplanned Pregnancy Window



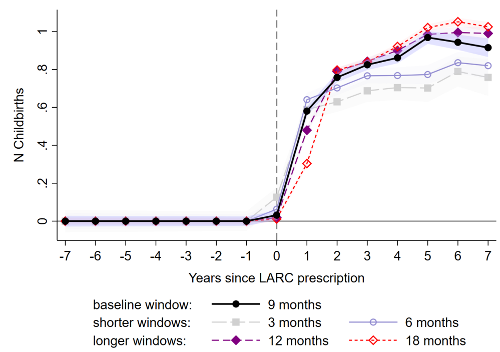
(a) Earnings (%)



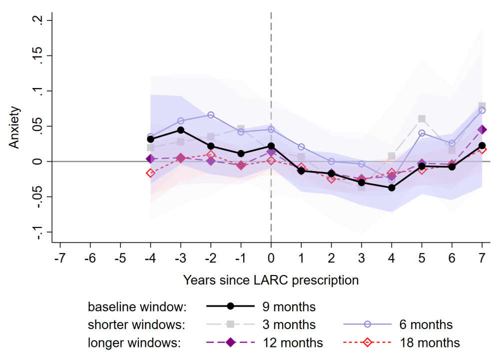
(b) Employed in the last week of November



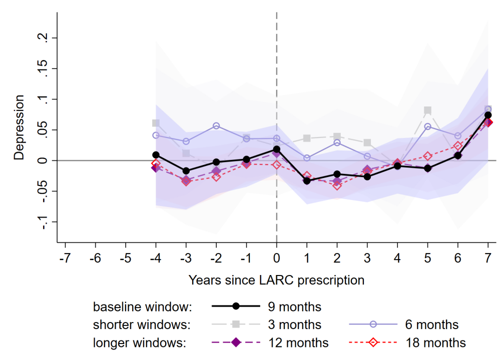
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



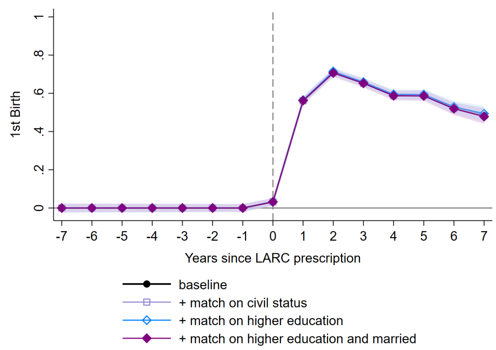
(e) Anti-Anxiety Prescription



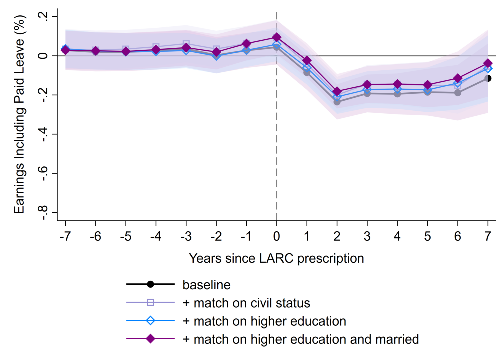
(f) Anti-Depression Prescription

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis) for (a) earnings in percentage terms, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. The vertical dashed line marks the year of LARC prescription ($t = 0$). Controls include a saturated model with indicators for year of LARC prescription and age. Treatment group: Women who conceive the first child within 3, 6, 9, 12, or 18 months of LARC prescription, respectively. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

Figure A9: Matching Analysis: Robustness to Matching on Civil Status and Education



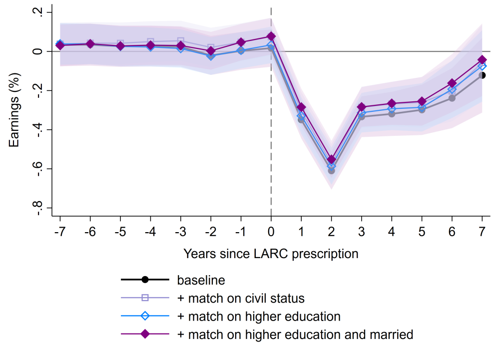
(a) First Childbirth



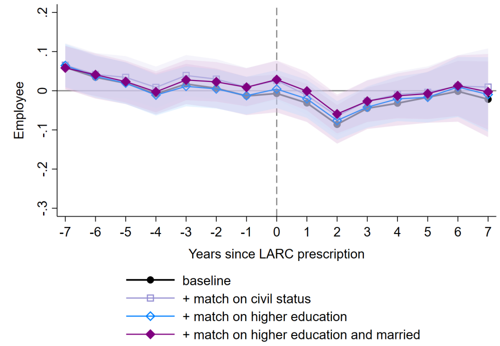
(b) Earnings incl. paid leave

Note: This figure displays the impact of unplanned pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis) on (a) the probability of first childbirth and (b) earnings including paid leave as a percentage of the control group mean. The vertical dashed line marks the year of LARC prescription ($t = 0$). Unplanned pregnancies are defined as conception within nine months of LARC prescription. Baseline control variables include a saturated model with indicators for age and year of LARC prescription. We compare the baseline to three specifications that additionally match on civil status and education in the year before LARC prescription: civil status (married and divorced indicators, and years since changing civil status), an indicator for higher education, and this indicator for higher education interacted with marital status. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

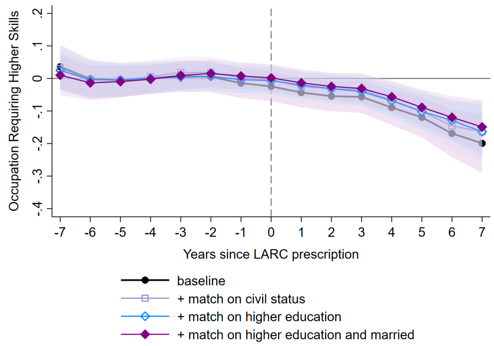
Figure A10: Matching Analysis: Robustness to Matching on Civil Status and Education



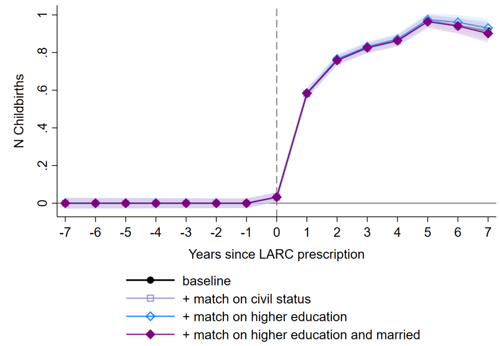
(a) Earnings



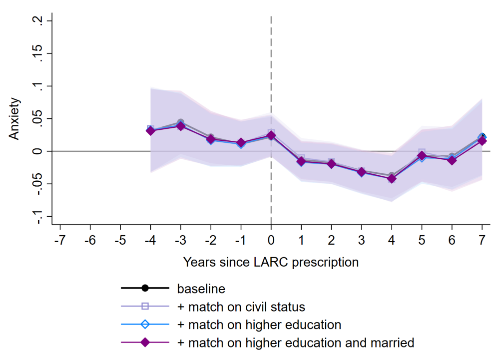
(b) Employed in the last week of November



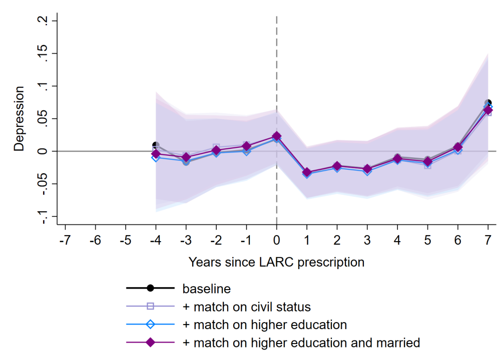
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



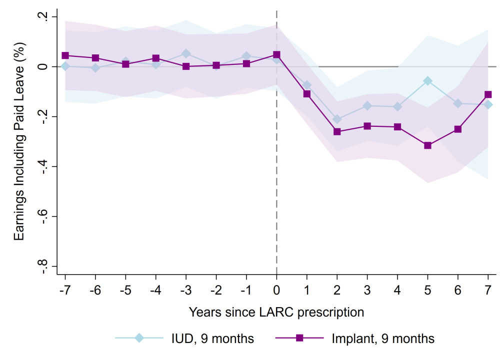
(e) Anti-Anxiety Prescription



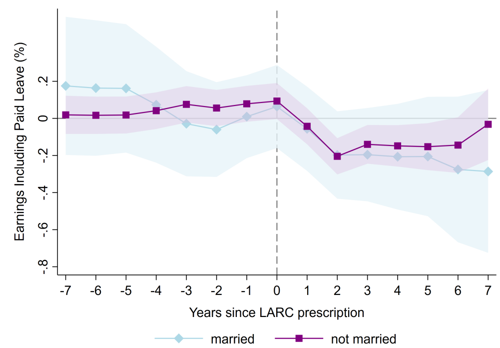
(f) Anti-Depression Prescription

Note: This figure displays the impact of unplanned pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis) for (a) earnings in percentage terms, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. The vertical dashed line marks the year of LARC prescription ($t = 0$). Unplanned pregnancies are defined as conception within nine months of LARC prescription. Baseline control variables include a saturated model with indicators for age and year of LARC prescription. We compare the baseline to three specifications that additionally match on civil status and education in the year before LARC prescription: civil status (married and divorced indicators, and years since changing civil status), an indicator for higher education, and this indicator for higher education interacted with marital status. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

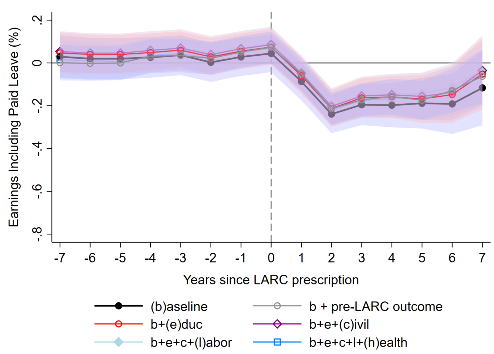
Figure A11: Matching Analysis: Dynamic Effects of Unplanned Pregnancy (LARC): Robustness



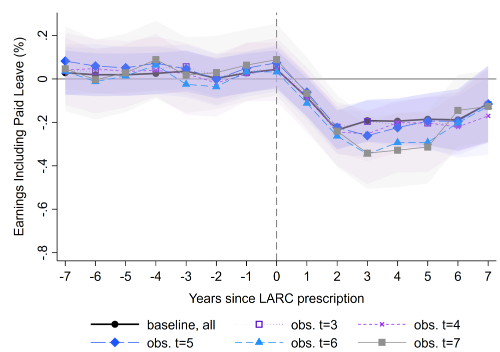
(a) Robustness to Type of LARC



(b) Robustness to Marital Status



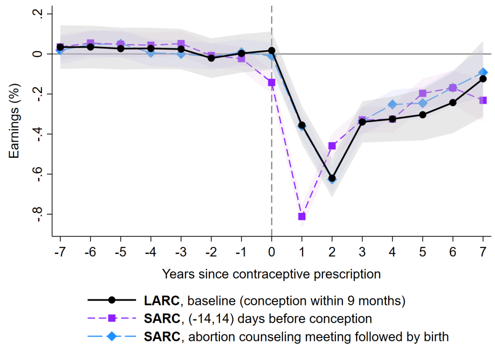
(c) Robustness to Additional Controls



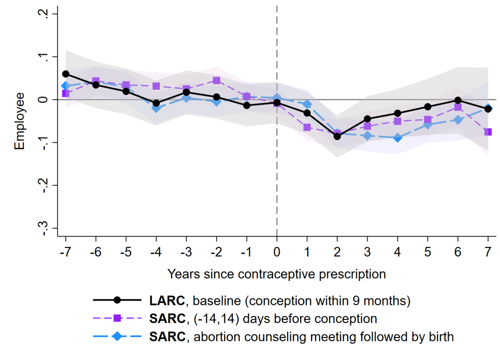
(d) Robustness to Panel Balance

Note: This figure displays the impact of unplanned pregnancy on earnings including paid leave (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). These figures show how the dynamic effects of unplanned pregnancy and 95% confidence intervals vary by (a) when splitting by type of contraception: IUD and implants, (b) for married and unmarried women in the year before contraception, (c) adding additional controls, and (e) the balance of the panel; i.e. whether we only include women observed at $t = s$ for $s = 3, ..., 7$). Baseline control variables include a saturated model with indicators for age and year of LARC prescription. When we include additional controls in panel (c), the first specification adds the average outcome over the three years prior to LARC purchase, and the last four specifications sequentially add controls for: (e)duc, level- and field of highest completed education and an indicator for being enrolled in an education program; (c)ivil, married and divorced indicators, and years since changing civil status; (l)abor, indicator for being employed in the last week of November and being in an occupation with higher skill requirements; and (h)ealth, indicators for any longer- or short-term sick leave payments including injury and rehabilitation payments. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

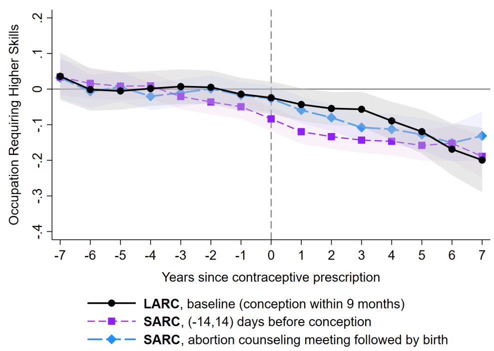
Figure A12: Matching Analysis: Dynamic Effects of Unplanned Pregnancies Using SARC Definitions



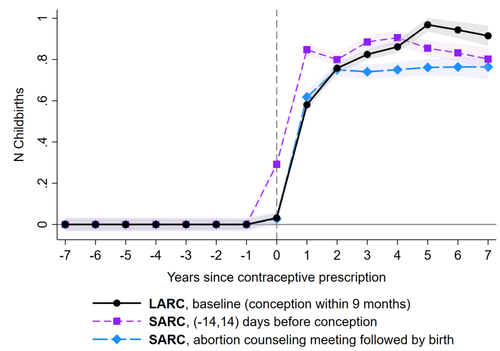
(a) Earnings



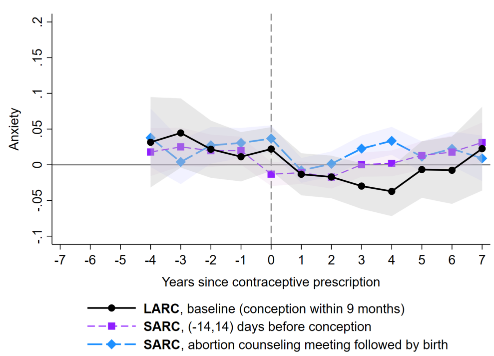
(b) Employed last week of November



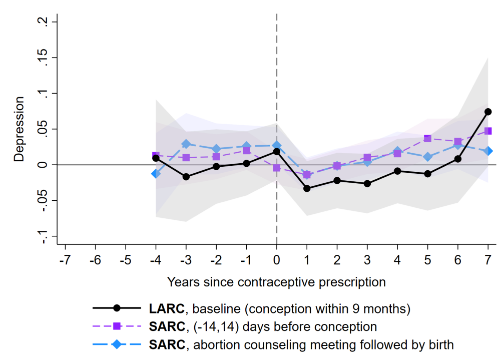
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



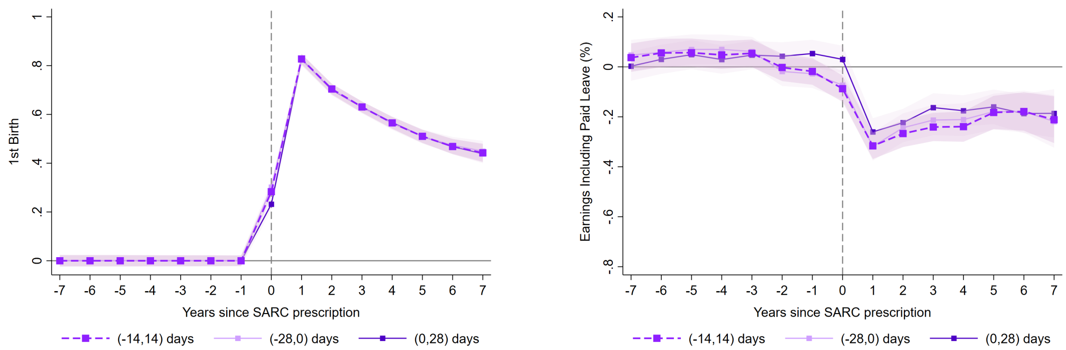
(e) Anti-Anxiety Prescription



(f) Anti-Depression Prescription

Note: This figure displays the impact of unplanned pregnancy on labor market and birth outcomes (y-axis) by time since contraceptive prescription (x-axis). The vertical dashed line marks the year of prescription ($t = 0$). These figures show the dynamic effects and 95% confidence intervals for (a) earnings in percentage terms, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. Baseline control variables include a saturated model with indicators for age and year of prescription. Our baseline definition of unplanned LARC pregnancy is compared to two alternative definitions based on SARCs; see Section 2.3.2 for definitions and Appendix Figures A13 and A15 for comparison of SARC definitions to alternatives. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC or SARC prescription.

Figure A13: Matching Analysis: Robustness to SARC Pregnancy Window

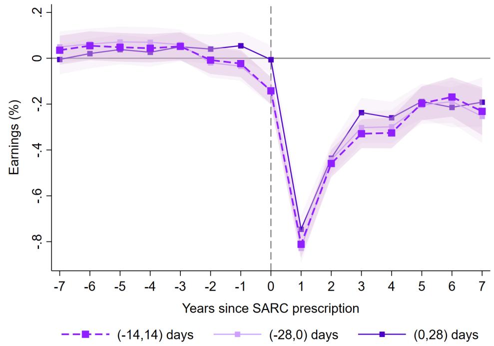


(a) First Childbirth

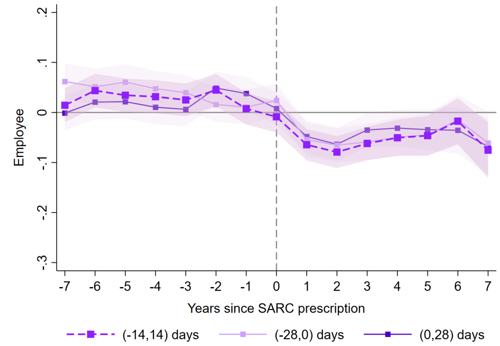
(b) Earnings including paid leave

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis) for (a) probability of first childbirth and (b) earnings including paid leave as a percentage of the control group mean. The vertical dashed line marks the year of SARC prescription ($t = 0$). Controls include a saturated model with indicators for age and year of SARC prescription. Treatment groups for women that have an unplanned pregnancy: (i) Baseline from Figure 4, women who purchase SARC prescription (-14,14) days before conception, (ii) switches the window two weeks later to (-28,0) days before conception, and (iii) switches the window two weeks earlier to (0,28) days before conception. Control groups: Women who purchase a SARC at the same time (age $\times$ year), but do not have a child that is conceived within the given time-window. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of SARC prescription.

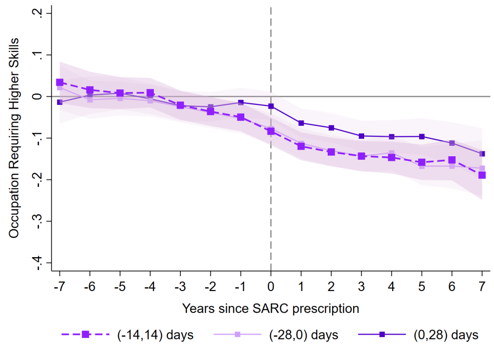
Figure A14: Matching Analysis: Robustness to SARC Pregnancy Window



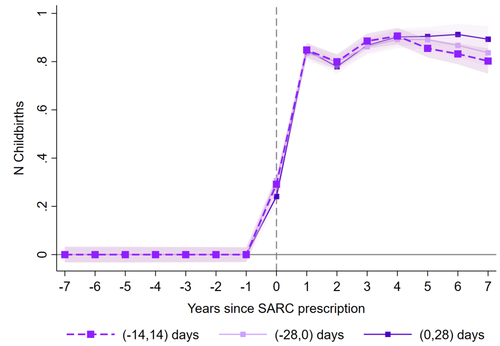
(a) Earnings (%)



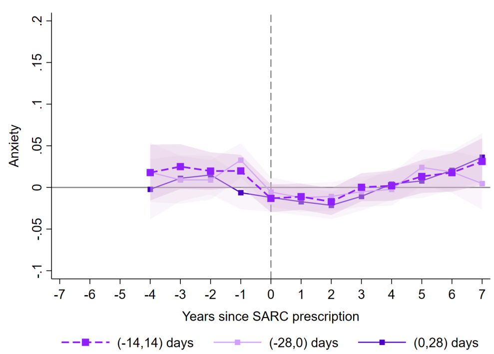
(b) Employed in the last week of November



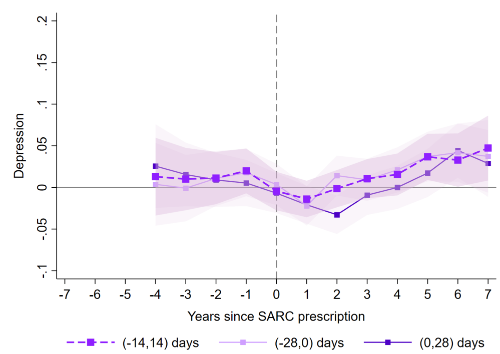
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



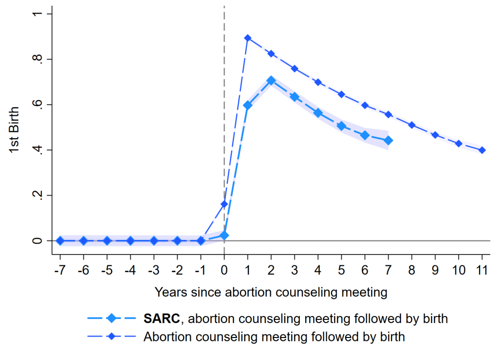
(e) Anti-Anxiety Prescription



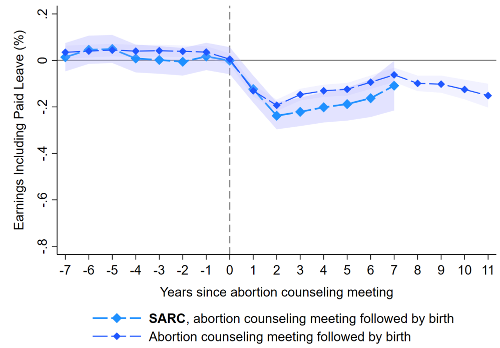
(f) Anti-Depression Prescription

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of SARC prescription ($t = 0$). The figures show the dynamic effects and 95% confidence intervals for (a) earnings in percentage terms, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. Controls include a saturated model with indicators for age and year of SARC prescription. Treatment groups for women that have an unplanned pregnancy: (i) Baseline from Figure 4, women who purchase SARC prescription (-14,14) days before conception, (ii) switches the window two weeks later to (-28,0) days before conception, and (iii) switches the window two weeks earlier to (0,28) days before conception. Control groups: Women who purchase a SARC at the same time (age × year), but do not have a child that is conceived within the given time-window. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of SARC prescription.

Figure A15: Matching Analysis: Robustness to (SARC) Abortion Counseling Definition



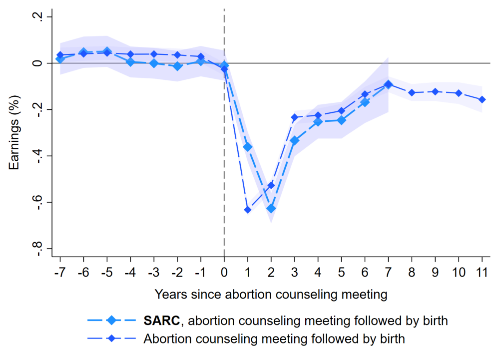
(a) First Childbirth



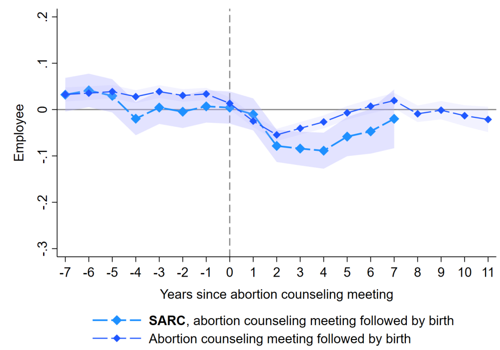
(b) Earnings including paid leave (%)

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since abortion counseling meeting (x-axis) for (a) probability of first childbirth and (b) earnings including paid leave as a percentage of the control group mean. The vertical dashed line marks the year of abortion counseling meeting ($t = 0$). Treatment groups for women that possibly have an unplanned pregnancy: (i) Baseline from Figure 4, women who give birth to their first child after having an abortion counseling meeting and having purchased a SARC (14,365) days before conception, compared to (ii) all women who have an abortion counseling meeting followed by first childbirth within the term (i.e. without conditioning on having an active SARC prescription). Control groups: all other nulliparous women of the same age, and in the SARC sample, who additionally purchased a SARC at the same time (age x year). Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of SARC prescription or abortion counseling meeting.

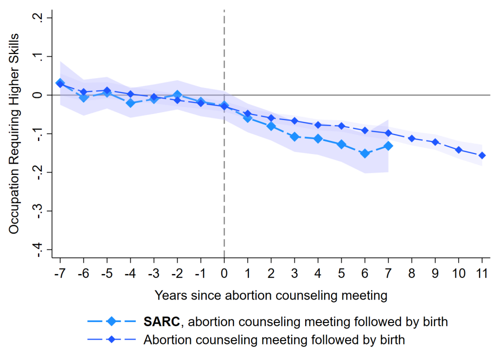
Figure A16: Matching Analysis: Robustness to (SARC) Abortion Counseling Definition



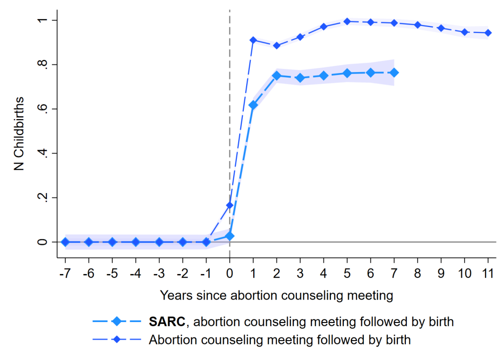
(a) Earnings (%)



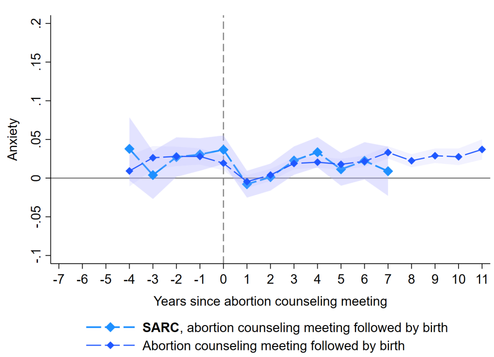
(b) Employed in the last week of November



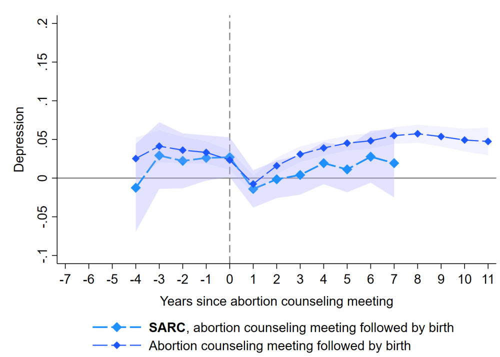
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



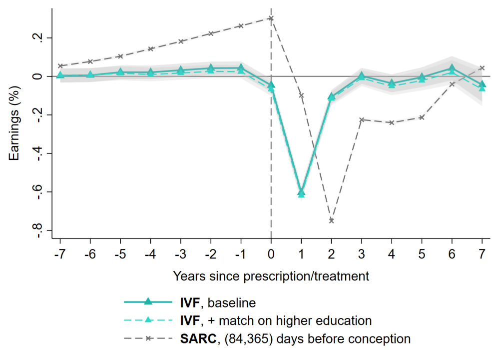
(e) Anti-Anxiety Prescription



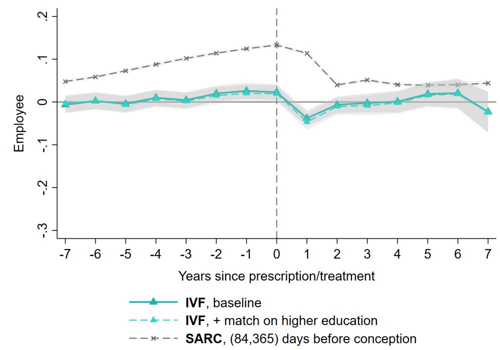
(f) Anti-Depression Prescription

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since abortion counseling meeting (x-axis) for (a) earnings in percentage terms, (b) employment in the last week of November, (c) being in an occupation requiring higher skills, (d) number of children born, (e) taking out a prescription for drugs to treat anxiety, and (f) taking out a prescription for drugs to treat depression. The vertical dashed line marks the year of the abortion couseling meeting ($t = 0$). Treatment groups for women that possibly have an unplanned pregnancy: (i) Baseline from Figure 4, women who give birth to their first child after having an abortion counseling meeting and having purchased a SARC (14,365) days before conception, compared to (ii) all women who have an abortion counseling meeting followed by first childbirth within the term (i.e. without conditioning on having an active SARC prescription). Control groups: all other nulliparous women of the same age, and in the SARC sample, who additionally purchased a SARC at the same time (age x year). Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of SARC prescription or abortion counseling meeting.

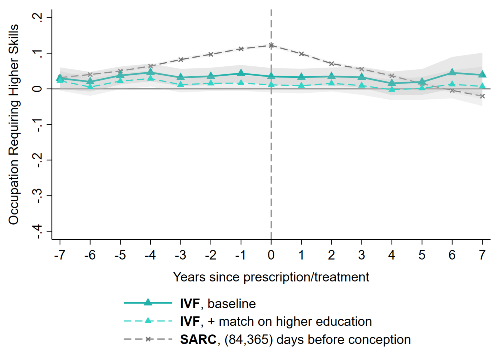
Figure A17: Matching Analysis: Alternative Definitions of Planned Pregnancy



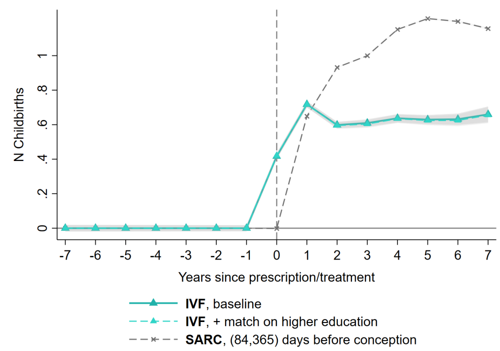
(a) Earnings (%)



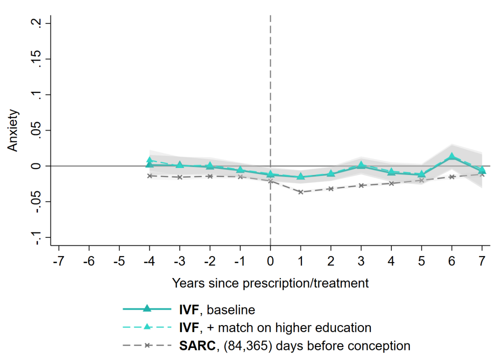
(b) Employed in the last week of November



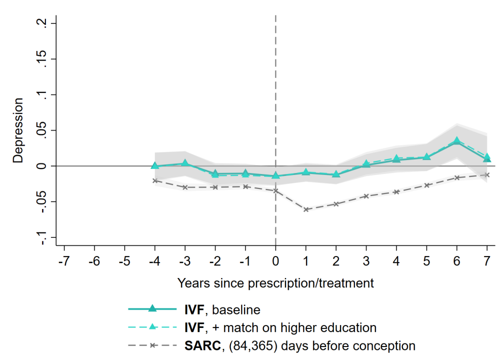
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



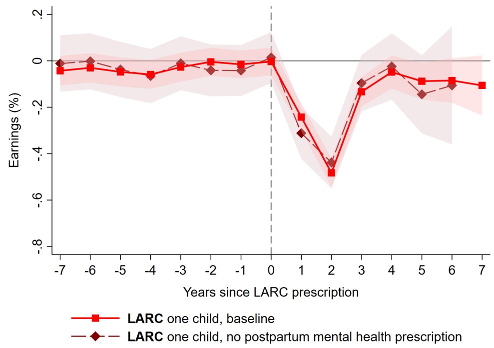
(e) Anti-Anxiety Prescription



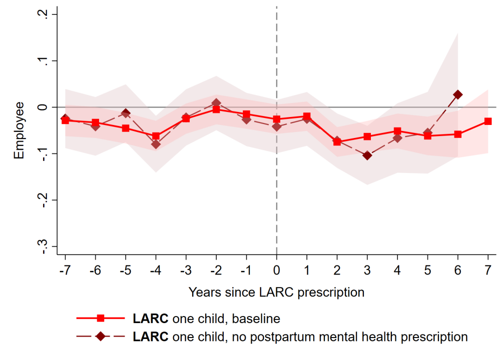
(f) Anti-Depression Prescription

Note: This figure displays the impact of pregnancy on labor market, family, and mental health outcomes (y-axis) by time since SARC prescription or IVF treatment (x-axis). The vertical dashed line marks the year of SARC prescription or IVF treatment ($t = 0$). These figures show the dynamic effects and 95% confidence intervals. Baseline control variables include a saturated model with indicators for age and year of SARC prescription or IVF procedure. The IVF sample additionally interacts age and year of IVF treatment controls with time since last contraceptive and the last specification an additional interaction with an indicator for completing higher education. Sample: Women born in 1965-83 with no prior childbirths at the time of the SARC prescription or IVF treatment.

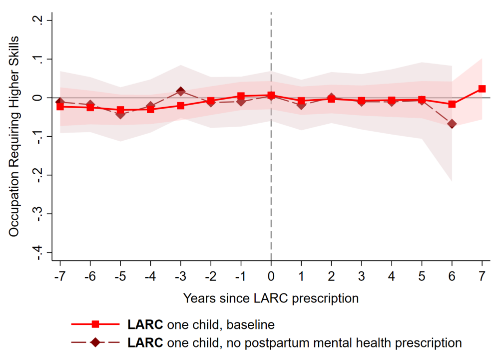
Figure A18: Matching Analysis: 2nd Child for LARC Users



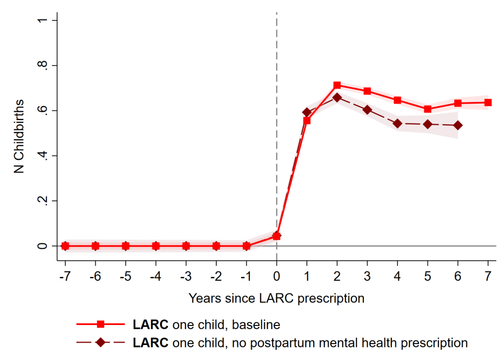
(a) Earnings (%)



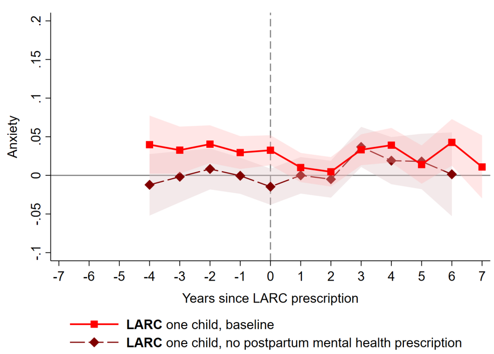
(b) Employed in the last week of November



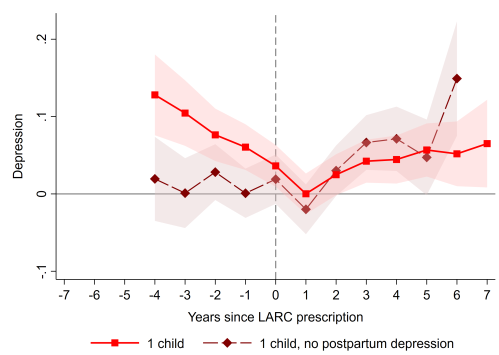
(c) Occupation with Higher Skill Requirements



(d) Number of Childbirths



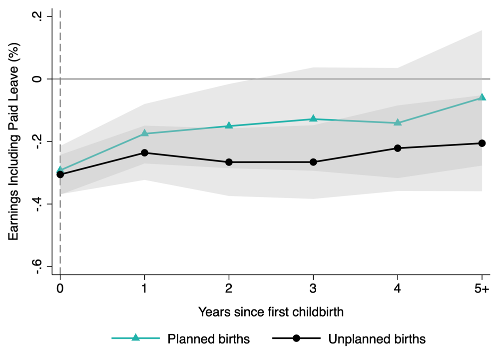
(e) Anti-Anxiety Prescription



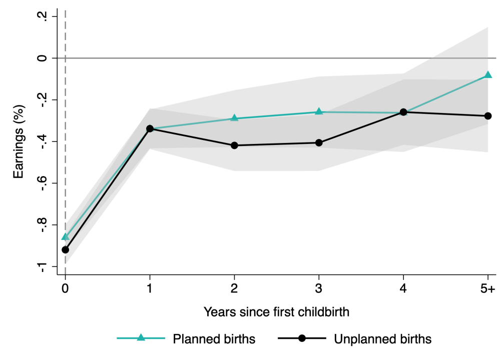
(f) Anti-Depression Prescription

Note: This figure displays the impact of pregnancy on labor market, family, and mental health outcomes (y-axis) by time since LARC (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Baseline control variables include a saturated model with indicators for age, year of LARC prescription, and years since first childbirth. Sample: Women born in 1965-83 with one prior child birth and less than 44 years old at the time of LARC prescription. We also present results for a LARC subsample of those who do not take out a prescription for any anti-depression or anti-anxiety medication in the year their first child is born ("no postpartum mental health prescription").

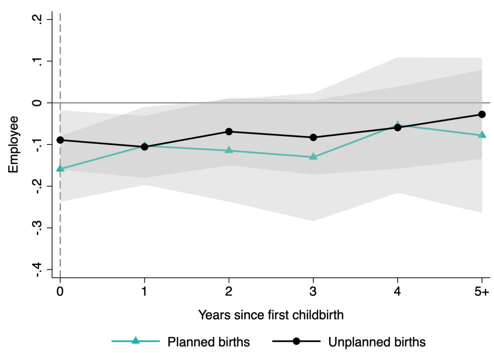
Figure A19: Weighted LATE Analysis: Dynamic Effects of Unplanned Births Compared to Planned (Labor Market Outcomes)



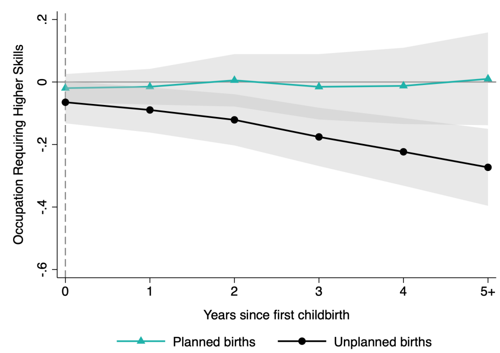
(a) Earnings including paid leave (%)



(b) Earnings (%)



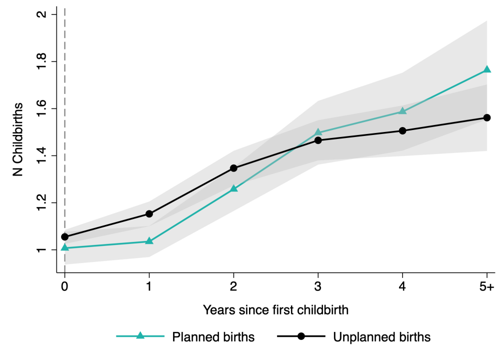
(c) Employed in the last week of November



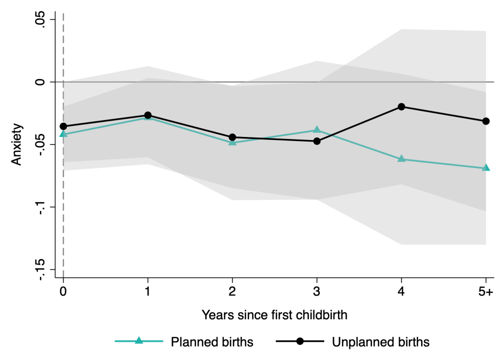
(d) Occupation with Higher Skill Requirements

Note: This figure displays the impact of first child and 95% confidence interval (y-axis) by time since birth (x-axis). Two estimates of the impact of first child are displayed: the effects estimated in a subsample of women who wanted and planned for children (women undergoing IVF), in teal; the effects estimated in a subsample of women who wanted to delay children (LARC users), in black. Compared to Figure 7, these results reweigh women in the IVF sample to match the characteristics of LARC users using income, education level indicators, married, divorced, employment, indicator for occupation requiring higher skills, private sector indicator, and age in the year of the fertility procedure. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

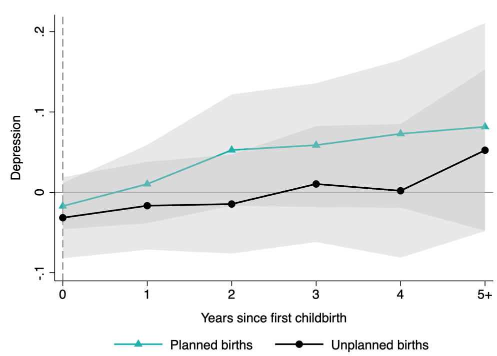
Figure A20: Weighted LATE Analysis: Dynamic Effects of Unplanned Births Compared to Planned (Family and Mental Health Outcomes)



(a) Number of Childbirths



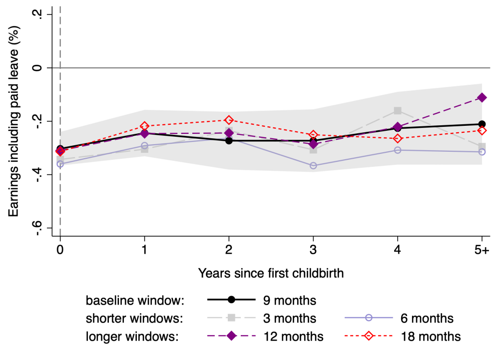
(b) Anti-Anxiety Prescription



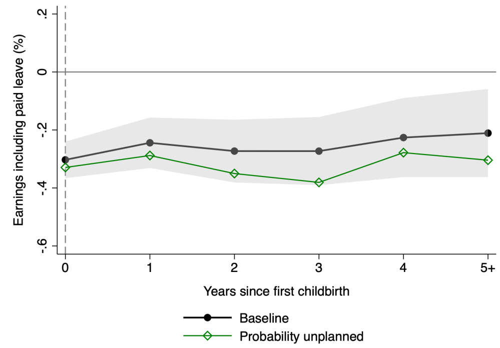
(c) Anti-Depression Prescription

Note: This figure displays the impact of first child and 95% confidence interval (y-axis) by time since birth (x-axis). Two estimates of the impact of first child are displayed: the effects estimated in a subsample of women who wanted and planned for children (women undergoing IVF), in teal; the effects estimated in a subsample of women who wanted to delay children (LARC users), in black. Compared to Figure 8, these results reweigh women in the IVF sample to match the characteristics of LARC users using income, education level indicators, married, divorced, employment, indicator for occupation requiring higher skills, private sector indicator, and age in the year of the fertility procedure. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

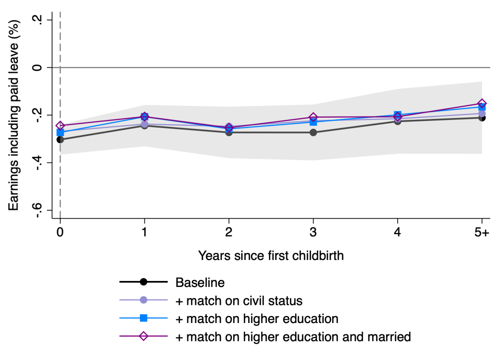
Figure A21: LATE Analysis: Dynamic Effects of Unplanned Birth: Robustness of Unplanned Estimates



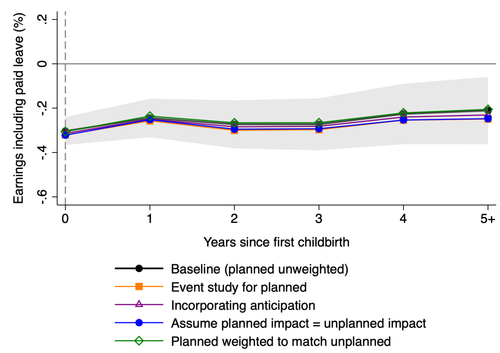
(a) Robustness to LARC window



(b) Robustness to Probability Weights for Unplanned



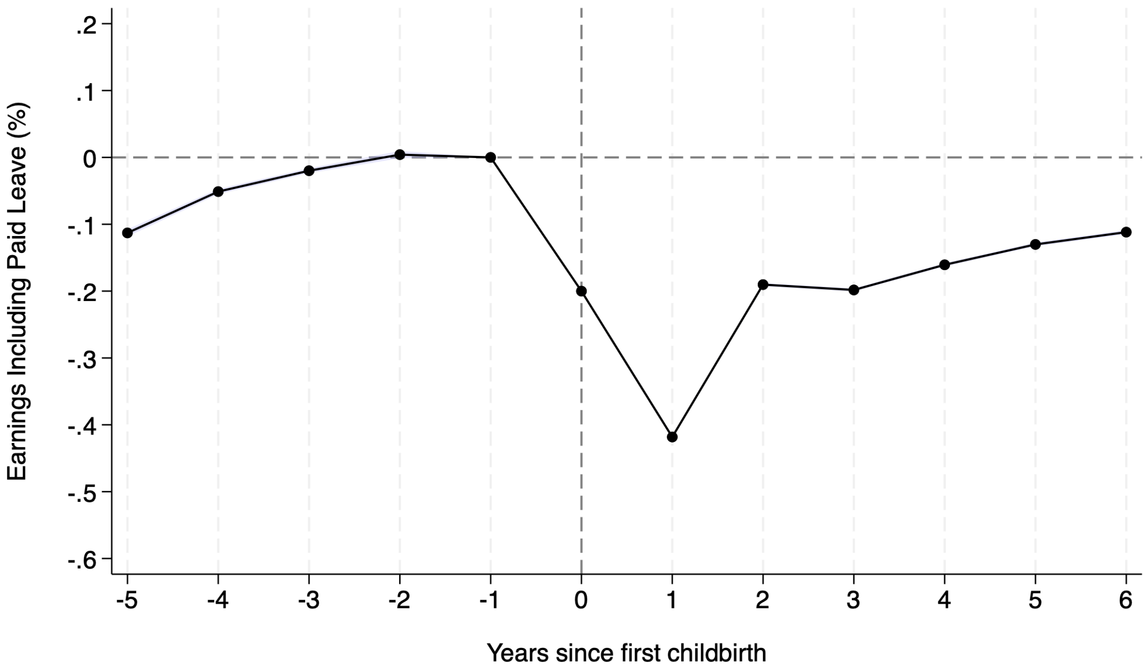
(c) Robustness to Additional Matching



(d) Robustness to Assumptions about Later-treated Effects

Note: These figures show alternative estimates for unplanned births (see Figure 7 for more details). The 95% confidence interval is corresponds to the baseline estimates. These figures show how our impact of birth estimates vary by (a) definition of unplanned pregnancies (different windows), (b) weighing the LARC sample for probability of being unplanned, (c) when matching on additional pre-LARC characteristics, and (d) when making different assumptions about the impact of planned births. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of IVF procedure or LARC prescription.

Figure A22: Event Study Estimates



Note: This figure presents estimates from a regression of earnings including paid leave on indicators of years since birth of first child, age, and year for the years 2005-2012. Effects are measured relative to the counterfactual mean earnings including paid leave with all event time indicators set of zero, as in Kleven et al. (2019a). Sample: Women born in 1965-83.

Table A1: Prenatal Environment, Mother and Child Health at Birth

	All	LARC		IVF			
	1st births 2005-12	LARC before 1st birth	Unplanned birth	IVF before 1st birth	Planned birth	IVF before 1st birth (reweighted)	Planned birth (reweighted)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mother during pregnancy and at 1st childbirth							
age at 1st childbirth	31.09	30.83	30.53	33.84	33.13	30.17	29.67
Prenatal environment							
Child Gestational Age (GA) at 1st prenatal visit (weeks)	11.24	10.29	10.85	10.93	11.21	10.82	10.97
Mother's BMI at 1st prenatal visit	24.23	25.02	25.22	24.40	24.24	24.46	24.33
Mother smoking...							
3 months prior to conception	0.15	0.18	0.26	0.08	0.09	0.14	0.16
at 1st prenatal visit	0.05	0.05	0.09	0.02	0.02	0.04	0.04
at 30-32 weeks of child GA	0.03	0.04	0.06	0.02	0.02	0.03	0.04
Mother snuffing...							
3 months prior to conception	0.03	0.05	0.07	0.02	0.03	0.03	0.04
at 1st prenatal visit	0.01	0.02	0.02	0.01	0.01	0.01	0.01
at 30-32 weeks of child GA	0.00	0.01	0.00	0.00	0.00	0.01	0.00
Mother's health during pregnancy							
any anti-anxiety drug prescriptions	0.03	0.06	0.10	0.03	0.03	0.03	0.02
any anti-depressant prescriptions	0.05	0.11	0.14	0.06	0.06	0.06	0.06
any pain-relieving drug prescriptions	0.13	0.21	0.31	0.22	0.21	0.22	0.22
diagnosed with repeated urinary tract infections	0.14	0.16	0.15	0.13	0.14	0.13	0.14
diagnosed with lung disease/asthma	0.07	0.09	0.08	0.07	0.08	0.06	0.06
Childbirth							
planned c-section	0.06	0.06	0.05	0.09	0.08	0.07	0.06
emergency c-section	0.13	0.16	0.21	0.19	0.17	0.15	0.13
days in hospital	3.52	3.50	3.42	4.27	4.09	3.68	3.59
Child							
any congenital anomalies	0.04	0.04	0.05	0.04	0.05	0.04	0.04
APGAR score 1 minute after birth (0-10)	8.61	8.61	8.43	8.56	8.62	8.62	8.67
APGAR score 5 minutes after birth (0-10)	9.68	9.66	9.59	9.66	9.68	9.70	9.73
birthweight (g)	3,435	3,448	3,407	3,352	3,318	3,354	3,294
Low Birth Weight (LBW) birthweight$<=$2500g	0.05	0.05	0.07	0.09	0.09	0.09	0.10
Very Low Birth Weight (VLBW) birthweight$<=$1500g	0.01	0.01	0.03	0.02	0.02	0.02	0.02
Small for Gestational Age (SGA) birthweight $<$ P10 for GA	0.03	0.03	0.03	0.04	0.04	0.03	0.03
Large for Gestational Age (LGA) birthweight $>$ P90 for GA	0.02	0.03	0.03	0.03	0.02	0.03	0.02
GA when born (weeks)	39.34	39.27	39.08	39.02	38.97	38.97	38.79
premature (GA $<$ 34 weeks)	0.02	0.03	0.05	0.04	0.04	0.04	0.05
preterm (34 weeks $<=$ GA $<$ 37 weeks)	0.05	0.05	0.02	0.06	0.06	0.06	0.07
days in hospital	3.99	3.91	3.80	4.83	4.71	4.64	4.83
N	300,535	3,906	271	4,973	2,274	4,752	2,183

Note: This table describes the prenatal environment and childbirth. The top panel describes mother's characteristics, behavior, and health during pregnancy and at childbirth, while the bottom panel describes child health. Column (1) shows averages for all first childbirths during 2005-12. Column (2) refers to the subset of mothers who had a LARC prescription prior to first childbirth, while column (3) only refers to those who had an unplanned birth (the initial compliers in our LARC treatment group). Column (4) refers to the subset of mothers who had an IVF treatment prior to first childbirth, while column (5) only refers to those who were successful in the first attempt. Columns (6) and (7) are reweighted versions of columns (4) and (5), respectively, to match the age distribution of the LARC group at the time of prescription. The last six columns condition on the woman being age 43 or younger at the time of LARC prescription or first IVF treatment, respectively.

Table A2: Balance: LARC 2nd Child Setting

	LARC: All Women with 1 Child			LARC: Women with 1 Child No Postpartum Mental Health Prescrip.		
	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff
Earnings (1000s)	142.002	145.468	0.618	112.844	120.292	0.454
Earnings Including Paid Leave (1000s)	176.692	176.937	0.928	173.604	174.468	0.941
Monthly Wage, FT-equivalent (1000s)	23.013	22.486	0.091	24.838	23.730	0.347
Fraction of Full-Time Employment	82.274	82.237	0.322	80.409	83.289	0.724
Observed in Wage Statistics	0.372	0.366	0.719	0.235	0.256	0.514
Employed	0.706	0.719	0.361	0.704	0.731	0.366
Occupations Requiring Higher Skills	0.288	0.285	0.801	0.305	0.314	0.761
Anti-Depression Prescription	0.163	0.101	0.000	0.009	0.009	0.957
Anti-Anxiety Prescription	0.074	0.046	0.006	0.005	0.005	0.966
Enrolled in Education	0.164	0.159	0.719	0.141	0.116	0.342
High School	0.435	0.457	0.242	0.390	0.409	0.568
College Degree or Higher	0.309	0.326	0.395	0.366	0.391	0.435
Married	0.327	0.338	0.656	0.380	0.401	0.535
Divorced	0.072	0.059	0.322	0.047	0.043	0.820
Observations	683	31,763		213	8,803	

Note: This table presents average labor market, education, and civil status variables for the "treated" and "untreated" in the year before LARC purchase ($t = -1$), as well as the p-value of the difference. The first three columns include the sample of all women who already have one child, while the last three columns are based on the subset of those who do not take out a prescription for any anti-depression or anti-anxiety medication in the year their first child is born (the sample is much smaller as we require first births to occur after 2005 so that prescriptions are observed in the year of first childbirth). The untreated are re-weighted to have the same mean age, time since first childbirth, and year of prescription as the treated. Yearly earnings are measured in thousands of 2010 SEK. Employment status is measured in the last week of November of a given year. Work hours are measured as a percent of a full-time equivalent worker. Wages and work hours are measured for the $\approx 50\%$ sub-sample (undersampling smaller firms) observed in the wage statistics. Note that a small number of women are not observed in the year before LARC purchase; for example, if they are out of the country, and these women are not included in these calculations. Sample: Women born in 1965-83 with one prior child birth and less than 44 years old at the time of LARC prescription.

Table A3: Occupation Transitions: LARC and IVF

	LARC			IVF		
Occcupation transitions from t=-1 to t=5	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff	First IVF Successful	First IVF Unsuccessful	p-value diff
Same 2-digit occupation	0.54	0.48	0.170	0.67	0.65	0.132
Same 3-digit occupation	0.51	0.44	0.114	0.63	0.60	0.082
Switch to occupation with lower skill requirements	0.01	0.03	0.287	0.03	0.04	0.153
Switch to occupation with higher skill requirements	0.17	0.25	0.042	0.09	0.10	0.108
Conditional on enrollment in education:						
Switch to occupation with higher skill requirements	0.24	0.36	0.048	0.14	0.15	0.474
Conditional on lower skill requirement occupation at t=-1						
Same 2-digit occupation	0.51	0.45	0.225	0.65	0.63	0.406
Same 3-digit occupation	0.47	0.42	0.234	0.61	0.59	0.520
Switch to occupation with higher skill requirements	0.22	0.33	0.016	0.18	0.22	0.116
Conditional on enrollment in education:						
Switch to occupation with higher skill requirements	0.29	0.45	0.021	0.28	0.30	0.623

Note: This table describes the persistence and switching patterns in occupation skill requirements from the year before LARC prescription (first IVF treatment) to five years after in the first three columns (last three columns). The first column includes women who had an unplanned pregnancy within 9 months of LARC purchase, the second column includes the women who did not have an unplanned pregnancy within 9 months of purchasing the LARC, the fourth column includes the women who had a successful first IVF treatment, while the fifth column includes those who had an unsuccessful first IVF treatment. The third (sixth) column displays the p-values for the difference in means between the two LARC (IVF) groups. The bottom panel shows occupation transitions for women who initially are in occupations with low skill requirements. Sample: Nulliparous women born in 1965-1983. Prescriptions are observed during 2005-2012. Occupation is observed yearly during the period 2004-2013 and non-missing in both years ($t = -1$ and $t = 5$) for 9,144 (3,051) women in the full nulliparous LARC (IVF) sample.

Table A4: Postpartum Environment, Parental Leave, Family- and Civil Status

	All	LARC		IVF			
	1st births 2005-12	LARC before 1st birth	Unplanned birth	IVF before 1st birth	Planned birth	IVF before 1st birth (reweighted)	Planned birth (reweighted)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mother prenatal leave							
any pregnancy leave	0.19	0.24	0.22	0.21	0.23	0.25	0.27
net days on pregnancy leave	6.78	5.46	7.80	5.83	6.67	7.65	9.75
any sickleave	0.42	0.49	0.50	0.51	0.51	0.50	0.52
net days on sickleave	16.67	13.78	23.57	16.44	18.18	19.53	23.11
Mother postpartum leave							
any parental leave	0.97	0.98	0.98	0.98	0.98	0.96	0.97
days on parental leave	262.48	229.47	281.24	220.14	222.03	248.85	250.30
net days on parental leave	201.84	94.35	182.73	129.51	135.56	170.59	181.97
Father postpartum leave							
any parental leave	0.77	0.75	0.67	0.81	0.81	0.78	0.76
days on parental leave	56.83	51.05	50.30	50.24	48.85	44.55	44.36
net days on parental leave	40.94	17.78	29.57	27.74	29.04	28.63	32.39
Father demographics at childbirth							
age	33.22	33.32	33.26	35.84	35.27	33.53	33.21
earnings at childbirth (SEK 1000s)	309.82	322.83	268.14	344.39	331.29	284.54	271.19
occupation with high skill requirements	0.48	0.48	0.34	0.55	0.55	0.38	0.37
Family status: Father living with mother and child							
at birth	0.90	0.87	0.79	0.94	0.93	0.91	0.89
1 year after birth	0.92	0.89	0.79	0.95	0.95	0.93	0.93
3 years after birth	0.89	0.85	0.80	0.93	0.92	0.89	0.89
5 years after birth	0.86	0.72	0.67	0.89	0.88	0.82	0.84
Civil status: Mother married							
at birth	0.43	0.36	0.34	0.61	0.58	0.46	0.43
1 year after birth	0.48	0.41	0.36	0.64	0.62	0.51	0.48
3 years after birth	0.53	0.49	0.46	0.67	0.67	0.56	0.53
5 years after birth	0.57	0.44	0.42	0.70	0.70	0.59	0.61
Civil status: Mother divorced							
at birth	0.03	0.04	0.08	0.02	0.02	0.03	0.03
1 year after birth	0.03	0.04	0.09	0.02	0.02	0.04	0.03
3 years after birth	0.04	0.05	0.09	0.03	0.03	0.04	0.03
5 years after birth	0.05	0.12	0.13	0.05	0.06	0.08	0.06
N	300,535	3,906	271	4,973	2,274	4,752	2,183

Note: This table describes the postpartum environment, including parental leave, and parental family- and civil status. Column (1) shows averages for all first childbirths during 2005-12. Column (2) refers to the subset of mothers who had a LARC prescription prior to first childbirth, while column (3) only refers to those who had an unplanned birth (the initial compliers in our LARC treatment group). Column (4) refers to the subset of mothers who had an IVF treatment prior to first childbirth, while column (5) only refers to those who were successful in the first attempt. Columns (6) and (7) are reweighted versions of columns (4) and (5), respectively, to match the age distribution of the LARC group at the time of prescription. The last six columns condition on the woman being age 43 or younger at the time of LARC prescription or first IVF treatment, respectively.

Table A5: Long-Term Effect Among Young LARC Users, Weighted to Reflect Older LARC User Characteristics

	27 and younger	
	Unweighted	Weighted
	(1)	(2)
Average Earnings Inc. Paid Leave (Years 1-6)	-79.082	-72.797
	(14.317)	(15.793)
...As % of Average Counterfactual Earnings	-0.332	-0.315
	(0.060)	(0.068)
Aggregate Years Employed	-0.621	-0.514
	(0.244)	(0.296)
Aggregate Years in Occupations Requiring Higher Skills	-1.291	-1.326
	(0.275)	(0.308)
Aggregate Years with Anti-Anxiety Prescription	-0.149	-0.141
	(0.104)	(0.156)
Aggregate Years with Anti-Depression Prescription	0.026	-0.027
	(0.190)	(0.207)
Aggregate Number of Children	1.565	1.660
	(0.086)	(0.101)
Observations	9,607	9,597
Controls	N	N

Note: This table presents results of our IV-GMM estimation of the dynamic effects of unplanned birth for women who are 27 and younger when using LARCs. Column (1) is identical to Column (3) in Table 3. Column (2) is estimated similarly, except each observation is re-weighted based on the relative probability that it looks like individuals in the *older* LARC users sample (those 28 and older at the time of LARC prescription) based on their characteristics when they were the same age as the *younger* LARC users sample, on average. Sample: Women born in 1965-83 with no prior child births and less than 44 years old at the time of LARC prescription.

Table A6: Occupation Transitions

	Age	
Occupation transitions over six years	**22-28**	**28-34**
Same 2-digit occupation	0.42	0.58
Same 3-digit occupation	0.39	0.53
Switch to occupation with lower skill requirements	0.02	0.04
Switch to occupation with higher skill requirements	0.30	0.15
Conditional on enrollment in education:		
Switch to occupation with higher skill requirements	0.39	0.23
Conditional on initial lower skill requirement occupation		
Same 2-digit occupation	0.42	0.55
Same 3-digit occupation	0.39	0.51
Switch to occupation with higher skill requirements	0.33	0.25
Conditional on enrollment in education:		
Switch to occupation with higher skill requirements	0.43	0.36
Conditional on initial service, care, and security (51)		
Same 2-digit occupation	0.55	0.68
Same 3-digit occupation	0.52	0.66
Switch to occupation with higher skill requirements	0.31	0.21
Conditional on enrollment in education:		
Switch to occupation with higher skill requirements	0.42	0.35
N women with non-missing six-year occupation transition	**170,735**	**315,008**
Fraction enrolled in education	0.70	0.41
Fraction initially in lower skill requirement occupation	0.92	0.60
...and enrolled in education	0.64	0.26
Fraction initially in service, care, and security (51)	0.39	0.26
...and enrolled in education	0.28	0.12

Note: This table describes the persistence and switching patterns in occupation skill requirements from age 22 to age 28 (first column) and from age 28 to age 34 (second column). The middle panel zooms in on women who initially are in an occupation with low skill requirements at age 22 (first column) or age 28 (second column), while the bottom panel zooms in on the women initially in the most common 2-digit occupation: service, care, and security (51). Occupation is observed yearly during the period 1990-2013. Sample: All women born 1965-1983 with non-missing occupation at the beginning and at the end of the six-year period.

Table A7: Occupation Transitions by Occupation (%)

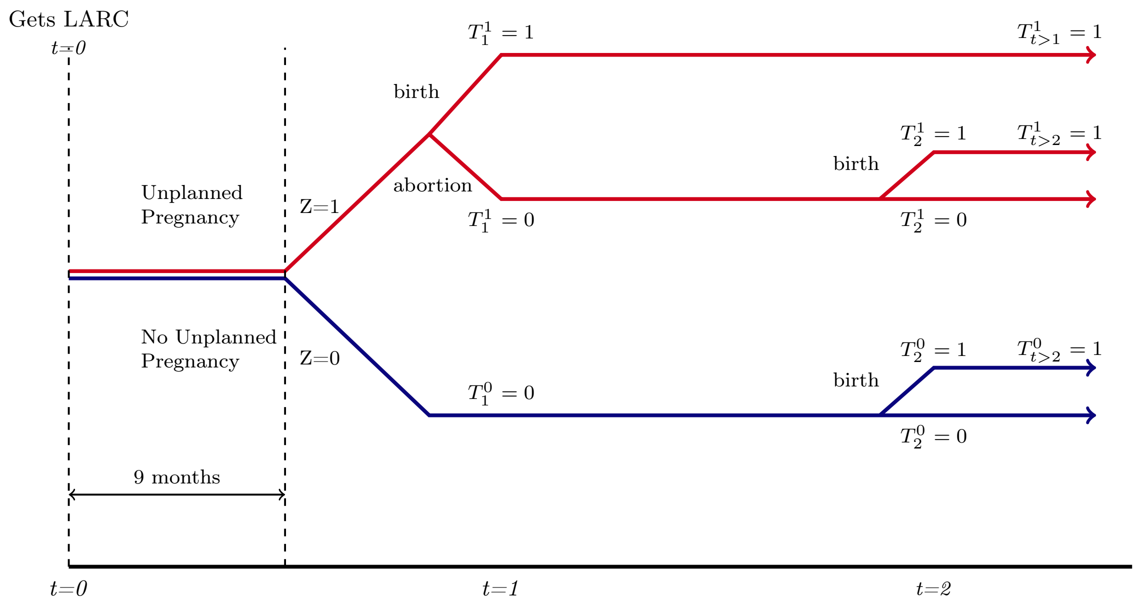
	Occupation at age 28 (2-digit)									
	(23)	(24)	(32)	(33)	(34)	(41)	(51)	(52)	(91)	N
Occupation at age 22 (2-digit)										
(23) Teachers with theoretical expertise	**35.42**	10.24	4.11	6.75	8.98	3.53	11.41	3.94	1.20	2,919
(24) Other work that requires theoretical expertise	4.02	**46.93**	2.01	1.79	16.09	7.71	6.15	1.90	0.89	895
(32) Jobs in biology and health care that require a college degree	1.83	1.60	**69.89**	0.15	2.06	1.14	4.88	1.22	0.38	1,312
(33) Teachers with a college degree	7.56	4.43	3.40	**44.76**	6.10	3.02	17.28	4.59	1.35	1,852
(34) Other work requiring a college degree	3.68	11.09	2.29	2.32	**39.33**	10.13	9.92	5.67	1.54	5,645
(41) Office work	3.54	9.28	3.20	2.06	14.25	**34.29**	8.55	5.32	1.87	11,957
(51) Service, care, and security jobs	4.13	4.72	7.67	4.53	5.47	3.02	**55.08**	3.97	2.36	66,919
(52) Sales work in retail	4.92	7.95	3.62	2.86	10.36	6.13	9.52	**37.19**	2.16	30,971
(91) Service jobs without vocational training requirements	4.26	5.58	3.37	2.99	7.44	5.21	19.10	8.40	**27.92**	22,343
Occupation at age 22 (3-digit)										
(513) Health care assistants, assistant nurses, personal assistants etc.	4.28	4.74	8.45	4.90	5.17	2.76	**55.82**	3.51	1.82	58,706

	Occupation at age 34 (2-digit)									
	(23)	(24)	(32)	(33)	(34)	(41)	(51)	(52)	(91)	N
Occupation at age 28 (2-digit)										
(23) Teachers with theoretical expertise	**73.98**	4.70	0.70	3.32	3.05	1.32	2.51	0.74	0.19	23,559
(24) Other work that requires theoretical expertise	1.94	**68.66**	0.51	0.25	11.74	3.23	1.25	0.48	0.23	21,062
(32) Jobs in biology and health care that require a college degree	1.19	1.99	**71.65**	0.09	1.75	0.61	1.35	0.42	0.13	14,911
(33) Teachers with a college degree	8.80	1.26	0.31	**77.51**	2.34	0.98	5.06	0.72	0.35	11,986
(34) Other work requiring a college degree	2.00	17.16	0.92	0.97	**50.69**	8.80	3.94	2.42	0.39	28,287
(41) Office work	1.95	11.25	1.23	0.84	17.68	**43.04**	4.64	2.77	1.17	25,962
(51) Service, care, and security jobs	2.85	3.42	4.21	2.94	4.45	2.71	**68.03**	2.23	2.43	82,301
(52) Sales work in retail	3.28	5.19	1.71	1.81	9.14	5.37	8.49	**48.39**	2.19	27,437
(91) Service jobs without vocational training requirements	2.27	3.05	1.68	1.54	4.66	4.09	19.50	4.93	**46.35**	19,044
Occupation at age 28 (3-digit)										
(513) Health care assistants, assistant nurses, personal assistants etc.	2.80	3.12	4.62	3.24	3.80	2.41	**70.67**	1.78	1.80	69,605

Note: This table describes the occupation transitions for the nine most common 2-digit occupations and the most common 3-digit occupation from age 22 to age 28 (top panel) and from age 28 to age 34 (bottom panel). Sample: All women born 1965-1983 and occupation is observed yearly during the period 1990-2013.

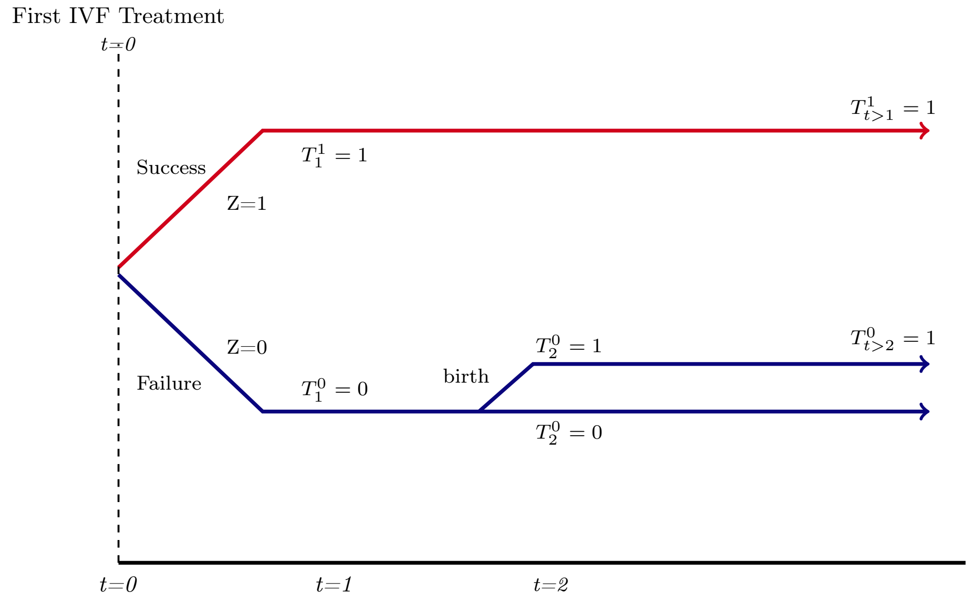
B Identification and Estimation Strategies for LATE with Dynamic Compliance

Figure B1: Dynamic Compliance: LARC Setting



Note: This figure displays dynamic compliance for a group of women who get a LARC in the same year and at the same age, at $t = 0$. Some of these women experience an unplanned pregnancy within 9 months following LARC purchase ($Z = 1$, the "treatment group", red line) and some have no unplanned pregnancy ($Z = 0$, the "control group", blue line). Not all women who have an unplanned pregnancy give birth to the unplanned child ($T_1^1 = 1$, the "compliers") and non-compliers at $t = 1$ are the women who have an abortion following the unplanned pregnancy ($T_1^1 = 0$, the "never-takers"). The later-takers in the control group are the women who do not have an unplanned pregnancy initially but later become pregnant and have a child ($T_2^0 = 1$). The notation and the estimator for the impact of unplanned birth are described in more detail in Section 3.2.

Figure B2: Dynamic Compliance: IVF Setting



Note: This figure displays the dynamics of compliance for a group of women who start fertility treatment at the same age and in the same year, at $t = 0$. The "treatment group" are the women who have success in first IVF procedure ($Z = 1$, red line). The "control group" are the women who are not successful in first IVF procedure ($Z = 0$, blue line). In this setting, there are no never-takers, but there are always-takers who conceive a few months later in the same year (not shown). The "later-takers" in the control group are the women who later give birth (e.g. $T^0_2 = 1$), likely conceiving through a subsequent IVF procedure.

B.1 Identification of LATE with Dynamic Compliance

Proposition 3.1. Given Assumptions 1-5, the dynamic causal effect for the compliers ($\mathcal{C} = 1$) for the first period is identified. Identifying the dynamic effects in subsequent periods requires accounting for bias terms that arise from compliers in the control group receiving treatment after the first period (dynamic non-compliance):

$$\underbrace{\mathbb{E}\left[\rho(1)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=1}} = \text{Wald}_1$$

$$\underbrace{\mathbb{E}\left[\rho(2)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=2}} = \text{Wald}_2 + \mathbb{E}\left[\rho(\tau=1)|T_2=1,\mathcal{C}=1\right]P[T_2=1|\mathcal{C}=1]$$

$$\vdots$$

$$\underbrace{\mathbb{E}\left[\rho(\tau=t)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=t}} = \text{Wald}_t + \sum_{s=2}^{t} \underbrace{\mathbb{E}\left[\rho(\tau=t-s+1)|T_s=1,\mathcal{C}=1\right]}_{\text{LATE for period } s \text{ later-takers}} \underbrace{P\left[T_s=1|\mathcal{C}=1\right]}_{\text{Prob. of later treatment}}$$

Proof. For this proof, we show identification sequentially, starting with the first period when the instrument influences treatment.

Period $t = 1$: First consider the reduced form for $t = 1$ (the year of treatment assignment), where we simplify the notation for the potential outcomes $Y_{i1}^{\tau} \equiv Y_{i1}(\tau)$.

$$\begin{aligned}
&\mathbb{E}[Y_{i1}|Z=1] - \mathbb{E}[Y_{i1}|Z=0] \\
&\quad = \mathbb{E}\left[Y_{i1}^0 + T_{i1}(Z=1)\left(Y_{i1}^1 - Y_{i1}^0\right)\right] - \mathbb{E}\left[Y_{i1}^0 + T_{i1}(Z=0)\left(Y_{i1}^1 - Y_{i1}^0\right)\right] \\
&\quad = \mathbb{E}\left[\left(Y_{i1}^1 - Y_{i1}^0\right)\left(T_{i1}(Z=1) - T_{i1}(Z=0)\right)\right] \\
&\quad = \underbrace{\mathbb{E}\left[Y_{i1}^1 - Y_{i1}^0 | T_{i1}(Z=1) > T_{i1}(Z=0)\right]}_{\text{LATE}_{\tau=1}} P\left[T_{i1}(Z=1) > T_{i1}(Z=0)\right],
\end{aligned} \tag{3}$$

where we use the no-anticipatory effect assumption in the first step (*e.g.* $Y_{i1}^0(T_{i2}=1) = Y_{i1}^0(T_{i2}=0) = Y_{i1}^0$) and the monotonicity assumption for the last step. Equation (3) shows identification in $t = 1$ which gives us the first year impact ($\tau = 1$) of treatment for compliers ($\mathcal{C} = 1$) in a framework that can account for multiple time periods and impact dynamics,[49]

$$\mathbb{E}\left[\rho(1)|\mathcal{C}=1\right] = \text{Wald}_1 \equiv \frac{\mathbb{E}[Y_{i1}|Z=1] - \mathbb{E}[Y_{i1}|Z=0]}{P\left[T_{i1}=1|Z=1\right] - P\left[T_{i1}=1|Z=0\right]}. \tag{4}$$

Period $t = 2$: Next, consider the reduced form for $t = 2$ (the year after the instrument influences treatment), where we further simplify the notation for treatment in period t for assignment z: $T_{it}^z \equiv T_{it}(Z = z)$. Recall that Y_{i2}^1 is the potential outcome Y in period 2 for someone who has experienced 1 year of treatment (was treated in period $t = 2$) and Y_{i2}^2 is the potential outcome for someone who has been treated for 2 years. The reduced form is

$$\begin{aligned}
&\mathbb{E}[Y_{i2}|Z=1] - \mathbb{E}[Y_{i2}|Z=0] \\
&\quad = \mathbb{E}\left[Y_{i2}^0 + T_{i1}^1\left(Y_{i2}^2 - Y_{i2}^0\right) + T_{i2}^1\left(1 - T_{i1}^1\right)\left(Y_{i2}^1 - Y_{i2}^0\right)\right] \\
&\qquad\quad - \mathbb{E}\left[Y_{i2}^0 + T_{i1}^0\left(Y_{i2}^2 - Y_{i2}^0\right) + T_{i2}^0\left(1 - T_{i1}^0\right)\left(Y_{i2}^1 - Y_{i2}^0\right)\right] \\
&\quad = \mathbb{E}\left[\left(Y_{i2}^2 - Y_{i2}^0\right)\left(T_{i1}^1 - T_{i1}^0\right)\right] - \mathbb{E}\left[\left(Y_{i2}^1 - Y_{i2}^0\right)T_{i2}\left(T_{i1}^1 - T_{i1}^0\right)\right] \\
&\quad = \underbrace{\mathbb{E}\left[Y_{i2}^2 - Y_{i2}^0 | T_{i1}^1 > T_{i1}^0\right]}_{\text{LATE}_{\tau=2}}\left(P[T_{i1}=1|Z=1] - P[T_{i1}=1|Z=0]\right) \\
&\qquad\quad - \mathbb{E}\left[Y_{i2}^1 - Y_{i2}^0 | T_{i2}=1, T_{i1}^1 > T_{i1}^0\right] P[T_{i2}=1|T_{i1}^1 > T_{i1}^0]\left(P[T_{i1}=1|Z=1] - P[T_{i1}=1|Z=0]\right),
\end{aligned} \tag{5}$$

[49] The monotonicity assumption gives

$$P\left[T_{i1}(Z=1) > T_{i1}(Z=0)\right] = P\left[T_{i1}=1|Z=1\right] - P\left[T_{i1}=1|Z=0\right].$$

where we use the exclusion restriction on treatment paths once we condition on treatment in the first period (i.e. $T^1_{i2} = T^0_{i2} = T_{i2}$ when $T_{i1} = 0$).

Re-arranging equation (5) to solve for the Wald estimator in period 2, we get the LATE for $\tau = 2$ and a bias term due to individuals who receive treatment in period 2 (period 2 later-treated).

$$\begin{aligned}
&\underbrace{\frac{\mathbb{E}[Y_{i2}|Z=1] - \mathbb{E}[Y_{i2}|Z=0]}{P\left[T_{i1}=1|Z=1\right] - P\left[T_{i1}=1|Z=0\right]}}_{\text{Wald}_{t=2}} \\
&= \underbrace{\mathbb{E}\left[Y^2_{i2} - Y^0_{i2}|T^1_{i1} > T^0_{i1}\right]}_{\text{LATE}_{\tau=2}} - \mathbb{E}\left[Y^1_{i2} - Y^0_{i2}|T_{i2}=1, T^1_{i1} > T^0_{i1}\right] P[T_{i2}=1|T^1_{i1} > T^0_{i1}].
\end{aligned} \tag{6}$$

We can now solve for the second period $LATE_{\tau=2}$:

$$\underbrace{\mathbb{E}\left[\rho(2)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=2}} = \text{Wald}_2 + \underbrace{\mathbb{E}\left[\rho(1)|T_2=1,\mathcal{C}=1\right]}_{\text{LATE}_1 \text{ for } t=2 \text{ later-takers}} P[T_2=1|\mathcal{C}=1].$$

Period $t = 3$: We can repeat the derivation for $t = 3$, where there are now two terms that bias the Wald estimator: one for later-takers that received treatment in period 2 and another for later-takers that receive treatment in period 3. Following similar approach to $t = 2$, the LATE for period $t = 3$ is

$$\begin{aligned}
\underbrace{\mathbb{E}\left[\rho(3)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=3}} = \text{Wald}_3 &+ \underbrace{\mathbb{E}\left[\rho(2)|T_2=1,\mathcal{C}=1\right]}_{\text{LATE}_2 \text{ for } t=2 \text{ later-takers}} P[T_2=1|\mathcal{C}=1] \\
&+ \underbrace{\mathbb{E}\left[\rho(1)|T_3=1,\mathcal{C}=1\right]}_{\text{LATE}_1 \text{ for } t=3 \text{ later-takers}} P[T_3=1|\mathcal{C}=1].
\end{aligned}$$

Period $t > 3$: It follows that later periods will include additional bias terms for the later-takers in each period until t. The LATE for an arbitrary period $t > 1$ is

$$\underbrace{\mathbb{E}\left[\rho(\tau=t)|\mathcal{C}=1\right]}_{\text{LATE}_{\tau=t}} = \text{Wald}_t + \sum_{s=2}^{t} \underbrace{\mathbb{E}\left[\rho(t-s+1)|T_s=1,\mathcal{C}=1\right]}_{\text{LATE for period } s \text{ later-takers}} \underbrace{P\left[T_s=1|\mathcal{C}=1\right]}_{\text{Prob. of later treatment}}.$$

□

B.2 IV-GMM Estimator

The baseline joint LARC-IVF IV-GMM estimator, described in the main text, jointly estimates the effect of planned births $\rho^p(\tau)$ from the IVF setting and the effect of unplanned births $\rho^u(\tau)$ from the LARC setting:

$$g_i(\theta) \quad = \quad \left[\begin{array}{l} Z_i^{IVF} \left(Y_{it} - \sum_{\tau=1}^{T} \rho^p(\tau) \mathbf{1}[\tau - 1 = t - t_i^p] \right) \\ Z_i^{LARC} \left(Y_{it} - \sum_{\tau=1}^{T} \rho^u(\tau) \mathbf{1}[\tau - 1 = t - t_i^u] - \sum_{\tau=1}^{T} \rho^p(\tau) \mathbf{1}[\tau - 1 = t - t_i^p] \right) \end{array} \right] \tag{7}$$

where the first (second) set of moments use data from the IVF (LARC) setting, t_i^u refers to the year of the first unplanned LARC birth, t_i^p refers to the year of other first LARC births and IVF births, and all variables are demeaned within age $\times$ year cells. In the traditional just-identified IV-GMM estimator there are as many equations as instruments. In our setting, we have one instrument in each setting, but we have multiple observations of Y_{it}.[50] We now present the IV-GMM specifications corresponding to the alternative assumptions about the effect of planned births (later-treated) discussed in the main text.

Approach (2): Same effects for planned and unplanned births. Under Assumption 6, the effects of initial unplanned and later planned births are the same, $\rho^u(\tau) = \rho^p(\tau) = \rho(\tau)$ for all τ. In this case, we do not need to separately identify the effect of planned births, and the IV-GMM estimator simplifies to a single-instrument specification:

$$g_{it}^2(\theta) \quad = \quad Z_i \left(Y_{it} - \sum_{\tau=1}^{T} \rho(\tau) \mathbf{1}[\tau - 1 = t - t_i] \right), \tag{8}$$

where $Z_i = Z_i^{LARC}$ for the LARC setting, t_i refers to the year of first birth, and all variables are demeaned within age $\times$ year cells. This is the approach taken by Angrist et al. (2025), who refer to Assumption 6 as the *wave ignorability* assumption. The different $\rho(\tau)$ are identified under Assumptions A1–A6, as described in Corollary 3.1.1. This specification is also used when estimating the IVF setting alone, where $Z_i = Z_i^{IVF}$ and t_i refers to the year of the IVF birth.

Approach (3): Event study estimates of the effect of planned birth. An alternative approach is to use event study estimates $\widehat{\rho}^{ES}(\tau)$ from the specification in Kleven et al. (2019a) as the estimate for the effect of planned births on later-takers. Under this assumption, we do not need the IVF instrument, and the

[50] In every specification that we estimate, we assume that ρ is the same for the last two periods as we observe only a small number of women six years after birth (included in "5+" in the figures). Hence, it is indeed a *generalized* method of moments estimator.

IV-GMM estimator uses only the LARC moments:

$$g_{it}^3(\theta) \quad = \quad Z_i^{LARC}\left(Y_{it} - \sum_{\tau=1}^{T}\rho^u(\tau)\mathbf{1}[\tau-1=t-t_i^u] - \sum_{\tau=1}^{T}\widehat{\rho}^{ES}(\tau)\mathbf{1}[\tau-1=t-t_i^p]\right), \tag{9}$$

where $\widehat{\rho}^{ES}(\tau)$ are event study estimates from a first-step that capture the association between first childbirth and earnings relative to childless women of the same age, and are treated as known when estimating $\rho^u(\tau)$. Here, only the unplanned birth effects $\rho^u(\tau)$ are estimated, while the planned birth effects are fixed at their event study values.

Approach (4): IVF-identified planned birth effects with propensity score reweighting. Women undergoing IVF differ from LARC users on observable characteristics such as income, education, and age. To address concerns that IVF-identified planned birth effects may not generalize to the population of LARC later-takers, we re-weight the IVF sample using DiNardo et al. (1996) propensity score weights. Specifically, we predict the probability that a woman in the IVF sample would appear in the LARC sample based on pre-treatment characteristics, and weight IVF observations accordingly. The reweighted joint LARC-IVF IV-GMM estimator is:

$$g_i^4(\theta) \quad = \quad \begin{bmatrix} \omega_i \cdot Z_i^{IVF}\left(Y_{it} - \sum_{\tau=1}^{T}\rho^p(\tau)\mathbf{1}[\tau-1=t-t_i^p]\right) \\ Z_i^{LARC}\left(Y_{it} - \sum_{\tau=1}^{T}\rho^u(\tau)\mathbf{1}[\tau-1=t-t_i^u] - \sum_{\tau=1}^{T}\rho^p(\tau)\mathbf{1}[\tau-1=t-t_i^p]\right) \end{bmatrix} \tag{10}$$

where ω_i are propensity score weights for observation i in the IVF sample, constructed from the estimated probability that a woman with her characteristics appears in the LARC sample relative to the IVF sample. The characteristics used to form the propensity score include income, education, civil status, employment, occupation, private sector employment, and age, all measured in the year before the fertility procedure. The LARC moments are unweighted. As in the baseline, t_i^u refers to the year of the first unplanned LARC birth, t_i^p refers to the year of other first LARC births and IVF births, and all variables are demeaned within age $\times$ year cells.

Lastly, the data must be prepared before using the GMM estimators described above. First, the LARC instrument is valid only after matching women on age and year of treatment.[51] We perform the matching by demeaning the outcomes and indicators in each (age) $\times$ (year) $\times$ (years since LARC) cell. Second, in order to reduce measurement error, the timing of the LARC prescription and the timing of the birth within a calendar year has to be accounted for in the analysis. The framework assumes that assignment of the

[51] The data preparation for the IVF groups is similar, except that women are additionally matched on education and years since last contraception.

instrument happens in the first year. How a birth affects the income of a woman will depend on if a baby is born in January, where she usually takes leave for the full calendar year, or if a baby is born in December, where she usually takes one or two months of leave in that calendar year. Accounting for the patterns in how Swedish women take their maternity leave, we assign the first year to be the year after the LARC if the baby is born before July of that year, it not, we assign the second year after the LARC to be the first year. In addition, when estimating the impacts on income, we replace indicators with fractions of the year exposed so that the first year effect of a child accounts for the amount of leave a women will take in a calendar year. This is important as the month of birth patterns are quite different in the LARC and IVF settings.

The main attractiveness of the GMM estimator is that each moment corresponds directly to the identification equation for period $t \geq 1$. There are three additional advantages to using GMM on demeaned matched data: First, it is straightforward to specify a fully-saturated model that includes controls non-parametrically when we match based on age, year, and time since the LARC.[52] Second, we can jointly estimate effects of planned and unplanned births by stacking the LARC and IVF data and using both instruments simultaneously.[53] Finally, we can easily estimate the model for different age sub-samples and include $t = -1$ controls in the equations above.

C Data Appendix

We merge several Swedish administrative registers via a unique individual identifier. The primary data sources are the Prescribed Drug Register (MLED), the Medical Birth Registry (MFR) and the National Patient Register (NPR) that are administered by the National Board of Health and Welfare (*Socialstyrelsen*).

We merge these registers with several registers administered by Statistics Sweden (SCB, "*Statistiska Centralbyrån*"). The main registry is the longitudinal integration database for health insurance and labour market studies (LISA) from which we have yearly observations during the period 1990-2013. The individual variables we observe include age, civil status, family status, highest completed education, enrollment in education, employment, sector, occupation, earnings, and public social transfers.

C.1 Health Data and Definitions

This section defines how we use birth records, prescription drug records, and specialist outpatient care records to measure fertility intentions and timing of events (conception, abortion, and birth).

[52] Blandhol et al. (2026) show that the 2SLS estimator with controls will not estimate the LATE unless the model is fully-saturated with non-parametric controls. Otherwise, the 2SLS estimator includes some of the treatment effects for always- and never-takers with negative weights.

[53] In principle, $\hat{\rho}^p_\tau$ could be estimated using 2SLS with the IVF setting in a first stage. Then $\hat{\rho}^p_\tau$ could be used when estimating $\hat{\rho}^u_\tau$ with the LARC setting in a second stage. Inference would likely require using bootstrap methods.

We have access to prescription drug records from July 2005 through 2013, and specialist outpatient records from 2001-2012. The outpatient data records all specialist visits excluding primary care, which in Sweden is provided at municipal care centers ("*Vårdcentral*") staffed primarily by nurses who can refer patients to specialized outpatient care. The prescription drug records contain all individual pharmacy purchases, but exclude drugs administered in hospitals. For each prescription, we have information on drug name, active substance, average daily dose, and Anatomical Therapeutic Chemical (ATC) code. The ATC classification system, used in Sweden and internationally, allows us to link drugs to the conditions they typically treat. The specific ATC codes and definitions of variables used in our analysis are provided below.

Births and Parity. The birth records contain comprehensive data on all Swedish births from 1973 to 2012, including measures of the child's in-utero environment, health status at birth, and pregnancy characteristics. The registry includes maternal diagnosis and complications during pregnancy and delivery, child birth weight, expected due date, birth type (singleton versus multiple birth), birth order, APGAR scores (Apgar, 1952) at 1, 5, and 10 minutes after birth, gestational age, indicators for large- (LGA) and small-for-gestational-age (SGA), and child diagnoses at birth. We also have indicators for cesarean section deliveries, labor inductions, and various pregnancy risk factors and delivery complications. We primarily use the birth records to classify women according to their parity, birth order of children, number of childbirths, and to calculate conception dates.

Conception Date. The birth registry gives us information about gestational age for pregnancies that end in childbirth. It contains information about the last period calculated from both the first ultrasound and the date reported by the mother. We assume that conception occurred two weeks after the last period. We calculate the conception date for an abortion using the median time (45 days) from conception to the first abortion meeting for women who have an initial abortion meeting but go on to have the child.

Abortions. The patient records contain information about the initial meeting a woman has with a doctor in the process of having an abortion due to unwanted pregnancy: diagnosis code Z640. This is similar to one of the definitions used by Janys and Siflinger (2024). We do not observe the actual abortion procedure in our data.[54] We assume all women who had such a meeting had an unplanned pregnancy, though we do observe that some of these women go on to give birth. We assume the woman had an abortion if we observe an abortion meeting not followed by a childbirth.

[54] Janys and Siflinger (2024) use a special local dataset for the region of Skåne (around 13% of the Swedish population) that records all in- and out-patient contacts with the health care system.

LARC. Hormonal IUDs containing progestogen are identified by ATC code G02BA03 and implants have ATC code G03AC08. Some implants are also included in ATC code G03AC03. We include prescription G03AC03, where we also require the Swedish word "*implantat*" to be contained in the prescription name. Copper IUDs do not require a prescription and, hence, are not observed in our data.

SARC. Almost 94% of the SARC prescriptions we observe in Sweden are the birth-control pill. The most commonly prescribed pill during our sample period is the Cerazette progestogen-only pill representing about 35% of all SARC prescriptions. The remaining SARCs are the vaginal ring (5.4%) and transdermal patch (1%). The specific codes we use are: G03AC09, G03AA12, G03AA07, G03AB03, G03AA09, G03AC02, G02BB01, G03AC01, G03AB04, G03AA11, G03AA13, G03AA05, G03AB, G03AA03, G03AA14, G03AB05, G02BB, and G03AC03.

To assess the external validity of the impacts of unplanned pregnancies in the LARC population, we also identify unplanned pregnancies in the population of SARC users—more than 50% of women in Sweden (see Figure A1a). Credibly constructing a control group and separately classify unplanned from planned pregnancies using data on women using SARCs is challenging because some women may not use this contraceptive as prescribed, increasing the likelihood of pregnancy. For example, the birth-control pill must be taken daily at the same hour to be most effective. Unobserved variation in the conscientiousness of taking the pill is likely to be correlated with many outcomes. Furthermore, women can simply stop taking the pill when their intentions change.

We propose two classes of unplanned pregnancies among SARC users that are based on the timing of purchasing a prescription relative to having conceived a child according to medical birth records. The primary classification is based on purchasing a SARC prescription when arguably being unaware of already being pregnant. The alternative classification is based on considering having an abortion while having an active SARC prescription, but still giving birth to the child.

First, we define unplanned first births as those occurring among nulliparous women who get a SARC prescription within two weeks of conceiving (i.e. during the same menstrual cycle). We interpret the purchase of a prescription as an indicator that the pregnancy was unplanned. We consider other windows in our robustness and the results are qualitatively similar.[55] The key assumption of our SARC strategy is that women are unaware that they are pregnant when they get the SARC prescription. As the date of conception

[55]We use three definitions: the baseline is receiving a SARC prescription (-14,14) days before conception and the two alternative definitions switch this window two weeks earlier and later, SARC prescription (-28,0) days or (0, 28) days before conception, respectively, where 0 denotes the estimated day of conception—imputed to be 14 days after the first day of the last menstrual period (LMP). In other words, you can think of day 14 as being the first day of the LMP. Assuming a regular 28-day cycle, you can think of day -14 as being the day you expect to get the period again. Thus the baseline (-14,14) definition assumes that women are unaware that they are pregnant if they take out a SARC prescription between the first day of the LMP and the day they expect to have the period again.

is not as precisely measured for abortions, we cannot be confident that the women are not getting the SARC in response to the conception. For this reason, we focus only on SARC births and not pregnancies.

Second, we consider a definition of unplanned pregnancies among SARC users based on abortion counseling meetings. Among women who took out a SARC prescription less than 365 days before conception, we define an unplanned birth as one in which the mother had an abortion counseling meeting and ultimately gave birth. For both definitions, we compare labor market trajectories to women of the same age who get a SARC prescription in the same year.

Finally, we compare the effect of births among SARC users who seem to have deliberately stopped birth control, in which case we assume the birth was planned. For this group, we require that the last SARC prescription was 84-365 days before conception. We again compare labor market trajectories of women with "planned births" to women of the same age who get a SARC prescription in the same year.

IVF. IVF treatment extracts eggs from a woman, fertilizes them in a lab and then re-inserts a viable embryo. Since 2003 (i.e., for our entire sample), Swedish policy has been to insert only one viable embryo (Bhalotra et al., 2022). In Sweden, health care is heavily subsidized and health insurance coverage extends to IVF treatment. Residents only pay a small amount annually (besides taxes) to access up to three rounds of egg extractions, conditional on a few eligibility criteria. These criteria require that women undergoing IVF procedures are in a stable relationship (married or cohabiting for at least two years), do not have prior children and recommends that they are below 40 years old at the time of the first treatment.[56]

Women undergoing IVF must take several prescription drugs with hormones as a part of IVF treatment. First, a woman takes a hormone to stimulate the development of the eggs. Second, she takes a "trigger" or "ovulation" shot that fixes the time of ovulation. Finally, she takes hormone supplements after the egg has been inserted to improve the chances of a successful pregnancy. Specifically, we use the ATC codes G03GA01, G03GA02, G03GA05, G03GA06, G03GA08. The procedure can take place at both public and private fertility clinics, but we only observe procedures from public clinics.

To validate that this is a woman's first IVF procedure, we require that these fertility drugs have never been prescribed before for a given women. We also check that we do not observe any prior IVF treatments, since our data on procedures goes back farther (2001) than our prescription data (2005).

Anxiety and Depression. Finally, we use the prescription drug data to create two measures of mental health based on purchasing anti-anxiety and anti-depressant medications: ATC codes starting with N05B are classified as anti-anxiety drugs, while ATC codes starting with N06A are classified as anti-depressants.

[56]Other criteria include a BMI within the normal range, no evidence of risky behavior and an assessment of the mental and physical health in general (Bhalotra et al., 2022). The age cut-off also varies slightly over time and region (Bögl et al., 2024).

This aligns with the definition in Persson and Rossin-Slater (2024).

C.2 Income Measures

Our main income measures are observed yearly in the *LISA* database. The income measure that most directly measures labor market productivity is earnings, which is the yearly gross labor income from all employment spells (based on the variable *LoneInk*).

Our primary income measure also adds all income sources related to public parental- and family-leave benefits (as summed up in the variable *ForLed*) and pregnancy-leave benefits paid out with sick-leave benefits because of reduced work ability (as summed up in the variable *SjukPP*). *ForLed* adds up the benefits related to having a child. That is, the sum of parental leave benefits (*ForPeng*), temporarily taking care of a sick child (*ForVab*), taking care of a child who is sick for more than six months (*VardBidr*), and from 2011 the municipal extension of taking care of a 1-3 year old in special circumstances (*KomVardBidr*) is also included. In addition, women can receive pregnancy leave benefits if they are unable to work during pregnancy.[57] Pregnancy leave benefits are not included in *ForLed* but included with sick leave benefits ("sjukpenning") in the variable *SjukPP*. Women are only eligible for pregnancy leave benefits during the last 60 days before the due date, so women having pregnancy complications or work deemed too demanding earlier than 60 days prior to the due date are on other transfers, mostly sick leave benefits. While sick pay ("sjuklön") for the first 14 days of sickness is included in the earnings measure (*LoneInk*), income related to longer-term sickness is not. If an employee is sick for more than 14 days, they have to apply for federal sick leave benefits and these are also included with pregnancy leave benefits in *SjukPP*.[58] We prefer this income measure because it comprehensively captures compensation, including firm-provided parental leave top-ups and sick pay that cannot be separately measured in public payment variables. The "raw" earnings measure may selectively include firm-provided parental leave benefits, particularly from more "family-friendly" employers that supplement public payments to replace a higher fraction of pre-leave earnings. Our primary outcome (earnings including paid leave) therefore accounts for both government-paid and firm-paid parental leave.

C.3 Occupation Measures

Our primary measure of occupation skill requirements is based on the first digit of the Swedish occupation code (`SSYK96`) in LISA that is based on the International Labor Organization (ILO) `ISCO88` occupation code.

[57] This benefit was called "havandeskapspenning" until 2011 when it changed name to "graviditetspenning".

[58] See e.g. Henrekson and Persson (2004), Johansson and Palme (2005), and Angelov et al. (2020) for more details on the sickness insurance system.

This occupation code is organized hierarchically, with increasing levels of granularity. The first digit represents the "occupation area", which is the broadest category (10 major groups). Each occupation area is split into multiple "primary groups" (second digit, 27 sub-major groups), which are further divided into multiple "occupation groups" (third digit, 113 minor groups).

To understand our occupation measure, it is useful to highlight the fundamental concepts underlying this classification. Occupation refers to the type of work performed, where a number of different jobs with similar tasks and duties constitute an occupation. Skills, defined as the knowledge and expertise needed to perform tasks and duties on the job, have two dimensions: skill level and skill specialization. Skill level is mainly used as the criterion at the 1-digit level, while specialization guides the more detailed sub-levels.

Skill level is defined as a function of the complexity and range of tasks to be performed in an occupation. It is measured operationally by considering: (i) the nature of work performed in relation to characteristic tasks, (ii) the required level of formal education according to the International Standard Classification of Education (ISCED-97), and (iii) the amount of informal on-the-job training and/or previous experience required for competent performance.

We construct an indicator for whether women work in occupations with high skill requirements. Following ILO guidelines and common practice in the literature, we classify three of the ten major occupation groups as requiring high skill levels: (1) "Legislators, senior officials, and managers". (2) "Professionals" (skill level 4), and (3) "Technicians and associate professionals" (skill level 3).

To illustrate which occupations are categorized as requiring high skill levels, we provide some examples of health care jobs from the hierarchical structure: Occupations classified as high skill (level 4) include "Professionals" (first digit 2), which encompasses "Life Science and Health Professionals" (2-digit code 22), and further includes "Medical doctors, dentists, veterinarians, pharmacists, etc." (3-digit code 222). Occupations with high skill (level 3) requirements also include "Technicians and associate professionals" (first digit 3), which includes "Jobs in biology and health care that require a college degree" (32), encompassing "Physiotherapists, dental hygienists, etc." (322) and "Nurses" (323). In contrast, examples of occupations classified as requiring low skill levels include "Service and shop sales workers" (5), including "Service, care, and security" (51) workers, such as "Health care assistants, assistant nurses, etc." (513) which is the most common 3-digit occupation among women in our sample.

Description of occupation transitions This appendix section provides a more thorough description of the occupational transitions across treatment groups. Around a quarter of our LARC sample are working in service, care, and security jobs with low skill requirements the year before LARC prescription. Most of these

(21% of our LARC sample) are health care assistants, assistant nurses, and personal assistants.[59] Tables A6 and A7 describe the persistence of occupations and how switches are related to skill requirements and enrollment in education. A few facts worth noting: First, occupation persistence increases with age and it is generally higher in the service and care occupation. Table A6 shows that 42% (39%) of women are still in the same 2-digit (3-digit) occupation at age 28 as they were at age 22, while this is true for even more women 58% (53%) from age 28 to age 34. Persistence is much higher for the service, care, and security occupation. Second, most occupation switches are associated with occupation upskilling and enrollment in education acts as a mediator for occupation upskilling—especially for younger women.[60] 30% switch to an occupation with higher skill requirements from age 22 to age 28, 39% of those who had been enrolled in education during the period, and almost all women who switched occupation following an education spell upskilled. The bottom panel of Table A6 shows that these numbers are even higher for the most common occupation with low skill requirements and 11 percentage points higher for women who were enrolled in education. Table A7 shows that the most common occupation upgrade from being a health care assistant, assistant nurse, or personal assistant is to jobs in biology and health care that require a college degree: 8.45% (4.62%) from age 22-28 (age 28-34).

C.4 Employment

Employment in the Swedish register-based labor market statistics (RAMS) follows the ILO's definition, which classifies anyone who performed income-generating work for at least one hour during a reference week as employed. The statistics focus on November as the reference month and the employment classification also links survey data from the Labour Force Survey (AKU) with the administrative variables to maintain consistency with survey definitions. Our measure of employment is an indicator for being an employee in the last week of November in a given year. That is, the *SyssStat* variable taking on the value 1.[61]

C.5 Wages and Hours

The Wage Structure ("*Lönestrukturstatistik*") data is a yearly snapshot that is intended to get an overview of the evolution of the wage structure in the economy. The data is collected by SCB and employer organisations through a survey of employers during a sample week once a year. The sampling differs by sector. The public

[59] Note that these numbers are similar for the full sample of women we observe in 2009 and lower for our IVF sample as 18% and 14%, respectively, are in these occupations the year before first IVF treatment.

[60] About half of the women who are enrolled in education the year before LARC prescription are in college or university, about a quarter of them are enrolled in adult education (*komvux*), and the rest are spread over various types of shorter and vocational education programs. Stenberg and Westerlund (2008) provide a detailed description of adult education in Sweden and evidence of its beneficial effects on labor market outcomes.

[61] This variable changes name and definitions during our sample period: *SyssStat* in 1998-2003, *SyssStatJ* in 2003-2011, and *SyssStat11* in 2011-2013.

sector has the broadest coverage, since data is collected for everyone employed in the state, regions, and municipalities during the sample week. For the private sector, however, only a subset of employers are surveyed about their workers during the sample week. This means that there are two levels of selection that make it challenging to use this survey data: selection into employment during the sample week and selection into the private sector, both which may vary by fertility intentions and labor market productivity (Nielsen et al., 2004). Therefore, we present impact estimates on the probability of being observed in the Wage Structure data alongside the impacts on full-time-equivalent (FTE) wages (measured by *MLON*) and actual work time as a fraction of full-time (measured by *TJOMF*).

C.6 Education

We measure enrollment in education based on the *LISA* variable *StudDeltTyp*. This is an inclusive definition, as it encompasses all types of education included in *StudDelt*. That is, enrollment in high school, municipal adult education (*komvux*), technical preparatory education between high school and university (*tekniskt basår*), undergraduate education, postgraduate education, vocational education, folk high school, and government-funded study abroad programs during the Fall semester each year. Note that labor market education was added to this definition in 2001, primary school education was added in 2002, and Swedish for immigrants (*SFI*) and supplementary education were added in 2012.

Highest completed education is based on the *LISA* definition of education, which is based on the education database *UREG*. Each year, a prioritization algorithm is used to determine each individual's highest completed education. In *LISA*, The 3-digit variable *SUN2000Niva* denotes the highest level of education. The first digit represents nine levels of education according to *ISCED 97*. The second digit represents the length (in years) of the highest level of education, and the third digit represents the specific type of education.

C.7 Family

Each family is identified by a family ID (*FamId*), based on the family definition in the Total Population Register (RTB). The family ID equals the individual ID number of the oldest person across at most two generations with formal relational ties to one another who are registered as residents at the same property.[62] When more than two generations live together, the family ID is based on the youngest generation, provided it is unmarried. An individual can only belong to one family. Unmarried adults who are registered on the same address/property *and* have common children are part of the same family regardless of the child's registered address. Cohabitants who do not have children in common cannot be connected to the same

[62] Formal relational ties include spouse, registered partner, cohabitant with shared biological or adoptive children, biological parent, adoptive parent, guardian (for children under 18), and foster parent.

family. Statistics Sweden (SCB) estimates that there are at least 500,000 people who are cohabiting, but cannot be connected to the same family. Cohabiting families may also be misclassified when a property contains several apartments. Those who are classified as cohabiting with common children can possibly live in different apartments in the same property. This type of misclassification is more prevalent for larger properties. However, more than 75 percent of the population lives in properties with fewer than 100 people. About 50 percent of the population lives in properties with fewer than 10 people.

D Institutional Setting

Sweden has a high level of social insurance, and it is a Nordic welfare state with a long history of providing high quality and low cost (to the individual) health- and childcare. Sweden simultaneously has maintained high fertility rates and women are almost as likely to work as men (Gustafsson and Jacobsson, 1985; Sundström and Stafford, 1992; Rønsen and Sundström, 2002) but not as likely to reach the top of the career ladder and earn top incomes (Albrecht et al., 2003, 2015).

This could partly be because Sweden has a generous family policy aimed at supporting the combination of working *and* raising children.[63] The Swedish welfare state provides both financial and in-kind support for families with children; including paid parental leave, subsidized child care, paid leave to take care of sick children, and universal child allowances. Swedish family policy asserts the same rights and obligations regarding family and labor market work for both women and men. Most support is conditional on (past) earnings, but individual and independent of civil status (Gustafsson and Stafford, 1992, 1994). This together with individual (progressive) taxation provides strong work incentives in single- as well as dual-earner families.

D.1 Parental Leave

Sweden already had paid maternity leave and strong job protection for mothers from the mid-1950s. Björklund (2006) describes the evolution of family policies from the 1960s through the following two decades in which there were several major extensions of its generosity. Importantly, in 1974, it became a parental leave system. Although fathers have the same rights to parental leave as mothers, mothers continue to utilize the bulk of paid leave opportunities (Sundström and Duvander, 2002; Duvander et al., 2020; Ginja et al., 2023). More recent papers describing the parental leave scheme during our sample period include Liu and Skans (2010) and Avdic and Karimi (2018).

Parents generally have the right to take full-time parental leave with a duration until their child is up

[63] See, for example, Hoem and Hoem (1996), Rønsen and Sundström (2002), Björklund (2006), and Duvander (2008) for an overview of Swedish family policy and related employment legislation and Jaumotte (2004) for an OECD country comparison.

to 18 months old. Since 2002, parents have been allotted a total of 480 days (approximately 16 months) of leave for each child. 390 of these days (approximately 13 months) are based on past income ("parental salary") provided they had been employed for 240 days before leave, otherwise they receive a basic "parental allowance" that is the same amount as the sick leave benefit. The parental salary amount depends on the collective agreement the individual is subject to. Most of the largest funds provide a parental salary such that the total parental benefits replace up to 90% of the parents' salary for the first six months of leave. Many employers also top this up such that many parents face an effective replacement rate of up to 95% for the duration of a year. The remaining 90 days are worth 90 SEK per day if the child was born before July 1, 2006, and 180 SEK per day if the child was born after July 1, 2006.

The only restriction on how parents distribute the parental leave time among them (during our sample period, or more specifically 2002-2016) is that two months of the allotted leave time is set aside to each parent.[64]

D.2 Child Benefits

During our sample period of 2005-2012, other benefits related to having a child include temporarily taking care of a sick child, taking care of a sick child for more than six months, and the municipal extension of taking care of a 1-3 year old in special circumstances.

The monetary transfer for temporarily taking care of a sick child is known as *Vab* (an abbreviation of "*vård av barn*"). Parents are eligible for this benefit when they are taking care of a child who must miss school due to sickness, accompanying a child to a visit with a doctor, children's health center, dentist, or child and adolescent psychiatry. Parents can receive these benefits for either 75, 50, 25, or 12.5 percent of a day. Payment is equivalent to slightly less than 80 percent of income. Parents are allotted a maximum of 120 days of *Vab* per year per child, with both parents sharing these 120 days. If a child is sick for more than seven days, parents must submit a letter from a doctor or nurse to the Swedish Social Insurance Agency.

If taking care of a sick child for more than six months, parents are eligible for an additional benefit ("*vårdbidrag*") primarily intended for parents whose child had a disability and/or a chronic disease.[65]

The remaining benefit related to having a child is the municipal extension of taking care of a 1-3 year old in specific circumstances ("*kommunalt vårdnadsbidrag*") which was fully in place in 2011, but it was discontinued in 2016. This transfer allowed municipalities to provide monetary benefits to parents if their child was registered in the municipality and did not have a full-time spot in a preschool program. Aid was

[64] See the Parental Leave Act (1995:584) for the complete law text and its changes over time, available in the Swedish digital law archives. The law was enacted in 1995 and there was only one minor change during our sample period in 2006 (2006:442). Also see the Social Security Code (2010:110) for more details.

[65] See the Act on Disability Compensation and Care Allowance (1998:703) and the Social Security Code (2010:111) for complete law text and changes over time.

split evenly between both parents, regardless of whether one of them was registered outside the municipality. In any given month, parents would not be eligible for payments if they had received parental leave benefits, unemployment benefits, vocational aid, government-provided sick pay, pension payments, aid for assisting the elderly, or certain aid for newly-arrived immigrants.[66]

D.3 Childcare

Local-government-subsidized center-based childcare is another important component of Swedish family policy. The contemporary daycare system was established in the 1960s and expanded substantially through the 1970s (Björklund, 2006; Gustafsson and Stafford, 1994). The children born during our sample period attend daycare during the post-reform regime described in more more details in Lundin et al. (2008), Mörk et al. (2013), Aalto et al. (2019), and van den Berg and Siflinger (2022).

Municipalities are required by law to provide highly subsidized, high-quality care to children whose parents work or study during regular work hours. Consequently, enrollment rates are as high as around 70% for children ages 1-2 years old and 90% for children ages 3-6 years old. Childcare is highly subsidized and parents pay a percent of household income but with a cap.[67] The cap is low as the intention in the law is that no parent refrain from childcare due to economic reasons. Child care is offered during regular work hours.

D.4 Health Care

Healthcare is universal, mostly public, and organized at the county level.[68] Co-payments and out-of-pocket expenses are generally low and capped.[69]

A key source of care for young children in Sweden is the child health care service (*BHV*). Its goal is to promote children's health, development, and well-being. It focuses on children's everyday lives, health, and development, raises awareness of various health risks, performs age-based health checkups, administers vaccinations, assists children with disabilities and/or chronic illnesses, and aids parents who are at risk. Like overall healthcare, *BHV* is controlled at the county level. *BHV* is free of charge and universally offered to children from birth to when they start pre-school (or first grade for those who do not attend pre-school).

[66] See the Act on Municipal Guardianship Allowance (2008:307) for the complete law text and changes over time.

[67] Domeij and Klein (2013) use data from Germany to show that subsidizing daycare can substantially increase welfare by encouraging women with small children to work in an economy with distortionary taxes on labor, while Lundin et al. (2008) find that female labor supply inelastic at the high level of daycare prize subsidies in the Swedish childcare system the mothers in our sample face.

[68] Aalto et al. (2019) and van den Berg and Siflinger (2022) describe health care for children in Sweden during our sample period in more details.

[69] Act (2002:160) on Pharmaceutical Benefits (consolidated until SFS 2013:1141) reflects all changes until the end of our sample period. This law contains provisions on pharmaceutical benefits and price regulation of goods included in the benefit.

For school-aged children, preventive health services and vaccinations are organized and provided by school nurses within schools. These services cover all children, and they are also free of charge.[70]

While healthcare is primarily provided at the public level, private healthcare is also available in Sweden. Private healthcare can either be under contract with the local/regional authorities or not be under contract. With healthcare administration being decentralized, patient fees differ across the country. Referrals from general practitioners are not required to contact a specialist, but if a patient does not have a referral, they may need to pay more and/or wait longer.

D.5 Abortion

Swedish abortion law remained unchanged during our sample period, with relatively unrestricted access. Abortion is legal until 18 weeks of pregnancy, with later abortions permitted only when the fetus is deemed non-viable Ministry of Social Affairs (1974).

In 2007, the Swedish Society of Obstetrics and Gynecology (SFOG) developed a formal midwife training program for abortion care at the National Board of Health and Welfare's request. The first midwife-operated abortion unit opened in 2009, expanding to nine units by 2012 (the last year we have data on childbirths) and approximately 50 units today out of 130 clinics offering abortion care.[71] Figure A11d shows that LARC pregnancies had smaller effects in later years (lower t in panel d). We would expect larger effects if we are not observing some abortions in later years. In fact, we observe no changes in abortion rates over time.

[70]The Health and Medical Care Act (1982:763) (consolidated until SFS 2013:1141 to reflect all changes until the end of our sample period) states the goal of health care and thus also child health care: "The goal of health care is good health and care for equal conditions for the entire population. Care must be given with respect for the equal value of all people and for the dignity of the individual person. Those who have the greatest need for health care must be given priority to receive care."

[71]SFOG's Reference Group for Reproductive Health, in collaboration with the WHO Collaborating Centre at Karolinska Institutet, created the first formal midwife course in abortion care and ultrasound, jointly offered by SFOG and the Swedish Association of Midwives beginning in 2013 (Endler et al., 2020) which was after our sample period.

E Children and Careers: Why Delay Matters

In this section, we present a model that illustrates how the labor market impacts of children depend on the prior and current human capital investments of mothers. In this simplified two-period Ben-Porath (1967) model, individuals have period utility over consumption c and make investments in human capital.[72] In each period, human capital h determines earnings, and investment in human capital i is costly because it takes time out of work. Future human capital is the sum of depreciated past human capital (depreciation rate δ) plus the result of past investment: $A(ih_1)^\alpha$ where A is the productivity of investment, and $\alpha \in (0,1)$ controls the rate at which returns to investment decrease with additional investment.

Individuals maximize utility from consumption, but in this setting we will assume perfect capital markets and that the discount rate β over future consumption equals $\frac{1}{1+r}$ where r is the interest rate. These assumptions imply that there is a separation between consumption choices over time and the problem of maximizing the net present value of future income. To maximize the lifetime income they have available for consumption, individuals solve:

$$\max_{i,h_2} \left\{ h_1(1-i) + \frac{1}{1+r} h_2 \right\} \quad s.t. \quad h_2 = A(ih_1)^\alpha + (1-\delta)h_1$$

which gives optimal human capital investment:

$$i^* = \left(\frac{\alpha A}{1+r} \right)^{\frac{1}{1-\alpha}} \frac{1}{h_1}$$

Suppose that the effect of an unplanned pregnancy is summarized by a reduction in A, the productivity parameter in the technology for transforming investments in human capital to future earnings. This captures effects of children in the form of increased difficulty studying for an exam when caring for a newborn, the impact of lost sleep time on cognitive ability, and many other potential shifts in human-capital accumulation technology imposed by children.[73] The derivative of i^* with respect to A is positive, so investment falls. Let $\overline{A} > 0$ be the baseline level and $\underline{A} < \overline{A}$ the productivity of investment in the presence of children. If i^* falls, then the net present value of income must also fall, since otherwise individuals would have had higher income by choosing $i = i^*(\underline{A})$ in the case without children.[74]

[72] A two-period model is sufficient to illustrate the main effects of children that we highlight.

[73] We also consider a richer, infinite period model in which the technology shifts only temporarily in the presence of children. We present a full Ben-Porath model and discuss the impact of a temporary technology shock in Appendix **??**.

[74] The derivative of the net present value of income with respect to A is given by

$$\frac{\partial I(i^*(A))}{\partial A} = \frac{1}{1-\alpha} \left(\frac{A}{1+r} \right)^{\frac{\alpha}{1-\alpha}} \frac{1}{1+r} \alpha^{\frac{1}{1-\alpha}} \left[\alpha^{-1} - 1 \right]$$

which is positive for $\alpha \in (0,1)$. $I(i^*(A))$ is the present value of lifetime income evaluated at the optimal investment choice: $h_1 - i^* h_1 + \frac{1}{1+r}(A(i^*h_1)^\alpha + (1-\delta)h_1)$

As emphasized by Ben-Porath (1967), investment in higher future earnings through human capital accumulation need not come from formal education, and can take place throughout the life cycle. In this simple model, the crucial takeaway for our paper is that having children early does not directly affect preferences or productivity later in life. Instead, children affect the ease of making present-day investments in the future. The relationship between human capital accumulation and early childcare has been noted in the literature. Increased educational investments are emphasized in Goldin and Katz (2002), Stevenson et al. (2021), and Steingrimsdottir (2016) as key outcomes of increased access to contraceptives.

In our sample, the youngest women are 22 years old and human capital investments are mostly focused on college education, adult education, and vocational training. How do investments like these interact with the presence of children? A recent New York Times article features interviews with women from a low-income background in the US. One interviewee describes wanting to start a spa business before having children, another is a dental assistant who hopes to earn a dental hygienist degree before having children because it would allow her a more flexible schedule. As noted in the article by sociologist Kathryn Edin, it is clear from these interviews that, "even among the poorest women, there is a recognition that a career is part of a life course" (Speranza, 2021). This motivation is especially important for the group of women we study, who are likely using LARCs precisely because the timing of children is important to them, as discussed in Gomez et al. (2021) and Bell et al. (2018).[75] Beyond leading to higher-pay jobs, current human capital investments would potentially allow women more control over their schedule and flexibility to care for potential children (Goldin, 2014). Our paper examines the realized impact of children on women's lives, among those who—like those in the article—were hoping to delay having children, and many of whom were making important career investments.

Human capital accumulation is only one of many reasons women may delay having children. They may be waiting for the right partner, they may be waiting to accumulate savings, they may be avoiding times of poor health, and they may simply be enjoying non-child related leisure. These mechanisms do not necessarily make having children as late as possible optimal: though there are many financial advantages that come with delay, declining fertility and idiosyncratic shocks make the problem of calculating the optimal age at which to have children non-trivial. In our setting, we will study differences in the effect of planned vs. unplanned children, holding age fixed, as well as the effect of delay, regardless of planning. We look at the effect of delay by studying women who have unplanned (and planned) pregnancies earlier vs. later in life.

[75] As discussed in Gomez et al. (2021) and Bell et al. (2018), unplanned pregnancy is most often associated with use of condoms or withdrawal, but these methods are endogenous to the fertility desires of women and their partners. Those using more effective forms of birth control typically are those who believe that "things will be different in the future" due to future changes in their financial circumstances (Gomez et al., 2021).